Japanese Studies in the History of Science
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THE HISTORY OF SCIENCE SOCIETY OF JAPAN
TOKYO
Japanese Studies in the History of Science
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Activities of "The History of Mathematics Society of Japan (Nihon Sugakushi Gakkai)"

KAZUO SHIMODAIRA**

The History of Mathematics Society of Japan started, as the present writer reported previously in our journal***, for the purpose of researches in the history of mathematics in general, not confined in particular fields of mathematics. However, the members of Society are inclined to restrict their field to Japanese mathematics and concentrate their studies in the history of the traditional ones—WASAN****. In fact, most of the members, with the exception of a few, have been keenly interested in WASAN.

The following are the table of contents which appeared in the recent issues of Sugakushi Kenkyu***** (or Journal of History of Mathematics, Japan).

Eiji CHIKIRA: “Sangaku of the Narushima Hachiman Shrine.” (1967, No.33)
Kusuo TAKEDA: “Chinese mathematics in the stream of the world history.” (1967, No.34)

* The address of society is Japan Mathematical Society (NIHON SUGAKUSHI GAKKAI).
Fuji Junior College, 3-chome, Tozuka-machi, Shinjuku-ku, Tokyo, Japan.
** Maebashi Technological Junior College, Kamisadori-machi, Maebashi city, Gunma-ken.
**** 和算 – Japanese Mathematics which was prevalent during the Edo period (1603–1867).
***** 数学史研究 The Journal of History of Mathematics, Japan.

JAPANESE STUDIES IN THE HISTORY OF SCIENCE No. 8(1969)
Shiko IWATA: “Problems of sangaku* — the causes of the long disputes between Saijo school and Seki school.” (1967, No.35)


Ryozo Funayama: “The mathematical education of postwar Japan (1).” (1968, No.36**)

R. Funayama: “The mathematical education of postwar Japan (2).” (1968, No.37)

Shigeru Kanda: “On Sanyoki***, the oldest of Japanese existing mathematical books which was printed ca. 1620.” (1968, No.37)

Taisuke Noguchi: “Mathematical recreations in Kikubuntoshu**** (1722).” (1968, No.37)


Kogo Hagino: “Significance of studying the history of Japanese mathematics.” (1968, No.38)

K. Shimodaira: “Significance of studying the history of western mathematics.” (1968, No.38)


Genzo Suga: “On the mathematical education in the early years of the Meiji period (1868–1902).” (1968, No.38)


Shigeo Takagi: “Western mathematical recreations available in mathematical education.” (1968, No.38)

* 算額
** A special issue featuring the history of Japanese surveying.
*** 算用記
**** 規矩分等集
***** A special issue featuring mathematical education and history of mathematics.
T. Noguchi: "Eastern mathematical recreations available in mathematical education." (1968, No.38)

Ryo-ichi Takekuma: "On two curves of which R. Descartes did not treat in his La Géométrie." (1968, No.39)

Tamotsu Murata: "How should the history of mathematics be studied in our actual state?" (1968, No.39)

Ichiro Yamamoto: "On Shinko Seino (or Kiyono)." (1968, No.39)


K. Shimodaira: "Some problems in the study of the history of mathematics (2)." (1968, No.39)

Heizaemon Kato: "How was infinite series to calculate the length of arc formulated in Japan?" (1969, No.40*)

R. Funayama: "The mathematical education of postwar Japan (3)." (1969, No.40)


Shiko Iwata: "Modern solutions of problems of WASAN." (1969, No.41)


Z. Katano: "Mathematical education and the history of mathematics (7)." (1969 No.41)

Of the books published by the members of the society, the studies of Sangaku are largest in number. Sangaku is a kind of tablet on which are written some WASAN problems or their solutions. It was generally dedicated to a temple or a shrine in a form of a prayer, and hung on the wall of the temple or the shrine. The custom of dedicating of Sangaku was already popular ca. 1660 and spread throughout Japan. Present-day Japanese people, however, find Sangaku unintelligible, because the custom went out of fashion more than 60 year ago.

* No. 40 contains a list of articles from No.1 to No.39.
It is not clear how many Sangaku-tablets were hung, but we suppose there were tens of thousands of them. It is true that it was very difficult for Japanese mathematicians to afford to publish their books, but some hundred kinds of mathematical books were printed during the Edo period (1603–1867). There must have been many manuscripts of unpublished works. There were also many mathematicians who are known to us only by means of Sangaku.

Sangaku gives us a key not only to the content of WASAN but also to some local histories of Japan. Indeed Sangaku should be studied systematically by students of the history of Japanese mathematics. However, it is unfortunate that most of them are inclined to study separately in this field.

The following are publications of the members of the society.


A commentary on articles and books on Sangaku. Shimpeki-sampo*, a collection of problems described on Sangaku, which was published in 1789, and its sequel (or Zoku-shimpeki-sampo**, 1807) was reproduced.


Explanations and reproductions of Sangaku, found in various provinces of Japan or cited in other sources, have been published by the following people.

Motohisa Matsuoka, Eiji Chikira, Hachio Norii, Yoshi Okada, Masaaki Iizuka, Kozo Oya, Taisuke Noguchi, Hiroshi Amano, Yoshimasa Michiwaki, Akira Hirayama, Chizuru Akabane, Nobuya Nakamura, Hideo Kuwabara, Ichiro Yamamoto, Toshio Matsuzaki, Masuo Honda, etc.

Ryuji Yoshida: Nippon no Sugakusho (Interpretation of WASAN Books, Private ed.).

An explanatory book of Zoku-shimpeki-sampo (1807) and Zoku-shimpeki-

* 神壁算法 (1789)
** 終神壁算法 (1807)
sampo-kigen* (1833)


In 1627, the Warring-states period in Japan ended and peace was resumed, Jingoki, one of the most famous WASAN books, was published.

This well-illustrated book thoroughly explains mathematics from the elements to advanced stages, and contains many mathematical recreations to stimulate readers; some of the illustrations are printed in three colours — red, green and black. There are some who have the opinion that, as a popular book of elementary mathematics, its construction is better than any other books of this kind in the world. Some say that the mathematics of the Edo period began with this book. This book not only covers a history of mathematics, but also contains pictorial wood-cut printing (Ukiyo-e), elementary mathematical text-books, economics and many other subjects. Though Jingoki was published in large numbers, it is very difficult to determine the first edition. Y. Yamazaki reproduced a copy of the presumably first edition and a few later editions, explaining them in his book.


It is not an exaggeration to say that there is no Japanese family which has no Soroban (Japanese abacus). All of us can use Soroban easily for arithmetic, and some for such complicated calculation as evolution and involution. This book explains many types of Soroban, with photographs, in an historical order.


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* 神秘算法起源(1833).
** 墓劫記 It is written by Mitsuyoshi YOSHIDA (1598–1672).
Naonobu **Ajima** (1732-1798) made many original contributions to **WASAN** such as the discovery of double integral and what is called Malfatti’s theorem in the West. **Hirayama** and **Matsuoka** collected all the works of **Ajima**’s, none of which has ever been published, and they explained these in English.

**A. Hirayama** and **M. Matsuoka** ed.: **Aida Sanzaemon Yasuaki** (Fuji Junior College Press, Tokyo, 1966).

**Yasuaki Aida** (1747—1817) was a mathematician who wrote more books than any other mathematicians of his day. He wrote 600 mathematical books and 1500 other titles, and had many followers. Since he was born in the Tohoku province, northeastern Japan, most of his followers lived in the Tohoku province and they had developed the primary and secondary mathematical education in this province during the **Meiji** period (1868—1912).

**Aida** is still respected in Yamagata-ken, his native prefecture. The above book was published in memory of the centenary of **Aida**’s death, in which his main work **Sampo-tenshoito-shinan** (1811) was reproduced together with explanatory notes.


There are many other guides to the history of Japanese mathematics. This guide is characterized by many pictures and an easy explanation.

**Fuji Junior College Study Group of History of Science** ed.: **Kyoshi no Tame no Sugakushi Koza** (Lectures on the History of Mathematics for Teachers. Vol. 1, Fuji Junior College Press, Tokyo, 1967).

Since only a few Japanese universities give lectures on the history of mathematics, most people have a poor knowledge of this subject. The History of Mathematics Society of Japan has made efforts to popularize the history of mathematics offering lectures of the subject twice a year. The book mentioned in the above consists of those lectures. Its table of contents is as follows.

**Shigeru Nakayama**: “Mathematics and science in the western culture.”

**T. Murata**: “A history of the foundations of mathematics.”

**K. Shimodaira**: “Mathematics in early **Edo** period.”

**S. Oya**: “A history of primary mathematics education.”
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K. Hagino: "Sangaku."
T. Noguchi: "A history of mathematical recreation of Japan."
S. Takagi: "Mathematical recreation since the Meiji period."
H. Suzuki: "On the introduction of Soroban."
The following is a list of recent lectures given by the society.
Katsuji Miyazaki: "A history of maps."
S. Oya: "Congruity of triangles."
Z. Katano: "The role of the history of mathematics in the mathematical education of high schools."
Toshio Watanabe: "The way to compose a calendar in old calendar days."
K. Shimodaira: "A history of divergent series."
S. Oya: "The beginning of four rules problems during the Meiji period."
A. Hirayama: "On the contribution of Yasuaki AIDA."
K. Shimodaira: "A biography of Yasuaki AIDA."
S. Oya: "Axiom of parallel lines."
Kesakatsu Koizumi: "A history of weights and measures."
H. Suzuki: "A history of slide rule."
S. Oya: "On the symbols of WASAN."
Z. Katano: "How to apply the topics in the history of mathematics to education."
H. Kato: "On the formation of infinite series in the Edo period."
K. Shimodaira: "Takakazu SEKI (or Kowa SEKI) (ca. 1640–1708)—A prominent mathematician."
S. Oya: "How to use Sangi*."
Koshiro Nakamura: "Problems in mathematical symbolism."
A. Hirayama: "On the contribution of Takakazu SEKI."
S. Oya: "A history of the teaching of function and set in primary education."
Kikuo Miha: "The mathematical education during the last war."
S. Oya: "On the fraction of old China."
Toshio Miyamoto: "The development of mathematical method in economics."

* Counting-rods made of wood.
Teaching the History of Chemistry in Japan*

Bun-ichi TAMAMUSHI**

Abstract:*** After describing the place of the history of chemistry in the Japanese system of secondary and higher education, the author cites his experiences in three types of courses: (1) a chemistry course for non-science students with liberal use of historical background material intended to acquaint them with actual scientific procedures; (2) an elective general education course in the history of science (Part I: Physics; Part II: Chemistry) for both science and non-science students, primarily utilizing Conant's "case study" approach; and (3) a five - semester program for training professional historians of science or chemistry. Special problems are then discussed such as bridging the gap between the modern scientific, technological culture rapidly assimilated from the West in the century since the Meiji restoration and Japan's traditional cultural values. Finally, the activities of Japanese scientific societies and journals with respect to the history of chemistry are considered.

This paper is based chiefly on my personal experience in teaching but I hope that it might give more or less a general scope for the status of teaching the history of chemistry in Japan.

In the early decade of the nineteen-twenties when I was student in the chemistry department of the University of Tokyo, professor Yuji Shibata, now president of the Japan Academy, gave us a lecture on inorganic chemistry, which was not mere information of the systematic knowledge of this field but accompanied by the historical introduction for every main subject. I remember that his lecture was particularly inspiring because of such treatment of subjects which otherwise might have been rather monotonous.

Soon after when I began to teach chemistry to students of a preparatory school for universities of the pre-war system of this country, I could hardly find

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* This paper is made on the basis of the talking paper presented at the Symposium on Teaching the History of Chemistry in the American Chemical Society's 155th Meeting in San Francisco from March 31 - April 5, 1968.

** Nezu Chemical Institute, Musashi University, Nerimaku, Tokyo.

*** The abstract in this form was prepared by Professor George B. Kauffman, Program Chairman of the above-mentioned Symposium.
time for weaving the historical background in my course plan for introductory chemistry, though I endeavoured to give students interest and stimulation in learning chemistry by telling them occasionally the history of some remarkable chemical discoveries.

My own interest for the history of chemistry, however, gradually increased through the teaching experience and I later became to believe that some pre-training in the study of the history of chemistry should be a requisite for every good chemistry teacher. I also remember that the *Journal of Chemical Education* began to be published in 1924 by the American Chemical Society just when I started my career as chemistry teacher and that, since then, how much I have been profited and stimulated by articles on the history of chemistry which now and then appeared in this journal.

After World-War II the educational system of Japan has been radically changed. We adopted the so-called six-three-three-four year school system after the model of the United States in place of the pre-war German type system. At present, the teaching of chemistry as an independent subject begins at senior high school, of which a certain number of credits are required for all students who enter colleges and universities. There is a minimum standard for the course plan of the secondary school chemistry set up by the ministry of education, where no special indication is given on teaching historical topics, although many high school text books are usually illustrated by the portraits of great chemists and appended by the chronological table of Nobel-prize winners or important chemical discoveries. On inquiry during these several years I have found that interest or knowledge of college freshmen in historical matters is generally poor and immature. Out of students of a whole class only some 10-20 percent students can give satisfactory answers for such questionaires as: what are the chief works of Lavoisier, Dalton and Mendeleef and approximate ages of their publication? This seems to indicate that secondary school students are rather crammed up by actual chemical facts to be memorized.

In Japanese colleges and universities the undergraduate course consists of two parts, namely the general education course and the specialized education course. The former covers usually the beginning three semesters, while the latter covers the rest five semesters, one academic year being divided in two semesters.
Accordingly, chemistry is also treated as a subject of the general education program which is commonly further classified in two course plans: one for science students and another for non-science students. The former plan usually takes a pattern of 'general chemistry' like Linus Pauling's well known text book, whereas the latter takes various forms and contents according to instructor's choice and inclination.

Teaching the history of chemistry is naturally a problem concerned with either the course for science students or that for non-science students. From my point of view it is supposed that every chemistry department of college or university should provide some practical plans for teaching the history of chemistry for both types of students. In actuality, however, curricula set up by most chemistry departments in our institutions look too tightly filled up to spare space for teaching the history of chemistry. For science-major students, even in the course of introductory chemistry most instructors feel shortage of time for handling historical subjects, because teaching materials of modern chemistry are ever increasing. In such a situation it is rather natural that our experiences in teaching the history of chemistry have been mainly relevant to the course for non-science students in liberal education programs.

In these eighteen years I have been occasionally engaged in teaching chemistry for non-science students at the University of Tokyo or at Tokyo Woman's Christian College and upon this teaching experience I have written a text book\(^1\) where I tried to treat some important concepts in chemistry, such as atoms and molecules, ions, nuclear atoms, chemical bond, chemical affinity, reaction equilibrium and kinetics, structural isomerism, macromolecules, etc., on the background of their historical development. For example, when I treat the concept of 'ions' I first introduce students into the phenomenon of electrolysis through the demonstration experiment on the solution of zinc bromide, as an example, to give them visual impression of the separation of elements at positive and negative electrodes and tell them how Faraday came to the notion of 'ions' as moving particles carrying electricity. Then I touch on the work of Hittorf of determining relative velocities of anions and cations under given electric field. After showing demonstration experiments on the electric conductance through gases, I explain how J. J. Thomson came to the discovery of electrons. Meanwhile students carry out in laboratory the determination of the Faraday constant and
further learn about Millikan's work on the determination of the elementary charge.

By comparing these two quantities students can conceive of 'ions' as particles carrying elementary charges in simple whole numbers. After going through the story on the discovery of atomic nucleus by Rutherford I proceed to explain the Bohr models for the atomic structure without going into detailed mathematical treatment of the theory. On the basis of such models students can finally grasp features of hydrogen ion or other ions as atomic nucleus or kernel deprived of some whole numbers of outer electrons.

This course is however not a course of the history of chemistry but rather a course of chemistry referred to its historical background. I presume that such a method of introducing chemical concepts as above exemplified should be effective in giving students an understanding for the actual scientific procedure which is to be considered one of the most important objects of teaching chemistry for non-science students. Though I am not still certain if my attempt has been quite successful, I could find at least some progress in students' understanding for scientific thinking and their appreciation in historical subjects through their response to questionnaires in final examinations.

It is often pointed out that in liberal education it is important to give students understanding for the relation of science to the general human cultures. For this purpose it might be more adequate to design a course for the history of chemistry (or science) as a single subject in curriculum and treat it on the basic idea suggested by George Sarton. The traditional outline course on the history of chemistry would be rather tiresome to ordinary students, while the method of case histories proposed by J. B. Conant is to be considered more effective.

In Tokyo Woman's Christian College there is a course for the history of science in the general education program open to both science and non-science students. This course is a one-year course, consisting of two parts: part 1, which is taken charge by Dr. Watanabe, includes such topics as: astronomy from ancient through Copernicus to Newton, studies on the nature of heat by Carnot, Rumford, Joule, and Helmholtz, science in relation to religion and literature in the seventeenth and nineteenth centuries, the development of science in modern Japan, etc. and part 2, which is under my charge, includes topics such as: the establishment of modern chemistry by Lavoisier, the development of atomic
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theory from Dalton to Rutherford, Pasteur and spontaneous generation of life, the development of synthetic chemistry and industrial applications, the twenty century's scientific scene and its philosophical implication, etc. This course, as it appears, is not an outline course but a type of case histories. The number of students of the class often attains more than hundred and we, instructors, feel pretty heavy burdens in assigning reading materials to students and reading their reports on these topics.

There are several other examples for similar courses on the history of chemistry (or science) which are prepared in Japanese colleges and universities. I mention here names of only few of chemists who are practicing such courses in their respective institutions: Drs. T. Dono, Nagoya Technical College, H. Kashiwagi, Nagoya University, M. Tanaka, Tokyo Institute of Technology and Y. Tsuzuki, Tokyo College of Science. The last mentioned person published recently a book on the history of chemistry which is characterized by its contents covering not only various aspects of the history of chemistry from ancient to modern ages but also the history of chemistry in modern Japan. A chapter on this latter subject may be considered to be an interesting case study material.

As is widely known, the development of science and technology in Japan has characteristic features in its rapidness and propagation. It is indeed a remarkable fact that in the history of science how Japan after the national isolation for a long period imported scientific and technological learning from western countries and how rapidly she assimilated and developed science and technology in the recent hundred years. In this respect I like to refer to a paper published by M. Tanaka in which he critically reviews the development of chemistry in this country in the period of 1837 – 1930. He further divides this period into the following four sub-periods: the period for germination of scientific and technical learning (1837 – 1867), the period for transition from traditional to western mode of learning (1868 – 1876), the period for foundation of educational and research institutions (1877 – 1900), and the period for independent research in science and technology (1901 – 1930). Japan is now recognized as one of the most active five or six countries in the World in scientific and technological research. Nevertheless, I cannot avoid to notice that there is still a deep gap between modern scientific, technological cultures and our own traditional cultures. The general public
including the literary people and politicians of this country tend to evaluate science mainly from the practical point of view. Their understanding for science is rather superficial. I therefore think that it is specifically important to give both future scientists and non-scientists in this country through education some sound understanding for the historical and philosophical background of the modern science laid in western countries since the time of Galilei and Kepler.

There is another problem we are faced with in our country, that is, how we should train and educate the specialist of the history of science or chemistry. There have not been any educational facilities in our colleges or universities for that purpose, except one in the University of Tokyo which was established in 1951 as one of senior courses in the college of general education for the study of the history and philosophy of science. Students, not more than ten every year, are adopted in this course who have finished the beginning three semesters of the college. The curriculum of this special course consists of 1) basic subjects in humanities and social sciences including history and philosophy of which a certain number of credits are required; 2) basic subjects of mathematics, symbolic logic, physics, chemistry, biology and cosmology, which are treated with special reference to methodology and history, of which student elects two subjects as majors; 3) foreign languages: besides English, German, French and Russian, one of which is required; 4) seminars and research works in respective major fields. When I was in charge of this newly established course, I used in my seminar some classical works in chemistry as reading materials such as Lavoisier's *Elements of Chemistry*, Einstein's paper on Brownian motion or Staudinger's work on macromolecules, and through such materials the historical and methodological aspects of chemistry were instructed. As there has not yet been established a graduate course for this field of the history and philosophy of science, the graduates from this course have to utilize other pre-existing facilities of the same university if they wish to continue their study. Otherwise they take job, for instance, in journalism. Among the graduates there are some excellent science journalists who are on active service.

Let me lastly mention some activities of Japanese academic societies relating the study of the history of chemistry. The Japan Academy has been publishing volumes on the history of science in Japan, one volume of which deals with the
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history of physical science and includes chapters on the history of chemistry in this country before and after the restoration of Meiji (1868). The Chemical Society of Japan, established in 1878, has at present several publications, among which two journals, Kagaku to Kogyo (Chemistry and Industry) and Kagaku Kyoiku (Chemical Education) include occasionally historical articles which may serve to arouse interest of professional chemists and teachers for the history of chemistry. The publishing committee of this society has edited a monograph series on chemical topics for young students, in which a volume on the history of chemical discoveries is added. The History of Science Society of Japan established in 1941 publishes now two journals: Kagakusi Kenkyu (Journal of history of science, Japan, quarterly) and Japanese Studies in the History of Science (in European languages, once a year, since 1962). In these journals one can find original papers, short notes, resource papers, book reviews concerning the history of science. In the last mentioned journal, besides the paper by Tanaka above referred to, there is an article entitled “On the studies of history of chemistry in Japan” contributed by Tsuzuki and his collaborator which deals with a brief but excellent survey of the papers on the history of chemistry appeared in this country in these twenty-five years.

In Japan there are at present only few scholars who are actually specializing in the study of the history of chemistry but there are pretty many chemists and chemistry teachers who are more or less interested in this subject. These people’s recognition for the importance of the history of chemistry with respect to education is now remarkably increasing. It is therefore expected that this symposium on teaching the history of chemistry organized on an international scale by the Division of History of Chemistry in the American Chemical Society will certainly give Japanese chemists and chemistry teachers great interest and stimulation.

Finally, I would like to express my best thanks to Professor George B. Kauffman, Chairman of the Symposium, for his kind invitation to contribute a paper to the Symposium. I also thank Dr. Minoru Tanaka for his kind advice and information given to me in preparing this paper.
References


Recently interests in the history of science seems to be spreading in our country among scientists and students. Reflecting this tendency, classics in the modern physics in Japanese translation are being published or planned to be published. For example, a new translation of E. Mach's *Science of Mechanics*, which was once translated nearly forty years ago, has been published in 1969. But the most remarkable success is attained by, it seems, the *Buturigaku Koten Ronbun Sōsyō* (Classical Papers in Physics) which has been planned and edited by the Group for History of Physics.

The Group for History of Physics is a body of those who are studying or interested in the history of physics. Since 1958 they have been publishing more or less regularly a mimeographed circular *Buturigakusi Kenkyū* (studies in the history of physics) which is opened for articles, preliminary reports, memoranda, book reviews, abstracts, guide to literatures and so on. It was about four years ago that a plan was proposed to translate selected classical papers which either played a decisive rôle or propounded ingenious ideas in the development of modern physics. At first it was planned to publish the translated papers in successive numbers of *Buturigakusi Kenkyū*. But after that since the Tokai University Press, knowing this project, proposed to undertake its publication, the project was changed and a new plan was adopted to publish them in a form of series of books under the above mentioned title *Buturigaku Koten Ronbun Sōsyō*. The selection of papers was carefully considered and revised several times. The first volume was finally published in February 1969 and, as well as the succeeding volumes, has been, to a surprise and a delight of

*) Department of Physics, College of Science and Engineering, Nihon University, Kanda Surugadai, Tokyo.
the editors, eagerly welcomed.

The *Buturigaku Koten Ronbun Sōsyō* restricts its scope to the generative period of the twentieth century physics and consists of following twelve volumes:

I. **Heat Radiation and Quanta** (edited by Tetsuo Tsuji)

This volume contains 14 papers covering the theoretical studies of heat radiation in the latter half of the nineteenth century which were inaugurated by G. Kirchhoff, were developed by L. Boltzmann, W. Wien, Lord Rayleigh, and J. H. Jeans, and culminated to the advent of quantum hypothesis of M. Planck.

II. **The Light Quantum** (edited by Tetsuo Tsuji)

This volume contains 7 papers of Einstein on the light quantum, the specific heat, the fluctuation of radiation field, and the emission of light by the atom; namely all the papers of Einstein on the quantum theory except those on the Bose-Einstein Statistics. Though *Collected Papers of Einstein* in Japanese translation were published in four volumes as early as in 1922 – 1924, the volume which was to be devoted to the quantum theory was eventually cancelled because of the difficult situation of the publisher caused by the Great Earthquake Disaster in Kantō area of 1923. The present volume therefore makes them for the first time available in Japanese.

III. **Old Quantum Theory** (edited by Tsuyoshi Ogawa)

Six papers by W. Wilson, J. Ishiwara, A. Sommerfeld, P. Ehrenfest, N. Bohr (1918), and J. H. Van Vleck are selected for this volume. They represent main stages in the development of quantum theory after Bohr’s 1913 paper.

IV. **Theory of Relativity** (edited by Tetu Hirosige)


V. **Kinetic Theory of Gases** (edited by Akira Taniguti)

This volume contains 10 papers which cover the genesis of the kinetic theory of gases by J. C. Maxwell and R. Clausius and also the Loschmidt-Boltzmann and Zermelo-Boltzmann controversies.
VI. Statistical Mechanics (edited by Tetsuo Tsuji)

This volume consists of three papers of L. Boltzmann which laid the foundation of the statistical mechanics. They are: that of 1866 in which Boltzmann attempts to give a mechanical illustration of the second law of thermodynamics; that of 1872 in which the H-theorem is proved and the Boltzmann equation is derived; and that of 1877 which gives a statistical interpretation of the meaning of entropy.

VII. Radioactivity (edited by Sigeko Nisio)

This volume traces the development of the study of radioactive substance and its rays from its inauguration by H. Becquerel to the establishment of the concept of isotope. The paper in which W. C. Röntgen announces his discovery of X-rays is also included. The number of papers amounts to 15.

VIII. Electron (edited by Tetu Hirosige)

Six papers which established the existence of electron are collected. They cover the experimental studies by J. J. Thomson on the cathode rays, experimental and theoretical studies by P. Zeeman, H. A. Lorentz, and J. Larmor on the Zeeman effect, and the first determination of the elementary electric charge by J. J. Thomson.

IX. Models of Atom (edited by Tetu Hirosige)

This volume contains 10 papers which describe the experimental investigation related to the structure of atom by P. Lenard, J. J. Thomson, H. Geiger and E. Marsden, J. A. Crowther, E. Rutherford, and H. G. J. Mosely as well as a theoretical consideration of the atomic number by A. van den Broek.

X. Theory of Atomic Structure (edited by Eri Yagi)

This volume is devoted to theoretical inquiries into the structure of atom. The eight papers collected are those by J. J. Thomson, J. Perrin, H. Nagaoka, W. Ritz, A. E. Haas, and M. Planck and the celebrated 1913 paper of N. Bohr. A synopsis of the Nagaoka-Schott controversy on the stability of electron ring is also added.

XI. Electron Theory of Metal (edited by Sigeko Nisio)

This volume contains three historic papers by P. Drude (1900), H. A. Lorentz (1905), and N. Bohr (1911), which laid the foundation of and revealed the limitation of the classical electron theory of metal. Thanks to this volume
Bohr's thesis, which though very well known hardly anyone in Japan would have actually read, has been made easily accessible for students.

XII. Magnetism (edited by Tetsuo Tsuji)

Three great papers by P. Curie (1895), P. Langevin (1905), and P. Weiss (1907) which constitute the foundation of classical theory of magnetism are translated for this volume.

To each volume is added a brief historical account by the editor of that volume. Of these twelve volumes, vols. II, IV, VIII, X, and XI have already been published during 1969. The remaining volumes are scheduled to be published before the beginning of 1971.
Synopsis of the History of Chinese Science

Shigeru Nakayama*

Continuous tradition

The Chinese maintained their cultural isolation fairly well in most of the time. Surrounded by high mountains, wastelands and oceans, China is effectively isolated from the rest of the world, save some nearby satellite countries like Korea and Japan, to such an extent that this geographical insulation is largely compelling a long-standing continuous culture — from the second millennium B. C. down to almost present time — without substantial interruption. This continuity is one of the most outstanding characteristics of Chinese science, forming a striking contrast with the main current of Western scientific development, in which the center of its activity shifted from Babylonia to classical Greece, to the Hellenistic world, to India, to the Islamic world, to the Renaissance Europe and so on. Thus, we find in Chinese science a process of gradual development on a single established traditional line rather than discrete scientific revolutions which were by no means the product of one race or continent and often resulted from active intellectual confrontation with an equally high culture. For science in which intellectual feedback is indispensable, cultural isolation meant a lack of challenge. Perhaps, the chief merit is that this long continuous tradition provides to the modern researchers — seismologists, astronomers and other scientists — invaluable records of uninterrupted observations of eclipses and other portents extending over two millennia.

We can go back to oracle bones of the Yin dynasty (mid fifteenth century B. C.) in tracing the evidences of some basic characteristics of the Chinese science. During the Chou period — the Warring States period (the fourth to third centuries B. C.) in particular — philosophic thought appeared — not only the humanism and naturalism of Confucius and Lao Tzu, but also systems like Mo Ti's which attempted to integrate axioms of optics, mechanics and semantics. The Mohist

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Canon contains many germinal ideas of science, but lacked a favourable climate to grow and flourish.

It was, however, in the Han period (second century B. C. to second century A. D.) that the most of basic features of the Chinese science were formulated and became a prototype of what the traditional Chinese would consider to be authentic curricula and topics of research. Most of major scientific classics, which later dominated as standard texts on the Chinese scientific community, were composed by the end of the Han time. The *Chiu-chang suan-shu* (Nine chapters on the mathematical art) in arithmetics and mathematics; *Chou-pi suan-ching* (The arithmetical classics of the gnomon and the circular paths of heaven) in astronomy and cosmology; *Huang-ti nei-ching su-wen* (Pure questions, inner classic of the Yellow Emperor) and *Shang-han lun* (On febric diseases) in pathological and clinical medicine; and *Shen-nung pen-ts’ao ching* (Pharmacopoeia of the Heavenly Husbandman) in pharmacology are most important. Within the government, calendar-making, astrology, medicine and mathematics were given a solid institutional basis.

**Institution**

The well-founded Chinese bureaucracy is also contributing to the continuity of Chinese scientific tradition. Even at the time of a cultural ebb, the institutions for astronomical sciences and medicine existed at least as a matter of routine and maintained a minimum of tradition as an official duty of civil servants.

Bureaucratic control of learning in China, however, restricted not only the influx of new ideas, but also stunted intellectual growth by absorbing all the talents into administration, a single key to social recognition.

In the earlier days, the higher posts in bureaucracy were monopolized by powerful aristocratic families but from the Sui period (seventh century), civil service examination system was founded and operated effectively until the time of its abolition in the early twentieth century.

Persons of almost any economic or social background were eligible to take the civil service examinations. Hence, the Chinese system of recruiting talents seems at first glance to be more effective and advanced than even modern Western educational institution. However, its main purpose was not the training of
prospective research scholars, but the selection of a privileged few among the
many career-seekers. It was, moreover, completely controlled by the government,
which was thus able to stereotype all form of learning.

This examination was chiefly a written one, placing a premium on memory
and comprehension of classical literature, which was regarded as an indisputable
canon beyond criticism. Thus, students did not develop powers of logical analysis.
Nor were they stimulated to think along independent lines, as were students in
medieval European universities who prepared for oral examinations involving
individual disputation.

In comparing the relative status of various scientific institutions within the
government, we find, in the T'ang governmental regulations, that astronomical
posts were most prominent since both astrology and calendar-making were
important imperial functions, while the government had little respect for the status
of mathematics. Most of the graduates of the mathematical institute were trained
merely to be minor functionaries such as tax-collectors and surveyors.

In the bureaucratic society, generalist administrators were always more highly
esteemed than technical specialists. Bureaucrat-scholars outside these technical
offices could overrule scientists on matters of crucial scientific problems like
calendrical reform. On the other hands, as long as one remains a technical
specialist, the promotion of his status were limited and hence, there were always
shortage of well-qualified personels in technical offices.

Major scientific principles

There are two principles that throughout history were applied to Chinese
astrology, as well as to medicine, alchemy and many other aspects of the Chinese
intellectual framework: the yin-yang and "five-elements" (or five-forces) principles.
The yin-yang principle explained all phenomena in the universe in terms of a
fundamental dichotomy which corresponded to that of heaven and earth, male
and female, and so on. The five-elements principle was used to systematize the
relations of things by placing them in the constellation and temporal cycles of
natural agents — wood, fire, earth, metal, and water.

The question of when these principles came into being still challenges the
historians, but it would be safe to assume that they were formulated and put together about the fourth century B. C. and became established during the second and first centuries B. C.

From their inception these principles were closely related to astronomical and cosmologic thinking. Unlike the Aristotelian counterpart in the West, however, these were prematurely divorced from phenomenal world and had been reputed with the status of the highest metaphysical principles. They were too abstract to apply successfully to explain phenomenal world. As the empirical observational data became abundantly available, calendar-calculators found no longer possible to deal with the framework of their rigid dogmatic notions, and finally gave up totally the attempt to express celestial courses in terms of cosmic cycles. Principles were often placed only in the preface or opening part of their treatises as an ornamental discourse of a cosmologic nature so as to convey an impression of profundity. In medical field, although these principles were employed in classifications and explanations of physiology and pathology, clinical medicine has been less influenced and developed more or less independently on its own empirical merit.

Hence, any new discovery would not create crisis in refusing or replacing these principles with some other new ones. For instance, the report of the discovery of the sixth planet, Uranus, during the Ch'ing period never caused serious panic to violate “five-elements” theory which was considered to manifest “five” planets in the sky. In fact, they were not so committed to phenomenal world as to refutable on account of the contradiction with measurable world.

