MASS-SPECTROMETRY - THE DEVELOPMENT OF A SCIENTIFIC COMMUNITY

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ABSTRACT

The aim of this thesis is to form a realistic theory of the process of scientific innovation, based on an account of the history of the development of mass-spectrometry from 1911, when Sir J.J. Thomson first invented the technique, to the 1950's, when the technique first became commercially available. Four stages of development are considered:

DISCOVERY - this section is concerned with prevailing 19th century attitudes, concepts and theories in science; a description of the contemporary scientific community, contrasting the late Victorian scientific world with that of modern research; Sir J.J. Thomson, his work at the Cavendish, and the events leading up to the invention of the first Parabola instrument; F.W. Aston, the Mass-Spectrograph and controversies and problems of isotopes.

DIFFUSION - this stage examines the dissemination of the technique to America and Germany; technological innovations and sophistication of mass-spectrometric technique; the controversies over Deuterium and Tritium.

CONCENTRATION - this section looks at the changes in science brought about by World War II and the Manhattan Project; the role of mass-spectrometry in Uranium separation, and the gradual spread of the technique into other fields.
EXPLOITATION - considers post-war technological advances, the spread of mass-spectrometry into other areas of science, and commercial manufacturing and marketing.

This account is then examined using several methodological tools to render it suitable for sociological analysis. A new bibliography of mass-spectrometry has been compiled; bibliographic and citation analysis has been carried out; twelve pioneer scientists in the field have been interviewed, and the current generation of mass-spectrometrists have been surveyed by questionnaire. The development of the community of mass-spectrometrists as evidenced by a variety of indices is compared and contrasted to other sociological investigations, thus this thesis contains a review of current sociological thought, in particular concerning the expansion of science, and the relationship between technology and science.

From this elucidation of the development of a central problem in science, in particular an examination of the flow of information and ideas, it is then possible to assess the degree of 'fit' of this case study to a synthesis of sociological theory.
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PART ONE: INTRODUCTION

Chapter I indicates the context in which this thesis has been undertaken, the choice of topic, its aims and goals, the methodology of the research, and an outline of the science and technology of Mass Spectrometry.

Chapter II reviews the spectrum of relevant sociological thought concerning the generation of scientific discovery, the organisation of science and scientists, the motivation and socialisation of scientists, aspects of the philosophy of science, and the relationship between science and technology.
INTRODUCTION AND METHODOLOGY

1.1 Preface:

This project - to study the growth of a scientific discipline - originated in the early 1970's when "Black Boxism" the sociology of science was problematic, and there was a paucity of studies of sizeable scientific communities.

My first degree - "Human and Physical Sciences" - at Surrey University had already provided an interdisciplinary context for research; consequently I sought to extend my work into the sociology and history of science, where an undergraduate training in the physical sciences would allow for a better appreciation of the subject matter, i.e. science and scientists.

I wished therefore to use a variety of approaches to try to produce a "total" study, to reconstruct and elucidate the development of a central area in mainstream science, and in particular to establish the flow of information and ideas which led to specific scientific advances.

1.2 Aims:

I was concerned to:

i) Examine to what extent existing sociological theories could account for the various growth patterns in this macro-study.

ii) Explore the relationship between the advancement of science and technology.

iii) Trace the development of the scientific community, and look both at interrelationships and at relationships with the wider scientific world.

iv) Draw together a realistic description and explanation of the historical and contemporary position.

v) Compare this new historical account with previous part-accounts, with particular reference to scientific error, and to 'read between the lines' of existing publications.
In order to correlate the results of a mainly historical investigation with current sociological theory, I needed to find a scientific problem area with the following principal topic and community characteristics:

i) The subject matter should be mainstream science.

ii) It must concern a fairly small visible easily definable scientific community to enable the construction of as complete a picture as possible, with no one 'missed out'.

iii) It must have taken a long time to develop, so that various occurrences of advancing, static and retrograde movements in theory and technology could be located.

iv) It must be an important problem area, so that a considerable number of scientists will have discussed and disagreed violently about it.

v) It must contain living pioneer scientists.

vi) It must be an area in which I had access to special scientific knowledge and expertise in order to interpret the facts contained in published work, and to be able properly to interview scientists working in the field.

1.3 Choice of topic:

The early history of the Mass-Spectrometer and its application to the discovery and subsequent problems of Isotopes and Ion-Molecule reactions (covering a period of approximately 40 years from 1910 - 1950) is an extremely important area in science; it concerned several Nobel Prize winners and adequately meets the other criteria.

Mass-Spectrometry lies in between Chemistry and Physics; its practitioners were easily identifiable before 1950 because mass-spectrometers were difficult to build and temperamental in operation. The number of instruments was therefore small and a mass-spectrometrist was someone who had access to and worked on a mass-spectrometer. Such scientists could be regarded as only a technique-based community but it is evident that they and others saw them as a separate and recognisable group. As one mass-spectrometrist has commented,

"One of the very attractive features of the field of mass-spectrometry when I first entered it was the camaraderie that existed between the workers in the field. This was sufficient to ensure that most people had a fair knowledge of what their colleagues in other laboratories were doing."

For many other reasons mass-spectrometry regards itself as a community - there are many speciality-based discussion groups and conferences, the field
is serviced by a number of specialist journals and periodicals, (see concluding chapter) and it is evident that other areas concerned with problems rather than techniques do not welcome the intrusion of mass-spectrometry. For example, the journal 'Transactions of the Faraday Society' (now 'Journal of the Chemical Society of Faraday Transactions'), the leading British physical chemistry journal, was for many years reluctant to publish papers on ion-molecule reactions, kinetics of decomposition etc, and many British mass-spectrometrists had to publish in foreign journals. In any case, the idea that mass-spectrometry is only a tool is really significant only after the technique became commercially available in the early 1950's. If mass-spectrometry is a community now, it surely was much more in the days when scientists had to build their own.

Other reasons for choosing mass-spectrometry were that the problems tackled with its aid were in the mainstream of science during the first half of this century, thus many pioneer practitioners still would be alive. Its early rate of development was slow, and incorrect theories had had time to get into the literature before they were refuted.

Finally, this thesis has been carried out under joint supervision, where one of my supervisors was a practising member of the community of mass-spectrometrists and possessed the necessary contacts and technical knowledge.

1.4 Mass-Spectrometry;

The mass-spectrometer is a classic example of an instrument that starts life as an esoteric laboratory contrivance and becomes one of the great general tools of science. It grew out of the electrified glass tubes with which physicists liked to experiment in the 1880's; today it is used as a research instrument in fields as widely diverse as chemistry, geology, medicine and space research, and is an indispensable piece of equipment in many various industries. As recently as 1940 there were only a couple of dozen such instruments in operation in the world; now there are many thousands, and their usage steadily grows.

A mass-spectrometer is primarily a device for determining the weights of atoms and molecules. It also can separate and identify what components are present in complex mixtures of atoms and molecules, and can indicate their relative abundance.
Many designs have been invented, but all consist of three stages:

**IONISATION** → **DISPERSION** → **DETECTION & ANALYSIS**

First, there is an ionisation stage in which the atoms or molecules are given one or more positive charges. At the same time molecules tend to break up so that from the ionisation section emerge not only ions (ie. charged atoms or molecules) of the parent substance but also fragment ions resulting from the break up of the molecules.

These ions next enter a dispersion stage where they are separated by electric and magnetic fields according to their mass-to-charge ratio \( \frac{M}{Q} \), and finally they enter a detection stage where the ions are detected to give a so-called mass-spectrum. Strictly speaking, if the ions are detected by being focussed onto a photographic plate, the instrument is called a mass-spectrograph; onto a fluorescent screen a mass-spectroscope. Only if the ions are detected electrically is it really a mass-spectrometer. However, the term mass-spectrometry is generally regarded as covering all fields.

The mass-spectrometer is most widely used nowadays as an analytical tool. In the mass-spectrum of a substance, the peak at the highest \( \frac{M}{Q} \) value gives the atomic or molecular weight of the substance. In addition, the pattern of fragment peaks is a 'fingerprint' for the substance and frequently allows it to be identified unequivocally. This application, though responsible for the mass spectrometer's commercial success, is not glamorous, and mass-spectrometrists themselves tend to be interested in more esoteric applications of which there are a large number. Many have, or would like to have, a mass-spectrometer for use in their own research, and regard the provision of an analytical service as a chore.

1.5 Methodology:

As previously stated, there are three main concerns to be studied in order to undertake sociological analysis:-

i) The technical development of mass-spectrometry.
ii) The evolution of the prevailing physical and chemical theory.

iii) The emergence of mass-spectrometrists as a recognisable community.

In the attempt to compile a comprehensive history of mass-spectrometry, I depended partly on the recollections of scientists who had been involved in its development or who had been on the sidelines as it had happened. (See Volume 2, Appendix IV). It is also necessary to establish quantitative and objective standards and measures against which the collection of anecdotal history can be assessed. Through this multiplicity of approaches appear certain relationships, additive trends and processes.

1.6 Bibliographic Analysis (See Appendix II)

A convenient measure of the size and growth of a scientific discipline is the number of papers in that field published in the scientific literature. Within the field, specialities develop and their relative growth can be assessed by sorting papers into relevant categories. For example, these have been counted and analysed for physics by de Solla Price who was able to regard Physics Abstracts as embracing all physics. The task is more difficult for mass-spectrometry because of the lack of a standard bibliography. For 1909 to 1937 I compiled my own bibliography by searching the literature, (App. I.). From 1938 to 1962 I used several bibliographies published by Associated Electrical Industries (App.II.4) and from 1967 onwards I used the comprehensive bulletin published by the Mass Spectrometry Data Centre - MSDC (App.II.4). The period 1963 to 1965 is covered by a bibliography compiled by McLafferty (App.II.4), so that only 1966 is missing. The number of papers published each year from 1909 to 1974 calculated from these bibliographies is shown on a semi-logarithmic scale in App.II.5. At first sight the graph suggest that tremendous growth took place in 1962-3 and 1965-7 but it is much more likely that changes of classification took place in these periods and that the later bibliographies made a wider selection of papers. I have adjusted for this and obtained a more accurate growth curve by counting the entries in Chemical Abstracts subject index under 'Mass Spectra', 'Mass Spectrometers' and 'Mass Spectroscopy' from 1956 to 1973. The resulting curve was matched at its lower end with the AEI data, and the MSDC data were matched to its upper end to give the overall curve shown in FigX.App.II.5. This may be thought of as the growth in numbers of papers published by the community of mass-spectrometrists; the upper,
MSDC curve represents total usage of mass spectrometers and includes many papers by people who were not mass-spectrometrists but found someone who would run mass spectra for them.

For the period 1920 - 1940, Mass-Spectrometry experienced a growth rate of approximately 5% per annum, followed by a decline during the war years, then taking off again in 1946 and settling down to an average yearly growth of approximately 16% per annum. (de Solla Price has shown that science experiences an exponential expansion with a doubling approximately every 15 years). There is a very good degree of 'fit' between the A.E.I. bibliography and my own. It would have been surprising if this were not so, for at no time until after the Second World War did the number of papers published annually exceed 35. It is relatively easy to ensure that one is counting all the papers in an area of science that is emergent, publishing in a few journals, and not showing marked tendencies to branch into sub-fields.

Fig.App.II.8 shows the mass-spectrometry curve compared with the number of papers for all physics obtained from Physics Abstracts, and an estimated total number of papers in Chemistry derived from the number of columns (adjusted for variations in column size) in Chemical Abstracts. These curves are similar in shape.

Fig.App.II.11 shows the ratio of papers in mass-spectrometry to total columns in Chemical Abstracts. Mass-spectrometry has claimed a slowly increasing proportion of total papers since 1909 but the ratio shows sudden rapid jumps after each World War. It also shows a sudden jump in 1963 and subsequent rapid increase. The effect is too large to be attributed to the problems with bibliographies discussed above, and coincides with the commercial introduction of high resolution mass spectrometers, (e.g. the AEI M59) and relatively cheap, relatively small instruments, (e.g. the MS 12 and quadrupole instruments). In addition the entry of the Japanese into the US/British/German market increased interest and sales pressure.

Though mass-spectrometry started off as a single discipline, it soon subdivided with separate groups of scientists working on instrument design, isotopic analysis etc. The growth of these sub-disciplines is shown in App.II.6. Problems again arise in the transition between bibliographies in
AEI's category "Application to isotopic problems - isotopic analysis of solids, determination of atomic masses, isotopic analysis of gases" appears similar to the MSDC's "Isotopic analysis, precision mass measurement, isotope separation and ageing etc". On detailed inspection, however, it transpires that about 75% of the papers in that latter category deal with geological ageing whereas such papers are largely omitted from the former category. In spite of these discrepancies it is clear that the application of mass-spectrometry to isotopes, which dominated the early years of the subject, has become relatively of less importance, and that the growth areas since the Second World War have been Organic Chemistry (which includes analysis of organic compounds) and Atomic and Molecular Processes.

In the early years of mass-spectrometry, research groups grew up around the few instruments which existed. It is therefore possible to classify papers published up to 1940 according to the group from which they came, and this classification is shown in App.II.2 & 3. F.W. Aston was prolific after the First World War but had no research students and his early dominance in the field was challenged by groups of US workers and by J. Mattauch's group in Vienna. Between 1924 and 1929, nine laboratories had published their first papers alongside the pioneer contributors from Cambridge and Chicago. The different groups tended to work on different aspects of the subject so that the rise in sub-disciplines was related to the building-up of research groups outside Cambridge.

1.7 Citation Analysis:

The use of citation levels as a measure of the importance of a scientific paper has been widely used and also criticised, partly on the grounds that a paper which was visibly and spectacularly wrong might be widely cited for this negative reason, and partly because a first class review article could pre-empt further citation of papers in it. Garfield has also studied the development of molecular biology with the aid of citation 'trees' in which selected papers were traced back to the seminal paper in the subject.

I have constructed a citation table for mass-spectrometry papers published before 1940. The top corner of this very large table is shown in App.II.12 & 13. All papers were listed chronologically along the top of the diagram (representing papers cited) and down its side (papers citing). When a paper cited another paper, a mark was placed where the row intersected the column. This diagram makes it possible to see which papers were widely cited and at
what times. One can pick out papers whose conclusions were incorrect
and see if they are indeed extensively cited and can see also if review
articles have pre-empted other citations.

In the period covered by this citation analysis most papers are not cited
many times, and the number of widely cited papers are few. It is also evident
that theoretical papers are quoted much longer than experimental ones.

A citation analysis of this type has other uses - it is possible to
determine what de Solla Price has described as the "Immediacy Factor" -
the phenomenon of more frequent citation of recent papers relative to
earlier ones, indicating their rate of 'obsolescence' and also the degree
of 'fashionableness' in research topics.

A number of citations in the mass-spectrometry literature, of course,
are of papers outside the field of mass-spectrometry. In other specialities,
the proportion of outside citations is approximately 11%. In mass-
spectrometry there were 310 publications between 1910 and 1937. The number
of outside citations was 128 out of a total citation count of 760, that
is 14.4%. In the earlier pioneer period, 1910 to 1928 however, the
percentage of outside citations was 21.8%, whilst that for the later period
1929 to 1937 was 11.5%, very much in accordance with the typical average
of 11%. The high proportion of outside citations between 1910 and 1928 was
due to a large number of references to physical measurements (e.g. the
atomic weight of oxygen) made by methods other than mass-spectrometry and
used for purposes of comparison.

On the basis of the citation analysis, it is also possible to construct
a citation 'tree' and this is shown in App.II. Papers are shown
according to the year in which they appeared and are coded with a letter
identifying the principal author or research group and a number. Thus
HOG1 represents the first paper produced by Hogness and his group and
inspection of the 'tree' shows that it was published in 1924 and cites
HDS4 and HD85 and D2, papers by Smyth and Dempster respectively.

A striking feature demonstrated by citation analysis is the rapidity which
the various research groups carved out fields for themselves and became
isolated from each other. Note, for example, the small number of citations
by Smyth's US group of Aston's work at Cambridge and vice versa. No doubt
there was an element of chauvinism in this, and the groups were indeed
working on different problems. Nonetheless, the scientists involved seem
to have avoided direct competition. This may be because scientists are
seeking to work on topics which potentially produce the
up, they avoid areas where a colleague has been working for many years. A further possibility is that they might feel that a new technique opened up so many opportunities that it would be foolish to compete when there could be prizes for everybody.

1.8 The Professor-Student Relationship

The process by which a professor trains students who themselves become professors and train further students is one method by which scientific communities conventionally expand. There is a family tree among organic chemists deriving from Alfred von Bayer, and modern British physical chemists stem from the legendary figure of Sir Harold Hartley.

The professor-student relationship, however, is not the only way communities grow. It is possible that scientists move into a field simply because it interests them, that they learn about it from books and by talking to people at conferences.

I have therefore constructed a family tree of mass-spectrometrists and this is shown in App.II.1. An outstanding feature is that neither J.J.Thomson nor F.W.Aston had any 'descendants'. Aston himself refused to supervise research students, and Thomson's students, though he had several, never made much of a mark on the academic world and none of them became mass-spectrometrists. (It was Rutherford who nurtured the outstanding students).

1.9 Questionnaire

To bring the family tree up-to-date, I sent out questionnaires to all the United Kingdom mass-spectrometrists who could be identified and asked for details of how they entered the field and received their training. The questionnaire results will be discussed in the concluding chapter, (see Appendix III).

1.10 Summary

I have tried in this introduction to set out the aims and goals of this thesis, to identify particular fruitful avenues of exploration, and to indicate the methodology utilised in the gathering, quantification and
sociological theory, I shall examine various stages in the historical development of mass-spectrometry, commencing with nineteenth century contemporary scientific thought and leading through to the commercialisation of mass-spectrometry as an analytical technique.

Sections will be discussed and summarised as indicated, and the concluding chapter will contain further results and sociological discussion.
REFERENCES

6. ibid.
8. GARFIELD, E., SHER, I.H. & TORPIE, R.J., "The Use of Citation Data in Writing the History of Science", Institute for Scientific Information, Philadelphia, (1964).
9. DE SOLLA PRICE, op.cit.
10. ibid.
2.1 INTRODUCTION

In this thesis, which is an analytical examination of the birth and growth of a scientific sub-discipline, I have endeavoured to use a plurality of tools - in effect a methodological 'triangulation' - in order that the multifaceted picture is examined satisfactorily.

Essentially, I have approached the research from both historical and sociological perspectives, and, at least at the outset, I have not held any particular preconception of theoretical interpretation, nor allied myself at the outset to any particular orthodoxy.

Sociologists have often tended to generalise either from uncritically accepted evidence, or from evidence diligently assembled but restricted in time and place; conversely, historians have not always compared or conceptualised their research. Sociologists have not always avoided mistaking correlational links for adequate causes, and some historians have relied on dogmatically asserted intuitions where only cautious inferences would have been in order, (C.F. Whitley in Ref. 2).

There are advantages in studying past events - the perspective is clearer and virulent attitudes are to some degree attenuated. But the study of completed events is forever limited to recorded facts, which are likely to be a small remnant of a multitude of relevant aspects. The student of contemporary society, on the other hand, suffers almost from an embarrassment of data so he must select those portions that bear directly on a specific problem. In other words, the sociologist creates his own sources, in line with hypotheses which he has formulated. However, there are many pitfalls, especially if data are selected to fit a methodological device rather than the other way around. In that case, what is statistically significant may be of
little relevance theoretically or materially. This problem has been discussed at some length by R.D. Whitley, who states:

"Science as an important area of human activity has attracted the attention of historians and philosophers as well as sociologists. Sociologists, in their attempt to establish an independent sub-discipline, the Sociology of Science, have too often disregarded work in the History and Philosophy of Science, and moreover, have separated the Sociology of Science from the Sociology of Knowledge. This has resulted in a conception of the sub-discipline that suggests it should be devoted to the study of the producers of science without much or any reference to the internal cognitive nature and form of science." 2

As an illustration, Norman Storer 3 sees the sociology of science as being concerned with the behaviour and organisation of scientists. The internal organisation of science, science as a social institution, as a profession and as a communication system are the chief areas of study.

Sociologists of science have sometimes produced equivocal results because they were not sufficiently conversant with scientific practice fully to understand and appreciate the subject of their investigations. Consequently, several contributions to theories of innovation and scientific advance are constructed on the basis of the work of a few scientists concerning topics only peripheral to scientific advance.

This approach excludes any discussion of the subject matter of science itself, and it largely ignores the cognitive aspect of scientists' activities. By assuming that the cognitive aspect is non-problematic, a large number of sociologists of science have adopted a particular view of scientific knowledge, where scientists are assumed to be perfectly rational in their cognitive activity - there is but one method by which assertions about the world may be examined, and every scientist knows and can apply this method.

The sociology of science, in this view, is the study of persons who practice the scientific method, how they learn it and what rewards they receive. The nature of this scientific method, seems to consist principally of diligent systematic observation of nature, or as Robert Merton states: "... Knowledge is the accumulation of observations which provide the basis for empirically confirmed and logically consistent predictions." 4
2.2 THE NORMATIVE BASIS OF SCIENCE

The basis for much of the current orthodoxy in the sociology of science is provided by the work of Robert Merton and his more or less faithful disciples, and his work has provided a series of benchmarks against which the work of other and subsequent sociologists of science can be measured.

2.2.1 R.K. MERTON

Merton's functionalist approach to the study of the social organisation of scientists and their research maintains that

"... Certified knowledge will accumulate automatically as a consequence of reasonable conformity to the institutional norms of science, which are held to be binding on the man of science and they are legitimatised in terms of institutional values." 5

Merton assumes that intellectual development can take place only in 'open communities' where certain values such as 'open-mindedness' and the 'quest for truth' are upheld. As science has certainly progressed more rapidly than other intellectual movements so the scientific community must be more open than other social groups. Merton states:

"The institutional goal of science is the extension of certified knowledge. The technical methods employed toward this end provide the relevant definition of knowledge: empirically confirmed and logically consistent predictions. The institutional imperatives (mores) derive from the goal and methods. The mores of science possess a methodologic rationale but they are binding, not only because they are procedurally efficient, but because they are believed right and good." 6

Merton conceptualised four institutional imperatives and Barber later adds two more which he considers important. They are:

UNIVERSALISM

The norm of universalism involves the assumption that physical laws are the same throughout the universe and that the validity of a scientific statement has nothing to do with the character of its author. A statement may initially be accepted because of the known integrity of its author (or rejected for the opposite reason) but in the long run it must be confirmed by other workers and subjected to 'pre-established impersonal criteria'. A corollary of
this assumption is that scientific careers should be open to anyone of talent. To restrict entry to the community of science on grounds other than lack of competence is to prejudice the furtherance of knowledge. It is this assumption too, which makes science international. If physical laws are the same all over the world, then what a Russian scientist discovers about the atom will be valid in America and his work will be appreciated by scientists everywhere whatever political ideologies may divide them. Storer sees this norm as being orientational rather than directive, in this context. Universalism affects the whole community of science and scientists in an ideological manner, rather than being a directive consciously affecting the thoughts and actions of the individual scientist.

COMMUNISM
The norm of communism directs the scientist to share his findings with other scientists, for if science is universal, then the findings will apply also to their work. Knowledge that is not in the public domain cannot be part of the accepted body of knowledge to which other scientists refer, and against which the scientific community measures the creativity of other scientists. The implication here is that the scientist wishes to be a member of the scientific community and to be judged by the community and that consequently he will take the initiative in placing his work before other scientists. A consequence of this norm is that a scientist's findings do not 'belong' to him: he has no 'property' rights, but in return for publication of his work he will receive recognition, esteem and membership of the community, which, if justice is done, will be in line with the significance of his discovery. Someone discovering and operating a 'secret' process would be seen as not a 'true' scientist, and indeed the taking out of patents is regarded with suspicion by the community. Given that a scientist's reward for his discovery is the ensuing recognition, his concern with scientific priority becomes a natural response to this norm.
DISINTERESTEDNESS

The orientational norm of disinterestedness as Storer interprets it, makes it illicit for the scientist to profit personally from his research. Merton relates it almost exclusively to prohibiting the scientist from making the search for professional recognition his explicit goal. Barber, however, pays closer attention to how this norm dissuades the scientist from an active interest in doing research as a means towards financial success. In general, it serves to encourage 'science for science's sake', and makes research and discovery an end in itself. However, Merton stresses that disinterestedness is not to be equated with altruism nor interested action with selfishness. The reason for the rarity of fraud in the annals of science is because scientific results are supposed to be verifiable and because such research is carried out under the eye of a supervisor or the scrutiny of colleagues. It should, nonetheless, be noted that no one knows...

* There are nevertheless several well-known cases of fraud in the annals of science, some of which have achieved notoriety. The 'Piltdown Man' fraud is a classic, and recently the veracity of Sir Cyril Burt has been called seriously into question. Two cases of fraud in chemistry are quite well-known, both of which involve fabrication of experimental data. Without delving into the details, one case involved a student of Sir Robert Robinson, who whilst a postgraduate student, falsified her organic chemistry results. She received her PhD, got married and ceased to be involved in chemistry. Subsequent workers tried unsuccessfully to repeat her syntheses and when asked for further details she admitted the falsification. The other case involved a student of Ingold at University College who produced some experimental results which were in line with the theory accepted at the time. His work was repeated in R.P. Bell's laboratory and the results were found to be the opposite of those predicted. He was widely believed to have fabricated the results. He left the country and the matter was hushed up.

The subject of fraud has recently been discussed in the New Scientist. Commenting on a spectacular example of 'fudged' experimental data, Muller observes, "...A few dry letters to scientific journals over the past three months retracting a series of ten research papers are the only formal response to the revelation of that rare event, a full-blooded scientific fraud." The work of Dr Robert Gullis which apparently developed the 'Lands Cycle' biochemical reaction and was received into the scientific community as an important contribution, was admitted by the author to be fraudulent in a letter to 'Nature', after he and several other scientists tried and failed to reproduce the results. He left the country and the matter was hushed up.

One conclusion that is possible to draw from these cases, and what must be a consolation for the scientific community, is that although scientists occasionally cheat, if their work is at all important, any fraud will soon be detected.

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how many cases of undiscovered fraud have occurred. They are certainly rare in the mainstream of science where results are checked and rechecked, but may be commoner than is realised in the less important tributaries.

ORGANISED SCEPTICISM *

The norm of organised scepticism holds that each scientist is individually responsible for making sure that previous research by others on which he bases his work is valid. He cannot be excused if he has accepted a false idea and then pleads innocence. The scientist should suspend judgement until 'the facts are at hand'. This norm also obliges a scientist to be critical of the work of others and to make his criticisms known. No claimed contribution to knowledge should be accepted without careful scrutiny, and the scientist should doubt his own findings as well as those of others.

2.2.2. B. BARBER

Beyond these four, Barber has added two more norms:

RATIONALITY

The norm of rationality demands a faith in the moral virtue of reason. The universe should be understood in as abstract and general a fashion as possible. Scientists should rely on empirical

* This norm stands in direct contrast to the medieval norm where proof consisted of showing that something derived from the works of the Greek masters. The Greeks forbade dissection and the second century anatomist, Galen, derived his theories of human anatomy from that of the pig, the ape, the ox and the dog. Man's liver was supposed to be many-lobed like the hog, his breastbone segmented as in the ape, his hip-bones flared as in the ox, and the uterus horned as in the dog. When Vesalius showed, in the 16th century, that Galen's description of the hip bones was wrong, the defenders of Galen, (who was, like Aristotle, an authority who was instantly accepted) claimed that man had changed his shape from wearing tight trousers. Looked at in present day terms, however, the number of papers criticising the work of others in science is very small, especially papers devoted solely to critical assessment. In general, work is criticised by being ignored.
tests rather than tradition, and adopt a critical approach to all empirical phenomena rather than accepting certain phenomena as exempt from scrutiny. This norm could perhaps be seen as a blend of organised scepticism and universalism and is essentially a product of 18th century rationalist philosophy. It is a norm in scientist's public, though not necessarily their private lives.

EMOTIONAL NEUTRALITY

The norm of emotional neutrality demands that the scientist avoid becoming emotionally involved with his theories to the extent that he cannot adopt a new approach or reject an old answer when his findings suggest this is necessary, or that he distort his findings in order to support a particular hypothesis. Again this norm of Barber can in turn be seen as arising out of universalism and organised scepticism.

2.2.3 N.W. STORER

Storer has schematised the norms as outlined by Merton and Barber in tabular form:

<table>
<thead>
<tr>
<th>Focus of Norm</th>
<th>The body of scientific knowledge</th>
<th>Interaction among scientists</th>
<th>The scientists Psychological State</th>
</tr>
</thead>
<tbody>
<tr>
<td>Orientation</td>
<td>Objectivity</td>
<td>Organised Scepticism</td>
<td>Emotional Neutrality</td>
</tr>
<tr>
<td>Action</td>
<td>Generalisation</td>
<td>Communism</td>
<td>Disinterestedness</td>
</tr>
</tbody>
</table>

Merton and Barber consider this combination of norms is ideally suited to ensure the optimal progress of science. Merton sees his norms as "... a distinctive pattern of institutional control of a wide range of motives which characterise the behaviour of scientists." However,
real-life scientists often do not obey these norms. Reputable scientists keep some of their findings secret, use knowledge for private or corporate financial gain, hang on grimly to pet theories, avoid criticising their colleagues, rely overmuch on the results of eminent men in their field and perhaps even do a little cosmetic work on their own results to give a tidier and more aesthetic outlook. Merton replies by stressing that one must not confuse "institutional and motivational levels of analysis" but suggests no relationship between them.

One can criticise Merton here on two counts: firstly, Mertonian norms could emerge amongst those engaged in almost any context of individual creative activity, and where the product was not necessarily scientific knowledge. It therefore seems inescapable that some definition of scientific knowledge has to be present among the normative commitments of scientists. Secondly, on Merton's account, there is no criteria of criticism which enables the 'open minded' person to distinguish scientific knowledge from non-science - an area of discussion which Merton prefers to leave to philosophers of science.

I have only given here a brief review of one area of Merton's work. In addition to his conceptualisation of the normative structure of science, Merton has also examined other problem areas in sociology of science, for example the process of evaluation, the reward system, priority disputes and the ambivalence of scientists. These topics will be discussed as they become relevant in the analysis stages of the thesis.

In an attempt to go beyond Mertonian functionalism, Hagstrom and Storer have used Homan's 'exchange theory' for examining the social relationships among scientists, who in pursuit of the institutional goal of the advancement of knowledge exist in a situation where they exchange information for recognition.
2.3 COMPETENT RESPONSE AND THE REWARD SYSTEM IN SCIENCE

2.3.1 N.W. STORER

Storer states that scientists support the norms of science because they are in some sense aware that the norms are necessary if the exchange system of science is to operate properly. His argument is that one cannot look to the functions of the norms of science in relation to the goal of science for an explanation of why scientists take them to be morally 'right and good'. The 'functional basis' argument seems inevitably to suggest a close correlation between personal commitment to the 'advancement of knowledge in all science' and personal commitment to the norms of science. Storer argues that an alternative explanation is that the norms are important for the continued allocation of recognition - i.e. competent response, the circulation of which forms the basis of the social system. Therefore the norms are important to the scientist because they concern something in which scientists have an immediate stake, not because they are beneficial to science as a whole.

2.3.2 W.O. HAGSTROM

Hagstrom provides a comprehensive account of the exchange system which he sees operating as the central feature of the social system of science. He maintains that the individualistic Mertonian viewpoint is incomplete: it does not account for the fact that scientists seek to publish their accomplishments and are disturbed if proper recognition is not forthcoming. If scientists receive their major satisfaction from problem-solving alone, then the mere fact that others have already solved the problem should not deter their attempts. This scarcely ever happens, because scientists desire 'social' recognition and Hagstrom argues that the theory is also directly controverted by obvious facts about the scientific community.

1 The autonomy of the scientific community cannot be taken for granted; it must be maintained by internal social controls.
Communities of autonomous specialists tend to be rigid: they incorporate new goals and standards with difficulty - ie: scientific training commits scientists to certain techniques and theories.

Commitment to norms tends to erode in the absence of reinforcement, which are important because they make possible communication among scientists.

Hagstrom's central theory is that work (often called contributions) is submitted to the scientific community in the form of 'gifts', a phenomenon to be found in all institutions concerned with the maintenance and transmission of common values. The acceptance of the gift generates reciprocal rights, recognition of status and allocation of prestige. In this view, the organisation of science consists of an exchange of social recognition for information. However, as in all gift-giving, the expectation of reciprocation cannot be acknowledged publicly as the motive for making the gift, for the exchange would be replaced by contractual exchange.

Not only does the desire for recognition induce the scientist to communicate his results: it also influences his selection of problems and methods in order to achieve greater recognition, and instils conformity with methodological standards so that his contribution is more readily acceptable. Sanctions to enforce conformity in this respect are of two general kinds. Firstly, work which deviates too far from the norm will be refused publication, and consequently there will be no ensuing recognition. Secondly, although not primarily important, there is the withholding of extrinsic rewards - ie: position and money.
Hagstrom answers the question as to why gift-giving, which is frequently an insufficient and irrational form of control, persists in science by suggesting that the scientist, in giving, retains an interest in the work. He is not alienated from it and so exercises a form of moral control over its use as it is still, in a sense, his property. It binds the donor and recipient in a community of values. Institutionalised recognition takes place through the various channels of communication, and most importantly, by publication. Status is established when research contributions are accepted by reputable journals, and prestige is conferred when work is cited and emulated by others. What Hagstrom terms elementary recognition through interpersonal approval and esteem takes a wide variety of forms in science. Meetings of scientific societies have their institutional function, but also serve to transmit elementary recognition. This recognition allows for the quick dissemination of ideas, as do preprints and letters, and also for the reinforcement of motivation and values. While institutionalised forms of recognition are important in maintaining conformity to higher scientific norms, the elementary forms mediate between the larger scientific community and the individual scientist.

Hagstrom also considers the various ways in which productive scientists contribute information to their colleagues, and he offers a typology of scientific personality 'profiles'. To begin with six different information channels are identified:

* Ben-David has commented that under the system as put forward by Hagstrom it is nonsensical to talk in terms of 'gifts' - by Hagstrom's own admission, nothing is being given, at most a contribution, a sharing of knowledge is being made.
Publishes articles, books and papers. This is the most important channel, and those who do not use this cannot be considered scientists.

Contacts through meetings with scientists.

Informal contacts with others in the same speciality, through correspondence, visits, etc.

Informal contacts with departmental colleagues.

Contacts with former and current graduate students.

Contacts with different disciplines and non-scientists.

The set of persons with whom most communication is made will have the greatest influence on the scientist's own perspective. From this Hagstrom constructs a typology of scientists:

1 Highly involved leaders - Scientists who participate a good deal in all the communication channels within the scientific discipline.

2 Informal leaders - Scientists with many informal contacts, but few formal ones. They have a tendency to avoid the activities of scientific societies and also not to read the literature in their fields.

3 Scientific Statesmen - Scientists who have established reputations in their own disciplines, and devote much of their time to specialists in other fields and to non-scientists without personal jeopardy.

4 Student-orientated Leaders - These are men who relate to their discipline primarily through their students. They are felt to be a leader of a 'school' and their eminence will stem partly from the success of their students in advancing their distinctive points of view.

5 Student-orientated Scientists - For a less eminent man, a group of present and former students may be practically his only link with the scientific community.
6 Intradepartmentally-orientated Scientists -
Some, having strong needs for approval and esteem but few students, and lacking the prestige necessary to approach specialists in the wider community must rely on communication with departmental colleagues.

7 Productive isolates -
Highly productive scientists relatively isolated from informal contact with colleagues.

8 Non-productive isolates -
Scientists who are retired from scientific life, but who may turn their interests to the teaching of undergraduates.

9 Marginal Scientists -
The scientist who has made few formal communications to his own speciality will tend to become a participant in non-specialist groups.

The differences in the types of scientists and in the contexts in which they work permit wide variations in communication practices. To some extent the variation can be viewed as a form of differentiation that helps meet organisational and institutional requirements. Leaders are required who are willing to engage in organisational action even when it interferes with their own research. Similarly, it permits creative geniuses to be influential without burdening them with organisational activities. It requires devoted teachers, and it needs eminent men to represent it in the wider society.
Kornhauser in 1962 summarised the conflicts that he claimed arise between science, as a system of professional values, and industry, with its emphasis on economic and administrative values, and suggested four main areas of conflict.

1. Conflict over goals: scientists want fundamental research, while industry steers scientists into working on problems which will benefit the company.

2. Conflict over the work situation: scientists seek to control their projects in terms of 'how', 'where' and 'when', whereas industry prefers to control time schedules and priorities.

3. Conflicts over incentives: scientists prefer rewards related to their professional needs, but industry is geared to organisational rewards - i.e. promotion.

4. Conflict over responsibility: scientists feel an ethical responsibility for their knowledge, while industrial managers regard it as their domain with decisions being determined mainly by commercial considerations.

Kornhauser summarises this position as being a battle between 'structural autonomy' and 'functional integration'.

The work of Stephen Cotgrove and Stephen Box is relevant here. They have investigated differences in the roles and identities of scientists who are engaged in different locations. They compare scientists working in academic circles, in industry, or with an organisational orientation, and they have studied the conflicts between science and industry.

Cotgrove and Box, using a Mertonian framework to guide their research, argue that in science as a social system-
a) scientists are strongly motivated to communicate with others in the scientific community, especially to other members of the numerous 'invisible colleges' of scientists concerned with a particular activity.

b) emphasis is placed on the imperative to publish.

Given this institutionalised goal of science, the norms as propounded by Merton necessarily follow. They construct a typology of scientists differentiated by role and identity.

**SCIENTIFIC ROLES AND TYPES OF SCIENTIST**

<table>
<thead>
<tr>
<th>Roles</th>
<th>Academic (Knowledge)</th>
<th>Professional (Application)</th>
<th>Non-Scientific</th>
</tr>
</thead>
<tbody>
<tr>
<td>Identities</td>
<td>Public</td>
<td>Private</td>
<td>Organisational</td>
</tr>
</tbody>
</table>

(Cotgrove and Box: 'Science, Industry and Society', p 24)

In this typology:

1. "Public" scientists are those who identify with the scientific community.
2. "Private" scientists are those for whom the attraction is the work itself and who do not seek recognition from other scientists.
3. "Instrumental" (organisational) scientists are those who intend to use their qualifications for occupational advancement and who are willing, for instrumental reasons, to abandon directly scientific work entirely. 22
These types can be systematically related to Merton's norms of science. Cotgrove and Box suggest that the norm of Communism is the most important in the social system of science, since it is central to the gift-exchange process and scientists who are orientated towards this can be classed as "public". Those scientists, while attaching importance to the norms of disinterestedness and organised scepticism, do not seek recognition, can be termed "private".

This leads to a typology of scientists:

<table>
<thead>
<tr>
<th>Type of Scientist</th>
<th>Importance attached to norm of science</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Communism</td>
</tr>
<tr>
<td>Public</td>
<td>+</td>
</tr>
<tr>
<td>Private</td>
<td>-</td>
</tr>
<tr>
<td>Organisational</td>
<td>-</td>
</tr>
</tbody>
</table>

(+ attaches importance)  (- attaches little importance)  (Cotgrove and Box op.cit. p 27)

Cotgrove and Box examine the view that the employment of scientists in industry generates widespread strains from dissonance between norms and values of science and industry and argue that this picture is too simple, and that scientists in industry adopt various strategies of role bargaining to maximise their autonomy.

It is impossible to consider implications of the functionalist / exchange theories and examine the more recent extensions, modifications and criticisms of the scientific ethos as principally propounded by Merton, without first reviewing other contributions which examine science and the scientific community from a different perspective, and which are directly in conflict with functionalist interpretations. Only when the two different perspectives have been discussed, is it useful to look at ways in which combinations of theories can be linked or superimposed to give a fuller, more realistic interpretation of science and the scientific community. An account of the work of Thomas Kuhn is relevant here.
2.5 NORMAL AND REVOLUTIONARY SCIENCE

2.5.1 T.S. KUHN

In direct contrast to the 'open' community of science previously described, Kuhn argues that it is quite possible to view scientific growth as a product of intellectual and social closure, where scientific activity for most of the time consists in the "attempt to force nature into conceptual boxes supplied by professional education".

This view emphasises that scientists develop a strong commitment to a particular theoretical-methodological tradition, and there are consequently powerful forces within science working to limit the possibility and acceptance of innovations. As Martins has pointed out one of the major weaknesses of the functionalist theories was that it contributed to the split between the sociology of knowledge and the sociology of science. Kuhn's 'The Structure of Scientific Revolutions' brought back into the arena a whole range of problems which sociologists themselves seemed determined to avoid and placed much more emphasis on the cognitive aspect of science. Martins comments, "...Part of the shock value of Kuhn's theory of science stems from his adoption of terms like consensus, authority, dogma, tradition, faith, conversion - a whole array of irrationalist symbols - in a serious, tightly argued theory of science and scientific knowledge." ²⁵

Kuhn's picture of science is that for most of the time scientists are operating in a period of 'normal science', carried out in a climate which is very closely circumscribed in conceptual, methodological, experimental and other ways. Research is not orientated primarily towards the pursuit and testing of fundamental aspects of theory, so much as to the production of expected solutions to prescribed problems according to standardised
procedures, and within what Kuhn terms 'dogma', or 'paradigm'. Mulkay succinctly defines the term - paradigms are the...

"...universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners, and include law, theory, application and instrumentation." 26

Under a particular prevailing paradigm, normal science is only concerned with articulating the paradigm theory, and revealing and matching to the theory data that the paradigm suggests. Under these conditions, cumulative growth, or 'rapid and consequential advance' occurs.

During periods of normal science, Kuhn argues that there is a hostility to alternative rival theories, and scientists are not able to give an objective evaluation of alternatives. Kuhn suggests that this may increase efficiency since science progresses rapidly when noone is fretting over fundamental ideas. Towards the end of this period, as the paradigm is exhaustively explored, various anomalies, or 'violations of expectations', occur and ad hoc modifications to the paradigm are made, although to begin with, anomalies are ignored. During the period of uncertainty, while anomalies are accumulating within the paradigm, scientists do not reject it until there is an alternative, and in the meantime various strategies are adopted to protect it. Occasionally, the anomalies instigate a major scientific crisis; the rules of normal science are loosened and a revolutionary situation arises, which is resolved by the replacement of the old paradigm by a new one. This involves an intellectual 'jump' to a new paradigm within which another cycle of normal scientific research occurs.

Kuhn stresses that paradigms are "psychologically exclusive, socially monopolistic, historically discrete, logically and epistemologically
incompatible, incommensurable and non-cumulative. Major scientific progress takes place through scientific revolutions, which destroy the truth content of the previous paradigms each time they occur."

There has been considerable discussion of the Kuhnian account of the growth of science and the process of scientific discovery, both with reference to other sociologists and also from its philosophical standpoint. An excellent source for this latter area of controversy is the publication 'Criticism and the Growth of Knowledge' which is the text of the Proceedings of the International Colloquium in the Philosophy of Science, 1965. 27

Two areas of philosophical difficulty have been: firstly, Kuhn's understanding (or misunderstanding) of the fundamental nature of science, in particular of the genesis of scientific revolutions. 28 Whereas Kuhn argues that for most of the time scientists are engaged in 'normal science', and in general are not constantly trying to refute current theories, Popper argues that science is constantly potentially on the verge of revolution, and that this situation exists within an 'actual' state of normal science, rather than in Kuhn's ideological interpretation of normal science, governed by rigid dogma. 29 Secondly, Kuhn has been taken to task for his cavalier usage of the term 'paradigm' which, although the central concept of his theory of scientific advance, is never adequately defined. Masterman has elucidated Kuhn's conception of a paradigm and shown Kuhn's multiple definition and use of the term. 30

Within Kuhn's framework for scientific advance, Mertonian norms can in many ways be considered to account for the activities of normal science; guided by the prevailing paradigm scientists contribute and receive recognition for further articulation of the paradigm. (However, the norm of organised scepticism is attenuated with reference to the paradigm itself).
It is important when considering the scientific ethos to distinguish between an interpretation which bears upon the activities of an individual scientist, and a view which is focusing on science as a discipline and a community. It has been pointed out by scientists themselves on innumerable occasions that there is a level of hypocrisy in internalising the norms of science and in later manifestly repudiating them through activity. Thus Kemp has commented that "...it is more profitable to interpret the Mertonian ethos of science as an ideology, not as a rule book for everyday guidance." 31

However, during Kuhn's periods of 'crisis' and 'revolution', Mertonian norms do not appear to operate effectively; a conflict discussed by Mulkay. 32
Mulkay and others argue that although the Mertonian tradition is widely adopted by sociologists it has a very tenuous empirical foundation. In fact, commitment to purely social norms is far outweighed by the commitment by the community of scientists to a specific body of knowledge and associated techniques, and these operate as the dominant source of normative controls. Mulkay has examined one particular case, the so-called Velikovsky affair, where marked deviation from Mertonian norms and radical theoretical innovation occurred together. He suggested the general premise that "deviation from established norms generates corrective responses within the group, and brings into the open normative commitments which might otherwise remain implicit as a means of bringing the deviance back into line." In 1950, Velikovsky published a book called 'Worlds in Collision' which challenged many of the central presumptions of astronomy, geology and historical biology, and developed a theory to account for various cosmological and terrestrial phenomena. His works became popular best sellers but his thesis was met with derision by eminent scientists because his ideas were not consistent with the currently accepted ideas in science at that point. His thesis was not evaluated by the criterion of scientific scepticism; he was hindered in his attempts to test his ideas empirically, and his papers were rejected without being read and his books were boycotted by other scientists.

Mulkay makes the following points to support his argument:

a) The prevailing scientific paradigms operate as norms, and Velikovsky's massive departure from these led to an emotional rebuttal of his propositions and a denial of his scientific integrity.

b) As a consequence, other scientists felt justified in judging the man instead of the work, and in this way failed to live up to the rules of universalism and organised scepticism, and did not examine the work before assessing its validity.
c) In addition, the norm of communism was violated because the scientific community restricted access to Velikovsky's work, and sanctioned persons who supported the work.

d) The functionalist approach predicted that Velikovsky's work should have been subjected to rigorous examination, and any emotional public reaction by the scientific community would have been restrained by reaffirmation of Mertonian norms. Instead, it is not the Mertonian norms which are affirmed, but the established theoretical/methodological paradigm.

e) Rigidity rather than fluidity typifies the intellectual commitment of the scientists concerned. Mulkay in fact suggests that some of the Mertonian norms exist in science more as 'institutional fictions' at the verbal rather than the behavioural level, and it is the Kuhnian model which offers a more realistic explanation of cultural change in science.
2.7 FURTHER DISSENTING ANALYSES

Many other sociologists have found difficulty in empirically endorsing the attachment of scientists to the Mertonian norms of science and have all found contrary evidence indicating a focus of activity at variance with Merton's thesis.

2.7.1 R.A. ROTHMAN

Rothman criticises the Mertonian framework and questions whether the scientific community has ever really adhered to the scientific ethos. He argues that the effectiveness of the norms of science which seem to receive widespread verbal support is neutralised by a complex of internal and external factors. One is the growth of what de Solla Price calls 'big science', its resultant dependence upon external money, frequently accompanied by more or less subtle pressures. Another factor may be the intrusion of the values of the broader 'middle-class' culture from which new scientists are typically drawn.

Rothman considers that the norm of Universalism is perverted by particularistic criteria and elitist trends, illustrated by the distribution of grant money, institutional affiliation and patterns of journal publication. Organised Scepticism is replaced by hostility to deviation from conventional methods of thinking, and frequent rejection of scientific contributions on other than technical criteria. The norm of Ethical Neutrality is modified in circumstances where nationalistic political ideologies dominate scientific activity, and a social policy ethos dictates the morality (and subsequent prohibition) of certain scientific research. The norm of Communalism is attenuated by the secrecy which scientists assume in their competition for priority and rewards. This competitive aspect similarly runs counter to the norm of Disinterestedness; the practice of science is becoming less for its own sake than for the advancement of scientists. Rothman asks, but does not answer, whether these violations of the scientific ethos represent a trend, or if there has always been this deviation.
2.7.2 R. WUNDERLICH

Wunderlich has endorsed Rothman's critique of contemporary science, but argues that the notion of 'disinterestedness' as a fundamental norm of science is widely misinterpreted. Wunderlich considers that an excessively individualistic psychological interpretation is usually made of a scientific motivation towards disinterestedness, whereas the norm should be regarded as residing within the scientific community.

2.7.3 I. MITROFF

Mitroff, using a Mertonian perspective, in a case study of moon scientists has postulated the existence of 'counter norms' and has provided evidence of scientists commitment to them.

Each norm, Mitroff argues, has a corresponding counter norm:

<table>
<thead>
<tr>
<th>NORMS</th>
<th>COUNTERNORMS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Faith in the virtue</td>
<td>Faith in both rationality and non rationality</td>
</tr>
<tr>
<td>Rationality</td>
<td>Emotional commitment</td>
</tr>
<tr>
<td>Emotional Neutrality</td>
<td>Particularism</td>
</tr>
<tr>
<td>Universalism</td>
<td>Solitariness</td>
</tr>
<tr>
<td>Communism</td>
<td>Interestedness</td>
</tr>
<tr>
<td>Disinterestedness</td>
<td>Organised Scepticism</td>
</tr>
<tr>
<td>Organised Scepticism</td>
<td>Organised Dogmatism</td>
</tr>
</tbody>
</table>

Mitroff argues rather tautologically that science contains both norms, and counternorms, not necessarily operating equally in every situation. He considers that the conventional norms are dominant for well-structured problems and his proposed counternorms are dominant for ill-structured problems.
2.8 AREAS OF IGNORANCE

I now want to consider another aspect of sociological interpretations of scientific activity. The functionalist/exchange perspective, whilst illuminating a range of behaviour, nevertheless regards science itself, either as theory or technology, as non-relevant to the analysis.

Although Kuhn gives very little account of how oligarchies surrender their power or change their evaluations, he does at least emphasise the need to concentrate on what scientists do as opposed to assuming a priori that they all do the same - ie: apply the scientific method.

Once we have allowed the possibility that scientists may differ in what they do then we can start to study relationships between what they do and what they produce and consider what is it is they define as scientific knowledge.

2.8.1 G. HOLTON

Moving on from Kuhn, Holton asserts that the greater proportion of scientific advance comes about from the spread of paradigms into areas of science which have yet to establish themselves and define their distinctive area of enquiry.

Science tends to proceed therefore by the discovery of new areas of ignorance - these not being associated in the minds of scientists with established paradigms. The emergence of a new field generates a rapid influx of researchers and consequently a rapid development of ideas; in other words, a bandwagon effect. Although this pattern of growth does not fit neatly into Kuhn's model, where development takes the form of intellectual stagnation within established areas of enquiry, the two combined provide a fairly satisfactory account of the processes whereby new ideas are accepted into science. Mulkay provides a good
description of the scheme:

"In certain fields which have clearly defined paradigms, growth will be facilitated by systematic training, by group closure and by intensive research into a narrow range of problems. Within such fields major innovations will meet with strong resistance from the majority of scientists who have become committed to the existing paradigm. At the same time, within all scientific disciplines there will be a strong tendency for ideas to escalate into new fields thereby facilitating the development of new knowledge without the accompaniment of vigorous resistance. This view centres the analysis around specific bodies of knowledge. It allows for the normative aspects of established paradigms, and it takes account of resistance to innovation while including the idea of rapid growth. In addition, it links up with Hagstrom's important work (1965, chapters 4 and 5) on the processes whereby scientific disciplines become structurally differentiated."
2.9 THE CROSS FERTILIZATION OF IDEAS AND ROLE HYBRIDS

The foregoing seems to describe a more realistic view of scientific development than the functionalist analysis, but neither Merton nor Kuhn provide an adequate analysis of the structural sources of innovation with respect to the generation of ideas. Holton's hypothesis of scientific progress by migration caused by discoveries of new areas of ignorance is a pointer to theories of the cross-fertilisation of ideas.

2.9.1 J. BEN-DAVID

Ben-David has examined the mechanisms and structural determinants of cross-fertilisation in modern science. He has shown that the rate of innovation is hindered by strongly centralised organisation and assisted by a loose and competitive structure. In addition to this condition of the general community of science, where intellectual innovation appears to be positively related to the conflict of ideas within an open social structure, Ben-David has also shown that scientists who combine roles in their activity - 'role hybrids' - are in an advantageous position for innovation:

"... analysis of the beginnings of bacteriology and psychoanalysis lends general support to the proposition that contact with practice may be important in reorientating research toward the investigation of new and fruitful problems. The practitioner-scientists appear as forerunners, supporters and disciples in the history of the two innovations bacteriology and psychoanalysis, where the central figures in both were 'role hybrids' who were led to the innovation by an abrupt change from theoretical research to applied science. The practitioner-scientist, as well as the role hybrid, is in a position to shift frames of reference relatively easily." 47

An example of particular relevance to this thesis is seen in the interplay between engineer and physicist. (A very high proportion of pioneering mass-spectrometrists had an engineering as opposed to a scientific background.) It was in 1883 that Edison, studying the blackening of his early lamps, inserted a metal plate into the bulb
to catch the emanations from the filament and noted that a positively charged electroscope connected to the plate was discharged, whereas a negatively charged instrument was not. The 'Edison effect' remained unused until Fleming, in 1904, looking for an improved form of radiofrequency rectifier to increase wireless telegraph signalling speeds, remembered Edison's observations of twenty-one years earlier. The Edison effect was comprehensible to the engineer/physicist Fleming in terms of J.J. Thomson's work on the electron, and so he patented his 'oscillator valve' as he termed it. This was followed three years later by de Forest's insertion of the grid electrode onto Fleming's diode for amplification and oscillation and so, through the triode, modern electronics was born.

Ben-David's findings indicate that a situation favouring the generation of innovation, and of the developments of new cognitive frameworks, may occur when a scientist embraces two (or possibly more) roles, especially when each role involves a distinctive approach to the research problem.

2.9.2 SIR ERIC EASTWOOD

On another level, Sir Eric Eastwood has indicated the existence of a different kind of cross-fertilisation. Progress in experimental physics stems from improved methods of making observations and measurements, and the history of physics is well sprinkled with accounts of the development of particular scientific instruments. Many such instruments are later recognised as having a contribution to make in fields remote from their origin. As we shall see, a typical example of this cross-fertilisation process is provided by the development of the mass-spectrograph which extends over many years. A modern instrument with built-in computer and on-line
gas-chromatograph is a far cry from the original experimental equipment used by Aston for his work on isotopes but the basic principles of operation remain unchanged. It is remarkable how instruments of this complexity have come into widespread use not only as research tools in chemical laboratories, but as control instruments for on-line use in chemical production processes. What was once a physicist's curiosity spearheaded the revolutionary change in chemical analysis.
I would like now to examine some further concepts which have contributed to the sociology of science, before discussing some of the criticisms of the central arena occupied by Merton and Kuhn. In general, published papers give a misleading impression of how scientific advances come about. In particular, what we can term 'blind alleys' are very rarely described. Research is presented as a logical progression, while results may very well be the consequence of lucky chances, or serendipity.

2.10.1 B. BARBER

Barber comments that because of certain norms that are strongly institutionalised in their professional community, scientists are expected to focus their reports on the logical structure of the methods used and the ideas discovered in research in relation to the established conceptual framework of the relevant scientific field. All else that has occurred during the actual research process is considered 'incidental', and as a result of such norms and practices, the reporting of scientific research may be characterised by the occurrence of 'retrospective falsification'. By selecting only those components of the actual research process that serve their primary purpose, scientific papers leave out a great deal, as many scientists have indicated in memoirs and informal talks and interviews. This phenomenon creates problems for the sociology of science.