Together with the lack of rigorous proof, this is the reason why the Chinese science is generally characterized as empirical and practical. The Chinese scientists often showed scepticism or apathy towards the existence of rigorous underlying regularity in Nature and a thorough-going “general conceptual scheme” at all as existed in mechanistic philosophy in Europe. For instance, inaccuracies in predicting solar eclipses were attributed not necessarily to imperfections in scientific technique, but often to the inherent indeterminacy of celestial motions, or to their susceptibility to at least some control by human desires operating through ritual and magic.
While basic cosmological ideas appeared in fragmentary form in the oldest classics, the first Chinese treatise on "scientific" cosmology — scientific in the sense of being mathematical and entirely divorced from mythopoeic tradition — is found in the Chou-pi suan-ching, which is based on gnomon observations and the conception of heaven and earth as parallel. This was opposed by another important cosmological school, who, obviously closely associated with the development of armillary sphere observations; recognized the sphericity of the sky. Cosmologists' debate had been most active during the Han and Six Dynasties period. The controversy, however, dies out and astronomers lost their interest in it, occupying solely with routine observations and calendrical calculations. In the T'ang time the Chinese astronomers' attitude towards this particular speculative pursuit was that "Our business is exclusively calendrical calculations and observations in order to provide the people with the correct time. Whether flat or spherical cosmology is no concern of the astronomers!" While occasional cosmological debates were found among the Sung philosophers, scientific cosmology was long set aside and forgotten in professional astronomy circles until the time of the Jesuits' impact.

Thus, we can scarcely find in the later development of Chinese astronomy any tendency towards a conceptual scheme or a mechanistic model. The approach of the Chinese official astronomers was to represent numerically the course of the celestial bodies, without depending upon a geometrical model. Their final aim was to reduce observations as accurately as possible to algebraic relations. Unlike Ptolemaic astronomy, Chinese astronomy showed no concern for the calculation of radius vectors or dimensions of the universe; for all purposes of measurement, heaven was treated two-dimensionally. On account of considerable developments of eclipse-predicting techniques, it is surprising to note that even the sphericity of the earth was not explicitly recognized among astronomers until the time of Jesuits' visit.

**Calendar-making**

Perhaps the most striking thing about recastings of the civil calendar in China was their frequency. The Chinese calendar was revised more than fifty times in two
thousand years and another fifty unsuccessful proposals are recorded. Two major reasons to explain such repeated efforts are apparent.

1. Among the Chinese the idea prevailed that a ruler received his mandate from heaven. In the early period, therefore, after important changes of individual reign and always after important changes of dynasty, the new emperor was prompted to reform the official calendar in order to confirm the establishment of a new order which a new mandate implied; a new mandate meant a new disposition of celestial influences. This notion was responsible for the subsequent course of development of Chinese calendrical science. Calendrical science enjoyed government sponsorship throughout its history, and had more prestige than other branches of science; it was China's most genuine contribution to exact science. The history of Chinese astronomy is, for the most part, the history of calendar-calculation.

2. In the course of time, however, the political importance of calendar reform dwindled. The restriction of calendar reform to change of dynasty was not strictly observed by the fifth century A.D. In the T'ang dynasty, the motive for calendar revision became simply to correct disagreements of the calendar with observed celestial phenomena. Hence, reforms were carried out whenever a small error was found. This accounts for the frequent revisions in later phases.

The Chinese calendar, from early times until its replacement by the Gregorian calendar in the twentieth century, was a typical luni-solar calendar. The Chinese calendar-calculators were not satisfied with providing a conventional calendar, in which the course of the sun and moon were reconciled, merely for civil use; but they also tried to include the anomalistic motions of the sun and moon. On the other hand a sharp separation of scientific astronomy and civil calendar-making did not take place in China. Its scope was confined to the composition of a luni-solar ephemeris which stood or fell on the accuracy with which it could predict eclipses — the best way to check the validity of any luni-solar calendar. Analysis of planetary motions was rather ancillary to the main problems of Chinese astronomy.

Mathematics

Throughout Chinese history the main importance of mathematics lies in
relation to the calendar-making. Unless associated with the work of calendrical composition — a basic concern of the Empire —, mathematics itself could never achieve a high status in the hierarchical disposition of learning within bureaucracy, nor enjoy a high intellectual position in the appreciation of scholars-literati class, but tended to be a mere technique, having few philosophical overtones, for counting-clerks to employ in such minor works of petty-officials as mensuration, tax-collection and book-keeping. Thus, the Chinese way of presentation of mathematical topics were definitely inclined to application, or at least application-conscious. One did not find abstract and systematic proofs reflecting a concern with mathematics for its own sake.

The Chinese had relied on mechanical tools in calculating. Early use of counting-rods and later of abacus certainly facilitated the advancement of numerical solution of algebraic problems. They could handle negative number by using different colours — black and red — of rods as early as the Han time. Numerical quadratic and cubic equations, simultaneous linear equations, the value of circular constant: these were better calculated in the third century China than the rest of the world by use of counting-rod.

The absence of calculation by writing up until the introduction of Western mathematics at the Jesuit time was, however, doubtlessly extreme limitation. Adherence to mechanical tools allowed calculations to vanish without trace, leaving no record of the intermediate stages by which the answer was reached. In ordinary mathematical texts, only problems and final answers were given. This custom could be related to the absence of the idea of rigorous proof in Chinese mathematics.

Geometrical problems were treated only algebraically. Graphical treatment was not found (at least explicitly) in the texts. There is not much to say about trigonometry in the ancient Chinese mathematics, save the Pythagorean relationship, was enthusiastically studied as early as in the *Chou-pi suan-ching* in connection with astronomical measurements for which the gnomon was required.

**Medicine and the related fields**

Chinese physiological theory as it appeared in the *Huang-ti nei-ching* is deeply involved in their characteristic *Naturphilosophie*. According to its pathology, the
cause of disease lies chiefly in malfunctions of the circulation of ch'i (a sort of pneumonia). External ch'is of wind, coldness, hotness, humidity and so on come into internal organs and cause disease. Internally, disturbance of ch'i circulation through the five tsang (the heart, liver, spleen, lungs and kidneys) and the six fu (the gall-bladder, stomach, large intestine, small intestine, bladder and san chiao, an imaginary organ) is also the cause of disease. It is considered that the dissection of the human body was being practiced to some extent before the Han time. Nei-ching states that “After death the body may be dissected and observations made as to the size of the organs, the capacity of intestines, the length of the arteries, the condition of the blood, and the amount of pneumonia.”

In Chinese physiological and pathological theories, the brain never played a significant role. Brain was considered to be merely a part of marrows of bones. Mental activity was attributed to the function of the heart, the prince of body. The symptom of disease can be readily detected by the pulse and hence sphygmic treatment was considered to be as important in diagnosis as gross symptoms and case history. Climatic and topographical factors were well taken into consideration, and the body was always treated as a whole. Most unique therapy in Chinese medicine is perhaps acupuncture.

Natural history, chemistry and alchemy existed only in subordinate relation to therapeutics. Presumably, Chinese word pen-ts'ao, equivalent to pharmacology, originally meant the study of a medicine for longevity or immortality, and later it was applied for the study of materia medica in general, which includes mineral drugs. Unlike the Western counterpart, the Chinese did not try to extract essence out of herbs.

The primary goal of the Chinese alchemy was to find out a recipe to make individuals immortal rather than to prepare authentic noble metal from base metal. The transformation of metals into gold was tried, but in the earliest phase of alchemy, goldmaking was associated with immortality only indirectly; eating off utensil made from the gold was supposed to lengthen life to the point that the thaumaturgic and ritual prerequisites to actual immortality could all be satisfied.
Foreign influence

There are two major foreign cultures which influenced pre-modern China: Indian and Islamic.

Buddhist pilgrims returning from study in India brought home elements of Indian culture and science. We do not find, however, much substantive influence of Buddhism on science despite the fact that Buddhism was the most important and influential imported culture throughout Chinese history.

Buddhism emphasized contemplation and rejected the phenomenal world as illusory. Physical theory and scientific institutions were outside its scope. The Buddhists were concerned with science only as one of the peripheral features of the Indian cultural tradition, transmitted as an incidental part of theology.

With a single exception of I-Hsing, there is no Buddhist monk found in the list of notable astronomers. “Nine upholders” calendar, translated into Chinese during the T’ang period, includes unmistakable influences of Hellenistic astronomy, but it remained outside of the main current of the development of Chinese calendrical science.

Influences were rather found in peripheral topics such as horoscopic art and the custom of week cycle. More Indian influence can be found in medical field. Many Indian medical treatises were translated into Chinese during the T’ang period.

Islamic culture must have imposed some influence upon the science and technology during the Sung period, but no systematic importation was ever attempted. During the Yuan dynasty, when the Mongols established a huge Empire, uniting East and West, cultural traits of both were often mingled. The Chinese gunpowder and papermaking techniques were allegedly transmitted to the West at this period, while Islamic astronomers were employed in the Chinese court and competed with the Chinese native school in precision of predicting eclipses. However, these competing schools never merged, and thus the Chinese astronomy remained virtually independent of any substantial foreign influence.

Such a piecemeal infusion of knowledge could never have led to a conceptual revolution in Chinese science.
The coming of Jesuits

It was the Jesuit time in the seventeenth to mid-eighteenth century when indigenous Chinese science was for the first time substantially affected by another culture. The Jesuits in China generally took a flexible, and sometimes conciliatory, attitude toward the existing social order. They attempted to gain converts indirectly by impressing the elite classes with their superior knowledge of astronomy, then exploiting these groups to bring about wholesale conversions.

Their efforts were rewarded in making some influential converts among the Chinese high officials and succeeded in changing the official calendar from one based on traditional Chinese technique to that based on Tychonic astronomy.

The first generation of the Jesuits, Matteo Ricci, had started to bring in the Ptolemaic kind of astronomy while the second generation of missionaries introduced the core of Tychonic astronomy and pre-Cartesian mathematics. Copernicus was quoted as the discoverer of eleventh sphere and as a skillful observer; no mention of the heliocentric theory appeared until the middle part of the eighteenth century.

While geometrical treatment, the values of parameters and contents were all Tychonian, the way of presenting and arranging calendrical treatises, and also the very purpose of Chinese astronomy, i.e. the composition of a luni-solar calendar and the most precise prediction of eclipses, remained unchanged. Thus, the celebrated slogan of HsüKuang-ch'i, an eminent Chinese collaborator of Ricci, that “Let us melt their (Western) materials and cast them into the mold of the traditional calendar” was firmly carried out.

Introduction of modern science

The second wave of the Western influence came to China during the nineteenth century along with the invasion of Western powers and the missionary activities of Protestants, such as Alexander Wylie. Although the Protestant missionaries followed a similar educational policy toward China as their Jesuit precursors, the Protestants approached commoners rather than high officials, with the latest knowledge in modern science. For instance, John Herschel’s *Outline of Astronomy* was translated by Wylie with the assistance of a native Chinese in 1859. At
the same time, some less conservative Chinese officials started to introduce the core of Western military technology and its concomitant science by establishing translation bureaus and schools for modern engineering and military arts in order to compete with the military superiority of Western powers. They emphasized the role of surveying and mathematics, in which Western superiority was already recognized by the Jesuit activities. On the other hand, modern medicine was entirely left in the hands of foreign missionaries.

The abolishment of time-honoured civil service examination system in the early part of the twentieth century created a great stimulus among Chinese youth to seek a new nourishment of learning in the Western science, and many of them rushed to study abroad. “Science” became a new fashion of thought to replace old tradition of learning since the second decade of this century.

The present generation of senior scientists were mostly trained in the United States and Europe (some in Japan). Under the regime of the Chinese People’s Republic, many students of the generation born in 1930’s were sent to the Soviet Union during the time of Russo-Chinese collaboration in 1954-59. Because of the split in ideology, China started pure imbreeding of scientist since 1960.
Sur l’Irregularité de Numérotation des Figures
dans les Lettres de Dettonville

Kokiti Hara*

§1.** Chose bien curieuse, les célèbres Lettres de A. Dettonville contenant quelques-unes de ses inventions de géométrie, que Pascal publia en 1659, comportent une grande irrégularité dans la numérotation des figures annexes. Ce désordre, déjà frappant dans l’édition des Grands Ecrivains reproduisant en facsimilé les figures originales, demeure pourtant jusqu’ici inexpliqué. En fait, nous croyons que la plupart de ces anomalies s’expliquent assez facilement, pourvu qu’on admette que normalement, un auteur numérottera ses figures au fur et à mesure qu’il s’en servira. Tout écart de la suite naturelle des nombres étant ainsi imputable à quelque perturbation survenue au cours de la rédaction, nous aurons la chance, en examinant les numéros des figures, d’assister à la genèse d’un ouvrage. Certes, l’explication que nous essayerons pour chaque numérotation irrégulière ne sera enfin que conjecturale. Mais, du fait que le processus de rédaction de ce chef-d’oeuvre pascalien est encore tout couvert de mystère, cette sorte d’examen et de conjecture ne sera pas sans intérêt.

§2. Il faut d’abord rappeler la structure générale de l’ouvrage(1). Il a la forme d’une collection de plusieurs fascicules, à savoir:

[I] LETTRE DE A. DETTONVILLE A MONSIEVR DE CARCAVY, EN LVY
ENVOYANT Vne Methode generale pour trouver les Centres de grauité de toutes sortes de grandeurs. Etc. A PARIS, M.DC LVIII(2)

Lettre de Monsieur de Carcavy. A Monsieur Dettonville. (sans pagination.)

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[1] Lettre de Monsieur Dettonville, à Monsieur De Carcavy, cy-deuant Conseiller du Roy en son grand Conseil. (pp.1-26.)


[3] Propriétéz des Sommes Simples, Triangulaires, & Pyramidales. (pp.1-8.)

[4.1] Traité des Sinvs du quart de Cercle. (pp.1-9.)

[4.2] Traité des Arcs de Cercle. (pp.9-24.)

[5] Petit Traité des Solides Circulaires. (pp.1-7.)


[II] LETTRE DE A. DETTONVILLE A MONSIEVR HVGGVENS DE ZVLICHEM, EN LVI ENVOYANT La Dimension des Lignes de toutes sortes de Roulettes, lesquelles il monstre estre égales à des Lignes Elliptiques. A PARIS, M.DC.LIX. (pp.1-7.)

[III] LETTRE DE A. DETTONVILLE A MONSIEVR DE SLVZE CHANOINE de la Cathedrale du Liege, EN LVI ENVOYANT La Dimension & le Centre de grautie de l’Escalier. La Dimension & le Centre de grautie des Triangles Cylindriques. La Dimension d’un Solide formé par le moyen d’une Spirale autour d’un Cone. A PARIS, M.DC.LVIII. (pp.1-8.)


L’ouvrage se termine par quatre planches comportant des figures géométriques:

P1.I. Fig.1-12.
P1.II. Fig.13-20. Et, au coin droit inférieur du feuillet, Fig.36, 37, 40, rassemblées dans un petit cadre(3).
P1.III. Fig.21, 22, 23, 24 & 27(4), 25, 26, 28 & 29(5), 30–35.
P1.IV. Fig.38, 39, 41–45.

Tous les numéros entre crochets ont été introduits par nous-même. La Lettre I (traités 1-6), donnant la solution des problèmes proposés à condition des prix, constitue la partie principale de l’ouvrage. Désormais, nous l’appellerons souvent
"le Traité de la cycloïde". Les Lettres II-IV sont les annexes. On voit que non seulement celles-ci, mais encore les pièces composant la Lettre I ont chacune leur pagination propre. Seul le Traité des arcs de cercle y fait exception, et c'est pourquoi, l'ayant combiné avec le traité immédiatement précédent, nous les avons numérotés 4.1 et 4.2. Encore aurait-on préféré la pagination continue pour toutes les pièces de la Lettre I, qui forment un tout cohérent, alors que les trois Lettres annexes sont indépendantes l'une de l'autre pour leurs sujets.

§3. Voici encore quelques observations préliminaires, à propos de la rédaction de cet ouvrage.

Vers la fin du Traité I de la Lettre I, nous trouvons des phrases importantes, révélant l'attitude générale de l'auteur:

"Je n'ay donc plus qu'à vous prier d'excuser les défauts que vous verrez ici, ce que j'espère de votre bonté, & de la connaissance que vous ayez du peu de loisir que j'ay de m'appliquer à ces sortes d'études, ce qui fait que je vous envoie ce Discours à mesure que je l'escris; De sorte qu'il me pourra bien arriver de repeter plus d'une fois les mêmes choses, & peut-être que je l'ay déjà fait, ne me souvenir pas assez de ce que j'ay veufois envoyes."  

Il faut que cet envoi successif des manuscrits ait beaucoup diminué la chance du remaniement du texte. La chance en devait être encore diminuée par le fait que l'auteur n'a pas examiné personnellement les épreuves, comme on le verra tout à l'heure. Et tout cela doit s'entendre aussi des trois autres Lettres annexées.

En ce qui concerne la date de rédaction de l'ouvrage, on sait déjà que les problèmes de Pascal au sujet de la cycloïde avaient été proposés en deux fois: d'abord par une circulaire de juin 1658, ensuite dans la circulaire intitulée Histoire de la roulette ou Historia trochoïdis, et portant la date du 10 octobre de la même année. Or, vers le milieu du Traité I déjà cité, Pascal mentionne le deuxième groupe de problèmes en ces termes exprès: "ceux que je proposai au commencement d'Octobre dans l'Histoire de la Roulette". Il faut donc que dans la Lettre I (nous mettons encore les Lettres annexes à part), la partie commençant par ces mots ait été rédigée après le 10 octobre 1658, et nous pourrons probablement en dire autant de tout le Traité de la cycloïde.
D'autre part, l'indication "1658" faisant suite au titre de la Lettre I est corroborée par la phrase suivante de la lettre de Mylon à Pascal du 27 décembre de la même année:

"... ayant lu aujourd'hui les imprimés nouveaux de la Roulette avec M. de Carcavi et votre démonstration *more veterum* de l'Égalité de la spirale et d'une parabole..."(9)

Par conséquent, nous devons nous mettre en garde contre la lettre de Carcavy à Pascal, mise en tête du Traité de la cycloïde. Cette lettre, datée du 10 décembre 1658, commence ainsi:

"Personne n'ayant donné les Solutions des Problèmes que vous auez proposez depuis si long-temps, vous ne pouuez plus refuser de paroistre pour les donner vous mesme, comme la promesse que vous en auez faite vous y engage; je sçay que ce vous sera de la peine d'écrire tant de Solutions & de Methodes, mais aussi c'est toute celle que vous y aurez: car pour l'Impression ie ne songe pas à vous la proposer, i'ay des personnes qui en auront soin"(10).

Qui croirait qu'exhorte par cette lettre à commencer la rédaction du Traité de la cycloïde, Pascal l'eût terminée assez tôt pour que son texte, et aussi celui de la Lettre IV fussent achevés d'imprimer avant le 27 décembre? Qui le croirait, dans le cas même de l'envoi successif des manuscrits? Carcavy n'aurait donc fait, dans sa lettre citée, que feindre d'espérer ce qui avait déjà été réalisé. Réalisé jusqu'à quel point? Peut-être complètement, comme nous le montrerons ci-après (§ 11). Cependant, en supposant même que Pascal avait commencé à rédiger le Traité de la cycloïde immédiatement après la publication de *l'Histoire de la roulette*, la promptitude de son travail rédactionnel ne laisse pas de nous confondre, étant donné la longueur et la complication du Traité, et l'intervention des autres affaires qui occupèrent Pascal à la même époque(11). L'examen des numéros des figures de l'ouvrage aura justement pour effet de résoudre cette difficulté.

La date de rédaction des trois Lettres annexes doit être considérée à part (§§ 10–13). Qu'il nous suffise ici d'indiquer que toutes les *Lettres de Dettonville* furent achevées d'imprimer à la fin de janvier 1659, fait attesté par une phrase de la lettre de Mylon à Huygens du 31 janvier de cette année:
Numérotation des Figures dans les Lettres de Dettonville

“L'impression des propositions de nostre excellent Anonime est tantost acheuee(12).”

§4. Pour entrer dans notre sujet propre, voici les figures géométriques dans chaque traité de la Lettre I. Nous les énumérons d’après l’ordre de leur première apparition, et en soulignant la numérotation irrégulière.

| Traité 1. | Fig. 1, 2, 3, 4, 5, 6, 10, 8, 9, 28, 11. |
| Traité 2. | Fig. 12, 13, 7. |
| Traité 3. | Fig. 14, 15. |
| Traité 4.1. | Fig. 26, 32, 16. |
| Traité 4.2. | Fig. 17, 18, 25, 24, 30, (27), 28, 29. |
| Traité t. | Fig. 28, 31. |
| Traité 6. | Fig. 19, 20, 23, 22, 21. |

En suivant l’ordre naturel des numéros, on obtient les Fig. 1-21, et les figures restantes, 22-32, s’y trouvent interposées de façon régressive, et dans des positions fort variées. Seule la Fig.7 est descendue contrairement du Traité 1 au sein du Traité 2.

Si nous en venons au fond de la Lettre, nous pouvons remarquer le fait suivant. On n’ignore pas que cette Lettre se divise substantiellement en deux parties: (1) les Traités 1-3 exposant une théorie générale applicable à un “triligne” quelconque, et (2) les Traités 4-6 qui visent à résoudre les problèmes particuliers publiquement proposés, en limitant l’objet de l’étude au triligne cycloïdal. Dès lors, la table précédente montre que le désordre de numérotation est surtout grand dans la partie 2, ce qui aurait d’ailleurs été naturel chez Pascal, qui écrit comme suit dans l’Histoire de la roulette:

“... une occasion imprévue m’ayant fait penser à la Géométrie que j’avais quittée il y avait long-temps, je me formay des méthodes pour la dimension et les centres de gravité des solides, des surfaces planes et courbes, et des lignes courbes, ausquelles il me sembla que peu de choses pourroient échapper: et pour en faire l’essay sur un sujet des plus difficiles, je me proposay ce qui restoit à connoistre de la nature de cette ligne [de la cycloïde](13).”
Ainsi la méthode générale qui devait être exposée dans la partie I de la Lettre avait été préalablement établie, tandis que son application à la cycloïde n’était point simple. Par suite du remaniement plus fréquent du texte, la numérotation des figures devait subir plus de désordre dans la partie 2 de la Lettre.

Mais ce sont encore là des observations purement quantitatives. Au point de vue de la qualité, le désordre moins grand dans la partie 1 est plus significatif.

§ 5. Examinons maintenant l’une après l’autre les irrégularités de numérotation des figures dans la Lettre I.

Les irrégularités dans le Traité I sont le manque de la Fig. 7, l'inversion de la Fig. 10 par rapport aux Fig. 8, 9, et l'intercalation de la Fig. 28. Nous commençons par le dernier cas; la suite de la discussion fera comprendre pourquoi.

Que prouve en effet la présence, dans ce traité\(^{(14)}\), de l'allusion à la Fig. 28 (celle de droite, comme il a été indiqué dans la note 5), sinon que l'auteur disposait alors au moins des Fig. 1-28, et qu’à plus forte raison, la Fig. 21, la dernière en position, avait déjà été dessinée? Cela n’est pas étonnant. Car Pascal n’avait proposé ses problèmes au public qu’après les avoir résolus lui-même, et de nombreuses figures devaient être dessinées pour cette résolution. Mais le fait qu’elles comportaient déjà la numérotation atteste une rédaction assez soigneuse de la solution, puisque les numéros des figures ne sont nécessaires qu’à la référence explicite au cours de la démonstration. En outre, il faut qu’à la suite des Fig. 1-21,
l'intercalation compliquée des Fig.22-28 ait été aussi effectuée. Cela suppose une révision très attentive du manuscrit. Plutôt donc qu'un simple mémorandum à usage personnel, Pascal devait posséder alors une ébauche déjà soigneusement préparée en vue de la publication. Avec cette préparation, le Traité de la cycloïde aurait pu être achevé en moins de trois mois. Ainsi disparaît notre embarras devant la promptitude apparente de cet auteur.

Avec cette préparation cependant, Pascal fut encore obligé d'écrire: "le peu de loisir que j'ai de m'appliquer à ces sortes d'études" (§3). Le désordre qui a subsisté dans la numérotation des figures s'expliquerait par là. Nous voulons penser que Pascal a pris le parti, lors de la rédaction définitive de la Lettre I, — et aussi des trois autres, — de ne pas se soucier de réparer ce désordre, travail non essentiel enfin, et qui risquait d'entraîner de nouvelles confusions du texte.

Par rapport à cette allusion à la Fig.28 dans le Traité 1, qui nous conduit déjà à une conjecture importante sur la genèse de l'ouvrage en question, toutes les autres irrégularités relevées dans le §4 ne présentent enfin qu'un intérêt théorique. Mais certaines d'entre elles touchent au fondement même de la Méthode exposée, et il en est déjà ainsi des deux autres irrégularités dans le Traité 1. Etant donné, toutefois, que la Fig.7 fut renvoyée par l'auteur au Traité 2, nous l'envisagerons dans le paragraphe suivant. Il ne reste ici qu'à rendre compte de la position de la Fig.10.

Cette figure concerne la définition pascalienne des "ordonnées" et des "sinus"(15). (1) L' "axe"AB du triligne ABC ayant été divisé "en un nombre indéfini de parties égales" par les points D, élevons, de chaque point de division,
les perpendiculaires sur AB. Faisons de même à l'égard de la "base" AC du
triligne, prenant E comme points de division. Pascal appelle ces perpendiculaires
les ordonnées à l'axe et à la base respectivement. D'autre part, la courbe BC ayant
été divisée de la même manière par les points L, abaissons, de chaque point de
division, les perpendiculaires sur AB, AC. Pascal appelle ces perpendiculaires les
sinus sur l'axe et sur la base respectivement. (2) Par "somme des ordonnées à l'axe
(à la base)" , Pascal entend la somme de ces ordonnées multipliées chacune par
l'intervalle DD (EE); également, par "somme des sinus sur l'axe (sur la base)" , il
entend la somme de ces sinus multipliés chacun par l'intervalle LL. Mais en fait,
tout cela a été déjà expliqué au moyen des Fig.2-4(16). (3) Ce qu'il y a de
vraiment nouveau dans la Fig.10 est la supposition de la triple égalité: DD = EE =
LL(17). Même avec cette dernière condition, il n'y a pas encore de nécessité pour
que la Fig.10 précède les Fig.8, 9, lesquelles ne supposent point cette condition-là.
Nous comprenons pourtant l'empressement de l'auteur à exposer ces trois
conditions dans leur ensemble. Car ce sont là précisément les règles de Pascal,
fermement maintenues non seulement dans le Traité de la cycloïde, mais dans
toutes les Lettres de Dettonville à l'exception de la Lettre IV, procédant par une
tout autre méthode.

On connaît déjà bien l'importance historique de la condition 2, qui approcha
la géométrie des indivisibles de Cavalieri du calcul intégral. Quoiqu'elle ne fût
point l'originalité de Pascal(18), personne ne l'exposa plus nettement que lui à
cette époque-là. Par contre, les conditions 1 et 3 sont certainement discutables.
Tout revient ici à la division des lignes en parties égales, car une fois aboli ce mode
de division, on voit disparaître du même coup, et la distincion entre les
ordonnées et les sinus, et la triple égalité: DD = EE = LL. Et nous rencontrerons
bientôt ce problème dans le Traité 2.

§6. La "méthode générale" exposée dans le Traité 1" pour les centres de
gravité de toutes sortes de lignes, de-surfaces et de solides" a ceci de particulier
qu'elle utilise dès le début la notion de "somme triangulaire"(19), et c'est cette
notion, d'origine arithmétique, qui imposa à l'auteur de diviser la "balance" (axe
de coordonnées) en parties égales. On a donc affaire aux "ordonnées". Mais le
Traité 2 a pour but d'établir, à un niveau plus élevé, des méthodes générales pour
les problèmes contenant ceux qui étaient proposés comme autant de cas
particuliers, et Pascal crut nécessaire, à l'égard du type de problèmes d'octobre, de considérer les "sinus" du triligne proposé. Croyance curieuse, mais quelques pages du Traité 2 nous apprennent qu'elle a encore pour cause l'adhésion de l'auteur à la notion de somme triangulaire. Il se heurta ainsi à une difficulté de principe, qu'est la discordance entre les ordonnées et les sinus. La Fig.7 est employée pour écarter cette difficulté.

En fait, la notion de somme triangulaire est le substitut pascalien de celle de moment statique. Mais cette notion-ci, qui ne suppose nullement la division de la balance en parties égales, est plus générale et plus commode que celle-là. Le mode particulier de division imposa une condition superflue à la méthode de Pascal.

Mais il ne s'ensuit point que la difficulté que nous venons de signaler ait pris cet auteur au dépourvu. Son argument pour écarter cette difficulté consistait, premièrement, à considérer des "poids" nuls, et deuxièmement, à négliger certains intervalles infinitésimaux de la balance. Mais ces deux points, par lesquels Pascal sut sauvegarder son principe de la division en parties égales, se trouvent en fait déjà exprimés dans le Traité 1. L'argument pascalien qui nous intéresse actuellement ne fait que répéter le premier point, et que préciser le deuxième. Nous ne nous étonnerions donc pas si la Fig.7 avait été préparée dans le Traité 1 selon l'ordre naturel de numérotation. Mais nous comprenons aussi la raison pour laquelle elle fut enfin réservée au Traité 2. C'est que la difficulté ci-dessus mentionnée ne devient explicite que dans ce traité-ci, donnant plus de détermination quantitative à l'argument du Traité 1. Il nous semble au reste que cette conjecture jette une lumière inattendue sur le 7e "Avertissement" du Traité 1, qui suit de tout près l'utilisation de la Fig.6:

"Fay voulu donner ces exemples de l'usage de cette Methode,... l'en donnerois bien ici d'autres exemples plus considerables, mais on les verra dans la suite,... (24)".

Supposez que les "exemples plus considérables" avaient compris le cas de la surface de révolution. Ne peut-on pas bien penser que la Fig.7 avait été dessinée normalement en cet endroit?

§ 7. Comme tout est normal dans le Traité 3, nous procédons à l'examen des numéros des figures dans la seconds moitié du Traité de la cycloïde.
L’irregularité dans le Traité 4.1 est l’intercalation des Fig.26 et 32.

La Fig.26 est surtout célèbre pour l’influence qu’elle exerça sur Leibniz. Mais, comme l’a judicieusement remarqué A. Koyré, le triangle infinitésimal contenu dans cette figure, et qui apparaît “caractéristique” aux yeux de Leibniz, ne l’était toutefois pas pour Pascal lui-même. De fait, la proposition à laquelle se rapporte cette figure est modestement qualifiée de “lemme”. Quelque important qu’en ait été le contenu (intégration définie des sinus), Pascal sut la démontrer trop aisément, nous semble-t-il, pour sentir le besoin de la consigner expressément dans son manuscrit. En considération toutefois de ses lecteurs, il aurait fini par y intercaler ce Lemme et la Fig.26. Et nous rencontrerons dans la suite plusieurs autres exemples qui semblent s’expliquer par le même motif.

La Fig.32, utilisée pour les Prop.1-4, est digne d’attention sous plus d’un rapport. Vu que ces propositions se déduisent immédiatement du Lemme qui vient d’être mentionné, déjà la même sorte de motif chez l’auteur pourrait être alléguée à l’égard de cette Fig.32, la dernière, selon la numérotation, de tout le Traité de la cycloïde. Mais voici pourquoi nous préférons cette fois une autre explication. D’une part, le mot “Fig.32” n’est point imprimé dans le corps du texte; il y est seulement inscrit à la plume. Remarquez d’autre part, dans la reproduction de la figure originale, la mention explicite de l’usage: “Pour le Traité des Sinus”, seule exception dans toutes les Lettres de Dettonville. Ces deux

![Diagramme de la Fig.32](image)
particularités nous inclinent vers la conjecture suivante. Pascal aurait dès l’abord énoncé et démontré les Prop.1-4 au moyen d’une figure, qui est la Fig.32 actuelle; mais, peut-être faute d’avoir indiqué explicitement la figure utilisée, il ne l’aurait pas numérotée\(^{(28)}\); de plus, il aurait oublié même de l’envoyer à Carcavy conjointement à son manuscrit; s’apercevant de cet oubli après la composition du Traité de la cycloïde, il aurait envoyé la figure avec l’indication de son usage; mais le manuscrit du Traité 4.1 ayant déjà été imprimé, l’allusion à cette figure dans le texte aurait été ajoutée à la plume par l’éditeur, si ce n’est encore plus tardivement par un lecteur, sans que cette retouche pût atteindre tous les exemplaires.

Nous avons affirmé que les Prop.1-4 se démontrent aisément. Mais cela ne veut pas dire que la démonstration en a été simple pour l’auteur. Le fait est que les conditions relatives à la Fig.10 (§5) sont ici en cause, et il nous semble opportun de formuler, une fois pour toutes, notre vue sur ces règles pascaliennes. Les Prop.1-4 énoncent que pour un nombre naturel quelconque \(m\), et quand l’arc DD devient infiniment petit, on a \(\Sigma D I^m \cdot DD = AB(\Sigma D I^{m-1} \cdot II)\). D’après la condition 2 de la Fig.10, le membre gauche de l’égalité donne la “somme des sinus élevés à la \(m\)\(^e\) puissance”, mais le second facteur du membre droit ne peut pas être appelé la “somme des sinus élevés à la \((m-1)\)\(^e\) puissance”, à cause de l’intervention des \(\Pi\). Aussi, pour démontrer cette proposition générale d’après son propre vocabulaire, Pascal prend AQ égal à AO, le divise en parties égales par les points H, et, en élevant les perpendiculaires HL, énonce l’égalité: \(\Sigma D I^m \cdot DD = AB(\Sigma H L^{m-1} \cdot HH)\)\(^{(29)}\), dont le second facteur du membre droit est bien la “somme des ordonnées élevées à la \((m-1)\)\(^e\) puissance”, et l’auteur finit par affirmer l’égalité de ces deux facteurs envisagés. Ici se révèlent à la fois l’avantage et le désavantage de la condition 2. Celle-ci, une fois déclarée au début, dispense désormais de mentionner des éléments infinitésimaux comme DD et HH. C’était là sans doute un avantage non méprisable, au temps où le symbolisme mathématique était encore très peu développé, et surtout chez notre auteur, qui n’était point innovateur en cette matière. En revanche, son raisonnement se compliquait forcément à cause de cette condition 2, étroitement liée à la condition 1\(^{(30)}\). Nous avons déjà signalé la futilité de la division des lignes en parties égales, impliquée dans les conditions 1 et 3 (§6). Reste à apprécier ces trois conditions dans leur ensemble. L’examen serré des Lettres de Dettonville nous fait conclure
que par cette triple règle, Pascal a perdu plus qu'il en a gagné, et la Fig.32 semble en témoigner d'une façon typique.

§8. Dans le Traité 4.2, toutes les figures, autres que les deux premières, montrent l'irrégularité de numérotation.

La Fig.25 se rapporte au "Lemme [1]"(31), énonçant un cas particulier de l'équation générale des moments. Nous pouvons donc l'expliquer, de même que la Fig.26, par le souci de l'auteur pour la compréhension de l'ouvrage. Ou plutôt, vu les deux nombres consécutifs 25 et 26, on pensera plus naturellement que, motivé par l'intercalation de ce Lemme 1, l'auteur s'est avisé de mettre, au début du Traité 4.1, le lemme énonçant l'intégration des sinus.

La Fig.24 se rapporte au "Corollaire" du Lemme 1. Mais le contenu de ce lemme-ci étant presque évident, nous pouvons bien supposer que l'auteur a d'abord énoncé la proposition actuellement qualifiée de corollaire, et qu'il a ensuite mis le Lemme 1 avant elle.

La Fig.30, introduite relativement au "Lemme 2"(32), et utilisée ensuite pour les Prop.3-6, contient quelques éléments nouveaux par rapport à la Fig.24. Mais ces éléments n'apparaissent, en fait, ni dans les prémises, ni dans les conclusions de ces propositions; ils n'interviennent qu'à titre auxiliaire dans leurs démonstrations, possibles d'ailleurs au moyen de connaissances rudimentaires. La Fig.24 aurait donc déjà permis à l'auteur d'établir toutes ces propositions. Cette conjecture est encore appuyée par le fait que seul le cas particulier où le point Q coïncide avec B était l'objet du calcul effectif(33).

La Fig.27 n'est rien d'autre que la Fig.24, et il semble très difficile
d'expliquer ce double numéro, constituant l'unique exception de toutes les

Lettres de Dettonville. Mais nous voulons d'abord signaler deux faits. Appelons, conformément à l'état du texte, cette seule et même figure tantôt la Fig.24, tantôt la Fig.27, selon l'usage qu'en fait l'auteur. La Fig.24 concerne le Corollaire du Lemme 1, comme nous l'avons déjà vu. La Fig.27 concerne la suite depuis la Prop.7 jusqu'au "Corollaire" de la Prop.10. Or le premier fait à signaler est la complication inutile de ces deux Fig.24 et 27; à des degrés près, elles renferment, l'une et l'autre, des éléments qui ne sont point mentionnés dans l'argument de l'auteur. Mais le deuxième fait est beaucoup plus important: dans la Fig.24, les points R, S sont tout arbitrairement situés sur l'arc de cercle CQ, tandis que dans la Fig.27, le même arc CQ doit être divisé par les points D, D, en parties égales. En retraçant exactement les figures sous ces deux aspects, on obtient les Fig.24' et 27'. Le premier fait signalé devra être imputé à la confusion chez l'auteur lui-même (inscription des lettres inutiles T, V, dans la Fig.27). Mais il n'en est pas ainsi du deuxième fait. Faire coïncider R, S, et D, D, en superposant les Fig.24' et 27' l'une sur l'autre, c'est, sinon une erreur inexcusable, du moins une représentation ambigüe; c'est certainement déroger au scrupule constant que nous reconnaissons chez notre auteur. Au lieu donc de lui attribuer la "Fig.24 & 27" actuelle, nous aimons mieux supposer que lui-même a dessiné des figures distinctes, plus ou moins proches de nos Fig.24' et 27'. A cala s'ajoute un troisième fait. C'est que dans les quatre planches mises à la fin des Lettres de Dettonville, les figures sont généralement disposées de façon serrée, et qu'il en est surtout ainsi dans la Planche III contenant la "Fig.24 & 27" qui nous occupe. Aussi
sommes-nous tenté d’imaginer la circonstance suivante. L’éditeur de l’ouvrage, embarrassé par de nombreuses figures à présenter en peu d’espace, n’aurait-il pas été trompé par la ressemblance apparente des deux figures envoyées par l’auteur(34)? Ne se serait-il pas avisé d’économiser l’espace en les réunissant? Nous ne prétendons pas par là que l’éditeur ait ainsi procédé sans permission de l’auteur. Mais il ne serait pas impossible qu’un auteur interrogé, surtout en termes imprécis, sur son manuscrit une fois expédié, fît une réponse inattentive. Cela n’est certes pas facile à admettre, quelque pressé qu’ait été alors notre auteur de rédiger la suite de son ouvrage. Mais il nous semble encore plus difficile de lui attribuer le double numéro, qui compromet, répétons-nous, un souci théorique constamment gardé par notre auteur.

Les Fig.28 et 29 (la première ayant déjà été discutée dans le §5, ) sont utilisées dans le reste du Traité 4.2, à savoir les Prop.11-16. Nous pouvons cependant penser que ces figures, non plus que ces propositions, n’étaient pas indispensables pour le compte de l’auteur. En effet, le calcul pouvait s’arrêter, ici encore, au cas du quart de cercle (35); de plus, le segment parabolique tracé dans la Fig.28 n’était pas encore en question. Dans cette situation, la Fig.17 au début du Traité 4.2 aurait pu tenir lieu de ces Fig. 28 et 29. Le segment parabolique mentionné n’entre en jeu que dans le traité qui suit.

§9. Le Traité 5 peut être regardé toutefois comme une collection de lemmes, ou plutôt comme un long scolie. Nous ne serions pas étonné que tout ce traité
eût été inséré postérieurement. Il n’est pas besoin, par conséquent, de discuter les irrégularités de numérotation qu’il comporte. Encore ne voyons-nous pas bien pourquoi la Fig.30 fut antéposée dans le Traité 4.2. Voici une conjecture précaire. Dans le Traité 5, l’auteur considère presque toujours les "ordonnées" du demi-cercle, mais parle des "sinus" dans l’article 5, en faisant allusion au Traité 4.1 (36). Allusion suffisante, au point de vue théorique. Mais la connexion intérieure est surtout étroite entre les Traités 4.1 et 4.2, comme l’indique la pagination continue qu’ils comportent. Dès le moment, donc, où l’auteur eut ainsi mentionné le Traité 4.1 avant de se servir de la Fig.31, il aurait pensé tout naturellement au Traité 4.2, et se serait avisé d’y suppléer le Lemme 2 et la Fig.30.

Les irrégularités de numérotation dans le Traité 6 s’expliquent facilement. La Fig.23 se rapporte au "Lemme 1" de ce traité, et la Fig.22, au "Lemme 2". Toutes les deux étant ainsi pour des propositions auxiliaires, leur position ne présenterait qu’un problème technique pour faciliter la compréhension de l’ouvrage. Nous comprenons en outre l’ordre renversé 23 et 22, du fait que le Lemme 2 est le plus important de ces deux, et que le Lemme 1 ne fait que le préparer. De toute façon, les numéros des figures montrent que le Traité de la cycloïde une fois composé, l’auteur retoucha en premier lieu ce dernier traité.

§10. Notre principe d’explication des numéros irréguliers des figures est le même à l’égard des trois autres Lettres, qui utilisent les figures suivantes:

Lettre II. Fig. 41, 42, 43.
Lettre III. Fig.24(27), 44, 30, 1, 45.
Lettre IV. Fig.33, 35, 34, 36, 37, 38, 40, 39(37).

Vu les contenus respectifs des trois Lettres, l’ordre adopté pour leur présentation paraît raisonnable. Car la Lettre II porte encore, quoiqu’en dehors du concours, sur la cycloïde (généralisée relativement à la position du pôle), tandis que la Lettre IV, procédant selon la "manière des Anciens", se distingue par là de tout le reste des Lettres de Dettonville. Pour juger cependant d’après les numéros des figures, ces trois Lettres semblent avoir été composées dans l’ordre de IV, II, et III. Aussi les considérons-nous dans cet ordre. Mais, pour nous justifier pleinement dans cette marche, il nous sera indispensable d’expliquer ce fait: les
Lettres IV et III furent publiées en 1958 ainsi que la Lettre I, alors que la Lettre II le fut en 1659 (§2), contradiction apparente avec l’ordre présumé de composition.

§11. La Lettre IV débute par un préambule portant la date du “10 Décembre 1958” (38), indication précieuse, permettant de situer assez exactement le moment où le Traité de la cycloïde fut achevé. Car tout porte à croire que, de la Lettre I à la Lettre IV, l’auteur a travaillé sans relâche ou presque, soit qu’il ait composé celle-ci en partant normalement du préambule, soit qu’il en ait d’abord rédigé le corps principal.

Il n’est pas difficile de comprendre les irrégularités des numéros des figures dans cette Lettre IV. Les Fig.35, 34 se rapportent respectivement aux articles 2, 3 du paragraphe intitulé “Propriétés du cercle”, et l’art.3 se démontre au moyen de l’art.2. Mais l’art.2 est une proposition assez simple pour dispenser l’auteur de son énonciation expresse avec une figure. Il en est de même, en substance, de la relation entre les Fig.40 et 39. La Fig.39 est probablement la plus importante de toutes celles annexées à la Lettre IV, tandis que la Fig.40 ne fait que retenir, de la précédente Fig. 38, la partie représentant la parabole, et qu’y ajouter un peu d’éléments nouveaux. Certes, l’argument auquel se rapporte la Fig. 40 (“Propriétés de la parabole”) n’est pas simple. Mais un géomètre aussi doué que Pascal n’aurait pas eu besoin de le consigner pour son propre compte. En somme, toutes les irrégularités dans la présente Lettre peuvent s’expliquer par le souci de l’auteur pour la compréhension de l’ouvrage.

§12. Le préambule de la Lettre II nous apprend la raison pour laquelle elle fut dédiée à Huygens.

“Comme l’ay sçeu que Monsieur de Carcauy vous deuoit enuoyer mes solutions des problèmes que j’auois proposés touchant la Roulette, ie l’ay prié d’y joindre la dimension des courbes de toutes sortes de Roulettes, que ie luy ay donnée pour vous l’adresser, parce qu’il m’a dit que vous auez tesmoigné d’auoir quelque enuie de la voir(39).”

C’est par une lettre du 16 janvier 1659, paraît-il, que le savant hollandais communiqua une telle envie à Carcavy. Car, quoique la lettre soit aujourd’hui
perdue, le "sommaire" qu'en rédigea l'expéditeur contient un passage qui se traduit ainsi :

"Je crois les choses de Pascal encore plus subtiles: je voudrais bien qu'elles fussent publiées, et je le prie." (40)

Rien d'étonnant à ce que Carcavy ait trouvé, dans la lettre en question, le désir de Huygens pour la Lettre II, vu que les phrases qui viennent d'être citées font suite, dans ce sommaire, à l'allusion à la rectification de la cycloïde réalisée par Wren. Aussi ne serait-il pas erroné de croire que le préambule de la Lettre II a été rédigé peu après que Carcavy avait reçu cette lettre de Huygens, donc vers la fin de janvier 1659 (4). Mais nous savons déjà qu'appliquant la même datation au corps de la Lettre II, on tombe en contradiction avec la numérotation des figures.

Il faut rappeler ici le fait que la publication de la Lettre II avait déjà été promise par Pascal dans ses circulaires d'octobre 1658: *Histoire de la roulette et Historia trochoidis* (42). Il y a plus; un "Avertissement" du Traité 1.6 renferme, relativement à la rectification de la cycloïde réalisée par Wren, cette affirmation de l'auteur: "j'ai moi-même démontré la même chose dans un Traité à part"(43). Quel était ce "Traité à part"? S'il s'agit là du corps de la Lettre II, il faut que celle-ci ait été rédigée au cours de 1658, avant que le Traité de la cycloïde fût achevé. Mais le "Traité à part" peut désigner aussi une rédaction antérieure de la Lettre II, que Pascal avait déjà communiquée au début de septembre de 1658 à plusieurs géomètres français et étrangers (44). Dans l'un et l'autre cas, la numérotation des figures n'en peut pas être identique à celle des figures de la Lettre II. Mais, puisque c'était là un traité indépendant, il a bien pu comporter sa propre numérotation, commençant par l'unité. D'où est venue alors la numérotation actuelle de la Lettre II? Il convient de répondre à cette question après l'examen de la Lettre III.

§13. La présence des Fig.24(27), 30, 1 dans la Lettre III ne présente aucune difficulté; les figures anciennes du Traité de la cycloïde y sont employées de nouveau selon les nécessités. Mais la présence des Fig. 44 et 45 ne fait-elle pas penser que cette Lettre III a été rédigée après la Lettre II, quoique la page de titre de celle-là porte "1658" comme date de publication? Il semble que le début
du préambule de la Lettre III en rende compte.

"Le n'ay pas voulu, Pascal y écrit à Sluse, qu'on vous enuoyast mes problèmes de la Roulette, sans que vous en receussiez en mesme temps d'autres que ie vous ay promis depuis vn si long-temps, touchant la dimension & le centre de grauité de l'Escalier & des Triangles Cylindriques. l'y ay joint aussi la resolution que i'ay faite d'vn problesme, où il s'agit de la dimension d'vn solide formé par vne Spirale au tour d'vn Cone. C'est vne solution que j'ayme, parce que i'y suis arrivi par le moyen de vos lignes en Perle, & que tout ce qui vous regarde m'est cher."

A cause de ces circonstances particulières, Pascal aurait certainement désiré voir la Lettre III parvenir vite à son ami belge. Par contre, son contact direct avec Huygens était récent; il commença lorsque Pascal reçut un exemplaire de l'Horologium, que Huygens lui avait adressé par l'intermédiaire de Du Cast, vers octobre-novembre de 1658. Pascal en remercia le savant hollandais le 6 janvier 1659.

Nous sommes ainsi conduit à la conjecture suivante. Ayant achevé le Traité de la cycloïde au début d'octobre de 1658, Pascal aurait procédé sans interruption à la rédaction de la Lettre IV. Il aurait voulu ensuite achever la Lettre III. Et pourtant, il aurait rédigé en fait le corps principal de la Lettre II, peut-être en le croyant plus facile à cause de l'existence d'une rédaction antérieure de ce traité. Il aurait eu l'intention de dédier aussi cette Lettre à un ami. Mais, avant de réaliser cette intention, il aurait rédigé la Lettre III pour tenir sa promesse à Sluse. C'est vers ce moment qu'ayant reçu un exemplaire de l'Horologium, Pascal aurait éprouvé un respect sincère à l'égard de son auteur. Peut-être aurait-il pensé alors à lui dédier la Lettre II. En tout cas, Carcavy lui communiquant le désir de Huygens de voir justement ce traité, l'attitude de Pascal aurait été décidée; il aurait ajouté un préambule au texte déjà prêt. Ainsi s'expliquent, croyons-nous, tous les numéros des figures dans les trois Lettres annexes de ce chef d'oeuvre de Pascal.

Notes

(1) Nous nous référons à la réimpression photographique de la première édition: Lettres de A. Dettonville [Balaise Pascal], 1659, Dawsons of Pall Mall,
Numérotation des Figures dans les Lettres de Dettonville


(2) En fait, le titre énumère, après cette “Méthode générale”, les Traités 2, 4.1, 4.2, 5, 6. Voir le facsimilé donné dans les Oeuvres de B. Pascal, éd des Grands Écrivains de la France (par abrév. GE), t.VIII, p.326. A propos du Traité 6, il n’y est fait mention que des problèmes “proposés publiquement au mois de Juin 1658”. Mais l’absence de la mention des problèmes d’octobre, probablement due à une inadvertance de Carcavy, est commune au titre général de l’ouvrage, et il en est de même de l’omission du Traité 3. Peut-être le titre général aurait-il été dressé d’après le titre de cette Lettre I.

(3) La Planche II est reproduite en facsimilé dans la Rev. d’hist. d. sci., t.XV, n°3-4, p.340, ou L’OEuv. sci. d. Pascal, p.188.


(5) Voyez le facsimilé inséré dans le §5. On croirait naturellement que les Fig.28 et 29 fussent disposées de gauche à droite. En fait, l’auteur entend presque toujours par “Fig.28” la figure de droite (1ère éd., 1, p.25, 4.2, pp.20, 23; GE, t.VIII, p.382, t.IX, pp.96, 101), tandis que celle de gauche est indiquée littéralement comme “un autre quart de cercle pareil ABC” (4.2, p.20; t.IX, p.97). C’est seulement dans le Traité 5 que la figure de droite est numérotée 29 (5, pp.1, 5; t.IX, pp.105, 113). Il semble ainsi plus conforme à l’intention de l’auteur de regarder la figure de droite comme numérotée 28, et celle de gauche comme numérotée 29. Aussi adopterons-nous cette interprétation dans la suite. La numérotation inversé aurait eu pour cause la volonté de l’éditeur de fourrer des figures dans un espace limité de la Planche III, et l’auteur aurait été forcé, dans le Traité 5, de respecter la réalité de la planche. Nous montrerons ci-après (§8) que
le double numéro mentionné dans la note 4 serait résulté de la même attitude fâcheuse de l'éditeur.

(6) 1ère éd., 1, p.24; GE, t.VIII, p.380.
(8) 1ère éd., 1, p.18; GE, t.VIII, p.368. Il n'est pas vraisemblable, d'après le contexte, que cette mention ait été intercalée postérieurement.


(10) 1ère page de l'édition princeps. Voir aussi GE, t.VIII, p.331.


(13) GE, t.VIII, p.201.
(14) 1ère éd., 1, p.25; GE, t.VIII, p.382.
(15) 1ère éd., 1, p.p.18-19; GE, t.VIII, pp.368-369.
(17) L'auteur signale bien qu' “il ne faut pas craindre l'incommensurabilité” des lignes considérées (1, p.18; t.VIII, p. 369).


(19) 1ère éd., 1, p.1; GE, t.VIII, p.335.
(20) 1ère éd., 2, pp.7-13; GE, t.IX, pp.14-26.
(21) 1ère éd., 2, pp.22-23; GE, t.IX, pp.40-42.
(22) 1ère éd., 1, pp.3-4, 10; GE, t.VIII, pp.339-340, 351-352.

(23) En effet, la Fig.1 montre très nettement la surface de l’ “onglet” construit sur un triligne donné, surface qu'on sait tenir lieu de celle d'un quart du
solide de révolution engendré par le triligne de base. De plus, dans le 4e "Avertissement" du Traité 1, l'auteur explique comment s'applique sa méthode du centre de gravité à la surface de cet onglet, en ne considérant toutefois que les "ordonnées" du triligne (1ère éd., 1, p.9 ; GE, t.VIII, pp.344, 349).


(26) 1ère éd., 4.1, p.1 ; GE, t.IX, p.60.

(27) 1ère éd., 4.1, p.2. L'écriture ne saurait être celle de Pascal. Il paraît d'ailleurs que cette inscription n'existe pas dans l'exemplaire utilisé par les éditeurs de GE (t.IX, p.63).

(28) La Fig.45 fournit un autre exemple du cas où la figure considérée n'est point numérotée dans l'argument de l'auteur (1ère éd., III, p.6 ; GE, t.IX, p.146).

(29) Le point Q n'est pas indiqué dans la figure originale. Il coïncide avec le point H le plus éloigné du centre A.

(30) Sur la liaison de ces deux conditions, cf. surtout l'affirmation générale suivante de l'auteur: "quand on parle de la somme d'une multitude indefinie de lignes, on a toujours égard à une certaine droite, par les portions égales & indefinies de laquelle elles soient multipliees. Mais quand on n'exprime point cette droitte (par les portions égales de laquelle on entend qu'elles soient multipliees) il faut sousentendre que c'est celle des diuisions de laquelle elles sont nées" (1ère éd., 1, p.11 ; GE, t.VIII, 353).

(31) Ce lemma n'est pas numéroté dans l'original (1ère éd., 4.2, p.12 ; GE, t.IX, p.82). Nous lui prêtons ce numéro, du fait qu'un autre lemma va le suivre. Cf. la note suivante.

(32) L'original porte "Lemma III" (1ère éd., 4.2, p.14 ; GE, t.IX, p.85). Les éditeurs de GE expliquent de leur manière cette numérotation (ibid., n.1), mais il est plus simple de modifier le numéro.

(33) Par suite de la condition du concours, ajoutée par Pascal dans une circulaire de juillet 1658. Voir GE, t.VIII, p.19.

(34) Une méprise semblable de la part de l'éditeur se constate déjà dans la
Fig. 5, qui dessine les "sinus" de la cycloïde, alors qu'il y s'agit réellement de ses "ordonnées" (1ère éd., 1, p. 14; GE, t. VIII, p. 359).

(35) Pour la raison déjà signalée dans la note 33.

(36) 1ère éd., 5, p. 4; GE, t. IX, pp. 110-111.

(37) Nous ne recourons plus au soulignement, pour raison de la numérotation particulière dans la Lettre III. Voir le § 13. On notera d'ailleurs sans peine tous les autres numéros irréguliers.

(38) 1ère éd., IV, p. 2; GE, t. VIII, p. 257.


(40) Huygens, t. II, p. 316.

(41) Le "sommaire" actuellement considéré apprend aussi que Huygens a communiqué à Carcavy son propre travail sur la quadrature de la surface de révolution du 2e degré, travail dont Pascal fait encore l'éloge dans le préambule de la Lettre II.

(42) GE, t. VIII, pp. 208, 222.

(43) 1ère éd., 6, p. 6; GE, t. IX, p. 126.

(44) 1ère éd., II, p. 6; GE, t. IX, p. 199. Le texte de cette rédaction demeure toutefois totalement inconnu.


(47) GE, t. IX, pp. 162-163.
X-rays and Atomic Structure
at the Early Stage of the Old Quantum Theory

Sigeko Nisio

I. Introduction

In the previous paper we concluded that at the earlier stage of the development of the theory of atomic constitution, namely in the decade of 1910's, chemical considerations have played the cardinal part. Atomic models capable of explaining the chemical properties, which were proposed and developed by Kossel, Lewis, and others, exhibited right properties as far as the general features were concerned. Especially they gave the correct number of electrons in each ring- or shell-grouping of electrons inside atom.

In the course of this investigation, we were compelled to trace the development of the theories of characteristic X-ray spectra and to assess properly the role played by these theories in the history of the theory of atomic constitution in the same period (in the 1910's). We indeed found that although works on X-ray spectra made some contributions which could not be looked over, they were subordinate to the chemical approach.

The following is the result that we have obtained concerning the part played by the studies of characteristic X-rays in elucidating the structure of the Bohr atom.

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**) After the author finished the preliminary note on this subject, she received the issue of ISIS in which J. L. Heilbron published a paper on the theory of X-rays and the structure of atom in the same period as hers. Making use of, in addition to the published papers, unpublished various documents, he traced in detail the process of the rise and fall of the ring atom model mainly in consequence of the evolution of the theories of X-ray spectra. The author, however, considers the cause of the decline of ring atom model not only in X-ray spectra but also in the chemical properties of atom. It should be noticed that the relation of the X-ray investigations with the chemical considerations is outside the scope of Heilbron's paper.
In 1912 J.J. Thomson proposed a tentative theory about the origin of characteristic X-rays that they were emitted when a tightly bounded intra-atomic electron group was removed from the atom and a subsequent recombination took place\(^4\). He wrote: "Suppose that besides the corpuscles which can be dislodged from the atom by the expenditure of an amount of energy measured by a few volts, there are in the atom other systems which require for their dissociation a much greater amount of energy, say an amount measured by thousands of volts. When the residue and the corpuscle unite and reproduce the original system, this amount of energy will have to be radiated away. The maximum energy would be in the neighbourhood of wave-length \(10^{-8}\) cm., i.e. the radiation would be Röntgen radiation". In the second paper of 1913\(^5\) Bohr, as did J.J. Thomson, supposed that "the characteristic Röntgen radiation is sent out during the settling down of the system if electrons in inner rings are removed by some agency, e.g. by impact of cathode particles". On this supposition he could show that the characteristic X-radiation of the K-type might be ascribed to the innermost ring. He determined the minimum velocity of cathode ray particles which was necessary to produce the K-radiation by calculating the energy required to remove one of the electrons from the innermost ring. For the approximation, he took, instead of the actual atom, a simple model system consisting of a bound electron rotating in a circular orbit around the positive nucleus of charge \(Ne\) The value calculated for the minimum velocity approximately agreed with Whiddington's experimental results on the velocity of cathode ray particles capable of producing the K X-rays.

In the same year Moseley\(^6\) examined systematically the characteristic X-rays emitted by various elements, and obtained the well known empirical formula bearing his name which could be expressed as the relation between the frequency of X-rays and the atomic number:

\[
\begin{align*}
V_{K\alpha} &= R(Z - 1)^2 \left( \frac{1}{12} - \frac{1}{22} \right), \\
V_{L\alpha} &= R(Z - 7.4)^2 \left( \frac{1}{22} - \frac{1}{32} \right)
\end{align*}
\]  

\( (1) \)
where $R$ is the Rydberg constant, $Z$ the atomic number, $V_{K\alpha}(V_{L\alpha})$ the frequency of $K\alpha_1(L\alpha)$-line.

Moseley's work was received as strongly supporting the hypothesis that the atomic number of an element was the number of its nuclear charge, that is, the number of electrons in its atom. For example, Rutherford stated that "the strongest and most convincing evidence in support of this hypothesis will be found in a paper by Moseley in *The Philosophical Magazine* of this month." Similar opinion was also expressed by an author of a review article on the recent works on the structure of atom. This hypothesis, together with the experimental fact that the position of a certain line, say $K\alpha_1$, for the successive elements of the periodic system revealed a regular progressive shift, seemed to support strongly the view that the origin of the characteristic X-ray spectra must be located in the immediate neighbourhood of the nucleus, that is, in the innermost part of the atom. At the same time it became to be believed that the study of the characteristic X-ray spectra would shed much light on the structure of the innermost part of atom.

Furthermore, the similarity of Moseley's formula with Bohr's formula for the hydrogen spectrum motivated attempts to find an explanation of X-ray spectra on the basis of Bohr's atomic theory. In fact, Moseley himself proposed an explanation of his formula that the $K\alpha_1$-line was emitted during a transitation of the innermost ring as a whole between two states in which the angular momentum of each electron was equal to $\frac{\hbar}{2\pi}$ and $\frac{\hbar}{2\pi}$ respectively. Assuming that the fact that rather $Z-1$, not $Z$ itself, appeared in his formula was a reflection of the effect of other electrons in the same ring, Moseley deduced that the number of electrons in the ring concerned was equal to 4. However, Moseley realized that, as Bohr later pointed out, his own theory seemed to face a difficulty in the energy consideration. In fact, the approximate agreement of Bohr's calculation with Whiddington's measurements of the energy required to produce the characteristic X-rays was generally interpreted as strongly indicating that the X-ray spectrum was due to a displacement of a single electron, not the whole ring. If the ring as a whole was to displace, the energy should be several times larger.

*) About Moseley's work, J. L. Heilbron wrote a separate article. For the detailed account of Moseley's papers, the reader is referred to Heilbron's article.
III. Kossel and Sommerfeld.

Now, of succeeding investigations on the characteristic X-rays, two important theories must be considered in detail: one was presented by Kossel in 1914 and the other by Sommerfeld in 1916. These authors both seem to have paid particular attention to Bohr's theory of atomic constitution since immediately after its emergence.

In 1914, Kossel made investigation on the absorption of X-rays passing through material layers and obtained the absorption curves in the two regions of wavelength, the regions of K-series and L-series, for several metals. Each curve showed a sharp absorption near the wave length of the second line of each series (that is, the $\text{K}_\beta$-line or $\text{L}_\beta$-line), which could be related to production of the emission lines. Kossel asserted that this result could be explained by means of Bohr's model, and proposed a new theory of the mechanism of emission of characteristic X-ray which was different from Moseley's one. Whereas Moseley thought that the transition from one state to another of the innermost electron ring as a whole gave rise to the emission of characteristic X-rays, Kossel adopted Bohr's view on the origin of the characteristic X-rays as described earlier, according to which they were emitted by the displacement of an electron from the innermost ring.

Kossel thought, according to Bohr, that a vacant place might be produced in the innermost ring (K-ring) by the removal of an electron from it when the atom was bombarded by cathode rays or X-rays. He argued that if the vacancy was to be filled up by an electron from outside the atom, the frequency of radiation emitted by this electron would not be smaller than the minimum frequency (Grenzfrequenz) of the incident X-rays that was necessary to produce the characteristic line. But no X-ray line of a frequency not smaller than the Grenzfrequenz had been observed. The strongest line, the $\text{K}_\alpha$-line, had a wave length that was longer than that corresponding to the Grenzfrequenz and was the longest among observed K-lines. The $\text{K}_\alpha$-line, therefore, had to be assumed to correspond to the emission of the smallest amount of energy. Thus Kossel supposed that the $\text{K}_\alpha$-line would be produced when a vacant place in the K-ring was filled up by one of the electrons in the second ring, not by an electron from
outside the atom. The state of atom, in which the second ring has given away an
electron, is identical to the state in which an electron on the second ring (L-ring)
has been removed from it directly by X-rays. Thus the same would then occur,
Kossel thought, to the L-ring with a vacant place as that occurred to K-ring. The
wave length of the L-line would correspond to the smallest energy emitted as
L-lines. In analogy with the K_\alpha-line, the L_\alpha-line would be ascribed to the
transition of an electron from the third ring to the second. Successive transitions
of electrons between two adjacent rings would thus give rise to emission of K_\alpha-,
L_\alpha-, M_\alpha-, ..., lines, if M_\alpha-, ..., lines were to exist at all.

Guided by such a consideration, Kossel was led to the conclusion that the
sum of the energies emitted by each transition of electron never exceeds the
energy required to remove one electron from the innermost ring to outside the
atom, which corresponds to the K-Grenzfrequenz. In fact, making use of his
absorption curves and Moseley's formula, Kossel confirmed the relation:

\[ \nu_{K_\alpha} = \nu_{K_\alpha} + \nu_{L_\alpha} \]  \hspace{1cm} (2)

where \( \nu_{K_\alpha} \) is the K-Grenzfrequenz corresponding to the energy required to
remove an electron from the first ring (K-ring), \( \nu_{K_\alpha} \) is the frequency of K_\alpha-line
corresponding to the jumping of an electron from the second (L-ring) to the first
ring, and \( \nu_{L_\alpha} \) is the L-Grenzfrequenz.

Kossel then assumed that in addition to the transitions mentioned above,
another type of transition that the vacant place in the K-ring would be filled up
by an electron from an outer ring, say from the third, or forth, or ... ring might
occur. On this assumption it was expected that the energy emitted by the
transition from the third to the first ring would be equal to the sum of the energy
corresponding to the L_\alpha-radiation (transition from the third to the second ring)
and that corresponding to the K_\alpha-radiation (transition from the second to the first
ring): \( h\nu_{K_\alpha} + h\nu_{L_\alpha} \). By making use of Moseley's measurement of frequencies for
the K_\alpha and K_\beta and calculating the value of the frequency of L_\alpha with the help of
Moseley's formula, Kossel could show that for the elements from Ca to Zn this
sum of the energies actually corresponded to the K_\beta-radiation:
\[ \nu_{K\beta} = \nu_{K\alpha} + \nu_{L\alpha} \quad (3) \]

He expected the relation \( \nu_{K\delta} = \nu_{K\alpha} + \nu_{L\beta} \) for the combination of the first, second, and forth rings, and also similar relations for other combinations of rings.\(^*\).

Kossel's results above was generally received as adding support to Bohr’s ring atom and as indicating that the combination principle established in optical region is valid also for the characteristic X-rays. For example, an author of a review article on the works on the structure of atom expressed similar opinion.\(^{15}\) Kossel himself noted in his 1914 paper\(^{16}\) that his relation (2) was closely analogous to Rydberg-Schuster rule, according to which, in optical spectral series, the difference of the frequency of the limiting line of P series and that of S or D series is equal to the frequency of the first line of P series. Referring to Kossel's view, Bohr in his 1915 paper “On the Quantum Theory of Radiation and the Structure of the Atom”\(^{17}\) said: “It will be seen that these relations correspond exactly to the ordinary principle of combination of spectral lines”. And referring to some experimental results obtained recently agreeing with Kossel’s results, Bohr said” ...\(^*\)

\(^*\) Heilbron asserted that “of course Kossel’s main thesis was not these relations but the assertion that ionization precedes X-ray emission. It is here, I believe, that we may well see the influence of Lenard. During all of Kossel’s time at Heidelberg, Lenard was on crusade for his view that spectral emission occurs only during the recapture of an electron by an ionized atom, ... . By the time of Kossel's gradation, Lenard’s view that ionization must precede emission had been widely accepted; J. J. Thomson, among others, held it, and there is good evidence that Bohr did so as well. But in 1913 Bohr specifically abandoned this mechanism, at least for optical spectra; and it seems very likely that part of the reason he and Moseley were unable to hit on Kossel's simple X-ray model was that they had already firmly rejected it for the optical case. Kossel, however, ... , did not part with it so readily”. This Heilbron’s argument, however, is not acceptable to the present author. She as well believes with Heilbron that Lenard’s view had been widely accepted in the period considered. As was mentioned above, J. J. Thomson held it. But in 1913 Bohr too did so, at least for X-ray spectra. This is sufficiently evidenced by Bohr’s calculation of the minimum velocity of cathode ray particles required to produce K-radiation. The author does not think that Kossel sticked on the view that ionization must precede emission. As will be shown later Kossel expected that in addition to the process of ionization another process would be responsible to the emission of X-rays, in which an electron was excited from the innermost ring to an possible outer ring.
it will be seen that even if Kossel’s considerations will need modification in order to account in detail for the high frequency spectra, they seem to offer a basis for a further development’”.

However, the validity of Kossel’s relations (3) and others between frequencies of X-ray lines, and consequently that of the combination principle of X-ray lines, became soon to be suspected by Sommerfeld. In his 1916 paper that will be considered later, Sommerfeld pointed out that his expressions of X-ray spectral terms showed that no frequency of L-lines was derived from such combination of the frequencies of K-lines as the Kossel relation, \( \nu_{L\alpha} = \nu_{K\beta} - \nu_{K\alpha} \), and to support this view cited the experimental result obtained recently by Siegbahn for M-series the first two lines of which were not derived from any combination of L-lines. Being aware of Sommerfeld’s suspicion above, Kossel noted that the value of \( K \) Grentzfreqenz expected from the assumption that the combination principle would hold for the X-ray lines, which had been believed to be equal to the first term of Moseley’s formula for \( K\alpha \)-line, was somewhat greater than the observed value of Wagner. Nevertheless Kossel believed that these defects might be cleared away by minor additional assumptions and in 1916 said that his view was increasingly supported by various experimental data. Predicting the duplicity of the second ring (L-ring) from the presence of two absorption edges in the absorption curve in the L-region obtained by Wagner, Kossel expected from his relation (2) that \( K\alpha \)-line too should split into doublet with components of frequencies,

\[
\nu_{K\alpha} = \nu_{K_{g}} - \nu_{L_{g}} \\
\nu_{K\alpha'} = \nu_{K_{g}} - \nu_{L_{g}},
\]

respectively, and showed that the value of the splitting \( \Delta \nu \) of \( K\alpha \)-doublets observed for Pt and Au by Malmer was equal to the value of the distance between two absorption edges in the L-region:


\[ \Delta \nu = \nu_{K\alpha} - \nu_{K\alpha} = \nu_{L\gamma} - \nu_{L\gamma} \]  

He could further show that this was approximately equal to \( \nu_{L\beta} - \nu_{L\alpha} \). Finding in the existence of doublets an analogy between the X-ray and the optical spectra, Kossel was convinced of the validity of his relations. But Kossel’s effort in clearing away the defects of his relations was soon to turn out fruitless. Final solution of these difficulties was not obtained before the development of the correspondence principle and of the further studies in the fine structure of X-ray spectral lines.