Barber comments that one component of the process of scientific discovery that is left out or concealed in research reports following the practice of 'retrospective falsification' is the element of unforeseen development, of happy or lucky chance, of what Merton has called the 'serendipity pattern'. So-called 'accidental' discoveries such as penicillin or X-rays abound
in science, and many sociologists have investigated occurrences when an important discovery has been made in scientific research which was the result of following up chance observations made in experiments concerned with a different area of research. Perhaps we can sympathise with the layman for sometimes wondering if scientific advance is not due to lucky chances where even the most mediocre and conservative scientist can grasp their significance. To the scientist himself, the picture is somewhat different. There are always too many pathways to follow, too many chance observations which in nine times out of ten cases will lead nowhere. There are always crossroads in research; "... should you boil, or freeze, filter or centrifuge?" When serendipitous results are published, the failures are not mentioned. Far from having a guardian angel to jog his shoulder at the right moment, the scientist sees himself as battling against a malign Nature who gives her secrets grudgingly if at all.
2.11 RESISTANCE TO SCIENTIFIC ADVANCE

2.11.1 B. BARBER

Barber has also commented on the resistance of scientists themselves to scientific discovery, as distinct from the resistance of technological, religious and ideological elements outside science itself. Barber also suggests that:

a) Substantive concepts and theories held by scientists at any given time become sources of resistance to new ideas (Kuhn's notion of paradigms).

b) The methodological and experimental concepts constitute a source of resistance.

c) The tendency of scientists to think in terms of physical models similarly hinders advance.

In relation to social sources of resistance, Barber notes that a key factor may well be the relative professional standing of the particular scientist involved in a discovery. In general, higher standing in science is achieved by the more competent, but sometimes, when scientists of lower standing make discoveries, they are hotly resisted by scientists of higher standing partly because of the authority their higher position provides.

* Indeed, it has often been the case that propositions have been rejected just because they could not be analogised in the form of some physical model. Lord Kelvin, when he found himself unable to translate into a dynamical model the abstract equations of Clerk Maxwell's electromagnetic theory of light, said, "...I never satisfy myself until I can make a model of the thing." (53) Thus models, while usually helpful in science, can also be a source of blindness.
One must not imagine that there are never times when a revolutionary discovery is not immediately seized upon by the scientific community. Sir Nevill Mott, for example, has commented upon the ease and speed with which the ideas of quantum mechanics were accepted in Cambridge:

"In Rutherford's laboratory with its tradition of string and sealing wax, the right experiment and the simple model, this (acceptance of quantum mechanics) needs some explanation. There was then a division of theorists, between those who sought for elegance and mathematical completeness, and those who liked to visualise what was happening, albeit in terms of waves rather than particles. I think a great deal of the credit for this speedy acceptance must go to George Gamov - his theory of the penetration of particles into and out of nuclei was so simple, so obviously useable, that the experimental people could only accept it with joy. I think useable is the key word. Is a theory a thing in itself, something which illuminates our view of the Universe, or is it something which suggests the right experiments and helps us to increase our power over nature? The quantum mechanics which Cambridge took to its heart I consider was of the latter kind." 54

Barber, however, also indicates other areas of resistance to scientific discovery. The pattern of specialisation that prevails in science at any given time, on the whole, is efficient for internal and environmental purposes. But occasionally, innovative 'outsiders' to a field of specialisation are resisted by 'insiders'.

Scientific organisations serve a variety of useful purposes for their members, and scientific publications are indispensable for communication in science, but when these operate to the detriment of the norms and values of the scientific community, they serve as another social source of resistance to innovation.
'Older resist the younger' is another pattern - 'ageing' covers a wide variety of cultural and social sources of resistance. The older scientist is more likely to be restricted in his response to innovation by his substantive and methodological preconception, and by his other cultural accumulations. He is more likely to have a high professional standing, to be a member of an established organisation, and possibly to be associated with a 'school'. The dignitaries who hold high honours for past accomplishments do not usually like to see the current of progress sweep events too rapidly out of their reach.
Another area of investigation for sociologists has been to examine the extent to which recognition varies for a particular contribution by scientists of unequal rank.

2.12.1 R.K. MERTON

It is Merton's contention that from the standpoint of science conceived as a system of communication, the same scientific discovery will have greater 'visibility' in the community of scientists when it is introduced by a scientist of high rank. In fact, eminent scientists get disproportionately greater credit for their contributions, and therefore opportunities for scientists at different levels of esteem are unequal, operating on the premise that "For unto everyone that hath shall be given". Merton shows that this general characteristic can be conveniently separated into two forms:

1. In cases of collaboration on papers.
2. Where independent multiple discoveries are made by scientists of distinctly different rank.

Confronted with the task of identifying significant work published, scientists use such clues as the repute of the author.

Merton states:

"Science is not composed of a series of private experiences of discovery that remain solipsistically confirmed to each of many scientists. Science is public, not private. It is not enough for fruitful ideas to be originated to advance science. That is what we mean by a contribution to science. For the development of science only the work that is effectively perceived and used by other scientists matters."
However, there is a difference between what ought to count as scientific knowledge, and what tends to be counted as such. A scientist does not publish his own results, and there can only be scientific knowledge of what a group of people can agree on. To this extent, all scientific work is and must be objective. But while the scientific speciality demands high standards of presentation of evidence and arguments for conclusions, there are, as Kuhn argues, all sorts of procedural assumptions of strategy and techniques which are brought into play and which assist in propelling the innovatory work into the limelight.

Merton postulates that independent but similar discoveries make it more probable that the particular discovery will be incorporated promptly into the current stock of scientific literature. It is also evident that great talents in science are typically involved in many multiple discoveries - for example, Lord Kelvin was involved in at least 32 multiple discoveries.

Charismatic personalities are of great importance in science, for they excite intellectual enthusiasm among others who ascribe exceptional qualities to them. Their function rests not only in their own excellent accomplishments, but also consists further in their capacity for evoking excellence in others. They socialise their associates in the norms that govern important research, and charisma becomes institutionalised into schools of thought and research establishments.

* A question raised here which Merton does not consider is whether charisma is an intrinsic property of some scientists, or a characteristic ascribed to others by, for example, their pupils or colleagues. "Some are born great, some achieve greatness, and some have greatness thrust upon them." Perhaps the latter is more common.
individual personality and an acquired set of high standards often leads outstanding scientists to discriminate more rigorously between work which is worth publishing and that not worthy. This is a situation which reinforces the expectations of their fellow scientists that what these eminent scientists publish is worth close attention. In other words there develops a self-fulfilling prophecy.

This becomes harmful under certain conditions, for although eminent scientists have a greater probability of making significant contributions, they are not alone in making them. Scientists do not begin by being eminent, and the history of science is littered with cases in which important fundamental papers have been written by comparative unknowns only to be neglected for years. Merton offers the caveat that, "When the Matthew effect is thus transformed into an idol of authority, it violates the norm of Universalism and curbs the advancement of knowledge." At the same time he holds the contradictory view in the analysis that although the Matthew effect may seem to be unjust to some individuals, it is in fact efficient for the system.

2.12.2 J. COLE and S. COLE

Related to this is Jonathan and Stephen Cole's work on citation analysis which suggest that only a few scientists contribute to scientific progress. They offer an analysis diametrically opposed to the views of Jose Ortega y Gasset. The 'Ortega hypothesis' states that much of the growth of science has been accomplished by 'small, mediocre' scientists who have paved the way for the men of genius. This 'brick by little brick' view of science is widespread. For instance, Lord Florey, a recent President of the Royal Society expressed this point of view: "... Science is rarely advanced by what is known as a 'breakthrough' - rather
does it depend on the activity of thousands of our colleagues who add small points to what will eventually become a splendid picture." This view assumes that the ideas of an average scientist are both visible and used by outstanding scientists, and that this minor work is in fact necessary for the production of major contributions.

In a study of the citation practices of academic physicists, Cole and Cole put forward the counter-hypothesis that work used by the producers of outstanding research is itself produced by a small minority of scientists. They look at the relationship between the number of scientists and the rate of advance in science, and question whether it is possible that the number of scientists could be reduced without affecting the rate of advance.

Cole and Cole suggest that we should be concerned primarily with the teachers of the future members of the elite, and say that: "... it might be facetiously asserted that the best way to win a Nobel prize is to study with a past laureate" - a view held not only by Cole and Cole, but one consistently put forward by scientists, and discussed at some length by Holt, and by Hans Krebs. 62

2.12.3 H.KREBS

Krebs examines the pattern of genealogic influence in chemistry, and argues that scientists are not so much born as made by those who teach them research. Krebs uses the distinction of the Nobel prize as a criterion for excellence and demonstrates, using two genealogical 'trees', that distinction develops if nurtured by distinction. In the first 'tree' showing the distinguished
ancestors of Krebs, himself a Nobel prize winner, he points out that in every case the association between teacher and pupil was prolonged, extending to the mature stage of the pupil.

- Berthollet (1748 - 1822)
- Gay-Lussac (1778 - 1850)
- Liebig (1803 - 1873)
- Kekule (1829 - 1896)
- Von Baeyer (1835 - 1917)
- E. Fischer (1852 - 1919)
- Warburg (1883 born)
- Krebs (1900 born)

The argument is borne out by a consideration of a more extended family tree of the seventeen Nobel Laureates descended from Von Baeyer.

Diagram:

```
  Von Baeyer
     /  \
    /    \\  \
  Willstatter  Wieland  Buchner  Fischer
     |       |      |       |
  Kuhn      Lynen       \
     /  \
    /    \\  \
  Diels    Warburg  Windaus
     |      |      |
  Alder    \
     /  \
    /    \\  \
  Meyerhof Krebs  Theorell
     |       |
  Lipmann  Ochoa
```
Krebs considers that although techniques can be learned from many teachers, what is of paramount importance is the attitudes conveyed by the distinguished teacher to the pupil.

In this section I have attempted to review modifications to the functionalist/exchange perspective which have shed light on communication patterns and genealogical influences, and have pointed to the existence of inequalities between scientists in their pursuit of original research. This area has also been analysed from a Kuhnian perspective.
2.13 INVISIBLE COLLEGES AND COMMUNICATION PATTERNS

As a meeting of like minds without a material building, the precursor of the Royal College called itself 'The Invisible College', and this title has been given by de Solla Price to the typical sub-community of the modern scientific world which finds its focus on the innumerable areas of research that are current. The experts on a particular subject constitute an 'invisible college'.

2.13.1 J. ZIMAN

As John Ziman vividly puts it: "The world is traversed daily by the letters they write to one another; each morning they avidly scan their specialist journals for citations of their 'contribution' to this new and significant science; every three months they hasten to Nairobi, Lima or Astrakhan to confer, confute and controvert, duly observing the norms of the scientific community."

2.13.2 D. CRANE

The phenomenon of the 'invisible college' has been examined by Diana Crane who reviews various points of view on the relationship between the content of science and the internal organisation of the scientific community, and shows how significant such groups are, not only for the individual scientist but also for the growth of science generally. Crane's central theme is that the characteristic pattern of growth in each area of science is closely linked to the pattern of social interaction between scientists. She measures 'growth' by numbers of published papers in several areas, and distinguishes four stages, each with its own typical social structure. Her model draws on Thomas Kuhn's theory of the structure of scientific revolutions, and concept of paradigms.
In stage one, the paradigm appears in one or two innovatory papers, but there is little or no social organisation. In stage two, there is rapid exponential growth, representing normal science performed by groups of collaborators organised in a recognizable invisible college. In stage three, growth slows down and there is increasing specialisation, and stage four typifies saturation, exhaustion, intellectual crisis and the decline or disintegration of the college.

2.13.3 N. MULLINS

Crane argues that groups develop around expanding literatures, but Mullins asserts that specific social developments precede the literature, and that theoretical changes occur within the boundaries of groups defined by coherent communication patterns, which he describes as "thickenings" in the normal open pattern of scientific communication. Briefly summarised, Mullins' theory holds that a group which creates theoretical breaks in a discipline passes through four stages: normal, network, cluster and speciality or discipline. Mullins has examined the growth and development of a group of social science theorists - ethnomethodologists - and points out that, as his theory predicts, the group possesses recognisable characteristics at specific points in its development, and his data indicate that theory development is preceded in time by social development.
I would like to turn to Mulkay's investigations into the development of research networks. As previously discussed in this thesis, Mulkay has compared Merton's work with that of Kuhn and described their theories of scientific development as the 'model of openness' and the 'model of closure' respectively. Building on the work of Holton and Ben-David, Mulkay has put forward a third model - the 'model of branching'. There is a different kind of discovery not included in Kuhn's analysis when unexpected results occur which do not necessitate upheaval of existing scientific assumptions. Such discoveries reveal 'new areas of ignorance' to be explored by means of the extension and modification of existing conceptual and technical apparatus. Mulkay states:

"A central assumption of the model of branching is that in science new problem areas are regularly created and associated social networks formed. It is also assumed that the evolution of any one network depends considerably on developments in neighbouring fields .... Thus the exploration of a new area is usually set in motion by a process of scientific migration. Scientific migrants tend to come from research networks with definite characteristics; networks in which there has been a pronounced decline in the significance of results; networks whose members have few or no avenues of research easily available; networks whose members have a special competence in knowledge or techniques which have given some indication of being more widely applicable; and networks which have been disrupted, often by events originating outside the research community, and whose members consequently have no firm commitment to an established problem area." 70

Mulkay argues that the emergence of a new scientific network is brought about by intellectual migration. Elsewhere he has examined 71,72 possible factors involved which may prompt scientists to leave one area of activity in favour of a new one,
and Mulkay considers these factors as indicating a process of intellectual non-conformity.

This non-conformity is stimulated firstly when within a particular field, problems seen by the scientist decline in significance, and secondly, for scientists towards the summit of the status/prestige hierarchy, there is a diminishing return of recognition and esteem for contributions made to the corpus of knowledge within that field. In preference to resisting scientific revolution within their own field, many scientists will migrate to fields where they can bring their eminence and capabilities to bear on the construction of a new problem area.

Mulkay has documented the emergence and growth by branching of radio astronomy, where unexpected, but not anomalous observations led to the formulation of questions which defined a field previously unknown to those involved. But the researches undertaken to solve these problems themselves generated new areas of investigation. Mulkay states:

"It is possible that this cumulative burgeoning of scientific activity has been concentrated within a fairly stable number of research areas. But it seems much more likely that the dramatic increase in the size of the research community has been achieved by the continual creation of new fields of enquiry." 74

The development of these new avenues of investigation occurs through the formation of new research networks, and Mulkay has identified three main stages in the growth of a research area, where there is a highly complex web of social relationships associated with the creation of scientific knowledge. Mulkay names his three stages:

Exploration / Unification / Decline or Displacement

where each stage is characterised by certain intellectual and
social developments. At the beginning of stage one, the network has a minimal social organisation, and the scientists are unlikely to have a clear conception of the new research area. This phase of exploration is one where preliminary attempts at investigation are undertaken. "Usually at this stage the financial resources are small. For this reason, and because exploratory research often depends upon specially designed apparatus, this is a period when experimental ingenuity is most important."

(Mulkay, Gilbert and Woolgar, op cit)
Another feature of this period is the particular content of the early published papers. These are often statements concerning theory and methods which the author considers appropriate in the new field, not only for his personal work, but also for others working on his problem. The next flurry of papers results in a definition of a common area of interest via a process of 'negotiation'.

It is in stage two that the growth of research areas becomes 'exponential' (Price has shown how the pattern of bibliographic references indicates the nature of the research front.) As Mulkay points out, however, this growth in size is rarely matched by an equivalent increase in innovations or significant findings. This subsequent work consists primarily of exploration and elaboration of these central contributions. By the end of this stage the following characteristics should be noticeable: a firmly established intellectual framework, cognitive and technical standards, institutionalised mechanisms of recruitment and funding, well-established research teams, highly productive and influential scientific leaders, declining opportunities to contribute significant findings, and declining opportunities for attaining professional recognition and advancement.

The third stage, depending on the particular research area, can take a number of variations. Akin to Kuhn's description of the latter stages of a period of normal science, this period is marked by trivial research, anomalous discoveries leading to cognitive uncertainty and marked specialisation. This culminates either in the formation of opposing schools and a rash of disputes or the field is seen as one which is nearing 'death'. A scientist's response is either to abandon the field and move into a neighbouring area, or to attempt a redefinition of aims, problems and methods, in other words along the lines of a
Kuhnian revolution. However, research areas which have become well established often take a long time to die out and a scientifically active rearguard is likely to remain with a strong conservative commitment to the original paradigm.

Mulkay's model of branching is not incompatible with the 'closure' model; indeed there are many areas of common ground, for both models focus on the connections between cognitive and social differentiation in the research community, and both stress the existence of pressures towards intellectual conformity.

2.14.2 J. PARKER

Parker has referred to ambiguities in Mulkay's model of branching and has suggested that Mulkay has mis-interpreted Kuhn in emphasising the dynamics of status acquisition rather than cognitive criteria in his extension of Kuhn's account of the relation between normal and revolutionary science. Mulkay has responded by pointing out that he regards the Kuhnian sequence as atypical, in that cognitive revolutions do not occur frequently.

2.14.3 C. DEAN

Dean, too, has argued that Mulkay is in error in asserting their serendipitous discoveries lie outside Kuhn's analysis of normal science, and Dean suggest that the difference between Kuhn's account and Mulkay's alternative model of branching lies in the fact that Kuhn concentrates on cultural innovation while Mulkay centres on social processes.
2.15 CASE STUDIES OF SCIENTIFIC SPECIALITIES

There is now a considerable amount of case study work which has investigated the cognitive and social frameworks of scientific specialities. Several of these studies have been mentioned in this chapter: Mitroff has investigated the community of scientists working on the Apollo Moon Project, Mulkay has examined the proliferation of problem areas in Radio Astronomy, Mullins has studied the development of ethnomethodology as a distinct sub-field in social science, and Dean has used a study of the discovery and subsequent exploration of the phenomenon of pulsars to investigate the nature of serendipitous discoveries.

I briefly want to review three more studies of subdisciplines within science so that my examination of the growth and development of Mass-Spectrometry can not only be interpreted using existing sociological concepts, but can be compared and contrasted to the emergence, growth and establishment of other scientific specialities.

2.15.1 J. LAW

In a recent study of the development of X-ray protein crystallography, John Law emphasises that important cognitive and social differentiation exists between scientific specialities. He distinguishes three types: firstly, a technique- or methods-based speciality, where the solidarity of the participating scientists is based upon shared scientific apparatus; secondly, theory-based specialities which are based upon a shared formalism; and thirdly, subject-matter specialities, where a wide variety of techniques and theories may be brought to bear on a particular subject problem. In Law's view, theory and methods-based specialities constitute communities where the basis of solidarity is mechanical, and in Mullins' scheme represent specialities at either 'cluster' or 'speciality' stage, but not earlier. Subject matter specialities on the other hand arise on a basis of organic solidarity, and depend on the identification of a shared problem, and hence correspond most closely to Mullins' 'network' stage of speciality development.
2.15.2 J. GASTON

Gaston has studied the British high energy physics community and highlighted a pervasive dilemma in science. Scientists are trained and motivated to do original research, but those at the frontiers of knowledge inevitably find themselves in direct competition with other scientists: the decision then arises whether to work cooperatively or to work in secret, thus maximising the chances for recognition of an original discovery. This problem is exemplified by high energy physics, which has a large number of researchers, enormous funds and a rapid rate of discovery.

2.15.3 G. N. GILBERT

Gilbert, in a study of the emergence of radar meteor research, has illuminated the relationship between the social institution and the scientific knowledge produced, and demonstrated that an important determinant of growth depends on an interaction between social and intellectual factors. He suggests that scientists strive to find and solve problems whose solutions are significant for future work, and that during such circumstances, their socialisation within their scientific sub-community is optimised.
Clearly there are many other topics of sociological enquiry pertinent to the interpretation of science as a cultural, creative and developing entity, but these are peripheral to my area of research. In addition, I have not entered into much analysis of those theories and concepts that do not appear in this review chapter, for I consider it to be premature at this stage to examine difficulties and implications of interpretation without recourse to data derived from this present study of the development of Mass Spectrometry.

What follows is a detailed picture of this development which will then be examined in the light of the sociological theories outlined in this chapter, in order to see to what extent the detailed picture fits theories of scientific advance. I shall then consider what modifications of existing sociological theory might be necessary if the development of this particular scientific community is to be explained in realistic terms.
CHAPTER II

REVIEW OF CURRENT SOCIOLOGICAL THEORIES OF SCIENCE

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Chapter III is concerned with providing the current scientific contexts prevailing at the end of the nineteenth century when the invention of mass-spectrometric techniques took place. It explores contemporary frontiers in chemistry and physics and discusses possible reasons, both philosophical and technological, why certain topics in science at that time were far more advanced than others.

Chapter IV covers the period from approximately 1910 - 1925 and discusses the early experiments and instrument design of mass-spectrometry as carried out by J.J. Thomson, F.W. Aston and A.J. Dempster. It explores the modifications to the atomic theory which came about following mass-spectrometric experimental evidence, and discusses the growth and development of the theory of isotopic constitution of atoms, graphically demonstrated by the early experiments of Aston.
CHAPTER III

SCIENCE AT THE END OF THE 19TH CENTURY

3.1 INTRODUCTION

In order to examine the developments of science in the twentieth century in any meaningful way, it is necessary to establish how the prevailing ideas and theories came into being, and to understand how the scientific community operated. To set the scene for J. J. Thomson's invention of the mass-spectrograph, I shall first describe briefly the state of the relevant sections of science at the end of the nineteenth century. These are chemistry and physics, with the overlapping area of atomicity and atomic structure which is of particular importance.

By the end of the nineteenth century, physics was a mature science. Over 200 years had elapsed since Newton's "Principia" and his contributions to pure mathematics, optics, astronomical theory and dynamics were as firmly established as the motions of the planets which he had done so much to explain.

Many physicists regarded their discipline as being more or less complete: in fact one eminent physicist making an address in 1893 declared that it was probable that all the great discoveries in the field of physics had been made. He sketched the history and development of this science, finally summarising the well-knit, and as he thought, all-sufficient theories of the nineteenth century. The physicist of the future, he said, would have nothing to do but repeat the experiments of the past. The field of chemistry, however, did not share this complacency.

3.2 DALTON AND EARLY CHEMICAL THEORY

A convenient point to begin is with the publication of John Dalton's "New System of Chemical Philosophy" in 1808. A forerunner of Dalton, Joseph Proust, had established the principle now known as the "Law of
Definite Proportions", showing that any chemical compound always combined in exactly the same proportions by weight.

Dalton repeated Proust's experiments and enunciated a second law which Proust had not stated, but for which he had laid the ground work. This was the "Law of Multiple Proportions". Certain chemical elements united with each other to form a variety of compounds. This second law states that when this is the case, the different amounts of one element, by weight, which will unite with a given weight of another element, will be exact multiples of each other.

* This law proved to be the centre of a bitter controversy at the beginning of the nineteenth century between Proust and the French chemist Berthollet. The latter believed that the composition of a compound was variable and produced various arguments based on experimental evidence, concerning lead oxides and mercury salts. In every case Proust was able to counter the objectives raised by Berthollet, whose one aim at this time seems to have been to discredit Proust's law.

The development of accurate balances was a pre-requisite for progress in accurate determinations of weights in chemistry, recognised particularly by Black, a Scottish scientist who was a pioneer in the regular use of the chemical balance for checking his ideas, and gradually scientists learned the importance of definite quantitative results, instead of the vague qualitative statements with which they had been satisfied hitherto.

Also, an important factor was the increasing availability of pure simple substances - a Greek philosopher contemplating grass or trees would not in any circumstances have formulated the laws of chemical combination which really become evident only in the bizarre and noxious substances so carefully isolated by the much abused alchemists during the Middle Ages.

(The existence of non-stoichiometric compounds is, of course, well-established nowadays and is the basis of semi-conductor technology, urea de-waxing, etc.)
From these considerations, Dalton formulated his atomic theory, the essential points of which were that:

a) Matter consists of small indivisible particles called atoms.
b) Atoms are indestructible and cannot be created.
c) The atoms of a particular element are identical and are different from atoms of all other elements.
d) Chemical combination takes place between small whole numbers of atoms.

A problem which Dalton studied with great care, was to find the relative weights of the atoms of different elements. If all compounds contained only one atom per molecule of each of the elementary constituents, this problem would be an easy one, but the very existence of the law of multiple proportions shows that this is not always the case, and when, for example, two elements combine together to form several compounds, it is not obvious, at first sight which, if any, of these has a molecule consisting of only one atom of each element.

In fact, the problem of determining the relative weights of atoms exercised chemists for nearly sixty years after the foundation of Dalton's theory and in some cases the uncertainty lasted even longer.

Controversies over this question occupied a great deal of time and energy of chemists in the first half of the nineteenth century. Take, for example, the apparent paradox that when one volume of Hydrogen reacted with one volume of Chlorine not one, but two volumes of Hydrogen Chloride were formed. This proved an absurdity when attempts were made to explain the occurrence using the Atomic theory, without the knowledge that both Hydrogen and Chlorine, as elements, exist in the
free state as molecules, \( \text{H}_2 \) and \( \text{Cl}_2 \), not, as thought, as separate single atoms.

The evidence available at the time afforded nothing better than a balance of probability between the various views, and some chemists were in favour of abandoning the search for atomic weights altogether, and of considering only the equivalent weight, or combining proportions which were a matter of direct experiment.

Fortunately for chemistry (for it would have been a calamity if the atomic conception had been abandoned in favour of a colourless statement about combining weights), substantial agreement about atomic weights was eventually reached as a result of the application of a principle originally enunciated by Avogadro shortly after Dalton's work (1811). The importance of this idea was overlooked for nearly fifty years, by some curious blindness, and it lay unnoticed until resurrected by Cannizzaro in 1858: the principle being that equal volumes of all gases at the same temperature and pressure contain the same number of molecules. The importance of this hypothesis is the fact that it enables the enormous step to be taken from volume measurements to actual molecules, probing right to the heart of chemical reactions.

During this period of chaos in chemistry from about 1820 to 1860 it must be remembered chemists were not attempting to obtain the weights of individual atoms, but rather, as stated before, comparing the weights of atoms of different elements.
Dalton took as his standard the weight of the Hydrogen atom and for some time chemists followed his lead. The chemists had the fairly simple task of finding out how many grams of a given element combined with one gram of Hydrogen (its equivalent) and the separate task of finding out how many atoms of hydrogen will combine with one atom of the element (its valency). It was in this second requirement that the chemists found their difficulty - they could not determine the valencies of elements.

In fact, chemists did not fully realise the meaning of valency and the need for knowing it, until it was specifically defined by Frankland in 1852, half a century after Dalton's theory was suggested.

Without clearly formulating the idea of valency Dalton did, of necessity, make certain assumptions in which the idea was involved. This assumption was that if only one compound of two elements A and B existed it would be the simplest compound - i.e.: one atom of A + one atom of B gave the compound AB. This gave Nature credit for making chemical combinations as simple as possible - however, the credit was often undeserved and the assumption wrong.

* It so happened that the atomic weights of many elements determined on this standard were very nearly whole numbers: the number of such elements was very much greater than that indicated by probability and so Ebenezer Prout assumed that they ought to be whole numbers. He put forward his famous hypothesis (5) (1815) that the atomic weights of all elements are multiples of the atomic weight of Hydrogen. If this hypothesis was correct, it was considered that it ought to be possible to prove it by experiment, but attempts failed. The elements Copper and Chlorine, with atomic weights of 63.57 and 35.46 respectively, proved particularly recalcitrant in this respect, and the inability of Prout's theory to cover these cases proved its downfall.
One very important case where Dalton was wrong was that of water, where he assumed the simple structure HO instead of H\textsubscript{2}O.

The fact that an incorrect formula for so simple a compound as water was proposed, and accepted by the scientific world, shows the confusion in which chemists were floundering. It is important to realise that Dalton's assumptions were nothing but guesses — progress was made because in a certain number of cases, Dalton's guesses about valency were correct, or he made errors which cancelled each other. This meant that correctly related atomic weights were assigned to some elements, particularly the commoner metals. From this arose the first definite advance towards methods of determining valency, and it came indirectly from work undertaken with quite a different object in view — ie: investigation into the specific heats of metals.

The end of the eighteenth century had seen great activity in physics; in particular Lavoisier and Laplace had determined the specific heats of a number of metals with considerable accuracy.

In 1819 Dulong and Petit pointed out that if the accepted atomic weights for many of the metals were multiplied by their respective specific heats, a figure was obtained for each metal, of approximately 6.4. In some cases, the number obtained was very different from 6.4, and where this occurred, Dulong and Petit suggested that perhaps the accepted atomic weight was wrong.

3.3 EARLY ATOMIC THEORY

No expectation of actually seeing atoms or detecting the effect of a single atom was considered at this time. Sir William Thomson in 1883 had 'established' their extreme minuteness by four lines of reasoning —
"Founded respectively on the undulating theory of light, the phenomena of contact electricity and capillary attraction, and on the Kinetic theory of gases, agree in showing that atoms or molecules of ordinary matter must be something like 1/100,000,000th of a centimeter in diameter."

The leader of the opposition to the atomic theory was Wilhelm Ostwald of Berlin (1853-1932). Idolising the principle of conservation of energy, and regarding energy as the ultimate reality, Ostwald endeavoured to free science "from hypothetical conceptions which lead to no immediate experimentally verifiable conclusions". He abandoned the atomic and molecular theories - "those pernicious hypotheses placing hooks and points upon the atoms". He took up the more direct study of experimental facts and of the resulting graphic charts. The evidence for the atomic theory (mainly the laws of chemical combination) was neither direct nor compelling. Contrary to the view put forward in many chemistry text books, the theory was not immediately acclaimed. The existence of atoms was disputed with varying degrees of intensity for over 100 years. Suffice it to say that while Dalton's theory was a notable and imaginative advance, few of his postulates are now accepted.

Nonetheless, many scientists continued to regard the atomic theory as indispensable. In 1869 the Russian chemist Mendeleef published his 'periodic classification', showing that when all the chemical elements were arranged in the ascending order of their atomic weights, there were periodic recurrences of elements which resembled each other every eight positions on his table. For instance, starting with lithium and counting eight elements down the list one found sodium which displays similar properties. Mendeleef had to assume the existence of certain gaps in his classification in order to make the periodicity fit, and he was bold enough to predict the existence of new elements to fill these gaps, describing in advance what their properties should be, by considering
those of their 'sister' elements. Nevertheless, at least until 1913, there were still considerable difficulties in the table - in two cases elements were in the wrong order, there were still a good many gaps, and the whole arrangement in the latter part of the table was uncertain, and the regularities were for the most part less well marked. By the end of the nineteenth century, chemistry generally was less advanced than physics, although the manipulative techniques and the laboratory skills of chemistry had made tremendous progress.

Synthetic organic chemistry was highly developed. The students of Alfred von Baeyer had spread around the world, and no issue of 'Berichte' went without the synthesis of some more or less obscure organic compound. The same could be said of inorganic chemistry, but physical chemistry was in a different position. Classical thermodynamics, electrochemistry and topics related to them were firmly established: equivalent weights were being measured with impressive accuracy, and Rayleigh had discovered the inert gases and added a whole new column to the periodic table.

In 1880 Captain Wylie de Wiveleslie Abney RE, FRS, together with Lt. Col. Festing RE using an excellently planned spectroscope showed "The influence of atomic groupings in the molecules of organic bodies on their absorption in the infra-red region of the spectrum." Abney detected this phenomenon by putting a strip of filter paper soaked in alcohol in the path and noticed that this dried selectively in lines. Also, subsequently, he discovered that carbon tetrachloride gave no
lines (due to its symmetry). His discovery was forgotten to such an extent that in his Royal Society obituary notice, around 1920, this remarkable discovery was not even mentioned, and it was not until organic chemical analysis by spectroscopic means became fashionable that his work was 'rediscovered'.

Even at the beginning of this century the leading chemist of the day, Wilhelm Ostwald, was still vehemently making a stand against the atomic theory. In his Faraday lecture of 1904 to the Chemical Society he made a case for Chemistry without the atomic theory:

"It is possible to deduce from the principles of chemical dynamics (ie: thermodynamics) all the stoichiometrical laws:—
the law of constant proportions,
the law of multiple proportions,
the law of combining weights.
You all know that up to the present time it has only been possible to deduce these laws by the help of the atomic hypothesis. Chemical dynamics has therefore made the atomic hypothesis unnecessary." 12

3.4 RADIOACTIVITY

A new field of chemistry, relevant to subsequent work in mass-spectrometry, was radioactivity. It was discovered by Becquerel in 1896, and in 1898 the Curies discovered radium. By 1900 alpha, beta and gamma radiation had been discovered and associated with differing reactions to electric and magnetic fields, and varying penetrative powers.

Between 1900 and 1903 Rutherford and Soddy found that radium gave helium when it decayed, and they suggested that atomic disintegration was the cause of radio-active properties. They published a general theory of radioactivity in 1903, only to have their theory of
A central area of attention in science was the study of electrical phenomena.

In 1747 Benjamin Franklin, recognising two kinds of static electricity hitherto referred to as 'vitreous' and 'resinous', introduced the terms 'positive' and 'negative' to distinguish them, but proposed no theory of electrification. Accurate proof of the relation between positive and negative did not come until the time of Michael Faraday's ice-pail experiment in 1837 when it was discovered that positive and negative charges always appear simultaneously and in exactly equal amounts.

During this period, spanning approximately a century, there arose the so called 'two-fluid' theory of electricity, which, in spite of its intrinsic difficulties, dominated the development of electrical science. This theory divorced the notions of electricity and matter, but if one did not bother overmuch with the underlying physical concept, the theory lent itself admirably to the description of electrical phenomena and also to their mathematical formulation. It became possible to treat electricity as a category quite by itself, without any troublesome questions as to the relation, for example, between electrical and gravitational forces.

* The idea that all natural phenomena—electricity, magnetism, chemical and physical properties, mass, energy, etc.—were interrelated has not really developed since the unified field theory (20) was first propounded by Larmor. Faraday 'did' electricity and magnetism, Einstein 'did' mass and energy, but gravitation still eludes us.
But in spite of these advantages, the theory was makeshift. The notion that two fluids could exert powerful forces and yet be absolutely without weight, and further, that they had no physical properties whatsoever - i.e. they disappeared entirely when mixed in equal proportions - these notions were in a high degree non-physical.

Indeed, as J.J. Thomson remarked in his Silliman Lecture to the Royal Society in 1903 -

"... the physicists and mathematicians who did most to develop the fluid theories confined their attention to questions which involved only the law of forces between electrified bodies and the simultaneous production of equal quantities of plus and minus electricity, and refined and idealised their conception of the fluids themselves until any reference to their physical properties was considered almost indelicate."

The idea of a particular structure for electricity was foreign to the two-fluid theory, and there was seldom any mention in connection with it of an electrical atom, even as a speculative entity. Earlier, in contrast, Franklin had unquestioningly believed in the existence of an electrical particle or atom.

"The electrical matter consists of particles extremely subtle, since it can permeate common matter with such freedom and ease as not to receive any appreciable resistance." 22

The first piece of experimental evidence which appeared in its favour came in 1833 when Faraday found that the passage of a given quantity of electricity through a solution containing a compound of hydrogen, for example, would always cause the appearance at the negative terminal of the same amount of hydrogen gas irrespective of the kind of hydrogen compound which had been dissolved. 23

*Fantastic as this may seem, Franklin's hypothesis has certain formal similarities with the luminiferous ether, and the two fantasies have appeared to buttress each other.
After further experimentation with different chemicals, in 1834 Faraday stated these relationships quantitatively, in his laws of electrolysis:

"1) The weight of an element deposited or liberated in electrolysis is proportional to the current and the time for which it flows.

2) When the same current passes through different electrolytes for the same time, the weight of the element deposited is proportional to its chemical equivalent."

Striking and significant as these discoveries were, they did not serve at all to establish the atomic hypothesis of the nature of electricity. From 1834 until 1881 it remained almost unnoticéd that if Faraday's laws were true, and matter was atomic, then electricity, like matter, must be discrete. (It was Helmholtz who first pointed this out in his famous Faraday lecture before the Chemical Society in 1881.)

These discoveries were made at the very time when attention began to be directed strongly away from the concept of electricity as a substance of any kind, and it was Faraday himself who started this second period of the development of electrical theory, a period in which electrical phenomena were thought of almost exclusively in terms of stresses and strains in the medium which surrounded the electified body.

3.6 ELECTROMAGNETISM

Up to this time, a more or less definite 'something' called a charge had been thought of as existing on a charged body and had been imagined to exert forces on other charged bodies near it - analogous to gravitational force. This notion was repugnant to Faraday and he found experimental reasons for discarding it, when he discovered that the electrical force between two charged bodies, unlike gravitational pull, depends on the nature of the intervening medium (the dielectric).
Faraday pictured this medium as transmitting electrical force in approximately the same way as an elastic deformation is transmitted through a rod. Further, since electrical forces act through a vacuum, he had to assume that it was the ether which acted on the transmitter. For this to be so, the properties of the ether had to be modified by the presence of matter to account for the dielectric phenomenon.

These views, conceived by Faraday and put into mathematical form by James Clerk-Maxwell, called attention away from the electrical phenomena in or on a conductor carrying electricity and focussed it upon the stresses and strains taking place in the medium around a conductor.

Clark-Maxwell's treatise on the electromagnetic theory of light and electricity met with hostile reception which stemmed from the intangible nature of the hypothesis. As A. S. Eddington commented in 1929:

"One of the greatest changes in physics between the nineteenth century and the present day has been the change in our ideal of scientific explanation. It was the boast of the Victorian physicist that he would not claim to understand a thing until he could make a model of it; and by a model he meant something constructed of levers, geared wheels, squirts or other appliances familiar to an engineer. Nature in building the universe was supposed to be dependent on just the same kind of resources as any human mechanic; and when the physicist sought an explanation of phenomena his ear was straining to catch the hum of machinery. The man who could make gravitation out of cog wheels would have been a hero in the Victorian age." 28

However, a conclusive experimental proof of the validity of the electromagnetic theory was given in 1887 by the German physicist Heinrich Rudolph Hertz, who discovered a method of generating electromagnetic waves with the aid of an induction coil, and
obtained standing waves from these by means of reflection and interference. The nodes and antinodes of these waves could be determined by means of resonators, and it proved that electric forces were indeed transmitted in the form of electric waves, subject to the same laws as light rays and travelled through space with the speed of light, exactly as the Faraday-Maxwell theory had predicted. The triumph of the ether-stress point of view was complete.

Although the "field" approach of Faraday and Maxwell to electromagnetic phenomena seemed clearly superior to the "action at a distance" view, the new ideas were not immediately adopted by the physicists of the day. Thus Hertz wrote in 1893—

"as long as Maxwell's theory depended solely upon the probability of the results, and not upon the certainty of its hypothesis, it could not completely displace the theories which were opposed to it." 30

However, after describing his series of investigations, he was able to say—

"... the result of the experiments is to confirm the fundamental hypothesis of the Faraday-Maxwell theory." 31

Although the theory was not irreconcilable with the atomic hypothesis, it was antagonistic to it because it considered the strain as being distributed continuously about the surface of a charged body, rather than radiating from definite spots or centres.

By this time, both electricity and other electromagnetic radiation, eg: light, could be shown experimentally to behave both as particles and as waves. Evidence of a particular structure for electricity had been overlooked, so now, Clerk-Maxwell's brilliant theory of light together with such well-attested phenomena as Newton's rings, meant that the
evidence for light as particles was also overlooked. But if electromagnetic radiation consisted of transverse waves (the waves had to be transverse to explain polarization of light), the waves had to be propagated in some all-pervasive medium, and this was named the luminiferous ether, which was assumed to carry electrical force.

3.7 ELECTROLYSIS

Between 1834 and 1900 the physicist was in a peculiar position: when he was thinking of the passage of electricity through a solution, he pictured definite specks of electricity travelling through the solution. When thinking of the passage of electricity through a metallic conductor, he attempted to picture the phenomenon as a continuous 'breakdown of strain'. In other words, he recognised two distinct types of conduction - electrolytic and metallic: and since more of the problems of the physicist concerned metals than electrolytes, the atomic conception, as a general hypothesis, was neglected.

* It must be remembered that only one hundred years previously Newton's corpuscular theory was deeply entrenched in the scientific world. Scientists exhibited hostility when the first evidence of light behaving as waves appeared. This was the reaction to Thomas Young. Young's papers, containing the great principle of interference, constituted by far the most important publication on physical optics since the time of Newton. Yet they made no impression upon the contemporary scientific world. They were violently attacked by Lord Brougham in Nos. II and IX of the 'Edinburgh Review'. Young's articles were declared to contain "nothing which deserves the name of either experiment or discovery", to be "destitute of every species of merit." "We wish to raise our feeble voice against innovations that can have no other effect than to check the progress of science. We now dismiss, for the present, the feeble lucubrations of this author, in which we have searched without success for some traces of learning, acuteness and ingenuity, that might compensate his evident deficiency in the powers of solid thinking, calm and patient investigation, and successful development of the laws of nature, by steady and modest observation of her operations." Young issued an able reply, published in the form of a pamphlet, which failed to turn opinion in favour of his theory because, as he said, "One copy only was sold." 34
It would be unjust, however, to say that physicists failed to appreciate this gap between contemporary views: Maxwell himself in "Electricity and Magnetism" in 1873[^5^], recognises the significance of Faraday's laws in the chapter on 'Electrolysis' and says - "... for convenience in description we may call this constant molecular charge one molecule of electricity". Nevertheless, a little further on he negates this - "... it is extremely improbable that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a more secure basis on which to form a true theory of electric currents and so become independent of these provisional hypotheses."

In 1894 Johnstone Stoney, in furtherance of Faraday's experiments in electrolysis, went so far as to estimate the value of the elementary electric charge, called it the 'electron', and asserted that this was a fundamental constituent of nature. He considered that there is a single definite quantity of electricity which is independent of the particular bodies acted upon and to make it clear, he expressed Faraday's law in the following terms:- "For each chemical bond which is ruptured within an electrolyte, a certain quantity of electricity traverses the electrolyte which is the same in all cases."[^6^]

All this, however, was concerned with electrolytes - ie: solutions of metallic salts in water, and it did not follow that this unit of electricity had anything whatever to do with the structure of matter.

Faraday's electrolysis laws lead only to the conclusion that there exists a fundamental unit of electricity and that this is in some way connected with valency, for a monovalent ion bears one, a divalent two, a trivalent three, and so on, of these units of electricity. Even so, they did not show that these units entered into the composition of the...
atom, and the next steps in the unravelling of the structure of matter came from observations on the conduction of electricity through gases, first studied systematically by Faraday in the years 1836-8.

3.8 CONDUCTION OF ELECTRICITY THROUGH GASES

When a potential is applied to a gas such as air at ordinary atmospheric pressure, the gas behaves as an insulator, and a negligible current passes. If, however, the applied potential is sufficiently high, and the pressure is reduced, a situation is reached when a current flows, and the tube containing the gas is filled with a glow. If the pressure is reduced still further, the glow becomes less symmetrical and a dark space appears around the cathode (negative electrode). This is called the Faraday dark space, and is separated from the cathode by a bluish glow. Between the dark space and the anode is a series of luminous striations, and it was these more than anything else that attracted scientists to study the discharge. This work was hindered by technical problems, and important progress was possible only after the invention of the mercury pump, for producing better vacua, by Heinrich Geissler in 1855 who then produced great numbers of 'Geissler Tubes'—glass tubes with inserted electrodes, with a trace of a different gas in each. When connected to a potential, they produced colourful and striated glows. Geissler's pump was improved by Sprengel and Töpler in the sixties.

(Faraday took up the study of electrical discharge again in 1858.)

* G. Porter has pointed out(47) that immediately prior to Faraday's death in 1867 he had virtually discovered the electron and had done all the preliminary work on the passage of electricity through gases. Though he did not believe in atoms, he had already shown the "atomic" nature of electricity by his earlier electro-chemical work.

From his notebooks (held at the Royal Institute, Albemarle Street) Faraday appears to have been planning experiments very similar to those eventually carried out by J. J. Thomson some 40 years later.
Hittorf, in 1869, experimenting with Geissler tubes, noticed the increase of the Faraday dark space with the process of exhaustion, and that the discharge from the cathode caused considerable fluorescence against the glass.

In 1858 Plücker showed the effect of a magnet on the discharge, an occurrence which was recognised by Varley in 1871 and by Crookes and others in 1879.

William Crookes' experiments were extremely impressive. He took the width of the dark space to be the "... measure of the mean free path between successive collisions of the molecules of the residual gas." In Crookes' highly exhausted tubes "... the molecules of the gaseous residue are able to dart across the tube with comparatively few collisions, and radiating from the cathode with enormous velocity, they assume properties so novel and so characteristic as entirely to justify the application of the term borrowed from Faraday, that of 'Radiant Matter', and Crookes considered that this was a fourth state of matter, which he discovered would proceed in straight lines, cast shadows when intercepted by solids, was capable of turning a small paddlewheel, and was deflected by a magnet. Crookes argued that these streams of negative particles - cathode rays - resembled the processions of ions which, on the new ionic theory, were accounting for electrolysis.

In 1887 Schuster used this concept to calculate the charge-to-mass ratio (e/m) for the cathode rays from their deflection in a given magnetic field. The results were huge (and other workers obtained even higher values) but they were not at that time attributed to the low value for 'm'. What was needed was a method for disentangling the inter-related...
quantities 'e' and 'm' of the particles and their velocity 'v'.

Wiechert obtained 'v' in 1896 by using the oscillatory discharge of a condenser to obtain bursts of known very short length and showed it to be about one tenth of the speed of light. A few months later, Kaufman pointed out that this was consistent with the idea that all the energy of the rays came from the voltage between the anode and the cathode, though if this were so, the energy of the rays (assuming that their mass was of the same order as that of ions appearing in electrolysis) must be enormous.

The best way to find 'v' was to compare the deflection of the rays in a magnetic field with that in an electrostatic field, and it is at this stage in the research that J. J. Thomson's work takes the forefront.

3.9 J. J. THOMSON

Joseph John Thomson was born in Manchester, England, in 1856. His intended career was to have been in engineering, but owing to the difficulty of finding an opening, his father sent him to Owens College, Manchester where he came under the influence of Balfour Stewart, Professor of Physics, and Thomas Barker, Professor of Mathematics, and while there...
formed lifelong friendships with Arthur Schuster\(^{a}\) and J. H. Poynting\(^{b}\). Thomson always regarded this more or less accidental circumstance as the turning point in his life, for after this experience he gave up the idea of an engineering career and entered Trinity College, Cambridge, at the second attempt in 1876. In 1880 he was Second Wrangler\(^{\ast}\) in the Mathematics Tripos, (Larmor was Senior—later Sir Joseph Larmor FRS, discoverer of the Larmor precession\(^{\dagger}\)).

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\(^{a}\) Arthur Schuster (1851-1934) devised formulae to generate lines in the Rydberg series in spectra. Born Frankfurt-am-Main.

\(^{b}\) J. H. Poynting (1852-1914) went to Owens College (now Manchester University) in 1867. BSc at London 1872, then went to Trinity College, Cambridge. Came third in Maths Tripos in 1876. Back to Owens College under Balfour Stewart till 1878 when he returned to Cambridge to work under J. Clerk-Maxwell, first Cavendish Professor of Physics. In 1880 he got the chair of Physics at Mason College (now University of Birmingham) which he held until his death. Worked on pressure of light, studied electric and magnetic fields, and discovered Poynting's vector. (Poynting was instrumental in Aston going to work for Thomson). Noted among his publications: J. H. Poynting, The Pressure of Light (1910), J. H. Poynting and Sir J. J. Thomson, A University Textbook of Physics\(^{\ast}\) which was frequently published.

\(^{\ast}\) It is interesting to note how many second Wranglers in the Mathematics Tripos became physicists — James Clerk-Maxwell, J. J. Thomson.

\(^{\dagger}\) Sir Joseph Larmor (1857-1942) succeeded Stokes as Lucasian Professor of Physics. His life's work in theoretical physics to a large extent paralleled that of Thomson's experimental researches. Larmor's first major work was to describe a development of electromagnetic theory (Aether and Matter 1900) by which matter was composed of 'electrons' which were themselves 'freely mobile singular points in the specification of the aethereal strain'. Among later achievements he produced a theoretical account of the Zeeman effect, examined the problem of the reflection of radio waves in the upper atmosphere, and attacked the problem of aether-drift (the negative result of the Michelson-Morley experiment). Larmor stood between the old and the new physics: on the one hand one of the last great exponents of classical mechanics, while on the other hand certain aspects of his work tended towards relativistic ideas, and concepts of quanta — equally so J. J. Thomson's life time traversed the modern history of experimental physics.
He immediately took up research work in science which he had begun at Manchester (he worked with Rayleigh on Vortex atoms (a nineteenth century blind alley in the atomic theory)), was elected a fellow and in 1884, at the age of 27, was elected Third Cavendish Professor of experimental Physics following Lord Rayleigh's retirement. This choice was to him and to others somewhat of a surprise, as he had not taken his candidature himself very seriously, and was generally regarded as more a mathematical than an experimental physicist. It was remarked that things had come to a pretty pass when mere boys were made professors.

As Rayleigh's son remarks in his obituary of Thomson –

"... he was not himself a skilful manipulator; indeed he was somewhat incompetent with his hands, and had little knowledge of mathematical processes. Nevertheless, his great abilities and his natural inventiveness overcame these difficulties and he commanded the willing help of others whose qualities were complementary to his own."


+ At Cambridge, Sir Isaac Newton had carried out his early experiments in his own rooms; the Cavendish Laboratory was not opened until 1874, when the Duke of Devonshire, then Chancellor of the University, founded the laboratory and the professorship which bear his family name of Cavendish.

a Lord Rayleigh (Fourth Baron) died 1947, FRS 1905, Professor of Physics at Imperial College, London.
Almost immediately Thomson gave his main attention to the study of the discharge of electricity through gases: his earlier work was largely exploratory, searching, as it were, for a clue by which to explore the problems of this subject, so rich in experimental detail but with so few clues for theoretical guidance.

Thomson first concerned himself with investigations on the electrodeless discharge, endeavouring to eliminate the complications introduced by the presence of metallic electrodes, specifically that which occurs at the electrode/gas boundary.

As a method of studying the mechanism of discharge it proved somewhat disappointing, the intermittent character of the induced discharge being a serious handicap to quantitative investigations. Thomson also investigated the electrolysis of steam, and in 1894 attempted unsuccessfully to determine 'v', the apparent velocity with which luminosity was propagated along a long vacuum tube. However, the most fruitful period of Thomson's investigations into electric discharge began with the discovery by Röntgen of X-rays in 1895. By this time, Thomson's work and writings, in particular his "Recent researches in electricity and magnetism" as a supplement to Clerk-Maxwell's treatise had spread his reputation far and wide, and willing help for this new area of research into X-rays came from a group of men who took advantage of a scheme in operation at Cambridge allowing postgraduate students from other universities to come into residence and obtain a degree either by advanced study or research. Men of exceptionable ability, from several colonial and foreign universities gathered in the Cavendish Laboratory.

* Postgraduate students took MA by research. The PhD was not introduced until 1926.
There were:
Ernest Rutherford from Wellington College, New Zealand.
J. S. Townsend from Trinity College, Dublin.
J. A. McClelland from University College, Dublin.
J. C. McLennan from Toronto
P. Langevin from Paris
and several others. In addition there were C.T.R. Wilson, W.C.D. Whetham
(afterwards Lord Dampier) and others who had been trained at Cambridge.

The discovery of X-rays caused great excitement amongst scientists,
and triggered off similar experiments in almost every physics laboratory
in the world. Röentgen had, however, made a clean sweep of most of the
more easily accessible discoveries and consequently a good deal of this
early rush passed with comparatively little effect on the progress of
physics.

In the Cavendish, however, it was otherwise. Thomson soon discovered
that gases exposed to the influence of X-rays became conductors of
electricity under small potentials.* 56

This basic discovery was made simultaneously elsewhere, but it was
in the Cavendish that the implications of this new phenomenon were
unravelled. Thomson invited Rutherford to assist him, and an important
joint paper was published in which it was shown that the function of
X-rays was to liberate charged ions in the gas which moved under the
applied potential, thus constituting the carriers of the current.

*As A. Rupert-Hall remarks (A History of the Cambridge Philosophical Society,
1819-1969)

"As everywhere the mysterious powers of the X-rays are investigated and
demonstrated by photographing the bones of the living hand, coins in a box and
so on, it was characteristic that it was J.J. Thomson who discovered with
delight that X-rays ionised gases".

The discovery of X-rays was narrowly missed by several physicists: in the case of
Hertz and Lenard, because of failure to distinguish them from cathode rays. An
Oxford physicist Frederick Smith, having found that photographic plates kept
in a box near a Crookes tube were liable to be fogged, told his assistant to
keep them in another place!
The discovery of X-rays caught the imagination of both the scientific world and the popular press. It was so rapidly exploited in medicine that portable X-ray sets were being used to examine casualties on the British Expedition up the Nile to Omdurman in 1897. This is a classic example of the value of pure research. As J.J. Thomson bitingly said;

"Now, how was this method discovered? It was not the result of a worker in applied science trying to find an improved method of locating bullets in wounds. This might have led to improved probes, but we cannot imagine it leading to the discovery of X-rays. No, this method is due to an investigation in pure science, made with the object of discovering what is the nature of electricity"

If the rays were turned off, these ions disappeared by recombina-
tion; if, on the other hand the rays were kept going continuously then
the current which passed depended on the value of the applied electro-
magnetic field. If a large electro-magnetic field was applied, the
motion of the ions became so rapid that the whole number of ions was
usefully employed in conveying the current, none being wasted by
recombination and the current attained its maximum value, which Thomson
called the "saturation current". This phenomenon was quite unparalleled
in cases of conduction through metals or electrolytes and constituted a
striking proof of the correctness of the interpretation.

3.10 DISCOVERY OF THE ELECTRON

Thomson now turned his attention more definitely to the nature of
cathode rays. This had occupied him for many years and he had always
inclined to the view (advocated by Varley and by Crookes) that these
rays consisted of negatively electrified particles fired out of the
cathode, in opposition to the view taken by German physicists, notably
Goldstein, Hertz and Lenard, that the rays were of the nature of waves
in the ether.

Thomson was mainly influenced by the fact that the rays were
deflected by a transverse magnetic field, and on measuring the deflection
he began to doubt the view that these charged particles were molecules
or atoms, the deflection being too large for this hypothesis.

Hertz, Lenard and other workers had looked but failed to find any
electrostatic deflection, largely because the cathode rays collided with,
and ionised, particles of the residual gas. But Thomson, working with a
very high vacuum and therefore, necessarily, higher voltages, succeeded
in demonstrating electrostatic deflection.

* Pumps had progressed little in the previous 25 years and hand-operated Toepler
pumps backed by water jet pumps were still being used. Sources of stable low
voltages and intermittent high voltages were available but not always used.
He then combined the electrostatic and magnetic fields so that their mutual effect on cathode rays passing through them would cancel out the deflection arising from the effect of one field alone, and Thomson thus obtained a value for e/m - the ratio of the charge to mass of the cathode rays - which proved to be of quite a different order of magnitude from that of hydrogen atoms produced in electrolysis.

Assuming that the charge was the same in both cases it followed that the mass of the cathode ray particles was very small compared with the mass of the hydrogen atom.

The next step was to determine the absolute value of the charge (e), which he did by making use of C.T.R. Wilson's discovery that these ions could act as centres of condensation for cloud drops, so that from the rate of droplet formation, and by collection of the charge on the ions, the specific charge on one ion could be determined*. This value was found to be $6.5 \times 10^{-10}$ e.s.u., close to another value calculated by J. S. Townsend from the rate of diffusion of gaseous ions.

Although it appeared highly probable that this ionic charge was the same as the charge on a hydrogen atom in electrolysis, the identification involved wide limits of uncertainty. Up to this point there was no case where the value of e/m and e was simultaneously determined. Thomson saw the possibility of getting this by using particles which carried away the negative charge when ultra-violet light fell on zinc.

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*Wilson's cloud chamber, which was to become the most powerful instrument in understanding nuclear reactions, had developed from a field of science investigated long before the nucleus was conceived. Here, as so often in science, researches begun with divergent purposes have yielded results which converge on the solution of a problem outstanding in importance.
He devised a method of determining e/m for these particles and also determined the charge on them by using Wilson's Cloud Chamber, to show without any ambiguity that these particles had a mass of about one-thousandth of that of the hydrogen ion * , and had a similar charge. Thomson at first called these particles corpuscles, then later adopted the word 'electron' which had previously been used by Johnstone Stoney in a less definite connection. Thomson's suggestion that electricity consisted of particles was not new. During the century of controversy it had been put forward time and again. The importance of Thomson's work lay in the proof.

Thomson then developed a theory relating to these new sub-atomic particles, and discussed how they were to be regarded as constituents of atoms.

He drew the conclusion from Barkla's experiment on the scattering of X-rays * that the number of electrons in an atom was comparable with its atomic weight and devised a new model of the atom which he supposed to consist of a sphere of positive electricity in which electrons were held in a stable static equilibrium by their mutual repulsion and attraction with the positive electrified sphere, and he was able to show that such a model would have periodic properties, the electrons grouping themselves in successive rings as their number was increased.

After having elucidated the nature of cathode rays and discovered the electron (for which he was knighted and awarded the Nobel prize),

* The electron has an actual mass value of $\frac{1}{1840}$ of the hydrogen atom:

Mass of an electron = $9.11 \times 10^{-28}$ gm

Electric charge of an electron = $4.80 \times 10^{-10}$ e.s.u.
the second great period of activity for Thomson (from about 1906-1914) was occupied with his development of the study of positive rays. This line of research led directly to the mass-spectrometer and it derived equally directly from work on the electron.

3.11 POSITIVE RAY RESEARCH - THE BEGINNINGS OF MASS SPECTROMETRY

In the intensive field in front of the cathode in a discharge tube, the atoms are ionised. The negatively charged components fly away from the cathode, forming cathode rays. There are also positive rays which travel towards the cathode. There is an intense field in front of the cathode, and if the cathode is perforated, the rays pass right through it into the force-free region behind, and cause a glow in the gas. These rays were originally discovered in 1886 by Goldstein who observed the luminous tracks streaming through holes in the cathode and called them 'Kanalstrahlen' or 'canal rays'. (These really should be called 'channel rays'—'canal' is a mistranslation, but nevertheless became the accepted name.) In some respects they resembled cathode rays, in other ways they were conspicuously different. They produced different colour in the gas, a different kind of phosphorescence of the glass tube, and what was more important they were, by comparison, quite insensitive to magnetic deflection. Describing his results to the German Royal Academy, Goldstein wrote, "I was then ... very much surprised to see the cathode (constructed of metal netting) surrounded by a pale golden yellow light, which, penetrating the net, filled up the whole space from the cathode to the wall completely."

W. Wein in 1898 succeeded in deflecting them by means of very powerful magnetic fields, and showed that they were corpuscular, carried a positive charge and had atomic dimensions.

When Thomson took up the subject no one had previously succeeded in obtaining clear separation of the different kinds of atom which might be present in these rays.
This preliminary chapter is concerned with the pre-history of Mass-Spectrometry - I do not propose to discuss it in detail, but merely to draw attention to a number of outstanding points.

The discovery/invention of the Mass-Spectrometer was in no way a matter of serendipity. It followed logically from 75 years continuous research into the conduction of electricity through gases. Though Thomson introduced many experimental refinements, and realised the importance of attaining really low pressures to avoid secondary ionisation, the Mass-Spectrometer involved no dramatic technical or conceptual breakthrough. Thomson was familiar with the idea of using two kinds of fields - his apparatus to determine the charge to mass ratio (e/m) for the electron was to all intents and purposes the parabola type mass-spectrograph, and he just "twiddled it about a bit". (George Paget Thomson's word)

Far more remarkable is that the study of gaseous discharges had attracted so many of the best minds in the scientific community and that it was regarded as a promising field of research at a time when half a century of research had produced a mass of empirical data but no theoretical explanation. In his presidential address to the Royal Society in 1893 Lord Kelvin said that discharge experiments provided -

"... almost a complete history of the new province of electrical science which has grown up, largely in virtue of the great modern improvements for exhausting air from glass vessels, culminating in Sprengel's mercury shower pump by which we now have "vacuum tubes" and bulbs containing \( \frac{1}{190,000} \) of the air which would be left in them by all that could be done in the way of exhausting (\( \sim 1 \) torr) by the best air pump of fifty years ago. Discoveries in this new field
(eg: deflection of beam by magnet) have been arrived at not by accident, and not merely by experimental skill and acuteness of observation, but by carefully designed investigation into radiation in rarefied systems."

This is a curious statement five years before the first of the major discoveries which emerged from discharge tube work, and implies some sort of hunch among physicists about what was likely to be important.*

Geissler discharges were varied, brightly coloured, pretty, and occurred at pressures where matter might be thought to behave simply - viz: Crookes' 'fourth state of matter'. It is difficult to escape the feeling that concentration on gaseous discharges involved aesthetic as well as scientific judgement. Another point worth considering here, and remarked upon by Lord Kelvin, is the extent to which the prerequisite technology prevailing at the time dictates the level of scientific research and concomitant discovery. Research into the conduction of gases was to a very large extent governed by the limits of vacua - ie: the quality of the pumps available.

* Another curious point is that in the same issue of the Proceedings of the Royal Society, (1893), appeared a paper by J. Larmor (66) - "the Luminiferous Ether", in which he attempted to describe a unified field theory for the ether, but without any reference to the Michelson-Morley experiment of 1887, which he would certainly have known about. This is extremely interesting when looked at in the light of normative science - Larmor obviously was not going to be thrown off producing a unified, definitive theory merely by a piece of conflicting evidence, and chose to ignore it, rather than argue round it.

To explain further, the great majority of nineteenth century physicists since the time of Fresnel were more confident of the existence of the luminiferous ether than of matter. And yet, difficulties were encountered in postulating properties which were consistent. To explain polarisation of light Fresnel and Young were driven to assume that light waves have vibrations transverse to the direction of propagation, and this in turn necessitated an ether that was an elastic solid, for an elastic fluid like air would admit of only longitudinal vibrations.
But if this ether is an elastic solid, how can the planets move through it without hindrance? To explain Bradley's aberration of light, Fresnel assumed that the telescope moves with the earth through space without disturbing the ether and the wave motion in it.

(Stokes and Kelvin offered the explanation that the ether was like shoemakers wax, which would vibrate under a sharp blow, yet would be plastic and permit slow motions of a heavy solid through it.)

However, if the ether is not moved in the least by dark bodies moving through it, how can a light wave cause molecular motion called heat, and how can molecular motion set up vibrations in the ether? Stokes modified the hypothesis by assuming that the ether close to the surface of the earth is completely dragged along by the earth, while higher up only partially, and "at no great distance it is at rest in space." All in all, physicists preferred a free ether which was not dragged along by moving bodies. In 1887 Michelson undertook to test by direct experiment whether the ether was stationary or not. His now famous experiment at Cleveland, Ohio, indicated that the earth drags the ether with it completely or almost completely. 67

Kelvin in 1900 spoke of "two clouds 'obscurring' the beauty and clearness of the dynamical theory which asserts light and heat to be a form of motion". One of those clouds was the Michelson-Morley experiment.

It is important to note that the atomic theory, even at its most primitive stage, was useful because it was not trivial. It was Einstein who said "... a non-trivial theory even if wrong is more useful than a trivial one."

The atomic theory offered a basis on which to formulate new experiments and a practical hypothesis against which to measure experimental data - aethereal theories gave philosophical satisfaction to many scientists but proved extremely intractable to use as a working theory in experimental work.

The Marxist theory of science in its simplest form is deterministic. Discoveries are made at a particular time not because of brilliant (or lucky) individuals, but because the ineluctable tide of history has reached a particular landmark. The theory is unfalsifiable in that it is impossible to prove that under only slightly different circumstances
a discovery would have been made at another time. It is nonetheless interesting to speculate whether Faraday might have discovered the electron had he lived a little longer.

In the light of the foregoing comments, a final question which might be posed is why it was that J. J. Thomson discovered the electron and no one else? The answer, flippant though it sounds, must be that the Cavendish professor of experimental physics at the University of Cambridge was, by definition, the pre-eminent physicist in Britain, and that if he does not make major discoveries, who else should? (Not forgetting the supporting role of the brilliant postgraduate students surrounding Thomson).

There were other laboratories of comparable distinction, especially in Germany, but nowhere which could be regarded as superior. In addition, the Germans tended to be more enthusiastic about wave theories than were the pragmatic British: the abstract nature of the luminiferous aether fitted in better with the traditional abstractions of German philosophy. Pledge has claimed that the mid-nineteenth century was dominated by ideas of continuity - Cauchy's function theory, the aether for light, infection leading up to Pasteur and Darwin's continuity of species. The atomic theory was in eclipse and nowhere more than in Berlin where Ostwald viewed energy as the true reality. We must not be led by this sort of observation into deterministic theories of science, but it is significant that the opponents of atomic theories of matter and energy in the nineteenth century were on the European mainland while the proponents tended to be British or American.
CHAPTER III

SCIENCE AT THE END OF THE NINETEENTH CENTURY

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In 1910 Thomson first separated the different mass-components of a chemical substance, using his new "Parabola" method of analysis, in which both the principles involved and the apparatus bore many resemblances to his experiments and work in determining e/m for the electron.

G. P. Thomson comments on this:

"He had done something very like the parabola method before, not identical, but he was very familiar with the idea of using two kinds of fields, electric and magnetic fields - you see he used these to find e/m for the electron.

Q. Do you think this was extrapolation?

Oh yes, it was, he just twiddled it about a bit.

Q. Do you think J.J. realised at the time that getting these parabolae provided the first experimental proof of Dalton's atomic theory that all atoms of a particular substance had the same mass?

Yes, I think he did. What he was really interested in was looking so to speak for the proton. All right, electrons are negative, so they've got to be positive obviously - what is this - is it conceivably something which is different for every element, which would fit in very nicely with his first theory of how the electrons were arranged, or was there some fundamental unit? He came to the conclusion that there was no positive thing lighter than the Bohr hydrogen atom less an electron. That was the first object of the operation, so to speak, and that was successful."

In Thomson's positive ray apparatus, positively charged particles passed through a long narrow tube (a hypodermic needle was used): the fine beam thus produced was then subjected to the combined action of parallel electrostatic and electromagnetic fields, so that the

George Paget, later Sir George Paget Thomson, son of Sir J.J. Thomson, who also received a Nobel prize for physics.
THOMSON'S POSITIVE-RAY PARABOLA APPARATUS

The letters indicate the following:-

A, discharge tube; B, cathode; C, water jacket for cooling cathode;
D, anode; E, gas inlet; F, pump lead; G, photographic plate;
I, magnetic shield; M M₁, magnetic poles; N N₁, mica for electrical insulation of P P₁, which are pieces of soft iron to serve both as condenser plates and to define the magnetic field.

(From Aston, 1942)

[4.1]
displacements of the beam trajectory produced by the fields were mutually at right angles. Thomson placed a photographic plate in the vacuum envelope, so that it was normal to the central undeflected ray.

If all the positive ions had had the same velocity, and had differed only in mass/charge ratio, a series of spot images for each mass/charge number would have been produced on the plate. However, the spread of ion energy in these experiments was of the order of 1000 ev; consequently, the images were drawn out by the action of the fields into a series of truncated parabolae. The limits of 'cut-off' of these curves were determined by the maximum and minimum velocities of the particles; but each parabola corresponded to some definite value of mass/charge ratio, and ions with identical mass/charge ratios fell on the same parabola.

In this way a great variety of different atoms and atomic groupings were proved to be present in the discharge tube, some due to ions formed in the discharge tube from gases he had introduced, and others arising from impurities in the system which desorbed from the walls, etc., and their nature could be identified by measurement of the co-ordinates on the picture combined with a knowledge of the strengths of the fields.

Thomson thus attained an entirely new way of separating atoms which showed that such atomic groupings as \( \text{CH}_1^+ \), \( \text{CH}_2^+ \) or \( \text{CH}_3^+ \) could exist, groupings which have no stable existence in the chemistry of matter in bulk, and also that the atom of mercury, for example, could take up a great variety of charges from one to seven times the electronic charge.

Wilhelm Ostwald, the last and greatest opponent of any form of atomic theory, recognised even more clearly than Thomson himself the implications of Thomson's work. In 1909, before Thomson had published the details of his mass spectrometric work, he wrote:

The earlier investigations of the rays were made with screens coated with powdered villemite, which glows a faint green when struck by positive rays, and as early as 1907 Thomson made observations on hydrogen and helium rays by this means (4).
"I am now convinced ... that the isolation and counting of gaseous ions on the one hand which have crowned with success the long and brilliant researches of J. J. Thomson, and on the other, the agreement of the Brownian movement with the kinetic hypothesis ... justify the most cautious scientist in now speaking of experimental proof of the atomic theory of matter. The atomic hypothesis is thus raised to the position of a well-founded scientific theory."

After Ostwald's admission, it became possible in the next few years of this century, actually to see the effects produced by a single atom.

Previously in 1903, Crookes had devised a simple instrument known as a 'Spinthariscope', in which the observer sees a display of intermittent scintillations, and later, quantitative study using the spinthariscope by Regener, Rutherford and Geiger established that each flash arose from the violent impact of a single 'α' particle on the surface of a zinc sulphide crystal.

As G. P. Thomson has previously commented, it is important to note here that the sharpness of the parabolae obtained was the very first experimental proof of Dalton's hypothesis that atoms of the same substance were of even approximately similar masses. The construction and use of Thomson's positive ray apparatus presented formidable experimental problems. Production of a beam of positive particles in itself was very difficult. To produce enough particles to register on a photographic (footnote continued from previous page) Hammer photographed the illuminated screen from the outside(5), but the real advance in technique was made in 1910 by the recording of the impact of these rays in a photographic sensitised surface introduced inside the evacuated chamber. This device was first published by Koenigsberger & Kutschewski(6), and was used independently by Thomson.
plate, Thomson had to use a large flask, the entrance to which was closed by the cathode. The pressure in the flask was higher than that in the rest of the apparatus; the particles produced passed through the tiny channel in the centre of the cathode into the deflectory fields. The particles themselves often hit the side of the channel, and eroded it like a sand-blast, causing the channel to silt up, and its limited life considerably increased the experimental difficulties.

Amongst Thomson's many experimental refinements of the parabola method, was his discovery of the crucial importance of working at the lowest possible gas pressure, so as to avoid secondary phenomena due to the particles making collisions and acquiring or losing charges while they were traversing the fields. These 'secondary rays', as Thomson called them, produced parabolae very much like the 'genuine' primary ones, and could be easily confused with them.

One of the shortcomings of the photographic method for recording the incidence of ions was that the plate had different sensitivities for the various ions, and this made accurate quantitative measurements of the relative intensities impossible. When Thomson introduced such polyatomic molecules as COCl₂ and hydrocarbons into the discharge tube, he noted the formation of many parabolae which indicated the formation of a variety of positively charged fragments (e.g.: COCl⁺, CO⁺, C⁺, O⁺, etc., from the COCl₂). To investigate these more thoroughly, Thomson substituted an electrical recording method for the photographic plates, and used a Wilson tilted electroscope and Faraday cage behind a parabolic slit. By varying the magnetic field and taking 10 second measurements, he measured the current as the various positive ions were brought over the slit: i.e: he obtained a plot of the ion current as a function of the m/e ratio; what we would now term a mass-spectrum.
Thomson pointed out that for widely varying mixtures of hydrogen and oxygen, with the photographic method, there was little detectable difference in the intensities of the $\text{H}_2^+$ and $\text{O}_2^+$ ions. But with the electrical detection system, a more quantitative measure of the relative proportions in a given mixture was possible.

As Cavendish Professor of Physics, and also as Professor of Natural Philosophy at the Royal Institution, Thomson had a great many other affairs to occupy his time besides his positive ray researches. He had been assisted in his experiments by Everett on the technical staff in the Cavendish, who built most of the first machine and performed most of the routine analyses of positive rays. Thomson wanted to speed up the work a little, and decided another similar parabola apparatus should be built, and another assistant employed. The first person to fill this vacancy was a man named Eagle, another technician but one who had very little of Everett's skill and expertise. Thomson therefore decided to replace him with a person who would be a little more independent from him than a technical assistant. Money was available to employ a demonstrator, and so, in 1910, by a most fortunate decision Thomson invited Frederick Aston, who at this time was 33 years old, to join him on this work. This invitation was largely at the suggestion of Thomson's great friend J.H. Poynting, Aston's former teacher at Mason College.

Aston had studied chemistry and physics at Mason College, and had then been awarded a sponsorship to work as a research organic chemist under Frankland. Faced with the necessity of earning a living, Aston was persuaded to study fermentation chemistry and went to work in a brewery in 1900. Throughout this period in his spare time he designed and built a new pattern of Sprengel pump, and with it exhausted small X-ray tubes made from test-tubes, and for which he wound an induction coil.
Continuing his spare time work he invented an automatic form of Toepler pump and with it discovered a form of irreversible discharge tube with which the discharge of an induction coil could be rectified.

Aston had thus spent a large proportion of his earlier years in experiments involving high vacua: this concentration of interest he attributed to the profoundly exciting discovery of X-rays.

Aston's skill and technical expertise in manufacturing and modifying pieces of equipment for research use together with his skill in vacuum technique was invaluable to Thomson's research in positive rays. It was Aston who introduced the use of large global low pressure discharge bulbs, designed the particular type of camera which was to remain standard equipment in the parabola method of analysis, and invented the stop-cock switch which enabled objects to be moved in vacuo.

It is impossible to ascertain from Thomson's published papers in which of the two apparatuses particular experiments were carried out: Everett and Aston at this early date took their instructions from Thomson as to which particular positive rays to study.

By 1911, however, Aston was beginning to devise his own experiments. He carried the subject into the field of rare gases in collaboration with H. E. Watson, a pupil of Ramsey, and learned from him the technique of manipulating these gases which were only available in very small amounts.

At this time (1912) Thomson's attention was occupied (using Everett's apparatus) with the investigation of Hydrogen, and in particular the positive ray parabola of mass 3 to be discussed in Chapter 5.
By the summer of 1912, a modified version of the positive ray apparatus was constructed which was a considerable improvement on those made previously, in which parabolae corresponding to masses differing by only ten per cent could be clearly resolved and distinguished.

In November a sample of the lighter constituents of air was introduced. An excellent description of the results was given by Sir J. J. Thomson in his address to the Royal Institution on Friday 17th January 1913:

"I now turn to the photograph of the lighter constituents: here we find the lines of helium, of neon (very strong), of argon, and in addition there is a line corresponding to an atomic weight 22, which cannot be identified with the line corresponding to any known gas. I thought at first this line, since its atomic weight is one half that of CO2, must be due to a carbonic acid molecule with a double charge of electricity, and on some of the plates a faint line at 44 could be detected. On passing the gas slowly through tubes immersed in liquid air the line at 44 completely disappeared, while the brightness of the one at 22 was not affected.

"The origin of this line presents many points of interest; there are no known gaseous compounds of any of the recognised elements which have this molecular weight. Again, if we accept Mendeleeff's Periodic Law, there is no room for a new element with this atomic weight. The fact that this line is bright in the sample when the neon line is extraordinarily bright, and invisible in the other when the neon is comparatively feeble, suggests that it may be possibly a compound of neon and hydrogen, NeH2, though no direct evidence of the combination of these inert gases has hitherto been found. I have two photographs of the discharge through helium in which there is a strong line, 6, which could be explained by the compound HeH2, but, as I have never again been able to get these lines, I do not wish to lay much stress on this point. There is, however, the possibility that we may be interpreting Mendeleeff's law too
rigidly, and that in the neighbourhood of the atomic weight of neon there may be a group of two or more elements with similar properties, just as in another part of the table we have the group iron, nickel and cobalt.

"From the relative intensities of the 22 line and the neon line we may conclude that the quantity of the gas giving the 22 line is only a small fraction of the quantity of neon."

Thomson's two separate neon parabolae were an indication of its complexity*, though he was reluctant to draw this conclusion. Thomson comments in a letter in reply to a question put to him at his Bakerian Lecture of 22nd May 1913:

"I do not think myself that it is impossible to separate the gas with atomic weight 22 from Neon (20) by fractionation: in fact F. W. Aston is at present engaged on this work. In W. Watson's experiments he was fractionating against a light gas helium and so threw away the lighter samples, the result was to increase the proportion of 22 to 20 beyond the normal. When we tested the gas Watson had used by the positive ray method we found a considerably larger percentage of 22 than in any other sample of neon we had."

While not adopting the view that the heavier constituent of Neon was a compound NeH₂ which could have given the observed atomic weight within the limits of experimental error, Thomson was not convinced that this explanation was absolutely excluded.

All other attempts on different samples of neon gave similar results, and even the most careful purification of the neon did not appreciably alter the intensity ratio.

* The word complexity is used in its particular scientific sense to indicate that an element is comprised of more than one isotope; not to be confused with the use of the word 'complex', a chemical term to indicate that in a particular compound a ligand is present.
The only alternative was that the second line was due to a modification of neon differing from normal neon only in atomic weight, a relation identical with that described by Soddy as existing between chemically inseparable radioactive elements. As early as 1910 Soddy made a remarkable summary of the situation:

"These regularities may prove to be the beginning of some embracing generalisation, which will throw light, not only on radioactive processes, but on elements in general and the Periodic Law. Of course, the evidence of chemical identity is not of equal weight for all the preceding cases, but the complete identity of ionium, thorium and radiothorium, of radium and mesothorium 1, of lead and radium D, may be considered thoroughly established ... The recognition that elements of different atomic weights may possess identical properties seems destined to have its most important application in the region of inactive elements, where the absence of a second radioactive nature makes it impossible for chemical identity to be individually detected. Chemical homogeneity is no longer a guarantee that any supposed element is not a mixture of several of different atomic weights, or that any atomic weight is not merely a mean number. The constancy of atomic weight, whatever the source of material, is not a complete proof of homogeneity, for, as in the radioelements, genetic relationships might have resulted in an initial constancy of proportion between the several individuals, which no subsequent natural or artificial chemical process would be able to disturb. If this is the case, the absence of simple numerical relationships between the atomic weights becomes a matter of course rather than otherwise." 13

After the Rutherford nuclear atom (1911), positive ray analysis of neon (1912) and the Bohr atom model (1913), came Soddy's definition of isotopes (1913) as atoms of identical chemical properties but different nuclear characteristics.
[4:3]

Mass Number 20

[Doubles and Fine Structure, J. Mattauch (1938)]

An indication of the many multiplets that occur at the same mass number as neon
"The same algebraic sum of the positive and negative charges in the nucleus when the arithmetical sum is different gives what I call 'isotopes' or 'isotopic elements' because they occupy the same place in the periodic table. They are chemically identical, and save only as regards the relatively few physical properties which depend upon atomic mass directly, physically identified also."  

The parabola method of separation was not sufficiently accurate to distinguish between $^{22}\text{Ne}$ and $^{20}\text{NeH}_2$, and also the general diffuseness of the parabolae made it difficult even to distinguish between $m/e=20$ and $m/e=22$. This led Aston to undertake an intensive investigation of the constitution of the gas, and his initial experiments were attempts to separate the constituents by physical methods.

He first tried fractional distillation and carried out a three-week experiment, at the end of which he determined the densities of the various fractions with a quartz microbalance, an ingenious instrument which he had had to invent before any experiments involving density determination could be made.

The most volatile fraction had an atomic weight of 20.19 which was identical with experimental error, using the accepted atomic weight for neon of 20.2, and it was evident that no appreciable separation had occurred. A positive ray photograph of the two extreme fractions showed no appreciable change in the relative intensities of the two parabolae.

Aston made a further attempt at physical separation by the method of diffusion through a porous body, in this case pipe clay. The final densities of the two fractions were 20.15 and 20.28, a small difference

* Remember that the neutron had not yet been discovered.
but apparently greater than the experimental error. Encouraged by this result, Aston devised an elaborate diffusion apparatus, which automatically performed the mechanical operations many thousands of times, but the separation achieved was only about half that obtained in the first diffusion experiment.

Aston comments:

"We have reached the position where it could be said that the presence of two isotopes in neon was indicated by several lines of reasoning, although none of these could be said to carry absolute conviction."

At this point the work was interrupted by the outbreak of the Great War. Aston went to work at the Royal Aircraft Factory (later Establishment) at Farnborough. He was staying in a boarding house together with George P. Thomson (J.J.'s son) and F. A. Lindemann, (later Lord Cherwell). G. P. Thomson describes several aspects of life together, in particular a series of discussions which went on over many weeks concerning the problem of the constitution of neon.

"We were all together in a boarding house called 'Chudleigh', a kind of unofficial civilian mess; a good many of the leading people came along.

"Aston was already convinced that neon had the two isotopes, but Lindemann was not. Lindemann always liked to try and disprove other peoples' discoveries. Everybody expected 20, that was fine, then there was this 22; Lindemann argued that it was doubly charged CO\textsubscript{2}, Aston replied that he never got a doubly charged molecule, and so the argument went on evening after evening.

"Now Aston, though a superb experimenter, did not know very much about physics, and hadn't got an analytical brain, whereas Lindemann had an excellent analytical brain, and whey they went up to bed Lindemann always won the argument. But next morning
A splendid example of Lindemann's personality appears in a Faraday Society discussion in 1922 concerning the radiation theory of unimolecular reactions, a theory current up to 1922 that whereas reactions whose rate was proportional to the square of the pressure were due to collisions between the participating molecules, reactions whose rate was proportional to the first power of the pressure (i.e., first order reactions) were photochemical reactions brought about by infra-red radiation from the walls of the containing vessel.

At the tea break Lindemann said that the radiation theory was 'bloody stupid' and produced several good reasons against it. Lindemann had the reputation for being a highly destructive critic, and someone turned round and remarked that if he didn't like the theory, he should produce a better one. Lindemann did this on the back of an envelope and presented it after tea. It became known as the Lindemann theory of unimolecular reactions and in outline is accepted even today. Lindemann never published it properly and so this very important theory is only to be found in his comments recorded at the Faraday Society discussion.

This account is interesting from several aspects. Lindemann, though he got the Chair of Physics at Oxford very young, made very few contributions to science (though as Churchill's main adviser on scientific affairs, he wielded tremendous political influence and was the instigator of the disastrous strategic bombing offensive against Germany during the Second World War.)

Lindemann emerges as a man who knew so much about his subject that he was unable to make any truly novel contribution to it. Discoveries are often made by people who do not know enough about something to be aware of the difficulties (e.g., Marconi, and Watson's role in the elucidation of the structure of DNA as outlined in 'the Double Helix').

It is curious that his only major contribution was to a branch of science (chemistry) not his own, and that it appeared as a mere comment.
Aston was back where they had started from, completely unconvinced by counterarguments. This was very characteristic of Aston.

"He had an awful job to convince Lindemann, because they had done really all they could do, by the time war broke out, with the existing positive ray type of machine using parabolae, which does not give you a very high resolution. The only thing would have been to make the apparatus very much larger, but this would have been fraught with difficulties, with vacuum and so on. Vacuum was a very serious matter because you had to have enough gas in the discharge tube to get a discharge: all these things were made by gaseous discharges so you could not just pump the thing out and keep it at that, you depended on the difference of pressure you could get between the actual discharge you wanted and the place where the rays were to be observed, where of course ideally you would have liked to have had no pressure at all, and it all depended on the use of charcoal and liquid air as a means for absorbing gas.

"Actually when Aston decided he would not go on with the parabola method but try physical separation Lindemann may have been some help because of the isotopes. Lindemann was very well up in what there was of the quantum theory in those days, having worked with Nernst on things like specific heats of diamonds and that sort of thing, so he helped Aston quite a bit, in fact they published a joint paper after the war discussing the times in which you could expect separation and when not. As you know, Aston was pretty disgusted with the negative results of his experiments. I must say that most people would have given up on that but he didn't, he decided to do it a different way, culminating in the design of the famous mass-spectrograph and he was rewarded, because he not only proved the complexity of Neon but found practically all the other elements were like it too!"

By the time work resumed after the end of the war, the existence of isotopes among radioactive substances was generally accepted. This fact automatically increased the urgency of the confirmation of the complex nature of neon.
Aston realised that physical separation of isotopes could only be partial, and that the most satisfactory proof would be by the method of positive-rays. Unfortunately, no apparatus in existence was adequate.

Meanwhile, Thomson had examined chlorine by the parabola method and had come to the conclusion that it was 'normal' - i.e. he was able to find only a single parabola at the position corresponding to its chemical atomic weight. Chlorine (Mass 35.7) has in fact two isotopes of masses 35 and 37 present in the ratio 3:1, and Thomson's failure to observe a second parabola must have involved a degree of wish fulfilment. His erroneous conclusion, naturally not stressed during his lifetime, is interesting historically, and does something to explain his critical attitude when he subsequently opened the discussion on isotopes at the Royal Society in 1921. Aston was of course well aware of the shortcomings of the parabola method, having worked with it for three years from 1910-1913. On his return to the Cavendish in 1919 he therefore devised a new and more powerful method of analysis and hand built all the necessary apparatus.

4.4 ASTON'S MASS SPECTROGRAPH

This new type of positive ray apparatus could be likened to an ordinary optical spectrometer, in that it gave a spectrum of lines, but each line corresponded to a definite m/e number instead of a wavelength. Aston's particular arrangement of electric and magnetic fields may be likened to an achromatic set of prisms without lenses. Positive ions of the elements or compounds under analysis were created in a discharge tube at potentials ranging from 20-50 kV and were collimated by two very narrow parallel slits. The resulting thin ribbon of ions passed through electric and magnetic fields so arranged geometrically that the dispersion of the ions produced by the electric field was cancelled by the dispersion produced by the magnetic field. All particles of equal mass/charge ratio,
ASTON'S FIRST MASS-SPECTROGRAPH

$S_1$ and $S_2$ are collimating slits, $P_1$ and $P_2$ are condenser plates, $D$ is a diagram for selecting a portion of the beam emerging from the condenser, $O$ is the centre of the uniform magnetic field, $GF$ is the photographic plate, $\theta$ and $\phi$ are the deflections in the electric and magnetic fields respectively.

(From Aston, 1942)
but of different velocities within a broad velocity band were thus focussed to the same line on a photographic plate. The ion energy spread was in the region of 1000 eV, much the same as in Thomson's experiments. A significant increase in intensity and resolving power achieved in this instrument, as compared with the parabola equipment, made possible for the same ion source conditions shorter exposure times and application to more difficult elements. The increase in intensity resulted from three design features:

1. Substitution of slits for circular apertures,
2. Evacuation in the region between the beam defining apertures to reduce loss in intensity by scattering of the ions,
3. 'velocity focussing'.

The actual determination of the masses was purely an empirical process: the position of the unknown lines caused by the masses in question were compared with the positions of known reference lines. At the same time attempts were made to determine the relative abundances of the different masses by the laborious method of measuring photometrically the density of the traces on the photographic plate. There were numerous sources of error, which Aston fully recognised: for example, the density of the image produced on the plate was an unknown function of the intensity of the corresponding ion beam, and it was necessary to make a number of tests, none of which was conclusive, before any reasonably definite relationship could be established. Also, ions of different mass/charge ratio followed entirely different paths through the electromagnetic analyser, and corrections had to be made for this. In addition, imperfect focussing, for which no correction could be made, gave rise to variations as great as those which resulted from inequalities in the photographic emulsion.
A indicates the analysing chamber, B and C are brass walls of the vacuum chamber, D is a diaphragm to prevent transmission of reflected rays, E is the ion collector unit, G is the ion source region, S₁ and S₂ are slits (From Dempster, 1918).

DEMPSTER'S MODIFIED APPARATUS (1922)

W is the Wilson electroscope
L is the ionisation chamber
Aston concluded that accurate measurements would be possible only by measurement of the total ion current: with Aston's type of analyser, however, the beams were far too weak to be measured with any acceptable degree of accuracy by electrical means.

Consequently, this type of apparatus (velocity-focussing) was useful only for the accurate determination of isotopic mass, and not for abundance measurements. As developed by Aston, Bainbridge, Mattauch and others it became one of two main branches of positive ray analysis, the other being the development of mass-spectrometry, the measurement of isotopic abundance, which stemmed from A. J. Dempster's pioneer work at the Ryerson Physical Laboratory, Chicago, who until the outbreak of World War I had been studying at the University of Würzburg under the professor of physics W. Wien, and both Dempster and Wien were engaged in researches involving the deflection of cathode beams and positive rays by electronic and magnetic fields.

4.5 Dempster's Mass-Spectrometer

At about the time (1919) that Aston was developing his first mass-spectrograph, Dempster in the USA had produced a different type of positive ray apparatus which was well suited to making abundance measurements but not to accurate mass determinations.

His was essentially the same method as used in 1907 by Classen to determine e/m ratio for electrons. If the positive rays all passed through the same potential and so possessed the same energy, instead of the spread of energies in the discharge tube, then analysis could be by magnetic field alone - i.e.: a system of direction focussing. Whereas Aston used his deflecting magnetic field as 'prisms' to disperse, Dempster used the magnetic field as a 'lens' to condense or focus.
Dempster's mass-spectrometer employed this system. An ion beam having reasonably mono-energetic particles passed through a slit into a uniform magnetic field. The various ions described semicircles of different radii proportional to their respective momenta and refocussed after 180°. They passed through an adjustable collector slit and fell onto an electrical collector connected to an electrometer.

This method was limited by the fact that the ions had to be generated with a velocity negligible compared with that produced by the accelerating potential. However, the resolution possible with this first apparatus was in the region of one part in 100, and the method was later successfully applied in the analysis of complex elements.

Dempster's first results were obtained from ions produced by heating salts on platinum strips, or by bombarding them with electrons. In 1922 he modified his mass-spectrometer in which the metals themselves were vaporised. The vapour so produced is ionised by bombardment with electrons, and the positively charged ions pass into an accelerating field so that all of them reach the slits with the same energy.

In Dempster's first experiments he used a quadrant electrometer to measure the ion current, but this was replaced in the modified apparatus with a Wilson tilting electroscope and a compensating arrangement, where the positive ion current from the collector is balanced by an equal negative ionisation current from the electrode of an ionisation chamber L., and the electroscope is used to indicate when balance was obtained.

Dempster found that alteration of the accelerating potential (in order to bring rays of different mass to the collector slit) was responsible for a serious source of error in measurements of the relative intensity of isotopes, an error not detected in his earlier experiments. It was due to a stray electric field, and Dempster eliminated this error by the use of gauzes.
To return to Aston, his first mass-spectrograph at the Cavendish was an immediate success, and the first results obtained with Neon vindicated Aston's earlier arguments and showed beyond doubt that this element was a mixture of two constituents which fell into the category of Soddy's isotopes and had whole number atomic weights of 20 and 22.

As the work progressed with chlorine and other complex elements it soon became clear that the atomic weights of all the isotopes had integral, or very nearly integral values. The 'whole number rule' was formulated by Aston, and this was of the greatest theoretical importance. It stated that the weights of the atoms of all the elements measured, with the exception of hydrogen were whole numbers to the accuracy of the measurement. This 'whole number rule' enabled the simple view then to be taken that atoms were built of two units, then thought to be protons and electrons, all the former and half the latter being bound together to form the nucleus.

Aston, however, found hydrogen to be a simple element (i.e., no other isotopes) and that the mass of its atom corresponded to the chemical atomic weight and was definitely divergent from a whole number. Experiments on Hydrogen also furnished proof that the particle of mass 3 first observed and investigated by Thomson was actually a molecule consisting of three hydrogen atoms.

The difficulty in obtaining the necessary rays for analysis varied enormously from element to element. Two main devices were employed by Aston: the ordinary gas discharge which requires the element to be volatile or form suitable volatile compounds, and the anode ray discharge in which the halide or other compound of the element is treated as an anode in a discharge at low pressure.
Aston frequently comments in his book and papers that the knowledge of the mechanism involved during discharges was very incomplete and largely empirical.

As late as 1940 Aston wrote:

"After over thirty five years experience with vacuum discharges I know no method of obtaining, still less of reproducing, first-class results with any certainty. Although our knowledge has advanced enormously there are still many gaps and the extreme complexity of the mechanism of this form of discharge renders its use much of an art rather than as a science - both its phenomena and its limitations abound in the curious and the unexpected."

Aston's original apparatus, which was adapted from time to time to suit minor modifications of the technique was in continual use from 1919 till it was dismantled in 1925, after more than fifty elements had been analysed.

The results obtained by his first mass-spectrograph resulted in Aston's election to a Fellowship at Trinity College; in the same year, 1920, he was made secretary to the Cambridge Philosophical Society. Two years later in 1922 he was awarded the Nobel prize for his work in proving the existence of isotopes in elements that hitherto had been regarded as simple.
The most crucial and lasting effect produced by Aston's invention of the Mass Spectrograph was its fundamental importance in speedily establishing concrete elements of a testable atomic theory - an enormous step forward from former speculation.

This was quickly seen to provide in the mechanical sense a simple demonstration of how atoms and molecules combined in forming substances and compounds. The very fact that at last scientists were dealing with the physical characteristics of atoms, rather than interpreting their behaviour through chemical theory came first as a relief and then as a great impetus to further research.

It is remarkable to note that as early as 1920 Thomson foresaw and actually carried out experiments using electrical detection where the mass-spectrograph was an analytical tool to elucidate chemical structure. However, until the 1950's, mass-spectrometry was always perceived as belonging to the physicist's domain, and the concentration of work in mass-spectrometry from 1918 to 1945 was dominated by the physicist's concerns with atomic and electronic structure, isotopic constitution and abundance.

The main reason for this late development of analytical mass-spectrometry lay in the fact that enormous technical and experimental difficulties had to be overcome before the apparatus would even work, let alone produce reliable results.

As in many cases, cross-fertilisation of ideas and techniques brought about innovation and the establishment of new research areas. One can see in J.J. Thomson and even more clearly with Aston, how they both came to
experimental physics with a wide range of useful expertise and knowledge, Thomson through engineering and Aston through brewing technology and his private apparatus building.

On this point, it is difficult to appreciate fully the difference in experimentation facilities between scientists of the 1920's and those of today. Aston, for instance, in the construction of his first mass spectrograph fabricated the batteries to provide his high voltage completely by hand; blew all his glass, and virtually built the rest of the equipment from pieces of brass, copper and steel.

However, several commentators have noted that Aston, although a supreme experimenter, did not have a particularly good grasp of physical theory. It therefore seems right to allow Lindemann some credit for Aston's staggering achievements. Lindemann must have provided Aston as much through argument as well as advice with the theoretical speculations and conceptions necessary to formulate particular experiments.

Of course Aston, once it became clear that the apparatus he had designed and built would produce useful results, stuck to this area of physics and methodically explored all the main avenues. So great was his dominance in the field that other prospective researchers were deterred from participation in mass-spectrometry, although his activities did not greatly impinge on scientists working with Dempster-type apparatuses on isotopic abundance problems.*

* However, during the Second World War, it proved to be Dempster's methods, electrical detection, spark sources, introduction of double focussing and the 180 degree geometry, which proved to be the most suitable, and the techniques as developed by Aston, which were highly idiosyncratic, were given up.
Even so, in 1921 Aston found himself the target of a heavy attack on his methods and conclusions by J.J. Thomson. Thomson was always reluctant to believe in the general applicability of isotopic structure, preferring to consider the existence of isotopes only in relation to radioactive substances. Thomson, at the Royal Society meeting, delivered a violent polemic directed against the notions of integer atomic weights, the constancy of isotopic ratios and even Aston's experimental methods. This attack was indicative of the continued strained relationship between the two men, and Thomson must have felt that it was personally up to him to defend the status quo of the current corpus of scientific knowledge.

However, Aston's determination finally produced conclusive proof of the isotopic nature of neon and chlorine after building a more sensitive mass-spectrograph, conceptually similar but radically different in design from the earlier parabola instruments.

From a sociological standpoint, this chapter in the chequered history of mass-spectrometry, sheds a good deal of light on the theories already discussed in Chapter II. In terms of the normative behaviour of scientists - what we might term the 'steady state' theory of scientific advance - we can see in the attitude of Thomson to Aston something very much removed from the notion of open-mindedness, Thomson, with all the undoubted attributes of a great man of science, strongly resisted the attempts to alter his own particular scientific theories in this respect. He remained unconvinced about isotopes for some time after Aston, who appeared to subscribe to all the norms of pure science but was in fact an extremely good technologist, had provided what others regarded as conclusive proof.

* Galileo too, in his 'Dialogues concerning two new sciences', comments on this syndrome:

"... a strong desire to maintain old errors, rather than accept newly discovered truths. This desire at times induces them to unite against these truths, although at heart believing in them ... indeed, I have heard from our Academician many such fallacies held as true but easily refutable; some of these I have in mind".
Yet this 'establishment' attitude to young scientists and their novel theories, borne out by many people's experience, would seem almost to be the rule rather than the exception, and it would therefore appear that Merton's conceptualisation of the behaviour of scientists is an ideal towards which scientific practitioners only pay lip service. In other words, what Merton would regard as deviate is in fact a perfectly acceptable form of behaviour. Mulkay has similarly commented that it would appear more profitable to interpret the Mertonian ethos of science as an ideology, not as a rule book for everyday guidance.  

C.H. Waddington, a wartime physicist has commented:

"It (science) is an ethos based on the recognition that one belongs to a community, but a community which requires that one should do one's dammedest to pick holes in its beliefs. I know of no other resolution of the contradiction between freedom and order which is so successful in retaining the full values of both". 27

Kuhn's theory of periods of normal and revolutionary science far more closely follows this pattern of scientific advance, and provides a more satisfactory conceptualisation of the processes whereby new ideas are accepted into science.

As far as theories of serendipity are concerned, the history to the discovery of isotopes was much confused by the occurrence of reactions between gaseous ions and neutral molecules. Thomson was correct in thinking that the peak at \( m/Q = 3 \) in the hydrogen spectrum was due to \( H_2^+ \), but wrong in attributing the \( m/Q = 22 \) in Neon to \( NeH_2^+ \), and so through his genius and scientific prestige, the discovery of isotopes was delayed for five years.
The majority of isotopes had been identified by the end of the 1930's and Aston did much of the work himself. Serendipity was absent to such an extent that noone at all bothered to study ion-molecule
reactions for their own inherent interest until the middle of the 1950's.
One can similarly reject as significant serendipitous factors if we ask
the question, why did Thomson build the parabola mass-spectrometer? He
had already carried out similar work in discovering the electron, he was an expert in the use of high vacua, magnetic and electric fields, and it was well known that charged particles could be deflected. So positive rays was something he obviously had to try. Thus the discovery did not constitute any real breakthrough or innovation, far-reaching in its future consequences notwithstanding.

We have seen that in the early experiments the extra lines that Thomson found could either have been isotopes or ion-molecule reactions, (with hindsight we know that some were one kind, some the other). Thomson backed the latter, Aston backed the former, so there was no serendipity here - both pathways were followed up. In subsequent experiments to resolve this problem, Thomson found that an ion-molecule reaction was responsible for $\text{H}^+$; then the subject was dropped. In following up the isotopic route, Aston, who was regarded by his contemporaries as one of the luckiest of scientists, undertook a large number of methods of detecting isotopes, all of which produced equivocal results before he found a method which gave good results. Again there was no serendipity here; Aston tried every avenue, and eventually triumphed because he was a good apparatus builder. This must be seen therefore as a technological breakthrough, rather than an intellectual one. Anyone could have built a mass-spectrometer given good pumps and a steady high voltage. It is arguably the case that isotopes were discovered because of advances in technology, and this close link between advances in mass-spectrometry and related requisite technology is a basic aspect of its history, where the availablity of particular techniques largely determines the range of questions it is possible to ask.
CHAPTER IV

THE PIONEERS OF MASS SPECTROMETRY

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Chapter V takes a particular topic which was highly problematic in mass-spectrometry in the period 1912 to 1932. It traces the tortuous, erratic paths which the process of scientific discovery very often takes, and sheds light on the dilemmas and procedural tactics which scientists are faced with when fundamental innovations take place.

Chapters VI and VII examine the main research areas in which mass-spectrometry developed. They explore the nature and interrelationship of particular scientific and technological advances, and show how the technique disseminated from the original pioneer laboratories into many other scientific establishments in different countries. They also provide evidence of cross fertilisation of scientific ideas and techniques between mass-spectrometry and other scientific disciplines.

Chapter VIII looks at the way in which mass-spectrometry advanced during the War years, and examines its fundamental contribution to the harnessing of nuclear energy, culminating in the production of the nuclear fission bomb. It discusses the effect of secrecy in research and the suppression of publication of experimental data on the speed of scientific advance, and similarly explores the implications when the 'normal' pure science laboratory organisation of scientists is supplanted by goal-orientated deployment of manpower, heralding the rise of 'Big Science'.

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5.1 EARLY MASS DETERMINATIONS OF HYDROGEN

Hydrogen, the lightest of all the chemical elements, has played an important part in the research and development of the mass-spectrometer. It was invariably present in the normal discharge tube, whether from the water in the glass walls of the tube, or from other compounds such as the wax used in sealing the equipment. The effects of its atom and molecule were in fact the first phenomena observed in positive ray analysis. As Aston remarks, "Since, with beams of ordinary intensity, these two bodies are the only ones giving fluorescent effects with easily discernible visibility, they are, and likely to remain, of indispensable value in testing and developing apparatus for analysis."

Thomson obtained three parabolae from a sample of hydrogen, corresponding to m/e values of 1, 2 and 3, and attributed them respectively to hydrogen atoms, hydrogen molecules, and a new polymeric form of hydrogen H$_3^+$. Thomson discovered that gas rich in H$_3$ could be produced by bombarding certain substances, such as solid potassium hydroxide, KOH, with cathode rays.

The parabola at m/e = 3 could have been due to a multiply-charged ion of some impurity in the apparatus. However, Thomson reasoned that it was indeed due to H$_3^+$ and not C$_4^+$, the most likely offender, since many carbon compounds when analysed gave no C$_4^+$ and since, when a line, at m/e = 3 was observed, he also had an extremely large H$_2^+$ ion intensity.

This new polymeric form of hydrogen, H$_3^+$, Thomson considered to occur "only under certain conditions of pressure and current", but later changed his mind and suggested that it was "more stable than ozone", and could
combine with both oxygen and mercury under the influence of an electric discharge. He pointed out that it was not possible to reconcile the existence of this substance with the ordinary conception of valency if hydrogen is always regarded as monovalent.

In 1916, Dempster showed the hydrogen line at m/e = 3 was indeed H₃⁺, that it was not a stable species, and that it was formed only under conditions in which hydrogen was dissociated. He noted especially the variation with pressure in the proportion of H₃⁺ formed.

At the outset, from the most general considerations, and in spite of its fractional atomic weight, it appeared extremely improbable that hydrogen was a complex element — ie: made up of more than one isotope — for Aston's 'whole number rule' would imply a difference of at least 100 percent in mass for a possible heavier constituent.

When Aston demonstrated in 1920 that the mass of the hydrogen atom did in fact exceed a whole number by the amount assigned by chemists from chemical molecular weight determinations, all doubt seemed at an end and hydrogen was announced to be a simple element.

Aston originally deduced the masses of particles from the position of their lines on the photographic plate by relating them to known reference lines — lines given by particles of elements and compounds the relative masses of which were known at least to the order of accuracy aimed for. This method was simple and had the advantage of not requiring an accurate knowledge of the numerical values of the electric and magnetic fields. Later Aston used a method which used a 'coincidence' or 'bracketing' technique, in which one or other of the fields is altered. If two masses M (known) and M¹ (unknown) are to be compared, then the line caused by the unknown mass M¹ is brought to the identical position on the plate previously
occupied by the line due to M by adjustment of the strength of one of the fields. The first application of this method was in the measurement of the masses of hydrogen and helium. Obviously total coincidences such as described above could not be detected on the same spectrum photographically. Aston therefore added and then subtracted a small potential from one of the large potentials, so that two lines were obtained which closely bracketed the third. The hydrogen molecule line was found symmetrically bracketed by a pair of atomic lines showing within experimental error that the mass of the molecule was exactly double the mass of the atom.

When the same procedure was applied to the helium line and that of the hydrogen molecule, the asymmetrical results showed that the mass of He is less than twice that of \( H_2 \). In the same way He was compared with \( O^{++} \) and \( H_3 \).

<table>
<thead>
<tr>
<th>Line</th>
<th>Method</th>
<th>Mass assumed</th>
<th>Mass deduced</th>
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<tbody>
<tr>
<td>He</td>
<td>(Bracket)</td>
<td>( O^{++} = 8 )</td>
<td>3.994 - 3.996</td>
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<tr>
<td></td>
<td>(Direct)</td>
<td>( C^{++} = 6 )</td>
<td>4.005 - 4.010</td>
</tr>
<tr>
<td>( H_3 )</td>
<td>(Bracket)</td>
<td>( C^{++} = 6 )</td>
<td>3.025 - 3.027</td>
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<tr>
<td></td>
<td>(Direct)</td>
<td>( He = 4 )</td>
<td>3.021 - 3.030</td>
</tr>
<tr>
<td>( H_2 )</td>
<td>Bracket</td>
<td>( He = 4 )</td>
<td>2.012 - 2.018</td>
</tr>
</tbody>
</table>

The above figures led to the conclusion that hydrogen was a simple element and that its mass corresponded to the chemical atomic weight, and was definitely divergent from a whole number. In 1925, as previously mentioned, Costa repeated Aston's bracketing experiments with greater precision, and measured a value of 1.0079 for the weight of the hydrogen atom.

In the same year too, H. D. Smyth, and Hoggness and Lunn, following experiments concerning ionisation, correctly suggested that the triatomic modification of hydrogen, \( H_3 \), was formed in the discharge via the ion-
molecule reaction \( H_2 + H_2^+ \rightarrow H_3^+ + H \). It was the first ion-molecule reaction of this kind to be postulated.

Further determinations of the weight of the hydrogen atom were made by Aston in 1927 during his first survey of the packing fractions with his second mass-spectrograph, and together with Bainbridge's direct measurement of the doublet \( ^1 H_2 - ^2 He \) six years later in 1933, a figure of 1.00778 was arrived at which was identical with the best determinations of its chemical atomic weight. The packing fractions and atomic weights of carbon and nitrogen, both believed to be simple, also agreed exactly with the chemical data.

5.2 DEUTERIUM

For eight years, then, Aston's proof that hydrogen was monoisotopic was accepted, but this concordance was completely upset in 1929 by the unexpected discovery of the heavier isotopes of oxygen by Giauque and Johnson by a spectroscopic method involving the appearance of rotational lines which could only have come from an asymmetric molecule. It was soon proved that the abundance of these involved a change of scale of at least two units of packing fraction, and by a strange coincidence, similar heavier constituents of carbon and nitrogen were revealed in quantity enough to give the corresponding shift in their atomic weights.

At that time the chemical atomic weight of hydrogen was regarded as completely trustworthy, and one of the most accurately determined natural constants. It was given as \( 1.00777 \pm 0.00002 \) by Birge in 1930, who later with Menzel in 1931 pointed out from theoretical considerations that to bring the results of mass-spectroscopic measurements and those of chemists into accord, hydrogen must contain a heavier isotope, and predicted it to one part in 5000.
Aston himself has given an excellent account of the painstaking work by Harold Urey which led to the discovery of deuterium and for which the latter received the Nobel prize in 1934. In 1972, Harold Urey illuminated some of the mistakes and errors which occurred during this search for a heavy isotope while he was at Columbia University.

"Aston in England had determined the atomic weight of hydrogen relative to oxygen - of course it was Hydrogen 1 relative to Oxygen 16, but he didn't know that. Then Professor Lamb, a chemist at Harvard, determined the atomic weight of hydrogen relative to oxygen, but in this he was determining the ratio of the mixtures of isotopes of hydrogen relative to the mixture of oxygen isotopes, and didn't know it. Therefore if you have heavy isotopes of oxygen, you have to have a heavy isotope of hydrogen, which was predicted to be 1 in 5000. We felt we had to concentrate this in some way." 14

Urey, at this time, was unaware that the error in each of the estimates concerned was greater than the total discrepancy he was seeking to explain.

"Together with my colleague, George Murphy, we worked out a theory that indicated that there should be a difference in the vapour pressures of the hydrogens. Then Ferdinand Brickwedde in Washington distilled some hydrogen and got a concentrated residue. Using optical methods, we found by observation of the Balmer Spectrum the presence of an isotope of mass 2 to the extent of 1 part in 1000, where we expected to find 1 part in 200.

"The mistakes that were made along this route are interesting. First of all, one of the few mistakes that Aston ever made was that he was comparing the two light isotopes of H and O and thought he was comparing simple elements. In the second place the Harvard chemists purified their hydrogen by electrolysis of water, and they didn't know that this fractionated the light from the heavy isotope. The two effects cancelled each other out and the prediction of Birge and Menzel came out right. Then we
made a mistake: Brickwedde in Washington had just dismantled his electrolytic cells with which he made hydrogen and had thrown away all that beautiful water that had an increased concentration of deuterium in it. He put in nice fresh clean water, electrolysed it, and of course got hydrogen that was depleted in deuterium by about a factor of five. Then we distilled the hydrogen and concentrated it down from 25,000 to 1,000."

As Aston points out\textsuperscript{15} deuterium filled the gap in the natural doublets by which the isotopic weight of $^1H$ could be directly ascertained, and when this was done the error, which had had such fortunate results, was clearly revealed: "... the pretty paradox of the isotope discovered providing the means to remove that very discrepancy which seemed to point the way so clearly to its discovery."

5.3 BLEAKNEY

The work of Urey and colleagues which led to the discovery of deuterium was published as a letter in the Physical Review on 1st January 1932 and followed by two subsequent papers later in the year. On 1st February 1932 the editor of the Physical Review received a communication from Walker Bleakney at Princeton University.\textsuperscript{20} Bleakney had run samples of hydrogen in his mass-spectrometer at a number of different pressures and had plotted the ratio of the ion currents due to $^3H^+$ and $H_2^+$ against the total pressure. If the peak at m/e = 3 is due entirely to $H_3^+$ formed by the reaction $H_2 + H_2^+ \rightarrow H_3^+$, this technique should give a straight line through the origin. Bleakney's results, taken from a subsequent publication, are shown in the diagram.

Curves I and IV were obtained from ordinary commercial electrolytic hydrogen run under different field strengths. Curves III and V were obtained from samples of hydrogen supplied by Urey and were supposed to have been respectively enriched and impoverished in the heavier isotope.
BLEAKNEY'S EVIDENCE FOR THE EXISTENCE OF DEUTERIUM

$1/P$ plotted as a function of $P$.
 Curve III, BRILKREDDE'S CONCENTRATED SAMPLE,
 CURVES I + IV, ORDINARY COMMERCIAL ELECTROLYTIC
 HYDROGEN EACH TAKEN UNDER A DIFFERENT SET
 OF FIELDS,
 CURVE V, SAMPLE OF HYDROGEN ENRICHED IN
 $^2H$ BY DIFFUSION PROCESS.

[S.3]
All these curves made a positive intercept on the vertical axis, proving the existence of a stable species of \( m/e = 3 \) in the hydrogen. Since \( \text{H}_3 \) had been shown by Heitler and London in 1927 to be quantum-mechanically unstable, the only possibility was that the stable species corresponded to \( ^1\text{H}_2 \), or what is now known as hydrogen deuteride. Bleakney had thus independently proved the existence of a heavy isotope of hydrogen, but, since Urey had published first, Bleakney was not greatly honoured for his discovery. Study of the literature, however, reveals an intriguing possibility. The interval between Urey's original announcement and Bleakney's confirmation of the existence of a heavy isotope of hydrogen was only one month. This begs two obvious questions. Did Bleakney plan, modify apparatus, carry out, analyse and write up all the experiments listed in his paper in the brief interval between 1st January and 1st February 1932? Alternatively, had he obtained significant results before he analysed Urey's samples without realising their significance?

To investigate in greater detail the history behind this episode, more information than that contained in the published papers was required. Since the principal characters involved were still alive, it was possible to obtain first-hand information.