In 1915 Sommerfeld presented the celebrated paper\(^{20}\) in which he extended the quantum condition of Bohr to the system with several degrees of freedom. As is well known, Sommerfeld dealt with Keplerian orbits by means of the generalized quantum conditions

\[ \int_0^{2\pi} p_\phi d\phi = n\hbar, \]  \[ \int p_r dr = n'\hbar, \]  

where \( n \) and \( n' \) are whole numbers, \( p_\phi \) and \( p_r \) the momenta corresponding to the polar co-ordinates \( \phi \) and \( r \), and gave satisfactory explanation of the fine structure of hydrogen type spectrum by taking into account the relativistic variation of the mass of electron\(^{21}\).

Sommerfeld also succeeded in applying his theory of fine structure to the explanation of X-ray doublets.\(^{22}\) The possibility of applying his theory to K- and L-doublets was suggested to him by Kossel’s empirical relations of the splittings of \( K\alpha \) and \( L \) doublets which implied that the L-ring consisted of two rings. At the same time, Sommerfeld noticed the similarity of Moseley’s formula for \( K\alpha \)-line with Bohr’s formula for hydrogen type spectrum. That the terms in Moseley’s formula has the same form as those in Bohr’s formula except that \( Z \) is replaced by \( Z-1 \) suggested him that the electrons, at least, on the innermost two rings would be describing hydrogen like orbits. He thus thought that the relativistic theory of the fine structure of hydrogen type spectra might be applied to K- and L-doublets. If the relativistic correction was taken into account, he imagined, it would become possible to get a more precise expression for the terms of X-ray spectrum.

Substituting the effective nuclear charge in Sommerfeld’s expression \( W = - \)
\[ \frac{\alpha Z^2}{(n+n')^2} \left[ 1 + \frac{\alpha^2 Z^2}{(n+n')^2} \left( \frac{1}{4} + \frac{n'}{n} \right) \right], \]
where \( \alpha = \frac{2\pi e^2}{\hbar c} \), of the energy of electron moving on the orbit characterized by the principal quantum number \((n+n')\), Sommerfeld got expressions of two L-terms, which were characterized by \(n = 2, n' = 0\) (circular orbit) and \(n = 1, n' = 1\) (elliptical orbit) respectively, and of K-term:

\[
\begin{align*}
L &= R \cdot \frac{(Z - J)^2}{2^2} \left( 1 + \frac{\alpha^2}{4} \frac{(Z - J)^2}{2^2} \right), \\
L' &= R \cdot \frac{(Z - J)^2}{2^2} \left( 1 + \frac{5\alpha^2}{4} \frac{(Z - J)^2}{2^2} \right), \\
K &= R \cdot \frac{(Z - k)^2}{1^2} \left( 1 + \frac{\alpha^2}{4} \frac{(Z - k)^2}{1^2} \right).
\end{align*}
\]

(6)

L' – L, being equal to the difference of two frequencies which correspond to the energies required to remove an electron respectively from L' -orbit and from L-orbit, should, according to Kossel's empirical relations for K- and L-doublets, be equal to \(\nu_{Lg} - \nu_{Lg'}\), and consequently to \(\nu_K - \nu_{K'}\), or \(\nu_L - \nu_{Lg}\). The value of the splitting of L-doublet, \(\Delta \nu = L' – L\), which was proportional to the 4th power of the effective nuclear charge, well agreed with measurements for the elements from \(Z = 24(\text{Cr})\) to \(Z=92(\text{U})\), if the screening effect \(l\) was put equal to 3.5. Showing that in the nonrelativistic case the screening effect \(k\) should be equal to 1.6 if \(l\) is equal to 3.5, Sommerfeld obtained the following expression for the frequency of \(K_{\alpha}\)-line:

\[
\frac{\nu_{K\alpha}}{R} = \frac{(Z - 1.6)^2}{1^2} - \frac{(Z - 3.5)^2}{2^2}
\]

(7)

Concluding his considerations outlined above, Sommerfeld pictured the atom as follows\(^2\,3\): The difference between the optical spectra and the characteristic X-ray spectra lies in that while the former is produced by the displacement of the electron outside “the electron cloud”,\(^*) the latter is produced by the displace-  

\(^*)\) Sommerfeld called the electrons other than “light electron”, which takes part in the emission of spectral lines, the electron cloud.
ment of the electron in the innermost part inside the electron cloud. The similarity of the spectral terms for $K_\alpha$-line with the hydrogen type terms suggests that the electrons describing the first (K) and the second (L) orbits are hardly disturbed by the electron cloud. This explains the fact that the larger the quantum number is in the case of optical spectra and the smaller in the case of X-ray spectra, i.e. the more distant from the electron cloud are the electrons taking part in the emission of spectra, the more approaches the spectrum formula to the hydrogen type formula.

Thus Sommerfeld's atom in the normal state consists of the inner electrons belonging to K- and L-orbits separated from the electron cloud (whereas M, N... orbits penetrate into the electron cloud), the electron cloud, and the outer electrons taking part in the emission of the optical spectra.

IV. Germination of the Conception of "Closed Shell"

Now, it should be noticed here that through the works of Kossel and of Bohr, one of the most important conceptions for understanding the constitution of atom was germinated. It was the idea of completion of the electron ring, namely that of the closed shell as we now understand.

It can be seen from Kossel's discussion that he presupposes that the vacant place in an electron ring must immediately be filled up by another electron from an outer ring. This implies the idea of completion, that is, the assumption that each of electron rings has tendency to contain a certain definite number of electrons. But Kossel did not yet realize this idea. In his paper of 1914 Kossel expected that the transition of an electron from the innermost ring not only to outside the atom but also to an outer, the second or the third ring, etc. of the atom would take place. He however concluded from his absorption curves which showed no peak of absorption corresponding to the $K_\alpha$-emission line that the transition of electron at least from the innermost ring to the second ring did not occur so frequently as the ionization process.

*) Kossel thought that this conclusion could not be applied to the transition to much outer rings.

26) He continued searching for absorption lines corresponding to these processes and in 1919 he concluded the existance of such absorptions from the results obtained bt Stenström.
It seems to be Bohr who first understood clearly the idea of completion.²⁸) Bohr noticed the previous results of W. H. Bragg which had showed that, in order to produce any line of the K-radiation of an element, the frequency of the exciting radiation had to be greater than the frequency of all the lines in the K-radiation. He stated that this lack of the reverse process to that of emission, in contrast to the ordinary optical resonance absorption, might be simply explained on the basis of Kossel’s view: “The simple reverse of the process corresponding to the emission of, for instance, $K\alpha$ would necessitate the direct transfer of an electron from ring 1 to 2, but this will obviously not be possible unless at the beginning of the process there was a vacant place in the latter ring.”²⁹) It is therefore necessary for the emission of any line in the K-radiation that an electron should first be completely removed from the atom. Bohr’s argument here clearly presupposes that each electron ring could contain only certain limited number of electrons.

From the similar considerations as Bohr’s, Kossel in 1916 stated that in the non-excited state the inner rings would reject the electrons coming from the outer rings or outside the atom, and that this conception, providing us with a concrete image of atom according to which the inner rings were “completely occupied” by the electrons, was important for the understanding of the conditions of stability of the electron arrangement.³⁰)

The idea of completion which was almost simultaneously deduced also from the chemical considerations by Kossel himself³¹) logically implied the exclusion principle, but no one at the time noticed this. It might be interesting to know how this idea, connected with the development of the study of the fine structure of spectra and with the chemical considerations, was developed to the exclusion principle. However, except Bohr and Kossel, no investigator who attempted to estimate the numbers of the electrons in the inner rings did pay attention to the idea of completion.

V. The Numbers of the Electrons on the Inner Rings
It seems to us today as if Kossel and Sommerfeld provided physicists with the basis on which the emission of the characteristic X-rays could sufficiently be explained in terms of the Bohr atom. But the situation was not so simple. In those
days it was not so easy to form the picture of atom that, in the ground state (or the normal state as Bohr called), consisted of electrons distributed on several rings characterized by different quantum numbers. According to Bohr's original assumption, in the normal state of the atom all the electrons had indeed the same angular momentum $\frac{h}{2\pi}$, that is, they were distributed on rings with the same quantum number 1.

Furthermore Kossel's mechanism that the X-rays were emitted when a vacant place in an inner ring produced by the removal of an electron from it to outside the atom or to an possible outer ring was filled up by an electron from an outer ring was not generally accepted. Neglecting the effect of electrons on the outer rings, if such rings existed at all, many dealt with the emission of X-rays by assuming only the transition of one or more electrons between inner two adjacent rings.

This can be seen from various attempts made in those days to determine the numbers of electrons on rings.

On the assumption of ring arrangement of electrons, it was possible to estimate the numbers of electrons on the inner rings if one calculated, with approximation, the frequencies of spectral lines taking into account the influence of other electrons on the same ring and of electrons on the other inner rings and then compared the values thus obtained with the observed frequencies of X-ray spectra. In fact, numerous investigators, such as Debye, Vegard, Sommerfeld and so on, turned themselves to this problem.

As was mentioned in the section II, Moseley had attempted to determine the number of electrons in the first ring. In somewhat different way the simple case of an atom having solely one ring was dealt with by Debye in 1917. Debye assumed that $p$ electrons, each of which had the angular momentum $\frac{h}{2\pi}$, moved at equal angular intervals on a circular orbit around a nucleus of charge $eZ$. He supposed that the electrons on the outer ring, if these existed at all, would be so distant from the first ring that they had no influence on it. The total energy of the ring was expressed as $-hRp(Z - S_p)^2$, where $S_p = \frac{1}{p} \sum_{i=1}^{p-1} \frac{1}{\sin \left( \frac{\pi i}{p} \right)}$. It was supposed that when an electron was excited to another possible ring, the angular momentum of each remaining electron was preserved while these remaining
electrons would be rearranged so as to be situated again at equal angular intervals. Kα - line was supposed to be emitted when an electron excited to the nearest possible ring with an angular momentum $\frac{2h}{2\pi}$, then fell onto the original ring. The total energy of the excited system was calculated to be $-hR \left\{ (p-1)(Z-S_p-1)^2 + \frac{1}{2} (Z-p+1)^2 \right\}$. According to Bohr’s frequency condition, the frequency of the Kα - line is $\nu = hR \left\{ (p-1)(Z-S_p-1)^2 + \frac{1}{2} (Z-p+1)^2 \right\}$. By comparison of this equation with the measurement of Kα - lines for the elements from Na (Z = 11) to Nd (Z = 60), it was shown that by putting $p = 3$ a good agreement was obtained for the small values of $Z$. Furthermore by taking into account the relativistic variation of the electron mass Debye could show that for larger $Z$'s, too, a good agreement was obtained with $p = 3$. Debye thus concluded that in atoms of all elements (except for the first few elements in the periodic table) the innermost ring had three electrons on it.

In the same year, claiming the extension of Debye’s consideration, Vegard assumed a process that a group of $q$ electrons were excited from the innermost ring with $p$ electrons to the second ring, and then $q$ energy quanta were emitted simultaneously by the falling down of all the $q$ electrons to the original ring. This process comprises both Debye’s solution of $p = 3$ and $q = 1$ and Moseley’s assumption $p = 4$, $q = 4$. Vegard could show that both assumptions led to the frequencies of Kα - lines which agreed equally well with the observations, although he considered the assumption of Debye to have an advantage in its physical implication.

Vegard then proceeded to explain the L-lines in the following way. In view of the fact that the sharp absorption in the L region corresponded to a wavelength somewhat shorter than that of Lα emission line, though it was found near the latter, it was necessary, argued Vegard, to assume that there existed electrons on the second ring (L-ring) in the normal state of the atom and that the Lα - line was produced when an electron excited from the second ring to the third returned to the original second ring. It could not be assumed, as done by Debye, that the Lα-line was emitted when an electron excited from the first ring to the third fell
onto the second ring.

Furthermore Vegard assumed that this second ring should be characterized by the quantum number 2. He showed that from calculation of the frequency of \( L\alpha \) line by assuming that the \( L\alpha \) line was emitted by a transition of electrons between two adjacent inner rings and that electrons on these two rings were equally considered to have the same angular momentum \( \frac{h}{2\pi} \), empirical formula of Moseley's type for the frequency of \( L\alpha \) line was never deduced. This result necessitated to alter in some way Bohr's assumption that in the normal state all the electrons had the same angular momentum \( \frac{h}{2\pi} \).

Thus Vegard assumed that \( L\alpha \) line was produced when an electron excited from the second ring of quantum number 2 to third ring of quantum number 3 fell onto the original ring. And he could get the solution that if one, according to Debye, takes 3 as the number of electrons on the first ring, there should be 7 electrons in the second ring in order to obtain the expression for \( L\alpha \) line which agrees with the observations.

Here it must be noted that Vegard in his calculation pointed out that in the normal state of atom there must be several rings with the quantum number larger than 1 (or "multiple-quanta" rings or orbits as were called in those days). He was the first to mention the existence of the multiple-quanta rings.\(^*)\) Bohr in his 1913 paper assumed that all the electrons in the normal state had the same angular momentum \( \frac{h}{2\pi} \). He, however, accepted Kossel's explanation of X-ray spectra in which the existence of the multiple-quanta rings in the normal state of the atom was tacitly suggested. Sommerfeld's atom consisted of the multiple-quanta orbits of electrons. Therefore it might as well be possible that Bohr would have realized the existence of the multiple-quanta rings in the normal state before Vegard pointed it out\(**). As far as we know, however, it was not until 1921 that Bohr explicitly mentioned it, when he wrote in a letter to the editor of *Nature*: "The assumption of the presence in the normal state of the atom of such multiple-

\(^*)\) Heilbron asserted that the modification of "Bohr's one-quantum ring atom" was the main contribution of the study of X-ray spectra to the theory of atomic structure during the period under consideration.

\(**) According to Heilbron, before Kossel and Sommerfeld, namely in the end of 1913, Bohr still believed that in the normal state the atom had not multiple-quanta orbits.
X-rays and Atomic Structure in 1910's

quanta orbits has already been introduced in various recent theories, as, for instance, in Sommerfeld's work on the high-frequency spectra..." 34) In any case, the fact that Vegard for the first time pointed out explicitly the possibility of the presence of multiple-quanta rings in the normal stage of atom seems to show that Bohr's conception of atom was generally received literally, even after the works of Kossel and Sommerfeld, as stating that the atom in the normal state has only rings of quantum number 1 (or one quantum rings as were called in those days) and the so-called multiple-quanta rings exist only in the excited state.

In 1918 Sommerfeld reinterpreted his theory of X-ray spectra so as to conform his elliptical electron orbits to the ring configuration, and on Sommerfeld's theory Kroo estimated the numbers of electrons on the first two rings.

Sommerfeld thought that the screening effect to the nuclear charge in his formula (6) could be explained rather by the assumption of ring arrangement of electrons than by his own atom which had electrons moving on circular and elliptical orbits, and attempted to conform the existence of the elliptical orbits of electrons to the ring configuration by assuming that the electrons moving on their own elliptical orbits could be imagined as being situated at any instance at equal angular intervals on a circle with the nucleus at the center. 35) Sommerfeld called such a configuration "Ellipsenverein". Then he could discuss generally the energy of the system consisting of many electron rings. Showing that the energy of interaction between rings might be neglected in dealing with, at least, the Kα -emission, Sommerfeld concluded that the frequency of Kα - line could be calculated in the similar way as Debye and Vegard, i.e. Sommerfeld assumed that the electrons on rings inside the ring concerned were concentrated at the nucleus, other electrons on the same ring were situated at the same angular intervals, and electrons on outer rings could be neglected.

On this theory of Sommerfeld Kroo calculated the frequency of the Kα-line. 36) The atom in the normal state was assumed to consist of k electron ring of quantum number 1 and of l electron ring of quantum number 2. He also assumed that Kα is emitted when an electron excited from the first to the second ring falls onto the first ring. Putting k = 3 and l = 8, that is, comparing the energies of the excited state with 2 electrons on K-ring and 9 electrons on L-ring and the
final state with 3 electrons on K-ring and 8 electrons on L-ring, he obtained an expression of the frequency of K\textsubscript{\textalpha}-line which well agreed with the observation.

Estimations of the numbers of electrons on inner rings were not successful on the basis of the ring model. As was mentioned in the previous paper\textsuperscript{37)}, the fact that the result was not in conformity with the numbers of electrons expected from the periodic system of elements, forced Reiche to suspect whole conception of the distribution of coplanar rings and to suppose that the electrons would take spacially symmetrical configurations\textsuperscript{*}).

Furthermore it is seen that in these works the idea of the completion of the ring was not taken into account. If it were taken into account, it would be impossible that the excited state has larger number of eletrons on L-ring than in the normal state. Therefore, the scheme by Kroo for the emission of K\textsubscript{\textalpha}

<table>
<thead>
<tr>
<th></th>
<th>K-ring</th>
<th>L-ring</th>
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<tbody>
<tr>
<td>Normal state</td>
<td>3</td>
<td>8</td>
</tr>
<tr>
<td>Initial state</td>
<td>2</td>
<td>9</td>
</tr>
<tr>
<td>Final state</td>
<td>3</td>
<td>8</td>
</tr>
</tbody>
</table>

would have to be replaced by the scheme:

<table>
<thead>
<tr>
<th></th>
<th>K-ring</th>
<th>L-ring</th>
</tr>
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<tbody>
<tr>
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<td>9</td>
</tr>
<tr>
<td>Final state</td>
<td>3</td>
<td>8</td>
</tr>
</tbody>
</table>

Excitation or Ionization of K-ring

Emission of K\textsubscript{\textalpha}

Whether the idea of completion may be taken into account or not, there will be no essential difference in the calculation but there would be a difference in the interpretation of the results obtained. In order to conform the results to the conclusion based on chemical considerations, Sommerfeld interpreted the Debye's

\textsuperscript{*}) The same suspicion was also raised by the works on the X-ray spectra on the more complex assumptions on the electron arrangement and by the investigations on the crystal structure. For the detailed account of this, the reader is referred to Heilbron's article.
X-rays and Atomic Structure in 1910's

conclusion in such a way that the initial state in which there were 2 electrons in the first ring and 1 electron in the second was the normal state of the atom and the final state in which there were 3 electrons in the first ring was the state of supersaturation.\textsuperscript{38)}

The emphasis should however be placed on the fact that through these works on the arrangement of electrons, the presence of the multiple-quanta rings in the normal state of atom gradually became to be recognized clearly and at the same time the importance of the “energy levels” in the considerations of the constitution of atom was begun to be realized.

VI. Conclusion
After Bohr and Moseley it became to be believed that the study of the characteristic X-ray spectra should shed much light on the structure of the innermost part of atom, by the reason that while the optical spectra and the chemical properties of the atom were supposed to be the manifestation of the behaviour of outermost electrons in atom, the X-ray spectra were supposed to be originated from the innermost electrons in atom. On the basis of the assumption of the ring arrangement of electrons various attempts were made to determine the numbers of electrons in the innermost rings by comparing the calculated frequencies of the characteristic X-rays with the observations. At the end of the 1910’s the results of such investigations, however, turned out to be altogether unsatisfactory. In the first place these results were not in conformity with the results obtained on the basis of chemical considerations. Those who attempted to determine the electron arrangement on the basis of the study in X-rays, thought that their results should ultimately be able to explain the chemical properties of atom.\textsuperscript{39)} For example, Vegard attempted to build up a periodic system which was conformable to his results: thus the numbers of electrons on the first and the second rings for the elements in the first short period were assumed to be Li (2, 1), Be (2, 2), ... F (2, 7), Ne (3, 7).\textsuperscript{40)} In order to conform Debye’s conclusion to the chemical atom which had 2 electrons on the first ring, Sommerfeld proposed an interpretation that the configuration in which there are 2 electrons in the first ring and 1 electron in the second was the normal state of atom and that which has 3 electrons in the first ring was the state of supersaturation.\textsuperscript{41)} Smekal also
pointed out that Kroo's conclusion could not explain the property of Na \( (Z = 11) \) which should have a single valence electron.\(^{42}\)

The doubt against the ring arrangement of electrons was also strengthened by further attempt to determine the number, \( p_3 \), of electrons on the third ring. Smekal showed that no satisfactory expression of the frequency of \( L_\alpha \)-line could be obtained whatever number might be chosen for \( p_3 \) if one, according to Kroo's result, chose 3 and 8 for the numbers of electrons on the first and the second rings.\(^{43}\) Furthermore the lattice dynamics of ion crystals by Born and Landé revealed that their compressibility calculated on the basis of the ring atom model did not agree with the observation.\(^{44}\)

As was shown in the previous paper\(^{45}\), according to the conclusion obtained on the basis of chemical considerations in the period considered, the atom consists of several rings or shells each of which contains a certain limited number of electrons. This number of electrons was determined, for instance for the atom of Rn \( (Z = 86) \), to be 3, 8, 18, 32, 18 and 8 from inner ring to outer. This configuration is correct as far as the rings or shells could be assumed to correspond to the quantum number 1, 2, ... . But it is not possible to deduce from the chemical consideration that each ring or shell should be characterized by different quantum numbers. However, when the presence of "multiple-quanta" rings or orbits in the ground state of atom became to be established, one could assign the quantum number 1, 2, ... to successive rings.\(^{46}\) This was, believes the author, the contribution from the studies in X-rays to the theory of atomic structure during the period considered.

The main achievement in the study of atomic structure during the 1910's, therefore, was the establishment of the atomic model consisting of several rings or shells each of which is characterized solely by the principal quantum number and contains certain definite number of electrons. However, the problem how to re-distribute electrons on each ring over the sub-states to be characterized by sub-quantum numbers was left unsolved. Satisfactory answer to this problem was not obtained until the 1920's when detailed investigations on the fine structure of both optical and X-ray spectra were advanced.\(^{47}\)
Acknowledgements

The author is indebted to Prof. Tetu Hirosige for invaluable advices and discussions throughout this study. She also thanks to Mr. J. Nemoto of Japan Meteorological Agency for offering her the convenience to gain access to some literatures.

References

9) For example, P. Debye: Ref. (32) especially p. 276; S. Dashman: Ref. (8).


16) W. Kossel: Ref. (14). See the foot note 1) of p. 960.

17) N. Bohr: Ref. (11).


21) For the detailed account, the reader is referred to, for example, M. Born: *The Mechanics of the Atom*, Frederic Ungar, New York, 1960 (Translation of *Vorlesungen ubver der Atommechanik*, 1925).

22) A. Sommerfeld: Ref. (18).

23) A. Sommerfeld: Ref. (18), especially p. 165.

24) W. Kossel: Ref. (14).


26) W. Kossel: Ref. (14), the foot note 1) of p. 963.


28) N. Bohr: Ref. (11).


30) W. Kossel: Ref. (19).


38) A. Sommerfeld: Ref. (35).

39) Ref. (1), especially p. 27.


41) A. Sommerfeld: Ref. (35).


43) F. Reiche: Ibid.

44) Ref. (43).

45) Ref. (1).

46) Ref. (27).

47) In 1921 Bohr considered the quantum structure of atom for all elements of the periodic system, half theoretically and half empirically, by making use of all the evidences provided by physics and chemistry, especially of the spectral data. N. Bohr: The Theory of Spectra and Atomic Constitution, Essay III. "The Structure of the Atom and the Physical and Chemical Properties of the Elements", Cambridge Univ. Press., London, 1924 (1st ed. 1922). For the account of this theory of Bohr, the reader is referred to S. Nisio: Ref. (1).
The Early Scientific Work of John Milne

John WARTNABY*

Introduction

John Milne is best-known as one of the leading seismologists of the nineteenth century. His work in this field began in 1880 and continued to his death in 1913. His contributions to seismology and his place in the history of this subject will be discussed in a further work, now nearing completion.

It is the purpose of the present work to draw attention to Milne's little-known earlier work accomplished between the years 1874 and 1886. It covers a wide field including mining geology, glaciology, mineralogy, crystallography, volcanology, archaeology and natural history.

Life and Work of J. Milne

Accounts of Milne's life and scientific career have appeared previously; this section is a synopsis of those accounts (1)-(5).

Born in Liverpool on 30th December, 1850, John Milne was the only son of Emma, daughter of James Twycross of Wokingham, and John Milne of Milnrow, Rochdale. As a boy, Milne was educated in Rochdale and subsequently at the Collegiate College, Liverpool. A striking example of his fondness for travel, his initiative and independence occurred at an early age when he used savings and a school prize of money to go to Iceland without first gaining his parents' consent. The object was to see the volcanic area of Vatna Jokul, descriptions of which had fired his imagination. This episode marked the beginning of his long-standing interest in volcanic phenomena.

On leaving school, Milne entered King's College, London, and later attended the Royal School of Mines where he studied geology under Professor Sir

Warrington Smyth. After gaining some practical experience of mining engineering in Cornwall and Lancashire, he spent a short time at the Bergakademie, Freiburg and visited the principal mining districts of Germany. On his return to England, Milne was selected by Cyrus Field, Sir James Anderson and others to report on the mineral resources of Newfoundland and Labrador, a project accomplished during the summers of 1873 and 1874. The survey resulted in Milne making very useful contributions to existing knowledge of these resources and to the petrology of Newfoundland. Icebergs were often to be seen floating in the seas surrounding this country, coast-ice was a common feature and the rocks bore clear evidence of past glaciation of the land surface. These circumstances aroused in Milne a considerable interest in glaciology, a subject to which he also made useful contributions. While in the area, he took the opportunity to visit Funk Island where he made observations on the geology, flora and fauna and paid special attention to remains of the Great Auk. In 1874, he was appointed geologist to an expedition made under the auspices of the Royal Geographical Society and led by C.T. Beke (6) in quest of the true site of Mount Sinai in N.W. Arabia. Bake believed this mountain to be situated some 95 miles north-east of the district in which it had hitherto considered to be. Milne's geological investigations on this expedition provided material for two interesting papers.

In the following year he was appointed Professor of Geology and Mining in the Imperial College of the Public Works Department, Tokyo. The post was one of many created in response to the Emperor's wish to bring western learning and methods to Japan.

The usual method of travelling to Japan at that time was by P. and O. mail packet but it was characteristic of Milne that he chose an arduous but much more interesting route overland across Europe and Asia. His journey began at Hull on 3rd August, 1875. After crossing to Gothenburg by sea, he travelled through Norway, Sweden and Finland to Russia and thence across the Urals into Siberia. Next followed the most difficult and uncomfortable part of the journey, made by camel across Mongolia to China. For 31 days on one section of this journey there were no roads, no dwellings, bread and vegetables were lacking to augment the unpalatable food, temperatures were extremely low with snow abounding, and there was no possibility of washing or changing clothes. Shanghai was reached on
24th February, 1876, from where he subsequently travelled by sea to Japan, reaching Tokyo on 8th March, 1876.

Milne kept very full notes of his experiences and observations, even under the most arduous conditions, and they enabled him later to publish two long accounts of his journey. One account was chiefly of geological interest (7), the other was a travelogue of more general interest (8).

Milne’s experience of earthquakes began on his first night in Tokyo when Yama Gouchi, the house provided for him by the Japanese Government, was shaken by a minor shock. Occurrences of this kind were frequent and could scarcely fail to arouse the interest of Milne and the other scientists and engineers who were his contemporaries at the College. It was not, however, until a semi-destructive shock took place at Yokohama in 1880 that Milne’s career as a seismologist could be said to have begun.

His early years in Japan were spent in attending to his duties at the College, carrying out a little research in the laboratory, and exploring the country, with special reference to its geology, volcanoes and archaeology. As an outcome of his lectures on mining, he published in 1878 and 1879 respectively two pamphlets intended largely for the guidance of his students, works which were later to appear as part of a much more extensive textbook on the subject. His lectures on crystallography also formed the basis of a textbook, published in 1879. His investigations in the laboratory led to a publication on the elasticity of crystals.

Observations in the field, coupled with the perusal of existing collections of minerals, led to the publication in 1880 of a paper on the minerals of Japan in which he described examples of a number of minerals previously unrecognized in that country.

Milne’s strong interest in glaciology led him to seek evidence for past glacial action in Japan; he presented his interesting conclusions on this hitherto-neglected topic in 1881.

While pursuing his geological interests, Milne came across examples of Stone Age artifacts which let him to undertake two archaeological studies of this period, the results of which he published in 1879 and 1881 respectively, although the earlier of these papers was not published in full until 1881.

It was in volcanology, however, where his greatest interest lay during his early
years in Japan and in this field he made significant contributions especially in regard to the distribution of volcanic activity in Japan and to analysis of the shape of certain regularly-formed volcanic mountains.

An earthquake which caused widespread damage in Yokohama on 22nd February, 1880, had very important consequences for the progress of seismology. Shortly after the earthquake had occurred Milne issued a call for a public meeting, a move which met with considerable support and resulted in a crowded hall at which was formed the Seismological Society of Japan, the first to devote itself exclusively to the study of seismology and volcanology.

The Japanese Government encouraged the new wave of interest in seismology by forming an Earthquake Committee supported by an annual grant and by establishing a Chair of Seismology at the Imperial College. Milne was appointed to this post, which he held until he left Japan in 1895. This allowed him to give all his time to seismological research. His output was immense and his boundless energy and enthusiasm did much to stimulate others to carry out similar work.

Milne’s work was also aided by a grant he received from the British Association. The Association appointed a Committee on the Earthquake Phenomena of Japan, with Milne as Secretary and his reports of this Committee’s work and that of the Association’s Committee on Earth Tremors form useful summaries of the research carried out in Japan by Milne and his colleagues. Milne’s close link with the Association lasted for over 30 years, until his death in 1913.

Following his investigation of the Yokohama earthquake, the more important work carried out by Milne in Japan was the preparation of local and regional earthquake catalogues on which he based his studies of earthquake distribution in Japan, experimental investigation of the transit of elastic waves through the ground, recording and interpreting earth tremors (9), and his considerable achievement of developing a sensitive and reliable seismograph capable of recording an earthquake occurring in any part of the world. Milne also prepared a textbook on seismology, *Earthquakes and other Earth-movements* published in 1886. A companion volume, *Seismology*, was published in 1898 after he had left Japan (10).

Sometime during his residence in Japan, Milne married Tone, daughter of
The Early Work of John Milne

Horikawa Noritsune, high priest of Hakodate, but the date of the marriage has not been recorded by his biographers.

On returning to England in 1895, Milne lived at Shide near Newport in the Isle of Wight. He quickly set up a seismological observatory at his new home where he continued to pursue his studies with the help of his Japanese assistant, Hirota. Through the medium of the British Association, Milne was successful in persuading the authorities in many countries to set up observing stations at which Milne seismographs were installed and from 1898 onwards he carried out the considerable task of interpreting and correlating all their records and subsequently preparing circulars for twice-yearly dispatch to the stations, providing them with full details of all the earthquakes recorded. By 1912, there were some 60 stations involved in this procedure.

With this record of achievement, it is not surprising that many honours and distinctions were conferred on Milne. He was made an Honorary Fellow of King's College, London in 1896 and was Lyell Medallist of the Geological Society in 1894. In 1887, he was elected a Fellow of the Royal Society of which he was Royal Medallist in 1908. In 1895, at the close of his career in Japan, the Emperor conferred on him the Order of the Rising Sun and granted him a pension. Milne was awarded an Honorary Doctorate in Science by the University of Oxford in 1906.

Milne's death took place on 31st July, 1913 after a short illness.

Geology and Mining

In this section, Milne's published work in mineralogy, petrology, glaciology, crystallography and mining is considered. Volcanology is considered separately in a later section. The sub-sections are arranged in approximately chronological order.

1. The Mineral Resources of Newfoundland

Mention has been made that Milne's first professional duties were to report on the mineral resources of Newfoundland, a task he began in the summer of 1873. His first paper, published in the following year, was an account of this survey(11). He made it clear in his opening remarks that the subject matter was largely an account of his own investigations:
"The geological characters of many of the places referred to having already been so thoroughly described by Mr. Murray in his reports on the geology of this island, or else remaining to form at some future period the subject of such reports, it is purposed (sic) in this communication to omit all that has already been written, and only to touch briefly on the remainder." (12)

After describing the principal physical features of Newfoundland and correlating them with the geology and glaciology of the island, Milne described his mineralogical findings, working round the country anti-clockwise from St. John's.

At St. Mary's Bay he found slight indications of lead and copper and on the south-east side of the harbour there was a vein of white quartz, three to four inches thick, carrying galena, iron pyrites, blende, and occasionally copper. The vein cut through slates and quartzites in a direction N.76°E. to S.76°W., with a dip of 38° (13).

Milne drew attention to the green fluor spar in which galena and blende were worked in existing and abandoned mine workings in the Placentia Bay area. Fluor spar was a comparatively rare mineral on that side of the Atlantic and its occurrence in Newfoundland ought especially to be noted (14).

He pointed out that the remaining part of the south coast as far as Cape Ray consisted, with few exceptions, of granites, syenites, mica-schists and other allied rocks of Laurentian age. The metalliferous deposits of this formation in Canada were associated with bands of limestone which were missing here; in their place were garnets, staurolite and cyanite. The garnets, almost opaque and dull red, showed rhombic dodecahedral forms and occurred scattered through mica-schist. Garnets were particularly plentiful east of Long Island and near Port au Basque (15).

A large deposit of staurolite was discovered by Milne on the east side of Fachoux Bay, where it thickly covered upturned micaceous slates with dark brown crystals about half an inch long. Since crystals of staurolite had previously been imported from India in considerable quantities, he judged the Facheux Bay deposit to be of decided economic importance (16).