Immediately prior to Urey's discovery, Bleakney at Princeton had built a new 180° mass-spectrometer which could be completely baked out, and this instrument had much greater sensitivity than his previous ones.
In an interview with Walker Bleakney, he commented:

"It was just about that time I was continuing some further work on hydrogen, when Urey made his discovery of the deuterium isotope. Now when that happened of course it led to a lot of excitement. His first evidence for that isotope was not regarded by everybody as fool-proof—it was a faint companion line taken on a grating with which you had to take a long exposure. This brought out a lot of ghosts, and many spectroscopists said '...if you expose long enough you can get anything'."

(D.R.P.): "It depends whose side your're on."

"Yes, of course! Urey was very anxious to get confirmation, and so he brought some of his enriched sample down to Princeton and I ran it through my mass-spectrometer."

(D.R.P.): "Straight away? Did you have to modify your machine?"

"No, I was all set up perfectly."

(D.R.P.): "That was quite a coincidence."

"I was already studying some hydrogen for other purposes—I had tried to make an improved measurement of the ionisation potential of molecular hydrogen."

In an interview with John Hipple, Bleakney's first graduate student at Princeton who went there in 1934, he comments:

"My recollection is that Bleakney at this time wasn't working on the separation of isotopes, but on ionisation studies. There was this interesting way he had developed then of using the triatomic ion, which was in a secondary reaction. It required the formation of the atomic hydrogen ion with a neutral molecule to form the $H_3^+$ ion. He varied the pressure of the hydrogen that was put into this Mass-spectrometer tube and used the height of the peak of the molecular $H_2^+$ ion as a measure of the relative pressure in the tube. So then he could plot the height of the peak for the formation of the triatomic $H_3^+$ as a function of the pressure using $H_2^+$."

143
2.9 However, John P. Blewett, who went to Princeton in 1933 under
Harry-Smyth, in an interview recalled that Bleakney,
together with Philip T. Smith and Wallace Lozier were in fact looking
specifically for an isotope of hydrogen-deuterium, and built a mass-
spectrometer for that purpose. This machine was not very sensitive
for direct mass determinations, but otherwise had quite high sensitivity.
Unfortunately, it was not quite sensitive enough.

It is evident that at the time of Urey's discovery, Bleakney's
equipment was set up perfectly, he had worked out an elegant theory and
experimental method for distinguishing between various ion-molecule
reactions, and was engaged in measuring the formation of hydrogen ions.

In my interview with Bleakney, he further commented that:

"When you bombard molecular hydrogen gas and look at the
mass-spectrum, you always see some mass three's. It's true
the mass three was pretty weak, but it was there. Therefore,
how was I to distinguish between HD and H\textsuperscript{3+}? I said it would
show up because the H\textsuperscript{3+} is the result of a secondary collision,
and therefore its intensity should vary with the square of the
pressure, whereas the HD, if there is one, should vary linearly
with the pressure. So I put it through on that basis (the
enriched sample) and it was clear - it stood out like a sore
thumb - that there was an extra linear contribution.

"So had I had this knowledge (of deuterium) I could have looked
for the isotope in the first place, but I thought that even if
it was there it would be too weak, since I hadn't seen any
evidence. On the other hand I hadn't concentrated on this
point."

Bleakney then goes on to say:

"After doing Urey's sample I went back and did some ordinary
hydrogen and I found a little intercept there that indicated
an abundance of about one part in 35,000. Now, you may ask
why it was so weak, because we later came to know that it's
much more abundant than that. Well, the reason again is a
funny one. I was using electrolytic hydrogen (presumably for his previous experiments as well (DRP)), and it was later discovered that this impoverished it of deuterium."

(D.R.P.): So you were fighting against all odds?"

"Quite!"

5.4 TRITIUM

It must be very galling to realize two weeks too late that you have just let a Nobel prize slip through your fingers. One can well imagine Bleakney’s disappointment and subsequent resolution to be first past the winning post next time. To discover a hydrogen isotope of mass 3 would be more than adequate compensation for missing Deuterium.

His first experiment (sensitive to 1 in $10^5$) gave negative results, but Bleakney improved the sensitivity of his apparatus and ran a sample of almost pure deuterium. He plotted the ratio of the peak at m/e = 6 to that at m/e = 4 ($D_2^+$) versus pressure and m/e = 5:m/e = 4 ratio versus pressure. As mass 6 should correspond entirely to $D_3^+$ formed by a collision process, the line should pass through the origin. At mass 5, the ions would correspond to $HD_2^+$ (from an ion-molecule reaction involving protium impurity) plus any DT which would manifest itself by a positive intercept on the vertical axis.

The experimental results are shown in the diagram. Bleakney had apparently discovered Tritium and he calculated it was present in natural hydrogen in the proportion of about seven in $10^{10}$. His Princeton colleagues moreover, showed that it could be formed by bombarding deuterium with 50 to 80 kV deuterons from a discharge tube, and the proportion of tritium in deuterium treated in this way rose from one part in 200,000 to one part in 5000. Subsequent research in the same laboratory confirmed these
bleakney's evidence for the existence
of tritium
conclusions as did experiments on the bombardment of deuterated ammonium chloride, ammonium sulphate and phosphoric acid by fast deuterons in Cambridge. Tritium was therefore included in the first international table of isotopes in 1936.

It was obviously important to obtain sufficient quantities of tritium for precise mass determination and for chemical investigation of its properties, and at the instigation of Lord Rutherford 43.4 kilos of heavy water, resulting from the electrolysis in Norway of 13,000 tons of water, was further electrolysed down to 11c.c. The product was handed to Aston for analysis but no trace of DT could be found. Intercomparison with the Princeton sample was not possible since it had been lost, and attempts at Cambridge to reproduce the transmutation experiments also failed. Bleakney consequently repeated his experiments using even purer deuterium and obtained no evidence at all for the existence of tritium, throwing the whole situation into the melting pot once more.

Subsequently, Alvarez and Cornog showed that tritium indeed existed, but that it was radio-active and present in natural hydrogen in amounts much too small to be detected by a mass-spectrometer (about one part in 10^17). They themselves used a Geiger counter, and the full story of the discovery of tritium has been written by Eidinoff.

It is difficult after such a long interval to decide what ion of m/e = 5 could have led Bleakney astray. It must have been a volatile impurity formed in a discharge tube possibly from impurities present in the original deuterium sample. The most attractive hypothesis is that the peak was due to the collision-induced dissociation of carbon monoxide in the tube of the tube.

*This choice of Aston by Rutherford to act as an independent judge was a typically British thing to do, for Aston had been to some extent left behind by developments in the USA, and Aston was not so highly thought of by this time.
The apparent mass of the collision-induced peak is given by $M^2/M_1$, where $M_1$ and $M_2$ are the m/e values for the initial and product ions respectively. The peak for this process, therefore, should occur at an apparent mass of 5.14. It would, however, be a broad peak, and recent work by Moran has suggested that it might be shifted to slightly less than the predicted mass under certain conditions because of the translational-internal energy conversion required to dissociate the molecule.

Bleakney remarks about this particular sequence of events:

"We thought that there might be another isotope of hydrogen, and at one time considered we had found it; not in natural hydrogen, but in the products in a discharge set up by Harnwell, Smyth and others. They took samples out and we analysed them in our Mass-Spectrometer, and again using the study as a function of pressure, tried to find a linear component of mass 4 or higher. We thought we found a positive result, and that apparently was a spurious effect. We published that, to our regret. After we realised that it must be wrong, we tried to do further experiments to understand why. We thought that what we found was that there must have been a hydrocarbon gas out of which you could knock something off as a unit, and therefore it would go directly with the pressure, something of mass higher than 4. That seemed to be the explanation, but it was a very, very faint thing and so it was pretty hard to pin down." 38

3.6 It is illuminating to read Aston's remarks concerning the developments over the discovery of the isotopic constitution of hydrogen, and relating these to the scientific world in general, finding itself yet again in a state of flux.
"Confidence in accuracy of measurement and the inviolability of natural constants, which seemed so firmly founded by a century of science, has suffered many shocks in recent years, of which few have been so disturbing as the discovery of heavy hydrogen and its subsequent revelations. Those in respect to the isotopic weight of 'protium' (the lighter isotope, a name used on those comparatively rare occasions when the distinction between it and normal hydrogen might be necessary) were startling at the time, but the still more recent findings in connection with the atomic weight of hydrogen have a much deeper significance. That the value set by a single observer (Aston) in a preliminary survey should be out by three or four times his estimated error, even after standing for eight years, was nothing new, but that the accepted figure for an important natural constant, supported by the innumerable and apparently convergent observations of the best chemists and physicists in the world; for over half a century, should be found in error by over ten times its estimated uncertainty was indeed unexpected. Yet the incredible has happened and the familiar figure, founded on Morley's classical work and trusted for so long, has to go into oblivion, to be replaced by an approximate value based exclusively on mass-spectrum measurements."
CHAPTER V

THE ISOTOPES OF HYDROGEN

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CHAPTER VI

HIGH RESOLUTION MASS-SPECTROMETRY

6.1 EARLY PRECISION INSTRUMENTS

The 'whole number rule', established by Aston with his first Mass-Spectrograph, was never supposed to be mathematically exact. Although the deviation was only measurable with any accuracy in the single case of Hydrogen, it was soon clear that the masses of the Lithium isotopes 6 and 7 exceeded whole numbers by at least 1 part in 1000, the mass of Carbon being taken as exactly 12.

The first attempt to measure these deviations to an accuracy greater than 1 in 1000 was made by Costa, who in 1925 published results obtained on a Mass-Spectrograph set up in Paris. The instrument was identical in principle with Aston's, but included refinements which enabled Costa to claim an accuracy of 1 in 3000 for comparisons of mass. With this apparatus he compared, by the method of bracketing, Helium with Hydrogen, Helium with Carbon, Helium with Lithium 6 and Nitrogen with Lithium 7. By use of a moveable anode he overcame the serious experimental difficulties of obtaining the ions of Lithium (a solid) and of Nitrogen (a gas) in the same discharge tube. It was realised that the accurate determination of the divergencies from the 'whole number rule' was of fundamental importance. Having added up 4Hs and getting an aggregate mass of more than one He, Aston recognised that this was one of the few avenues by which nuclear structure could be explored.

Aston had been considering improvements to the design of his first mass-spectrograph as early as 1921, and in 1925 the improved apparatus, his second mass-spectrograph, was set up in the Cavendish Laboratory.
$S_1$ and $S_2$ are collimating slits, $D$ is the discharge tube, $C$ is the cathode, $J_1$ and $J_2$ are the electrostatic plates, $M$ is the Magnet pole, $W$ is the photographic plate, $I_1$, $I_2$, $V$ and $P$ are inlets and controls for the vacuum system.
Every effort was made to push the precision of analysis to its practical limit, and Aston finally decided that this increase of resolving power could best be obtained by doubling the angles of electric and magnetic deflection, and using finer slits placed further apart.

This instrument, with slight modifications, was used during the following ten years. It had five times the resolving power of the first mass-spectrograph, and its accuracy of measurement approached 1 in 10,000, which was just sufficient to give rough first order values to the divergencies from whole numbers. (See Technical Note 1 at the end of this chapter).

When the apparatus was set up for preliminary tests an anomaly was noticed which caused some misgiving. The curvature of the lines (i.e., straight slits give curved images, due to a caustic of rays) observed in the original apparatus but never satisfactorily explained, now appeared enormously exaggerated. This effect was found to be associated with an entirely unexpected and very serious difficulty, that of polarisation of the surface of the electrostatic plates, which after several different unsuccessful attempts to eliminate it, finally had to be taken into account. Experiments showed, however, that the polarisation effect remained constant as long as the conditions of discharge remained unaltered.

*As K. T. Bainbridge comments during discussion at a Conference on Nuclidic Masses in 1960:

"Polarisation is a question which arose thirty years ago with Aston's mass-spectrograph, and the answer to it, I think, partly explains the increase in size of modern instruments. If the ions go between the plates of the condenser, and there is some charging up, as Aston observed, one way of reducing this is to cut down the current density of the ions hitting a particular spot on the plate. Aston covered his plates with gold. His plates were only a millimeter or so apart, and the deflecting voltage quite low. In any electrostatic analyser, in order to bend the beam, you must have a certain ratio of deflecting potential to the potential of the particle. Thus one way of beating..."
the problem is to open up the condenser so you require a larger deflection voltage, in which case a small voltage added to the plates by polarisation is proportionally less. Another thing is to improve the vacuum technique. The third thing is to get rid of oil pumps, in favour of mercury, because then you minimise the chance of an insulating layer forming. Another thing is to build an ion source in which the spread in energy is such that nothing hits the plates. This is the present situation with spectrometers being built all over the world."

And A.D.C. Nier also comments:

"I'd like to add a concluding remark to Ken's observations. The Hydrogen test that we make is perhaps the best test of this very effect. When we started ten years ago, we entered the field not expecting to play seriously in it, because we thought that the techniques had gone so far that probably we couldn't make any improvement. But as we developed the electrical method, the Hydrogen test came out closer and closer to the Hydrogen unit. We use this as a test for something going wrong in the electrostatic analyser plates. We can tell if the plates develop a film (of polarisation), for then we no longer get the right hydrogen mass-unit."

The first important piece of work performed with the second mass-spectrograph, and the one for which it was primarily designed, was the measurement of divergencies from the 'whole number rule'.

Oxygen at the time was considered to be a simple element (one with no isotopes), and so $^16\text{O}$ was chosen by Aston as a suitable standard for measurement. The percentage deviations of the masses of other atoms from whole numbers on the scale $^16\text{O} = 16$, were expressed in parts per 10,000 and called 'Packing Fractions', since they were thought to be a measure of the closeness of packing of the electrical particles forming the nuclei of the atoms concerned.

In 1927, when about twenty elements had yielded results, Aston plotted 'packing fraction' against mass number, and obtained the historic 'Packing Fraction Curve'. Starting with a very high positive value for Hydrogen,
Aston's Original Packing Fraction Curve (1927)
the curve drops steeply through Lithium and Boron, and crosses the zero line at Oxygen. It descends to a minimum in the region of Iron and Nickel, and then rises gradually to cross the zero line again near Mercury. Helium, Carbon and Oxygen lie below the curve, a fact which was in good agreement with Rutherford's observations of the extreme stability of these elements when under bombardment with alpha particles.

The most important implication of the packing fraction curve was that, together with Rutherford's work, it underlined the importance of nuclear binding forces and showed that certain nuclei were much more stable than others.

Knowledge of the Packing Fraction can lead to the establishment of unambiguous molecular formulae. From a list of the molecular formulae containing Carbon, Hydrogen, Nitrogen and Oxygen with a nominal molecular weight of 44, the mass-spectrograph will distinguish:

<table>
<thead>
<tr>
<th>Formula</th>
<th>Mass</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>CO₂</td>
<td>43.989830</td>
<td>Carbon Dioxide</td>
</tr>
<tr>
<td>N₂O</td>
<td>44.001063</td>
<td>Nitrous Oxide</td>
</tr>
<tr>
<td>CH₂NO</td>
<td>44.013639</td>
<td>found only as fragment ion</td>
</tr>
<tr>
<td>C₂H₄O</td>
<td>44.026215</td>
<td>Ethylene Oxide or acetaldehyde</td>
</tr>
<tr>
<td>CH₄N₂</td>
<td>44.037448</td>
<td>found only as fragment ion</td>
</tr>
<tr>
<td>C₂H₆N</td>
<td>44.050024</td>
<td>found only as fragment ion</td>
</tr>
<tr>
<td>C₃H₈</td>
<td>44.062600</td>
<td>Propane</td>
</tr>
</tbody>
</table>

Note that the high positive packing fraction means that the packing fractions are in approximately the same order as the hydrogen contents.

With the advantage of hindsight, it is interesting that the packing fraction curve depicts the release of energy to be expected from transmutation of nuclei, not only by the aggregation of light atoms to form
heavier ones, but also from the fission of uranium by neutron bombardment, a phenomenon undreamed when the curve was first drawn.

During the period 1930-35, Kenneth T. Bainbridge, the first man to challenge the precision of Aston's mass measurements, built his first mass-spectrograph in the laboratory of the Bartol Research Foundation of the Franklin Institute, and a preliminary description of it by Swann appeared in 1930.

It was originally intended to work on the same principle as Dempster's mass-spectrograph, and was to be used to extend his work to the heavier elements. As this required much increased resolution, and also had to bend the heavier particles into semi-circles, its main feature was an extremely large electro-magnet, which gave a uniform field up to 15,000 gauss.

To extend the instrument's use to ions produced by ordinary discharges, Bainbridge applied a new and original principle where the stream of rays to be subjected to semi-circular focussing was not monoenergetic, as was Dempster's method, but homogenous in velocity.

The beam of ions produced by discharge was collimated through slits, and then passed through a 'velocity selector'. The 'Wien filter' was invested by W. Wien, a professor at Munich and Nobel prize winner (1911) who studied the deflection of positive rays in electric and magnetic fields.

Bainbridge was the first to photograph the doublet formed by the doubly charged line of helium and the hydrogen molecule, and by it obtain a direct measurement of the hydrogen-helium mass-ratio. He was also the
The comment concerning the 'completeness' of science made by the scientist in the 1890's was destined to be repeated. In 1930, concerning fundamental aspects of science, one eminent physicist remarked, "... when one knew the mathematical laws governing the behaviour of the protons, electrons, photons and gravitation, one would know everything there was to know of the physical world, and physics in principle will be a subject which has reached its destination." But, as has happened before in physics, a flood of new discoveries came to upset their simple schemes. One of these floods was the discovery of the neutron by Chadwick in 1932, the positron by Anderson in 1931, the \( \mu \) meson by Anderson and Neddermeyer as predicted by Yukawa, and subsequently a whole host of other elementary particles.
Scheme of Baird's 1st Mass Spectrograph

S1, S2, S3 are collimating slits, P1 & P2 are the plates of the velocity selector, M is the magnetic field, P is the photographic plate. (1930)
first to measure the isotopic weight of Deuterium — heavy hydrogen — discovered in 1932. With this apparatus, Bainbridge successfully obtained the mass lines of light metallic elements, at the same time as standard comparison lines, which was a matter of great technical difficulty. He established accurate comparisons of the masses of light particles involved in nuclear disintegration, and in 1933 demonstrated experimentally Einstein's theory of the equivalence of mass and energy, advanced in 1905.

The next stage in the precise determination of nuclidic mass was the discovery by Cockcroft and Walton in 1932 of the artificial disintegration of lithium by protons. For the first time physicists had a nuclear transformation for which they could measure the energy set free in a nuclear process and compare it with the change of mass. It was at once apparent, however, that although the directly observed energy change could be measured with comparative accuracy, the uncertainties in the masses involved (hydrogen, helium and lithium) amounted at that time to forty per cent. of the change in mass resulting from the transformation. To have the mass differences match the energy measurements in accuracy, it would be necessary to determine atomic masses to four or five decimal places, a precision approaching that of optical spectroscopy, and a hundred times greater than that previously attained. This provided a great stimulus to high resolution mass-spectrometry and the decade, 1932-42, saw great progress in the precise measurement of mass. (See also Technical Note 2 at the end of this chapter).

Fortunately during this period the discovery of deuterium supplied the one link required to enable the isotopic weights of light atoms to be compared with the oxygen standard by direct measurements of a series of natural doublets \( H_2 - D \), \( D_3 - C^+ \), and \( CH_4 - O \).
In order to measure the isotopic weights of atoms with sufficient accuracy to determine the small mass differences resulting from transmutation experiments, more sophisticated apparatus was needed with, as Bainbridge indicated, a resolving power of at least 2000.

One instrument which achieved this was the third Mass-Spectrograph built by Aston in 1937. This embodied no new principle - in fact, it was merely a further modification of his original design - and he achieved the desired increased in accuracy and resolving power by improving the collimation and focussing of the rays.

For the first time the beam-defining slits were adjustable, and their width could be varied from 0.05 mm to zero. As the slits were narrowed, the line width would decrease. The gains from slit narrowing had to level off eventually, even with theoretically perfect focussing, but unfortunately for Aston this happened earlier than foreseen, and was probably due to the intense polarisation of the metal surfaces under the action of the beam - a serious problem which Aston had already encountered in his second apparatus. This breakdown of the focussing constituted the major disappointment in the performance of the apparatus.

However, the resolving power was improved enough for him to make a preliminary measurement of the $H_2 - D$ doublet.

The next stage in the modification was the reconstruction of the main part of the instrument with dimensions such that second order focussing could be achieved. Aston and Fowler in 1922 had already discussed this theoretically, and the second mass-spectrograph (1927) was modified to incorporate the improvement. This problem was again discussed and worked
out theoretically by Sawyer in 1936, who showed that an electric
deflection of a quarter of a radian in a radial field (r = 40 cm)
followed by a magnetic deflection by a uniform field between sickle
shaped sections of a circle (r = 15 cm) would be favourable\textsuperscript{20}. While
not coming up to Aston's expectations this modified instrument achieved
accuracy approaching 1 in 100,000 and Aston continued to make even more
precise measurements of isotopic weights.

In all the mass-spectrographs which had been built until 1935 the ion
beam was made homogenous in some respect before the focussing device was
applied. In the Dempster-type mass-spectrograph the ion beam is of
constant energy and is deflected or focussed geometrically in a uniform
magnetic field. In Aston's mass-spectrographs, the beam is first rendered
homogenous geometrically by the use of a collimating device of two narrow
slits, and then deflected and focussed electromagnetically. In Bainbridge's
system, and that of Smythe and Mattauch a particular velocity is selected
and then deflected and focussed geometrically in the first case by a
uniform magnetic field, in the second by a radial electric field. This
information had hitherto been obtained with instruments which were either
velocity focussing or direction focussing, with some special provision for
rendering unnecessary whichever of these two focussing properties was
missing.

As early as 1929 the idea of a double-focussing instrument which
would incorporate both properties had been suggested by Bartky and Dempster\textsuperscript{21},
together with an analysis showing that a deflection of $\pi/\sqrt{2}$ radians in
crossed electric and magnetic fields would accomplish this result.
However, as this arrangement would only transmit a limited portion of the mass-spectrum, it was not exploited. As is so often the case in research, this principle of 'double-focussing' received the attention of several investigators simultaneously and independently.

The double-focussing principle, as applied to actual instruments, was a consequence of a better understanding of the focussing properties of magnetic and electric fields. Among the chief contributors to this understanding of ion optics were:

Hughes and Rojansky (1929) Barber (1933) Henneberg (1934)
Stephens (1934) Bruche and Schertier (1934) Smythe (1934)
Hertzog and Mattauch (1934) and Hertzog (1935)

Hertzog gave finality to these studies by deriving a complete theory of all arrangements of a radial electric field and a homogenous magnetic field for which both direction and velocity focussing could be obtained. A feature of this important work of Hertzog, which was undertaken at Mattauch's suggestion, was his demonstration that the focussing conditions could be expressed in familiar geometrical optics form.

With knowledge of this kind, it was possible to link electric with magnetic fields to form double-focussing combinations in which the velocity dispersion by the former was counteracted by the latter, while at the same time direction focussing was maintained through the pair.

At the time these papers appeared, two mass-spectrographs of entirely different design, but both making use of the principle, were in the course of construction, by Dempster in Chicago, and by Bainbridge and Jordan in Harvard.
In 1935 Dempster was the first to publish the description of a new double-focussing machine. The double-focussing was achieved by the use of a cylindrical electrostatic field deflecting through 90°, for direction focussing, followed by deflection through a further 180° by a magnetic field, for velocity focussing. For one particular radius, the beam will come to a sharp focus on the photographic plate.

In this particular form of double-focussing a perfect focus is obtained at one point only on the plate, and becomes less sharp as the distance from this increases. Dempster points out that this limitation is of minor importance, because the apparatus was designed for the measurement of close doublets and analysis of the heavier elements, where the range of mass of the isotopes is small. At the same time, Dempster introduced an entirely new ion-source, the vacuum spark source. Three types of spark were tried: the "Trembleur dans le Vide" or vacuum vibrator (Fabry and Perot, 1900); the 'hot sparks' from a large condenser discharge (Millikan, Sawyer and Bowen, 1921); and the 'Tesla' spark. The second of these was developed at Chicago in the early 1920s by Millikan, using the 'hot spark' light source for the enhancement of spectral lines in the far ultraviolet region. These lines arise from highly ionised, or 'stripped' atoms, and Dempster was interested in the deflection of such multiply-charged ions. Dempster's source development paralleled Millikan's, and he finally developed a system using a spark coupled inductively to a high-frequency oscillating circuit - a 'Tesla' spark. This high frequency spark proved successful, and yielded an abundance of ions of the elements comparing the spark electrodes, which permitted for the first time isotopic analyses of the four elements palladium, iridium, platinum and gold, which still remained to be analysed. The peculiar chemical properties of these
DEMPSTERS PACKING FRACTION CURVE (1938)

6.3
elements had so far defeated all attempts to obtain their mass-spectra. In addition to these important results a number of rare isotopes of other elements, particularly among the rare earths, were discovered. A valuable contribution made by Dempster in 1938 was an extension of the 'Packing Fraction Curve' to the heavy elements, (Aston's 'Packing Fraction Curve' of 1927 was made largely with results for light elements) which showed that the original one was too far in the region of the heavy elements. It was characteristic of Dempster that with this instrument, completed in 1935, he exhaustively explored the isotopic constitution of the elements, and was working with it at the time of his death. Others took up the new principles which he had helped to introduce, and constructed more elaborate instruments, but he kept to his 1935 machine and extracted from it every bit of information which time allowed him.

6.4 BAINBRIDGE AND JORDAN'S DOUBLE-FOCUSSING MASS-SPECTROGRAPH (1936)

This instrument was designed by Bainbridge while he was working at the Cavendish Laboratory at Cambridge in 1934, and completed at Harvard University in 1936.

Double-focussing is achieved by a linear combination of a \( \frac{1}{\sqrt{2}} \) electrostatic analyser and a 60° sector magnetic field (this is the first use of a sector magnetic field). A beam of 20,000 volt ions, generated in a cylindrical discharge tube passed through two defining slits \( S_1 \) and \( S_2 \) and through the deflecting plates, on which a large potential of about 2,400 volts derived from radio batteries was present. Bainbridge considered this high voltage necessary to reduce the danger of polarising effects. The beam then passed through another defining slit \( S_3 \) through the magnetic analyser and came to a focus on the photographic plate.
One advantage of the Bainbridge-Jordan instrument over the Dempster double-focussing instrument was that the former gave twice the dispersion while requiring only one third as much field area. Another advantage was that the mass-scale, over the 140 mm spectrum, was very nearly linear, whereas in the Dempster instrument the mass-scale varied as $\sqrt{m/e}$.

Some of the most accurate measurements of the masses of the light elements were made by Bainbridge and Jordan with this instrument: masses differing by as little as 1 part in 10,000 were completely separated. The lithium mass was remeasured, and the decrease in mass in the transformation by protons was found to agree accurately with the value computed from the mass-energy formula.

One of the objects of this machine was to search for rare and doubtful isotopes, particularly isobaric pairs. Bainbridge found that some of the lines in the mass-spectra of Cd, Sn, Hg, and Pb, previously thought to be isotopes, were spurious, but at least three isobaric pairs of odd mass numbers were confirmed beyond doubt.

In 1936, immediately after Bainbridge and Jordan's paper appeared in the 'Physical Review', K. Ogata and H. Matsuda, under Professor T. Asada at Osaka University designed a similar double-focussing mass-spectrograph. By 1939 the construction had been completed and mass-determinations started. This work continued up to the beginning of 1943 by which time the masses of over 31 isotopes had been obtained. The war intervened, and consequently it was not until 1947 that reconstruction and improvement of the apparatus started, and research commenced in 1950.

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*Isobars are elements with the same atomic weights, but different chemical properties (isotopes have the same chemical properties but different atomic weights). Any product due to the loss of a beta ray, or electron, must be an isobar of its parent substance, for, without change of mass, it has moved in the periodic table and so changed its chemical properties.
The vacuum system of the apparatus was never, as Ogata remarks, very good, so that together with rather wide slit widths the resulting experimental resolving power was about 7,000 though its calculated resolving power was about 17,000. (Bainbridge obtained a resolving power of approximately 10,000 in 1936.)

6.5 MATTAUCH AND HERTZOG'S DOUBLE-FOCUSING MASS-SPECTROGRAPH (1936)

The two types of double-focussing mass-spectrographs - ie: the Dempster and the Bainbridge-Jordan - gave theoretically perfect focussing for one mass only, but this remarkable instrument, constructed at the Institute in Vienna, in accordance with the theory of Mattauch and Hertzog already mentioned, was unique in that it gave theoretically perfect double-focussing along the whole length of the plate. The plate was nearly 300 mms long and masses ranging from 1 to 10 could be recorded simultaneously over three octaves of mass, an enormous range compared with that of other instruments. The dispersion and line widths varied directly as \( \sqrt{m/e} \).

In a review paper given to introduce the 3rd Mass-Spectrometry Conference in Paris 1964, Mattauch discusses some of the steps which led finally to his double-focussing mass-spectrograph of 1936:

"In the thirties I was lucky to have gathered in Vienna an enthusiastic group of students who were eager to learn something of the new focussing methods by experiment. I had already set up a new Smythe filter in combination with a \( \sqrt{m/e} \) electrostatic analyser and R. Hertzog was helping me to take mass measurements with it. Since it was desirable to know the energy distribution of our ions, I asked Miss Goyer to measure it in another set-up in which the velocity filter was omitted. She was baffled to find in 1932 the following effect: without change of intensity the whole energy distribution curve shifted depending
on whether the inner or outer plate of the cylindrical condenser was connected to earth. We soon found that this was equivalent to displacing the entrance slit along the field boundary, which meant that at the field edge the ions would have to surmount a jump of potential, a fact not realised by Aston when in his old paper he gave examples of electrostatic lenses, because if you consider a beam spread out over the range $-8$ when entering the field it will become inhomogenous in $mv^2$ after passing through. This is the exact reason why the action of an electrostatic field can be set in analogy to that of an optical lens.

In contrast to this the similar focussing action of a magnetic sector field on a beam of ions homogenous in $mv$ is a purely geometrical affair of circles with the same radius.

Of course our Viennese group was not alone in the world. Similar ideas had cropped up and various parts of the general focussing problem had been solved in 1933-34. But I think I can safely say that the most general solution was given by Hertzog.

In the following two years Dempster and Bainbridge produced double-focussing mass-spectrographs which were special cases of our theory, in which the intermediate image is a real one, and the condition for double-focussing can be satisfied for only on value for the radius of deflection in the magnetic field. However, a double-focussing mass-spectrograph worthy of its name should form such images for the entire mass range of the plate. By placing the entrance slit in the focal point of the electrostatic field, pushing thereby the intermediate image to infinity, this gives an infinite series of field arrangements, and the plate is placed at the locus of the focal points for all ions of varying mass.

In our first instrument we chose what seemed the most simple case of this series of field arrangements."
Schematic Diagram of Maltzach's Double Focusing Mass Spectrograph (1936)
In addition to precision mass measurements, Mattauch turned his double-focussing instrument to a variety of important researches. He investigated numerous molecular dissociation bands, and by photometry investigated the isotopic abundances of many elements including molybdenum, europium and lutecium, in which he discovered the new isotope 176.

The principal advantage of this type of mass-spectrograph, of course, lies in the fact that a large portion of the mass-spectrum can be photographed at once, since sharply focussed lines are obtained throughout the entire length of the plate.

Other original scale versions of this instrument have been built by Mattauch and Lichtblau, Ewald, Mattauch and Biers, and Everling et al.

A much larger version of this apparatus has been built by Bainbridge at Harvard (approximately eight times the original, with electrical detection, and has achieved a resolving power in excess of 100,000 at the base of the peaks.

6.6. SECOND ORDER FOCUSING

The next step forward in the development of mass-spectroscopes for precise mass measurement was the incorporation of higher order focusing.

In the double-focussing instruments described so far, the second order image aberration had been neglected, because in the derivation of the theory of focussing of ions by electric or magnetic fields, it was generally assumed that the rays from a point object, after passing through analysers, were brought to a point focus, but this was true only to a first order approximation.
The first double-focussing mass-spectrometer with second order correction was built by Nier and Roberts in 1951 using a two field combination in accordance with the theory of Johnson and Nier, and thus achieved second order focusing. This complex instrument was designed for precise mass-determinations. The Nier-Roberts machine also employed for the first time in precise mass measurement an electrical detection system with an electron multiplier as the ion-detector. The different masses were brought to a focus by varying the electrostatic deflection voltage. Since the two peaks of a doublet were recorded at different instants, the fields in the instrument had to be highly stable, and Nier achieved this by placing an auxiliary mass-spectrometer tube in the field of the magnetic analyser. Ions of nearly the same mass as those under study were detected, and a differential amplifier measured and amplified the difference in signals, which was fed as an error signal into the main power supply and thus automatically compensated for any fluctuation in the voltages.

The Nier-Johnson geometry is now widely employed in commercial instruments for mass-measurement. Nier's apparatus was also the first of its kind to make precise mass-measurement possible by the peak-matching technique.

In discussing his new machine at the N.B.S. symposium in 1951, Nier had this to say about the move to electrical methods:

"Several years ago we at Minnesota became interested in the possibility of measuring atomic masses using a mass-spectrometer rather than a mass-spectrograph. It was quite natural that we should be thinking along this line since all of the work which my associates and I in the past have done (abundance measurements) has been with electrical detection. While we hoped that our instrument could compete with conventional mass-
spectrographs, we always reconciled ourselves that if it could not measure masses to the highest precision, it would at least be valuable to have an instrument of high resolving power in the lab.

The machine which we designed and constructed worked as well as we secretly hoped it would - better than we would openly admit."

It should be noted that the development of deflexion type instruments by Dempster, Bleakney, Mattauch and Nier involved first or second order focussing, which was still only approximate.

6.7 TIME OF FLIGHT INSTRUMENTS

In 1938, however, Bleakney and Hippie demonstrated a method of attaining perfect focus, as well as of producing a spatial mass-dispersion. This was to inject the positive ions from the ion source into crossed homogeneous magnetic and electrostatic fields. The magnetic field deflects the ions through $360^\circ$, and the electric field gives the ions a linear transverse movement. The combination of circular and linear movement results in a trochoidal path.

Experimentally, Bleakney and Hippie showed that not all trochoidal paths are desirable: for example, a curtate cycloidal orbit with a sharp cusp caused 'space-charge' defocussing', and they found that prolate orbits were the most satisfactory.

Following from this small-scale prototype model, several larger instruments were built. The Hippie and Sommer instrument, built at the National Bureau of Standards in Washington, was extremely large, and used a trochoidal path of five cycles, thus giving a source-to-collector distance of approximately two metres. This machine, almost at completion as a magnetic deflexion instrument, was converted to a 'time-of-flight' instrument using radio frequency techniques.
SCHEMATIC DIAGRAM OF THE BLEAKNEY-
HIPPLE TIME OF FLIGHT MASS- SPECTROMETER
67.
1938
We now come to the development of high-resolution mass-spectrometers involving the use of the cyclotron principle. These are part of the generic family of 'time-of-flight' instruments of which many types have been built (including the pioneering Smythe-Mattauch instrument, described earlier) 'pulsed beam' - Stephens$^{58}$ 1946, Cameron and Eggers$^{59}$ 1948, Katzenstein & Friedland 1955, Wiley and McLaren 1955, 'energy gain' - Bennett$^{60}$ 1948/50/53, Redhead$^{61}$ 1952, 'ion bunching' - Wilson 1952 and Glenn 1952, but we are only concerned here with those used for precise nuclidic mass measurement.

The common characteristic of all cyclotron-type instruments is that the ions describe circular orbits, where the period of rotation is independent of the ion velocity, but depends linearly on the mass. Coupled (as with the trochoidal M-S) with a linear component the ions describe helices or spirals, and can be made to circulate a large number of times. The mass of the ions can be measured either by time of arrival of the ions at the detector or by the frequency of their rotation.

The first instrument of this kind, first proposed by Goudsmit in 1948 (later named 'chronotron') and subsequently constructed by Hays, Richards and Goudsmit at Brookhaven National Laboratory in 1957, incorporated the 'time-of-arrival' principle. The magnetic field was produced by a spherical air-core magnet which produced a low-intensity field, and the necessarily low-energy ions were strongly influenced by polarisation on the walls of the vacuum chamber. Goudsmit used a pulsed ion source (with pulses up to 0.25 millionth of a second), and the ions after travelling several revolutions, were detected. The pulses were
displayed on an oscilloscope, and the time interval measured. Masses could be measured with a precision of about 1 millimass unit, and because of the linear mass-time relationship, the resolving power is virtually independent of the mass of the ion and therefore constant resolution at all masses could be achieved.

The balance of the theoretical advantages versus the practical disadvantages of this machine was summed up in Goudsmit's 1951 paper:

"The present machine will therefore be used only to obtain a few mass values of immediate interest; in view of improvements to be expected in the future, we do not plan any extensive programme of mass measurements. It is felt that the present investigation has shown that time of flight methods offer particular advantages in precision measurements of heavy masses." 67

One instrument taking the principle of cyclotron resonance a stage further was the 'Omegatron' of 1949 built at the National Bureau of Standards by Hipple, Sommer and Thomas. It was essentially nothing but a small cyclotron adapted to sharpen mass-discrimination, and they used this to make an important preliminary determination of the H₂/D mass difference. The ions travelled at least 5000 revolutions, which resulted in very sharp mass discrimination, and Hipple achieved a resolution of \( \sim 1/10,000 \) for 1 a.m.u.

Sophistication in instrumentation for precise mass measurement ends with the series of radio frequency mass synchronizers designed and built by L.G. Smith, and the latest now represents the ultimate in resolving power.
The first instrument of 1957, built at the Brookhaven National Laboratory, was partially a result of Smith's association with the helical path device invented by Goudsmit. It was also a result of a conference held at the newly-founded Brookhaven National Laboratory in 1948 to inaugurate mass measurement there by mass-spectrometer methods.

At this conference the virtues of the crossed field instrument were propounded by Bleakney, Thomas and Hippie, and it was the consensus that:

1. Mass measurement work at Brookhaven was desirable,
2. a large crossed field spectrometer offered the best possibility of attaining the desired precision of at least 1 part in $10^6$; but,
3. because of the high cost of such an instrument, a less costly instrument should be developed.

Smith, following on from this, combined Goudsmit's chronotron principle with a crossed electric field of small extent in space which varied in time. In it the ions, while constrained by a uniform magnetic field to follow circular paths, are exposed in the 'pulser' to a local modulating field, which performs both the functions of forming pulses and deflecting them.

In the first model of the synchronometer, with the first pulse of $\frac{1}{\mu}$ sec in duration, a group of ions in the neighbourhood of mass 28 was decelerated sufficiently to avoid the ion source house and experience a free circulation in orbit 2. After these ions had made a number of such revolutions, a second pulse decelerated the group still further, with the result that they reached the collector a half cycle later along orbit 3.
With this arrangement, ions of mass 28 accelerated to an energy of 250 volts were observed after performing 90 revolutions (72 meters). This gave a resolving power of 1/24,000, but the intensity had reached a very low level.

Smith found that the difficulty of achieving adequate intensity represented a fundamental drawback of time of flight spectrometers like his synchrometer and the earlier chronotron. This was because the fraction of time when ions are being collected was extremely low (≈ 0.01% or less) as compared with conventional mass spectrometers and also because many ions were lost in the course of so many revolutions.

Both these considerations led Smith to construct several modified versions of his instrument by converting it to an R-F mass-spectrometer which provided much higher resolution and intensity, and with far greater precision of measurement. A classic piece of work which Smith carried out using the R-F synchrometer was to ascertain the mass of the neutrino.²⁷⁻⁴

Tritium is known to undergo beta-decay into He³ giving off a beta ray and a neutrino. The energy of the former can be measured on a beta ray spectrometer.

Smith measured the mass difference of the doublets:

\[
\begin{align*}
\text{HD and He}^3 &= a \\
\text{D}_2 \text{ and HT} &= b \\
\text{C}_2\text{H}_4 \text{ and C}_2\text{D}_2 &= c
\end{align*}
\]

The separation of the doubtlet T and He³ (which cannot be obtained directly) is given by

\[
T - \text{He}^3 = a - b - \frac{1}{4}c = \text{rest mass of beta ray} + \text{energy of beta ray} + \text{mass of neutrino}.
\]

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The mass of the neutrino came out to

\[ 0.55 \pm 0.28 \text{ KeV} \]

\[ 0.00000059 \pm 0.00000030 \text{ atomic mass units} \]

This number is not the rest mass - the rest mass of a neutrino is believed to be zero (latest measurement gives it as less than 60eV - ie: less than \( \frac{1}{10} \) of Smith's value) but it has "mass" when it is moving, due to its energy.

6.8 DISCUSSION

The chronotron, omegatron and first edition of the synchrometer were first discussed at the National Bureau of Standards symposium on Mass-Spectroscopy in physics research in 1951, when all three instruments were pioneers in new methods of precision mass-measurement, involving not only new overall techniques, but also incorporating novel design features such as pulsed sources and electron multiples which had come about as a result of scientific advances during the war period in radar and uranium separation.

The history of this central area of mass-spectrometry can conveniently be concluded with a summary of a conference held in 1960 - 'The International Conference on Nuclidian Masses', held at McMaster University, Hamilton, Ontario, Canada September 12-15th.

The intention of this conference was to examine "... the present status of knowledge of nuclidian masses from the theoretical, experimental and technical standpoints, and to discuss what methods could be exploited for future research."

Chairing the conference, Mattauch stated that "Knowledge of atomic masses is mainly derived from three classes of observations:"
a) of total disintegration energies of beta decays
b) of total energies (Q) released in nuclear reactions
c) of mass differences of mass-spectroscopic doublets."

A.H. Wapstra commented:—

"We should not forget that, after all, the mass-spectroscopists are responsible for calling us all here. They have been doing a wonderful job. About four years ago, at the conference in Mainz, the situation was so that people computing nuclear masses did not quite dare to use mass-spectroscopically obtained results at all in the low mass region because they disagreed too much with nuclear reaction data. It is quite amazing to observe that now mass doublets, even after the correction which we felt obliged to make to their errors, are as accurate as nuclear reaction results."

It was at this conference that L.G. Smith from Brookhaven presented the design of his latest R.F. Mass-spectrometer (synchrometer) which incorporated important improvements which allowed the attainment of significantly higher intensity, resolution and accuracy of mass measurement than was achievable with any mass-spectrometer presently known, and Smith considered a precision of 1 part in $10^9$ perfectly feasible.

In the discussion which followed Smith's paper, A.O.C. Nier commented:

"I should like to add a word of encouragement. I think that we have come to a point in mass-spectroscopic mass determinations where systematic errors constitute an important fraction of the total error. Any instrument, such as this, which is different in principle from the older type machines, will at least have systematic errors which are different from them. This is a real advantage. The earlier machine has produced very fine results, and if this new apparatus proves to be as much, or even a fraction as much, of an improvement, it will be an extremely important development."

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RESOLUTION, RESOLVING POWER, DISPERSION AND ACCURACY

Resolving Power:

Electrical: Resolution and Resolving Power:

1. If two peaks are 'just resolved', one can distinguish that two peaks are present.

2. The 'resolving power' is the difficulty of separation, and is stated in terms of $M / \Delta M$ where $M$ is the mass of a given ion. The decision as to how $\Delta M$ is to be determined for a given $M$ is most frequently an arbitrary one. For many purposes the choice of $\Delta H / H = 0.1$ is quite satisfactory. When this is met or exceeded, then the two peaks are considered separated.
Photographic:

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<th>15</th>
<th>16</th>
<th>17</th>
<th>18</th>
<th>20</th>
<th>22</th>
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Mass-spectrum obtained with Aston's first Mass-Spectograph (1920)

If two lines are 'just resolved', one can distinguish that two lines are present.

The 'resolving power' is the difficulty of separation, and is stated in terms of \( \frac{M}{\Delta M} \), where \( \Delta M \) is the mass of a given ion.

(e.g. Aston's first mass-spectrograph could just separate the lines of xenon, thus giving a resolving power of 130).

The numerical relation between mass and position on the spectrum is the 'dispersion'.

This is usually calculated in terms of shift in millimetres for a 1 per cent difference in mass, and is called the 'dispersion coefficient'.

'Accuracy' is the degree of precision in measuring atomic mass, i.e. the number of decimal places a deduced mass is given.

(e.g. Aston could give a figure of 3.994 a.m.u. for Helium, denoting an accuracy of 1 in 1000).
### SOME MASS-SPECTROMETERS AND SPECTROGRAPHS OF VARIOUS TYPES OF ANALYSERS

<table>
<thead>
<tr>
<th>ANALYSER TYPE</th>
<th>INSTRUMENT TYPE</th>
<th>WORKERS</th>
<th>YEAR</th>
<th>RESOLVING POWER</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 Parabola</td>
<td>Electromagnetic</td>
<td>Thomson</td>
<td>1913</td>
<td>15</td>
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<td>2 Direction-focussing</td>
<td>180 Magnetic</td>
<td>Dempster</td>
<td>1918</td>
<td>100</td>
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<tr>
<td>3 Velocity-focussing</td>
<td>Electromagnetic</td>
<td>Aston</td>
<td>1919</td>
<td>130</td>
</tr>
<tr>
<td>4 Velocity-focussing</td>
<td>Electromagnetic</td>
<td>Costa</td>
<td>1925</td>
<td>400</td>
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<tr>
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<td>Electromagnetic</td>
<td>Aston</td>
<td>1925</td>
<td>650</td>
</tr>
<tr>
<td>6 A.C. Time of flight</td>
<td>Linear time of flight</td>
<td>Smythe &amp; Mattauch</td>
<td>1926</td>
<td></td>
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<td>1930</td>
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<td>60 Magnetic 127 17 Electrostatic</td>
<td>Bainbridge &amp; Jordan</td>
<td>1936</td>
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<td>11 Double-focussing</td>
<td>90 Magnetic 31 50 Electrostatic</td>
<td>Mattauch &amp; Herzog</td>
<td>1936</td>
<td>3000</td>
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<td>12 Direction-focussing</td>
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<td>1937</td>
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<td>Aston</td>
<td>1937</td>
<td>2000</td>
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<tr>
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<td>60 Magnetic</td>
<td>Nier</td>
<td>1940</td>
<td>80</td>
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<tr>
<td>15 Direction-focussing</td>
<td>180 Magnetic</td>
<td>Hoover &amp; Washburn</td>
<td>1940</td>
<td>500</td>
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<tr>
<td>16 Pulse-type velocity selector</td>
<td>Time of flight</td>
<td>Stephens</td>
<td>1946</td>
<td>2</td>
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<tr>
<td>17 R.F. Time-of-flight</td>
<td>Omegatron</td>
<td>Hippie, Sommer &amp; Thomas</td>
<td>1949</td>
<td>Variable</td>
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<td>Bennett</td>
<td>Bennett</td>
<td>1950</td>
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<td>19 R.F. Time-of-flight</td>
<td>Bennett</td>
<td>Glenn</td>
<td>1952</td>
<td>250</td>
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<td>20 Parabola</td>
<td>Electromagnetic</td>
<td>Henglein &amp; Ewald</td>
<td>1953</td>
<td>50</td>
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<td>21 Pulsed time of flight</td>
<td>Linear time of flight</td>
<td>Wolff &amp; Stephens</td>
<td>1953</td>
<td>20</td>
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<tr>
<td>22 Pulsed time of flight</td>
<td>Linear time of flight</td>
<td>Katsenstein &amp; Friedland</td>
<td>1955</td>
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<tr>
<td>23 Pulsed time of flight</td>
<td>Linear time of flight</td>
<td>Wiley &amp; McLaren</td>
<td>1955</td>
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<td>24 Pulsed time of flight</td>
<td>Linear time of flight</td>
<td>Harrington</td>
<td>1962</td>
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CHAPTER VII

OTHER DEVELOPMENTS IN MASS-SPECTROMETRY

7.1 DETERMINATION OF ISOTOPIC ABUNDANCES

The investigation of the isotopic constitution of naturally occurring elements has involved firstly the identification of isotopes and secondly, the determination of their relative abundance. Identification has an aspect of finality in that an isotope can only be discovered once, whereas relative abundance can continually be redetermined.

Aston's contribution to the first area may be judged from the fact that since 1939 only six new naturally occurring nuclides (isotopes) have been added to the present day total of some three hundred stable nuclides. (With regard to the relative abundance of the stable isotopes, twenty-two of the naturally occurring elements have only one isotope, twenty have two isotopes, and the remaining thirty-nine, all even numbered elements, share the remaining 221. It is remarkable that no two patterns of abundance are alike, and that there are no really satisfactory theories to explain how the different elements should have acquired the particular abundances they now have.)

Apart from Dempster's early use of electrical methods in abundance measurements of several light elements, most of the isotopic abundance work prior to 1932 employed photographic detection. As we have already seen, Aston was by far the leading figure, and undoubtedly the incredible patience he needed for the laborious and painstaking methods of calibration and photometry he had to use, (complicated by the non-linear behaviour - a characteristic of the photographic process), helped form the opinion, which
he expressed on several occasions, that mass-spectrography would die with him.

Following the pioneer work of Aston and Dempster, there was little development in this area of mass-spectrometry during the next twenty years, until A.O. Nier at Minnesota University introduced refinements to instrument design.

Previous to Nier, however, Smythe and Mattauch built the first apparatus to be called a 'Mass-Spectrometer' for the detection and measurement of isotopic abundances. W.R. Smythe in 1926 first laid down the theory and design concept, but it was not until 1932, when J. Mattauch came to California Institute of Technology that the instrument was built. Smythe's initial training in mass-spectrometry was at Chicago University until 1923 under Dempster, and he was the first to obtain a mass-spectrum for fluorine, an extremely difficult element to handle.

Their instrument analysed ions without the use of magnetic fields, and depended instead on a special type of velocity filter. Smythe and Mattauch were the first workers to apply radio-frequency (R-F) techniques to mass-spectrometry, and the term 'time-of-flight mass-spectrometer' is used to describe this family of instruments. The operating principle was

*Hipple has recounted(2) that Aston loomed very large in the field of Mass-Spectrography, which still in the thirties appeared an exclusively British subject. Because of this reputation, Mattauch was initially deterred from taking up Mass-Spectrography in Vienna. It was through Mattauch's graduate student Hertzog, who produced a brilliant general theory of electrical and magnetic focussing(3), that he ultimately made the decision to build a mass-spectrograph. Mattauch and Hertzog sent the theoretical paper on focussing to Aston who kept it for a very long while before replying, finally indicating very little evaluation of Hertzog's ideas. There was some question as to whether Aston really understood the work, and so Mattauch, to some extent out of pique, took up the ideas himself. Dempster started at the same time as Aston, but their work was in two separate areas and involved fundamentally different equipment. Dempster did not concern himself with mass-determination until 1940.
that if the ions produced in a discharge tube pass successively through two identical alternating fields, all but those having certain velocities can be removed. The velocities could be selected by altering the distance apart of the plates at which the signal was applied, and by altering the frequency of the signal. The beam of ions was then analysed by the deflecting electric field and the spectrum was produced by the ions falling on a collecting plate coupled to an electrometer. In practice, the design had several serious defects. Fringing fields were troublesome, and ions moving with certain velocities other than the preferred one gave rise to spurious peaks, referred to by Smythe as 'ghosts'. These were caused by the symmetry of the arrangement of the R-F fields. The Smythe Mattauch method of mass analysis was ultimately abandoned, although the use of R-F has increased enormously.

In an interview at California Institute of Technology in 1972, Smythe commented that although at first the symmetry of the instrument was aesthetically pleasing from the design point of view, he had "completely unanticipated the 'ghosts'". It took Smythe and Mattauch a long time before they found a way of removing the 'ghosts' by the simple expedient of cutting grooves in the R-F plates which made them unsymmetrical and thus stopped the resonance.

During the early thirties, owing largely to improved vacuum techniques and the development of new methods of electrical measurement, mass-spectrometry advanced rapidly. Previously ion current had been measured on quadrant electrometers, but these were replaced by vacuum electrometer tubes. The advance was particularly marked in the USA where Bleakney, Brewer, Blewett and Samson, Nier and others constructed improved versions of the original Dempster type 180° magnetic mass-spectrometer.
A typical one of outstanding merit was constructed by Nier in 1937. The analyser was of the simple semicircular focussing type, and its most important feature was that it was made from nichrome and copper. It was completely housed in a pyrex glass container, so that it could be effectively baked and 'outgassed'. This not only enabled such low pressures to be obtained that only very moderate voltages needed to be used, but also, and much more important, it removed the possibility of chance contamination.

Nier's apparatus could detect abundances as low as 1 part in 100,000 and his accuracy of measurement of abundance ratios was within 1 per cent., at that time an unprecedented degree of refinement.

Although Barber and Stephens showed in the early thirties that refocussing at 180° is only a special case of the focussing action of any wedge-shaped magnetic field, almost all mass-spectrometric work prior to 1940 on relative isotope abundance measurement was carried out on instruments with 180° magnetic analysers. The two main designs were the Dempster-type direction focussing machine, and modified versions thereof, such as that of Nier, and the Bleakney mass-spectrometer, first described in 1932 which differed substantially from the original Dempster instrument. The uniform magnetic field for ion dispersion in this instrument was produced by a solenoid. The effective radius of the ion beam path was limited, of course, by the dimensions of the solenoid: in this instrument the radius was only 3.5 cm. Nevertheless, the machine proved an invaluable model for many such instruments constructed during the next decade for mass analysis of light elements. Bleakney's instrument is interesting in two other respects:
1) The electron beam used for electron impact ionisation was immersed in the solenoid field which collimated and concentrated it.

2) The design permitted the use of long ion source slits (approximately 5.5 cm. in length) so giving a corresponding increase in sensitivity.

Up to 1940 all mass-spectrometers described in the literature for relative isotope abundance measurement differed only slightly from the Dempster or Bleakney models, both of which were of American origin. In fact, apart from Aston in Cambridge, and Oliphant, Shire and Crowther who designed a cross-field spectrometer for lithium isotope separation, virtually no mass-spectrometric work had been attempted in the rest of the world.

Although considerable experience had been gained in the USA in the development of special techniques, a much greater impetus to further progress came in 1940 with the publication of details of Nier's first sector-field direction-focussing instrument. This was a true milestone in mass-spectrometer instrumentation.

The ion-optical properties of the sector mass analyser had been known for some years, but only exploited in mass-measurement mass-spectrography. The ion beam enters and leaves the field at right angles to the field boundary so the deflection angle is equal to the wedge angle, namely 60°. The geometry is symmetrical in that source and detector are equidistant from the magnet. This instrument was designed for gas isotope analysis, and had a resolution of 1 part in 100. The ion source, a relatively simple electron bombardment design, has become known as the "Nier-type" source, and the detector design, utilising a double collector arrangement with a null method of determining abundance ratios has also become famous.
This instrument and an improved version built by Nier in 1947 have served as the model for many subsequent designs, including Hippie who published details of a sector field instrument employing a magnetic angle of 90°. All important sector field instruments described since have employed a magnetic angle of either 60° or 90°.

It should not be supposed that the 180° machine was thereupon discarded - in fact, the appearance of the sector field instrument had the reverse effect, stimulating those in favour of the 180° machine to greater activity, and workers in abundance mass-spectrometry were divided more or less equally into two schools: for and against sector instruments.

The sector field instrument in fact gave a loss in relative sensitivity, and introduced more fringing effects because the ions had to cross the magnetic field boundaries twice. It was difficult to arrange for the fields to have sharp boundaries which the ion beam crossed at right angles. However, mass-discriminating effects were reduced. In its creation, Nier was inspired mainly by a desire to produce a relatively cheap all metal instrument. Reduction in the size of the magnet reduced both the cost of construction and of operation. It made possible the use of a permanent magnet to provide the stable magnetic field, and became a basic design in commercial mass-spectrometers, which will be discussed later.

7.2 ELECTRON IMPACT STUDIES

The study of collisions between electrons and molecules began with Lenard in 1902. In 1913 Franck and Hertz demonstrated that electrons require a certain minimum energy in order to cause ionisation in a gas,
and that this minimum energy, or 'ionisation potential', depends upon the nature of the gas. This led to many investigations in which the ion current was plotted as a function of electron energy. Here, the ionisation potential corresponds to the energy at which ion current is first detected. These experiments suffered from the limitation that, except for monatomic gases and metallic vapours, the nature of ions so formed was either not known or, at best, surmised.