Milne also discovered, in black slates on the eastern shore on the Bay, veins of plumbago which appeared as irregularly formed dyke-like masses striking out east and west (17).
At Big Barbe Head, Bay of Despair, he described quartz veins carrying galena and specks of copper and iron pyrites and further north, serpentine rocks containing diallage and pyroselerite.

Milne found at the north end of Codroy Island a small vein of siderite and on the mainland opposite this area shales which assumed the character of slates; he recommended that these might be used for supplying local requirements (18).

Between Cape Anguille and the north end of St. George's Bay, a number of rivers and streams cut the Coal-measures at right angles. Milne travelled up one of these streams for about four miles and found the general relation of the rocks to be that represented in the section shown here reproduced from his paper (19). The western (left-hand) end of the section corresponds to the south of the stream where cliffs of red sandstone dipped at an angle of about 10° downstream. This was underlain by the rocks listed in the sketch section, the series repeating itself as an anticlinal further up stream.

1. Bright red sandstone.
2. Yellowish grey limestone.
5. Shales.
6. Conglomerate.

Fig. 1: Sketch Section of Coal-measures near St. George's Bay

This region where the Carboniferous rocks were predominant was one of the most important areas in Newfoundland for the occurrence of minerals of potential economic importance and led Milne to investigate in some detail the deposits of gypsum, coal, magnetite and galena which he found there (20). The exposed beds of coal in the St. George's Bay area were highly tilted at an angle of about 85° but many were too thin to be worked with advantage or were rendered unsuitable by an admixture of shales and iron pyrites. Of these deposits he concluded:

"Whether the coal improves in quality from south to north, as it would seem to do from the examples cited, and as certainly the gypsum which has been spoken of does, there is not yet a sufficient accumulation of data for satisfactory proof. It is hardly fair to judge of the value of these
Coal-measures from the eight or nine seams or exposures which have been found cropping out upon the surface, as upon further exploration by boring and other processes seams of greater value may yet be discovered...” (21)

The magnetite deposits were too disseminated to be of economic importance but Milne drew attention to the galena deposits in the vicinity of Port-au-Prince Peninsula:

“One the south side of this peninsula several of the small north and south dislocations hold galena; but these are merely indications and leaders up to the larger depositories in the east and west fault, where this valuable ore is apparently accumulated.” (22)

The only remaining area in which economically important mineral deposits were found was at Tilt Cove in Notre-Dame Bay. These deposits had been fully described by Murray in 1868 and although further investigated by Milne, he appears to have added little to Murray’s findings.

Milne concluded on an optimistic note which must have encouraged the sponsors of the investigation:

“....but from the numerous indications on the coast, not one half of which has yet been traversed, it might be argued that Newfoundland will in future rank high in the lists of the mining-world.” (23)

2. The Rocks of Newfoundland

Milne’s survey of the rocks of Newfoundland was carried out in part at the same time as his investigation of the island’s mineral resources in the summer of 1873, but the task was not completed until 1874. An account of his survey was published in 1877 (24). It was based on visits to almost every bay and cove and treks into the interior which was then virtually unknown territory. In this way he made two complete circuits of the island and collected many specimens of rocks and fossils. Nevertheless he acknowledged that he was much indebted to previous work by Murray (25).

Near the beginning of his paper Milne provided a table of strata, including a comparison with the sequence in North America and Britain. This table is so useful in following his account of the survey that it is reproduced here.
<table>
<thead>
<tr>
<th>NORTH AMERICA</th>
<th>GREAT BRITAIN</th>
<th>NEWFOUNDLAND</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lower Laurentian</td>
<td>Laurentian</td>
<td>L. Laurentian.</td>
</tr>
<tr>
<td>Upper Laurentian</td>
<td></td>
<td>U. Laurentian.</td>
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<tr>
<td>Huronian</td>
<td></td>
<td>Huronian.</td>
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<tr>
<td>St. John’s Group</td>
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<tr>
<td>Paradoxides Slates</td>
<td>Lingula Flags</td>
<td>Primordial Silurian.</td>
</tr>
<tr>
<td>Lower Potsdam</td>
<td>Tremadoc Slates</td>
<td>Potsdam.</td>
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<tr>
<td>Upper Potsdam</td>
<td></td>
<td>Calcareous.</td>
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<tr>
<td>Lower Calciferous</td>
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<tr>
<td>Upper Calciferous</td>
<td>Levis</td>
<td>Levis.</td>
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<td></td>
<td>Quebec Group</td>
<td>Quebec</td>
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<td></td>
<td>Lauzon</td>
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<td>Sillery</td>
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<td>Chazy</td>
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<td>Trenton and Bird’s Eye Limestones</td>
<td>Caradoc Beds</td>
<td>Bird’s Eye Limestone.</td>
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<td>Utica Slate</td>
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<td>Hudson River Beds</td>
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<tr>
<td>Oneida Conglomerate</td>
<td>L. Llandovery Rocks</td>
<td>M. Silurian.</td>
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<tr>
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<td>U. Llandovery</td>
<td>Clinton.</td>
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<td>Clinton Group</td>
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<td>Niagara?</td>
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<td>Niagara Group</td>
<td>Wenlock.</td>
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<td>Onondago Group</td>
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<td>Lower Helderberg</td>
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<tr>
<td>Oriskany Sandstone</td>
<td>L. Devonian</td>
<td>Devonian?</td>
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<td>L. Devonian</td>
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<tr>
<td>Schoharrie Grit</td>
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<td>Gaspe Sandstones.</td>
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<td>Gaspe Sandstone</td>
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<tr>
<td>Mid. Devonian or Upper Helderberg</td>
<td>M. Devonian</td>
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<tr>
<td>U. Devonian, Portage Group, etc...</td>
<td>U. Devonian</td>
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</tr>
<tr>
<td>Lower Carboniferous (Gypsiferous)</td>
<td>Carboniferous</td>
<td>L. Carboniferous.</td>
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<tr>
<td>Middle Carboniferous (good coal)</td>
<td></td>
<td>Millstone Grit.</td>
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<tr>
<td>Upper Carboniferous</td>
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Table 1: Rock formations of Newfoundland with their British and American equivalents
Milne took the groups in order, beginning with the oldest, and described his findings in each case.

Rocks of the Laurentian series occurred abundantly in the northern, southern and central parts of the island. They consisted largely of granite, syenite and gneiss. There were many igneous dykes one of which, at Harbour Deep, had an average width of 25 yards and was investigated by Milne in some detail. The rock forming the dyke was identified as a melaphyre, the country rock being an hornblendic gneiss traversed by small veins of quartz with which was associated specks of copper pyrites. He found not only the well-known feature of variations of the constituents at different points across the breadth of the dyke but also marked variations along the length (26).

The group above the Laurentian, and roughly equivalent to the Huronian and Cambrian series, was largely made up of beds of slates which contained bands of diorite, quartzite and jasper. Milne thought it remarkable, in view of the great age of the series, that fossils were to be found embedded in the strata but Murray pointed out that the fossils were in the clay-slates quite high up in the series, immediately below sandstones and conglomerates (27).

Ascending the series, Milne noted that he had seen a fine exposure of the Primordial Silurian in the form of cliffs several hundred feet high which occurred in the Cutler’s Head area of Bonavista Bay:

"The rocks are fine-grained, chloritic and argillaceous. In many places they are coloured with red oxide of iron. Some of the rocks of this neighbourhood of an amygdaloidal character appeared to be altered diorite..... Further up the bay conglomerate and more igneous rocks of a chloritic character and rich in kaolinized felspar were observed." (28)

In the Potsdam group, consisting of dark-coloured slates and conglomerates, Milne found dykes of felsites and highly chloritic melaphyres containing quartz. In contrast, the Calciferous series, well-exposed in the west of the island, particularly on the northern side of the Port au Prince Peninsula, Milne found:

"... definitely stratified grey limestone rich in fossils, large *orthosceri*, Corals and *Mactrea* being very noticeable."(29).

He explored two quite large caverns formed in the limestone and suggested that exploration below the bed of clay covering their floors might yield material
of interest to the study of the island’s fauna.

Milne was especially interested in the Quebec series, particularly the middle division known as the Lauzon, since this series was economically the most rewarding. It outcropped extensively and Milne had visited all but one of the areas concerned. A predominant feature was the presence of dark green serpentines showing traces of actinolite. Veins of chrysotile were common. There were many signs of these rocks being derived from volcanic rocks and Milne thought there was evidence of abundant volcanic activity in the country during mid-Silurian times, a view not altogether shared by Murray who put the time of maximum activity rather earlier (30).

Above the Lauzon group, but still part of the Quebec series was the Sillery, a formation composed largely of black slates and limestones. Milne had closely examined an exposure in the northern part of the island where:

“I have observed both intrusive and embedded masses of diorite. They are generally of a dark grey or greyish green colour, and in some cases amygdaloidal, the amygdules being filled with calcite. Under the microscope, altered felspar, hornblende and grains of magnetite are generally to be seen.” (31)

The Niagara and Clinton group had been recognized only at the head of White Bay; Milne described it as a series of conglomerate and slates capped with limestone. The total thickness was about 2,800 feet but the presence of faults, some with a throw of as much as 1,000 feet, made it difficult to trace the sequence of the members of this formation (32).

Milne said of the Devonian formation, found in the Cape Rouge and Fox area, that it consisted of:

“..... a series of plant-bearing sandstones, coarse conglomerates and reddish-green slates, amounting altogether in thickness to about 3,700 feet, which have provisionally been called Devonian, and are apparently the equivalents of the Gaspe sandstones.” (33)

The most recent formation found in the island was the Carboniferous. It outcropped in two localities and in both these areas rested directly on the Laurentian. Milne described the formation as consisting of red sandstones, shales, greyish limestones, gypsum and conglomerates. The gypsum occurred in great
masses like cliffs of chalk and examination of surrounding rocks led to the rather unexpected conclusion that it acted as an intrusive rock, contorting and breaking the rocks in close contact with it. He found fossils of Silurian age in the limestone of the conglomerate which also contained pebbles of magnetic iron derived from the Laurentian series. There were several seams of coal, one of them 3 feet 6 inches thick and Milne thought it likely that many more would be discovered.

Above the Carboniferous formation there was a covering of alluvium which in many parts of the island showed strong evidence of glacial action by the presence of striated angular stones. In addition, the surface of the rocks on which the alluvium rested was often found to be roundly smoothed and striated. Milne believed that it was coast-ice acting on an area which was rising that caused these effects, rather than the action of glaciers (34).

3. Glaciology

Milne's interest in glaciology was aroused by his visit to Newfoundland in 1873, when he was greeted on arrival at St. John's by the sight of icebergs surrounded by floe-ice. His geological survey amply confirmed the conclusion reached by previous investigators that the island had been subjected in the past to an intense glacial period but he differed from then in ascribing many of the effects observed to the action of coast-ice on a steadily rising area rather than to the action of great ice-sheets. His work in Newfoundland is considered below under the headings of evidence of glacial action in Newfoundland, theories concerning icebergs, and the role of coast-ice. Milne retained his interest in the subject for a time after taking up residence in Japan, where he found evidence pointing to a limited glacial period having occurred in that country. Consideration of this evidence forms the fourth section of the present review.

(a) Evidence of Glacial Action in Newfoundland

Milne described his geological findings relating to the action of ice on the rock of Newfoundland in the same paper as that presenting his survey of the mineral resources of the island (35). He found that ice-scratches, usually in a north-east—south-west direction were common in some areas while roches moutonées and heaps of drift filled with regularly striated pebbles and boulders were discovered in many parts of the island.
The presence of raised beaches and terraces indicated that the coast of Newfoundland had been rising steadily and it could be assumed that at one period, when about 1,000 feet below its present level, the land was subjected to massive ice action which gave rise to the parallelism of many natural features of the island. Milne recalled that Agassiz in 1864 had accounted for these features by the action of a great glacier 6,000 feet thick. A theory had also been put forward in terms of a polar ice flow or a polar glacier cutting its way over the island from N.E. to S.W. (36).

In either case there were anomalies to be explained, and the general tenor of Milne’s comments leaves the reader with the impression that he was not satisfied with any of the explanations offered.

Milne’s second published work on glaciological topics appears in 1876, by which time he had extended his observation of glacial action in Newfoundland. He now advanced theories accounting for the time of arrival of icebergs in Newfoundland waters, the manner in which the bergs floated, and the mechanism of glacial action in the island (37).

(b) Icebergs

Milne pointed out that icebergs usually appeared around the coast of Newfoundland on about 1st January each year. This fact called for some explanation since the bergs must presumably have been formed from their parent glacier in the far north during the previous summer and would be expected to appear during the late summer or early autumn, making due allowance for the distance travelled. He believed that the explanation lay in the observation by Artic navigators that in very high latitudes, ice was in motion much earlier than further south. Thus on 20th May the western side of Smith’s Sound had been navigable, but Barrow Strait was blocked until August. Another possible factor in delaying the icebergs was the westerly winds which prevailed in the autumn; these gave way to northerly winds in the spring. The former would tend to keep the bergs at sea but the latter would aid the current bringing them to the shore (38).

Milne was also interested in the way in which icebergs floated. It was known that the mass of the submerged portion was seven to nine times greater than that above the water but he could not accept the view that the depth of the submerged part also bore this ratio to the height of the visible part. He thought that this might
well be the case when the berg left its parent glacier, the shape then being a close approach to a prism. As the bergs travelled towards lower latitudes, however, encountering higher temperatures and constant battering by waves, they lost their regular shape, the greatest change being suffered by the upper portion exposed to the atmosphere.

"As this waste goes on, the berg must rise, and the ratio of the height of the exposed portion to the depth of that which is hidden grows greater. The result of this is that the exposed portion becomes less and less in diameter than that which is protected beneath the surface of the water, which at last may be looked upon as kind of foot or pedestal." (39)

This tended to be confirmed by observations, since although icebergs were occasionally seen to ground in deep water in low latitudes, the reverse was true in most cases. In fact the grounding of icebergs was used by the fishermen of both Newfoundland and Labrador as an indication of shallows suitable for fishing.

Milne proceeded to consider the matter mathematically:

"For example, in the accompanying figure let AB be the surface of water in which we see a piece of ice floating as indicated by the black line, the general direction of that beneath the water corresponding to that which is above. Approximating to such a figure, draw on the 'give-and-take' system a many-sided pyramid, or in the limiting case a cone approximately equal in volume to that of the supposed berg. This is shown by the dotted line."

He then took the volume of the cone exposed above the surface to that of the
solid submerged beneath it to be in the ratio of 1 : 8 and the corresponding vertical heights to be \( h \) and \( H \). Then calling the volume of the complete cone \( V \) and that of the cone projecting above the surface \( v \):

\[
\frac{V}{v} = \frac{2}{1} = \left(\frac{h+H}{h}\right)^3
\]

or

\[
\frac{h+H}{h} = \frac{3}{\sqrt[3]{9}} = 2.08
\]

giving \( H = 1.08h \)

Hence the depth below the surface was little greater than the height of the exposed portion (40).

Criticising this analysis in a letter which appeared in a later issue of the *Geological Magazine*, O. Fisher said that Milne had not sufficiently considered the conditions of stable equilibrium. An iceberg of the shape illustrated in Milne's paper would overturn. Fisher supported his view by some rather unconvincing experimental evidence. From a set of model crystals made of boxwood he took a tetrahedron and placed it in water, where it would float only with one of its angles downwards. The position of stable equilibrium depended on the shape of the floating body and on its specific gravity:

"The specific gravity of boxwood being about 0.95, is so nearly the same as that of ice, that the positions assumed by a floating mass of either substance will be as a rule almost identical." (41)

Milne replied to this criticism in a paper published in the following year in which he discussed the stability of equilibrium of a cone of ice floating on sea water (42). He did not accept Fisher's views and wrote of the experimental use of a tetrahedron to simulate an iceberg:

"This I consider to be an unfair comparison, which no doubt has led many casual readers to the belief that a cone will also float with its apex downwards, and perhaps, in consequence, that my conclusions, being founded on false assumption, must also of necessity be false." (43)

Milne then proceeded to explore mathematically the conditions required for stable equilibrium, basing his analysis on the method used in Thomson and Tait's
Naural Philosophy, section 767. In the case of a cone floating in a liquid with apex downwards, he showed that the condition for stable equilibrium was:

\[ r > a \sqrt{\frac{1}{\rho^{1/3}} - 1} \]

where \( r \) = radius of base of cone
\( \alpha \) = height of cone
\( \rho \) = ratio of density of cone to that of the liquid

Then taking the specific gravity of ice to be 1.028 and that of sea water as 0.918.

\[ \rho = 0.893, \]
\[ r > 0.196a \]

In the case of a cone floating with its apex upwards, the condition for stability was:

\[ r > a \sqrt{\frac{1}{(1-\rho)^{1/3}} - 1} \]

which reduced to \( r > 1.05a \)

In the first instance, the limiting cone would be one having only 1/25 of its depth above water and in the second case, the limiting cone would have nearly half its height above water. These limiting cones drawn to scale were represented in Milne’s paper in the form reproduced here (44):

Fig. 3: Limiting cone, \( r = 0.196a \)
Milne pointed out that:

"The more probable form in which the generality of icebergs exist are those which have their limit represented by Case II., where we have a series of stable forms, more or less conical in their shape. Here the depth below the surface of the water never exceeds the height which is above, but is probably always less." (45)

Thus Milne saw no reason to change the views he had expressed in his earlier paper.

(c) Coast-Ice

Mention has been made of Milne's dissatisfaction with the theories which had been put forward to explain the abundant geological evidence of glacial action on the rocks of Newfoundland. In his paper Ice and Ice-work in Newfoundland, part of which has been discussed above in connection with his views of icebergs, he put forward the theory that coast-ice had played a part in the glaciation at least equal to that of glaciers and icebergs (46). He described the forms taken by coast-ice and discussed its origin. In addition to its rôle as a scratching and grooving agent, it also transported masses of material. Textbooks of geology referred to the way in which icebergs could carry rocks but it was a matter of common observation around the coasts of Newfoundland that this was a rare occurrence whereas coast-ice in the form of "balacada", barrier-ice, or ice-foot transported masses of boulders and gravel kelp.

Milne drew attention to another factor which favoured coast-ice rather than icebergs as the agent leaving a permanent record of its action on the rocks. Any grooves and scratches left by icebergs were likely to be obliterated after the ocean bed had risen to become dry land whereas coast-ice, driven backwards and
forwards by the tides and advancing and retreating with the seasons, would be acting on a steadily increasing area, starting with what were now the highest points and subsequently affecting lower ground as the land mass rose.

Milne reiterated his views on the rôle of coast-ice in a paper published in 1877 in abridged form in which he pointed out that Finland too, bore evidence of a glaciological history similar to that of Newfoundland (47).

In further support of his theory, Milne drew attention to the fact that maps of northern Europe on which were indicated the general direction of ice-markings, showed the latter to be more or less at right-angles to the sea coasts. This was certainly as favourable to the view that the markings were due to coast-ice acting on a rising area as it was to the action of great ice-sheets.

He concluded:

"I will say that one thing seems to me to be certain — namely, that, even if we accept the most favourable views of large ice-caps, the appearance presented by many countries, which have hitherto been ascribed to their action, ought rather, for reasons already stated, to have been accredited to the action of coast-ice on a rising area." (48)

(d) Evidence of a Glacial Period in Japan

Various hypotheses were put forward in the latter part of the nineteenth century to account for the coming of the Ice Age and those which gained a firm measure of support held that the cause was not to be found locally on the earth’s surface but rather in changes in the amount of solar radiation falling on the earth or in changes in the earth’s orbit.

It occurred to Milne that if explanations along these lines were correct, there should be evidence of glaciation in the Eastern Hemisphere as well as the Western, but:

"... so far as I am aware it is but little that has been written about the evidences of a glacial period in the western portio of the Pacific Area, and about the evidences of this period in Japan not anything." (49)

He set out to correct this omission, his findings being published in 1881 (50). He pointed out that the most likely areas in which to seek evidence were the northern and western regions of Japan, since they were the coldest and bore accumulations of snow. Many of the highest mountains, however, were volcanic in
origin and geologically recent; they were unlikely to have existed during the Ice Age. This narrowed the field considerably and among those mountains which were not of recent volcanic origin some had snowfields and even small glaciers but only one was found which appeared to provide direct evidence of a glacial period (51). This mountain was Gwassan, in the north of Japan. It was 6,000 feet high and had snowfields up to 400 yards in length when seen in July, 1879. The core might have been old volcanic rock but the flanking schists indicated great age. Here Milne found rounded rocks which he took to be roches moutonnées. At the base of a steep slope of the mountain there was:

"... a small tract of gently sloping country with a very singular appearance .... The peculiarity of this country lies in its contour, which is that of a series of waves or hummocks, the average height of which may perhaps be 20 feet. The steep slopes of these in many cases were observed to point towards Gwassan. At the time I saw these undulations they were thickly covered with grass, and from this and also from the nature of the soil on which the grass grow, it was impossible for me to determine the nature of the rocks which lie beneath them. Their appearance was certainly very suggestive of glacial action, and not unlike the hummock districts we met with in Labrador and Newfoundland." (52)

Apart from this direct, but limited, evidence of former glaciation, Milne based his conclusions on indirect evidence of a somewhat speculative nature. This included the presence of terrace formations in northern Japan which he argued could have resulted from changes of cca level associated with retreat of water to the poles, the curious mixture of semi-tropical and palaearctic plants found in Japan and certain fossils found in alluvial deposits which indicated the extinction of tropical animals and the existence in southern Japan of forms of life which now existed only in the colder northern areas.

Milne concluded that a glacial period had taken place in Japan though to a much more limited extent than that associated with northern Europe; conditions were perhaps analogous to those in northern Spain at that time (53).

4. The Beke Expedition

This Expedition, led by C.T. Beke, took place in 1874 under the auspices of the Royal Geographical Society. Its purpose was to locate the true site of Mount
Sinai in N.W. Arabia. Milne was the Expedition's geologist and his observations led to the publication of two papers which appeared in 1874 and 1875 respectively.

Milne described the first of these papers as a note to accompany the specimens he collected in the Cairo area (54). He acknowledged that this region had already been described by several geologists and that there was little additional information to impart. It is therefore not proposed to discuss this paper in detail but merely to record that it was divided into four principal sections which described respectively the middens or rubbish heaps, the Moccattam Quarries, the Jebel Achmar hills and the approaches to the Petrified Forest.

His second paper was of greater interest (55). Observations began at Ras Sheik el Ballan, situated some 50 miles south of Suez on the coast of the Sinaitic Peninsula. Here the mountains were found to be of grey granite, consisting largely of quartz and black mica, with only a little felspar. They were cut by numerous dykes, appearing as broad red or dark-coloured bands, the red ones consisting of felsites and the others of felspathic porphyries. Milne also described unusual features in the ripple marks found in the sandy ridges running from the foot of the hills to the plains below (56).

Further south, Milne noted that the village of Tor had the very unusual feature of being built almost entirely of blocks of coral which had been obtained from large mounds of sand near-by. The fact that these mounds also contained shells in imperfect condition and arranged in an irregular manner suggested that this was the result of drift accumulation.

Investigating the area from Sherm to Ras Abou Mohammed, the most southern part of the Sinaitic Peninsula, Milne noted that the granite rocks retreated some six or seven miles back from the coast, their place being taken by low hills and cliffs of limestone and sandstone (57). East of Sherm harbour, there were cliffs of sand capped by two horizontal beds of limestone some 14 feet thick which on closer examination proved to be a breccia of shells and coral rather than a compact limestone. Beds of sand, dipping to the south at an angle of about 12°, were too friable to be termed a sandstone and in places assumed the character of a grit. Near-by, horizontal bands bends capping steep banks of sand were identified as a volcanic felspathic breccia, probably doleritic particles cemented by a triclinic felspar. With them were fragments of a coarse-grained black rock which Milne
found consisted of quartz and felspar cemented by limonite (58).

At Ras Abou Mohammed the cliffs were some 90 feet high and Milne found them to consist of the same coral limestone and dip in the same direction as that capping the sand at Sherm (59).

On reaching the mouth of the Gulf of Akaba, the Expedition embarked for the town of Akaba, landings being made on the way, giving Milne opportunities to investigate the geology of the areas concerned. The first call was at Madian which he noted was situated on the boundary between two sets of lithologically different rocks. Hitherto, both sides of the Gulf were bounded by granite hills, but here they gave way to beds of sandstone and conglomerate. He investigated both formations. The surface of the granite had weathered considerably; it contained a predominance of felspar both at the surface and in the deeper, more solid, parts of the formation, but its most prominent feature was the large number of dykes by which it was traversed. The strike of the latter was generally from north to south and they dipped steeply at 80° — 85° to the east. They were of two types, consisting respectively of dark-coloured coarse-grained prophyries and pink felsites or fine-grained porphyries. On the other side of the boundary strip was brecciated conglomerate containing pebbles and even large boulders. This gave way to a mixture of sandstone, grit and conglomerate. Milne collected and described 15 specimens of rocks from this area (60).

The next landing was made at Bir el Mashiyah. Here granite predominated and Milne noted evidence suggesting that the land in this area was rising, a phenomenon he associated with the volcanic nature of the adjoining peninsula of Arabia (61). Rock specimens were again collected and described (62).

Moving on to Akaba, Milne found that the rocks of the area presented the feature, familiar by now, of granite traversed by a large number of dykes. Of greater interest was the territory to the east of Akaba towards Wady Ithm. Here he investigated the mountain wadies, narrow defiles of considerable length which wound their way between the granite hills. After describing their appearance in some detail and considering the agencies at work on the rocks, he summarized his findings as follows:

"With regard, therefore, to the general appearance of the beds of these mountain wadies, it may be briefly stated, in conclusion, that their characters
are, in the main, rather due to a stream of sand than to water; small furrows formed in the central parts of the wady retreat towards the hills by being undermined and then falling by their weight. By this falling, boulders, often 20 feet in diameter, are rolled forward, and strewn across the plain from the hills towards a central line in which they accumulate. Whilst all this is going on, an almost continuous draft of air up or down these funnel-like defiles is in operation, carrying sand to polish the scattered debris, thus helping in the production of appearances not unlike those of some ancient river-bed, in which action it is aided by a slight trickling of water after the winter showers.” (63)

While in the area Milne made further interesting, though not original, observations on the weathering effects of sand-blasting (64); he also collected and briefly described 21 rock specimens from Wady Ithm (65).

Among the granitic hills between Akaba and Petra, Beke identified to his own satisfaction Mount Baghîr, or Jebel el Nûr, as the true Mount Sinai. Regarding it as a representative example of this range of hills, Milne provided a detailed description of its appearance and rocks (66). While on the summit of Mount Baghîr, he noticed some flat-topped hills which, he suspected, were either not granite at all or were formed of this rock and capped by some other material. Milne confirmed this observation when he climbed one of them, Mount Atagtagheer, on the summit of which he found two large patches of sandstone, each about 100 feet thick, deposited on granite (67).

With their purpose accomplished, the members of Beke’s Expedition made their way north-westwards towards Suez. Milne continued to make geological observations en route. The principal facts which emerged were that granite gave way to limestone, chalk and sandstone, with corresponding differences in topography. Some of the limestone was fossiliferous and from this Milne collected specimens of Echini, Pectines, Ostreoe and Nerinoea (68).

Milne concluded that it would not be advisable to make a definite statement about the identity of the geological horizons over which the Expedition passed because the speed of the journey had not allowed him to make sufficiently detailed observations for this purpose. However, the observations outlined above indicated that the succession was comparable with that found by Bauerman
5. The Minerals of Japan

During his first three years in Japan, Milne found time among his many other activities to collect and catalogue the country’s minerals. A paper based on his findings was published in 1880 (70). It included a list of 77 minerals compiled from material derived from four sources, namely specimens collected in the field during the course of extensive travels in the Japanese mainland and the Kurile Islands, specimens in the Mining Department’s collection of minerals, material exhibited at the National Exhibition of Japan of 1877, and minerals from the Kioto Exhibition of 1878. The list included 21 minerals described as rare, 11 as common and nine of doubtful identity. Of the latter group, Milne described four minerals in some detail since these were

“..... probably new varieties of old species, if not altogether new.” (71)

He investigated the nature of each of these four minerals using the routine tests and methods commonly employed for the systematic examination of specimens at that time. The first specimen, tentatively labelled vermiculite, was found to consist of short six-sided prisms about 6 mm. long and 3-4 mm. broad. The prisms were laminated at right angles to the long axis. Rough measurements of the angles indicated that the crystal system was rhombic. Cleavage was parallel to the basal pinacoid. Milne was unable to find any distinguishing optical properties. The streak was yellowish-brown and the cleavage surfaces had a blackish-green to brown colour and a brilliant but slightly pearly lustre. The hardness was found to be about 1.5, the specific gravity about 2.7 and the fusibility above 5. The mineral dissolved slowly in nitric and sulphuric acids, leaving a residue of silica; it dissolved more rapidly in hydrochloric acid, with the same result. When heated strongly it broke up into metallic-looking golden scales. Deposits of the mineral were found near Tsurasee in the beds of streams running down from granite hills (72).

The properties of the other three minerals were investigated by the same methods and were tentatively labelled fluorite, var. chlorophane, hisingerite, and wollastonite respectively (73).
6. *Crystallography*

The course of lectures given by Milne at the Imperial College of Engineering included crystallography and his lecture notes on this subject were published as a short textbook in 1879 (74). The Editor, Thomas Davies of the British Museum, included a Note which described the sequence of events leading to publication. He said:

“In the latter part of 1877, Prof. J. Milne sent home from Japan lithographed copies of his *written* Lecture - Notes on Crystallography and Crystallo-Physics - to Prof. N.S. Maskelyne, F.R.S., Dr. H. Woodward, F.R.S., Prof. J. Tennant, F.G.S., to the Editor, and other friends, with a request to me to publish the same in the GEOLOGICAL MAGAZINE, or elsewhere.

Owing to the absence of the Author and from other causes, a long delay has occurred in presenting them to the scientific public in their present form; and it is only due to Prof. Milne to state that these notes (*as now printed*) were completed, and lithographed by his Japanese assistant, in 1877.” (75)

The book consisted of some 70 pages and was based largely on material published in existing textbooks on the subject.

In addition to this, Milne carried out one piece of experimental work on the elasticity of crystals, the results of which were published in 1879 (76). After reviewing briefly the experimental evidence on the conduction of heat and electricity, the passage of light, the dilatation, and the magnetic properties of crystals in relation to their axes of symmetry, Milne wrote in this paper:

“..... in order to more fully appreciate the above coincidences, we should be greatly assisted if we could shew that along the directions which in any crystals exhibit different phenomena, that there was also a difference in material elasticity, which probably means difference in intermolecular space or a difference in density. The chief experiments which have a direct connection with the material elasticity of crystals, are those made by Savart, who shewed that the figures formed upon vibrating plates of crystals were directly connected with their optical axes. Lately, I have endeavoured to shew that the material elasticity in a crystal was different in different directions, and at the same time to give some idea of its relative values by bumping together spheres cut out of calcite and quartz.” (77)
Milne began his experiments by placing a calcite ball of diameter about 23.7 mm. at the bottom of a cycloidal groove and allowed a quartz ball of diameter about 26.0 mm. to roll down the groove from a fixed point and strike the lower ball. This procedure was repeated, striking the calcite ball at different points on its surface each time. He said of the results:

"Although the calcite ball was observed in consequence of being struck to roll different distances, indicating that these might be due to differences of elasticity in different directions, the experiments were too crude to be worth noting." (78).

An improved experimental method was then devised using the same quartz ball as the bob of a pendulum, the suspension consisting of a silk fibre 6 feet 5¾ inches long. When at rest this ball just touched the calcite ball, which was fixed in wood. The quartz ball was drawn back to a given distance and released, the distance of rebound being noted. The experiment was repeated with the calcite ball presenting different parts of its surface to the quartz ball. The results showed that similar distances of rebound were obtained from points diametrically opposed on the surface of the calcite ball and that the greatest rebound was obtained along the direction of no double refraction (79).

Three further series of experiments were carried out on similar lines, the principal difference being that the ball struck by the pendulum was placed on a billiard table and allowed to move after impact, the distance travelled being noted in each case. In the last experimental series, two quartz balls were used, the replacement for the calcite ball having a mean diameter of 36.8 mm. (80).

Milne concluded:

"From the above experiments it would seem to be shewn that crystals have different material elasticities in different directions. In the case of a ball of calcite, the greatest rolling effect was obtained when it was struck parallel to the principal axis of the crystal.

In a direction at right angles to this the least effect was obtained, and in intermediate directions, intermediate effects.

Before endeavouring to shew what relations numbers such as those which I have obtained hold to the elasticity of a crystal, it would be better that such experiments were repeated by persons who have instruments, good materials,
and skilful workmen at their command, which I am sorry to say I have found it impossible to obtain in Japan.” (81)

7. Mining

Milne’s first published work in this field was a pamphlet, printed by the Imperial College of Engineering, for the use of his students (82). The need for this work is seen from the opening paragraph:

“It often happens that students visiting mines omit to gather all the information which they have good opportunities of obtaining merely from the want of a proper system of enquiry and of collecting facts, a misfortune which is the more observable when the visits are short. To obviate this, the following notes have been prepared in order to indicate leading points to be observed.” (83)

The pamphlet was thus on the lines of a chapter in a textbook on mining; Milne claimed no originality for the material it contained and provided a list of sources used in its preparation (84).

A second pamphlet prepared by Milne and dealing with the ventilation of mines was also a College publication; it appeared in the following year (85). Again, it was intended to serve the same purpose as textbook.

These two pamphlets later appeared as part of a much more extensive work by Milne, The Miner’s Handbook (86). No originality was claimed for the material it contained. The work was divided into three parts, namely, Mineral Deposits, Mining Operations, and Ore Dressing. The first part consisted of the pamphlet of 1878 while the second pamphlet was included as one of the sections into which the second part of the work was divided.

Natural History and Archaeology

There is no evidence that Milne had received formal training in the biological sciences or in archaeology; his work within these disciplines was that of a gifted amateur. It was carried out concurrently with his geological investigations and provides further examples of the considerable breadth of his interests and abilities.
Relics of the Great Auk

Reference has been made to Milne’s visit to Funk Island, situated in the Atlantic Ocean some 32 miles off the coast of Newfoundland. The visit was made by schooner on 24th July in either 1873 or 1874 (87) and was described in a paper published in 1875 (88).