In the meantime, gaseous ions had been studied extensively by Thomson, Wien, Aston, and Dempster, using positive ray analysis methods. In particular Dempster had investigated the relative numbers of $H_3^+$, $H_2^+$ and $H^+$ ions produced at different pressures.

In 1922 H. D. Smyth at Princeton combined these two types of experiment. In his work, ions were created by the impact of electrons whose energy was variable and known, and were then subjected to positive ray analysis. Within the next ten years this general method underwent many refinements and improvements, notably by Smyth and co-workers at Princeton, Hogness and others at Chicago, Kallman in Germany, and Tate and Bleakney in Minnesota (Tate did his doctorate under Franck and Hertz in Germany). During this period the modern arrangement of transverse electron beam was developed in Tate's laboratory (one of the pioneering labs for electron impact study), and was modified by Bleakney for operation at low pressures.

Since then, the study of electron-molecule collisions has proceeded in many directions, and has revealed much information concerning ionisation and dissociation processes.
The main areas of interest include the measurement and interpretation of appearance potentials and ionisation potentials and the use of such data in the evaluation of bond dissociation energies, heats of formation of molecular ions, and the determination of electron affinities.

7.3 DETERMINATION OF IONISATION POTENTIALS

'Appearance potential' is defined as the minimum energy which a bombarding electron must possess in order to produce a particular ion from a particular molecule - it is the potential at which the ion makes its first appearance. The ionisation potential is thus one of the appearance potentials.

The orthodox type of electron bombardment source was first used by Dempster\(^3^3\) and subsequently developed by Smyth, Bleakney, Tate and Smith\(^3^6\) and Nier\(^3^7\) and it is an interesting fact that with very few exceptions, all sources in routine operation on mass-spectrometers have been modelled on the Nier pattern.

As anticipated from the Franck and Hertz experiment, the ion current does not begin until electron energy exceeds ionisation potential, after which it rises rapidly. In this way the energy needed to remove a single electron from the molecule, and the corresponding energies for two, three or more electrons, may be determined, and such experiments not only substantiate spectroscopic values, but in the case of multiply-charged
Possible Franck-Condon transitions for the diatomic molecule case as the result of electron impact.
ions, frequently provide information not hitherto obtained from atomic or molecular spectroscopy.

Bleakney, working in Tate's laboratory at the University of Minnesota, was the first to use the mass-spectrometer to study the probability of multiply-charged ion formation as a function of electron energy. He obtained ionisation efficiency curves for Argon \( A^+ A^{2+} A^{3+} \) and \( A^{4+} \) and similarly for Neon and Mercury. Although the ionisation efficiency curves have the same general form for all gases, there are significant differences in the absolute values of their maximum ionisation cross-sections, bearing out the expectation that multi-electron molecules are more vulnerable to ionisation than those with small total nuclear charge.

This area of mass-spectrometry has also been concerned with the determination of energies of excited states of molecules, where the ions formed by electron impact may be formed in excited state, as well as in the ground state. Tate and Smith in 1932, with oxygen, showed there were breaks in the ionisation efficiency curves, and similarly Mann Hustrulid and Tate in 1940 observed clear breaks in the curves of ammonia.

Frequently ionisation, as a result of electron impact, is accompanied by dissociation of the target molecule.

The fact that dissociation products may possess zero or non-zero kinetic energy may be understood by reference to Figure 7.3(c). This diagram illustrates, for a diatomic molecule \( AB \), the transitions from the ground state, (curve 1), which are permitted by the Franck-Condon principle. Curve 2 represents a stable ionic state, while curve 3 represents a repulsive one.
By the use of retarding potentials Lozier at the Cal. Inst. Tech. studied the specific charge and state of excitation of each of the dissociation products and their respective appearance potentials.

The first attempt to study the initial kinetic energy of dissociation products was made by Bleakney who used retarding potentials in the source of the mass-spectrometer to discriminate between ions of zero and non-zero energy. Later, Hagstrom and Tate studied initial kinetic energies by their effect on the shape of mass-spectral peaks.

When it is possible for the dissociation products to possess zero kinetic energy, as in transitions to curve 2, the appearance potential enables the bond dissociation energy to be deduced, and these have been obtained for many compounds including hydrocarbons by Hippie, Stevenson and Dibeler and many others.

The main difficulty in obtaining accurate ionisation and appearance potentials lies in getting a monoenergetic beam of electrons. At best, a Gaussian distribution of energies, with a noticeable spread of a few tenths of a volt is obtained. Much effort has been expended since the nineteen-fifties both on narrowing the energy distribution, and on deconvolution techniques which accept a Gaussian distribution and correct for it by mathematical analysis.

Attempts to build electron monochromators have not been very successful partly because of the problem of getting a sufficiently intense beam and partly because of problems with surface charge and external fields. Deconvolution techniques are regarded with varying amounts of scepticism in the mass-spectrometry community and it is certainly easier to get the correct "breaks" in the appearance potential curve if one knows where they
are before one starts. Since about 1968, emphasis has swung towards photo-ionisation techniques in which ions are produced by a beam of monochromatic ultra-violet light. The useful wavelengths are all in the vacuum ultra-violet region which creates experimental problems. Nonetheless, in favourable conditions, ionisation potentials correct to three of four decimal places can be obtained, several orders of magnitude better than are given by electron impact.
CHAPTER VII

OTHER DEVELOPMENTS IN MASS-SPECTROMETRY

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I

CHAPTER VIII

THE DEVELOPMENT OF MASS SPECTROMETRY DURING WORLD WAR II

Innumerable histories have been written of the Second World War, many of them discussing in great detail the development of the nuclear bomb and the technology and engineering needed to produce it. Mass-spectrometry played a variety of roles in this development and was a vital part of the project. Besides this work, mass-spectrometry also played an important part in synthetic rubber production, in detecting oil and in analysing oil via 'cracking patterns'.

8.1 NUCLEAR FISSION AND ISOTOPE SEPARATION

Hahn, Strassmann and Meitner's epoch-making discovery of the fission process of Uranium, reported in Naturwissenschaften February 1939, was eagerly awaited by scientists in America, in particular those at the Carnegie Institute Laboratory, Columbia University and at the Lawrence Radiation Laboratory at the University of California, who immediately repeated the historic German experiments, and saw for themselves proof of the atomic forces that made the atomic bomb a reality six years later.

The following months saw further discoveries and speculation until it was reported from Paris that a chain reaction was indeed created in uranium fission.

As Robert Potter wrote in 1947,

"Those days in Washington with the Carnegie atom smasher - the summer of 1939 to the winter months of 1940 - were in their way fantastic. While scientists were saying for quotation in the public press that the release of atomic
energy in any usable form was decades away, they were already in consultation with Army and Navy scientists, and were pledged to secrecy, not only to the outside world, but also amongst the community of scientists."

The government applied wartime censorship to all discoveries concerning atomic structure and the element uranium.

During the subsequent four years, silence fell on American atomic research, and no word of any progress made was revealed, until the official report was published in 1946 called "Atomic Energy for Military Purposes", written by Henry D. Smyth of Princeton University. Smyth was one of nine American scientists called in to organise atom bomb research before America entered the war, and the report shows how two kinds of chemical elements were used to make the bomb — one, Plutonium, not known in nature, and the other U235, and various processes were developed both to facilitate the synthetic production of plutonium, and to carry out separation and purification of U235.

It was already known that when Uranium was bombarded with neutrons, it was only the U235 isotope which was fissionable — U238 simply absorbed the neutrons and did not split. But U238 was also vital for the production of plutonium.

Determination of the constitution of Uranium in the first place was not easy. Aston in 1931, using the volatile hexafluoride, concluded that Uranium was simple to at least 97%. The expected second constituent was detected by Dempster in 1935, and he estimated the abundance of U235 at less than 1%. 

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Dr. Alfred O. Nier
Department of Physics
University of Minnesota
Minneapolis, Minnesota

Dear Nier:

Since our discussion last spring in Washington on the possibilities of using a mass spectograph separation of the uranium isotopes for deciding whether the slow neutron fission is or is not due to 235 isotope, I have convinced myself that this is actually the best way to decide the question, which is of a considerable theoretical and possibly practical interest.

I understand that you have lately undertaken such a separation, and I should very much like to know whether and how this work is progressing.

Please give my best regards to Professor Tate.

Yours sincerely,

E. Fermi

E. Fermi
It was not until 1940 that Nier was able to isolate in a mass-spectrometer detectable quantities of the two isotopes, and was thus able to show that U235 was the isotope responsible for fission brought about by neutrons.

By 1941, four methods were put forward as possible ways by which the rare U235 could be isolated. All methods would probably work, but no one knew how well, neither did any scientist know which of the methods would be the fastest and most suitable for production. Moreover, it had become apparent that if a new transuranic element, plutonium, could be created, it too would serve as an explosive in the atomic bomb. Thus, scientists had to consider a total of five major projects. Besides plutonium production, these were:

1. **Magnetic Separation** - like a gigantic mass-spectrometer, the different uranium isotopes were ionised and separated in strong electromagnetic fields. The first form of this device was constructed out of E.O. Lawrence's cyclotron at Berkeley, and became known as The Calutron, after the University of California. From this prototype there ultimately resulted a whole battery of giant calutrons at Oak Ridge in Tennessee. It was realised that even on this gigantic scale the isotopic separation would be slow - however, it would be certain.

2. **Centrifuge** - the second method was to spin the isotopes in an ultra-high centrifuge, which would separate the isotopes because of their difference in mass. This method demanded that the uranium had to be in a gaseous form; the major compound used was uranium hexafluoride, which was extremely corrosive.
3. **Gaseous Diffusion** — in this method the uranium isotopes are separated by their different rates of diffusion through a porous material owing to their slight differences in mass. The degree of separation on passage through one barrier is slight, but the process can be used repeatedly, so that high separation can ultimately be achieved.

4. **Thermal Diffusion** — in the fourth method, uranium in liquid form is placed in long vertical tubes down whose centre passes a heated wire. In such tubes the heavy isotope begins to concentrate around the central hot wire. Moreover, the cooler part of the liquid (containing the heavier isotope) sinks to the bottom of the tube. Again, by repeatedly passing through successive thermal diffusion towers, U235 in highly purified form can be achieved.

Whichever method was used, Mass-Spectrometry was the only way to monitor the separation of U235 from U238.

With all these possible separation methods, the scientists were faced with a dilemma. What they feared mainly was time, for the Germans, who had discovered uranium fission first, were known to be working on the problem too. It is ironic that Werner Heisenberg was the only great physicist to

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*This was, of course, the method tried unsuccessfully by Aston before the first World War and he was dealing with a mass difference of 10% — \((\text{Ne}^{20}\text{Ne}^{22})\) whereas the Manhattan Project scientists were faced with a mass difference of only 1% — \((^{235}\text{UF}_6\text{ and } ^{238}\text{UF}_6)\).*

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remain in Germany to work on their Atomic Energy Project, and the German failure was in a large part his failure in directing it.

Under normal conditions scientists could take time to do many experiments, build small pilot plants of the four different methods and proceed cautiously to large scale production. But, with the wartime pressure, the stakes were too great, and it was decided, with the final approval of the Joint Chiefs of Staff and President Roosevelt, that all methods should be undertaken at once and on an enormous scale that would achieve mass-production. The whole effort was code-named the 'Manhattan Project'. The decision was not an easy one to make. It meant assigning scientists whose talents could otherwise be employed in radar development, submarine detection, etc. It meant releasing vital materials for the building of plant, and it meant assembling tens of thousands of workers for the necessary construction work at a time when the manpower shortage was being felt everywhere. The Manhattan Project budget was in excess of £2,000,000,000 at 1945 prices. Moreover, the scientists set out on large scale production of plutonium, element 94, using U238 as the 'mother' substance. The transmutation of U238 via Neptunium (following neutron absorption) to Plutonium (after beta-ray emission) was first experimentally achieved in 1942 by Fermi and his group at the University of Chicago, and they built the first atomic 'pile' in the squash courts of Staggfield, the University's games stadium.

However, the necessities of the war effort had anticipated that the Staggfield experiments would be a success, and plans had already been laid for full-scale production of plutonium, which called for a small laboratory pile at Chicago for research; a semi-industrial plant at Oak Ridge to make small samples of plutonium, and the huge full-scale master plutonium plant

*Footnote: Amongst a development programme littered with mistakes and errors, the main error the Germans made was in using D₂O, 'heavy water', to moderate the reaction, not carbon as did the Allied programme.
at the Hanford Project near Pasco on the Columbia River, Washington. The
immense complexity of the entire operation explains the bafflement of
many of the scientists and practically all of the technicians engaged at
Oak Ridge and at Hanford.

Whilst these two plants were manufacturing $^{235}\text{U}$ and plutonium under
the direction of H. Urey, E.O. Lawrence and A.H. Compton, there was still
no laboratory working on the problems of the bomb itself, until
J.R. Oppenheimer in November 1942 was appointed to run a new bomb
laboratory in the remote Los Alamos corner of the deserts of New Mexico,
and his first task was to build the laboratory.

Scientists and equipment arrived from Princeton, Chicago, California,
Wisconsin and Minnesota Universities, mass-spectrometers came from
Princeton, and Harvard dismantled its giant cyclotron and shipped it to
Los Alamos. Activity was so intense that although the rebuilding of the
Harvard cyclotron was started only on 14th April 1943, the first experiments
with it were performed in July of the same year. By the end of June 1945,
the laboratory was ready to test the first atomic bomb. As H.D. Smyth
said,

"A weapon has been developed that is potentially destructive
beyond the wildest nightmares of the imagination: a weapon so
ideally suited to sudden unannounced attack that a country's
major cities might be destroyed overnight by an ostensibly
friendly power. This weapon has been created not by the
devilish inspiration of some warped genius but by the
arduous labour of thousands of normal men and women working
for the safety of their country."
The scaled-up version of the mass-spectrometer, renamed the Calutron which was the instrument used for separation, was located at Oak Ridge. The diffusion plant was also located at Oak Ridge, and mass-spectrometers were not only essential for 'on-line' monitoring of the production of U235, they were also used as leak-detectors for the pipe system. This was the first time that mass-spectrometers were put to routine work, and they were so accurate that they were not believed at first.

Alfred Nier was in charge of the development of mass-spectrometers for the entire gaseous diffusion project. The equipment was first developed at Minnesota, and then at the Nash building in New York city, (converted into a laboratory for the Manhattan Project), by the Kellex Corporation. In an interview with Nier in 1972 (see appendix) he states,

"The instruments used for all the uranium analysis through the entire project were developed here. We built more Mass-Spectrometers here from 1941-43 than probably had ever been built before. We built seven uranium instruments; I think something like ten hydrogen/deuterium analysis instruments; four helium leak detectors; and a number of other miscellaneous instruments. So we became the biggest manufacturers of Mass-Spectrometers at the time. Of course, most of the instruments were then built by the General Electric Company, which got the contracts for the uranium instruments; for the leak detectors, which were produced by the hundreds; Mass-Spectrometers by the many dozens (which were used in the electromagnetic separation plant as well as the gaseous diffusion plant); and for the miscellaneous ones for the centrifuge work at the University of Virginia. And some of these original instruments are still in existence, by the way - I don't know if they're still used, but they still exist. Unfortunately, we don't have any of this equipment because it was all secret and taken away from us when our Project closed; it went to other places, and then when the war ended that's where these things were, so we have nothing to show for this. We still have some instruments around somewhat similar to parts of Mass-Spectrometers. I can show you one of those if you are interested in seeing it, but most of the original things have disappeared since.

*Footnote

In an interview with Professor A.J.B.Robertson at Kings College in 1971, he considered that "Nier was a brilliant student and worker, he built in less than a year a mass-spectrometer to monitor the gaseous separation."
DRP: During the Manhattan Project, did you see the use and
development of the Mass-Spectrometer going according to
plan: the goal was there and you followed the right path?

AN: Yes. Actually it turned out to be a very fruitful programme.
If you follow the different things that were done, I think it
really turned out quite well. For instance, take the helium
leak detector. The original ones we built here had a glass
spectrometer tube. We had enclosed permanent magnets; the
envelope surrounding the Mass-Spectrometer was all glass.
We made one concession, if I remember correctly: the filament
was waxed in. We had a ground joint with wax around the outside;
that was the one concession we made to having an ultra-clean
system. We had mercury diffusion pumps on those; when they
went into commercial production they all became metal. We
built the first all-metal spectrometer tube for this as a
sample for General Electric and they copied it, and those
early leak detectors were just about direct copies of ours.
They had, I believe, oil diffusion pumps, but they may have
had mercury diffusion pumps on them. There was a liquid
nitrogen trap on them, of course, and then there were metal
valves and so on. I don't think, given the time span, you
could have done much better. The instruments were operated,
since they were portable, by two 6-volt automobile batteries,
which of course later on were replaced by other things, but
that was very convenient then. Two 45-volt dry batteries ran
the acceleration supply, if I remember correctly, and so it was
a completely portable unit. It was big and ponderous, but still
it was portable, so they could have these in the foundries of
the Crane Plumbing Company that made valves for the Manhattan
Project, and various other places - on the test floors at the
Oak Ridge Diffusion Plant and other places. So while they were
unwieldy, they did work and were amazingly reliable.

An interesting thing was that one of the problems we had was
that since it was known that these were instruments right on
the forefront of technology at the time, there were a lot of
bright young people working with them who were supposed to do
maintenance, but they wanted to improve them all the time.
For instance, the feedback amplifiers we used which were
perfectly good - they worked beautifully well, they would
want to add stages of amplification. For instance, one came
in that was damaged or something had happened, it may have
needed maintenance: they would want to rebuild the circuit.
And so we had a great deal of trouble in the early days. We
finally insisted that the maintenance be put in the charge of
people who were not that bright, but could follow instructions,
whereupon the 'down' time dropped in a precipitous manner, and
I think they finally got things so that they would be in
operation 95% of the time, which at the time was really quite
good. The uranium instruments went into production, and while
there were the usual problems in the early days, the GEC in the
end did a pretty good job in getting these going. They were used
by the dozens: as I say, in Oak Ridge for the uranium analysis,
both at the Y 12 plant, which was the electromagnetic plant, and
specially the gaseous diffusion plant, which as you know became
the backbone of the whole effort later on. The circuits that were used were almost the identical ones that were developed here. The 6" instrument - the 6" x 60° angle - became the standard instrument. The ion source we had developed here originally with the multiple electrodes became the standard for this. The system we'd worked on, whereby you had no more electrons than necessary hitting any given surface, so that you didn't decompose any UF₆ to UF₄ to form insulating coatings, that was followed quite well. As time went on, they learned more and more what the tricks were in working with UF₆ (which was not easy, by the way); and there was a lot of maintenance on those instruments but the people in charge of these things finally got quite good at this, with the result that the 'down time' on those was remarkably low, considering the difficulty of working with UF₆.

DRP: You designed an extremely successful machine?

AN: That was quite a successful machine. It had to have high resolution to separate the uranium isotopes, and of course as time went on there in the plant, the question arose of the enrichment of the U²³⁴ as well as the U²³⁵; and then later on, when they began to feed in products that had been in reactors for a time - there were other isotopes to worry about - it worked out quite well.

The biggest development I worked on in those later years: I left the University of Minnesota in the summer of 1943 to take charge of the Kellex Laboratory in New York City, in the Nash Building as I said before, and our biggest project was the development of what was called the line recorder, which was the first on-line Mass-Spectrometer for monitoring the impurities in the Oak Ridge gaseous diffusion plant. And there we had to do a certain amount of investigating new techniques. The reason for having the Mass-Spectrometer on this was to watch for impurities building up in the gaseous diffusion plant which would kill the plant - either destroy the barriers or the platinum. For instance, if it developed a leak due to air getting in or a refrigerator pipe breaking somewhere, this of course could fill the entire plant with impurities, so it would have to be shut down.

As a matter of fact, it happened once: in the spring of 1945 the entire plant was shut down because the people in charge of operating refused to believe the Mass-Spectrometers. By that time we had them on the plant. This is an interesting story in itself - I don't know if you want to hear it? These are things that are never published anywhere as you know. But at that time the lower part of the plant was operating and was producing the feed material which was given to the electromagnetic plant. You see, if you could start with a higher base, this of course would help enormously in getting large amounts of material, and the feeling of some of the people - our idea in operating the plant was to have the Mass-Spectrometers sprinkled throughout the plant
at various stages - I've forgotten whether there was one Mass-Spectrometer every 100 stages or so of the plant. And then there would be slave recorders run from the strip chart recorders at the instruments - slave recorders in a master control room - so the operator could see the readings on each of the Mass-Spectrometers in each part of the plant simultaneously. And this was before the days of the electronic recorders; these were the mechanical balancing type which were made in this country by Leeds and Northrup, so they were rather slow. Also these were multi-point recorders, so that you could check - I've forgotten the exact number, but I believe these were 16-point recorders, so you could follow as many as 16 components by jumping from peak to peak on these. We didn't use all 16; I think we used something like 8, and then duplicated some of the signals such as nitrogen, which we were interested in monitoring more than other things.

Then one day in the spring of 1945, a major leak developed some place in the lower part of the plant. The Mass-Spectrometers showed this, because the air or nitrogen was leaking in - I've forgotten which it was - of course it went and worked its way up the plant, so you could see recorder after recorder showing this wave of nitrogen filling up the plant, coming along just like a wave of water, as though there was a flood. The so-called practical people running the operations refused to believe the Spectrometers, but began to shut valves.

DRP: They knew that there was a leak?

AN: They knew there was a leak, but they didn't know what to do about it. Whereas the man in charge of the instruments, he nearly went crazy because he knew exactly where the leak was, but they wouldn't believe him. It was obviously in the first building that showed, because you see the light gas did not go down in the plant; it only went up; so you could just trace it back to which was the first Mass-Spectrometer to show it. He told them exactly where the leak was, but they wouldn't believe him. By the time they got around to this, however, the entire plant had filled with air, so that the entire production of uranium\textsuperscript{235} was shut down until they straightened this thing out. Well, by sheer coincidence I had come into town that very day - into Oak Ridge - when all this happened, and my superior, the man who I think was not the President but the General Manager of the Kellex Corporation, also came to town that day. You see, the plant was being operated by Union Carbide personnel, but the Kellex people still had charge of it as the builders of the plant, so there was this mixed-up relationship. So we told our story to Mr. Baker, who was an electrical engineer himself and appreciated these new instruments, and was also in a position of high authority. The records on this were the sort of thing you could publish in a text-book on how you look for leaks in a big plant, because the records told exactly what had happened; yet no-one had been willing to believe them. Well, I can assure
you, after that had happened the man who had been crying in the wilderness when the first accident happened had a great deal more authority than he had had up to that time. After that the instruments found their rightful place in the Project, and were very useful for diagnosing many troubles which were taking place. This was perhaps the most interesting development."

8.2 SYNTHETIC RUBBER

Among other wartime projects which involved mass-spectrometry was the production of synthetic rubber. The Rubber Reserve was set up because the Japanese occupation of the rubber plantations of South-East Asia had stopped the supply of natural rubber to America. The study and development of synthetic rubbers was therefore a necessity.

During the 1940s, Keith Brewer was in charge of the Mass-Spectrometry laboratory at the National Bureau of Standards, the only laboratory available at the time which could perform the difficult calculations. With two mass-spectrometers made by Minnesota, and one by C.E.C. they carried out an intensive study of the cracking patterns of organic materials and the analysis of all kinds of plastic and fibrous materials. This of course was an extremely difficult process because no patterns were available, and Brewer's laboratory was instrumental in initiating this new area of mass-spectrometry.

Brewer has commented on the antagonism which he and his team were subjected to, as a result of it being considered that they were physicists doing chemists' work. The Chemistry Department at the National Bureau of Standards at one stage said that the mass-spectrometry section should be "ashamed to carry on such research in Government Laboratories" and that they could not possibly "use a machine based upon momentum to do chemical analyses". Eventually they capitulated and asked for a mass-spectrometer to use themselves.
SCHEME OF BREWER'S MASS-SPECTROMETER (1934)
At the same time, mass-spectrometers were first used in the oil prospecting industry. Harold Washburn, together with Herbert Hoover, who formed the Consolidated Engineering Corporation (C.E.C.), were interested in the development of instrumentation of any type that would be useful to the petroleum industry. They were looking for an apparatus which would analyse surface soil on the theory that if oil were located beneath, the surface soil should contain hydrocarbons. D.D. Taylor, from California where he had been developing a mass-spectrometer, joined C.E.C. and commenced work to produce a mass-spectrometer capable of soil analysis, and also to investigate the mass-spectrometric possibilities of refining stream analysis. C.E.C.'s main aim was to produce a simple apparatus which would remain calibrated for reasonable lengths of time, and their first, and indeed the world's first commercial mass-spectrometer was built and sold in 1942.

Similar work was under way at Westinghouse research laboratories in Pittsburgh, where John Hippie and E. Condon were instrumental in organising a cooperative programme to apply the mass-spectrometer to the analysis of hydrocarbon gases. Westinghouse organised a project with the participation of five industrial oil corporations, in which scientists were sent to work on analytical applications, and to develop accurate routine work on the mass spectrometer. (This was partly brought about because of the interest in identifying where the Germans were getting oil from. H.W. Thompson did some work on this by Infra-Red Spectroscopy.)

Hipple comments,

"So there was more and more interest in making some hydrocarbon analyses because people wanted to do various things - looking for oil, you know, and measuring the gas in the ground - there was a lot of that. So it began to look very attractive, I think primarily on the basis of the work that Bleakney had initiated at Princeton. It wasn't so clear at the time that this was going
to be the answer for making reproducible mass-spectra with regard to the pressure, because that wasn't the aim at the time, but it did mean that you could superimpose these different hydrocarbons and have a mixture and the peak heights would stay linear with pressure. So this was certainly one of the interests of Westinghouse, not that they were in the chemistry field, but they were looking for possible products. This didn't all work that way. The result was that I had all these people from chemical companies and oil companies there to use the instruments, and we really didn't have an instrument that was set up for that. It was true that there were instruments where you did have this linear response, but that's a long way from a calibrated instrument, an instrument that will maintain calibration for some time, making mixtures and doing it on a routine basis.

So after these people left, then they went and ordered instruments from Westinghouse, and Westinghouse just wasn't organised in any way at all to make a Mass-Spectrometer. The only place that they had any vacuum work in any of the plant was up at Bloomfield, New Jersey, where they made light bulbs. And up there they didn't do any work in electronics, and the only place that they had anything in electronics was in Baltimore, Maryland – that was the radio division. They made military radio equipment during the war. Well, that was really some circus. In the first place, we had a lot to learn, and we could run an instrument for some time and even after these people left it would be shaken down. The people in Baltimore – that was the place that had responsibility, but they couldn't make the key part of it, which was the tube, so the people in Bloomfield, New Jersey, at the lamp works had to assign someone to be in charge of making the tube for Baltimore. But of course they were just the sub-contractors and the profits were in Baltimore. People in Bloomfield just couldn't lose anything on it, because after all they made it for whatever it cost them. They were accustomed to making things like thousands and thousands of light bulbs. So they could tool-up; they made production lines with beautiful jigs and fixtures to make Mass-Spectrometer tubes. But they'd never made any, so it turned out that they really didn't use them in the end; they had to make then just about the way we make them in the laboratory. But it was an educational exercise on all sides – on my side to know how you operate with industrial divisions. They have certain procedures, and in Baltimore, in the radio division, they're accustomed to making radios that delivered, and they made quite a few of them. You have to have drawings before you do anything, so they made a fantastic number of drawings. The people down there knew just very little about the instrument. They came up and looked at it, you know, diddled around it a little bit. Then they made all the drawings and they tended to do it independently of us at the lab; we were some distance away, about 250 miles or so. Once it goes from the drawing department into production and they start to make it, well, to change anything would be catastrophically. Well, it was great. Anyway, we finally did make the
instruments, and then the people who were working for me were out in the field all the time getting these instruments — those who had been on the cooperative programme — getting them to work. It turns out that after the war Westinghouse took a look at this field. They made the decision that they wouldn't make them anymore, although we had gone through the exercise of claiming a new instrument. We had I think a very nice recorder which we devised and we learned a lot — new calibration systems, new tubes, and so on — many-sided improvements on the basis of a market study.

DRP: A market study?

JH: They did a market study, yes. It was an interesting study; they had this chap who made a study and he turned out to be a physicist. He was a theoretical physicist who decided after the war that he was going to go into industry, so he was in the economics group in Westinghouse. He found that the only companies that had a Mass-Spectrometer after the war — this was in 1946 — were companies that had at least 75 million dollars in sales a year. Furthermore, they weren't soft-drink manufacturers, they were some special category of oil company or chemical company. Well, how many companies in the United States were there like this? Figure that out, how many had a Mass-Spectrometer? Well, at that point there were quite some. I think there were something like 15 or so — Consolidated Engineering had sold I don't know how many — so you could project from that how many did have them; the market was getting to be saturated.

8.4 DISCUSSION

In considering mass-spectrometry from 1946 onwards, many of the important developments in this period sprung mainly from advances in technology made by well-coordinated teams of scientists operating under a wartime sense of urgency. Obviously, these exceptional responses to the desperately felt needs of war had to be followed by years of patient development, but the advent of most of them would have been long delayed if that wartime sense of urgency had not triggered off the initial advance.

One serious obstacle to bringing about major technological advances that can give society big new opportunities is that many of the most creative minds choose to go into pure science. From a very long term view — much more than thirty years — it is imperative that pure science continues to be pursued: periodically it yields unexpected discoveries
of the greatest importance. It appears, however, that occasional interruptions such as that created by the two world wars does not harm pure science and may even help it.

Practically no pure science was being carried out during the Second World War; the pure scientists joined the war effort, with fruitful results. Yet the gap in progress of science, which in no major country was less than four years, seemed to have no harmful effect. Pure scientists, on resuming their work after the war, found that the wartime advances in technology had so improved the tools available for scientific investigations that innovation took place at a faster rate than before. What had been a period when scientific freedom was severely curtailed, nevertheless was a period of great inventiveness and productivity.
REFERENCES

5. For a detailed account of the German nuclear effort, see "Alsos", by S. Goudsmit.
7. NIER, A.O.C., Private Interview, Minnesota, June 1972, (see VOLUME 2).
8. BREWER, A.K., Private Interview, Washington, June 1972, (see VOLUME 2).
PART FOUR : RECENT DEVELOPMENTS

Chapter IX follows the growth and commercial exploitation of mass-spectrometry after the war years into many other areas of science, becoming widely available as an analytical technique for other problem areas.

Chapter X is concerned with the development of a particular sub-community within mass-spectrometry, and shows how the organisation and relationships of scientists operate in a 'family structure', within the wider context of the mass-spectrometry community.
CHAPTER IX
POST WAR GROWTH AND COMMERCIAL DEVELOPMENT

9.1 INTRODUCTION

The six years of wartime work gave a boost to science in general and especially to mass-spectrometry. As previously described, the main research direction during the war was towards technological development. Apart from specific developments of the mass-spectrometer as incorporated in the production of plutonium and the separation of the uranium isotopes, other innovations occurred in three main areas.

Firstly, considerable emphasis was placed on refining the mass-spectrometer so that it became more reliable and gave reproducible results. This meant that it ceased to be purely in the domain of the physicists, (an integral, if temperamental part of their research, representing an adventure, both in ideas and in laboratory manipulation), and gradually evolved into an analytical instrument.

Secondly, wartime developments saw the spread of mass-spectrometric techniques into new fields. Whereas the pre-war research was involved largely with the measurement and comparison of isotopic masses, and with processes of ionisation taking place in the mass-spectrometer, post-war development was almost exclusively that of exploitation of the mass-spectrometer's analytical possibilities, and this growth has been linked inseparably with the needs of the burgeoning oil, chemical and semi-conductor industries.

Individual applications of the mass-spectrometer as a tool have included:-
fundamental research on chemical reactions in solutions and
in gases, including those that take place in flames;
technological research in the petroleum industry and control of
the routine operation of oil refineries;
the elucidation of metabolic processes;
the determination of the age of minerals, (archaeological dating);
the constitution of the upper, Lunar and Martian, atmospheres;
diagnostic procedures in medicine;
the understanding and preparation of materials in the solid state
for use in electronics;
in fact, the potential for mass-spectrometric analysis arises in almost
any branch of science to which the direct measurement of the relative
amounts of particles of different masses provides the answer.

Thirdly, a crucial development has been the link-up between mass-
spectrometry and advanced electronic techniques for accurate recording
and measurement, and for facilitating the vast amount of calculation
necessary to interpret the raw spectral data.

After the war, although there was a general increase in interest in
the applicability of mass-spectrometric technique, two fields boomed —
onorganic chemistry application, and use in solid state technology.

9.2 ORGANIC APPLICATIONS

It is significant that this area of exploitation was indicated right
at the earliest period of development of the mass-spectrometer. J.J. Thomson
in 1913 \(^1\), having carried out several investigations of the parabolic spectra
of hydrocarbons using electrical detection, stated,

"I feel sure that there are many problems in chemistry which
could be solved with far greater ease by this (the mass-spectrometer)
than by any other method."
In a recent interview, G. P. Thomson comments about his father, "Yes, he (J.J. Thomson) definitely envisaged it as having implications for chemistry, as a means of analysis and detection because of its very high sensitivity."

However, the first World War intervened, and the subsequent rapid advances in isotopic investigation led Aston to the conclusion that there was no use or future for mass-spectrometry in chemistry.

As discussed previously, the initial crucial developments in organic applications came from the urgencies of developing techniques to cope with analysis of oil - ie: petroleum hydrocarbons - via the discovery by Hoover and Washburn that each different hydrocarbon, or combination of hydrocarbons, when subjected to mass-spectral analysis, produced a unique 'cracking pattern', which can be considered analogous to a 'finger print'. This discovery by Hoover and Washburn made it possible to detect molecular species not only because of differences in their masses, but because of differences in the way their ions fragmented. However, the 'take-off' period for organic chemistry only occurred after the building and marketing of reliable commercial mass-spectrometers.

A problem which was originally considered a nuisance, that of dealing with mixtures of a large number of constituents, now became a central area of research. Organic chemists found that mass-spectrometers similar to those developed by physicists for the precise determination of atomic masses could be used for studying molecular structure in complicated organic compounds. When an electron strikes a complex molecule, a wide variety of fragments are formed. In a heavy hydrocarbon, there are a large number of carbon and hydrogen atoms and possibly atoms of other elements as well. As an example, if one observes an ion fragment at the mass number 387, its
composition may not be uniquely determined because obviously many various combinations of the masses of hydrogen, carbon and other elements add up to give the number of 387. However, if one determines the number 387, not just to the nearest integer, but to 6 or 7 significant figures - i.e.: 387.06492 - then there may be only one combination of masses which gives this exact mass since hydrogen, carbon and the other elements which go into the fragment deviate in different amounts from whole numbers.

With the benefit of hindsight, perhaps Aston's most important discovery was that the relative masses of atoms were not exactly integral numbers, but deviated from integers in a way which reflected nuclear binding energies.

9.3 SOLID STATE APPLICATIONS

The other major area of post-war scientific advance was the growth of the electronics industry, which at that time was looking for substitutes for glass-enclosed, thermionic radio valves to aid miniaturisation of electronic equipment. The breakthrough in solid-state electronics came with the invention of the transistor, following successful analysis and control of very low levels of impurities in such substances as Germanium.

9.4 DEVELOPMENT OF COMMERCIAL MASS-SPECTROMETERS

As previously discussed, the first commercial mass-spectrometer was developed by Hoover and Washburn and sold by their company, Consolidated Engineering Corporation in 1942. This first instrument, the CEC 21-101 (5" radius, 180° direction focussing), began a trend that soon placed these instruments in many major laboratories (by 1950 CEC had sold over 300 machines), and it is remarkable that after more than thirty years, the instrument remains basically unaltered (now the CEC 103C), and is still marketable and popular, and has become an industrial standard, particularly in the petroleum industry.
While Harold Washburn was the driving force behind CEC's decision to manufacture mass-spectrometers in the USA, the British involvement in commercial instruments was largely brought about by the efforts of Jack Blears, at Metropolitan-Vickers (M-V) research laboratories (which in 1929 became Associated Electrical Industries - AEI), and was subsequently taken over by GEC (1968) and recently by Kratos, an American company in September 1976.

Blears, who first went to M-V before the war, was influenced by C.R. Burch's work on development of high vacua, carried out in M-V's laboratories in the twenties (Burch invented the oil diffusion pump in 1928 after the discovery of Apiezon oils.) Blears' work was concerned with the development of ionisation gauges and vacuum pumps, and through this research saw the possibilities of building a cheap, reliable commercial mass-spectrometer. Oil diffusion pumps made the mass-spectrometer much cheaper to operate, but it was still necessary to use a refrigerant in addition.

The first decision by M-V to build a machine - the M.S-1 was taken in August 1944 and the first one was operating in 1945. These instruments were designed for the analysis of uranium isotopes (part of AEI's contribution to the War effort), and four M.S-1s were made. The first went to G. Pickavance at the University of Liverpool, the second to the Medical Research Council and the other two to Tube Alloys, which later became the Atomic Energy Authority at Harwell. Blears freely admits that the M.S-1 was more or less a direct crib of Nier's 1940 machine, and therefore was mostly an exercise in learning and building.
The decision to make the M.S-2 was made in 1946. This became the first fully commercial general-purpose machine, and it was designed to accommodate not only uranium isotopes but also hydrocarbon analyses and appearance potentials. The prototype was built and exhibited at the Physical Society Exhibition in 1948, and the first M.S-2 off the production line was delivered to Oxford University in 1949. The M.S-2, an all-purpose, work-horse machine was extremely successful. Over 150 were made and manufacture continued until 1968. The M.S-2 for many years was the British competitor against the CEC 103C. One of the principal virtues of the M.S-2 was the elimination of glass components, which were still incorporated in CEC's instruments. The M.S-2 was a sector instrument, while the 103C was 180°. The latter had better ion optics, and would detect hydrogen, although its resolution was not so good and it was much heavier and bulkier. It was largely through the wide availability of these two apparatuses that the boom in organic chemical applications took place.

However, not all early mass-spectrometric fields stimulated commercial growth. By the early fifties, isotopic research was of less interest as atomic energy was proving something of a disappointment, but later interest in isotopes revived with investigations in geological dating and isotope tracer experiments. Similarly, research into ionisation and dissociation of molecules did not greatly stimulate manufacturers. It was still the situation that physical chemists required custom-built instruments, and they tended still to build their own (Eg: A.J.B. Robertson at this time was building field-ionisation instruments with razor blades in the sources). During the rest of the 1950s, the pattern remained the same. There was little innovation in commercial instrument design - the main areas of development effort were cheapness and reliability, not sophistication, and the commercial firms marketing mass-spectrometers placed far less emphasis on publication of instrument developments than academics had done previously.
In 1951, AEI brought out a third machine, the M.S-3, which was basically a cut-down version of the M.S-2, and has been described as 'cheap and nasty'. About fifty were made, however, and it has undoubtedly been a commercial success, although technically it broke no new ground. The M.S-4 which followed was a low resolution machine designed for breath analysis. It was poorly designed and it required skilled operators who were not available in hospital environments, and it was a commercial flop, only about eight machines being made.

The M.S-5 was designed for isotope analysis. Although only incorporating a single collector, it had a sliding bar vacuum lock for speedy handling of samples, and a surface ionisation filament. The M.S-5 was intended to handle samples of transuranic isotopes no greater than $10^{-9}$ grams; it was a joint venture with the Atomic Energy Authority who built the electronics to AEI's specifications, and the first machine was available in 1954. The M.S-5, with its very high resolution, was later adapted to take a spark source, and evolved into the M.S-7.

The M.S-6 was a helium leak detector produced in a rush contract for the Atomic Energy Authority. It used the M.S-3 tube which was much larger than necessary, and about 15 unwieldy machines were made.

The M.S-7 originated with the realisation that mass-spectrometric analysis could be vital to the development of semi-conductor technology. The M.S-7 was a spark source, double focussing instrument, incorporating Mattauch-Hertzog optics, and designed specifically for trace element analysis in solids. Although the decision to build the M.S-7 was taken in 1953, it was not until 1957 that the first apparatus was available. However, the M.S-7 proved to be an extremely successful and important instrument, and it still has not been superceded.
Meanwhile, CEC had been producing their equivalents, bringing out the CEC-620A cycloidal mass-spectrometer for simple analytical applications, and for process and stream monitoring, and their equivalent version of AEI's M.S-7, the CEC 21-110 which also used Mattauch-Hertzog geometry.*

The M.S-9, used for organic application, proved to be a watershed not only in commercial development but also generally in organic chemistry. It arose out of the M.S-8, conceived in 1953 at the request of J. Beynon who wanted an instrument which would have a resolving power of 3000 and an abundance discrimination of one million to one at that resolving power. Blears persuaded Beynon to accept a much higher resolution, but lower abundance discrimination, and the result was the M.S-9. This used Nier-Johnson double focussing geometry, incorporated peak-matching techniques and became available in 1961. The original M.S-9 had a resolving power of approximately 1 in 2,000,000 which made possible the determination of molecular formulae by measurement of mass defect. This meant that the whole system of degradation experiments - the basis of organic chemistry since the nineteenth century - was finally superceded, and with it Beynon developed the art of using high resolution mass-spectrometry for organic analysis. The M.S-9 was commercially and scientifically very successful.

* CEC's venture with a modular mass-spectrometer including a spark source for solid state work was the brainchild of C.E. Berry. Unfortunately, this proved to be a total failure and was thought by many to have contributed to Berry's tragically early death.

It is interesting to note that the lag of the Russians in the developing field of solid state technology was at least partly due to their inability at that time to build an adequate spark source mass-spectrometer. They attempted to obtain a M.S-7 from AEI, but owing to the prevailing political cold war situation, the USA State Department put pressure on AEI not to sell, and in fact produced a regulation which stated that mass-spectrometers could only be sold behind the Iron Curtain if their tube radius was less than 5". (Blears, however, believed that this was because the AEI M.S-7 machine was 6" radius whereas the CEC instrument was only 5" radius). The Russians eventually obtained an M.S-7 smuggled via Bahrein, but servicing problems limited its usefulness.
It was at this time, late fifties to early sixties, that other companies in other countries began to take an interest in what was becoming a very fruitful field for commercial exploitation.

In Germany, the Atlas Company produced the CH-4, a medium resolution sector instrument designed to have wide application, and be a competitor to the AEI M.S-2 and the CEC 103C. The M-86 model produced by Atlas was designed for chemical analysis of lower molecular weight compounds and for isotopic ratio measurements. The Atlas UF mass-spectrometer was specifically designed to determine the $^{235}U/^{238}U$ isotopic ratios with UF$_6$ samples, although other corrosive gases can also be studied in the UF. Atlas also produce the SMI, a double focussing mass-spectrometer.

Hitachi of Japan has been building mass-spectrometers since about 1952, and has marketed several instruments. Their model RMD-3 is restricted in its mass range to 0-14 amu since it was built specifically to analyse hydrogen/deuterium combinations. Hitachi's RMC-1 is a lower resolving power instrument, whereas the RMU-6A has a much increased resolving power and versatility, and was marketed until recently in the USA by the Perkin-Elmer Corporation. The RMU-6D, similar but more powerful than the RMU-6A, accommodates accessory units which convert the single focussing instrument into a double focussing mass-spectrometer with a resolution greater than 12,000 and a mass range extending to 1500 amu.

Mitsubishi Electric manufactures the M.S-415, a medium mass range and resolution machine suited to the analysis of gaseous and liquid samples of lower molecular weights.

Nuclide Analysis Associates of the Nuclide Corporation in Pennsylvania have produced a large variety of mass-spectrometers, many of them custom built. Currently they market the model G-1, suited to high resolution and
high vacuum analysis. The RMS-2, and the RSS are suitable for isotopic ratio determination and gas analysis. The very high resolution Nuclide model 12-60 is a modular instrument, appears either under HT or SU variations and is applicable in practically every area of mass-spectrometry activity. Coupling two 90° deflection sector mass-spectrometers in tandem produced the Nuclide T2D/TDD models, with an abundance sensitivity of over one million for the uranium isotopes.

To return to AEI, in 1960 they began to market the M.S-10, a small mass-spectrometer with a 1" radius tube which was designed to be used for partial pressures and residual gas analyses, and respiratory gas analysis. In fact, with well over 500 models sold, it has proved as much a success as the M.S-4 was a poorly designed commercial failure.

The miniature mass-spectrometer market has since been taken over by the highly successful quadrupole mass-spectrometer which in fact was first offered to AEI by Paul in 1954. They turned it down because they thought that the small sector instrument was at least as versatile and efficient as the quadrupole. It should be realised that the rise of the new firms involved in manufacturing mass-spectrometers came about largely through their expertise in applying sophisticated electronic techniques to already well-tried mass-spectrometer engineering, pioneered by CEC and AEI.

In connection with the decision not to go into quadrupole mass-spectrometers (which, nevertheless) with other companies became a huge success), Blears was quite certain that AEI made a commercially correct decision, although they have been much criticised for this. After all, AEI at that time was essentially a Direct-Current (DC) engineering company with fairly primitive ideas about solid state electronics, and a lack of
radio-frequency (RF) experience and expertise. It is always dangerous to overstretch research resources. The Science Research Council proposed to AEI that they should develop and market a commercial NMR machine, the RS2. Blears rejected this idea, but he put the proposal to his department, who voted to go ahead with what proved a totally disastrous project. Only one of the eighteen instruments built and sold (SRC refused to allow their grants to be used for foreign instruments) operated for more than a short period.

AEI in 1962 first produced the M.S-12, a very large instrument designed to handle chromatographic samples. It incorporated the magnetic sector of the M.S-9 but the electric sector was omitted. At AEI, very little instrument development took place between 1962 and 1969, but since then, John Halliday has developed the double beam machine, M.S-14, which was authorised in 1967 and appeared in 1972.

The Bendix Corporation of Ohio pioneered the commercial exploitation of the time-of-flight mass-spectrometer - the 12-101 - which is extremely versatile, although not of very high resolving power. It does not depend on magnetic fields. Consequently there are no problems with hysteresis and thousands of spectra can be scanned per second. Magnetic scanning on a conventional instrument takes about 30 seconds per scan; electrical scanning about 1 second per scan. Quadrupoles will scan about 10 times per second. Time-of-flight mass-spectrometers are used in a variety of ways, including stream gas chromatography, sample analysis, high temperature studies and first reaction kinetic investigations. As a sub-community, time-of-flight mass-spectrometers hold occasional conferences, the proceedings of which are published under the title "Dynamic Mass-Spectrometry".
We have now reached the position where commercial mass-spectrometers have become a multi-million pound business. The total market in the USA is estimated to be 15-20 million dollars annually, AEI's annual revenue was in the region of five million pounds, for mass-spectrometers in the price range £3,000 to £50,000 and the total amount is roughly split between high, intermediate and low resolution instruments.

Throughout the 1960s the main developments in commercial mass-spectrometer technology were mainly in miniaturisation of the 'software' - i.e: data processing and analysis - as RF techniques became better understood.

These developments resulted in the quadrupole mass-spectrometer (QU), followed by the monopole mass-spectrometer (Perkin-Elmer were interested in this and carried out some of the instrument development but they never, in fact, went fully commercial with the monopole).

Increased portability has made it possible for mass-spectrometers to be incorporated in space probes (eg: Viking), but another far-reaching consequence of portability, miniaturisation and increasing cheapness of different kinds of commercial mass-spectrometers has been in physical chemistry applications where a chemist can now build a mass-spectrometer into his apparatus, whereas previously different types of chemists went to the one mass-spectrometer in the laboratory for analysis of their samples.

Another interesting feature of the increasing availability of commercial machines is that it has created a rising stock of obsolete machines (most of them cast-offs from organic chemists) which the physical chemists were able to take over and suitably modify for their experiments. This is evident in the boom in physical chemistry applications, notably in the field of ion molecule reactions.
Most recently, in the 1970s, the main area of instrument development has been the link up of the mass-spectrometric technique with other kinds of analytical procedures, the most spectacular of these being gas chromatography, and the package of G.C./M.S. instrumentation is capable of extremely precise, sensitive and accurate analysis. Also, the seventies has seen extensive computerisation of the data analysis and processing sequences. One might ask at this point, how has the traditional do-it-yourself physical chemist retained his self-respect in a scientific world increasingly inundated with a bewildering variety of commercially available instrumentation apparently capable of performing whatever esoteric problem he cares to dream up? Threatened by his 'art' becoming available to the hoi polloi of the scientific community — and the majority of scientists do not wish their particular techniques to be used by everybody (like doctors, lawyers, etc., there is a certain professional cachet in exclusivity) — physical chemists have achieved their goal via a process of continually modifying and adding to basic instrumentation. This has resulted in such developments as molecular beam systems, pulsed sources, and the tandem mass-spectrometer, which will be discussed in the next chapter.
REFERENCES

10.1 Introduction

In the previous chapters, the evolution of the community of mass-spectrometrists has been described. By the end of the nineteen-forties, when mass-spectrometers became commercially available, the community achieved a degree of maturity. The first major conference in the UK was held in 1959, although the community did not acquire its own journals until 1968. Meanwhile, as the main community grew, so sub-communities developed within it. It has been shown in previous chapters how there were sub-communities even in the earliest days - Aston in England, Dempster in America, Costa and Moran in France constituted a diffuse group interested in isotopes, and they had little or no contact with Smyth and Barton at Princeton, and Hogness and Lunn in California who were investigating ionisation phenomena.

The development of these early sub-communities, however, is confused by the development of the community as a whole, and it is proposed in this chapter to describe the evolution of the sub-community of mass-spectrometrists interested in the reactions between gaseous ions and molecules. At the end of the chapter the beginnings of an even newer sub-sub-group interested in the dynamics of ion-molecule collisions will be discussed.

The growth of number of publications in the field of ion-molecule reactions is shown in APP.II.21. The term "ion-molecule reaction" (IMR) is defined more rigorously in a review by Friedman and Reuben but it may be taken to refer to investigation of the kinetics and thermochemistry of such processes as:
i) \( \text{H}_2^+ + \text{H}_2 \rightarrow \text{H}_3^+ + \text{H} \)

ii) \( \text{CH}_4^+ + \text{CH}_4 \rightarrow \text{CH}_5^+ + \text{CH}_3 \)

iii) \( \text{CO}^+ + \text{CO} \rightarrow \text{C}_2\text{O}_2^+ + 0 \)

when they take place in the gas phase.

10.2 EARLY ION-MOLECULE OBSERVATIONS

The "take-off" year for papers on ion-molecule reactions was 1951, and their number has grown rapidly since. Observations of ion-molecule reactions, however, go back to J. J. Thomson who obtained three parabolas from his sample of hydrogen corresponding to m/e values of 1, 2 and 3. He also obtained a parabola at m/e = 19 from water samples. He attributed the hydrogen parabolas to hydrogen atoms, hydrogen molecules and a new polymeric form of hydrogen \( \text{H}_3^+ \). This last he originally considered to occur "only under certain conditions of pressure and current" but later he changed his mind and suggested it was "more stable than ozone" and could combine with both oxygen and mercury under the influence of an electric discharge. He pointed out that it was not possible to reconcile the existence of this substance with the ordinary conception of valency if hydrogen is always regarded as monovalent.

Thomson later obtained the mass-spectrum of neon and noticed a faint peak at m/e = 22 in addition to the expected one at m/e = 20. He originally thought the line was due to a new element but later, possibly under the influence of Lindemmann, he preferred to consider it a new compound \( \text{NeH}_2 \) by analogy with hydrogen. He was thus, broadly speaking, correct about hydrogen but wrong about neon.

In 1916, Dempster showed that the hydrogen parabola at m/e = 3 was indeed \( \text{H}_3^+ \), that it was not a stable species (\( \text{H}_3 \) does not exist) and that it was formed only under conditions when hydrogen was dissociated. Dempster noted especially the variation with pressure in the proportion of \( \text{H}_3^+ \) formed.
and this criterion was the principal one subsequently used to differentiate between primary ions, and secondary ions resulting from ion-molecule collision processes.

Dempster's result was confirmed by Aston in 1920 by accurate atomic weight measurements, and the work of Smyth, and Hogness and Lunn showed correctly that the mode of formation was via the ion-molecule reaction

\[ \text{H}_2 + \text{H}_2^+ \rightarrow \text{H}_3^+ + \text{H}. \]

It was the first process of this type to be postulated. Hogness and Harkness found a different method of distinguishing secondary from primary ions by variation of their path length through un-ionised gas.

These observations served more to inhibit than to stimulate progress in the field, because ion-molecule reactions were not considered as interesting processes to be studied from the standpoint of chemical kinetics, but as annoying phenomena hindering the mass-spectrometrist in his primary tasks of isotope identification and chemical analysis. The early efforts were directed toward the suppression rather than the investigation of secondary ions and this was achieved by operation of mass-spectrometers with the distance between the point of ion production and the exit slit from the ion source made as short as possible and by the use of low pressures.

Various other reasons can be adduced for the failure of chemists and physicists to study ion-molecule reactions before the Second World War.

1. In order to investigate the small yields of ions produced by the collisions in gases at low pressures, it was necessary to have sensitive ion detection techniques and to improve high vacuum techniques. The technology available in the twenties and thirties was probably inadequate for detailed investigation of ion-molecule reactions, though much more could have been done than was done.
2. Until the discovery of free radicals by Paneth in 1929, chemists were highly suspicious of species which did not obey the normal rules of valency. Until free radicals were established as acceptable chemical species, it was perhaps unlikely that gaseous ions, which are really free radicals with an electric charge, would become "respectable".

3. The number of mass-spectrometers was very small in the early days and were mainly used for mass determination and isotopic analysis. Many physicists were interested in electrical phenomena in gases, the conduction of electricity through gases and the related ion collision processes, but the emphasis was on the electrical nature of the phenomenon rather than the chemical nature of the ions and molecules involved.

4. Furthermore, the gap between chemists and physicists was very wide. In 1958, Gioumousis and Stevenson made a major contribution to the theoretical understanding of the rates of ion-molecule reaction by rediscovering and developing some work published by Langevin, one of the bright young men who had worked with Thomson in Cambridge half a century earlier. This paper was designed to meet the needs of physicists interested in electrical phenomena in gases. Eyring, Hirschfelder and Taylor in 1936 used statistical rate theory to derive a result identical with that of Langevin without reference to his pioneering effort.

With the small bibliography that these distinguished chemists faced, their oversight can be explained only by the gap between the professional communities.
The thirties saw virtually no publications on ion-molecule reactions apart from Eltenton's observations of \( N_2^+ \) in \( N_2/H_2 \) mixtures in 1938\(^\text{13} \) and \( CH_3^+ \) in methane \(^\text{14} \). There was also a brief study by Tate et al. of \( H_2O^+ + H_2O \rightarrow H_3O^+ + OH \).  

10.3 POST WAR DEVELOPMENTS  

A few years after the war, a third method was found by which primary and secondary ions could be distinguished. The rates of almost all IMRs drop rapidly as the relative velocity of the ion and molecule increases. Washburn, Berry and Hall\(^\text{16} \) found that they were able to suppress the \( H_3O^+ \) ion in the mass spectrum of water (from \( H_2O + H_2O^+ \rightarrow H_3O^+ + OH \)) by increasing the electric potential used to drive the ions from the ion source. Their objective was to determine low concentration of HDO in water but their method forms a basis for the determination of reaction rates. They did not recognise this aspect of the technique at the time.  

By 1955, a small body of knowledge on ion-molecule reactions had been assembled and such workers as Franklin, Field and Lampe\(^\text{17} \) in the USA, Tal'rose\(^\text{18} \) in the USSR and Lindholm\(^\text{19} \) in Sweden had already published their first papers in a field to which they were later to contribute prolifically. Nonetheless, their contributions were qualitative, and the break-through came as a result of the work of D.P. Stevenson at the Shell Research Laboratories in Emeryville, California. The crucial experimental paper was by Stevenson and Schissler\(^\text{20} \), in which the rates of ion-molecule reactions between various
monatomic or diatomic species were measured, and these results were correlated with a simple and elegant model put forward by Gioumousis and Stevenson.