The island was unpopulated by man but was rich in bird life, among which Milne identified colonies of terns, puffins and guillemots. He described the rock of which it was composed as:

“..... a highly felspathic pinkish granite, the small quantity of mica it contains being of a black colour. From its lithological resemblance to the rocks on the nearest mainland, its geological age is probably Laurentian.” (89)

Apart from grass:

“..... the most noticeable plants were the Alexander (Haloscias scoticum), and a Cochlearia (C. fenestrata), this latter occurring both as a flowering plant and a seedling. A fragment of chickweed (Stellaria media) was also discovered. In addition to these were some tufts of a plantain (Plantago maritima) and a common dock.” (90)

In the grass Milne found specimens of the beetle Pterostichus (Platysma) Luczotii and the butterfly Pieris oleracca. Eggs of the birds referred to above and also of the razorbill were found almost everywhere together with a superficial layer of guano which contained many fragments of bones, some of them being relics of the Great Auk (Alca impennis), an extinct flightless bird.

Milne was fortunate in finding during the very limited time available a spot, covered by up to two feet of turf, where fragments of at least 50 of the birds were buried. He collected many pieces of bone but:

“The remains, on the whole, are of such a fragmentary nature, that it is probable they will hardly suffice to complete a perfect skeleton. The collection which has been made, notwithstanding its deficiencies, will nevertheless be sufficient to throw considerable light upon the osteology of the bird.” (91)

After reviewing possible reasons for the bird becoming extinct, Milne provided a résumé of facts recorded about the Great Auk arranged chronologically for each of the following regions: North and N.E. Europe, Great Britain, The
Faroe Islands, Iceland, North America and Greenland (92). He also provided a useful bibliography of scientific works on this bird (93).

2. The Stone Age in Japan

Milne travelled extensively in Japan, investigating the numerous volcanoes and the geology of the country in general. In the course of his journeys he gleaned some interesting archaeological material which he presented in two papers, the first of which appeared in a highly abridged form in 1879, followed two years later by the complete version (94).

In the first paper, Milne described the artifacts of Stone Age man in Japan and gave details of a dating process based on geologically recent and rapid processes. There were three principal sources of material, namely, kitchen middens or shell heaps, tumuli, and caves, both natural and artificial.

The middens consisted of a layer of earth varying in depth from a few inches to about two feet, beneath which was a band of shells, all of them opened and many broken, this layer varying in thickness from six inches to three or four feet. Milne identified and named 15 genera of shell-fish from these pieces and found also many fragments of deer, bear, bird and fish bones (95). Of greater interest, were the numerous fragments of pottery which had apparently once formed vase-like vessels. The fragments were unglazed and only partially baked; they were about ½ inch thick. An illustration provided in his paper showing typical fragments of this pottery and also a complete vase, is reproduced here. The impressed punctated markings shown in fragment 1 of the plate were a common feature. Incised lines, possibly made with a stick or a sharp flint were found in some pieces. They are shown arranged obliquely to form a pattern in fragment 6. Occasionally, markings in relief known as cordmarks were found as seen in fragment 8. The complete vase no. 9 in fig. 5, was found by Milne at Nemoro. It was 131 cm. high, 8.5 cm. across at the mouth and 5.5 cm. wide at the base (96).

Stone implements, in the form of arrow-heads, axes, chisels and scrapers were also found and described in detail by Milne (97). They bore a striking resemblance to those found in European countries.

Milne referred briefly to tumuli, many of which had traditional associations. He appears to have investigated only one tumulus, at Macpherson's Hill near
Yokohama. This was 20 to 30 feet high and some 50 to 60 yards in diameter. Digging uncovered a few roughly-chipped celts (98). Likewise, Milne made no significant discoveries in natural or artificial caves, but referred to finds made in

Fig. 5: Stone Age pottery from Japan
Comparison of markings on the fragments of pottery with those found on the modern pottery of the Ainos (100) revealed distinct resemblances and what was known of the early history of Japan tended to confirm Milne's conclusion that the artifacts he found were of Aino origin (101).

Milne drew attention to the fact that the prehistoric middens were found slightly elevated above sea level and could be situated up to half a mile from the shore line. Shell heaps were still being formed in his day by fishermen who lived very close to the sea and subsisted largely on shell and other fish. The heaps were invariably situated between the fisherman's dwellings and the sea. This indicated that since the prehistoric middens had been laid down, either the land had risen slightly, or silting-up had taken place along the shore line, the land encroaching on the sea (102).

He found that both these processes were taking place in Yedo Bay. There was a vast amount of mud in suspension brought down to the Bay by the many rivers which entered it. In places, the sediment accumulated visibly even in the lifetime of an individual. Borings made at an earlier time in the faces of cliffs at Mississippi Bay and beyond Yokoska to record high water-mark were now many feet above the present high water-mark, indicating that the cliffs had been elevated in recent times. The recorded history of the area and an examination of old maps confirmed this evidence of rapid encroachment of the land on the sea. Evidence of elevation was also noted by Milne in the Yezo area and evidence of accumulation of sediment was found at Niigata and Osaka (103).

Knowing approximate rates at which the land was gaining on the sea, Milne estimated that the middens at Omori were on the seaboard at the most 3,000 years ago or at the least 1,500 years ago.

"History tells us that about 2,500 years ago Jinmu Tenno came to Japan and fought against the Ainos. It is therefore very probable that the Omori deposit was formed about the period when we know from history that Japan was inhabited by the Ainos." (104)

3. Discoveries at Otaru and Hakodate

Milne's second archaeological paper presented an account of his findings
further north at Otaru and Hakodate, places situated on the west coast of the island of Hokkaido (105).

The relics at Otaru were considered under the three headings of pits, inscriptions, and middens. In his previous paper Milne had referred briefly to the traces of pit-dwellers found on the island of Nemoro. He had seen but not explored these pits and had merely offered the opinion that they might have been the habitations of Kamschadales or Alutes, people who still lived in houses partly sunk into the ground. In Otaru, the pits were roughly conical in shape, some eight feet in diameter and three feet deep. Despite the tradition associated with them of having been formerly inhabited, he concluded that the pits were merely the result of agricultural processes, possibly the removal of tree stumps (106).

Ancient inscriptions were found carved on the face of a cliff at a height of some three or four feet above the ground. The characters were about an inch broad and half an inch deep and occupied a strip of rock about eight feet long. They were not Japanese and after considering a number of possible explanations of their origin, Milne concluded that they were likely to have been made by the Ainons (107).

Mounds and kitchen middens yielded a rich harvest of stone implements and pottery fragments. Milne provided detailed descriptions of representative examples of these artifacts which showed them to be very similar to those described in his previous paper (108). The latter had included descriptions of finds made by Milne at Hakodate but since then road-making and the laying out of public gardens had led to further discoveries of much prehistoric material, some of which he had obtained and was described in the later paper under review. Again, the material consisted of stone implements and fragments of pottery. From the comparative roughness of the latter, its glossy surface and its being unearthed at greater depth, led Milne to conclude that the Hakodate material was decidedly older than the Otaru fragments (109).

Volcanology

Milne became interested in volcanoes while still a schoolboy. Reading about them seems to have inspired his visit to Vatna Jokul in Iceland, to which reference
has already been made. It is thus not surprising that soon after taking up residence in Japan he began to explore the volcanic areas of that country. His general interest soon developed into systematic study.

This renewal of interest can be traced to January 1877 when he observed from close quarters an impressive eruption, an event considered in the first division of this section. The second division refers to Milne's investigations of the volcanoes of the Kurile Islands. His interesting work on the shape of regularly-formed volcanic cones is described in the third division while the fourth division provides an account of his extensive catalogue of Japanese volcanoes and eruptions.

1. The Oshima Eruption

In 1877, Milne published an account of an eruption he had witnessed in January of that year (110). The scientific content of this paper was small and it will accordingly be discussed only briefly.

The erupting volcano was situated on the island of Oshima, some 40 miles south of the Bay of Yedo, and the most northerly member of a chain of volcanic islands stretching out into the Pacific Ocean.

The position of the volcano in relation to its immediate surroundings was such that an excellent view of the vent was provided from higher ground. The diagrammatic section shown here is reproduced from Milne's paper (111). Milne's

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Approximate section of the upper portion of Oshima.

- S—A, Outer slope of old crater-wall. (prehistoric?)
- B, Floor of old crater, partly filled by later eruptions.
- C, D, F, Rim of modern crater.
- E, New volcanic cone formed by present eruption, January, 1877.

Fig. 6 Diagrammatic section of Oshima
party approached the erupting cone E by way of the old crater wall SA, the floor of the old crater B, and the outer slope BC of a modern crater. The latter was described as:

“.... an amphitheatre of rocks about half a mile in breadth, the walls of which, upon the opposite side, were about 300 feet in height. At the bottom of this pit, on the side nearer to us, a small cone, with an orifice of about 50 feet in diameter, was belching masses of molten lava to a height more than double that at which we were standing.” (112)

His account of the visible processes of eruption was graphic and worthy of note:

“In the intervals between the ejections the interior could be well seen, and it was observable that the sides had a slope of very nearly the same inclination as the exterior. Now and then large masses of these interior sides, which were black, would slide down towards the throat of the crater, and reveal a red-hot interior, showing that the cone itself was probably internally red-hot throughout. One side of the cone had been blown away, leaving a breach, almost level with the plane from which it rose. This opening greatly facilitated our observations. Looking down on the crater from this side, molten lava, approximately level with the base of the cone, could be seen. At each explosion it rose in waves, and swayed about heavily like a huge basin of mercury, a little of it being apparently pushed forward through the breach to add to a small black-looking stream upon the outside. The explosions, which I have referred to several times as resembling outbursts of steam, might be compared to the escape of steam from a slowly-working non-condensing steam engine greatly magnified.” (113)

2. The Kurile Islands System

The Kurile Islands form a chain stretching from Kamchatka to north-east Japan. Milne cruised among them during the summer of 1878 to observe and note the positions of the numerous volcanoes found on the islands. His findings were published in the following year (114). Altogether he provided notes on 31 named islands; between them they included 52 well-defined volcanic peaks of which he was certain that nine were active. His sketch map showing the positions of the volcanoes is reproduced here (115).
Fig. 7: Volcanoes of the Kurile Islands
Milne concluded from his investigations that the volcanic mountains of the Kurile Islands were probably of more recent date than those in Kamtschatka (116) to the north or in Japan to the south. The volcanoes of the Kuriles had suffered far less denudation than those in Japan, with the result that their sides were still covered with ashes and they had maintained their original slopes.

Specimens collected showed that the erupted rocks were augitic andesites and were characteristic of those found in the volcanic areas of Kamtschatka, Japan, Java and New Zealand. There was a striking absence of lava streams and some evidence that the activity of the Kurile chain was fast becoming spent (117).

3. The Form of Volcanoes

The external form and internal structure of volcanoes vary considerably but the best-known type, of which the Japanese mountain Fujiyama is a characteristic and beautiful example, is built up by the accumulation of ejected material round a central vent.

Milne became especially interested in the external form of this type of volcano and published an account of his observations and conclusions in 1878 (118). In this paper, after commenting on the ruggedness and lack of any characteristic form of volcanoes in Iceland and South America, he went on to say:

"With other volcanos (119) which have been built up according to the formula of our text-books, that is, by the ejection and accumulation of material round a central vent, the case is different. I will endeavour to show that such mountains which, for the want of a more accurate term, have been called conical, have a particular kind of regularity which does not appear to have been hitherto noticed." (120)

As representative Japanese examples of volcanic mountains of this type Milne named Fusiyama (Fujiyama), Ganjosan, Chokaisan, Twakisan and Kamagatake, from which he selected the first and the last for study of the profiles. These mountains appeared to be sections of cones near the summits, but lower down the profile swept outwards in each case to form a much gentler curve. Before proceeding to analyse these very fine examples, it was important not to ignore less regular examples but to consider the factors which influenced regularity of form; these could be divided into seven groups, namely:
1. The position of the crater - if this was central and remained so during successive eruptions, the mountain should be regular in form.

2. Irregular or uneven eruptions - regularity in form would be lost if a paroxysmal outburst removed part of the cone or if lava erupted unevently.

3. Parasitic craters - eruptions from the sides of a mountain destroyed regularity of form.

4. Asymmetry of lapilli projection - the direction in which lapilli and similar material was flung out was sometimes more to one side than another, even in the case of a central event.

5. Wind direction - a strong wind at the time of eruption could result in the lapilli accumulating asymmetrically.

6. Nature of the erupted material - the size, specific gravity, and porosity of the material would influence the way in which it accumulated and consolidated.

7. Denudation — this process was constantly drawing material to lower levels, sculpturing and modifying the original contours. Since its action was much the same on all sides of the mountain, the process tended not so much to destroy the regularity as to alter the character of the slope. The tendency was to make the slope steeper nearer the top of the volcano than lower down (121). Milne began his analysis of regular profiles by comparing the slopes of volcanoes with those of railway cuttings (122). The slopes taken up by the latter depended on the material through which they ran; it was important to note that any given material was composed of particles having much the same size. In the case of volcanoes the material consisted of particles having quite different sizes but Milne had noticed that along a given contour they were invariably uniform in size, the larger particles coming to rest near the base. He considered that the theory and rules given by Rankine in the latter’s *Investigations about Earthworks* could be applied.

“As there does not appear to be any formula given in books on engineering, respecting the slope or form which would be assumed by a heap of loose dirt, an engineering friend has shown me that it follows from Rankine’s theory,
notwithstanding that the same is incomplete, that the surface is that which would be produced by a simple logarithmic curve revolving about an axis, consequently such a curve would have a slope diminishing from the top to the bottom.” (123)

Milne next traced profiles of Fusiyama and Kumagatake from large photographs and inserted the vertical axis of symmetry. Along this he marked points five millimetres apart and through them drew a series of parallel lines at right angles to the axis. The result is shown on the next page, reproduced from his paper (124). The curves shown are as follows:

![Fig. 8: Profiles of volcanoes traced from photographs](image)

I. Upper part of Fusiyama, taken from Marayama, S.W. side.
II. Fusiyama, from near Hitoana, west side.
III. Fusiyama, from Suyama and Gotemba, S.E. side.
IV. Kumagatake, as seen from near Hakodate.
V. Monte Somma and Vesuvius.

The shaded portions show irregularities in curvature. The broken line on the right of II is a logarithmic curve drawn for comparison.

Milne made measurements from these curves of which this table represents the
Table 2: Measurements and Calculations from Profile I

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</table>

Commenting on this table, Milne wrote:

"Now it is found, as will be seen by looking at the fourth column, that \( \Delta R \) is equal to a number which is nearly constant, which is the peculiarity of a logarithmic curve." (126)

"That this result should agree with that obtained for the stability of a self-supporting mass of loose materials is very striking, and seems to show, notwithstanding its roughness and the observations to which it has been applied, an invariable occurrence." (127)

Milne noted that the greatest departures from the more or less constant values for \( R/\Delta R \) were due to variations in curvature near the summit and at the base of the volcanoes, and suggested that they arose not only from external causes but also from the presence of an internal core (129).
4. *Catalogue of Japanese volcanoes and eruptions*

Milne's last published work on volcanoes appeared in 1886. It was a lengthy paper which was primarily a catalogues of the volcanoes of Japan with details of their known eruptions throughout recorded history (130). Descriptions of the volcanoes and details of their geology were based on his field work; details of past eruptions were derived from an examination of the literature, much of it in Japanese manuscripts and printed books. A bibliography was provided (131).

The scientific importance of the work lay principally in the conclusions that could be drawn from it about the geographical distribution of volcanoes in Japan and their frequency of eruption. It allowed Milne to mark on a map of Japan

![Fig. 9 The Volcanoes of Japan](image-url)
those regions where volcanic rocks were found and the positions of active volcanoes. This map is reproduced here (132). A summary of the detailed information on which it was based was provided in the form of three tables listing respectively the volcanoes of the Kurile Islands, those of Yezo, and those in the regions of Honshiu and Kiushiu. In addition to giving the names of the volcanoes, the tables provided in many instances the heights of the mountains, the nature of the rocks, details of the craters and dates of the last eruptions (133).

The number of mountains which were easily recognizable as being of volcanic origin was 129, of which 51 showed traces of activity. Of the latter, 27 were situated in the northern regions of the country, the remainder being found in the central and southern regions.

Milne also tabulated eruptions in relation to the months and seasons of the year. He found that of the 233 eruptions listed, 80 occurred in the winter months, 73 in the summer months, and the remaining 80 at unknown times during the year. Eruptions took place more frequently in February and April. He attempted to explain this result thus:

"This winter frequency may possibly be accounted for in the same manner that Dr. Knott accounted for the winter frequency of earthquakes. During the winter months the average barometric gradient across Japan is steeper than in summer. This, coupled with the piling up of snow in the northern regions, gives rise to long continued stresses, in consequence of which certain lines of weakness of the earth's crust are more prepared to give way during the winter months than they are in summer." (134)

Milne was later to refute reasoning of this kind when he considered the apparent frequency of earthquakes in relation to the months and seasons.

Looked at generally, the volcanoes of Japan formed a long chain running from N.E. to S.W. A closer examination of the distribution of the vents showed that the system could be divided into four divisions, namely, a N.E. - S.W. line running from Kamchatka through the Kuriles and northern Yezo, a curved line following the back-bone of Honschiu and terminating on the western side of the Yezo anticlinal, the N.N.W. - S.S.E. line of the Oshima group passing from Landrones to Fujisan and intersecting the line through Honschiu, and finally the Satsuma line running from the Phillipines through Sakurajima and culminating in
Mount Aso.

The most recent volcanoes were those which formed, or were situated on, the islands of the Kuriles, the Oshima group, and in the Satsuma Sea. Many of these islands had been formed during the period of recorded history (135).

The volcanic rocks were mostly andesites. Those containing augite approximated closely to basalts. Hornblende andesite was common, some containing free quartz. Quartz trachytes occurred in the north of the country. The presence of magnetite resulted in many of the lavas being magnetic, some of them markedly so (136).

Periods of more intense volcanic activity in Japan coincided with similar activity elsewhere in the world; one of the most active periods of this nature in recent times occurred in 1780-1800 (137).

Milne concluded his paper by referring to his earlier work on the logarithmic curvature of regularly-formed volcanoes and to the work of G.F. Becker who had since taken Milne's analysis a stage further (138).

Conclusion

In his first professional task of reporting on the mineral resources of Newfoundland, Milne was aided by quite comprehensive reports on the geology of the island prepared earlier by Alexander Murray. Thus Milne was in no sense a pioneer of this field. His contributions were in the nature of filling in details, rather than elucidating a geological system. Within that framework, however, his discoveries were by no means insignificant. In particular, attention should be drawn to his discovery of a large deposit of staurolite at Facheux Bay, his elucidation of the outcrop of Carboniferous rocks between Cape Anguille and St. George's Bay, and his discovery of a large deposit of galena in the Port-au-Prince Peninsula.

Milne's survey of the petrology of Newfoundland was again one which led to further knowledge being gained in matters of detail, but in this instance it is not possible to select any discoveries of special significance.

In glaciology, the position was different; this subject was comparatively new, having emerged as a separate field of study only since about 1837 due to the work
of Agassiz (1807-1873). Little thought had been given to the migration of icebergs or to their stability. The explanation put forward by Milne for the time of arrival of icebergs in Newfoundland waters was based on cogent arguments, and by considering the conditions required for stable equilibrium of an iceberg, he successfully disproved the view prevailing at that time that the submerged part of an iceberg was of great depth compared with the height of the exposed part above the surface. His views on the rôle of coast-ice were original and stimulating and provided an interesting field for further study.

Milne’s paper on the evidence for a glacial period in Japan contained much that was speculative but if firm conclusions based on sound evidence were lacking, his arguments served the useful purpose of opening a new field of study, since no previous work along these lines had been carried out previously in that country.

The comparatively rapid pace at which members of the Beke Expedition reached their objective precluded Milne from making thorough geological surveys of the regions through which it passed but he nevertheless amassed much information about the geology of little-known regions and was able to show that the succession was comparable with that deduced from a more detailed survey carried out further to the south by Bauerman.

The importance of Milne’s study of Japanese mineralogy lay in his discovery of four minerals previously unrecognized in that country, namely, vermiculite, fluorite, hisingerite and wollastonite. His experiments on the elasticity of crystals, made at about the same time, clearly indicated anisotropy associated with the positions of the crystallographic axes. The experiments were too crude, however, to allow him to determine the elastic moduli of the materials.

Milne’s contributions to mining were of a pedagogic nature, his pamphlets and text-book being written with the special needs of his Japanese students in mind.

In the field of natural history, Milne contributed to the existing knowledge of the flora and fauna of Funk Island, but in particular his observations on the relics of the Great Auk were worthy of note. His contributions to the archaeology of Stone Age Japan lay less with the nature of the material uncovered as with his ingenious dating of the Omori middens leading to the strong supposition that they
were Ainu origin.

Milne's first contribution of note to volcanology was his elucidation of the distribution of volcanoes in the Kurile Islands. This was followed by his demonstration that the profiles of volcanic mountains of the type built up by the accumulation of ejected material round a central vent were logarithmic in shape and not sections of a cone. His painstaking and lengthy task of cataloguing Japanese volcanoes and their eruptions throughout recorded history led to his most important contribution in this field, a complete account of the distribution of volcanic areas in Japan.

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98) ————, ibid., 404.
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100) A fast-diminishing minority race in Japan now confined to the northernmost Island of Hokkaido. The usual spelling is Ainu.
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102) ————, ibid., pp.413-414.
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Chemical and Physical Models for Atomistic Notion – Its Conceptual Development in Relation to the Evolution of the Concept of Chemical Substance. 
A Contribution to the History of Atomism (IV)

Minoru Tanaka*

Introductory Discussion

The author presents, in continuation of his foregoing articles on the history of atomism(1), a theoretical consideration on the development of atomistic notion, particularly that in chemistry, in its relation to the evolution of the concept "chemical substance".

The latter part of this problem is considered to be an independent issue to be studied, because the chronological division of the history of chemistry from theoretical point-of-view should depend on the evolution of matter cognition. The chronological division of chemistry has been usually made more or less conventionally, but not in some logical sequence. A classical example is that of H. Kopp in his "Geschichte der Chemie". He depicted the development of chemistry by setting five periods, namely: (1) ancient chemistry, (2) Alchemy, (3) Iatrochemistry, (4) phlogiston theory and (5) quantititative chemistry. If we devide the last period into (5) Lavoisier's system of chemistry and (6) the Daltonian atomic theory, the whole range of chemistry until the beginning of its modern stage may be covered completely. Such mode of period division, although it is more or less convenient to bear in mind main events as well as to describe cultural-historical features of prehistorical stages of chemistry, it lacks logical consistency underlying the whole development of this science. The chemical atomic theory of Dalton was obviously the logical consequence of the Lavoisier's

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concept of chemical element. Then what could lead to the establishment of the latter? Was it merely a series of experimental discoveries like those of oxygen gas and its role in combustion and those of various new metals as well as their compounds or was it some basic chemical concept? If we take into consideration a general fact that experimental discoveries were led in many cases by some underlying basic ideas, it would be rather natural for logical understanding of history of chemistry to search for some fundamental concepts of substance being comparable with that of chemical element and responsible to the birth of the latter. Such is the first task of this article.

The development of atomistic concepts in chemistry to explain behavior and structure of matter occurred through several essentially different but logically continuous stages of scientific studies. The Daltonian atom was apparently descended from the corpuscle of Boyle and Newton. But they had different backgrounds. The Daltonian atom was without doubt directly born from the Lavoisier's chemical element, while it is problematical whether the Boyle's corpuscle had been based on some definite experimental concepts concerning behavior and sort of substances. The development from the Boyle's corpuscle to the Daltonian atom lacks apparently logical continuity. However, there are sufficient reasons to believe that the whole process of this development took place in one continuity. To make clear not only the differences and characteristics of each stage of atomistic concept, but also their common features and inherent continuity is the main task of this article. Although this problem has been treated by the author in his foregoing articles, the present article is based on a new consideration on the relation of atomistic notion to chemical concept of substance. It includes also revision and extension of his preceding discussions.

1. Pre-modern conceptual evolution of chemical substance

The concept "chemical element" is nowadays defined by that of atom. Historically, reciprocal relation between those two basic concepts was reversed, namely, "chemical element" characterized by its conservation and definite combining weight yielded "chemical atom" characterized by its relative weight.
In the Lavoisier's chemistry "compound" and "element" were coupled concepts. The compound is composed of elements and each element is a component of compound. The concept "component" is not, of course, identical with that of element, but broader than the latter. It must be emphasized that already before the establishment of the concept of chemical element by Lavoisier that of the component should have worked effectively to express such empirical fact that one sort of body yields by qualitative change more than one sort of other bodies or reversely the former is produced from the latters. The latters were to be conceived as the components of the former.

The said empirical concept of "component" should have arisen coupled with that of "chemical species." According to L. Pauling ("General Chemistry") the chemical species is to define as substance with a chemical composition which may be regarded as definite. The modern quantitative concept of "chemical composition" has its pre-modern, qualitative and empirical predecessor, namely "component" in the above-mentioned meaning. Such pre-modern concept may have been derived from a series of chemical operations like forming a body by combining two or more different bodies and vice versa (chemical combination and decomposition) and analysis (extracting a known body from another by qualitative change of the latter) with aid of procedures like heating, distillation, and so on. This concept "chemical species" differs itself from that of "substance species." The latter in its pre-modern meaning may have been derived through identifications according to apparent properties, like color, luster, smell, taste, form, solubility in water or other liquids, behavior under heat, dissolution in acids and so on.

Thus, we see the logical evolution of concept of substance, namely from somewhat vague, sensual and purely descriptive concept of "substance species" to more or less analytical and abstract concept of "chemical species" coupled with that of "component". Such is the pre-condition for the birth of the modern concept of chemical element.

It must be added that the purely empirical concept of chemical substance could be formed without or before introduction of that of chemical element in its meaning of Lavoisier. If we define several homogeneous bodies with one and the same group of specific properties as belonging to one and the same "phase" and
distinguish the “pure phase” from the “solution phase”, then the chemical substance is to be characterized according to the following proposition: When one pure phase may undergo any change into another single phase, these two phases consist of one and the same substance.(2)

2. Development of chemical cognition during
the pre-historical stages of chemistry

The author has, in his preceding article on the role of Boyle’s atomistic theory for his experimental chemical studies(3), proposed to introduce a “natural-historical stage” in the pre-history of chemistry. This stage of chemistry was explained by him as follows: “The main feature of natural-historical stage of chemistry consists in purely empirical distinction of substance-species. Distinction of substances through identification of various apparent properties, separation and purification of single substances from others, and further, their classification according to their remarkable properties (like metals, earths, acids, alkalies, salts though in vague notions) — these are the most basic, though naïve, task for chemistry. ... Such primitive concept of substance species combined with various procedures for recognizing them were gradually accumulated by metallurgical and pharmaceutical practice during the age of Alchemy and iatrochemistry and were refined to more or less logical cognition which served as starting point for scientific chemistry.” He argued that this progress of chemistry made in the time of Boyle owed much to newly developed analytical thinking about the nature of chemical change supported by invention of pneumatic apparatus and to some degree systematized procedure of chemical analysis.

Already during the time of Alchemy we find in Geber’s writing, “Summa Perfectionis”, one of the most authoritative alchemical books of presumably European origin, not few significant descriptions, which suggest that “Geber” was well accustomed to logical cognition of experienced chemical facts. M.Stillman described the main feature of Geber’s writing as follows(4): “the author is a man of practical experience in manipulations of chemistry and not a mere compiler or editor of authorities ... he is animated with the desire to explain experimental methods and apparatus so clearly that others may profit by his experience.”
Regarding the concept "substance species" in the above-mentioned meaning, Geber's accurate understanding may be proved by many passages from chapters on properties of metals and their assaying procedures. For example, in part 3, §97 of "Summa Perfectionis" (on Assaying — On Calcination and Reduction) he writes as follows (German translation by Darmstaedter)\(^{(5)}\): "Wir behandeln jetzt das Probieren durch wiederholte Calcination und Reduktion. Die vollkommenen (edlen) Metalle verlieren bei wiederholter Calcination und Reduktion nichts von den Merkmalen ihrer Vollkommenheit, wie Farbe, Gewicht, Glanz und Masse, so oft man auch diese Operationen wiederholt. Wenn also irgendwelche Metalle bei wiederholter Calcination und Reduktion aus ihrem Kalk etwas von ihren guten Merkmalen verlieren, so muss man annehmen, dass das Ergebnis des Künstlers trügerisch ist."

Geber thus declares that any metallic body cannot be identified with genuine gold or silver, when the sample lacks even one of "gute Merkmale" of the former. This statement coincides exactly with modern definition of substance, for example with that given by Wilhelm Ostwald in his "Prinzipien der Chemie". It must be taken into consideration that such logically accurate concept of "substance species" could be formulated with help of logical thinking presumably trained through adapting to scholastic and classical philosophical works.

We may now turn to the formation of the concept "chemical species" coupled with that of "component". To consider the problem, what was the progress of chemical cognition attained during the period of phlogiston chemistry, namely after the time of Boyle, Mayow and others, it must be emphasized that the hypothetical phlogiston was correlated with the correct idea that combustion and calcination belong to an essentially same phenomenon. Whether the phlogiston is in strict meaning to be regarded as "component" of metals and combustible substances is problematical. The empirically known increase of weight by calcination of metals meant no hindrance for the consequentiality of the theory, at least for phlogistonians. Its function resembled the "mercury" and "sulphur" of alchemical notion. However, the phlogiston was for some advocates a real constituent, because it could be liberated and caught as "inflammable air" from metals by action of mineral acids and calx could be
reduced to metals by heating with charcoals which were assumed to be rich in phlogiston. The weakness of phlogiston as real body contained in combustible substances is to be seen in its lack of weight, the basic property of matter in general. It must be emphasized that the idea of the conservation of matter had been since long almost axiomatically accepted and worked as guiding principle for not few chemical and physical studies. The outstanding example is the famous discovery of “fixed air” by J.Black.

The leading idea of Black’s experiment performed by a series of quantitative determination of participating substances was that a group of substances, like magnesia alba, lime stone and mild alkali, was composed of a hidden gaseous substance and a solid alkaline substance. Thus his discovery meant not only that “fixed air” was a new substance species but that it was a component fixed in mild alkalies and other bodies. In this meaning magnesia alba was recognized as a “chemical species” whose components are magnesia, fixed air and water, and mild alkali was another chemical species composed of caustic alkali and fixed air. These components characterize themselves by the fact that they are ponderable bodies which are able to be extracted from composite bodies by some appropriate chemical procedures.

These ideas of J.Black, a non-phlogistonian chemist, must have been more or less shared by preceding and contemporary phlogistonians. If it were not for such notions, though not always definitely conscious, discovery and preparations of many new substances, particularly those achieved by prominent chemists like Scheele, as well as positive effort to establish ideas on chemical affinity by Geoffroy and others would have not been attained. These very achievements of experimental nature as well as the underlying new concepts of substance formed the pre-condition for the birth of truly scientific chemistry by Lavoisier. It must be added that the said conceptions of chemical species and component did not belong exclusively to the period of phlogiston theory, as, for example, Tachenius had already during the period of iatrochemistry given a clear definition of salt: “all salts are compound of two parts, acid and alkali.” It would be interesting problem for history of chemistry to trace the origin and development of the concept “component” of chemical substance.
Chemical and Physical Models for Atomistic Notion

3. Role and character of Boylean corpuscle for chemistry.

It is true that the atomism as an effective working hypothesis in the sense of modern science began with the Dalton’s theory. However, this does not exclude the possibility that pre-Daltonian atomic theory could in some way correspond to experimental knowledges about matter and could serve as a leading idea in search of experimental chemical knowledges.

R. Boyle developed his ideas of the corpuscles in his “Sceptical Chymist” as follows: The basic matter, namely “universal matter”, exists in form of “little particles of several sizes and shapes”, which in turn unite with each other so as to form larger particles, “primary concretions”. The last mentioned particles, which are difficult to split into the original “little particles”, are the direct constituents of bodies.

This idea of Boyle’s particle, which may be called “structural corpuscle”, seems to be a mere product of speculation. However, the author wishes to pay attention to the fact that the Boyle’s corpuscle was carefully designed so as to be able to explain not only the variability and diversity of bodies, but also the conservation of matter and relatively stable existence of substance species.

It would have been quite unnecessary to assume such particles with complicated structure to explain purely physical properties, such as the “spring of air”, the law of which was discovered by Boyle himself and attributed by him to atomistic structure of matter. It must be, indeed, the chemical side of matter which required the structural corpuscles. This may be clearly seen when we consider the great achievement of Boyle in chemistry.

It would then be quite reasonable to assume that the Boylean structural corpuscle, that is to say chemical atom, corresponded to the concept “substance species”, a chemical notion attained with the establishment of “natural-historical stage of chemistry”.

For Boyle, the structural corpuscle made up from “universal matter” was a concept indispensable for his desire to let his particle be compatible with practical knowledges and logical way of thinking of chemistry at that time.
One of the representative opinions about the scientific role of the corpuscular theory of Boyle and his contemporaries is that of J.D. Bernal. He writes: In the seventeenth century chemistry was not yet in a state in which the corpuscular analysis could be applied. For that it needed the steady accumulation of new experimental facts that was to come in the next century. This statement would be valid as far as it means that the said theory could not be able to be applied as a working hypothesis in the sense of modern science. However, it would not be reasonable to deny at least indirectly and psychologically positive role of Boylean corpuscle for experimental researches of chemistry. His corpuscle, endowed with the above mentioned structure, which corresponded to the status of experimental knowledge and logic of chemistry at that time, must have helped experimental study of chemical facts.

K. Lasswitz, in his classical work, "Die Geschichte der Atomistik", called the pre-Daltonian atomistic view "mechanische Theorie". According to him the "mechanische Theorie" changed its character from "kinetic" to "dynamical". Admitting its "Fruchtbarkeit" to some degree in chemistry, Lasswitz considers that the effectiveness of the Boyle's theory was limited to mere explanations owing to the lack of such mathematical principles that could be used to analyse the mutual action of corpuscles.

Obviously such state of corpuscular theory could only be attained after completing certain steps of development. To realize the role and meaning of atomistic concept at this stage of scientific development, the author proposed in the first article of this study a couple of complementary concepts, namely "chemical atom" and "physical atom". The former means atoms or particles endowed with attributes corresponding to basic chemical properties of matter, namely the diversity in quality and the conservation in quantity. The physical atom was designated to correspond to physical phenomena of matter, such as the "spring of air". The "mechanical" corpuscle of Lasswitz belongs to the "physical atom" in the author's meaning.