Stevenson and his co-workers realised that ions were atoms or molecules from which an electron had been displaced and that consequently they had an odd number of electrons, the same as free radicals. Their reactions would then have little or no activation energy and the problem of calculation of rates would reduce merely to the calculation of a collision number. Gioumousis and Stevenson were able to do this with a kinetic model developed by Langevin. They were also able to deconvolute the experimental results of Schissler and Stevenson so that the measured rates obtained from ions with a range of velocities could be converted to a series of microscopic rates at single velocities. It was possibly fortunate that Schissler and Stevenson chose simple systems for their measurements. Other workers who examined polyatomic species would have had more difficulty correlating their results with a simple theory.

It is interesting to speculate as to what directed Stevenson's interest towards this area. He was an industrial scientist who had not previously made any very impressive discoveries and he was relatively old for a big discovery: (Stevenson was 41 when the first paper in the field appeared). It seems possible that the germ of an idea may have come from Eltenton, possibly passing on to Stevenson the hint as to the sort of problem on which Tal'rose was working in Russia. (Note that the availability of Russian literature in the US was very limited and Russian results were not available to American investigators until after they had begun publication.)
Whatever the inspiration for the Gioumousis/Stevenson theory, its effect was to increase interest in ion-molecule reactions. The success of the new field in becoming established can be seen in hindsight as having a number of causes:

1. Unlike previous decades, chemical theory was in a position in the fifties to embrace the new field. Free radicals were well understood and reaction kinetics was passing from a period when overall complex reactions were studied and mechanisms deduced to a period in which individual rate constants of very fast reactions could be established.

2. Gas phase kineticists were becoming 'kings' of physical chemistry. Hinshelwood and Semenov had made their reputations before the Second World War and were rewarded with a shared Nobel Prize in 1956. Norrish and Porter received Nobel prizes in 1967 for work done in the fifties. A long list of gas phase kineticists appointed to chairs of physical chemistry could be compiled and the field was in some ways becoming overcrowded. The students of this new elite were busy looking around for fields of kinetics which were not too crowded and offered the chance of some easy pickings.

3. A number of mass-spectrometrists who were employed really as machine minders and high-grade technicians who could carry out routine organic analyses decided that they wanted to do something else. In universities, the gas phase physical chemist in residence was often put in charge of the mass-spectrometer and again felt that something other than routine work would provide him with greater credit and professional advancement. Many older mass-spectrometrists also saw the performance of non-routine work as a way of regaining the professional 'mystique' of which the development and widespread use of the analytical mass-spectrometer had largely deprived them.
It is tempting also to say that the improvement in vacuum techniques, ion detection techniques and general electronics in the early fifties was a major contributory cause. These developments must have played a part, but it was not as great as might be imagined. More important was that the fifties saw the first commercial mass-spectrometers either being replaced or augmented by newer models. Much of the early ion-molecule research was performed on machines which were not required for routine work, and attempts to mix the two were not successful. Polar organic molecules found in the sort of mixtures which had to be analysed adsorbed strongly on the surfaces of the mass-spectrometer and set up electrical double layers and stray potentials which made it impossible to obtain reproducible results for low energy IMRs. Indeed, even the generation of consistent mass spectra from simple hydrocarbons required careful conditioning of the ion source and carbonisation of the tungsten filament under controlled conditions. Friedman carried out most of his early work at Brookhaven National Laboratory (where he was originally employed to take charge of mass-spectrometry services in a department devoted primarily to radiochemical work) on a CEC-103C Mass-spectrometer. Nothing but hydrogen and rare gases was permitted in the instrument for two years during which period it gave reproducible results. (A sample of pyridine was once run during one of Friedman's absences and rigorous cleaning and several months reconditioning were required before reproducible results could again be obtained.) In the early days, therefore, the boom in IMR research was probably due more to the availability of obsolescent mass-spectrometers which could be used specifically for research (even when they were based on valves rather than transistors) than on the latest technological advances.

A further reason for interest in IMRs was that the Gioumousis-Stevenson theory had predicted the rates on certain IMRs to within 2% of the measured values. This was dramatically better than theoretical
calculations of the rates of reactions between neutral species where agreement within a factor of ten was regarded as quite good.

The point at which the researchers into ion-molecule reactions became a distinct community is difficult to establish. At the ASTM E14 Committee on Mass-Spectrometry in Los Angeles in 1959, there was only a single IMR paper. At that time, only a handful of groups were involved - those of Stevenson in Emeryville, California, Tal'rose in Moscow, Franklin and Field at Humble Oil Company, Texas, Durup in Paris, Hamill at Notre Dame, Indiana, and Friedman at Brookhaven National Laboratory, Long Island. A handful of review articles appeared at the end of the fifties. Meanwhile, IMR research was not really seen as part of reaction kinetics but more as a branch of mass-spectrometry and it is curious that the sub-community of IMR workers is still part of an instrument-based speciality rather than having switched to a theory-based one. This is not to everyone's satisfaction. A determined effort by K.R. Jennings persuaded the organisers of the Third International Symposium on Reaction Kinetics held at Brussels in 1973 to include a section on IMRs but movement in that direction is slow. In the UK, the editors of Transactions of the Faraday Society resisted publishing papers on IMRs throughout the sixties on grounds which were complicated but in fact was because they did not accept IMRs as a branch of physical chemistry. Consequently, some British papers appeared in Zeitschrift für Naturforschung which took a broader view*.

* Allan McColl writes:

"Mass-Spectrometry as a discipline in its own right transcends the normal sub-division of chemistry not only as an analytical tool but also in its guise of gas-phase ion chemistry."

It is interesting to note that the MTP International Review publishes articles on the chemistry of gaseous ions both in volume 5 (Mass-Spectrometry) and volume 9 (Reaction Kinetics).
In this connection it is interesting to note that few of the workers on IMRs had previously established reputations in the field of reaction kinetics, although many were the students of eminent kineticists. K. R. Jennings is one example in the UK but his IMR interests were unlinked to his kinetics interests and replaced rather than supplemented them. F. Kaufman at Pittsburgh, on the other hand, developed IMR interests alongside his 'reactions of gas phase free radical' interests and he is probably the only important scientist currently working in both fields simultaneously. Even here, however, it need not be assumed that a theory-based speciality is replacing an instrument-based one. Kaufman had been active in developing a flow method for following the rates of reactions of free radicals and this has much in common with the 'flowing afterglow' method for the rates of IMRs developed by Fehsenfeld, Schmeltekopf, Ferguson and Schiff at Boulder, Colorado. Indeed Schiff had been one of the early workers on what later became known as the atomic titration method for estimating radical concentrations which closely related to some of the flow methods used by Kaufman. Thus, it is probably more correct to see Kaufman as employing his expertise on flow systems to look at IMRs rather than his interest in IMRs stemming from their similarity in theory to radical-molecule reactions.

Returning to the late fifties, Gioumousis and Stevenson's theories initiated wide interest in IMRs. Most workers were concerned with extending the theory to polar molecules and testing it under different conditions. The results were not encouraging and the $\text{H}_2/\text{H}_2^+$ and $\text{H}_2$/rare gas reactions appeared to be exceptions to general observations. Rates of reaction were frequently smaller than predicted by the G-S theory and the dependences of rate on ion velocity were quite wrong. Only the Brookhaven group under Friedman provided much support for the G-S theory and by 1964 it was widely felt that the theory needed to be discarded rather than amended. These doubts were expressed by Henchman, for example, in a 1965 review.
The small and embattled Brookhaven group (D.P. Stevenson having left to take an administrative job) claimed privately that much of the experimental work done by other groups had been carried out in systems of peculiar ion optics and doubtful cleanliness, that the investigations had been insufficiently thorough, and that as the opposition had no alternative theory to put forward, the Brookhaven group would remain with the one they had — "You can't fight something with nothing". In 1966, Friedman and Guidoni published an exhaustive study of the methane system which, at the time, was the best substantiated example of a system which did not conform to the G-S model. They were able to show that the reaction

$$\text{CH}_4 + \text{CH}^+ \rightarrow \text{CH}_5^+ + \text{CH}_3$$

appeared to occur at least that the predicted rate because some of the CH\textsubscript{5}\textsuperscript{+} decomposed before detection to give CH\textsubscript{3}\textsuperscript{+} and H\textsubscript{2}. This had not previously been noticed because the CH\textsubscript{3}\textsuperscript{+} was masked by primary CH\textsubscript{3}\textsuperscript{+} ions resulting from the impact of electrons on methane. If this additional reaction channel was taken into account, the overall rate came out to the value predicted by the G-S theory and the dependence of rate on ion velocity also fell into line. (The last article seriously critical of the G-S model was put forward by Henchman in 1972, but by 1974 he had recanted.) This experiment served to dispel many of the doubts and reservations surrounding the G-S theory, and the conclusions were strongly reinforced by subsequent experiments with more sophisticated apparatus, particularly the flowing afterglow experiments of Ferguson et al. There is now general agreement that the G-S model provides an accurate collision number for IMRs and that rates which deviate from its predictions do so for specific reasons which are open to investigation.

10.5 SUBDIVISIONS WITHIN THE I.M.R. COMMUNITY

By 1966 the number of ion-molecule reaction papers was between 50 and 100 a year, and the field was already beginning to be divided between the various groups. Friedman and Reuben arranged their long review article
they illustrated the sub-divisions of IMR kinetics research by the diagram shown. This 'fine structure' within scientific communities is not obvious to the observer from outside and its delineation requires careful and informed reading of the literature. The factors which cause a scientist to establish his own sub-discipline appear to be:

1. The desire not to compete, especially when others have been in a field longer than one has oneself.
2. The desire to break new and exciting ground rather than having first to repeat someone else's work - the desire to be original.
3. The desire to avoid having to read too much of the literature.

Factors pressing in the opposite direction:-

1. Rivalry, friendly or otherwise, with colleagues. This seems more prevalent among world-acknowledged experts. Minor figures tend to reassure each other rather than seek conflict. Major figures tend to work on what they see as mainstream problems and are confident that they can recover after a late start. They are often contemptuous of competitors' methods and are convinced they can do better.
2. Natural conservatism. Many scientists, having worked in one field and achieved a degree of expertise, are reluctant to change, perhaps because of lack of confidence. Many scientists spend their whole lives working on problems which arose out of their PhD theses, long after those problems have ceased to be meaningful. Such people are rarely successful at attracting research students and the theories they propound disappear not because they are disproved but simply because the scientists who believed in them or thought they were important die or retire. The number of scientists who actually change their minds about a major theory may well be quite small but the 'false' ideas disappear nonetheless. (Take, for example, three cases of the disappearance of sub-sub-communities all for
Fig. 6. Flow sheet indicating genetic relationships between problems investigated in the area of ion-molecule reaction kinetics, specific questions asked and techniques developed for the experimental attack on these problems and questions. For example, in the dynamics of collision processes the question of complex formation has been studied in beam experiments without mass analysis, in pulsed source experiments with velocity analysis, and in more sophisticated beam experiments with mass analysis and angular and/or velocity analysis. The general questions concerned with kinematic studies and cross section measurements are identified, but their isolation is obviously arbitrary. The relationships shown are designed to indicate the response of the experimentalist to questions of current concern in the field of ion-molecule reaction kinetics. Drift tube experiments and merging beams are not connected in this scheme although the latter are definitely related to kinematic studies while the former play a role in mobility measurements which for the present are related to cross section studies.

Subdivisions within the I.M.R. Community

different reasons: J. J. Thomson's work on the 'plum pudding' model of the atom after 1920 is of no importance; no one ever finally published the Landé energy diagram for Argon because the field became unfashionable; no one ever properly worked out the mechanism of the dry carbon monoxide/oxygen reaction because, although of tremendous interest, it was too intractable.)

The size of a sub-sub-community within a scientific sub-community is quite impossible to determine and depends a lot on definition. If we take it as the number of papers produced per year on broadly the same problem, then in mass-spectrometry sub-sub-community one could typically name five research centres publishing about twenty papers per year between them and involving perhaps ten established scientists and a number of research students, technicians, etc. After a few years, the sub-sub-communities may disappear having solved their problem or they may grow and sub-divide: they are, in fact, much more labile than the larger communities of which they form a part.

10.6 DYNAMICS OF ION-MOLECULE REACTIONS

Let us examine the sub-sub-community of IMR scientists who worked on the dynamics of IMRs. The topic has been reviewed by Friedman and Reuben and by Reuben.

In its developed form, the apparatus for the investigation of the dynamics of IMRs consists of two mass-spectrometers in tandem. The first mass-spectrometer produces a beam of ions which can be selected by mass-to-charge ratio and these are then slowed down and focussed by an electrostatic lens so that they all have the same velocity. The beam is then directed into a collision chamber or across a beam of neutral molecules and the ions produced in the collision process are analysed by their mass, velocity and angle of scattering. This gives information of the nature of the complex formed in the collision process and enables the characteristics of species with lifetimes of about 10-13 micro seconds to be investigated.
The earliest tandem mass-spectrometers, however, were not built with this purpose in mind. Inghram and Heyden\textsuperscript{37} built one in the early fifties designed to facilitate the detection of rare isotopic species. In 1954 Lindholm in Sweden reported attempts to measure certain thermochemical properties of ions with a tandem mass-spectrometer contained in an evacuated ball jar 1.3 metres in diameter and 1.1 metres high\textsuperscript{38}. This made alignment of the sections easier—(there is a classic photograph of Lindholm, wearing gym shoes, inside the bell jar lining up the instrument)—but there were problems due to lack of efficient differential pumping. Two mass-spectrometers which might be classified as semi-tandem machines were built by Cermak and Herman\textsuperscript{39} in the Czech Academy of Sciences, Prague. Then in the early sixties, Giese and Maier built a genuine tandem machine with so-called 'longitudinal geometry' which they used to look at certain ion-molecule reactions but which lacked facilities for angular or velocity analysis of the product ion beam\textsuperscript{40}.

By the early sixties, therefore, a few tandem or semi-tandem mass spectrometers existed but they were cumbersome, not very versatile, and beset by pumping problems. To improve on these required the invention and production of the small highly portable quadrupole mass-spectrometer and the improvement of ion counting techniques to the point at which single ions could be detected and statistical analysis of results carried out automatically.

Before this occurred, however, Henglein and Muccini\textsuperscript{41} had built a machine similar to that of Cermak and Herman and the results derived from it led them to suggest the idea of "stripping". They worked at the Radiation Research Laboratories of the Mellon Institute, Pittsburgh, Pennsylvania, and at the Hahn-Meitner Institut für Kernforschung in Berlin. To test this idea,
they built a new apparatus to study the reaction

\[ \text{Ar}^+ + \text{H}_2 \rightarrow \text{ArH}^+ + \text{H} \]

and the nature of the collision complex formed in it. Because the reactant and product ions were so simple, Henglein et al. did not require a mass spectrometer at all, but used a simple ion source to produce Ar\(^+ \) and analysed their products with a simple Wien (velocity) filter. They thus overcame critical problems of sensitivity. The results showed that at the moment of reaction, far from a semi-stable ArH\(^2+ \) complex being formed which decomposed after a few hundred vibrations, the argon merely stripped off one hydrogen atom without passing any energy on the second which was left as a 'spectator'. The former reaction model had been generally accepted since the pioneering work of H. Eyring in the thirties on 'transition state theory'. The theory had never been supported by precise experimental data and its success was due partly to its elegance and partly its ability to explain qualitatively certain observations which could not be explained on the simple collision theory which preceded it. It also benefited from the "you can't fight something with nothing" attitude and from the fact that there were no other reaction rate theories to be considered.

Where did Henglein get the idea of "spectator stripping" from in the first place? It was currently in the air among physicists in that this type of process appeared to occur among certain sub-atomic species, and also work was beginning on dynamics of reactions between alkali metal atoms and certain halides where spectator stripping also occurs. Even if the idea came via workers on alkali metal/halides, however, they must have got their idea from the nuclear physicists in the first place. It is interesting to note that IMR workers were also taking
many experimental ideas from nuclear physicists* (eg: ion counting techniques) and even borrowed their shorthand for writing reactions.

Instead of the reaction being described by a traditional chemical equation, the reactant ion is written first, followed in parenthesis by the neutral reactant and the neutral product. Finally comes the product ion so that the reaction is written

$$\text{Ar}^+ (D_2, D)\text{ArD}^+.$$ 

It is difficult to establish the extent of the stir caused by Henglein and Muccini's papers. They were working in relative isolation in Germany, because almost all the IMR research was being carried out in the USA where there is in any case a tendency to look askance at work done in foreign countries. Furthermore, to improve on Henglein's work presented formidable experimental difficulties ie: the measurement of the velocities and angular distributions of product ions. Apparatus to do this could not be built overnight and even scientists who had received early warning of Henglein's results would have taken time to get started. Indeed, even Henglein himself had to build new equipment to follow.

*E.E. Ferguson, one of the Boulder Colorado group, makes a similar point in connection with the work on drift tubes, etc., which he was instrumental in pioneering. He comments that after the early IMR work performed in mass-spectrometers with modified sources, subsequent workers applied physicists' tools such as drift tubes and "afterglows". Here is a concrete example of the diffusion of techniques from more to less rigorous branches of science. Ferguson also comments that "in aeronomy, direct measurements of terrestrial ion composition and indirect data on ion composition of other planets require knowledge of many ion-molecule reactions for their interpretation" (45).

This is mentioned by Ferguson to underline the wide application and interdisciplinary nature of ion-molecule research, but implies another point. Early IMR research could be done on modified obsolescent mass-spectrometers. The new generation of experiments required extensive funding. Partly these funds (hundreds of thousands of dollars rather than millions) could come from normal research budgets - (IMR research may be 'big chemistry' but it is not yet 'Big Science') - but quite a lot of the funds in the sixties were a 'spin-off' from the space programme and interest in planetary atmospheres and processes on the nose cones of re-entrant space craft. With the run-down of the space programme, IMR workers managed to get funds instead from agencies interested in processes in the upper atmosphere (eg: the enormous atmospheric modelling programme designed partly to estimate the effect of supersonic airliners such as Concorde on the ozone layer) and also on the possibility of developing a chemical laser, a sort of 'death ray' idea of long-term interest to the military.
up his ideas.

After the 1962 and 1963 papers, nothing on the complex formation versus stripping controversy appeared until 1965 when Henglein and his co-workers published five papers confirming and extending their earlier results. In that year, two papers appeared from Turner, Stebbings et al., but their equipment could only measure angular distributions but not velocities (unlike Henglein's second apparatus which measured velocities and not angular distributions). Their results were interesting from some respects but were irrelevant to the complex/stripping controversy and these workers did not publish further in this field, moving instead to a study of IMR reactions concerned with the atmosphere.

In 1966 a seminal volume appeared - "Ion-Molecule Reactions in the Gas Phase" (Advances in Chemistry Series, vol. 58) which was the first book on ion-molecule reactions. It contained a paper by Lindholm on his thermochemical work, one by Henglein and others by Futrell and Abramson reporting work on Tiernan's tandem mass-spectrometer at Wright Patterson, and Berta, Ellis and Koski at John Hopkins University, Baltimore. Neither of these groups could measure angular and velocity distributions. Meanwhile, Bailey at Gainesville, Florida, (probably inspired by some classic early work at Gainesville by E.E. Muschlitz), had a group building the necessary equipment. The apparatus used by Vance measured only velocity spectra, but a fully developed version built by Leventhal, aided by Doverspike and Champion could measure both angular and velocity distributions and the first results appeared in two papers published later in the same year.

In September 1967, there was a Faraday Society discussion entitled "Molecular Dynamics of the Chemical Reactions of Gases" at Toronto, where Wolfgang's group at Yale, and Mahan's group at the Lawrence Radiation Laboratory, had a group building the necessary equipment. The apparatus used by Vance measured only velocity spectra, but a fully developed version built by Leventhal, aided by Doverspike and Champion could measure both angular and velocity distributions and the first results appeared in two papers published later in the same year.

Note: Note how little of this work was being carried out at University laboratories (even Mahan working at the Lawrence Radiation Laboratory is not in a totally university situation). Eg: Stevenson; Shell, Emeryville - Henglein; Institut für Keinforschung - Franklin and Field; Humble Oil Co. - Friedman; Brookhaven National Laboratory - Tiernan; Wright-Patterson Air Force Base.
Berkeley, California, presented their initial results. In addition to other papers that year from two of the four groups who now had facilities for angular and velocity distribution measurements, there were also contributions from Henchman et al and Abramson and Futrell who had less high-powered equipment.

A noteworthy feature of the experiments described above and others related to them was that the stripping mechanism appeared to be widespread if not universal. Mahan has commented that "The occurrence of the stripping process in this series of hydrogen abstraction reactions is an interesting and unifying feature. However, the prevalence of this mechanism at this time must not be over-interpreted. The grazing or large-impact-parameter collisions associated with small angle scattering and stripping produced a large total reaction cross-section. In selecting systems for our first experiments, we naturally pick reactions which have large cross-sections so that the products will be easily observed. This process almost automatically selects reactions that display something close to the stripping phenomenon". In other words Mahan states that problems with the sensitivity of instruments cause experimentalists to look first at systems which give the largest possible yield of ions and that these are exactly the systems which one would expect on a priori grounds to proceed by a stripping mechanism.

Wolfgang et al did not share this view. They succeeded in observing stripping reactions of ions with very low velocities, and regarded the prevalence of this mechanism as significant. They commented, "At lower energies, however, it has been widely thought that IMRs in general and these processes in particular occur via an intermediate complex of sufficiently long lifetime to undergo normal unimolecular decomposition, that is having a lifetime of many vibrations. The evidence for such a belief is, however, circumstantial, being based largely on a proposed interpretation of observed isotope effects."
This statement by Wolfgang's group was incautious. It is always foolish to claim that something is unlikely to exist merely because one has not found it oneself. Ignoring for the moment "the circumstantial nature of the observed isotope effects", there was no question but that complexes had been observed in several ion-molecule reactions and they had had lives of the order of a microsecond or longer. Wolfgang et al's failure to recognise the phenomena observed by these workers was because their field was slightly different from Wolfgang's own, and because complex formation had not been observed in beam experiments, which was what they were concerned with. This bears an interesting similarity to J. J. Thomson's refusal to recognise the generality of isotopes although Soddy, Aston, and G. P. Thomson and other workers found the idea already familiar. Wolfgang's group, however, recanted much more rapidly when the evidence against their view accumulated.

Much indirect evidence from complex formation occurring under certain circumstances was found, but the problem was finally resolved when Henglein's group found evidence for complex formation at low bombarding energies in reactions such as

\[ \text{CD}_4^+ (\text{CD}_4,\text{CD}_3) \text{CD}_5^+ , \text{and Ar}^+ (\text{CD}_4,\text{CD}_3) \text{ArD}^+. \]

These reactions tended towards complex formation (i.e. showed intermediate behaviour) at accessible ion energies but did not achieve it. Henglein's group then succeeded in observing the transition from pure stripping at high ion energies to pure complex formation at low energies for two reactions of methanol:

\[ \text{C}_3\text{OH}^+ (\text{D}_2,\text{D})\text{CH}_3\text{OH}^+ , \text{and C}_3\text{OH}^+ (\text{CD}_4,\text{CD}_3)\text{CH}_3\text{OH}^+. \]

The former reaction is endothermic and has a threshold energy of 0.2eV - i.e. the reaction would not be expected to go at all unless the energy of the bombarding ions is above a critical value. This is to illustrate that one of the chief difficulties with tandem mass-spectrometer experiments is the obtaining of low

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energy beams of ions. Ions, because they all have the same charge, repel one another. If focussed carefully by an electrostatic lens, a sharp beam of high energy (and therefore high velocity) ions can easily be obtained because the ions have reached their target before inter-ion repulsion has become significant. The reactions of high energy ions are not of much interest, however, to chemists who are more interested in reactions taking place at thermal energies. Consequently, the response of many chemists to the concept of "stripping" was that although it may very well occur at high energies, the process at thermal energies (where transition state theory was assumed to apply) could be very different. The problem is to obtain beams of low energy ions. Because of the inter-ion repulsion, beams tend to "explode". The early electrostatic lenses were designed by rule of thumb methods, but elaborate computer-designed lenses were later built. Wolfgang's group managed to achieve substantially lower beam energies than anyone else, and it was probably their pride in this achievement which caused them to risk the statement that the stripping mode was universal.

After Henglein's discovery, Wolfgang's group also observed complex formation and although the two phenomena are far from understood, more recent work has tended to consolidate rather than invalidate the above ideas.

It should be appreciated that at this time there were only three major groups working in this field - Mahan, Henglein and Wolfgang. The other groups with tandem mass-spectrometers: Bailey's group at Gainesville, Rutherford at General Atomic, Tierman at Wright-Patterson, Friedman (whose instrument had been built by Leventhal who had moved to Brookhaven from Gainesville), though they made significant contributions were not centrally interested in the "stripping" versus "complex formation" controversy.
The most recent review of beam experiments is "Ion-Molecule Collision Phenomena" by Mahan, and publications in this field in 1973 were in the region of 25-30 per year. The area is now being consolidated and second-order problems concerned with reaction dynamics are now being studied.

10.7 DISCUSSION

App. II.22 shows the people involved in the area by 1974. Many are not centrally concerned, many are research students and others only made a single contribution, so that while the list of names reaches approximately the 100 names that de Solla Price considers is a research "network", the number of group leaders centrally involved is only three or four.

It would therefore appear that de Solla Price's estimation of what constitutes a real community is not very satisfactory. As discussed earlier in this chapter, an investigation of the 'fine structure' would indicate much smaller working groups. Though the overall field of mass-spectrometry is well defined, it contains within it a very large number of sub-sub-communities, and the individuals in these derive considerable pleasure and intellectual satisfaction from following their personal interests by being part of a small group interested in a particular problem or technique. Perhaps we should imagine the real social network as being a 'visible family', instead of an 'invisible college', operating along idiosyncratic, cosy and incestuous paths, where, as Hume's Paradéx illuminates it, "in the family no one questions the importance of what he (or his brothers) is doing".

This family basis for research organisation and choice of topic also relieves workers from coping with the mass of literature, and anyone working on a problem will know the other teams in the field. As mass-spectrometry and its particular sub-fields grow, it also attracts bureaucrats who run the larger departments, resulting in a change in pattern from a scientist publishing by
himself, e.g. Aston, to a scientist publishing together with a post-doctoral fellow Ph.D. plus two or three Ph.D. students. This is a change, but not a dramatic one, especially if this pattern is compared with the phenomenal size in research groups and subsequent publication in particle physics.

Kuhn has commented with reference to this group organisation:

"Several readers (of his book 'The Structure of Scientific Revolution') have concluded that my concern is with major revolution, e.g. Copernicus, Newton, Darwin and Einstein. A clearer indication of community structure should help to enforce the rather different impression I have tried to create. A revolution is for me a special sort of change involving a certain sort of reconstruction of group commitments. But it need not be a large change, nor need it seem revolutionary to those outside a single community, consisting perhaps of fewer than twenty five people. It is just because this type of change, little recognised or discussed, occurs so regularly."
CHAPTER X

THE DEVELOPMENT OF THE ION MOLECULE REACTION SUB-COMMUNITY

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CHAPTER XI

DISCUSSION, RESULTS AND CONCLUSION

11.1 INTRODUCTION

It is difficult to see the development of mass-spectrometry as part of an inexorable historical process. Mistakes were made, important individuals emerged who pulled things their way, there seems to have been an important random element in the determination of proper subject material at any given time, and we can see the not-unexpected polarisation between 'problem-pull' and 'technology-push' in affecting innovation.

11.2 EARLY DEVELOPMENT

We have seen that the development of mass-spectrometry in the very early period was slow, caused by a number of factors:

11.2.1 AHEAD OF ITS TIME

One can argue that mass-spectrometry was ahead of its time, where theory lagged a long way behind the technology and experimental capability. Science was not yet ready for it in terms of interest in molecular processes: physical chemistry traditionally dealt with bulk properties of substances and compounds, (thermodynamics, electrochemistry) whereas mass-spectrometry is concerned with the examination and behaviour of individual atoms and molecules, (c.f. with the very rapid growth of gas-chromatography).

Not until chemists were prepared to consider mass-spectrometry as genuine chemistry did any advances take place in the fields of ion-molecule reactions and the chemistry of gaseous ions. A great deal of effort and time was expended in improving the machines, without improving the science, and experiments at certain periods were in some respects naive.

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One can even argue that in many ways the main impetus in the development of mass-spectrometry was to turn it into a technology, a tool, a widely applicable technique, in as much as the aim was to push forward the precision of measurement. At the point at which the basic technique becomes non-problematic and non-esoteric, we can see the rapid development of other branches of science, e.g. organic and bio-chemistry.

One can point to innumerable examples which support this hypothesis of technology pulling scientific innovation in its wake. Cardwell has argued that the rise of thermodynamic theory was brought about by the necessities of steam engine design; in sociology there has been a revolution in the past 15 years which could not have taken place without the computer. Furthermore, the microscope had existed for two hundred years before anyone did anything other than describe what they saw on looking through it. Science was insufficiently developed to exploit the available technology.

Arising out of this phenomenon, Ben-David has put forward the (rather inaccurate) notion of 'pseudo science', that is, a branch of knowledge which is not really that but a clever technique which produces publishable results, a good example of which is bacteriology. Pasteur and Koch developed techniques for growing cultures of bacteria, isolating them, preparing vaccines etc, without having any idea why bacterial activity caused disease. The technique was a long way ahead of theory and therefore the bacteriologists concentrated on refining the technique without asking awkward questions. The breakthrough, when it came, was from outside the community of bacteriologists.
There are similarities apparent in mass-spectrometry. It was a technique which could find isotopes, but until the mechanism of electron bombardment was better understood, it could be used in no other way. The field grew very slowly and the breakthrough of mass-spectrometry into chemical analysis and ion-molecule reaction research was not made by an isotope chemist.

11.2.2 INTERACTION BETWEEN SCIENCE AND TECHNOLOGY

It seems indisputable that technology is a necessary, if not always a sufficient condition for a particular scientific advance. It was an interest in isotopes which led Aston to build the mass-spectrograph, but this would have been impossible without the requisite pump technology, although he could build much of the rest of the apparatus himself. Even with the Gaede pump available, both Aston and most subsequent workers in this early period experienced enormous experimental difficulties in building a mass-spectrometer satisfactorily, let alone getting it to work and produce reliable results.

Not until the 1950's, with the advent of the commercial machine, did the mass-spectrometer become anything other than an extremely delicate, highly temperamental piece of equipment. Commercial machines stimulated work by people who lacked the ability to build their own. One important feature of this early development, which obviously assisted in overcoming the technical difficulties, was that the majority of workers had an engineering background before crossing over into experimental physics, where mass-spectrometry remained very much a physicist's tool, until the war years.
One can also see in the slow development of mass-spectrometry in the early years a state of relative isolation from the main areas of physics and chemistry. Apart from isotopes, mass-spectrometry borrowed very little from other fields, largely because this was the formative period of technical development. Because isotope detection and quantification fundamentally rely on precision and accuracy, this topic provided the necessary stimulus to concentrate on machine building. This early isolation also came about partly from the personalities of the pioneers Aston and Dempster, who both carried out their early research on a non-collaborative basis, and started the rapid subdivision of mass-spectrometry into sub-fields, of which there are now a large number.

11.2.4 PERSONALITY

It can also be argued that the slow development of mass-spectrometry was partly the result of the personal characteristics of the early giants. Thomson and Aston had no 'disciples', and not until the 1930's do we see the beginnings of consistent mass-spectrometry training, e.g. at Tate's laboratory at Minnesota in the U.S.A. On looking back over a wide range of British scientific achievements over the past 60 years, this training and teamwork aspect appears to be a problem generally, and the migration of science and scientists to other countries may reflect British difficulties in adapting to a teamwork system. By comparison, the U.S.A. scores heavily in this respect, especially when faced with urgent definite objectives, (e.g. the war-time period).

This personality consideration together with age and breadth of training is also linked to the scientists' approach to 'blind alleys', (or even 'non-blind alleys') - when a scientist who sticks to his guns in the face of all
the evidence is eventually proved right), ie, whether he is prepared to change his mind quickly when shown to be wrong. This is also a function of the work situation and the options which he sees as open to him.

It appears that both Thomson and Aston at different times were in a position where it was hard for them to change their view. For instance, when Aston was in the 'mess' during the First World War, he had no real incentive to change his views because he had no apparatus on which to confirm or refute them, and capitulation would have wrecked the evening debates. Similarly, a semi-retired Thomson, in Aston's absence, was under no pressure to change, and his dislike (because he could not follow the mathematics) of wave mechanics, forced him to back his 'plum pudding' model of the atom (the neutron still not having been discovered) which he elaborated for the rest of his life.
11.3 FURTHER DEVELOPMENTS

We have discussed some aspects of the slow early growth of mass-spectrometry. The technique was invented in 1911 but then its development was interrupted by the First World War. After resumption of research, principally by Aston in the Cavendish Laboratory and in Chicago by Dempster, the field underwent slow growth. From 1928 until the Second World War, the principal area of growth in mass-spectrometry was in instrument design. Electrical methods of detection largely supplanted the photographic technique, which contributed to greater precision. More efficient ion-generating sources were developed, and better equipment was invented for handling the samples to be analysed in the mass-spectrometer. The new mass-spectrometry groups burgeoning in America concentrated on different aspects of technical development.

However, during this period, theoretical advances were few, chemistry was still a 'non-topic' for mass-spectrometric study, and the field developed in an isolated fashion, with very little cross-fertilisation with other areas of science.

Not until the Second World War did mass-spectrometry experience the necessary changes to develop into a mature area of science. The war time concentration of manpower and of activity produced considerable interaction between different areas of science, mass-spectrometric techniques were extended into new fields, other instrumentation techniques were linked up to mass-spectrometry, and great emphasis was placed upon reliability and reproducibility of results.
The rise in the numbers and size of mass-spectrometry research groups led also to considerable changes of manpower and organisation within those groups. Very often there were two prevailing different sets of attitudes held by the scientists within such a group – those held by the senior, successful workers who debated and decided upon the significant problems in the area, and those whose role was to try to fill in the gaps in the literature.

Unlike the strong 'great men train future great men' tradition among other areas of physical chemistry, (i.e. students of G.N. Lewis, Hinshelwood, von Baer, etc), where students of big names typically inherit their master's attitudes, the development of mass-spectrometry has been far more fragmentary and peripatetic, as witnessed by the genealogy in Appendix II.1. Nevertheless, although the pioneering days of sealing wax and copper wire were over, still a large proportion of the prizes went to mass-spectrometrists who were able to build their own machines. This ability to build has been the key to success in mass-spectrometry, and we have observed in the more recent history and development that the itinerant apparatus builder is still alive and well, e.g. Hippie, L.G. Smith, Leventhal, Herman and Friedman.

Friedman, in conversation, made the astonishing observation that apart from the instrument builders, everyone else in mass-spectrometry was or had been a scientific mediocrity! In fact, there has not been a Nobel prize since Aston, for Urey, who received a Nobel prize for the discovery of deuterium, got it for optical spectroscopy.

It is also noticeable that Friedman, like certain of his predecessors, made a large number of significant contributions because he refused to have too many students. Now as Assistant Director of Brookhaven National Laboratory
he has been forced to do less research work. Bureaucratisation will often hinder scientific research work, although top scientists often end up as administrators and fund-getters, success in one area generating success in the other. We can observe another aspect of the 'Matthew Effect', where often those who sit on the most committees receive the most funds. C.P. Snow similarly commented that if he had scientific funds to distribute he would do this on the basis of building on the previous success of large research groups. However, the distribution of funds should not necessarily be rationed out on the basis of the persuasive power of the bureaucratic (ex) scientists*. One can make a case for using citation analysis as a basis for assessing the significance and worth of research of a particular scientist or topic. This would make it more difficult for the professional proposal-writers and committee-sitters; obviously science per se should be funded. Referees therefore should at least partially use this tactic, because endemically there are intense rivalries and animosities within scientific communities.

One implication of Friedman's advocacy that the most efficient size (for mass-spectrometry) of a research group was only two or three scientists and no more than five Ph.D. students, is that in view of these small 'families', the 'fine structure' within the overall community, there is always the danger of their being overlooked. On the other hand, although the general premise is that science is an altruistic endeavour, one is led on and inspired by general patterns in the outside world. One might equally ask whether the meadval alchemists were really looking for the philosophers stone.

"Footnote:

"Science is not driven onwards by experimental skill alone - politicking, grantsmanship and infighting shape the development of science every bit as much as the driving hypothesis or the painstaking compilation of meter readings."
or putting up a case for funds with which to continue their research.

11.4 VACUA AND PUMP TECHNOLOGY

If we refer back to the hypothesis that scientific advance requires the existence of requisite technology, it is evident that the refinement of pumping techniques to achieve harder vacua was fundamental to the development of mass-spectrometry. There are many other aspects of technological achievements which have allowed mass-spectrometry to break new ground, e.g., hard glass, steady voltage production, electronic circuitry, etc, but an account of early pump technology will sufficiently indicate the relationship.

As already discussed, the development of interest in electricity in the 18th century inevitably led to experiments to see whether an electric spark would pass through a vacuum. The character of the electrical discharge in a near vacuum was much more seriously investigated by Faraday who derived his vacuum by means of an air pump and recognised that there was still residual air in his discharge tube.

Although Faraday used a mechanical air pump. (his working pressure was four to six inches of mercury), most of the pumps of his time were dependent on creating a Torricellian vacuum by extracting mercury from a glass receiver. There were many forms of these pumps. More than seventy of them have been described by Silvanus Thompson, some used by such famous experimenters as Swedenberg, Joule, Helmholtz and Siemens. All these pumps had the advantage over the mechanical type in not needing sliding seals and valves; their development was primarily pushed by the imminent commercial development of electric lighting, whilst generating higher degrees of
vacuum for scientific investigation.

The most successful of these pumps were those of Töpler (1862) and Sprengel (1865). Both types could be made to operate automatically, and given time could reduce the pressure in a vacuum to that of the vapour pressure of mercury (10 - 4 mm Hg). These two pumps were used by all the X-ray pioneers, Geissler, Plücker, Hittorf, and Crookes.

As we have seen, in the late 19th century the Cavendish Laboratory under J.J. Thomson was preeminent in the field, but one can reflect that if vacuum techniques had been better, Thomson's discovery of the electron might well have been anticipated by Hertz, who, not realizing that his vacuum was not hard enough, was thereby misled into believing that cathode rays could not be charged particles. J.J. Thomson in fact repeated Hertz's experiments of 1883 with a better vacuum.

As J.J.'s son, George Paget Thomson, has said:

"Since Hertz's work, 14 years before, pumps had improved, although they were still incredibly primitive by modern standards. This improvement was largely due to the demands of the electric lamp industry, one of the many cases in which industry has returned to pure science rich payments for the benefits which it received.

"J.J.'s work was done mainly with Sprengel or Töpler pumps, backed by water jet pumps, until the Gaede rotary pump was invented in 1905. This was expensive by laboratory standards, and when I started research work in 1913 I did not get one; there were only three or four in the Cavendish. J.J.'s work on positive rays was started without them, though one was certainly in use by mid-1910."
Aston, building the first mass-spectrograph in 1919, was fortunate to obtain a Gaede pump, but he did not have a diffusion pump which were just becoming available. Aston depended for the hardness of his vacuum on absorption by charcoal in a liquid air trap, a device invented by Denvar, who discovered the principle in 1875. This method is more effective for a time than even the Gaede rotary pump, but the charcoal gradually absorbs all the gas it can hold, and when saturated ceases to work.

Thomson and Aston used the air-trap effectively for exhausting the observation chamber or 'camera' of the positive-ray apparatus, which needed to be at a very low pressure. The discharge tube, which must not reduce too low in pressure or the discharge stops, was exhausted by the Gaede pump.

Aston's operating pressure in his first mass-spectrograph was about $10^{-4}$ TORR; his second instrument included a diffusion pump of the type invented by Langmuir. The first model of such a pump had been made in 1915 by Gaede, whose contributions to pump technology were unique. Gaede started in 1905 with his rotary mercury pump, which was in effect a continuously operating Töpler system, and went on to invent the rotary vane pump in 1909. He followed this by the 'molecular' rotary pump in 1915. Twenty years later he improved the rotary vane pump by the addition of gas ballast.

Gaede had started as an academic physicist who found himself handicapped by the shortcomings of the manually operated pumps which he had to use in his early researches. Thus he applied his knowledge of the kinetic theory of gases to the invention of better pumps. It was his rotary mercury pump which was taken as the prototype for the German electric lamp industry, and heralded the explosion of modern vacuum engineering and technology in the first half of this century.

Vacuum technology was essential in pointing the way to the release of nuclear energy; moreover, the development of the mass-spectrograph provided one of the main principles for separating the isotopes of...
uranium. The other main method was thermal diffusion, and both methods required high vacuum on a very large scale. By the time the military need to separate the isotopes was seen in 1941, vacuum technology had advanced to the stage where separation was feasible, although it involved a prodigious step from anything that had been done hitherto. (The thermal diffusion plant consisted of 600 miles of 8inch pipe, and all the welds had to be vacuum tight).

The availability of such vacuum technology by 1940 was due to other applications and developments of high vacuum techniques that were beginning to make their impact on the wider society. The discovery of the electron led to the thermionic valve, the cathode ray oscilloscope, and the iconoscope. This phase of vacuum electronics led to broadcasting, television, the computer, and the radar of the Second World War.

11.5 DEVELOPMENT OF SPECIALIST PUBLICATIONS AND SOCIETIES

As in other branches of the physical sciences, an excellent indicator as to the level of maturity reached by a particular area occurs when initial informal exchanges of information grow into the establishment of conferences.

We have seen that by the 1950's mass-spectrometry had assumed an important role in chemistry for both research and analytical work, and several conferences had touched upon mass-spectrometry topics. Physicists, however, had had no comparable opportunity to gather together to discuss advances in mass-spectrometry of particular interest to them and to derive thereby the longer term benefits from personal contact with leading workers in the field.

The first opportunity was provided by the symposium on "Mass-Spectrometry in Physics Research" held in Washington on September 6 - 8th 1951, as one of a series of twelve commemorating the semi-centenary of the National Bureau of Standards. This conference was attended by practically all the physicists working in mass-spectrometry; Hipple, Mattauch, Nier, Bleakney and many others. All the papers presented at the symposium were included.
in a volume, Circular 522, published by the N.B.S.

The first regular mass-spectrometry meetings took place in the U.S.A., in 1955. This was the so-called E-14 Committee on Mass-Spectrometry, a subcommittee of the American Society for Testing and Materials (ASTM) established to "promote the knowledge and advancement of the art of mass-spectrometry". This committee has met annually since, and has grown from being purely an American affair to a forum now co-sponsored by all mass-spectrometer manufacturers. Since 1961 the proceedings of the annual meetings have been published, but are available only to those attending the meetings, a policy which permits the contributors also to publish in conventional journals. In 1969 it was converted to the American Society for Mass-Spectrometry.

The European counterpart to the E-14 Committee is the International Mass-Spectrometry Conference, sponsored by a number of British, French,

*Footnote: The E-14 Committee grew out of discussion meetings organised by the Consolidated Engineering Corporation, (founded by Harold Washburn), the first commercial manufacturer of the mass-spectrometer, for operators of the instrument. This was established as an annual meeting, and involved a training and up-dating session. In addition, other scientists who were working in the field were invited to talk about their own work.

When General Electric entered the commercial field, they combined their training 'clinics' with C.E.C., and those meetings grew to the extent that it was agreed to organise it as a subcommittee of the A.S.T.M.
German, and other European scientific societies. The first conference took place in 1959, since then there have been nine meetings.

Proceedings of these meetings are officially published in the "Advances in Mass-Spectrometry" series, of which nine volumes have been published to date. (This is the publication which first included, and subsequently updated, the A.E.I. bibliography on Mass-Spectrometry).

As will be found in the appendix, books specifically related to Mass-Spectrometry have been published since Thomson's "Rays of Positive Electricity" in 1913. However, apart from a Japanese journal - Sitoryo Bunseki - with limited circulation, it was not until 1968 that the first journals devoted entirely to mass-spectrometry were brought out. Three new journals made their debut in 1969: "International Journal of Mass-Spectrometry and Ion Physics" (Elsevier, Amsterdam), "Journal of Organic Mass-Spectrometry" (Heyden, London), and "Archives of Mass-Spectral Data" (Interscience, New York).

11.6 SURVEY OF UNITED KINGDOM MASS-SPECTROMETRISTS

This survey by questionnaire analysis of the United Kingdom community of mass-spectrometrists has proved to be too great an undertaking to fully complete within this thesis, and it is now intended that an extended analysis of the questionnaire will be published separately.

*Footnote:

Mass-spectrometry in 1973 must have considered itself a thoroughly established branch of science, because for the first time the conference agenda at Edinburgh included not only purely scientific and technical papers, but also several contributions on related non-scientific topics, including an account of the history and development of Mass-Spectrometry, presented by myself.
However, I can indicate here the reasons for undertaking such a survey, the questionnaire format, and provide some initial results, (See Appendix III).

11.6.1 AIMS

There were originally six reasons for this survey:

(i) to complete the historical record of the development of the community of mass-spectrometrists at least for the United Kingdom.

(ii) to investigate the genealogy of training and influences, and to relate this to the earlier genealogy.

(iii) to examine the factors involved in scientists' decisions to enter and leave the field. Some workers remain for a lifetime in mass-spectrometry, others are part of a constantly shifting group of migrants. To investigate whether this pattern changes over time, and if so for what reasons. (For example, there is now less individual investment of time in learning the expertise of mass-spectrometry. There may therefore be fewer personal restraints in staying in one field). To look at differences between pre-war and post-war patterns.

(iv) to determine to extent to which mass-spectrometry has moved from being a research physicist's tool to that of being a widely available analytical technique.

(v) to compare the community of mass-spectrometrists (if indeed it still exists) with other communities of scientists.

(vi) to use the analysis of personnel in the current mass-spectrometry community as part of this sociological case study.
The results and early analysis of the survey (contained in Appendix III) indicate certain profiles, trends and patterns within the mass-spectrometry community. The principal feature is the radical shift of location of mass-spectrometry from physics to chemistry, further underlining the gradual change of focus from science to technology.

Approximately two thirds of the U.K. current generation of mass-spectrometrists are located within a university or other academic institution. Over 90% consider themselves as chemists, or at least work within the field of chemistry, and at least 75% are involved in some aspect of personal research work, using mass-spectrometric techniques. For the majority of scientists in industry, however, this research has to be fitted in between having to provide an analytical service for other scientists.

From the results obtained it is possible to construct a typical profile of mass-spectrometrists. At the time of the survey (1973) mass-spectrometrists were likely to be aged between 30 and 40 years, equipped with a chemistry Bachelors degree and doctorate in chemistry. They were likely to have been educated to doctorate level at one of six universities, i.e. Cambridge, Oxford, Birmingham, Glasgow, Manchester, and London. For the academically trained mass-spectrometrists, they would have received their training at Ph.D or post doctorate level in one of three universities, i.e. Cambridge, Birmingham, or Glasgow.

The industrially trained mass-spectrometrists may well have received this training at A.E.I. (mass-spectrometer designers and manufacturers), or else with a petrochemical company.

Further questionnaire analysis as previously stated is being held over for future publication.
The aim of this thesis was to provide a realistic explanation of the process of scientific innovation, based on an assembly and interpretation of the history of the development of mass-spectrometry.

An attempt to provide a degree of 'fit' of sociological theory to the actual developments in mass-spectrometry must recognise that within each period there are different processes and trends at work where perhaps one theory explains some aspects of development but certainly not all. We must be content to construct a synthetic additive model. Science, after all, is pluralistic, not monistic and therefore we must seek pluralist theories to explain the processes of scientific thought and development. Just as in science, for example, we have the wave/particle duality in the explanation of the behaviour of the electron, so we must contend that different sociological theories each have their own preferential area of applicability, whilst not necessarily being mutually exclusive.

We can conveniently organise the development of mass-spectrometry into four main periods (Figure 11.7): discovery (1911-1922), diffusion (1923-1940), concentration (1940-1945), and exploitation (1946 to the present day). Of course, there is a degree of overlap for some aspects of the field, but we have seen in this case study that the four periods as postulated each contain distinct features of growth and organisation. Discovery corresponds with the origins of mass-spectrometry, the pioneer work of Thomson, Aston and Dempster, the discovery of isotopes and early apparatus building. Diffusion occurs as the technique spreads to other groups of different countries, concentrating almost solely on instrument development. Concentration describes the expansion period of increased organised manpower and enforced secrecy during the Manhattan Project, integrating manipulative skills and theoretical work from many areas of
11.7.

Development of Mass-Spectrometry
science and the advent of 'Big Science'. Exploitation covers the rapid commercial development of mass-spectrometer manufacture and the rise of mass-spectrometry as an analytical technique used in many fields of chemistry, and the spread into many sub-fields.

It is possible to construct a matrix in order to determine which of the several main sociological theories might be the most convenient tool with which to explain the processes working within a particular period.

<table>
<thead>
<tr>
<th></th>
<th>DISCOVERY</th>
<th>DIFFUSION</th>
<th>CONCENTRATION</th>
<th>EXPLOITATION</th>
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<tr>
<td>MERTON</td>
<td>N</td>
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<td>Y</td>
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<tr>
<td></td>
<td>Conformity to Normative behaviour</td>
<td></td>
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<tr>
<td>BARBER</td>
<td>N</td>
<td>Y</td>
<td>SUSPENDED</td>
<td>Y</td>
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<tr>
<td></td>
<td>Retrospective falsification, Resistance to discovery, Serendipity</td>
<td></td>
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<tr>
<td>KUHN</td>
<td>N</td>
<td>Y</td>
<td>SUSPENDED</td>
<td>Y</td>
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<tr>
<td></td>
<td>Conformity to 'Normal' science, Revolution</td>
<td></td>
<td></td>
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<tr>
<td>BEN DAVID</td>
<td>Y</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
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<td></td>
<td>Cross Fertilization and Role Hybrids</td>
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<tr>
<td>HOLTON</td>
<td>Y</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
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<td></td>
<td>Migration to areas of ignorance</td>
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<tr>
<td>MULKAY</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
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<tr>
<td></td>
<td>'Branching' model</td>
<td></td>
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<tr>
<td>MARXIST THEORY AND TECHNOLOGICAL DETERMINISM</td>
<td>N</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
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</table>

It is impossible within the scope of one thesis to attempt a comprehensive analysis of this case study in terms of each of the existing contending theories. From inspection of the above matrix it would appear that
migration and branching models, which have themselves digested Kuhnian and Mertonian frameworks, proffer themselves as offering the most comprehensive explanation and one can conveniently consider mass-spectrometry in the light of the work of Mulkay.

As previously discussed, Mulkay postulates three main stages in the growth of a research area:

Exploration / Unification / Decline or Displacement

If we relate this to my conceptualisation of the growth of mass-spectrometry, it would appear that Mulkay’s complete three-stage model cannot encapsulate the whole field of mass-spectrometry, but is applicable to the formation and behaviour of the sub-groups as they occur, and we have seen in the ‘fine structure’ examination of the ion-molecule reaction sub-community just how prevalent the ‘branching’ process is. Mass-spectrometry in its totality is still somewhere in the exponential stage two ‘Unification’ period, whilst as we have seen along the way many subfields have originated, flourished, and subsequently disappeared or have been transmuted into other research areas.

11.8

As a final note, it would appear that after an eleven year period spent on this particular area of research, that in order to do the subject justice, I have taken on a lifetime’s work, rather than a thesis.

Consequently, whilst I have provided a reasonably full historical picture, I have not been able to take a variety of analytical methodologies into every period of the development of mass-spectrometry. I think, nevertheless, that the original aim of a macro-study was worth doing, albeit now with recognition that further work should be done in collaboration with other authors.
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13. EXPLANATION OF CITATION TABLE
14. TABULAR CITATION ANALYSIS 1910 - 1937
15. TABULATION OF RANK ORDER OF CITATION FREQUENCY 1910 - 1937
16. "PARETO CURVE" OF RANK ORDER OF CITATION FREQUENCY
17. TABLE OF CITATIONS GIVEN IN PAPERS 1910 - 1937
APPENDIX II:

CONTENTS Continued

18 TABLE OF PAPERS CONTINUOUSLY CITED 1910 - 1937
19 CITATION NETWORK 1911 - 1925
20 BIBLIOGRAPHY OF ION-MOLECULE REACTIONS 1905 - 1974
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### 1956-1973 Chemical Abstracts Totals

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1920-40
growth rate ~ 5% per annum

1946 onwards
growth rate ~ 10% per annum
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RELATIONSHIP BETWEEN GROWTH OF PHYSICS, CHEMISTRY AND MASS SPECTROMETRY

PUBLICATIONS

100,000
10,000

CHEMISTRY ABSTRACTS

PHYSICS ABSTRACTS

MASS SPECTROMETRY PAPERS

TIME →
# Analysis of Papers in Mass Spectrometry by Subject

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63 No analysis available.

64 Analysis of papers in mass spectrometry by subject reduced to percentages.

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II. Mass-spectrometry papers as a percentage of total chemistry papers.
Papers cited (chronological order)

No papers cited by subsequent papers

'KEY' papers are 1 and 4

Example: Paper 10 cites papers 4, 7 & 9

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**Table (94) Cont**
Rank order of citation frequency
1910 - 1937

[i.e. the number of times a publication was cited up until 1937]

| 70 | A5 |
| 60 | A4 |
| 50 | JG4 |
| 40 | HOS6 |
| 30 | D2 | WB1 |
| 20 | D6 |
| 10 | HOS5 | A3 | HOG2 | KTEG | KT22 |
| 0 | A1 | HOS3 | HOG1 | HOS13 | KTS | AG6 |
| 90 | A2 | HOS1 | HOG9 | BAR5 | D1 | WC8 | KT3 | BIR2 | NB8 | HCS | AG6 | AON5 | TXT |
| 80 | A3 | HOS4 | BAR | W63 | GST | KT7 | W86 | UR1 | VR2 | KTO | K77 | BRES | AON2 |
| 70 | D1 | A17 | A39 | COS1 | WSC1 | AG4 | A5 | W89 | KAL6 | BRE1 | WSC2 | AG4 | W822 |
| 60 | H4 | A19 | GP2 | A24 | A30 | HOG4 | A4 | GP1 | GPH2 | HOG5 | A45 | HOG6 | A42 |
| 50 | AN | A2 | JTI | AY | AX | J3 | JS | G1X | 25Z | A22 | HDS2 | KAL1 | A43 | BAR4 | HOG2 | AG6 |
| 40 | A2 | AF | D4 | ALL2 | AG5 | MAT4 |

| 20 | RW | KAL4 | KT2 | WBS | KAS5 | HOS1 | HOS12 | ALVI | AG5 | KTE | KTO | W87 | MAT1 |
| 10 | W8 | FT5 | A50 | A53 | MAT2 | A5 | KTO | MAT3 | WRS2 | FT5 | AG6 | BON1 |
| 0 | KT6 | HOS4 | A62 | W66 | W86 | MAT4 | IES | W82 | ALV2 | ZEE | W817 | D1XX |
| 90 | DI2 | D13 | ZEE2 | KT21 | W821 | N823 | NG24 | BRE4 | AON3 | SAW1 | KT23A |
| 80 | D14 | ZEE2 | JBP1 | JFB2 | BRE5 | BREC | A70 | HAT20 | WRS5 |
| 70 | PIS6 | AG67 |
| 60 | MAT6 | MAT8X |
Table (15) cont'd.

Publication cited less than 2 times have been excluded.

JJa - Thompson, Keys of Positive Electricity.
PARETO CURVE OF RANK ORDER OF CITATION FREQUENCY

Rank of Paper | Citation Frequency
-------------|------------------
1            | 30
2            | 21
3            | 17
4            | 16
5            | 11
data...
TABLE (16) CONT'D

PARETO CURVE - PAPERS ARRANGED IN RANK ORDER OF
NUMBER OF TIMES CITED

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CITATIONS GIVEN IN PAPERS 1911 - 1937

Total number of publications: 310.