Thus, we see that the Boylean corpuscle, which has been usually explained as "mechanical corpuscle" and considered to be indifferent to chemical nature of matter, is essentially "chemical atom". It would be then possible to assume the
Boylean corpuscle as the direct forerunner of the Daltonian atom.

4. Development and formation of the modern concept “chemical atom”.

It may be now possible to depict somewhat schematically the development of chemical atomistic concept in its relation to the evolution of the concept “chemical substance”. It proceeded according to the following steps.

i) In the Boylean chemistry the concept “substance species” established until and during his time led to formulation of his “structural corpuscle”, which is to be considered as “chemical atom”. The variability and diversity of substance species as well as the conservation of matter in general became thus explicable. However owing to lack of knowledges about chemical facts which should lead to the concept “component”, the structure of the Boylean corpuscle ended in a mere speculation (resume of the above mentioned discussion).

ii) The general formation of the concept “component” by chemical studies of iatrochemists, Phlogistonians and their contemporaries, being culminated in the Black’s view on the formation and composition of mild alkali and its analogous substances, made possible to establish some vague notion about “chemical species”, which should have had its final expression with help of the concept “chemical element”. Such vague and transitional phase of conceptual development could not have been enough to establish some definite notion of component. On the other hand, the Boylean speculative structural corpuscle had no ground to be revised so as to correspond to this transitional phase of the concept “chemical species”.

It is, however, from historico-chemical point-of-view, noteworthy, that Lomonosov in his chemical treatise, *Chymiae Mathematica* (1741) proposed a set of apparently modern chemical notions, namely elementum, corpusculum, principium, corpus mixtum and compositum, each corresponding to atom, molecule, simple body, compound and mixture. The present author pointed out in one of his foregoing articles that the Lomonosov’s chemical theory is to be evaluated as “Höhepunkt chemischer Atomistik vor-Daltonischer Periode”, although naturally it was not in state to assume what sort of substance was really
a chemical element or a compound.\(^{(11)}\)

iii) Regarding the epoch-making achievement of Lavoisier there are at least two important problems to be carefully examined. First: Was it possible for Lavoisier to reach the concept of chemical element without help of atomistic notion? In other words, had he consciously rejected the traditional Boylean or Newtonian corpuscle during his effort to establish the concept of chemical element? Second: What was the reason why he could not, after he had defined the chemical element in his *Traité*, go on to assume the chemical atom with its relative weight calculated from combining proportion of element? Although it is true that the Proust’s law of constant proportion was announced after Lavoisier’s death, it must have been for him nearly axiomatically valid that each chemical compound was formed by constant proportion of constituting elements.

The present author may express his desire that these questions could be answered in some reasonable way (for example, lack of experimental facts, philosophical tendency of his scientific thinking, his psychological character and so on) by historians of chemistry.

Anyhow, it must be emphasized that it was the concept of chemical element that led to the establishment of the exact concept of chemical species coupled with quantitative notion of “component”, and that the latter, as its logical consequence, led to the quantitatively defined concept of chemical atom of Dalton.

iv) The origin and nature of Dalton’s atomic theory, namely chemical atomism, should be examined in its inseparable relation to the concept of physical atom.

It must be noted that the Dalton’s atomic theory originated from his assumption that certain physical properties of gaseous substances like diffusion, partial pressure and solubility were to be attributed to the size of their spherical ultimate particles. Thus he went on to determine the “atomic weight” of each element by chemical method assuming that it could represent the relative size of his spherical atom. His primary intention was to depict a model of physical atom being capable to explain the above-mentioned physical properties of gaseous chemical species like oxygen gas, nitrogen gas, hydrogen gas and carbonic acid. In
comparison to, for example, D. Bernoulli’s kinetic theory, where the specific properties of chemical species were out of question, Dalton’s task was far from realization. The Daltonian atom was, so to say, a substitute for his originally intended physical atom, although his principle of simplicity to assume the number of combining atoms was mainly of physical nature.

v) Although Dalton’s theory failed to develop any fruitful concept of physical atom, it was an important step toward the unification of two complementary sides of atomistic notion. A further step toward this unification was taken by A. Avogadro, who for the first time succeeded to establish really working concept of physical atom, or more appropriately to say, “physical molecule.”

The reason why the Avogadro’s particle is to be regarded essentially as physical molecule is that it offered a physical basis for the determination of chemical atomic weight which had been pursued by Dalton quite arbitrarily. The first proposition of his hypothesis was of perfectly physical nature. As he mentioned in the beginning of his “Essai” (1811), it was derived from the Gay-Lussac’s law of gaseous reactions. The ground for the validity of this proposition was derived from an atomistic consideration of physical properties of gaseous substances. He stated:\(^{12}\): “Indeed, if it were to suppose that the number of molecules contained in a given volume were different for different gases, it would scarcely be possible to conceive that the law regulating the distances of molecules could give in all cases relations so simple as those which the facts just detailed compel us to acknowledge between the volume and the number of molecules.” This physical atomistic view had been already hold by Gay-Lussac himself in his “Momoir” (1808). He stated\(^{13}\): “The attraction of the molecule in solids and liquids is, therefore, the cause which modifies their special properties; and it appears that it is only when the attraction is entirely destroyed, as in gases, that bodies under similar conditions obey simple and regular laws. At least it is my intention to make known some new properties in gases, the effects of which are regular, by showing that these substances combine amongst themselves in very simple proportions,...”

Thus we see that Avogadro, following the Gay-Lussac’s way of thinking, reached his hypothesis, by which the relative mass of gaseous molecules was
defined by the ratio of densities and this ratio served to define the standard of chemical atomic weights. It is for this reason that the Avogadro's particle should be considered to be physical molecule, which unified in itself the concept of chemical molecule.

5. The development of the concept “chemical molecule”.
If the physical molecule is, at least in its originally proposed form, to be defined by relative mass of the particle as a whole estimated by use of any physical method, then the chemical molecule is to be characterized as a compound particle constructed by integration of its constituting parts, whose existence is assumed from more than one chemical metamorphosis of the substance. The Dalton's “compound particle” was nothing but such molecule. This feature of the chemical molecule was typically expressed in Liebig's chemical formulae based upon his radical theory. Berzelius' molecule may be regarded as, so to say, partially physical, inasmuch as he adopted his “volume theory” to assume elementary gases as monoatomic and constructed chemical formulae of some inorganic compounds by application of Mitscherlich's law of isomorphism and Dulong-Petit's law of atomic heat. However, his molecule was essentially chemical, as Lespieau pointed out\(^{(14)}\): "Qu'est ce qui a guidé Berzelius? Est-ce une règle précise et stricte conduisant automatiquement aux résultat? Nullement; il a eu du bon sens chimique, simplement; utilisant des considérations d'ordre divers, il a confronté entre eux les résultats obtenus, puis il a fait un choix."

It was, indeed, Berzelius, who established the electro-dualistic theory as a guiding principle to construct his molecules by finding inseparable relations between the electrical, not mechanical, behavior and chemical properties of substances. But this did not exclude substantially the chemical nature of his molecule.

The development of dualistic-radical theory to the unitary theory is noteworthy as an important step toward the unification of the chemical and physical molecule. The unitary theory intended to depict molecule as a whole by chemical method putting the dualistic radical, notwithstanding its great significance, out of consideration, as it is obviously seen in the known statement
of Gerhardt: “Tous les corps sont considérés comme des molécules uniques, dont les atomes sont disposés dans un ordre déterminé que les réactions chimiques n’indiquent que d’une manière relative.”

A noteworthy fact was that Laurent, Gerhardt’s colleague in their effort for the unitary theory, relied upon physical estimation of molecular weight so as to revise the Gerhardt’s method (two-volume-formulae for compounds and one-volume-formulae for simple bodies). C. Gräbe pointed out: “Der erste Chemiker, der diese Worte (= Atom und Volum) in dem Sinne adoptierte, wie sie Ampère und Gaudin 1833 benutzt hatten, ..... war Laurent.”

For the unitary theory the intention to depict molecule as a whole was a mean, but not its final aim, which should have been to elucidate the whole arrangement of constituting atoms, instead of presumably pre-existing radicals as they were asserted by Berzelius, Liebig and their followers. A significant approach to this goal was seen in the successful achievement of Williamson, who, adopting the atomic weights and molecular formulae of Gerhardt and Laurent, elucidated the mechanism of aetherification and established his water-type, announcing that in water molecule \(^{1}H_{2}O^{1}\) two atoms hydrogen seem to be combined to one atom oxygen. Further steps to the said aim should have required more conscious adoption of the Avogadro’s hypothesis.

Thus, the whole circumstances of the development of chemical molecular theory depended upon the general acceptance of the Avogadro’s hypothesis. The present author had treated this problem in one of his foregoing articles, “Über die Gründe der Verspätung der Anerkennung der Avogadroschen Hypothese”, the conclusive argument of which may be summarized in the following.

The main reason why the hypothesis could not enjoy general acceptance until the time of the Carlsruhe Congress was not, according to the present author, its partial invalidity for several substances, such as mercury, sulphur and arsenic, nor it was, as often asserted, strong influence of the dualistic view of Berzelius. It is rather to be attributed to the above-mentioned physical nature of the Avogadro’s molecule, in another word, lack of quality corresponding to the chemical nature of substances. Majority of leading chemists went on along their way to construct molecule by use of their familiar purely chemical method. When they thus
managed to depict molecule with formulae which could express diversity of chemical behavior of substances, they inevitably confronted difficulties arising from disunified standard of atomic weight and arbitrary molecular weight. It was under such circumstances that chemists, urged by their internal necessity, were compelled to adopt the Avogadro's hypothesis as one of their guiding principles.

The historical fact that the general acceptance of the hypothesis occurred after the Carlsruhe Congress means that the Avogadro's physical molecular concept had been kept in, so to say, frozen state until the classical theory of chemical structure approached to its final perfection. The famous Cannizzaro's lecture was not the only one cause of the acceptance, but it worked as a catalysis.

Thus we see perplexed fortune of the Avogadro's hypothesis that chemists had taken it into consideration implicitly and inconsequently until they approached their aim to construct their molecules in chemical way and they adopted it explicitly and consequently when they nearly arrived their final aim. In this sense the two molecular concepts, chemical and physical models of molecule, worked complementarily to each other. Indeed, the terms, chemical and physical molecule, were used by Kekulé as wrote in his Lehrbuch (1859): "dass die chemischen Moleküle identisch sind mit den physikalischen Gasmolekülen." It must be noted that this status of chemical notion was defined by A. Ladenburg as "Verschmelzung der Begriffe von physikalischen und chemischen Molekülen."(16)

6. The concept "chemical structure" and its shift to the up-to-date notion

For considerations on further phase of development of the molecular concept, it is required to examine what was meant by the so-called "Verschmelzung" of two molecular concepts. First of all, it meant the establishment, not perfection, of the concept "chemical molecule", whatsoever the way from Berzelius-Liebig to Kekulé was complicated. The chemical molecule with its rational or structural formula was, on another side, a physical one as a whole, at least for substances with measurable gas or vapor densities. This physical nature of the chemical molecule was not any more than the Avogadro's molecule was physical, because it
had nothing to do with actual arrangement of atoms within molecule or physical cause of chemical behaviors of molecule. The chemical molecule was apparently nothing but a mode of expression for chemical behaviors of substance.

In this meaning Kekulé was right when he wrote his view on the ideal and real of chemistry in his “Über die Konstitution und die Metamorphosen der chemischen Verbindungen...”(17) (1858): “Obgleich es also gewiss für eine Aufgabe der Naturforschung gehalten werden muss, die Konstitution der Materie, also wenn man will die Lagerung der Atome zu ermitteln: so muss man zugeben, dass nicht das Studium der chemischen Metamorphosen, sondern vielmehr nur ein vergleichendes Studium der physikalischen Eigenschaften der bestehenden Verbindungen dazu die Mittel bieten kann.”

However, he could not have been right or at least consequent, if he should have suggested by the said limitation of “Studium der chemischen Metamorphosen” that the latter was not in a state to let reflect the mutual relation of atoms within a molecule, because the very achievement attained in the same article, namely, quadrivalency and mutual linkage of carbon atom, meant nothing but the above-mentioned reflection. He was not, in fact, enough consequent, as seen from the following words(18): “Die rationellen Formeln sind Umsetzungsformeln und können, bei dem heutigen Stand der Wissenschaft, nichts anders sein.”

It was for Kekulé a self-contradiction that he, notwithstanding his recognition of the identity of chemical and physical molecule, could not explicity approve the function of chemical molecule with his rational formula to reflect the objective reality of actual “Lagerung der Atome” within molecule.

For the completion of the concept of chemical molecule there remained a single step which required no more experimental facts but rather methodological thought to recognize the real meaning of chemical formula. It was A.M. Butlerov who solved this problem by introducing a new concept “chemical structure”.

Butlerov distinguished “chemical atom” from “physical atom”. The “chemical atom” is “the smallest quality of element being contained in a molecule, or chemically indestructible quality, while the physical atom is “actual indestructible particle of substances.” G.V. Bykov interprets Butlerov’s attitude as
follows\(^{(19)}\): “Butlerov considered that any hypothesis on the existence of physical atom was not, for the moment, of importance for chemistry. The concept of chemical atom was more abstract than that of physical one. The chemical atom was to be more appropriately considered without taking into account (such attribute as) quantity, form and spatial arrangement of atoms. Consequently, the problem of spacial (“mechanical”) structure was also to be put outside the question. Thus Butlerov disengaged himself from the firm tradition of chemistry, a tradition to attribute chemical properties of molecule to its spacial structure” (translated from the Russian text)

Butlerov himself wrote in his article. “Einiges über die chemische Struktur” (1861)\(^{(20)}\): “Mit Gerhardt zusammen verneinen wir gegenwärtig die Möglichkeit, über die Lage der Atome im Inneren des Moleküls Rechenschaft zu geben und es scheint sehr natürlich, dass die Chemie, welche mit den Körpern nur im Zustande ihrer Verwandlungen zu tun hat, unmächtig ist, so lange die physikalischen Untersuchungen nicht zu Hilfe gekommen sind, über diese mechanische Struktur ein Urteil zu geben.”

As for the concept “chemical structure” he wrote continuing the above-mentioned phrase: “Von der Annahme ausgehend, dass einem jeden chemischen Atome nur eine bestimmte und beschränkte Menge der chemischen Kraft (Affinität), mit welcher es an der Bildung eines Körpers Teil nimmt, innenwohnt, möchte ich diesen chemischen Zusammenhang, oder die Art und Weise der gegenseitigen Bindung der Atome in einem zusammengesetzten Körper, mit dem Namen der chemischen Struktur bezeichnen.”

E. Hjelt summarized rightly what Butlerov attained for the establishment of his new concept as follows\(^{(21)}\): “Es wurde durch Butlerov einerseits klar gemacht, dass die chemische Struktur etwas ganz anderes war als die rationelle Zusammensetzung im Sinne der Typentheorie, d.h. nur ein Ausdruck der Analogie- und Umsetzungsverhältnisse. Andererseits wollte die Struktur nichts über die mechanische Lagerung der Atome im Molekül aussagen, also nicht das sein, was Gerhardt und anfangs auch Kekulé unter 'constitution moléculaire' verstand, nämlich 'le véritable arrangement de leurs atomes’.”

Thus we see that the establishment of the concept “chemical structure”
meant the completion of the concept “chemical molecule”.

It is now the turn to mention briefly further development of the concept “physical molecule”, which has already come to be incorporated within the concept “chemical molecule, which had reached the completion by the establishment of the concept “chemical structure”. As chemical molecule could be rightly recognized by making a couple with chemical structure, physical molecule was also to be fully realized when physical structure could be recognized in its real meaning. When this is attained, it means the real fusion of chemical and physical molecule, at the same time the real fusion of chemical and physical structure. To elucidate the latter fusion, the development from chemical to physical structure should be considered, including the transition stage of the development.

The fusion of chemical and physical structure could be completely realized through the introduction of quantum mechanics into chemistry, after the fine structure of crystalline compounds had been found by the X-ray analysis. However, from historico-chemical point-of-view, an important progress of atomistic concept of chemistry toward the said fusion was the stereochemical structure proposed by van’t Hoff-Le Bel and the co-ordination theory of A. Werner. They formed the transitional stage, through which the chemical structure of Kekulé-Butlerov could be transformed into the up-to-date chemical structure.

It must be noted that the stereochemical structure was a product of chemical researches helped and complemented by observations of physical properties of substances in question. It was dominantly chemical, but partially physical structure, as the Avogadro’s molecule was it. The same relation holds for the molecular asymmetry of L. Pasteur (1848) who stated in his lecture in 1860 that he could reach this concept by taking into consideration several physical properties of crystals and solution of the compounds he studied. But the time was not ripe for Pasteur’s theory to be taken into the main current of chemical theory at that time. The reason was, according to the present author, mainly lack of knowledges about chemical behavior which complement the rather physical insight of Pasteur.

It is another issue of the present author to examine this transition more carefully, which is already precisely treated by G.V. Bykov in his Istoriia
It may be finally proposed to call the chemical structure of Kekulé-Butlerov the "classical basis of modern (present) chemistry", through which the conceptual development of atomic notion from the Dalton-Avogadro's stage to the present phase seems to be logically explained.

**Resumé**

Briefly summarizing the author introduced a couple of complementary concepts, chemical and physical model, to interpret the historical development of atomistic concept in chemistry from the age of Boyle to the beginning of the present century. It was shown that these concepts worked at first as chemical and physical atom, next as chemical and physical molecule and then as chemical and physical structure.

The author made at the same time a consideration on the evolution of concept of substance and proposed a logical sequence of its development, each step of which seems to correspond to the said development of atomistic concept.

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Establishment of Biochemistry in Japan

Tatsumasa Doke*

Biochemical studies in Japan started after the Meiji Restoration (1868) and grew up with a rapid transplantation from Europe. About twenty years latter, i.e. in 1880s, the original studies of applied biochemistry full of local colour began to appear; thirty years later, i.e. 1900s, some of them were internationally recognized of their academic value in their own fields, and accordingly the more theoretical researches were brought up; and fifty years after the Meiji Restoration, i.e. in 1920s, it developed to a degree that Japanese students could attain in their Japanese institutions so excellent result that stands on the international level even in the field of pure theory. In 1922 two technical magazines on biochemistry, the \textit{Journal of Biochemistry} and the \textit{Acta Phytochimica} issued their initial numbers.

In 1925 the Japanese Biochemical Society was established and acquired 513 members of Japanese scholars. To all appearance it seemed that thereafter Japanese biochemistry should make a rapid progress in quality as well as in quantity, and shortly should come into full blossom. But it did not. On the contrary it slowly came to a stop as the aggressive policy of Japanese imperialism proceeded, and finally fell down far behind the international progress. Since the end of the World War II it nevertheless recovered swiftly and is now running its way at full speed though facing with new difficulties one after another.

In Europe around 1868 when the Meiji Restoration broke out, a new field of biochemistry was brewing in Germany. Incidentally we may notice that in 1840 J. Liebig published a paper entitled \textit{die organische Chemie in ihrer Anwendung auf Agrikultur und Physiologie}, in 1842 \textit{die Tierchemie, oder die organische Chemie in ihres Anwendung auf Physiologie und Pathologie}, and in 1871 \textit{Jahres-Berichte}.

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über die Fortschritte der Tier-Chemie was born; and in 1877 Hoppe-Seyler and others started the Zeitschrift für physiologische Chemie.

In Japan prior to 1868, the scientific researches had been impeded under the severe feudalistic reigns. Nevertheless the productivities were rising gradually promoting the necessity of scientific researches. At the same time the studies of physiology and chemistry, two bases of modern biochemistry, were being made and accumulated in Japan, mainly by the efforts of medical practitioners though scarce in number. The European achievements were translated and introduced through the studies of Dutch learning; such as the Seimi Kaisō (1837–47) which Yōan Udagawa translated from a Dutch translation of the Elements of Experimental Chemistry of W. Henry. Udagawa also wrote the Shokugaku Keigen (1833) which was the first book of systematic European Plant physiology in Japan. Even before the Meiji era, there had already existed some original and highly developed technologies for the brewery and for the medical science without which the Japanese biochemistry would have never been born and grew up rapidly. After the Meiji Restoration, Government, crying their slogans “National Industrialization First!” and “Strong Army!”, intended to take in the European natural sciences rapidly by opening colleges and universities of Western style, inviting European scholars, and assisting with enthusiasm the regular “Study Abroad” system. The import of biochemistry was realized also as one of these Westernizations of learning. However, it was naturally impossible as yet to transplant from Europe her biochemistry in systematic order as it had not yet been completely systematized even in Europe immediately after the Restoration. This was a peculiar situation somewhat different from in the case of transplanting chemistry and physics.1)

It was in the Tokyo University (including its predecessor) that the educating of biochemical students started first. According to their own purpose and blueprint, the Colleges (Faculties) of Agriculture, of Medicine, of Technology, and of Science each made their progress, for instance, College (Faculty) of Agriculture tried to increase the agricultural productivity, College (Faculty) of Medicine to promote modernizing the medical treatment. Once the necessity of introducing modern chemistry realized, each section began to invite European chemists with sufficient knowledge of modern chemistry. To the College (Faculty) of
Agriculture came E. Kinch (U.K.), O. Kellner (Germany) and O. Loew (Germany); to the College (Faculty) of Science R.W. Atkinson (U.K.) and the College (Faculty) of Technology E. Divers (U.K.). Selected students in turn were despatched to the distinguished scholars of international authority; for instance, A.W. von Hoffman (Germany), H.E. Roscoe (U.K.), C. Shorlemmer (Germany and later U.K.), F. Hoppe-Seyler (Germany) and E.L. Salkowski (Germany). Most of them were the disciples of J. Liebig and R. Virchow. It was not incidental that Japanese biochemistry at its preparatory stage tried by all means to learn from J. Liebig.

It was extremely difficult, however, for the early Japanese students to understand Liebig’s thought that underlies his scientific system even if they had succeeded in accepting the necessary technologies in short time. Both these foreign teachers invited and the Japanese students learning from them chose at the outset the practical themes and materials full of locality as subjects of their studies rather than the pure theoretical problems. It was a national request then but at the same time these subjects were regarded as a short cut to call attentions of the international academic world to the results of their studies.²)

R.W. Atkinson who was invited in 1874 to Kaisei Gakko (a predecessor of the Faculty of Science, Tokyo University) made with his students researches on the Japanese wine “sake” and published the Chemistry of Sake-brewing³) in 1881, which was a pioneering and most characteristic work of biochemistry in Japan.

It dealt first with areas where rice is cultivated in Japan and the annual yield, then with chemical analysis of rice, morphology, physiology and chemistry of Koji, and quantitative study of functions of diastase at Koji, chemical analysis of each processes of Sake-brewing, further with how to prevent the putrefaction of Sake in stock. He argued all these subjects based on his own experiments and observations utilizing E. Kinch’s data and in cooperation with J.A. Ewing and others. He even tried to design apparatus of new type. What is more, he expressed a great surprise to see that in Japan a germ-killing process at a low temperature had been discovered and practised by Sake-brewers nearly 300 years earlier than by L. Pasteur, and stressed that Europe had something to learn from Japan and even referred to a mutual profit born out of the intimate cooperation between science and technology. His work was quite a model worth to follow. Students
who learned chemistry from him began to engage vigorously in the chemical analysis of producing processes of Japanese products with long tradition such as "shoyu" (soy) and "urushi" (lacquer). As early as 1883 Hikorokurō Yoshida\(^4\) made a discovery of highly scientific value about laccase from his study of hardening of the lacquer. Another masterwork was achieved by Jōkichi Takamine who studied under E. Divers at Kōbu Daigakkō, a predecessor of the Faculty of Technology, Tokyo University, and succeeded in the industrial extraction of diastase from "Koji", and in its merchandization producing a large quantity of cheap enzyme as materials for researches. This diastase was named "Taka-diastase" by Takamine. As for the Faculties of Agriculture and of Medicine, they tackled their own objects full of local colour that deserved their chemical researches.

In contrast with this, chemical studies taking up a fundamental thesis of biochemistry as its direct object, that is, the elucidation of vital phenomenon, started much more lately. Jūgō Sugiura\(^5\) who studied under R.W. Atkinson and was ordered to study in England at the government expenses, and then moved to the laboratories of H.E. Roscoe and C. Schorlemmer, started on his return to Japan a periodical entitled \(Tōyō Gakugei Zassi\) (Journal of Oriental Arts) and contributed to its first issue (1881) an article saying: "To quote C. Schorlemmer, the only way to elucidate the problem of vital force should be to penetrate into the protein compounds. .....Once the chemist enabled to compose original protein, it probably must be identical with what Mr. E.H. Haeckel calls Monera."

The article was so pioneering to introduce the materialistic philosophy on life and in fact it was much more years later that Japanese students began to argue among themselves about such a problem trying to verify it by experiments.

At first, biochemical studies in the agricultural and medical fields concentrated to those of application so rich in local characteristics. For instance, themes chosen in the College (Faculty) of Agriculture were: chemical research on the influence afflicted by fertilizer upon crops; chemical change in the tea-leaf when processed; chemical analysis of producing processes of rice and "miso" (bean paste). In the College (Faculty) of Medicine, studies focussed on extraction, purification and chemical synthesis of the effective components of Chinese or Japanese herbs or poisons such as of swell-fish.
The trend continued for some time thereafter but since around 1900 an epochmaking change emerged. While these applied studies full of local colour were gradually developing into fruit appreciated highly from abroad, the pure biochemical studies began to take a clearer shape in parallel. In another words, a new movement to grasp the physiological processes of animals and plants from more chemical point of view began to appear among the students on the Medicine and the Agriculture. It was derived from an inner necessity that required more deepening of basic studies to gain more practical profits, accelerated further by the influence of foreign instructors. O. Loew at the Faculty of Agriculture who succeeded O. Kellner in 1893, and F. Hoppe-Seyler and E.L. Salkowski at the Medicine, started their less applied, more theoretical studies of physiological chemistry or pathological chemistry that threw a grave influence upon Japanese students working under them. Under these circumstances with inner and outer motives combined, the course of physiological chemistry was established one after another in the Faculties of Agriculture and of Medicine that yielded later so many physiological and chemical students for plants and animals. The new course called “Medical Chemistry” was set up in 1893 at the Tokyo University (Prof. Muneo Kumakawa in charge), then in 1899 at the newly established Kyoto University (Prof. Torasaburō Araki in charge).

The courses of the first (N. Matsui in charge) and the second (O. Loew in charge) “Agricultural Chemistry” was set up in 1893 at the College of Agriculture, Tokyo University, and Umetarō Suzuki succeeded O. Loew in 1907. The second Agricultural Chemistry was called “Biological Chemistry” as the common name.

M. Kumakawa was dispatched to Germany in 1884 for the study of internal medicine, worked under Salkowski of University of Berlin and returned to Japan fully conscious of the importance of biochemistry under the strong influence of an independence movement of biochemistry from physiology then flaring up in Germany. Fortunately his masters at the Tokyo University, Kenji Ohsawa (Physiology) and Hiizu Miyake (Pathology), were also aware of the significance of physiological and pathological chemistry and acted in favour of setting up the new course of Medical Chemistry. Then T. Araki, a desciple of Ohsawa, sent to study under Hoppe-Seyler, came back to Japan. From then on, a number of biochemists were hatched and fledged out of the laboratories of these two scholars.
Kumakawa, studying the fat metabolism, discovered as early as 1904 a new quantitative method for fat, for which Araki advanced his research in haemoglobin and nucleic acid.

Takaoki Sasaki (Kumakawa's pupil) studied under E. Fisher, F. Hofmeister, E. Abderhalden, E. Friedman et al., and made researches for protein chemistry, micro-biological chemistry in 1910-1920s, and pathological chemistry in 1930s in his private institute Sasaki-Kenkyu-Sho which was built by the found of his grandfather Tōyō Sasaki. Yashirō Kotake (Araki's pupil) studied under M. Jaffe, A. Ellinger (Germany), and research for amino-acid metabolism in animal body at the Medical College of Osaka.

O. Loew, who once in Japan was immediate to commence a basic study of synthetizing function of protein, brought up many students in cooperation with Yoshinao Kozai, the favourite disciple of O. Kellner. Out of this team work sprang a most distinguished follower, U. Suzuki, who reported as early as 1898 about the biosynthesis of protein in plants and went to study under Emil Fischer, then on his return to more practical research in the Japanese foodstuffs like rice and fish and discovered "Phytase" and "Oryzanin" (Vitamin B₁). His laboratory also yielded many students, and they engaged in the studies of Vitamin A.B.C.D.L. in the "Rikagaku-Kenkyu-Sho (Institute of Physical and Chemical Research)".

In parallel with these movements, the course of Plant Physiology (Prof. Manabu Miyoshi in charge) was set up in 1895 at the Faculty of Science in Tokyo University. His follower, Keita Shibata, deepened the study of plant physiology and organic chemistry working under W. Pfeffer and M. Freund in Germany, and then established and supervised the course of Physiological Chemistry of Plant at the Faculty of Science, Tokyo University (1924). Numerous distinguished experts in the field were to be born there. Keita Shibata and Yūji Shibata studied with Flavon in plants and metal complexes and published *Katalytische Wirkungen der Metall-Komplex verbindungen* (1936). Yūji Shibata, K. Shibata's brother, a chemist (Prof. of the Faculty of Science, Tokyo University.), studied under Kohichi Matsubara (Organic chemistry), then went abroad and worked under A. Werner (Switzerland), A. Hantzsch (Germany) G. Urbain (France).

On the other hand, the course of Biological Chemistry at the Faculty of
Science was set up in 1919 in Tokyo University, for the purpose of promoting the theoretical study of biochemistry by efforts of U. Suzuki, Kakiuchi & others, and was presided over first by Samurō Kakiuchi, and then by Tokurō Sohda. After two years the course of Biological Chemistry was set up in 1921 at the Faculty of Science, Kyoto University and presided over by Shigeru Komatsu.

S. Kakiuchi, who studied under Kumakawa and Kikunae Ikeda (physical chemistry), went to U.S.A. and studied biology (under Mendel, Harrison, Petrunkewitch, J. Loeb, Folin, Cawdray, Lillie, Herrick, Child, Benselay, Moulton, Wilson, Morgan, Calkins) and also physics and physical chemistry (under Moulton, Michelson, Bancroft). After he came back to Japan, he succeeded Kumakawa and worked as the organizer of a group of students of basic biochemistry thus brought up in each field.

In 1922 there were three big events for biochemistry in Japan:

(1) In 1922 Kakiuchi organized a circle called “Tokyo Seikagaku-sha Yoi-no-kai (Society of Tokyo Biochemist Evenings)” where everyone could talk quite freely regardless of his academic status. At the feudal society of academism in Japan, this event was revolutionary. While on the other hand S. Kakiuchi issued at private expenses a technical magazine for biochemistry entitled Journal of Biochemistry in close cooperation with his masters and seniors and his colleagues (U. Suzuki, K. Ikeda, T. Araki, T. Sasaki, Y. Kotake, Kōji Miyake, Katsuji Inoue etc.). In the forward of its first issue, S. Kakiuchi said, “During the last decade the number of published works in biochemistry in our country has greatly increased. Until the beginning of the World War of 1914-1918 a large number of the graduates of our universities and colleges went abroad for their post-graduates research work. During the war, however, the number of research students in each of our laboratories has been increased many fold, and the status of science now in our country has passed so to speak from the exclusively educational stage into that of laboratorial research. As a result there is now an increasing demand at home and abroad for an organization for publishing our works internationally. On the one hand, owing to the situation of our country remote as it is from the centres of science abroad, there are always some difficulties in contributing our reports to foreign journals, causing a delay in publication at times of more than half a year. On the other hand, as most of our reports are published in our mother
tongue, many friends abroad regret the inaccessibility of some of our valuable works. Stimulated by these increasing demands we have decided with the cooperation of certain of our biochemists to issue this journal in the interest of biochemistry. May this little Journal of Biochemistry have a prosperous future and be a contribution, though small, toward the promotion of true knowledge."

(2) In the same year Keita Shibata, also in co-operation with his disciples, established a private laboratory the “Iwata Institute of Plant Biochemistry” 1921 and published from there a magazine *Acta Phytochimica*. A series of studies promoted by Shibata’s group on the cellular respiration contributed to advance the research in the chemical process of respiratory function by joining in a scientific dispute with O. Warburg, and D. Keilin. Rikō Majima, Yasuhiko Asahina, and others contributed their works with organic chemistry of Japanese plants to this Acta.

(3) In the same year, L. Michaelis (Germany) came to Japan as the lecturer of biochemistry of the Aichi College of Medicine (Nagoya), and lectured physical chemistry at biochemistry (ex: the theory of pH).

The explosive energies of these vigorous and voluntary students culminated in the establishment of “The Japanese Biochemical Society” in 1925 that mustered so many biochemist coming from each different fields. With the opening of the Society, Japanese biochemistry was also firmly established as a section of natural science. It was a dawning of the new stage for the biochemical studies in Japan. Everyone felt happy in the highest spirits. Few people foresaw a speck of black cloud of the Japanese fascism gathering over them that would eventually doom all of their works not more than fifteen years later.
Establishment of Biochemistry in Japan

Literatures and Notes


2) To quote H. Miyake (later, the head of Medical College, Tokyo Imperial University): “If we succeeded in the chemical analysis of Oriental plants and minerals and reporting how to utilize them properly, it would not only contribute to the academic progress of medical science but also serve greatly for Japan by introducing all over the world the advanced level of Oriental Art.” (Tokyo Kagakukai-shi, 4, 41, 1883).


5) However, Sugiura immediately abandoned the studies in the field and went into the general education based on the oriental and later on the nationalistic philosophy peculiarly enough, few Japanese know that once he had been engaged in biochemical studies.


9) Articles were written in English, German and French, and continued to 1944 (vol. 36) when Kakiuchi abolished it in a desperate mood because of the World War II.
Mendel published only two genetics papers, in 1866 and in 1870 respectively. One is concerned with Pisum and the other with Hieracium. The first paper is well-known and is the source of the so-called Mendel’s laws of heredity on which the modern genetics is based and developed. The second is a short interim report and seems to have been regarded as a less important work.

Now, I would like to talk about my views on the characteristics of these two papers and also on Mendel’s idea inherent in or attitude taken in these studies.