Total number of citations:
  Internal: 760
  External: 128

Average percentage of external citations of total: 14.4%

"Review papers have been ignored, as these are biased in favour of internal citations."

From 1911 - 1928:
  Internal: 200
  External: 55

Proportion of external to total: 21.6%

From 1929-1937:
  Internal: 560
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Proportion of external to total: 11.5%

Typology of external citations:
1. Technical
2. Theoretical
3. Confirmation with other results.
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F.N. Harlee, H.M. Rosenstock & J.T. Herron

From Jan 1905 - March 1966.

[NAT. BUR. STAND.] TECH. Reports 291.
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Bibliography compiled by M.S.D.C.
(Mass Spectrometry Data Centre)
from Jan 1967 onwards.
APPENDIX III : QUESTIONNAIRE DOCUMENTS

CONTENTS

1 COVERING LETTER TO PILOT QUESTIONNAIRE
2 PILOT QUESTIONNAIRE
3 QUESTIONNAIRE
4 REMINDER
5 QUESTIONNAIRE CODING SCHEDULE
6 RESULTS
Dear

We are engaged in a study of the development of Mass-spectrometry, from its invention by Sir J.J. Thomson to the present day generation of mass-spectrometrist.

Apart from compiling a complete bibliography of published papers and collating existing literature, we have interviewed pioneer mass-spectrometrist, including Walter Bleakney, Kenneth Bainbridge and Alfred Nier in order to make the historical record as accurate as possible. We have also interviewed Lord Snow and Sir George Paget Thomson, who both were contemporaries of Sir J.J. Thomson and Frederick Aston at Cambridge University, and these have provided us with important additional information concerning scientific research in the early part of this century.

Since the current position of Mass-Spectrometry is as important as its past development, we are writing to all United Kingdom based mass-spectrometrist: we are particularly interested in education backgrounds and training so that the "genealogy" of major influences may be brought up to date.

Would you therefore please help us by completing the accompanying questionnaire. If you have any opinions either concerning the questionnaire itself, or any additional information which you consider relevant, would you please add your comments in the space provided at the end.

We appreciate that completing questionnaires is time-consuming, so are very grateful for any co-operation you are able to give us.

Yours sincerely,

David R. Panton
1. In which of the following sectors are you currently employed?
   - University or other Academic Institution: 1
   - Research Institute and/or Civil Service: 2
   - Industry: 3
   - Other (Please specify): 4

2. How would you describe your work with the Mass-Spectrometer?
   - Research: 1
   - Research and providing an analytical service: 2
   - Providing an analytical service only: 3
   - Research administration: 4
   - Development & Production of machines: 5
   - Marketing and Sales: 6
   - Other (Please Specify): 7

3. How important is Mass-Spectrometry in your work?
   - Your main interest: 1
   - One of your main interests: 2
   - A peripheral interest: 3

4. How long have you been working in Mass-Spectrometry?
   - ........ Years

5. How long do you think you will continue to work in Mass-Spectrometry?
   - Under 5 years: 1
   - 5 years and under 10 years: 2
   - 10 years and over: 3

6. Please give details of all academic qualifications.

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<th>Subject/Area</th>
<th>University/College</th>
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</table>


7. We are particularly interested in tracing influences in the training of the current generation of mass-spectrometrists. Would you therefore please answer the following questions as fully as possible.

a) Where did you receive your initial training? (e.g. name of university department, industrial concern etc).
   
   b) When did this training take place? (e.g. at doctorate level, after starting in industry etc).
   
   c) From whom did you receive this training? Please give name and details of not more than two persons whom you consider were the most important. If particular individuals were not involved please state briefly how you were trained.
   
   First.
   
   Second.

8. Was/is this/these person(s) your:—

   Immediate superior or supervisor..........................1. 1
   Superior to whom you were not directly responsible........2. 2
   Colleague of similar status..................................3. 3
   Technician or instrument specialist..........................4. 4
   Other (please specify)..........................................5. 5

9. Very approximately how many papers have you contributed to mass-spectrometry (include joint papers).
   
   a) Published in the literature................................
   
   Given at conferences etc..................................
   
   b) How many if any books have you written on mass-spectrometry and related topics?

END OF QUESTIONNAIRE. THANK YOU FOR YOUR HELP.
SURVEY OF MASS-SPECTROMETRISTS

We are engaged in a study of the development of Mass-Spectrometry, from its invention by Sir J.J. Thomson to the present day generation of mass-spectrometrists.

Apart from compiling a complete bibliography of published papers and collating existing literature, we have interviewed pioneer mass-spectrometrists, including Walker Bleakney, Kenneth Bainbridge and Alfred Nier in order to make the historical record as accurate as possible. We have also interviewed Lord Snow and Sir George Paget Thomson, who both were contemporaries of Sir J.J. Thomson and Frederick Aston at Cambridge University, and these have provided us with important additional information concerning scientific research in the early part of this century.

Since the current position of Mass-Spectrometry is as important as its past development, we are writing to all United Kingdom based mass-spectrometrists: we are particularly interested in education backgrounds and training in mass-spectrometry so that the "genealogy" of major influences may be brought up to date.

Would you therefore please help us by completing the accompanying questionnaire and returning it in the envelope provided. If you have any opinions either concerning the questionnaire itself, or any additional information which you consider relevant, would you please add your comments in the space provided at the end.

We appreciate that completing questionnaires is time-consuming, so are very grateful for any co-operation you are able to give us.

Yours sincerely,

David R. Panton
Bryan G. Reuben
1. In which of the following sectors are you currently employed?

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2. How would you describe your work with the Mass-Spectrometer?

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<tr>
<td>Other (please specify)</td>
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IF YOU ARE AT ALL CONCERNED WITH RESEARCH, PLEASE ANSWER QUESTION 3. OTHERWISE PROCEED DIRECTLY TO QUESTION 4.

3. (a) Did your research interests grow out of having to provide a service with the mass-spectrometer?  

   Yes 1  
   No 2

   (b) Was providing a service with the mass-spectrometer the price you had to pay in order to obtain an instrument with which to do research?  

   Yes 1  
   No 2

   (c) How important is Mass-Spectrometry in your work?  

   Your main interest 1  
   One of your main interests 2  
   A peripheral interest 3

4. (a) How long have you been working in Mass-Spectrometry?  

   [Number of years]

   (b) How long do you think you will continued to work in Mass-Spectroscopy?  

   Under 5 years 1  
   5 years and under 10 years 2  
   10 years and over 3

5. Please give details of academic qualifications.

<table>
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</table>
TRAINING OF THE CURRENT GENERATION OF MASS-SPECTROMETRISTS. WOULD YOU THEREFORE PLEASE ANSWER THE FOLLOWING QUESTIONS AS FULLY AS POSSIBLE.

6. (a) Whose work stimulated you most to enter the field of mass-spectrometry?

(b) Where did you receive your initial training in mass-spectrometry? (e.g. name of university department, industrial concern etc.).

(c) When did this training take place? (e.g. at doctorate level, after starting in industry etc.).

(d) From whom did you receive this training? Please give name and details of not more than two persons whom you consider were the most important. If particular individuals were not involved please state briefly how you were trained.

   First

   Second

7. Was/is this/these person(s) your:-

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8. (a) How many papers have you contributed in the field of mass-spectrometry? (include joint papers).

   Published in the literature

   Given at conferences etc.

   (b) How many books have you written on mass-spectrometry and related topics?
concerning this questionnaire or concerning the development of mass-spectrometry in general?

THANK YOU FOR YOUR HELP
19th January, 1973

Dear Sir,

You received a copy of the Questionnaire concerning the training of Mass-Spectrometrists last November. If you have not already done so, could you please return it as soon as you can. I appreciate that this is time-consuming, and I am very grateful for your co-operation.

Yours faithfully,

D. R. Panton

Department of Chemistry
Professor J. E. Salmon
Professor J. A. Elvidge
## SURVEY OF MASS-SPECTROMETRISTS

### CODING SCHEDULE

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- Manchester U. 08
- Swansea U. 10
- Birmingham U. 11
- Bristol U. 12
- Durham U. 13
- Keele U. 09
- Edinburgh U. 14
- Salford U. 15
- Oxford U. 16
- Bath U. 17
- London University College 18
- London U./Northern Poly 19
- Owens Coll, Manchester 20
- Nottingham U. 21
- Belfast U. 22
- London Kings College 23
- Battersea Cat. 24
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- St. Andrews U. 26
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- Brunel U. 29
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- Cardiff U. 36
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- Leeds U. 40
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- Queen Elizabeth Coll. 42
- London
- Birkbeck College London 43
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J.H. Leck 008
Ryhage 009
K. Biemann 010
C.P. Falshaw 011
K.R. Jennings 012
A.W. Tickner 013
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G.A. Errock 018
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F3.0 45 46 Q6B

Name of Place of Training

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| Imperial College    | 003 |
| Birmingham U.       | 004 |
| Abroad and Foreign  | 005 |
| I.C.I.              | 006 |
| A.E.I.              | 007 |
| Sheffield U.        | 008 |
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| F3.0     | 53 54  | Q6D2     | Second Choice 001 -- |
|          | 55     |          | Other Response 1 |
|          |        |          | Self-Taught 2 |
|          |        |          | Experience from Routine 3 |
|          |        |          | From Designing and Developing Machines 4 |
| F1.0     | 56     | Q6D3     | Other Response 5 |
|          |        |          | From other colleagues 6 |
|          |        |          | General Training Course 7 |
|          |        |          | From Lectures 001 -- |
|          |        |          | R.I.C. Summer School Course 001 -- |

| F1.0     | 57     | Q7A      | Status of Choice 1 |
|          |        |          | First Choice 2 |
|          |        |          | Immediate Superior 3 |
|          |        |          | Indirect Superior 4 |
|          |        |          | Colleague 5 |
|          |        |          | Technician 6 |
|          |        |          | Other 7 |

| F1.0     | 58     | Q7B      | Second Choice 1 |
|          |        |          | Immediate Superior 2 |
|          |        |          | Indirect Superior 3 |
|          |        |          | Colleague 4 |
|          |        |          | Technician 5 |
|          |        |          | Other 6 |

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| F1.0     | 60     | Q8A2     | Pub. in Lit. Joint 1 -- |
| F1.0     | 61     | Q8A3     | Given at Conferences Sole 1 -- |</p>
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RESULTS

1 PRELIMINARY CONSIDERATIONS

Since it was obviously too great a task with the resources available to elicit information from all mass-spectrometrists in all countries, it was decided to survey the population, not just a sample, of all U.K. mass-spectrometrists.

An indication of the current population resides in the mailing list of the Mass Spectrometry Group, (M.S.G.) which embraces practically all mass-spectrometrists who are involved enough in their topic to join a speciality group.

Basic information required via this questionnaire in order to answer the earlier stated questions included:

a) Personal chronological details
b) Personal influences in training
c) Attitudes to mass spectrometry
d) Attitudes to colleagues and other workers
e) Opinions as the the development of mass-spectrometry

It was decided to run a pilot questionnaire (see Appendix III.1 & 2) using six known scientists in the field, covering industry, the research establishments and universities. Their comments were taken into account in the construction of the final questionnaire.

It was also decided to design the questionnaire so that the analysis could be carried out on the computer at Surrey University using the S.P.S.S.
programmes.

On the basis of active membership of M.S.G., 230 questionnaires with a covering letter (see Appendix III.3) were sent out. 168 replies were obtained giving a response rate of 73%. (The questionnaire was sent as coming from the Chemistry Department, as it was felt that scientists might be more inclined to find time to reply to a detailed questionnaire if it was seen as being carried out by one of themselves, rather than from a social scientist.)

11.6.3 QUESTIONNAIRE DESIGN

As to the questionnaire format, (see Appendix III.3 & 5) the covering letter describes the intentions and relevance of the questionnaire, placing the current generation in context.

Questions 1 and 2 aimed to locate the respondent in terms of establishment and status, ie;

University v Industry v Research Establishment

Research v Analytical routine work

and these two questions provide the basic matrix to examine the other questions in the questionnaire.

This thesis is concerned with the development of mass-spectrometry in terms of discovery and research work, and the location of this research.

Question 3 was designed to establish for each respondent the cause/effect, push/pull balance in affecting their entry and participation in research work.
Question 4 aimed to investigate the movement (and reasons and attitudes behind it) of personnel into and out of the field, again in terms of University / Industry / Research Establishment, and Research / Analysis categories.

Question 5 was designed to elicit information about the training of the respondent, linking with Question 4 to provide genealogical links, and to show the structure of the U.K. mass-spectrometry community. It also provided a chronological and status framework.

Question 6 was intended to obtain general opinions as well as specific answers to important influences in the respondent's mass-spectrometry training. It was expected that there would be references to the 'founding fathers', as well as indications of more recent direct influences. This question also linked with Question 5 in elucidating the genealogical structure.

Question 7 links with Question 6 and was designed to indicate comparisons between University / Industry / Research Establishment sectors.

Question 8 contributed to the bibliographic and statistical analysis of the growth of mass-spectrometry.

Question 9 was provided to reduce the frustration of filling in coded questionnaires, and to ensure that the respondent has an opportunity to supply further information. Therefore asking for comments often helps with the other answers as well. Comments were invited as to the development of mass-spectrometry, the role of mass spectrometry as a research / analytical technique, to indicate new refinements, etc.
As previously stated, it is not possible to do more here than provide some early results and comments, based upon the responses from the 168 scientists who took part.

Q 1 Sector of current employment:

- University: 45%
- Research Institute: 20%
- Industry: 33%
- Other: 2%

ie. approximately two thirds academic institution, one third industry.

Q 2 Evaluation of work carried out in Mass-Spectrometry:

- Research: 27%
- Research and Analytical Service: 44%
- Analytical Service: 7%
- Research Administration: 5%
- Development and Production of Mass-Spectrometers: 5%
- Marketing and Sales: 2%
- Teaching: 3%
- Other (combination of above): 7%

ie. at least 75% of respondents had considerable involvement in research work.

Q 3 a) Of the respondents (75%) who declared some involvement in research, 66% stated that their research interests grew from having to provide a service with the mass-spectrometer.

b) 60% of these research respondents or 30% overall also commented that providing an analytical service was the price they had to pay in order to do research on a mass-spectrometer.

c) 92% of the scientists considered that mass-spectrometry was their main interest, or at least one of their main interests. For the
Comparing the responses to Question 3 a), b), and c) with the breakdown between sectors, ie University / Research Establishment / Industry, we find that:

**Q 3 a)** 56% of industrial scientists research interests grew out of the provision of mass-spectrometric analytical service, as compared with 30% of scientists in civil service establishments, and 14% of scientists in Universities.

**Q 3 b)** This question produced a more evenly distributed response. 42% of industrial scientists felt that their research time on a mass-spectrometer was obtained by also having to carry out their analytical work, as compared with 35% of the University scientists, and 23% of the Research Institute scientists.

**Q 3 c)** An analysis of 'main interest' by sector produced a response rate of 45% of industrial scientists, as compared with 30% of research institute scientists, and 25% of University scientists, indicating less specialisation in universities, possibly because industrial scientists have more closely specified jobs. However, an analysis of 'one of main interests' by sector produced the opposite effect, with 52% for university scientists, 28% for research establishment scientists, and 20% for industrial scientists, again possibly emanating from more specific job definition for industrial sector scientists.

**Q 4 a)** In reply to the question concerning the length of time a scientist has been working in the field of mass-spectrometry, the responses ranged from zero to 26 years. 70% of the respondents had been in mass-
spectrometry under 10 years, and 30% of the scientists had been involved between 4-6 years.

This prompts two questions - is it that the majority of respondents are young, or do the majority of mass-spectrometry scientists leave the field after 7-10 years? This could be established by cross-tabulating age with the date of academic qualifications. (This work has yet to be carried out, and will be reported elsewhere).

When asked how long they considered their future involvement in mass-spectrometry would be, 27% said they would leave before five years, 28% between 5 and 10 years, and 45% thought their involvement would last at least 10 years.

Again, relevant questions here include a consideration of whether those who have already worked in mass-spectrometry for a number of years are

a) the ones who wish to continue the longest (ie, once a mass-spectrometrist always a mass-spectrometrist).

or

b) is the effect the opposite, ie. if the majority are young it could still mean their leaving after a total involvement of approximately 10 years.

Analysis of this would indicate age of migrants, who were the potential migrants, those who practice "skimming the milk" in their research practice, and also shed some light on the characteristics of inward/outward migrants.

Q 5 a) An inspection of the kind of academic award or degree obtained by the respondents shows that 67% hold a B.Sc. and Ph.D., 18% a B.Sc. alone, 9% a B.Sc. and R.I.C. (Royal Institute of Chemists qualification), and 6% hold a technical qualification.
Q 5 b) 1 and 2. :- It is clear that for practically all of the respondents there is a 2-3 year gap between obtaining the first and second awards. If we just analyse for the date of second award, the sample ranges from 1940 to 1973, with quartiles at approximately 1957, 1962, and 1966. Over 50% of the respondents obtained their second qualification between 1957 and 1967, a period which generally experienced a boom for science education.

Q 5 c) When we consider the subject of first award, 83% of the responding scientists took a Chemistry degree, as compared with only 17% who qualified in Physics. This disproportion is also apparent when we look at the second qualification:

- Chemistry: 40%
- Organic Chemistry: 25%
- Physical Chemistry: 12%
- Gas Kinetics: 7%
- Physics: 5%
- Mass Spectrometry: 11%

This indicates at least 90% of the responding research scientists are chemists, of one or other area.

Q 5 d) 1 :- An inspection of the location of training for the first award highlights the following centres attended by six or more respondents:

- Glasgow University: 11%
- Bristol University: 11%
- Manchester University: 13%
- Oxford University: 14%
- Cambridge University: 15%
- Birmingham University: 16%
- London University: 20%
Q 5  d) 2  :-  Responses to location of second or postgraduate award training shows a slightly different pattern:-

- Manchester University 11%
- Glasgow University 13%
- London University 16%
- Oxford University 16%
- Birmingham University 21%
- Cambridge University 23%

Q 6  a) Respondents were asked to name influential scientists who stimulated them to enter the field of mass-spectrometry. In rank order of citation these were :

- Beynon (founding father)
- Nier
- Aston
- Biemann (current worker)
- Djerassi
- Lossing
- Williams
- Wolstenholme
- Majer
- Elliott (commercial M.S. manufacturer)

This question elicited two distinct and very different types of response :

a) It was answered in a historical sense; respondents mentioned pioneers, and various research schools. These respondents were mainly University based.

b) It was answered in reference to their present condition, indicating non-abstract motivation to be involved with
the subject. Several respondents phrased this as, "if my boss says do mass-spectrometry today, I do it". These respondents were largely industry orientated.

Q 6 b) Respondents were asked to name the location of their initial training in mass-spectrometry. Those centres selected by 4 or more respondents were:

- A.E.I. 25%
- Cambridge University 14%
- Abroad 12%
- Birmingham University 12%
- Glasgow University 10%
- Sheffield University 7%
- A.E.R.E. Harwell 7%
- Shell Research 7%
- Liverpool University 6%

Q 6 c) Respondents were asked where this mass-spectrometry training took place:

- At Ph.D. level 38%
- In industry 34%
- At post doctorate level 15%
- Other 7%
- While teaching 6%

Q 6 d) Respondents were asked to indicate from whom they received this training or, if not from a specific person, then to indicate the method of their training.

Those who responded by naming a person specified:

- Unspecified Technician 53%
- Elliott 11%
- Reed 8%
The reasons given by those respondents who indicated a non-personal pattern of training:—

<table>
<thead>
<tr>
<th>Reason</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Self taught</td>
<td>83%</td>
</tr>
<tr>
<td>Learned from experience in routine work</td>
<td>9%</td>
</tr>
<tr>
<td>Learned from designing and developing instruments</td>
<td>9%</td>
</tr>
<tr>
<td>Learned from General Training Course</td>
<td>9%</td>
</tr>
<tr>
<td>Learned from lectures</td>
<td>9%</td>
</tr>
<tr>
<td>Learned from R.I.C. Summer School</td>
<td>9%</td>
</tr>
</tbody>
</table>

Q 7 Respondents were asked to indicate their status relationship with their instructor in mass-spectrometry.

Of those who replied:—

<table>
<thead>
<tr>
<th>Status Relationship</th>
<th>First Choice</th>
<th>Second Choice</th>
</tr>
</thead>
<tbody>
<tr>
<td>Superior</td>
<td>55%</td>
<td>29%</td>
</tr>
<tr>
<td>Indirect Superior</td>
<td>9%</td>
<td>23%</td>
</tr>
<tr>
<td>Colleague</td>
<td>11%</td>
<td>14%</td>
</tr>
<tr>
<td>Technician</td>
<td>22%</td>
<td>21%</td>
</tr>
<tr>
<td>Other</td>
<td>3%</td>
<td>13%</td>
</tr>
</tbody>
</table>
a) Papers published in mass-spectrometry :-

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<thead>
<tr>
<th></th>
<th>1-5</th>
<th>6-10</th>
<th>11-15</th>
<th>16-25</th>
<th>26-50</th>
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</thead>
<tbody>
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<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>JOINT AUTHOR</td>
<td>43</td>
<td>32</td>
<td>10</td>
<td>4</td>
<td>5</td>
<td>2</td>
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</table>

No. of Respondents: 42

b) Papers given at conferences :-

<table>
<thead>
<tr>
<th></th>
<th>1-5</th>
<th>6-10</th>
<th>11-15</th>
<th>16-25</th>
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<td>SOLE AUTHOR</td>
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<td>6</td>
<td>3</td>
<td>-</td>
</tr>
<tr>
<td>JOINT AUTHOR</td>
<td>41</td>
<td>18</td>
<td>8</td>
<td>1</td>
</tr>
</tbody>
</table>

No. of Respondents: 68

c) Books written on Mass-Spectrometry and related topics :-

<table>
<thead>
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<th></th>
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<th>2</th>
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</tr>
</thead>
<tbody>
<tr>
<td>SOLE AUTHOR</td>
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<td>4</td>
<td>1</td>
</tr>
<tr>
<td>JOINT AUTHOR</td>
<td>5</td>
<td>5</td>
<td>1</td>
</tr>
</tbody>
</table>

No. of Respondents: 12

Q 9 was the open-ended section which allowed respondents, if they wished, to make a wide range of comments, either about the questionnaire itself, or concerning aspects of the development or community of mass-spectrometry.

57 respondents completed this section making a total of 90 different...
comments and statements. Their main concerns were classified under the following headings:–

1. Providing additional personal history 28%
2. That mass-spectrometry was perceived now as a tool for other work 25%
3. Description of types of mass-spectrometry instruments 13%
4. Historical changes in emphasis of mass-spectrometry 10%
5. Integration of mass-spectrometry with gas-chromatography 8%
6. Computerisation and sophistication of technique 8%
7. Questionnaire criticism 5%
8. The split in mass-spectrometry between physics and chemistry 3%

11.6.5 DISCUSSION

Amongst criticisms made of this survey, several respondents – possibly a strong indication why 27% of mailed scientists failed to respond – considered that the questionnaire was not really personally applicable, because they did not research in the field of mass-spectrometry, but used mass-spectrometry as an analytical tool for research in many other fields.

It was commented that there was no question directly enquiring the status of the respondent – technical, doctor, supervisory, student etc. Similarly in Question 2 there was no section for teaching.
It was felt by some that Question 4 b) - "how long do you think you will continue to work in mass-spectrometry?" - was difficult to answer because outside non-personal influences could alter their career, for example:

(i) they may be compulsorily retired,

(ii) they may be moved to a different type of work, especially in industry.

The response, therefore, may not necessarily be a measure of scientists' keenness to stay in, or get out of mass-spectrometry.

In a similar fashion it was felt by some that Question 8 relating to publication patterns was difficult to answer because some respondents considered that their work was not directly in mass-spectrometry, so that their publications fell outside the field as well. Perhaps the question should have been modified to "How many papers have you contributed in the field of/using mass mass-spectrometry?". There may then have been a greater response. Nevertheless, the respondents were all active members of the self-defined mass-spectrometry community (c.f. the mailing list), so one might have thought that they ought to be able to realise what was the purpose of the question.

In any case, only a fraction of papers listed in current mass-spectrometry bibliographies contain work that is fundamentally mass-spectrometry. Now most listed papers are on topics where mass-spectrometry is merely the analytical technique used.
MASS-SPECTROMETRY - THE DEVELOPMENT OF A SCIENTIFIC COMMUNITY

DAVID RENAULT PANTON

THESIS SUBMITTED FOR THE DEGREE OF MASTER OF PHILOSOPHY IN SOCIOLOGY AND CHEMISTRY

DEPARTMENTS OF SOCIOLOGY AND CHEMISTRY

UNIVERSITY OF SURREY

SEPTEMBER 1981

VOLUME II

APPENDIX IV - INTERVIEW TRANSCRIPTS
APPENDIX IV

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TRANSCRIPTS OF INTERVIEWS WITH 12 PIONEER BRITISH AND AMERICAN SCIENTISTS
WITH DAVID PANTON 1971 and 1972

1 LORD SNOW, LONDON, JANUARY 1971 (and with Dr B.G.REUBEN)
2 SIR GEORGE PAGET THOMSON, CAMBRIDGE, MAY 1971 (and with Dr B.G.REUBEN)
3 GAYLORD P.HARNWELL, PHILADELPHIA, JUNE 1972
4 AUBREY KEITH BREWER, WASHINGTON D.C., JUNE 1972
5 JOHN HIPPLE, NEW YORK, JUNE 1972
6 ALAN L.VAUGHAN, MINNEAPOLIS, JUNE 1972
7 ALFRED O.C.NIER, MINNEAPOLIS, JUNE 1972
8 HAROLD C.UREY, SAN DIEGO, JULY 1972
9 WALKER K.BLEAKNEY, SANTA BARBARA, JULY 1972
10 KENNETH T. BAINBRIDGE, BOSTON, JULY 1972
11 JOHN P.BLEWETT, B.N.L., JULY 1972
INTERVIEW WITH LORD SNOW

Q. Part of our work concerns the history of the Cavendish, of course - involving J.J. Thomson and the main reason why we wanted to see you was because you were there, you knew what was going on, and we wanted to have some kind of personal insight into the Cavendish Laboratory. I don't know, were you there at the time of Thomson?

A. "Well, I knew him, but actually he had given up being Cavendish Professor for some time, about eight or ten years before I arrived, but I did know him and I knew the background quite well."

Q. "..Was he doing any teaching at all? What was he doing?.."

A. "Well he was pottering on with a little bit of research, he had a room and a mechanic and did a bit of research of his own; he used to address the Cavendish Seminars and so on."

Q. "One of the things I've been reading about Thomson is that he had a rapport with quite a lot of his pupils in the laboratory; I wondered if you'd ever sat in perhaps on the tea hour or if you ever were actually present when he was doing any teaching?"

A. "Not when he was doing any teaching, but I have been present when he was giving talks of various sorts, but whether he had a rapport in that sense I don't know, he certainly didn't have much rapport with his staff because his relations with Rutherford were awful all the time as
you've probably guessed. After all, if it hadn't been for Thomson, Rutherford would have stayed at Cambridge, it was one of the classic mistakes of all scientific history. I mean, to have let Rutherford go to McGill in 1899/1900 was a ludicrous mistake and their relations were always very bad."

Q. "This was on a personal level presumably?"

A. "Yes, largely a personal level, that 'loud voiced colonial' so to speak wasn't quite J.J.'s cup of tea, but I think also J.J. does seem to me to have had on the whole very bad scientific judgement. He was a very clever man and obviously did one or two great things, but his judgement basically was, really was, far more often wrong than right I would have thought. He hadn't got Rutherford's very profound intuition; Rutherford was very rarely wrong. So you see, I mean his idea of the atom as a sort of great jelly, that's a rather typical Thomsonian thought which somehow anyone of deep intuition would never have had."

Q. "Well, I think we would like to come back to that one a little later on actually, would you like to talk about the time when he attacked Aston for instance?"

A. "Yes, now that is a classical case of getting things wrong. Aston was a very queer figure - it was said that Aston was psychologically and intellectually very much like a rabbit, but he was a rabbit who again tended to get things right. I mean he wasn't a clever man but he had real
Q. "Yes, I was about to say, how far do you think Aston's success was due to his being an extremely good apparatus builder?"

A. "That was, I should have thought 80% of it at least, he was marvellous. I wrote the little chapter of his book so I have a good deal to do with the theory. His actual intellectual understanding was not profound."

Q. "When you said that he was a rabbit, in what sense, nervous and on the look out?"

A. "Yes that's right, yes, yes."

Q. "As far as his relations with Thomson were concerned, what did he have to say about Thomson?"

A. "I don't think he had much use for him; after all, they disagreed so much."

Q. "Was the animosity between them a personal thing as well as just purely academic?"

A. "It's very hard to say, and I wouldn't like to answer it categorically; if you disagree profoundly on scientific questions of course there is liable to be some personal theories you enter into, I mean people aren't as detached as all that."

Q. "Yes, the classic attack by Thomson on Aston at the Royal Society in about 1918 on the isotope question. There always seemed in the back of one's mind, trying to be fair-minded, the possibility that Thomson was really just putting up a number of points which he felt ought
to be put, on the other hand it might be that he really
genuinely didn't still at that time believe in isotopes - ?"

A. "Yes, I think that's quite possible, I think his judgement
again was wrong."

Q. "You have no idea when he finally accepted them?"
A. "He had accept them in the early twenties, you know,
the evidence was overwhelming. After all, you could begin
to see them in various forms, you could begin to see them
in Infra-red Spectroscopy since by, really by 1919 when
the classical spectrum of HCl was beginning to be well
known."

Q. "Yes, that's fair, it's quite interesting the extent to
which one can read things into scientific papers; would you
agree that things that are not really there - just turns of
phrase and such like - give you some insight into the people
that write them?"

A. "It was of course a much more individual game; Physics was
still a matter of small apparatus, small experiments and
so on and personalities probably played a bigger part than
they would now, I think that is fair to say, you hadn't
got gigantic teams working on things."

Q. "So there was a lot more conflict over hundreds more
matters?"
A. "Yes"

Q. "Take wave mechanics, were the wave mechanic theories
held by any of the people in the Cavendish Laboratory to
A great extent?"

A. "No, they were very ignorant of that really, Dirac didn't arrive until '27 or thereabouts, it was nothing like the fashion as it was in Göttingen, or indeed at Copenhagen."

Q. "This is another interesting point, because of course they did so much of the basic work which impinged on it, yet there weren't really any wave mechanicians or indeed quantum mechanicians in the Cavendish.

A. "Yes, of course Rutherford was very anti-theoretical, his whole temperament was anti-theoretical and he ought to have got someone very much better than Fowler for instance on this sort of job. On the theoretical side he ought to have got some top class quantum theoretician."

Q. "Do you think this is traditional British pragmatism coming to the fore, or just Rutherford?"

A. "A mixture of both, they were very pragmatic and they didn't quite realise what was ahead of them, not for some years. Of course they showed in the end the mathematicians and so on. Dirac got the government chair as soon as he could possibly get it; that made a difference."

Q. "One thing we are interested in, and again it is difficult to get an impression from the publications, is the extent to which there really was a ferment in ideas about the atom, whether people really thought the 'plum-pudding' view of the atom was a sound one or whether people were saying, well really you know, something better has got to turn up in a year or two. Did people believe Rutherford's theory of the lumpy atom or indeed the later one with"
the nuclear electrons, or was there just a lot of uneasiness which they hoped would be cleared up later?"

A. "There was uneasiness, because clearly it was very odd, but I think they thought it was a very good first approximation. The theory immediately seemed to be much more right than wrong and I think only rather old-fashioned persons were trying to criticise him on old-fashioned classical lines, but they were the only people who were really uneasy, they thought something had gone wrong."

Q. To change topics can we take Aston again, it appears through all his various work that he was right a remarkable amount of times."

A. "Yes, certainly, certainly."

Q. "Did he always have the Cavendish Laboratory with an aura of always being right? How exactly did he appear in the laboratory?"

A. "No, he hadn't got that kind of charisma; I think people felt he was right because he proved himself, but he was not a powerful personality so he didn't have that air of immense confidence that, say, Rutherford had; everyone assumed that Rutherford was more likely to be right than anyone else; Rutherford certainly believed that, passionately himself, exhibiting a direct air of invincible confidence..."

Q. "One of the few occasions when Aston turned out to be wrong was the view that hydrogen was mono-isotopic which was due in fact to $^{18}_0$ not having been discovered, not in
any sense his fault, what happened here? Was he vitally interested in this from say 1920, when he did the accurate mass measurements to 1928 when it turned out that oxygen had isotopes and therefore the hydrogen calculation was wrong. Was this a central interest of his or was it just something he cleared away and forgotten about until it recurred?"

A. "I think he had forgotten about that until it recurred, he was pretty busy ticking off the list: he had other easy things to do."

Q. "That was his main concern then, to try and provide as complete a picture as possible, he didn't want to move into new material until he had cleared up all the old?"

A. "No - he was very slow to get onto the spectroscopic end, when it actually became cumulative then he had to because he realised there was something in it but he never really mastered it."

Q. "This was spectroscopic evidence for isotopes generally?"

A. "Isotopes generally, yes."

Q. "Because of course 0_{18} was one of the few isotopes that was first found spectroscopically. The other big group of people working in mass-spectra at the time was the Mattauch group in Germany and doing tremendously elaborate high-resolution stuff. This was something Aston wasn't really interested in, having thought of packing fractions he left the next few decimal places to the Germans?"

A. "Yes, well I think he felt he was doing enough. He wasn't
specially obsessive, he knew he'd made a major contribution and he was moderately satisfied."

Q. "Do you mean to say he had more or less given up any ideas of finding anything new?"

A. "I would have thought, so, yes, ideas so to speak weren't really his metier; no I think your phrase, your word 'serendipity' is very apt, very accurate about Aston. A most beautiful experimenter who happened to get on the one big thing which worked out."

Q. "Do you think it was Thomson who provided him with the idea; you said he wasn't particularly interested in formulating new ideas for himself?"

A. "I think he very rapidly got a clear idea about isotopes, you know that sort of hit him in the eye: a little like Raman doing ingenious experiments and then having one clear thing which stuck out; he never had anything else. This is not unlike Wilson the cloud chap of course, they were all of a not dissimilar scientific type. I mean Wilson wasn't a man of any magnificent prowess experimental, but he knew when he got something of importance and stuck to it for ever."

Q. "What was the extent of international contacts at that time? To what extent were Aston and Rutherford in an international league?"

A. "Well, Cambridge of course was to a sense which no University will ever be again, the centre of experimental Physics when Rutherford arrived, just as Manchester had
been while he was there and people - Bohr was constantly about the place for instance, he was very close to Rutherford on human terms as well as any other, and people came; the Germans came and so on, this was fairly common, not many Americans, no-one ever thought then that America was going to lead the scientific world and they came as pilgrims not as teachers on the whole. People like Milligan came, again not I think on very good terms with Rutherford. Bohr was the chief influence from outside, a very good one too."

Q. "Were there any Russian Scientists then?"
A. "-Yes, of course owing to Kapitsa there were quite a number, that's how H. Gamov first came to gain influence."

Q. "Would all these people enter into any large scale discussions
A. "No, Cavendish had of course a weekly tea-time meeting which everyone went to and then there was the Kapitsa Club which met on Tuesday evenings in Kapitsa's rooms, that was by invitation, you had to be a member to get a chance to go, that was probably where the most vigorous discussions went on, that was started quite soon after he arrived in Cambridge. It must have been going full strength by about '25/'26 and there you know things like the discovery of the neutron were first announced: there were quite a number of things of that kind."

Q. "Did you ever do very much experimental stuff yourself?"
A. "I did some spectroscopy, yes."
Q. "Am I wrong, were you in the Department of Crystallography for a time?"

A. "No, I was in the Department of Physical Chemistry, that was simply because Rutherford didn't like spectroscopy, which he regarded as putting things in boxes. On the other hand both Kapitsa and Chadwick realised there was something to be said for it so I was allowed to be a member of the Kapitsa Club. It was quite a small place, you see, the number of research students in the whole of this area would be not more than 60 or so."

Q. "Not a bad Department even these days I would have thought?"

A. "No, that's throwing them all together, the Cavendish, the Physicists of the Physical Chemistry Department, the Crystallographers and so on all told would be about 60, everybody knew everybody else."

Q. "And, would Thomson still come in on things like this?"

A. "No, he'd be out, and Rutherford of course would preside over the Cavendish tea-times, I don't remember him ever being at the Kapitsa Club, though of course he was very, very close to Kapitsa. Kapitsa had more influence on him than anybody else by a long way."

Q. "One of the interesting things was Aston acting more or less as a sort of referee in this dispute over tritium."

A. "Yes"

Q. "This was again presumably something where he was busy ticking off the elements when Rutherford came and said to him, 'Look can you -"
A. "Yes, that sounds about right, I didn't know the particular story but they were regarded as referees on a good many of these topics, people write to Rutherford and say is there anything in this, people like his old colleagues from America for instance, there was a good deal of debate on certain things ... Boulton and so on who is a Chemist would write and say is this right or not and Rutherford would be expected to give an answer, it was a very small scientific world."

Q. "Yes, I think this is something quite difficult to grasp. Were you aware of any isolation from the rest of the country, the rest of the academic world."

A. "Oh certainly, very confident but very small very isolated, that is true and to some extent it rather enjoyed that feeling I think, it didn't make any real effort to break out from."

Q. "What were the other people's opinion of you, the other people studying other things at the University?"

A. "I think there was great respect for Cavendish; I think by that time we had made a world reputation and people at Cambridge mightn't understand it, they mightn't even like it but they would regard it as one of the University's chief glories I think that would be true. Rutherford was such a commanding figure that you couldn't very well ignore Rutherford."

Q. "Did the scientists do anything else at the University beside experiment, did they have anything to do with the arts at all?"
A. "No, very little, very little."

Q. "So that really the two cultures thing was genuine as far as Cambridge was concerned?"

A. "Oh yes, yes. Rutherford himself read a lot he wasn't by any means illiterate, he was a nervous man curiously enough, and to get himself to sleep, he'd read lots of novels and a lot of history."

Q. "George Paget says in his book about J.J. that he only ever read detective stories."

A. "Yes, curiously enough that's probably true, but the impression you got talking to him was that a man of considerable cultivation. He was a very clever man, his IQ I think would be higher than Rutherford's, in fact a good deal more but you didn't get the impression he suffered with this absolutely deep intuition to anything he really put his mind to which you got with Rutherford."

Q. "In your 'New Men' at any period was the Cavendish a model for any part of it?"

A. "Well 'New Men' isn't of course concerned with the University at all, but some of the figures would have come from the Cavendish."

Q. "But the feeling you get ... machinations of power, I don't feel that it exists now in scientific departments so much but was it like that in the Cavendish?"

A. "Well, everyone wanted Rutherford's ear, it's very simple, this is rather like a court. Kapitsa was a sort of Prime Minister and Court Jester at the same time, and that
can cause a lot of resentment. He admitted in his letters the care he took to find out Rutherford's weak spots and play on them."

Q. "Did he publish his letters?"
A. "His letters have been published."
A. "You'll find them in the Russian library which has a lot of letters from Cavendish in there..."
Q. "How long ago do you think that was published?"
A. "Three or four years."
Q. "So presumably, from what you say, Aston wouldn't be saying on intimate terms with a man like Blakeney at Princeton?"
A. "That's right, that's quite right, the only international intimacy would be largely between the people who had worked with Rutherford at various stages, McGill, Manchester or Cambridge, that was the international circuit, Bohr remember had worked with Rutherford."
Q. "How do you feel personally about theories of scientific advance or technological change? Do you feel that this is largely a random process with lots of people trying lots of different directions, and someone making a breakthrough and everyone jumping on the band-wagon, or do you feel that there's much more selection than this, that really only a few really promising lines get tried?"
A. "Well, I think the first thing seems to me to be pretty true, that in fact there's an enormous mass movement and then something clicks like molecular biology for instance, and then of course you'll meet within a matter of months
almost, not years, a great many people on the band-wagon and this I think has been true probably all through this century. It's remarkable for instance how many bits of classical Physics were left to be picked up by persons who are not so fashion conscious. You see all the stuff on lubrication is pure classical physics; it might have been done in the 1890's except for certain experimental techniques, as well as it was done at in Cambridge in 1930's to 1960's.

Q. "Yes, while I was over in the States last year we were busy picking up some papers published by J.H. Symons in the 1940's on attenuation of ion beams which of course was all the rage between the wars, and then it got dropped like a hot brick just after the war, and so I was left with this series of really quite outstanding papers which nobody ever followed up."

A. "Yes. Oh there must be many other things of this kind, but on the other hand why certain scientific things happened at certain moments, you get this in the history of mathematics, why did three people at the same time, absolutely unknown to each other begin working on non-Euclidean geometry? I can't think of any Marxist theory which makes any sense of it or indeed any other kind of theory. Why did Lobachevsky, and Riemann and so on all at the same time and Gauss all start working on it. I've never heard any explanation which makes any sense."

Q. "To some extent, do you think perhaps it's charisma in the people, perhaps that a person who is putting one sort of theory forward appears at all the conferences?"
A. "That can happen in modern conditions, but that can't have happened with non-Euclidean geometry because they really didn't know anything about each other, in fact Gauss as you know just pressed his papers for instance. They just were at work on the same thing at the same time, and mathematical history never seems to be properly done because it's much purer than any other. I mean there are no obvious commercial reasons behind mathematics and there are no obvious social reasons behind it that I can think of and yet it follows a very curious pattern, why should analysis in its rigorous sense suddenly be all the rage for instance about 1890 and 1930 and now be dropped almost totally, I don't know."

Q. "Evidence in some areas suggests that you get a person who founds a laboratory with a tradition and that they then gather people around them who pick up this tradition and themselves often become great men and continue this tradition for some time, this seem an awfully interesting idea that the way to become a great man is to sit at the feet of a great man?"

A. "Yes, and I think there's a lot in it, you get this to an extent not only in industrial things but you do in academic things too, it's a very similar, it's the same phenomenon."

Q. "Presumably you got this at the Cavendish in the sense that they produced a succession of Nobel Prize winners?"

A. "Yes, yes, you've got it in molecular biology now."

Q. "Well then of course there were the rather poor relations between Thomson and Rutherford which suggest that
Rutherford certainly wasn't sitting at Thomson's feet."

A. "No, the Cavendish had this great explosion in the 90's. Thomson and Rutherford had a certain amount of carry-over, but I should have thought that anyone looking at the world of 1910 wouldn't have thought that the Cavendish was one of the great Physics laboratories of the world certainly not in the sense it became immediately Rutherford went back; Manchester would have seemed much more important."

Q. "Then of course there's the classic picture of G.N. Lewis isn't there, surrounded by about ten of his students who all got Nobel Prizes."

A. "Yes, this is very true and of course you've got the same kind of thing at Göttingen in Francks laboratory."

Q. "Yes, do you think that this had any implications for the way one ought to work in scientific research or do you think it just can't be harnessed?"

A. "There's a lot of luck in scientific research but I should have thought that building on strength is what I should do if I had the money to supply departments with large funds. I should be spending a lot of money on Molecular Biology at Cambridge at the moment, seeing all this snowballing effect going on, and be watching out for other things where some other figure was appearing. Mark you, I suspect you get some examples where you got people with great charisma and lots of pupils where the results were disastrous, I think that with probably a little thought you can probably think of someone."
Q. "Yes, I think without any difficulty. During this period in the Cavendish when you were there, did any outside industry have any interest in what was going on?"

A. "Very little, very few of the Cavendish, not even minor Cavendish people went into industry. There was a curious set of laws really, the Cavendish didn't enter into practical affairs till the war ended or just before then, Cockcroft was the only person who had any first-hand knowledge of industry and he was devoted to his work, he was a very humble man, he liked being steward of St. John's, ordering dinner and so on, and it was only the war that really got him into major industrial concern. Kapitsa always said how very odd it was that the English were so ignorant of technology, he of course was an engineer trained and that was one of the things that struck him most, (a) that we knew so little about technology, (b) that we were so lazy, on the other hand we somehow got the results!"

Q. "Actually, this reminds me of another point I meant to mention, how far do you think that the rate of progress of these various areas that Aston was interested in and Thomson indeed was determined by the, by improvements in say, vacuum technology?"

A. "Now that's a very interesting point, I should have thought quite a lot, so often the actual mechanical improvements determine what can and cannot be done. Often it's that more than the idea—that is the sort of improvements in crystallography were necessary before you got molecular biology, no-one doubted that these things could be done
in the 30's but they needed considerable refinements and also needed computers really to get the thing done fairly quickly."

Q. "So you think that the ideas weren't there without the machines in many cases in fact."

A. "Yes, the ideas and the machines have to come together before you get a major thing forward."

G. I think it was Aston who first invented the name.

Q. Your father definitely envisaged it as having implications for chemistry.

G. Yes he definitely considers it as a means of analysis and detection of chemical things because of its very high sensitivity.

Q. I don't think he said this in the first edition.

G. He may or may not, but he certainly did not stress it.

Q. This type of implication is what we are looking for, how far the things that have happened were foreseen.

G. Yes, that he certainly definitely foresee, no doubt about that.

Curiously enough, the first piece of research work which I did, interrupted by the war and never published was precisely on the kind of M.S being done nowadays, namely the analysis of the spectra of various hydrocarbons.

Q. Before the 1st WW?

G. Yes I had only taken my degree in the June of that year.

When I came back after the war I thought I'd do something more interesting.

G. C CH$_2$ CH$_3$ CH$_4$, not very well resolved (and pretty obvious indication of C$_2$)

C CH$_2$ CH$_3$ CH$_4$, not very well resolved (and pretty obvious indication of C$_2$).

My first research was with the Töpler pump. Gaede pump came later, Gaede was more or less a firm, the first one was a rotary thing.

The most interesting investigations when I came back to Cavendish after the war was on the isotopes of neon which was the first major discovery that came out of the C$_2$ after the war.

This had been discussed at Farnborough as it happened, I was working at Farnborough at the Royal Aircraft factory, now Establishment, so was Aston, so was Lindemann. We were all together in a boarding house called (i.dleigh,
a kind of unofficial civilian mess. A good many of the leading people came along.

Aston was already convinced that Neon had the two isotopes, but Lindeman was not. Lindeman always liked to try and disprove other people's discoveries.

Everybody expected, that was fine, then there was this 22, but could it not be doubly charged CO\(^2\), Aston said he never got a doubly charged molecule and so on.

The argument went on evening after evening. Now Aston, through a superficial experimenter, did not know very much about physics, and hadn't got an analytical brain, and Lindemann, with a good analytical brain, when they went up to bed he always won the argument. But next morning, Aston was completely back where he started from, completely unconvinced; this was very characteristic of Aston.

He had an awful job to convince him, because they had done really all they could do by the time war broke out with the existing positive ray type of machine using parabolae, which does not give you a very high resolution. The only thing would have been to make the apparatus very much larger, but this would have been fraught with difficulties with vacuum and so on. Vacuum was a very serious matter because you had to have enough gas in the discharge tube to get a discharge; all these things were made by gaseous discharges so you could not just pump the thing out and keep it at that; you depended on the difference of pressure you could get between the actual discharge you wanted and the place where the rays were to be observed, whereas of course ideally you would have liked to have had no pressure at all, and it all depended on the use of charcoal and liquid air as a means for absorbing gas.

First of all the whole thing was pumped down pretty low, then you let in a little gas where you wanted your discharge but kept the charcoal on until its absorption was complete.
You had about an hour for experiment, probably enough because something might well have gone wrong, and if I remember right the exposure time was between 10 - 15 minutes during the war so Aston decided he would not go on with the parabola method but do something different by fractional separation. Lindeman may have been some help because of the isotopes. Lindemann was very well up in what there was of the quantum theory in those days having worked with Nernst on things like specific heats of diamonds and that sort of thing, so he helped Aston quite a bit. In fact they published a joint paper on it after the war discussing the times in which you could expect separation by various methods. The method Aston used first was distillation which didn't work, and then he tried what he felt, correctly, must work irrespective of consideration of the quantum theory and that was separation by gaseous diffusion through holes from pipe clay, and the first thing he tried he didn't get anything, then he tried another with a small effect, but within the limits of experimental error. Then he built a very much better apparatus, also on the diffusion principle and which sheared the gas automatically and to his disgust however, he got no result, definitely negative. I must say that most people would have given up on that but he didn't; he decided he'd do it a different way, culminating in the design of the famous mass spectrograph and he was rewarded, because he not only proved Neon but found practically all the other elements were like it too! He missed O for a long time, and carbon. The difficulty there was that the Hydrides of those things always formed from the discharge through a gas in a vacuum which was not very good. Water has always coming off the walls, the air was only taken from the laboratory, and although attempts were made to keep it dry and so on, but if you opening apparatus several times a day it was practically possible to keep everything out. Carbon was always present and with 2 changes, always Oxygen.

It depended which way the discharge was working, they were not always proportional, it must have had something to do with the reactions between
the different ions and molecules that were in the discharge tube that
probably varied slightly with the temperature or the pressure. But you
always got C, sometimes CH, CO, CO₂ and so on.

There was one other thing that was odd and I never have known
what he really had done. He claimed not knowing the answer that you
got a line at 3 and he claimed not knowing the answer that there were
2 kinds of this 3. One of which he thought were 3ρ/81m combined
with a single charge. The other one he thought was different, how this
is no doubt true, because you get heavy hydrogen and light H₂. Now whether
his evidence was enough to justify that: I never heard him claim to be
the inventor of heavy hydrogen, but he did discuss it in his book 2nd edition.
(Paper of JJ refers to a line at 5 which he attributes to a Helium-Hydrogen
compound, in fact metastable compound of C₆, also contains several
dubious findings of unlikely compounds.

Q. How many a, abola M₃ were there?

G. There were quite a lot of people working on it, with more or less success.
There were two next door to one another, one of which was worked by Everett,
the other was worked by Aston or his predecessor, Eagle, and both under
the control of my father.

Both were made in the workshop as far as the metal parts were concerned,
and glass by Everett. Aston in fact made most of his original M.S.
with his own hands including his accumulators which gave a thousand volts.

Q. It is difficult to realise the difficulties which faced Aston in building
complex equipment, for instance he did not have the triode valve.

G. Oh, Aston must have been close to having a triode valve! Valves were
used in W.W.I and I think that these were triodes. I should think Aston
might. After all, the laboratory didn't really open at all till the
beginning of 1919, then he had to do this experiment which failed,
failing incidentally because the gas was insufficiently mixed (20 and 22 Neon)
How did you yourself feel about Isotopes, in the middle of this running battle?

I was convinced and on Aston's side that the Neon isotopes were all right. It was a sudden discovery when Aston discovered a lot of other things, notably which was the most exciting.

People like Prout said that atoms ought to be integral multiples of a suitable unit, but there were more cases of this than one would expect at random, mainly because isotopes of elements vary in order of magnitude in abundance.

From what sort of date?

From the moment Aston discovered the Chlorine. My father continued to disbelieve it, but not for very long, probably towards the end of 1920.

What can you tell us of the sort of relations that existed between your father and Aston?

Aston was paid by my father. My father was appointed as Professor at the Royal Institute and he had a salary and he was expected to give a course of lectures and there was also a sum, about £400 with which to employ an assistant.

Now he thought rather than to displace Everett who had been with him right since cathode rays time it would be nice to have someone who had some knowledge of physics and could work somewhat more independently and yet not be a research student, some one a little more senior. First he got a man named Eagle, who did not last long and then Aston came along and Aston had been a chemist in a Brewery (see his obituary) and so his physics was always rather bad and in fact when he became a Fellow of Trinity and was asked to do supervision he really wasn't a great success; he didn't quite know enough physics as a professional. So Aston came along and then they set up a second duplicate Parabola, and my father tried an awful lot of different things, different substances and Aston was responsible for very considerable improvements in the technique from pre to
post Aston, looking at photographs you will find substantial
improvement. In fact/sure, I think Aston was responsible for using
a very large discharge tube, a global one which enabled one to operate
at a lower pressure under discharge.

Anyway, this is what Aston was engaged for, and then engaged on
parabola machine, and my father liked him, and wangled some sort of
job for him at Trinity which gave him an extra stipend, and before
very long he became a fellow. He and Everett were working as a team,
you see on my father's project, you can't tell from the publication which
machine handled which substances.

Aston did not do much of this sort after the war, perhaps part
time while he was attempting to separate isotopes and building his M.S.
From now on he was entirely on his own, although his relationships with
J.J. were still very cordial. I think that the antagonisms which
appear in the meeting of the Royal Society on Isotopes were not real.
J.J. might have been jealous, I don't know, for missing things, although
I'm pretty certain he was right about the hydrides.

Q. Why do you think Aston refused to have any students working under him?
G. Oh, he was much too much of an individualist and my father would not
have pressed him in this direction. Aston was rather an old maid, fussy,
and he hated anybody coming into the room, well so did a lot of people,
but Aston more than most. He never married; he was occasionally
chaffed at Chudleigh about some lady, but nothing ever came of it,
I think, perhaps to his regret.

Q. Have you any idea what has happened to the rest of J.J.'s students?
G. The war, you see made a complete gap, there was no work done at all in
the Cavendish. As for the post war people you see, Rutherford came not
very long after the end of the war and naturally he had most of the
people and probably the best ones, so there wasn't much done, really,
after the war, by J.J.'s students. There were one or two people, of whom
Rutherford was jealous because he thought that anyone who didn't do nuclear physics was a fool; he was a very outspoken man. Appleton I think was one of them. He worked in a room opposite me for a time. He was trying to devise a device for recording alpha particles by shooting them through a little coil and measuring the electrical blow it gave to them. A clever thing before any electrical method of recording was available. There was "scintillations," of course, Rutherford started doing these before the war, but counting was no fun, an awful strain on the eyes and any thing electrical would have been very valuable.

Then there was McKeon the Irishman, went back to Ireland after the war. I remember a lot of people there before the war, before I was doing any research, because I met them socially. I remember very well those people who were on the staff, C.T.R. Wilson for example, I went to his classes. Now Aston, he was a good lecturer on Isotopes, semi-popular lectures. He had the sort of mind to do a course like this, where you needed to focus very clearly on one thing. He was an attractive man, pleasant socially, and easy to get on with.

Q. What was the scale of research student activity and how many students on an average would your father have working for him post graduate?

G. I should say that at the peak, about 30, immediately before the war. 25 would have been common. The extension was built, you see to accommodate them in the garage.

When Aston built his first M.S. he started with some brass tube and brass rod, I'm not sure if he did all of the metal work, but her certainly made the accumulator and I think he did the delicate bits, like making the slits. No I suppose the straightforward bits were done by Lyncombe in the workshop under Aston's very careful supervision, I expect he was popping in and out every half hour to make sure they were doing it right and of course the glass blowing was all done by Everett, Aston could glass blow, but he was not as good as Everett.
He may have done this bit of glass blowing, I am not quite sure.

Q. How much work did you do with Aston? There was one joint paper wasn't there?

G. Yes it was on the isotopes of Lithium. I did the thing first, but I wasn't brave enough or rash enough to go it alone. I got one picture which was absolutely clear with the two things and then somehow or other the apparatus would not work and I toddled on, but couldn't get another picture, Aston was doing it by a somewhat different method. The only reason was and I think I was a fool to have bothered about it, but during that exposure which happened to be fairly long and I left the room open and someone or other may have tampered with the equipment - moved the setting of one of the rheostats or something and not realised what they were doing, but I think now this was highly unlikely. There was absolutely no doubt about the picture at all. Perhaps the only thing was that the two lines were rather more equal in intensity than one would have expected. I don't think the ratios were far wrong, the chemical ratios were 6 & 7, they however looked more nearly equal than that, it must have been some photographic effect. Anyway if you've only got two things, it's practically impossible to say if one of them is three times blacker or 10 times. Anyway, we put the results in a letter to Nature, which we signed together.