Of the various fields of the highly progressed genetics in recent years, the relation of heredity and evolution is one. I would like to consider how Mendel thought of this theme, how he performed his experiments, how he evaluated his own studies, and what was his intention in writing and publishing his papers, etc.

The paper on peas is written in German and is 45 pages long without any illustrations. It is divided into 11 chapters beginning with “Introductory remarks” and ending with “Concluding remarks” (Table 1).

Table 1. Contents of Mendel’s paper on peas

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If I may give a number to each chapter, chapters 4-9 are the portion dealing with pea experiments, and chapter 10 is concerned with the generalization test to see whether or not the laws obtained from the study of peas can be applied to other plant groups, for instance, beans, as used here. The final chapter for "Concluding remarks", is rather long compared with the usual way of papers published nowadays, because here Mendel discussed possible interpretations of various problems presented by his predecessors, with his planned experiments, on the basis of the laws from the study of peas.

The whole paper on the study of peas can be separated into two parts. The first part occupying about two thirds of the paper relates experiments on peas, and the other part, one third of the paper, contains the chapter dealing with experiments on other plants (chapter 10) and the chapter for "Concluding remarks".

The first two thirds are the portion which form the foundation of today's genetics, giving us an impression that the paper had no other part. Of course this
part alone is so important that it deserves the highest evaluation. At the beginning
of the remaining one third, application of the laws obtained from the study of
peas to other plants is discussed. Here, Mendel succeeded in attaining useful
results of experiments, thought the data were not sufficient, for the interpretation
of the diversity of color changes of bean flowers, adding explicit explanations to
them which were developed later by his successors.

In the following chapter (11), “Concluding remarks”, roughly three problems
were taken up for discussion based on the renewed experiments. The first problem
was that living things vary both inwardly and outwardly. In the second place,
explanation was attempted about the distinction between a hybrid, such as peas,
whose offspring are changeable and one such as willows whose offspring are
unchangeable. Here we may be able to see that Mendel introduced a concept of
gene and its nature and action. The third problem was about the transformation
of species. These problems all can be said to be those of organic evolution. I
surmise that Mendel attempted to solve these problems by means of the laws
gained from his genetics study of peas. Altogether, it may be said further that the
paper on peas is, especially in the latter part, in conjunction with the first part,
entirely dealing with the problems of evolution.

When Mendel published this paper, “Origin of species” by Charles Darwin,
which had been published six years before, was gaining the attention of the world
and Mendel might have known the reputation. It is guessed from some passages of
Mendel’s paper that he was not always agreeing with Darwinism, though not
denying it entirely.

Mendel’s second paper dealing with *Hieracium*, is a short work written in
German of only 6 pages without any figures. This was a kind of an interim report.
Mendel thought he would have enough time for study if he were appointed abbot
of the monastery. But, contrary to his expectation, this duty kept him busy with
various responsibilities both in and out of the monastery, which almost prevented
him from studying of *Hieracium*. He continued, however, to work on this
chrysanthemum with his native perseverance. After all, he wrote in his last letter
to Nägeli, his esteemed teacher and friend (November 18, 1873): “The hieracia
have withered again without my having been able to give them more than a few
hurried visits. I am really unhappy about having to neglect my plants and my bees so completely.”** Mendel is said to have called peas and hieracia “my dear children”. The flowers of hieracia are very small and are open at the time between 7 and 9 o’clock in the morning when the tips of their pistils come out. In order to pollinate, he never failed to come to that flowers every morning at the right time. He even had eye trouble as a result. Concerning the publication of the short report of hieracia, he stated in his paper as follows:

“It will be clearly understood from the littleness of what I can report here that this study has progressed not a step from the starting point. I should have hesitated to report on an experiment which is just started. I believe it will take several more years to complete the schedule of experiments, moreover, I am not sure whether or not I can continue to work to the end. That’s why I decided to make this report.”**

Mendel gave in his paper a number of reasons for his taking up hieracia as an experimental material to substitute for peas.

1) Genus *Hieracium* has an abundance of independent varieties.

2) Among them, while there are fundamental types, in other words, species, there are intermediate types or transitive types with which fundamental types are bound to each other.

3) Therefore, the classification of the genus *Hieracium* is so difficult that it drew the attention of specialists and many papers had been published, but no definite conclusion was obtained. To solve the problem, Mendel thought, the value and meaning in classification of intermediate types or transitive types must be found.

4) There were different opinions on the relationship between polymorphism and crossability of this genus.

5) A problem of sterility of hybrids exists.

6) A problem was raised as to the origin of constant (stable) intermediate

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* Cited from an English translation of this letter included in “The birth of genetics”, Supplement to Genetics, 1950.

** English citations of Mendel’s two papers are made from appendix to Bateson, W.1909. Mendel’s principles of heredity. 2 ed.
types. This problem aroused interest after a famous specialist (probably Nägeli) stated on the basis of the Darwin's new theory that these intermediate types had been produced by changes of extinct or existing species.

7) For evaluating the effects of hybrid formation on the diversity of intermediate types, it is necessary to know well about hybrids in their types and sterility as well as the behavior of their offspring through many generations.

Mendel stated that, in order to obtain the laws of a hybrid formation in Hieracium, an experiment was the only means for approaching the problem solution, and that the purpose of his study on Hieracium consisted in the reasons enumerated in the above.

With such knowledge and views about the genus Hieracium, Mendel took up this plant group as the material for his experiments and reported some of the results obtained in his letter of 1866 to Nägeli. Nägeli advised Mendel to go ahead with his plan and sent him some kinds of hieracia. It is guessed that, besides the plan of applying the laws found in the study of peas to the case of hieracia, Mendel planned to extend his study from the problem of polymorphism of this plant group to the problem of species. In this series of experiments with hieracia, he succeeded in producing 6 kinds of hybrids which he examined and described from his own point of view. Sometime after discontinuing this study, he wrote the last letter to Nägeli in 1873, sending him 235 hybrid plants of 5 kinds of crossing in Hieracium.

The following may be pointed out as the findings Mendel achieved in this study on hieracia:

1) There are combinations of species which produce hybrids and ones which do not.

2) In the progeny of the first generation of hybrids, there are characteristics which follow the laws as found in the case of peas, and ones which do not.

3) There are some plants in the first generation of hybrids, which show polymorphism.

4) Those in the second and later generations showed sterility and homomorphism.

5) Self-pollination is not prevented by the parent’s pollen.
6) Even in the case of a wild and fertile species, sterility is observed in its pollen.

7) It is very important that the existence of constant and polymorphic hybrids (Salix and Hieracium, for instance) has some relationship with the peculiar behavior of hybrids. This problem, however, is not yet solved.

So far I stated outlines of the two papers of Mendel. Now, I would like to tell how Mendel himself thought about these two papers, in other words, what is the reason according to which I may regard these papers as works on evolution.

1) There were many persons who had tried crossing plants before Mendel did. Among them, two eminent hybridists engaged in their work on a large scale. Both of them were Germans, D. J. Koelreuter, whose book was published in 1761, and C. F. Gärtnner whose book, including the results obtained by him, was published later in 1848. They examined the problems of pollination, characteristics of hybrids, formation of species and varieties, by means of crossing many plants. In 1830, the Dutch Academy in Haarlem announced an essay contest on the themes of the question of artificial production of species and varieties, and of production and raising of useful or ornamental plants. Gärtnner participated in the contest and won a prize. After the death of Koelreuter, E. A. Regel opposed A. Knight of England and J. F. Klotsch of Germany who insisted that a species hybrid was sterile. Similarly, A. Jordan opposed D. A. Godron. With the rise of the controversy, the academy in Paris announced an essay contest on the themes of continuity and self-pollination of hybrids, and atavism of hybrids. Among the participants, C. Naudin won the first prize and D. A. Godron the second. In addition, in 1859, Darwin published his “Origin of species” which drew the world’s attention and was also discussed often at the Natural History Society at Brünn. Mendel mentioned in the “Introductory remarks” of his paper on peas the names of researchers on crossing such as Koelreuter, Gärtnner, W. Herbert, H. Lecoq and M. E. Wichura. He was familiar with the books of M. T. Schleiden who put forward the “Cell theory”, as well as with all books of Darwin in the evolitional cycle. From the above, it is apparent that Mendel was well convinced that the main current theme in the 19th century was the problem of species, in other words, that of evolution.
2) There are many points in Mendel's two papers which are understandable from the viewpoint of 1) above mentioned. If we look at the paper on peas, we will know that Mendel worked from beginning to end along the line of the problem of evolution. Followings are the examples quoted from his paper of 1866.

1. From "Introductory remarks"

a) First, he stated the motive (Veranlassung) of his study saying: "Experience of artificial fertilization, such as is effected with ornamental plants in order to obtain new varieties in color, has led to the experiments which will here be discussed." To obtain plants of new colors is none other than the problem of evolution. To fulfil his purpose, Mendel chose the method of producing hybrids and examining their numbers and behavior from generation to generation. Mendel said that he did this, because proper laws had not yet been found by the patient and careful predecessors such as Koelreuter, Gärtner, and others.

b) To accomplish this task, he said, a large scale "detailed experiment" should be done and at the same time some courage was required. "This appears, however, to be the only right way by which we can finally reach the solution of a question, the importance of which cannot be overestimated in connection with the history of the evolution of organic forms." His course of study is one of the good examples of what is based on the principles of what are called spirit and methods of modern sciences. The characteristic of his studies will not be mentioned here, but it had much originalities never seen before.

2. Secondly, in "Selection of the experimental plants", he said, "If we adopt the strictest definition of a species, according to which only those individuals belong to a species which under precisely the same circumstances display precisely similar characters,..." By this we can see that Mendel had concern with the concept of species and had his own view or definition of a species.

3. In the chapter 10 of the follow-up test using beans, returning to the problem of diversity of color change of ornamental plants, i.e., the motive of the study, he developed ingenious reasoning in an attempt to explain and analyze the color diversity, using the laws obtained by the study of peas, discussing that there
was no difference in principle between ornamental plants and wild ones in the relationship of heredity and environment.

4. In the next and last chapter 11, "Concluding remarks", as I mentioned before, Mendel took up some problems which had been treated by his predecessors. By additional experiments, he extended his theory and applied it to the interpretation of these problems. The passage in which Mendel stated it is very remarkable and interesting to me. It runs: "Gärtner, by the results of these transformation experiments, was led to oppose the opinion of those naturalists who dispute the stability of plant species and believe in a continuous evolution of vegetation. He perceives in the complete transformation of one species into another an indubitable proof that the species are fixed within limits beyond which they cannot change. Although this opinion cannot be unconditionally accepted we find on the other hand in Gärtner's experiments a noteworthy confirmation of that supposition regarding variability of cultivated plants which has already been expressed."

Even from the above which I cited and referred to, we may get some idea of the pea paper, in which I intended to show that Mendel's concern on evolution is discernible as he conducted the experiments.

As to the paper on hieracia, as I said before, this study might have been Mendel's attempt at solving the problem of diversity of the species in this genus in the light of hybridization, and this paper may be regarded as one dealing with evolution rather than heredity. At the end of the paper, after explaining that hieracia are different from peas in their ability to produce constant hybrids and that this phenomenon is not very peculiar, for instance, as observed by Wichura in the willow, he stated as follows: "It is supposed from this instance that Hieracium also shows a case similar to it. As such is the case, whether or not the hypothesis that the polymorphism observed in the willow and hieracia has something to do with the peculiar behavior of their hybrids can be formulated is yet to be determined. This problem is worth discussing, but has not been solved." Although this paper of Mendel on hieracia seems to have been overlooked, I think it is a remarkable piece of the classic literature of evolution in view of what I mentioned in the above.

It may also be pointed out that various comments have been made as to
Mendel's selection of *Hieracium* as the experimental material, such as "he was kidnapped by Nägeli," "it was a mishap for Mendel himself as well as for the development of science and the history of science," or "it was the cause of his failure that he wrestled directly with a complicated matters as *Hieracium* without following Descartes' way of thinking as he did with peas."

But I don't agree with these criticisms. Here too, keeping the attitude of adopting the consistent method of experiment as the way of problem solving, Mendel patiently faced a new problem as it arose. No one but Mendel himself chose hieracia with more than enough knowledge of this group which is a genus with many problems, and so is a proper material for the study of the problem of evolution. It cannot be denied that such a contemporary specialist as Nägeli was of some help to Mendel in selecting this plant.

As Mendel mentioned in the paper, our attention is drawn to the fact that this study had progressed no further than the first step. It should also be noted that at that time parthenogenesis of this plant had not yet been discovered and neither Nägeli nor Mendel knew the phenomenon, which was discovered as late as 30 years after. If Mendel had known the phenomenon, many of his questions would have been solved and his work made easier. But I feel that Mendel would have reached this point with his brilliancy, if he had been given 10 more years or even a shorter time for its study. Because, judging from this second paper and also from other data such as found in his letters to Nägeli, Mendel already had the idea of self-pollination in connection with the second and later generations of hybrids which are sterile and constant. It seems likely that a person of Mendel's ability could have managed to reach the conclusion concerning the mathematical relation between the sterility rate of pollen and self-pollination rate, and furthermore the discovery of parthenogenesis itself might not have been impossible. It is even felt that he was just on the verge of an important discovery. The term "mishap" or "unlucky", must not be used for Mendel's selecting hieracia as his experimental material, but it may be applied to the fact that he was too busy to secure the time for study, holding the office of high-ranking holy order, and moreover, being involved in a tax problem, etc. To the contrary, I believe, this second paper confirmed the Mendel's attitude and intention of study as a scholar of evolutional
problems. This fact interests us together with the work of the Mendelians who, in later years, came to develop the population genetics which is intimately related to evolution.

In short, these two papers may be the results of Mendel's attempt to find some solution to the problems of evolution by discovering laws in the course of analysing the offspring in their numbers and behaviors of hybrids in successive generations. For this purpose, he at first made experiments on peas and then applied the laws found to other plant groups including hieracia in order to generalize them.

It may be noted that it was not Mendel himself who extracted laws from his papers nor did he name them Mendel's laws. This fact seems to indicate to a considerable extent that Mendel was interested in the laws found in his crossing experiments merely as a means of explaining the evolution problems faced in his contemporary age, rather than in classifying heredity laws. In fact the so-called Mendel's laws were first advocated in 1900 by those whom you would call re-discoverers of Mendel's laws. At first, H. de Vries pointed out the most important part of Mendel's heredity laws by entitling his first re-discovery articles: “Concerning the law of segregation of hybrids” (“Sur la loi de disjunction des hybrides”) and “The law of segregation of hybrids” (“Das Spaltungsgesetz der Bastarde”). The second re-discoverer C. Correns entitled his first re-discovery article as: “G. Mendels Regel über das Verhalten der Nachkommenschaft der Rassenbastarde”. He discussed in this paper, rather emphasizing, the problem regarding examples of exceptions to Mendel's laws. So he seemed to have used the word Regel, not Gesetz, I presume. At any rate, Correns also must have meant Mendel's heredity laws by G. Mendels Regel. It may be natural that Mendel's intention or concern with elucidation of evolutionary problems or evolutionary implications in his papers have not come forward, because soon after the re-discovery, importance of one side of his study, that is the side concerned with heredity, in other words, the first part of the pea paper, was emphasized, and that by influential scholars, such as de Vries, Correns and Tschermak. H. Hoffmann (1869) of Germany may be regarded as the first person who quoted Mendel’s pea paper, but his purpose was to criticize Darwinism and he did not deal with
Mendel's paper from the genetical or evolutionary viewpoint.

It goes without saying, or it may rather be self-evident, that Mendel's works are deeply concerned with heredity and evolution. However, there seems to have scarcely been publications which discuss Mendel's two genetics papers by studying and analyzing their contents, especially emphasizing their evolutionary implications.* That is why I have reported what I have long (since 1938) had in my mind at this occasion of the centennial anniversary of the discovery of the Mendel's laws held in Japan.

Reference


The birth of genetics. Supplement to Genetics. 1950.


The Publication Com. of Mendel Centennial Anniversary in Japan 1967. Upon the

* By courtesy of Dr. V. Orel in Brünn, I received a copy of “Fundamenta Genetica, 1965” Selection and commentary by Dr. Jaroslav Kríženecký, after the Mendel centennial anniversary was held in Japan in 1965. Dr. Kríženecký commented rightly on Mendel's definite standpoint towards the theory of evolution, etc.


Pancreas Known by the Chinese in the Middle Ages!

Saburō Miyasita*

Regner de GRAAF, Holland, was the first to study the pancreas and its secretions (1664). The description of the pancreas (1316) by MONDINO, Restorer of Anatomy, is very obscure and even in the fifth book of the Fabrica written by VESALIUS in 1543 pancreas is regarded as several glands (glandular bodies) but not as a single organ.

In Asia, there have long been used the terms of wu-tsang\(^a\) (five viscera, i.e. the heart, the liver, the spleen, the lungs and the kidneys) liu-fu\(^b\) (six bowels, i.e. the small intestines, the gall-bladder, the stomach, the lower intestines, the bladder and the san-chiao\(^c\)), but neither term contains pancreas. It was usual in medieval Chinese works on anatomy that the pancreas and nervous system were ignored. This, however, does not mean that Chinese had no knowledge of the pancreas at all. We should start with a brief review of the historical background in China and Japan on the basis of which studies on pancreas as an organ has been developed.

II

In Japan, it seems that KURIYAMA Koan\(^d\), one of the pioneers of human dissection, was the first who observed pancreas on the dissection of a woman’s body on May 21, 1759, in his native town, Hagi\(^e\). He did not regard it as a normal organ but he thought it a clot of blood and pus near intestines and stomach\(^f\).

In the Kaitai Shinsho\(^g\), the “New Work on Anatomy”, published in 1774, SUGITA Genpaku\(^h\) introduced the pancreas as one of internal organs on which Chinese had given no description or explanation at all. He translated the Dutch alvleesklier into the Japanese term takiriiru\(^i\) which was synthesized by a Japanese

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* Library, Takeda Chemical Industries, Ltd., Nisiyodogawa Jōsō, Osaka. I am much obliged to Mr. NARABAYASHI Motohide for rendering the present paper into English.

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word *ta* meaning large and *kiribu* a Japanese phonetical equivalent of Dutch *klier*(gland). The modern term *suizô*¹, which literally means the organ gathered follicules, was first used by Udagawa Shinsai², one of Sugita's followers, in 1805.

**III**

China was much slower to accept Western medicine in spite of the Jesuits' earlier efforts to introduce modern anatomy into China. Chinese lethargy in medical field stood in sharp contrast to Japanese activities inspired by Dutch medicine. Although Dr. Benjamin Hobson, a London Missionary Physician, applied the term *thien-jou*⁵ (sweet flesh) to pancreas in Chapter 19 of the *Chhüan-thi hsin-lun*¹, the "New Work on Whole Body", published in Canton in 1851, at present day it is called *i-tsang*⁶.

It is noticed that, in Chapter 4 of the *I-lin kai-tsho*⁷, the "Correction of Medical Faults", published in 1830, Wang Chhin-jen⁸ says that the common name of the *tshung-thi*⁹ (pancreas) is *i-tse*⁹. Nowadays *i-tse* means soap, but, until soap was first imported in China, *i-tse* probably meant the pancreas of the domestic animals. If it be so, when did Chinese begin to make use of the pancreas for wash?

First, the descriptions we find are as the medical toiletary. In Chapter 52 of the *Chou-hou pei-chü fang*⁶, the "Prescriptions for Emergencies at Hand", written by Ko Hung⁹ (283-ca.343), there are three kinds of the beauty wash in which included the pancreas of a hog (*chu-i*) though the pancreas is no basal constituent of the prescriptions. These medicines for external application gradually developed to thirteen varities, in which pancreas of sheep was employed also. These varities are described in Chapter 6 of the *Chhien-chin yao-fang*¹⁰, the "Prescriptions Worth a Thousand" (650/659), and more than half of them are quoted in Chapter 32 of the *Wai-thai pi-yao fang*¹⁰, the "Arcane Essentials from the Outer Tribunal" (752). These, however, were supplied in forms of not only beauty wash but also washing powder (*tsao-tou*) and toilet ointment (*mien-chih* or *mien-kao*).

Secondary application of the pancreas is found in the medicine for cough. There are two prescriptions, in both of which main constituent are the pancreas of hog, in Chapter 23 of the *Chou-hou pei-chü fang* written by Ko Hung. We read:
Prescription for a patient had sudden attack of cough.

Pancreas of a hog.
Cut in slice, boil it in vinegar and eat entirely but don’t take over two doses.

Prescription for a patient suffering from lingering cough and dizziness, in spite of therapy with medicines for such a long period of time as ten or twenty years.

Three pieces of total pancreas of hogs.

Hundred dates of Jujube.

These two ingredients are extracted with three sheng\(^{y}\) (ca.600cc) of sake wine for several days. Drink two or three ho\(^{z}\) (ca. 40-60 cc) of the extract and then gradually increase the amount to four of five ho (ca. 80-100 cc). If you do so, you will soon recover.

There seems to be substantially no doubt that the latter of the prescriptions was popular in the Thang period, because the latter is quoted in Chapter 17 of the Chhien-chin yao-fang and also in Chapter 10 of the Wai-thai pi-yao fang.

IV

It is just and reasonable that the above two applications were reflected in the pharmacology at that time. In Chapter 18, Section ‘Hog’ of the Cheng-lei pen-tshao\(^{aa}\), the “Classified Pharmacopoeia”, compiled by Thang Shen-wei\(^{ab}\) in about 1082, there are the descriptions quoted from the Chou-hou pei-chü fang, the Pen-tshao shih-t\(^{ac}\), the “Supplemented Pharmacopoeia”, written by Chen Tsang-chhi\(^{ad}\) in 739, and so on.

The anatomical explanation of the pancreas of hog is in Chapter 50, Section ‘Hog’ of the Pen-tshao kang-mu\(^{ae}\), the “Great Pharmacopoeia”, published in 1596. We read:

It is in the middle of kindneys and looks like hog-fat but not hog-fat and also looks like pork but not pork. Stating this about the body of human being, the location corresponds to the ming-men\(^{af}\) (the acupuncture point between the kindneys) from where the san-chiao appears.

Li Shih-chen’s\(^{ag}\) description mentioned in the above is still obscure\(^{2}\). In general, the san-chiao (three burning spaces) was considered as the organ showing such a physiological function that the disorder of which resulted in a diabetes
(hsiao-khe⁸⁸⁹⁹⁹⁹) under certain circumstances, but being of invisible form. Chinese anatomists, indeed, failed to make sure of the san-chiao on the human dissections neither in 1045 nor in the Chhung-ning⁸⁷³ era (1102-1107)⁸. It is sure that practical knowledge of butcher had not been sufficiently reflected in the anatomical works or materia medica.

V

Other applications are read in the works of the traditional Chinese technology written in the Ming⁸¹ period.

The pancreas of hogs were used for the washing off the gum from raw silk. These technical descriptions are in Chapter 2 of the Thien-kung khai-wu⁸⁹, the “Creations of Nature and Men” (1637)⁹. Dazzling luster of silk fabric was produced by means of the boiling with an aqueous solution of ashes and then of the steeping overnight in an aqueous mixture of the pancreas of hogs. In Chapter 15 of the Pien-min thu-tsuau⁸¹, the “Collected Diagram for People’s Convenience” (the second half of fifteenth century), the method of preparing the pancreas for scouring is described. The technique in which we suppose the action of the digestive enzymes of the pancreas is made use of is thought to form part of the silk manufacturing processes.

In Chapter 14 of the Pien-min thu-tsuau⁸¹ there is a description that the pancreas of hog is used also for the food technology. The art to soften meat of birds by cooking with the pancreas of a hog is described.

So, workmen of silk textiles or of cooking must have had the actual knowledge of the pancreas in the medieval China.

VI

In short, Chinese had known the pancreas though it was taken from the domestic animals. Stating further, it was, not only observed but made to use for living, after the first half of the fourth century. The anatomical description, however, was rather poor in contrast with the plenty of applications. Under the social and economic system of the medieval China, after all, medicine was not established as a modern science.

In this connection, it should be taken into consideration that inhibited development in the culture as a whole acted to retard the further development of
medicine. Was Chinese imperviousness to external stimuli of medical science, in particular occasion of the Jesuits at Peking, caused by the same circumstances?

References


2. It is mentioned that i is san-chiao in brief in Chapter 4 of the Pen-ching feng-yüan (1965), but there exists no evidence.


4. The chu-i is put into the hog-fat in the translation by Sun E-tu Zen and Sun Shiou-chuan(1966). This is not correct because hog-fat is chih in a solid state and kao in a liquid state, and furthermore i was counted as the common unit chü (numerative of corpses, utensils, etc.) to internal organs but not as the sheng to fat. Indeed, there are the prescriptions which indicated how to remove fat or blood from chu-i in the medical works mentioned above.

The Equivalent Characters for the Chinese and Japanese Words Appearing in the Text

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<tr>
<th>a) 五臟</th>
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<td>e) 茔</td>
<td>f) 解體新書</td>
<td>g) 杉田玄白</td>
<td>h) 大機里兒</td>
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<td>i) 膀胱</td>
<td>j) 宇田川榛齋</td>
<td>k) 甜肉</td>
<td>l) 全體新論</td>
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<td>m) 胰臟</td>
<td>n) 醬林改錯</td>
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<td>q) 弤子</td>
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<td>u) 千金要方</td>
<td>v) 外臺秘要方</td>
<td>w) 澡豆</td>
<td>x) 面脂，面膏</td>
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<td>y) 齔</td>
<td>aa) 證類本草</td>
<td>ab) 唐憲徽</td>
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<td>ac) 本草拾遺</td>
<td>ad) 陳藏器</td>
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<td>ak) 天工開物</td>
<td>al) 便民圖纂</td>
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This book deals with the development of astronomy in Japan before the Meiji period except the last chapter. In the early periods (6th-16th cent.) Chinese astronomy was dominant in Japan. Then Western astronomy was gradually introduced into Japan after the middle Edo period (18th cent.). Therefore, in the Edo period there existed two different types of astronomy about which Dr. Nakayama explains in detail. It is his main intention to make clear the reason why modern astronomy could not have been established by Japanese scholars in the Edo period in connection with the coexistence of Chinese and Western astromonies. In this book the reason is well analyzed from the view-point of internal development of astronomy as well as that of social, cultural, and philosophical backgrounds of Japan in those days.

This book consists of the following three parts:

Part I The early impact of Chinese astronomy: from the sixth century to the early sixteenth century.

Part II The early impact of the West: from the late sixteenth century to the early eighteenth century.

Part III The period of recognition of Western supremacy: from the mid-eighteenth century to the late nineteenth century.

In addition to the above parts, there are ten Appendices and well prepared Bibliography.

In Part I Dr. Nakayama discusses how Japan introduced Chinese astronomy in the early periods (6th-9th cent.). In this connection, he describes Chinese calendar-making, cosmology, and other astronomical activities which were carried out at Chinese bureaucratic institutions in the T’ang dynasty. Through the analysis of Japanese acceptance of Chinese astronomy, Dr. Nakayama makes an interesting
remark that there was an important difference between Japanese and Chinese astronomical activities although both were mainly carried out at their bureaucratic institutions. Namely, at the Yin-yang Board to which all Japanese astronomical officers belonged astrological activities played more important role rather than calendar-making which was the main job of court astronomers in China. Because of the above recognition, he pays much attention to discuss the Japanese astrological activities about which few works have been written before. General readers might be impressed by the fact that a solar eclipse was still feared by Japanese court people even after its prediction was done by astrologers, and that considerable numbers of solar eclipses, which never occurred, had been recorded by the astrologers.

In Part II Dr. Nakayama discusses the introduction of Western astronomy which was mainly carried out by the Jesuits in the 16th to 17th century. The Western astronomy introduced was characterized by the medieval one which had rather poor contents with the lack of knowledge on calendar-making. However, he emphasizes the importance of the Kenkon bensetsu (Western cosmography with critical commentaries) and the Nigi ryakusetsu (outline theory of terrestrial and celestial globes) which first introduced Aristotelian cosmology into Japan. Besides, Joken Nishikawa found two approaches to astronomy: the heaven of meiri (metaphysical) and that of keiki (physical). It is pointed out by Dr. Nakayama that the shou-shih calendar, an outstanding achievement of Chinese exact science, was first studied in Japan in this period, and that Harumi Shibukawa finally compiled the Nihon choreki (a comprehensive chronology of Japan) in which he attempted to reproduce the calendrical system used in ancient Japan.

In Part III Dr. Nakayama deals with the period starting from 1720 and ending at the early Meiji period (about 1880). In the 1730's the ban on Jesuit works was modified. Therefore, astronomical knowledge was transmitted to Japan through both Dutch books and Chinese versions of Western astronomy. The heliocentric idea was first introduced into Japan by Ryoei Motoki, an official interpreter at Nagasaki. One of Motoki’s students, Tadao Shizuki, was known as the person who first tried to reconstruct Newtonian scheme in terms of Eastern natural philosophy. One can recognize the fact that Japanese astronomical studies arrived
at higher level, comparing with those in China in those days. Dr. Nakayama explains correctly the above situation: “although the Chinese had access to many Jesuit treatises on Western astronomy in their own language, they were generally indifferent to Western learning, whereas Japanese interpreters labored prodigiously to learn about Western accomplishments.”

The activities of Asada school of non-official astronomers were examined in detail, including their studies on a Dutch translation of Lalande’s *Astronomie*. They formulated the *hsiao-chang* method according to which the values of almost all astronomical parameters undergo. Dr. Nakayama highly evaluates the *hsiao-chang* method about which he published a series of interesting papers in the *Kagakusi kenkyu* (Journal of History of Science, Japan) in 1964. On the basis of his own research Dr. Nakayama maintains that the method was the only original idea in Japanese astronomy.

I have shortly summarized Dr. Nakayama’s newly published book. There is another book on the development of Japanese astronomy before the Meiji period, which was written by several authors and published by the Japan Academy in 1960. However, Dr. Nakayama’s book is more nicely arranged along his own line than the above mentioned book that is a kind of collected paper of several authors. It is very difficult to make any criticism about Dr. Nakayama’s book from the view-point of internal development of astronomy itself as well as social, cultural, and philosophical backgrounds of Japan.

Dr. Nakayama’s book is based on his thesis to Harvard University (1959). Some parts of the thesis have been rewritten according to his recent researches. His effort to improve the first draft during these ten years should be deeply appreciated. I, as one of his colleagues in Japan, am delighted to recommend this book to those who are interested in the field of Japanese studies as well as that of history of astronomy. The book provides useful informations on the process of a previous underdeveloped country who has arrived at the international level of science.

Kiyosi YABUUTI (Ryukoku University, Kyoto).
The History of Science Society of Japan met for its 16th annual meeting on May 3 and 4, 1969 at the Institute of Industrial Science, University of Tokyo. The following papers and special lectures were presented on that occasion.

**May 3**

Female Scientists in Japan  
Aiko YAMASHITA & Akashiko YOSHIMURA

The Dutch Translator, Lulofs, of J. Keill's Work  
Tadashi YOSHIDA

On Gennai HIRAGA  
Aiko YAMASHITA

Aikitsu TANAKADATE Materials  
Kunio AOYI

Torahiko TARA’s Philosophy of Science  
Yōichirō FUJI

The Influence of *Rigaku Shoho* on the Physics of Early Meiji Japan  
Mampei HASHIMOTO

Six Little-known American Science Teachers in Early Meiji Japan  
Masao WATANABE

Jūgō SUGIURA: The Introduction of Western Scientific Thought into Early Meiji Japan  
Kiyohisa FUJI

Jun-ichi SATŌ in the History of Expedition in Japan  
Junkichi NEMOTO

Japanese Chemists in the Early Meiji Period  
Kunika SUGAWARA

The Evaluation of Scientific Works in Japan  
Minoru TANAKA

**May 4**

Education in Descriptive Geometry in the Early Meiji Period, mainly at the University of Tokyo and at the College of Engineering  
Masatoshi HARA

Cultural Properties related to the Engineering in Modern Japan (I)  
Toshiro YAMAZAKI

The Development of the Tone Water System  
Yoshinori KANEZERI

The Research Organization for Electric Industry at the Turn of the Century  
Chikayoshi KAMATANI

Academic Societies and Universities in Nineteenth Century Germany  
Shigeru NAKAYAMA

On Aristotle’s Theory of Matter  
Hiroshi ICHINO

History of Hindu Physics (V)  
Isao OHAMI

Theories of Atomic Structure in the 1900’s  
Eri YAGI

Mathematical Items in the *Oxford English Dictionary*  
Tamotsu MURATA

History of Models of Nucleus (II)  
Shigeki MATSUO

Educational Significance of de Saussure’s Work on Photosynthesis  
Kazu MAFUNE

Biochemistry in Japan in the 1920’s  
Tatsumasa DOKE

On the Adoption of Population Genetics in Japan  
Zenji SUZUKI

**Special Lectures**

Science, Technology and Cultural Properties Protection  
Masaru SEKINO

Recent Studies in the History of Animal Husbandry  
Gichi KAMO
Errata


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No. 7 (1968)

Survey

History of the Chemistry of Taste in Japan:
Yojiro TSUZUKI and Aiko YAMASHITA

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La Physique de Descartes: Eizo YAMAZAKI
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On the Alleged Independent Discovery of Kepler’s Third Law
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Review

Meiji-zen Nippon Kagakushi (Histoire des sciences en Japon
avant l’ère de Meiji, en japonais), éd. Académie de
Japon, Tokyo, 26 volumes, 1954-1968:
Suketoshi YAJIMA
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1. Summary articles, surveys of a research field, source materials, documents, and original papers are acceptable.
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3. The title and name of the author should be typed on a page separate from the text, with the present position of the author indicated at the bottom.
4. The text should be typed double-space on paper of standard size and weight, with about 65 spaces per line, 25 lines per page, and margins of 4 cm. at the top and 3 cm. at the left.
5. All special type such as italics or Greek letters should be so designated in red.
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7. Figures and tables, other than simple unlined types, should be prepared on standard size paper apart from the text, and their location in the text should be indicated in red.
8. Figures should be drawn in black ink on white paper at actual size, one figure per page, and gathered together at the close of the text.
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2. In addition to the manuscript, the following information should be provided on a separate page: 1) the title of the manuscript, 2) the author's name, 3) present position (title, address, phone), 4) home address and phone, 5) preferred place of contact, home or office, 6) the number of copies of offprint of the paper which the author want to secure.
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