I was the first person to find e/m for anode rays. I went to Aberdeen in 1922 and when I came back I had to decide whether to continue with anode rays or try something different. I thought, Aston is so good at this, I shall only be trotting along behind him, so I dropped that and went on with scattering of positive rays, while Rutherford was doing particle scattering and I spent some years on that, which came to nothing. If I had have continued with anode rays in the parabola machine, I would indeed have been trotting along at Aston's heels because you see, apparatus was very important and difficult.
Q. Could you tell us about the events that led up to the discovery of the parabola method?

G. He had done something very like the parabola method before, not identical, but he was very familiar with the idea of using two kinds of field, electric and magnetic fields - you see he used these to find e/m for the electron.

Q. Do you think this was extrapolation?

G. Oh yes it was, he just wiggled it about a bit.

Q. Do you think he realised at the time that getting these parabolas was in fact a proof of Dalton's atomic theory that all atoms of a particular substance weighed the same?

G. Yes, I think he did. What he was really interested in was looking so to speak for the proton. All right, electrons are negative, so they've got to be positive, obviously - what is this - is it conceivably something which is different for every element, which would fit in very nicely with his first theory of how the electrons were arranged, or was there some unit?

He came to the conclusion that there was no positive thing lighter than the Bohr hydrogen atom less an electron. That was the first object of the operation, so to speak, and that was successful. Then he got very interested in the chemical side of it and all the funny things that came out of it, like non-valency compounds.

Q. We were going to enquire about the tea parties?

G. There were two sorts of tea parties, the ordinary daily tea which J.J. had in his so-called room, which was the office of the department. All research students would come to this. As Rayleigh remarked in his Life "the quality of the biscuits left much to be desired!"

The other was a little more elaborate; there was a colloquium which
was called the Cavendish Society; about once every three weeks, somebody would be told off to give a paper at the meeting. Before meeting in the lecture theatre there was a tea, and my mother usually presided at this.

I only once went to a meeting of the Kapitsa club; I was not strictly a member because I had left Cambridge before K came.

Q. Aston did not have any students, so when he died did anyone take over his machines, or did M.S in the Cavendish come to a full stop?

G. No one really took over, no, practically concluded everything he wanted to, with his 1st, 2nd and 3rd M.S.
DRP: Perhaps we could start by discussing your early career. Were you a chemist or a physicist to begin with?

GPH: Well, my initial start was at a small college here in Pennsylvania actually. I received a prize which would authorise me to go for a year to graduate school, and Sir Ernest Rutherford was sufficiently kind to let me into the Cavendish Laboratory where I went for my first year of any graduate training at all. And at that time I did not work directly with any Mass Spectrographers, but C. T. R. Wilson had his original cloud chambers there, upon which we worked.

DRP: You were engaged on that?

GPH: Yes, together with a chap from Wales. I had a very interesting year, and at that time I discovered that this was what I really wanted to do - namely physics - so I returned after that year to Princeton. I spent two years getting my doctor's degree there, working at that time with some ancient Mass-Spectrographic equipment which had been put together by Harry Smyth and Harry Barton, who were my predecessors in running this particular piece of equipment. So I did my thesis on various varieties of gas combinations and collisions of the second kind in this situation.

DRP: Henry Smyth was also at the Cavendish?

GPH: Yes, he was there, and indeed I think much of the tradition at Princeton stemmed from Alan Shenson, an Englishman, and Harry Smyth, who had both spent their time there at the Cavendish. Then after I got my degree and a scholarship to go to Cal Tech, where I spent a couple of years, I returned to Princeton to teach, and eventually came back here to run the Physics Department. By that time I was pretty much out of the experiment
business, because it took up all of one's time to try to build up somet
which didn't exist and get a lot of other people started out in physics
Well, that's my first-hand connection with Mass-Spectrography.

DRP: So for about how long were you actively engaged in Mass-Spectrometry?

GPH: I wouldn't think more than the two years I was working on my thesis at
Princeton, because after that I got into cloud-chamber types of activity
mainly, and the interests that Cal Tech had were not coincident with mi

There was another professor named Smythe there who was interested in Mas
-Spectrography, but his programme was not the one which Mr. Millikan allo
me to work on, so I worked on something else while I was out there.

DRP: Did you build any of the machines?

GPH: Oh yes. You were almost always rebuilding things when you were at it, a
you know, but the rebuilding was done primarily in the workshops at
Princeton. At Cal Tech with Smythe, the other one, we worked together
informally, as my business there was really not Mass-Spectrography. So
my connections go a long way back, but they've never really been continu
in any detail.

DRP: So were you actually building the machine yourself? Or were you supervi
or what?

GPH: No, at the stage where I was, I was supervising nobody, I was a dogsbody

DRP: It's quite interesting to see the development of Mass-Spectrometry: how
the technology at any time has more or less placed the limitations upon
the science that's based upon it. What were the problems with your mach
that you remember?
GPH: Well, the problems were of all sorts. One was that one didn't have any hard glass to work with in those days. As a result, one had a great deal of trouble with the soft glass and one became quite a competent soft glass blower. The things we were working with were very emanescent indeed because they would bust, but there was a competent glassblower to help y and a competent machine shop which helped you devise the metallic parts. The equipment was held together largely by varieties of wax, which presumably had as low vapour pressures as one was able to find at the tim but by no means the kinds of vacuums that you are accustomed to today.

DRP: What sort of pumps were you using?

GPH: Well, we used of course the ordinary variety of low vacuum pump which th Central Scientific Company here in Chicago made in considerable quantities then mercury diffusion pumps, for there were no low vapour pressure oils at all. So it was a straight mercury variety of pump, and one just accepted the vapour pressure of mercury as the limiting factor. Liquid oil was available, of course, and one used it, but the limitations in th design of the equipment and so on were such that one never achieved any that could be considered a reputable vacuum.

DRP: You were interested when you were working in Mass-Spectrometry in positiv ion kinetics. Was there any work on isotopes being done near you? Did you think that the general study of isotopes using the Mass-Spectrometer was coming to a conclusion?

GPH: Well, I think the general feeling was that probably there was quite a lot more to be done about isotopes. It was recognised that the orders of magnitude to which you could work in precision were not such that would uncover isotopes present in small quantities - a few per cent was consider quite good - so that as the technique did not actually disclose any very
low intensity isotopes, nobody knew they existed, but there was always the feeling that your order of precision was not such as to exclude them all, and it was fascinating to watch the table evolve and see what finally came out.

DRP: Did you have any contact at all with Frederick Aston at the Cavendish? Because of course when you were there he was busily engaged in ticking of the isotopes.

GPH: Well, of course, it was the kind of place which was very homey indeed, and Sir Ernest, known locally as 'the crocodile', was the kind of guy who came in to see you almost every morning, certainly once a week, to find out how you were going. His favourite phrase was that 'research is all difficulty, but you mustn't mind this.' People like Aston gave colloquia so that one got to know them. C.T.R. Wilson, of course, took an interest in this, because this was the equipment which he had himself built which we happened to be working with. It was an extremely stimulating place, the most stimulating laboratory I think in which I ever had the privilege of working.

DRP: But you didn't have an awful lot to do with Aston himself; would you agree that he was a bit of a recluse?

GPH: No, he was not a very outgoing individual; I saw a lot more of Cockroft, who was a much more outgoing individual. He was a few years older than I, but not so much as all that; he was kind of a permanent fixture by that time, because he had various colloquia which he managed also, and which were always very interesting to do to.

DRP: What was your opinion of Aston as a researcher while you were there?

GPH: I really have no opinion of his technical competence as an experimentalist, but I had great interest in his organising and synthesising interests which
he was carrying on very effectively, and which I think got many people interested in the periodic table to the extent they never would have, if not for the steps which Aston took toward getting away from the very simplistic approach that there was one kind of oxygen, one kind of hydrogen, one kind of iron, and so on, and realising that you had these multiplicities.

DRP: So he was a bit of a popularizer in this respect?

GPH: Well, that was certainly what I had learned from his, yes. He was a very clear expositor, and consummately interested in what he was doing, and he was a synthesiser, I would think, rather than a direct experimentalist himself. These are always the most interesting kinds of people, I think. If you are an experimentalist, you get so involved in what you're doing that your perspective is too limited to really serve the kind of function that Aston did.

DRP: How about Henry Smyth?

GPH: Well, I knew him and his brother Charlie very well indeed, because they were old Princetonians. I did my degree with Harry, and he was very much interested in gas kinetics. His brother was a chemist, he was a physicist, and they had a lot of common interests, so there was a good close co-operation between the Chemistry Department and the Physics Department there. He was not a terribly adept experimentalist himself, but had good ideas and was very constructively critical of all the kinds of things that were done, so that he I think helped to keep the kinds of experimentalist and general academic standards. These were of less interest to Karl Compton, who probably you would never have encountered in your activities, but who was the moving spirit, as far as Princeton was concerned, in physics in those days.
DRP: So Henry Smyth learned his Mass-Spectrometry from Aston more or less directly, didn't he?

GPH: Yes, that's right.

DRP: Did you remember any conversations you had with him concerning that train

GPH: Well, we had enough in common because I had spent a year in Cambridge before I came to Princeton, so that I certainly gleaned from him a very great respect, over and above what I had already gained by being in Cambri for the leading nature of so many people at Cambridge. The exceptional it was for people who could see interesting and significant things to be done in physics, and the entrepreneurial abilities of Sir Ernest Rutherford were just invaluable in this. Laboratories, as I'm sure you're finding out, have a wide variety of personalities in them; different ones perform different kinds of functions, but I don't know whether Sir Ernest ever pick up a piece of glass that he didn't break. He couldn't have been less adept as far as personal manipulations of himself were concerned, but on the other hand he had a very keen eye indeed for people with good ideas and supported them very well, and took great pride in what came out from other people.

DRP: A charismatic sort of person wasn't he?

GPH: Yes, Aston was much more of a recluse, and it took a longer time to get to know him, but I have great respect for his insights and his greater experimental ability than Sir Ernest.

DRP: Do you recollect the controversy that was raging right at the beginning of the 1920s, to do with the chlorine and the neon double lines, where Aston was proposing that it was possible for isotopes to exist for every single substance, but J.J., at that time the President of the Royal Society,
preferred to think that this wasn't the case; in fact, he attacked Aston's evidence quite savagely at a meeting of the Royal Society. Can you remember this period? What was your opinion, as far as isotopes were concerned?

GPH: Well, of course, by the time I got into this there was really no question whatever that there were isotopes. I have no personal recollections of that kind of a controversy, but I am not surprised that if there was an opening for controversy, it would indeed arise, because here were people who had a consuming interest in this and excellent intelligence and capability, so if they didn't agree, then they disagreed strongly. So this doesn't surprise me at all, but I have no idea what they said to one another in the early 1920s - I wasn't around.

DRP: With hindsight, you see, it is possible to argue that because of J.J.'s insistence, the general acceptance of isotopes was held back for five or seven or eight or more years.

GPH: This could be argued as a great handicap. On the other hand, though, the emphasis on being sure, on questioning somebody else's experimental result this has always been the obverse side to the coin. There are plenty of mistakes being made today as well as then, and so if there is not critical scrutiny applied and alternatives suggested, and so on - this is the leading edge of scientific development - if people just accepted immediately the results of someone else, this would be bad. I didn't get the impression, having seen them both together in the later 1920s, that there was any personal problem between them. One was a recluse and the other was kind of grumpy; I don't think there were any adverse effects that that might have had. It all got ironed out in time; they were convinced, and I don't believe there was personal animosity, or at least it was never displayed.

DRP: You worked with Walter Bleakney for some time - that was about 1934 or 1935?
GPH: I guess around then, I don't remember. He came to us from Minnesota where Jack Tate was, and we were interested in the same things, and we worked together for a time on one thing or another. I no longer recall the details because I was so busy with the jobs which were given to me by Dr. Compton in the teaching and development of primarily instructional laboratories, which they had rather few there, and trying to build up a modern variety physics laboratory. My participation in research became less as this went on, which is indeed why I ultimately turned into an administrator at the University of Pennsylvania.

DRP: But you did collaborate with Bleakney and a couple of others?

GPH: Oh yes, we were working together on - I can't remember whether we wrote any papers together or not - it's conceivable. We had some unsuccessful attempts at isotope separation together which didn't come out like we thought they should have, and indeed they were terribly ingenious ideas which did not work. However, if it had been done with current equipment I think they would have worked, it was just quite beyond experimental possibilities in those days.

DRP: Did you know him in 1931-32 when he discovered deuterium about a month after Urey's discovery? They were working on the same thing. Do you have any information about the happenings at that particular time? Talking of mistakes for example, he discovered tritium when in fact it was impossible to discover it with a Mass-Spectrometer. After all, he was beaten to the winning post over deuterium, so he was determined to find tritium; he found it, and then couldn't repeat his experiment because its abundance was much too small a quantity to show on the spectograph.

GPH: Yes, I guess he tried to set an upper limit to how much there could be. All I recall was a certain amount of scientific embarrassment; I believe the results were attributable to error and the uncleanness of the equipment at
that time, and that kind of thing.

DRP: Do you think this was a case where he knew he was looking for something: by Jove, he was going to find it?

GPH: That's a very hard question to answer, because it was done with the best intentions. There is always a desire to be first to publish something, while the experimental results were not - as I recall - repeatable to the order of magnitude that one would like to have them, there was enough there so it was a matter of judgment as to how strongly one put the case for what one had actually found; how modest one was in saying, well, there's plenty of chance for error here, but this may indeed be what we're looking for.

DRP: Aston tried to repeat the experiment immediately afterwards, but he couldn't find it. Do you think Aston was a sort of judge in these matters? Would he be called in by American physicists over disputes like this; what would happen? Was he regarded as being at the top of the tree?

GPH: Oh no, I don't think so. I don't believe the ideology was sufficiently fixed so that anyone was convinced of the theory to the extent that one had some hierarchy of people who knew more than anybody else. But there was great uncertainty as to how sure indeed we are of this theory, so that I don't think that anybody ever felt like calling in anybody else, except somebody who could devise better techniques for showing the kinds of impurities, and better techniques for measuring the things which you were trying to measure. When you think of those quadrant electroscopes - I suppose you've seen them in a museum - similar to the ones we worked with, and the high watchmakers' technique that went into it - wax was used in the construction, etc. - experimental physics was quite primitive.
That period was very much a period of tinkering around with machines.

It was tinkering, that's right. The technology of instrumentation had not reached the stage that one really knew what one's precision was.

Was the Mass-Spectrometer a very hit-or-miss type of apparatus altogether?

Yes, it was. You spent more of your time trying to keep it in what you considered reasonably good shape than you did in any measurements with it. The materials were vastly inferior to what they are now. The techniques were again vastly inferior. In a way, the development of science itself of course had tended to improve the equipment that it has to use, but one always pushed this shaky stuff to the limit of what you thought it would do, and wrung one's hands when, after weeks or months of work, it didn't work at all. One had to go back and pull it all apart and see if you could put it together again in a way that it would work.

So with the Mass-Spectrometer, were the results considered pretty shaky even when you did get them, or could you be reasonably sure that you were getting something of what you expected?

In the way of fitting it with a theory to improve the quantitative data with which you were working, the limits of error were sufficiently large so that one felt rather like an explorer who was being buffeted by winds and wolves and so on, and who couldn't see well enough out of his parka to be perfectly sure what it was, but who was finding a very interesting lot of landscapes in front of him, which were discerned only vaguely.

Was Mass-Spectrometry at this period a fairly competitive sport, if you...
GPH: Oh yes, I think so. Of course, the Cavendish was its home and the various centres over here at Minnesota and Princeton - they are the ones I think of primarily, because I knew the Minnesota group so well, but I guess there were quite a number of laboratories, and I guess you probably know better than I do which ones were actively engaged on it. There was an American Physical Society which brought all these people together periodically. Small groups of Mass-Spectrographers got together and argued the various shortcomings of their equipment and the dubiousness of their results. They built large castles on their correctness and found that they tended to intersect in a few places, but also diverged to a wide variety of facades.

DRP: So in this period it was fairly international, nevertheless. Aston would come over and have a look at the machines.

GPH: Yes, indeed, and I think they had more skilful technicians at the Cavendish. Oh, I don't know that that's so, but I got the impression that this was the case, so that I would guess that much of Aston's equipment was indeed more skilfully constructed. I never knew how much of a fabricator Aston was himself.

DRP: He was a pretty good one. To take his first machine: he manufactured with his own hands his batteries for his steady voltage. He more or less started off with a lump of lead and a few pieces of copper tubing and a chunk of glass, and he went on from there.

GPH: I think there was nobody like that on this side of the Atlantic Ocean that I ever encountered.

DRP: He was one of the most complete experimenters.
GPH: I agree this was a characteristic of his which I'd heard of before. Although he was rather a recluse in nature, at least this was my impression of him in talking with him and working with him you didn't quite get this impression - I guess he glossed over the extraordinary diligence and meticulousness which he had, and concentrated on his results, but as an offshoot of that I'm sure he had great technical skill. His results were in general better than anybody else's. It wasn't sufficiently clear to everybody that this was the case, but it should have been.

DRP: So where did the competitiveness lie? Just in building the machines, or in the results as well? Was there a race on?

GPH: Oh yes, there was definitely a kind of race on. I can think of no-one on this side of the Atlantic Ocean who did as much of the work himself as Aston did. I don't know what kind of teaching loads and so on Aston had, but most of the people who were engaged in this research over here spent maybe half of their time in the laboratory, if that much, and the other half doing the ordinary instructional jobs around the Department, so that the degree of application per man I think was less intense here. I guess, on looking back, that this was a great disadvantage really - if your interests are divided and time is divided and so on...

DRP: Was this just a question of politics, or was this to some extent a choice by the experimenters themselves? Aston enjoyed lecturing; he wouldn't have preferred to experiment all the time.

GPH: Well, how much time do you think he put into his general undergraduate-type of instruction service? Over here the fraction was generally quite large.

DRP: I don't think it would be as high as that. It would be about a quarter of his time, probably.
GPH: Well, the additional time he had to apply himself and the additional abilities which he had indeed to do all these things himself paid off.

DRP: When you were working with Smyth and Barton in Mass-Spectrometry, what sort of budgets were you working to? Who financed it?

GPH: I never know anything about budgets, or at least, I never did.

DRP: Did it feel pretty shoestring at the time?

GPH: Oh yes. The laboratory could afford to buy a certain number of electrometers and these were then used to the best of their ability. Dr. Compton was generally dividing up laboratory resources, and there was a glassblower, there was a machine shop which was not very good or high-precision. I don't think it was by any means as good as the one at the Cavendish. It should have been, but the artisanry here was newer I guess, or was taken less seriously, or something like that, because I don't believe that in general the level of skill was as high as it was amongst a few people at the Cavendish. I guess that's why the reputation of the Cavendish was so much better as far as physics was concerned.

DRP: Nevertheless, Mass-Spectrometry came to a dead stop in the Cavendish. Aston had no research students, or very few, and once he completed his work, 'that was it. Whereas over here it snowballed much more so than in Great Britain. The Cavendish took a pioneering role, but having done so...

GPH: Well, this is a phenomenon which I had associated more with economics and the degree of industrial expertise which had permeated the United States. That kind of explosion of techniques which occurred here was, I think, because of the general greater tendency toward the engineering side of sciences by Americans, and the greater basic interest which a laboratory
like the Cavendish had in the theoretical significance of what it was they were finding.

**DRP:** Do you think Mass-Spectrometry was one of the most important branches of research physics at that time? How was it seen in regard to the rest of research?

**GPH:** Well, I think inasmuch as it measured masses, and masses were one of the foremost important kinds of qualities that matter had, there was no question whatever of its fundamental nature; but I think that vision was not sufficiently broad to see how many different kinds of application this had - from nuclei to molecular reactions, indeed to the fabrication of new materials, and so on, and astrophysics and heaven knows what. But I think what were regarded as the borderline techniques which contributed later to the modern variety of Mass-Spectrographs were belittled to some extent.

**DRP:** Have you kept up with the progress of Mass-Spectrometry?

**GPH:** I have read, of course, with great interest, but superficially, about the experimental and technical advances in the way of greater refinements in precision, and both in measuring electrical magnitude as well as in measuring all the other quantities that come into this, and it's entirely as an amateur that I do this.

**DRP:** It's interesting to see how Mass-Spectrometry to begin with was a physicists apparatus; it was developed by physicists, and up to the Second World War it was more or less dominated by physicists. Then as the advantages could be seen of it as an analytical tool, well, as you say, it was taken over
by peripheral interest groups, and then the chemists really took it up an important piece of machinery. How have you viewed this?

GPH: I think that's certainly true. Anything that succeeds in measuring something to the next decimal place automatically, apparently, spreads out in its impact throughout all of the exact sciences. I'm sure the bio-organic chemists today would never be putting together the kind of models they do if there hadn't been this degree of precision to back the up. It's just incredible the kinds of things electron diffraction and Mass-Spectrography could get into. Almost any really solid advance anybody makes in the way of more precision seems to pay off enormously in all kinds of related fields.

DRP: One interesting thing is that in the 1920s, ion molecule reactions were discovered at the same time as isotopes in general were discovered, but isotopes were felt to be physics and this was an OK subject to study with a Mass-Spectrograph. Ion molecule reactions were somehow under the chemist's umbrella, and these were left severely alone. It was only real with D. P. Stevenson in the early 1950s that ion molecule reactions began to be studied for their own inherent interest. How do you think this sort of resistance to some extent of various uses of the machine happened

GPH: Well, I doubt very much if that particular facet of the subject is at all localised in physics. I think it is the kind of thing where whatever you are closest to yourself, and the things you do, grow out of all proportio to the individual significance of what you've got. The generalist who eventually comes along and has sufficient competence in two fields to put these two things together makes the greatest contribution, I think. There are enormous numbers of resources in physics which were not ever associate with chemistry until quite recently, but which made an enormous amount of difference to chemistry. In the same way, the physicist who felt that the
chemical reactions were things which were beneath contempt, but which are indeed only interpretable in terms of the kinds of things that he knew; if he had been broad-minded enough to put the two together ... I guess, however, that human beings are limited to the extent to which their real intellectual concentration can be extended. Unfortunately, we've got blind spots and all kinds of things.

DRP: Do you think it might be attributable to the rather more classical aspect of physics education vis-à-vis the training of the chemist, where, for example, their thermodynamics is basically billiard-ball stuff? Do you think there's a subtle difference in the education of the two branches?

GPH: Yes. And I think modern approaches to scientific education have helped quite a little. There was a time when one could become a physicist here or at Cal Tech without I think taking more than a course in general chemistry to start with, and very elementary mathematics, but the situation now is different in most institutions. It's by no means perfect, but you really have to have a considerable amount of insight into the neighbouri areas of scientific knowledge before anybody thinks that you can approach maturity as a scientist.

DRP: Yes, it's interesting to see this change, isn't it? Go two centuries back the scientist was a generalist and he explored all pathways, he was a philosopher. Then we have a period - say before the Second World War - where the education was pretty rigid; the physicist went down here and the chemist went over here. Do you think we're coming back to the rather more general philosophical aspects in the education of scientists?

GPH: I have no idea whether we're pursuing a wiser course now than we used to. It requires another fifty years perspective back on us to see what is
going on, and I don't know whether other people's reactions are the same as mine, but the solid things which I think I learned in college or graduate school had very little to do with the current kind of interest in what is artistic or unique or peculiar in the human being, and the kind of flaunting of not all of the customs of the society, but at least considerable denigration of them. Surely one becomes more interesting if one knows a great deal about music and drama and poetry, but my prejudice being such as they are, I feel that although these are delightful advocates the idea of their having something sufficiently solid upon which to build any intellectual edifice that you would like to live in, it just isn't there. This is not for the record; no, I've expressed my views very freely. I wonder what's going to happen to the scientific effort which been so fruitful for the last century or so, because the intellectual tenor of the time is now in a somewhat different direction, and maybe we have indeed come to something of a change. It'll take a while to digest what we have found out before there is again a consuming curiosity to make further explorations.

DRP: Did you think during the 1920s and 1930s that you were living in a waters of science at the time? This was a period where an enormous amount of discoveries were made, in the microcosm at any rate, as well as anywhere else. How did you see science at that particular time?

GPH: Well, you had a very much narrower view of it because, as you've just been saying, the compartmentalisation of science was more complete, so that I for one thought that what was happening that was interesting was in physi because this was the area I happened to know about. The chemists were do what they could, and the biologists clearly didn't have much to build on anyway, because their experiments were not of a precise kind that really left you with something in the way of a puzzle which was solvable. So that you lived in a very much smaller world and you were only aware of a small
fraction of what was being learned. Today it may be better or it may not be, because while things have gotten farther along, it's a little simple to understand things after a generation has absorbed them. On the other hand, I think I detect a lesser interest in intellectual exploration and a sort of practical interest in their applications, but within the last eight or ten years, some uncertainty as to whether even that is worth studying, because men put these various practical applications to such dubious uses that it may be just as well if we didn't know any more than we do - which is a sad state in which to get, but there it is.

DRP: To ask a fairly general question - quite a few of these have been pretty general: to take physics, how have you seen its progress say from the years 1900 to 1950? Do you think there has been a steady state system of advance, where each individual scientist has added on a building block to the general pyramid, or has it in the main been due to scientists having a piece of luck in grasping the significance of a particular phenomenon; an accident, if you like, which one can argue happens for quite a few things? Do you think science has progressed according to a combination of the two; steady state for say a period of five years, and then suddenly a revolutionary change in both the theory and the practical applications of it, and science has moved onto a different level of consciousness and appreciation? How do you regard the general development

GPH: Well, that's a tough one, isn't it. It's a little like all the blind men exploring the elephant; the only part that you have really any critical cognizance of is the particular part that you happen to be examining at that moment. Sometimes with age, and maybe sometimes much more youthfully now, you endeavour to get these overviews of the boundary areas between the sciences, or the relevance of science, or its applications, which are very meritorious it seems to me, because it is these applications which enable the basic scientist to get instruments which enable him to do more precise
explorations of the particular things that he is interested in. I mean, he is not interested in the instrument itself, which is an offshoot from another evolutionary line that has come back to help what it is that he has in mind. I guess maybe I have as yet to meet anybody who is sufficient a generalist who begins to comprehend the consequences of the great assemblies of precise knowledge which have been put together. The whole area of integration is so large that it tends to get broken down into smaller areas, but the 'totality of knowledge' is so large that the refinement of the structure within it has gotten into some difficulty. I can think of no-one who is capable of pursuing a position at the top and claiming really to be able to integrate all the kinds of things we know, and I would guess with the passage of time the possibility of this would get less and less.
DRP: Perhaps we could begin by talking about your initial training. Were you a physicist or a chemist?

AKB: Both. I started out in chemistry. I got my Master's degree in chemistry and then my Scottish ancestors didn't like my burning holes in my pants, so I decided to be a physicist. I liked mechanical things better anyhow, so I changed over to physics, and got my PhD in physics in 1924.

DRP: What was that in?

AKB: It was a kind of combination of chemistry and physics. I was trying to see what forms of energy could produce ionization, and whether chemical energy could produce ionization. I was studying the gaseous oxidation of gases to see if any ionization would be produced, and I found there was some ionization. Then I had a National Research Fellowship in physics after that, and studied under Millikan at the California Institute of Technology. I was there three years.

DRP: So you knew Gaylord Harnwell then, because he was also working at Cal Tech.

AKB: Yes.

DRP: So when did you first start studying Mass-Spectrometry?

AKB: After I left Cal Tech I came to the Fixed Nitrogen Laboratory which was actually under the Department of Agriculture. But I was studying surface forces - like on the platinum catalyst and that sort of thing. I was using photoelectric emissivity and the thermionic emission. I had made quite a study of thermionic emission in the presence of gases, and the effect of gases on the shift in the photoelectric threshold.
Well, the question was then: do some gases shift the photoelectric thre
because ions are produced on the surface? Well, that was my theory; the
were plenty of chemists who didn't agree with me, but I was trying to gi
it a strictly physical interpretation. But we had another reason why I
started in Mass-Spectrometry besides finding out what the ions were. Th
other was that we had one of the foremost authorities on potash - potass
in our laboratory. He was a very influential man and he wanted me to fi
out all I could physically about potassium. His name was Turnkein and h
later became the President of the American Potash Institute. But the
problem then was to find what are the ions which are made on surfaces, a
also to find out all I could about potassium. So I built the first Mass
Spectrometer in the East. There was one out at Berkeley, but it wouldn'
give you good abundance ratios; you couldn't get accurate abundance rati
at all.

DRP: What geometry did you use for that?

AKB: It was a 180° Dempster-type machine, only Dempster never really got sharp
peaks on his. He never could get true abundance measurements because he
didn't have a good ion source. I started in first to try to solve the
potassium problem, so what I was interested in there was in getting a go
ion source. You see, I'd been working with thermionic emission and I wa
the therm-ions which were given off anyhow, so naturally the first thing
did was to supply a thermionic source. The first thermionic source I ma
that worked, worked quite well, only it spread the beams out quite wide,
then I developed electrostatic foucussing by putting an electrostatic shi
around it where I could put a potential on that with respect to my therm
source, and so concentrate right on my slits. When I did that, I found
that I could get extremely sharp peaks, but I didn't want too sharp a
peak, I wanted a peak which came up and was flat-topped. - then I was sur
I had the entire beam, you see. I had very good flat-topped peaks, so I could get the abundance ratios of various isotopes of potassium, and I was able to get a very good picture of the potassium 40 peak.

The problem was then to find out whether I was measuring the true abundance ratio or whether there was an isotope effect in the evaporation of the potassium from the platinum disc. What I did was to take a little platinum disc, about a millimetre-and-a-half in diameter, and spot-weld that on to a tungsten hairpin which was then in my machine. I then put just a spec of salt - any salt of, say, potassium - on the disc, put it in a tube and evacuated it. After achieving a good vacuum I heated it, and I found that the potassium - or lithium, or caesium, or rubidium, or what have you - would dissolve in the platinum. After it had dissolved in the platinum, took it out and scraped all the salt off the disc, so that I would have uniform platinum surface, because I wanted a constant work function over the surface. If there were several volts variation in the potential over the surface, then I wouldn't get a sharp peak. So I got a nice clean platinum disc, and then I put that in the Mass-Spectrometer, and the result was that the variation in the work function over this platinum surface was very small, it could have been at the most only a small fraction of a volt. So that's why I got such sharp peaks and could bring out so clearly the potassium peak.

DRP: How long did it take you to build your machine?

AKB: Well, naturally I had some troubles because I didn't have an ion source to begin with. There were ion sources in the literature: one used out in California and Dempster's. Well, Dempster's I saw right off couldn't give me a sharp peak, but otherwise it was a Dempster-type instrument. You could follow the Dempster instrument, but then came the problem of getting the ion source. Well, I think I played around - not too long, maybe a month - to get it,
but I already had the experience of studying the photoelectric thermion emission of platinum surfaces and all types of metal surfaces, so actual I didn't have to go back and discover that effect again.

DRP: What pumps did you use on the machine?

AKB: Well, I had my own pump - a vacuum pump. I used just an ordinary pump, but I had the design of a high-vac pump of my own which was very m faster and gave you a better vacuum. So I was working at vacuums around $10^{-7}$. Now this pump - it was an ordinary mercury vapour pump, but it wa a type I designed myself. I can give you a picture of it if you want me to; it is also pictured out in some of the drawings I have of the Mass- Spectrometer.

The big problem of the Mass-Spectrometer of course is to get reliable re I had a NAT-P54 tube that I was reading currents with, and I could read down to $10^{-16}$ of an amp. Now most of the people that worked with these were dealing with around $10^{-12}$ or $10^{-13}$ of an amp at the very most. Wel the larger your ion current, the more you get a space-charge spreading, s $10^{-13}$ of an amp was the biggest current I'd ever use. Narrow it down to $10^{-16}$ by going to very small currents this way - I had essentially no spreading. Then I read it with a galvanometer, but that was no problem.

The first big problem I had was to find out whether, in the emission of potassium from platinum, there was an isotope effect. Should I have ap the square roots of the masses on the laws of evaporation from the surf So I impregnated a platinum surface and then ran it and ran it and ran in the Mass-Spectrometer until I couldn't get any more potassium out of and I found that there was only just an infinitesimal change in the abu ratio. Now when you do that with lithium, then you get all the changes the world, for the simple reason that the lithium ion, when it is
dissolved in the platinum, is very small compared to the spacing in the platinum, so it can float around and you get re-evaporation from the surface. But with potassium and rubidium you got no change at all. In fact, with rubidium I couldn't get even a semblance of change in the abundance ratio, because the ion was so large that they came out just like checker

Now, my value for the abundance ratio of potassium of 1420 is not the same as Nier gets. I think Nier gets about 1411, 12 or 10. But I think he has some isotope effect in his machine which he doesn't want to admit, and I don't want to admit it in mine, so the question is which one's right, but then it is rather a little difference anyhow.

But as far as my work was concerned, after I had made a study of potassium and got potassium 40, I naturally wanted to see which one was the radioactive isotope. Well, there wasn't a decent Geiger counter on the market at that time, so I had to design a Geiger counter. But that again was very easy for me because I used the knowledge I had in electric disc.

I bought all the Geiger counters I could find at that time, but they were really sensitive enough to use on potassium. I was working in the laboratory one night and the idea just came to me how to make an End-wing Geiger counter. Well, I made it - just stuck together with sealing wax - and it worked so perfectly that the boy who working with me - next morning his eyes just stuck out of his head when he saw it working. Well, we went to the machine shop then and got good ones made. Unfortunately at that time the Government had a patent policy which meant that I couldn't patent it. While they were trying to get that corrected, some agencies in the Government - like the Navy - let you claim shop price. I could have made myself rich out of that Geiger counter because everybody used an End-wing Geiger counter now. But anyhow I was the first to publish on it. Anyhow I studied the isotopes - each one of them - and I found of course that it was potassium 40 that was the radioactive one, which was very easy to do.
Then the thing I was interested in - which as far as I'm concerned has had a big repercussion - was: what is the potassium isotope effect in diffusion through membranes? In other words, is the ratio constant over the surface of the earth between organic and inorganic matter? I found that in ocean water - I got samples of that and went down to 6000 feet it was essentially the same except for a little sediment that was in the ocean water. As far as the pure water was concerned, there was no isotope effect. I later showed to five significant figures that the ratio was constant. Then I studied rocks and so forth and found there were varia in the rock when there had been extraction effects, or like in some volc ash there had been thermionic effects. Then I went to nature, and I fo out that certain types of cells will concentrate the light isotopes, certain types concentrate the heavy, but most of them do nothing. Well the types that concentrate the light isotope turned out to be young embryonic tissue - you know, little shoots that had just started growin and cancer tissue. And what concentrated the old is when the tissues a dead; there's no longer really a membrane effect any more. I didn't h a chance at that time to study it, but I soon got into the isotope separation work, and then when the war came along in 1939, I moved to t Bureau of Standards and set up the isotope laboratory there, and later built the first big (and by all odds the best) Mass-Spectrometry labora in the East by about 1941.

DRP: Who did you collaborate with? Who was helping you at that time?

AKB: Who were the people that were working for me? Well, there was a boy n Paul Block; Lee Pelham; later a fellow named Dibler (I think he's still the Bureau of Standards at their laboratory); and then we had quite a f girls. We had one girl named Dorothy Thompson who turned out to be a wonder in computing mass spectrographic patterns; she really was a wond at that. She could make a more difficult computation than anybody else
in existence.

Well, our problem at the beginning of the war was to work out new methods of isotope separation, and you could laugh at this in a way— I immediately thought of the cancer cell. It had a means of concentrating the light isotopes, and also there were some things that could concentrate the heavy. How did they do it? Well, we made a study of all different kinds of things, but one of the first ones we did was to set up diaphragms like this and get the potassium diffusing through it under a potential. Well, I found out that it worked and I patented the thing. Of course, I didn't tell the Patent Office that the one-celled organisms had beaten me to it by three billion years; I simply applied what I thought took place in the living cell, and I got recognition for it as discovering the only new method. I say the joke of it was that it was the only new method of isotope separation, and the one-celled organism had been doing it for three billion years!

Now if they hadn't stopped our work I think we could have made it work much better, but we found that we'd studied every method we could to separate them, and we found out what concentrates the heavy isotope—it is due to selective absorption on ion exchanges. Then we could concentrate the K4 but by diffusion through on a potential gradient we concentrated the 235 and the one-celled organism like a cancer cell could give us essentially the same effect that we would get in the one-celled membrane in the laboratory. In other words, the plants never have a counter-current effect because they just simply give you a one-celled effect, which is what you would because it's only one simple membrane.

DRP: How strong was the separation in fact?
AKB: Well, it's essentially the square root of the masses - a little less than that. It varied a little bit from cell to cell of course when you tried but then we got essentially that in our cell. Of course, in the laborat we put a whole bunch of cells in the counter-current reflux, so there we built up high separations. But our first attempt was with a rather thic membrane and we got a ratio of I think 15:3; anyhow, it was way beyond the experimental limit of detection, so I was tickled when we got that because everyone in the country was hunting for a new method of isotope separation.

DRP: Did you envisage it at the time as being able to produce weighable quant

AKB: Well, that's what we hoped for. That's when I called up Urey as soon as we got this effect, and he came tearing down as fast as he could, and he was tickled pink. But the thing we didn't know at that time, and it took us some time to find out, was that it's not the mass of the ion per but the mass of the hydrated ion, and when we tried this on lithium we g very little separation, for the simple reason that the lithium ion carri with it 60 molecules of water. Potassium only carries 7. But when we g to uranium, which carries a couple of hundred molecules of water, it did work very well; it was a very small separation. But we never tried our next idea. We were going to when they cut off our appropriations beca they got the other method working - gaseous diffusion. I think this was a mistake; they still should have kept on looking for new methods of isotope separation, but they didn't want to, which was a very narrow-min approach, because we were spending very little money. The thing to have done would have been to apply high frequency to this ion migration; had we done that we could have jerked the ion right out from this entraining water, so I'm sure it would have worked. And of course the membrane cel actually does that, because you get into potential gradients across a
membrane of \(10^5\) to \(10^7\) volts per centimetre - terrific gradients across them. Of course, the difference in potential is only a tenth of a volt but when you figure they are only 70Ås apart, why, a tenth of a volt o 70Ås gives you a terrific gradient. So I am sure we could have come u with a very low-cost method had we superimposed high frequency with ver high potential gradient to separate the water from the ion.

Well, at the Bureau of Standards we bought a CEC Mass-Spectrometer, and then we got a Mass-Spectrometer made by the University of Minnesota aló Nier's general design, for uranium, so we had three Mass-Spectrometers running there. We had excellent computers. The Rubber Reserve was bei set up then, because you see our supply of natural rubber had been shut off and we were studying synthetic rubbers, so the Rubber Reserve wante us to make the measurements for them. There was no other Mass-Spectrom laboratory at that time that could make the difficult calculations, and we could. So that's why we really bought the Consolidated instrument, we made a big study of the cracking patterns of organic materials and t analysis of all kinds of plastics and fibrous material, which turned ou very fortunate.

We did a lot of things there with this machine; I'll just give you one amusing little anecdote. We got a sample of a German raincoat which wa picked up and it was a beautiful raincoat: a nice, soft, flexible mater that didn't get hard in cold weather. Well, the question was, what was made of? The Chemistry Department couldn't analyse it so they sent the raincoat to us. Well, how are'you going to put a raincoat into a Mass Spectrometer? You can't do it very well! So I designed first a small molecular still about the size of this little finger, with a tube comin off at one side into which we put, say, about two- or three-thousandths
of a gram, a speck of this raincoat just big enough that you could see.
Well, we put the material in the little side arm coming off like this
which had an electric coil around it so we could heat it, and we sealed
off. It had a tube so we could stick it in the Mass-Spectrometer. We
sealed this off, then heated it, and then put it in the Mass-Spectromet
and measured the gaseous products. Well, when we put this sample of
raincoat into the Mass-Spectrometer, we got a pattern we had never seen
before. It came out as a nice series of peaks. What was it? Well, I
remembered that when I was a boy we had an old German friend; of course
I was English with a little Scottish so I didn't know any German, but I
liked to talk to these old Germans about the old country, and this old
boy used to tell me about how popular linseed oil was in Germany, and
how they used it for everything, and how he even liked to dip his bread
in it. So I said to the girl that was trying to analyse the pattern,
"I'll bet you a nickel that's linseed oil with something in it to keep
from getting hard." So we got a sample of linseed oil and tested it,
she could pick the linseed oil pattern right out of it, and then we were
able to find that the other ingredients were plasticisers, so it turned
out very simple. But you see until you have the pattern on a Mass-
Spectrometer you can't do those things. When we were starting in busine
we had to work up all our own patterns, because there weren't any patter
available to us. So we analysed that and were quite successful.

We got into another problem with the Mass-Spectrometer which is a big on
We had every car-load of butadiene that was shipped to the plants sample
to see how good it was. They sent these samples to us; we analysed one
of the samples and found it was very high in 1-3 butadiene, which is not
good one, so I reported this. Well, I got a letter back from the compan
they were just frantic and replied that it couldn't possibly be 1-3
butadiene. This got me to thinking, "My God, I bet you we've taken
our sample wrong." So this started a whole big project on how properly to take your sample. The Mass-Spectrometer analyses what you put in it but if you put the wrong thing in it you get the wrong answer. So we had a big project going on how to remove a sample properly. And when we did that properly, well, we found that the 1-3 butadiene was present to about a hundredth of a percent, but through our sampling process we let it diffuse out through a small orifice, which is why the 1-3 got concentrated. We found a similar effect with quite a few things, where mistakes in our original analyses were due to a poor sampling. We had got a very good boy named Howard Bond, and we put him in charge of the sample room and made a big study then on how to take a sample properly. If you don't get your sample right you're not going to get the right answers. Like a computer you get what you put into it.

DRP: Garbage in - garbage out. Do you think this type of mistake was repeated throughout other various Mass-Spectrometer labs? Do you think you were the first to realise the importance of this?

AKB: Well, I think we were. I don't think there's any question about it. A matter of fact, we had number 7 Mass-Spectrometer, but the others were only doing very crude work. Now the company itself - I don't know whether you're familiar with Herbert Hoover Junior's work with the Mass-Spectrometer or not, but Herbert Hoover Junior was a very brilliant fellow, like his father, and he had an oil prospecting company. They got the idea that they could detect oil buried deeply in the soil by taking a sample of the surface material, and if there were some hydrocarbon vapours in it, well that was indicative of oil being a couple of thousand feet down. But the question was, how are you going to analyse it and find out what kind of a hydrocarbon you get out of this sample of surface soil which you'd dig out just a few feet down? Well, there was no method, so Herbert Hoover Junior had read
a little of the work Nier had done at Minnesota. Nier hadn't really do
anything quantitative, he was just using tape type of electric discharg
to get a few crude measurements at that time, so Herbert Junior got
Harold Washburn and a couple of other boys and they designed a Mass-
Spectrometer which turned out to be a mechanical wonder. They really d'
a beautiful job on it. We used to laugh about Herbert; he was very de
and so he would take off his hearing aid and he could work in the labor
completely undisturbed, and he worked very hard on it. Well, they came
up with this instrument. I went out to see it. I got very well acquai
with Washburn and I went out to see the people in his laboratory, but t
had never solved the sampling problem. In fact they wanted to keep the
whole thing secret when we got this at the Bureau of Standards, but I t
them that we couldn't keep a secret, and the best advertisement for them
to publicise on the work. So we published the first paper; a fellow m
Dibler and I published the first paper on the Mass-Spectrometer.

DRP: What date would this have been?

AKB: About 1945; I've got a reprint here which I can give to you. But we'd
done a lot of work of course before that on it. We'd worked on all kin
of things, especially for the Rubber Reserve, because they were the one
who were so anxious to get the analyses and they had no method of getti
the analyses at all. It was kind of amusing though: when we were doin
this work, where would you suspect our biggest opposition came from?

DRP: When you were doing this work for the Rubber Reserve? I don't know.

AKB: The Chemistry Department. Oh, they objected something terrible. They
everything in their power to get us to stop. They said, "You can't
possibly used a machine based on momentum to do chemical analyses". It
was really amusing. Finally we proved to them beyond all about: we ha
them prepare samples and we ran them through and we could analyse them better than they could themselves, and then they finally broke down and wanted a Mass-Spectrometer for themselves. But it was quite amusing; it was a really bitter fight. They went to everybody that could possibly have any influence on our work and tried to get our work stopped.

DRP: You think it was because they were jealous of their preserve?

AKB: Well, yes, they were jealous. They had admitted they couldn't do it themselves. They were just jealous of their preserves, that's it, and they couldn't believe that another discipline could come in and function. But people are that way today. Biologists are the worst, I think. I have nothing but trouble with biologists, because they hate like sin to see a physicist come along and solve their problems for them. They are the worst. I think they are worse in this country than they are in England.

I have been reading Sir Alexander Hadow - I don't know whether you are familiar with him or not - he's Britain's foremost authority on cancer. He is one smart man. But he is interested in checking all the different disciplines, not just confining himself to one; but the average biologist hates like sin to admit a physicist can do anything.

But the trouble we had with these chemists. Of course, we happened to be as the Head of the Chemistry Department a belligerent fellow who wasn't too good, and he was afraid that he was just going to lose his job. The trouble he caused us was certainly amusing - though it wasn't amusing at the time. His name was Martin Shepherd. He was the one that led the opposition to us, although it permeated pretty much all around the Bureau of Standards. They just couldn't admit that a new group like this could come in and do all the things that we were claiming to have done. Even Fred Rosini - well, Rosini broke down first and admitted that we were doing it all right - we had a little trouble with him too, but not much. His
trouble was minor compared with the others, and he was smart enough to realise that here was a new technique.

So we got it going and we did a lot of interesting things. For instance one of the things which interests me more than anything else is cancer. We made a study of tobacco smoke and we found out that when you take an organic material (now it doesn't make a particle of difference really whether you've got tobacco or not - tobacco's a little worse than some the others but it doesn't make a real difference) and subject it to pyrolysis - that is, destruction by heat which doesn't actually burn in a flame - then you will get (even if you start off with something like polyethylene where it is all straight chains) in your gaseous product off all kinds of cyclic compounds. And in the destructive distillation tobacco you wind up with a lot of cyclics, and it is these cyclics of course that are the carcinogens. So there is no question but that tobacco smoke is carcinogenic, and of course now our Cancer Institute has published many papers on it. They've done it in a different way: they haven't shown why it is carcinogenic, but that there is a good basis for it. We found it using our molecular still I told you about. We put in just a of tobacco of all kinds of organic materials and subjected them to pyrolysis. Now if you burn anything completely, why then you don't get these cyclics and that explains a lot of things. Recently the Standard Oil of New Jersey has found out that if you have a knocking engine, where you don't have complete combustion, then you get the cyclics showing up, but when you have an engine that runs normal, where the flame is all burning, why you don't get the cyclics. But I think we were the first to show that you actually get these cyclics in the destructive distillation due to pyrolysis, which I thought was rather interesting.

**DRP:** Did you have any contact with the Cavendish Laboratory in Cambridge at a
AKB: Well, I used to know Rydeel there but, no, I have never actually been o
to Cambridge. I think Rydeel is long since dead. I used to correspond
with Wainsborough Jones. I don't know what became of him; I think he
went into politics. So I really haven't any contacts there now.

When they cut off our funds at the Bureau of Standards for isotope
separation, then if I had stayed on there my work would probably have b
nothing but doing routine chemical analyses for people, because we had
research funds. Well, I wasn't interested in doing that; I was intere
in doing something original. I had a very good offer to go to the Navy
which I took, and I've never regretted it.

The Navy has a lot of advantages. It has the best patent policy: in
other words, when you take out a patent in the Navy, you retain all
commercial rights - you just assign the Government free use of it - so
I've taken out a lot of patents in the Navy. All the patents I took ou
at the Bureau of Standards, you just sign them away and the AEC takes
them and they're dead. They just lock them up and no-one ever uses the
We've got a project going on there now - I'm working with some other pe
in which we are trying to correct this, because actually the AEC has be
the best depository of ideas of any place in the country. They just go
there and they are locked up in the safe. But I think we can begin to
break them out. I think Nixon is a very bright man who wants to exploit
new ideas rather than lock them up in safes, so my feeling is now that
are going to be able to get a Government patent policy which will permit
the exploitation and development of new ideas, rather than just putting
them in safe keeping. The Navy was the first to do that, so I took ou
a lot of patents with the Navy, but since I retired from the Navy I've
been spending my time on pieces of research.
I've got a paper now which I've just finished on the mechanism of the cell membrane - how does the cell membrane work? You read about it in the literature, and there's nothing but absurdities on it. Most of the wind up by talking about processes that are completely mysterious. Well it's not completely mysterious at all. It doesn't do a darned thing that our membranes didn't do which we were studying in the laboratory and developed, and it actually combines the two. The cell membrane, when you look at it correctly, is an ion-exchanger because it's got all these double bonds, especially P double bond O, where the O is very electronegative compared to the P, and so it carries a high negative field about it, and that's a perfect ion-exchanger. And so this will permit the cell membrane to pick up potassium preferentially over sodium, which gives the sodium-potassium pump. But when you excite the membrane, then its ability to pick up becomes so large that both the sodium and the potassium will move through the membrane. Well, nobody can explain that. But the interesting part of it is: the rate at which they move through takes place with the excitation period, so it's responsible for action potential nerve action. For instance, I do that; well, there's a nerve impulse which takes place in a little less than $10^{-4}$ of a second. Well, this would explain it. So actually all I was doing when I worked out this paper on the cell membrane was to apply the knowledge that I gained on Mass-Spectrometer.

However, I lost touch with all my Mass-Spectrometry contacts. I sometimes go over to the Mass-Spectrometer laboratory Naval research and look up cracking patterns now and then for the fun of it, but I haven't carried on any contact with the people that are actually using them. I think the last two people where I carried on any contact with those working on Mass-Spectrometers were Professor Mike Higgitsberger at the University of Vienna and a chap in Stockholm. They had a Mass-Spectrometer there, no a very good one, but Mike had a pretty good one in Vienna. But I haven
paid much attention to the work in this country, because I've gotten al
interested in the cell membrane, and that is so important to me because
I think it will give us the first step in resolving cancer. And it will
also help us in the understanding of the ageing process. So I'm spendi
all my time now on applying my knowledge of physics to the cell and the
biological processes.

I had of course studied fluorescence and phosphorescence quite a bit,
again that's applying the knowledge I accrued at the old Fixed Nitrogen
Laboratory the same time I was doing this Mass-Spectrographic work. I
was always interested in medicine and its biological applications so t
'help' out there used to come around to me to get cheap medical advice.
Well, they had an epidemic of skin fungus in the laboratory, so I studi
not only the fungus on the Mass-Spectrometer, but I also studied it in
my photoelectric equipment. I had some good equipment, so I studied it
there and I found it would both fluoresce and phosphoresce, so later wh
I was in the Navy I came up with methods which would detect bacteria in
the air. I used to go down to visit Porton and talk to the boys there, and
now I have a method which is based on this work which will detect a
bacteria cloud ten miles distant in the air. This will tell you while
are sat in this room right here how many bacteria, how many dust partic
how many virus there are per litre of air. Also, I've got a method whi
will give you a very rapid analysis on phosphorescence decay patterns.
it was that sort of thing I got to working on when I was in the Navy,
because there was a real demand for it. Of course, I actually spent mos
of my time on atomic problems, but then we could go so far on that and
reach a dead end as far as advancement was concerned, and in my section
the Navy I had all phases of science under me, so I had begun to take a
personal interest in the biological problems. That's why I was interest
in going down to Porton and seeing what the boys were doing there and
conferring with them. We had a very good exchange of ideas.

So, since my retirement I'm not playing around with atomic energy any more. All I'm doing there is working with this group to try to break some of the new ideas out of the Patent Office. For instance, I have a patent application on a super-centrifuge which I think will step up the efficiency of the super-centrifuge about a hundredfold. But what did they do? They just put 'secret' on it and filed it; they never even tested it. That's that sort of thing we're trying to correct here in the Government. But I suppose all Governments do that. It's only natural to protect what you have - just like the boy at the Chemistry Department in the Bureau Standards. He was worried about his prerogatives and he wanted to protect them, so he objected to the Mass-Spectrometer. That's nature. Science advances in steps: you get some new ideas and a whole lot of new developments, then it will level off, and everyone wants to protect his position. Same thing in politics.

DRP: Do you think this is true in theory as well as in the practical technical advances? This stepwise process that we're engaged in, filling in a paradigm if you like, between revolutionary periods?

AKB: Yes, it's true in theory too. You'll find that if you want to publish a paper which runs contrary to the prevailing trend, you find you have a lot of trouble. I think the big need in the world is a more open mind towards new ideas. Of course, 'Nature' used to be like this, but even 'Nature' is now getting conservative. I'm sorry to see that, but as with most of our magazines, if you don't support work that's already been published you'll have difficulty in publishing it. And what we need more than anything else is new ideas because it's new ideas that are the breakthrough which will give us the advancements; this is more true I think in biology than it is in any other subject. I think it's probably the least true
physics, because most physicists are sort of looking for a new idea. In the 'Physical Review' you don't have trouble getting new ideas published but you go to any biological magazine and you really have trouble. I'd think, though, that men like Sir Alexander Hadow in London are trying to get around that. Hadow has a theory on laboratories: the thing that worries him more than anything else is somebody wanting to protect his idea. I'm very glad to see Sir Alexander taking this attitude, because he's a real scientist of course.
DRP: By the way, I was down in Washington talking with Kieth Brewer. Did you ever come across him?

JH: He took charge of the Mass-Spectrometry section at the National Bureau of Standards; he was chief of that. I was head of a section. I went down there with Ed Condon, and so I was chief of the atomic physics section in the National Bureau of Standards, but Fred Mohler and Bernard Bagler - Bagler is at the National Bureau of Standards now, he's still very active in Mass-Spectrometry. Neither of them had been so active before. Mohler was a well-known person - he worked on the discharge of gases - and they had been at the National Bureau of Standards for years. Well, they published the studies of metastable ions and the dissociation by electron impact in hydrocarbons. They had many papers; they made catalogues of them. So this is what happened in Brewer's section after he left.

DRP: Were you a physicist or a chemist when you started?

JH: A physicist.

DRP: And then when did you start doing your Mass-Spectrometry? Did you take your PhD in that?

JH: Yes. I went to Princeton University Graduate School, and then I was I think - I have very hazy views of this - but I'm quite sure that I was the first graduate student under Walker Bleakney at Princeton. But Walk Bleakney came to Princeton from the University of Minnesota; they had been doing work on ionisation by electron impact, one of the few places - one of the pioneer places that did this. Professor Tate was the Head of Department and was very interested in this general field at the time. And Walker Bleakney was a graduate student - he had already got his PhD.
DRP: Did you have much contact with the Cavendish Laboratory?

JH: Not very much, because, well, there wasn't really so much activity there. I think it could be that Aston was such a large figure, and I didn't know him personally but I've heard that he tended to be a person who had to work very much alone.

DRP: He was a bit of a recluse, certainly.

JH: Yes, and so I think this must have had something to do with it. I got to know Mattauch very well after the war, and he told me some stories about how they got into Mass-Spectroscopy in Vienna. And the great deterrent was for them to get into this field, because of the stature of Aston. This just didn't seem like a good thing to do. All this was done at Cambridge.

DRP: Mass-Spectrometry was a British subject.

JH: Yes. However - well, Mattauch got into it because he had a graduate student R. F. K. Hertzog, who is now up in the Boston area. Well, he asked Hertzog to do as a thesis a theoretical study of focussing in Mass-Spectrographs. They didn't have money really to do any experimental work at that time right after the war. Well anyway, Hertzog came up with a beautiful general theory of focussing. And it was not so special as the work that was done actually someone at Cambridge worked with Aston on some of the theory, but it was for the focussing condition in a specialised case on a particular machine that Fowler was concerned with. It was really very nice design work of a theoretical nature for that machine; an extension of what he had done before.

Well Mattauch was very much concerned then what to do with this. Here he had what he judged in his very capable way - a charming person, as I learnt
later when I got to know him - what to do with this. Well, I think you would probably do the same and I would do the same in the circumstances. He sent it to Aston. Well, nothing happened and nothing happened. So finally he wrote, you know, that there was really no analysis, no evaluation of the importance of this, how it could be used. Actually, Mattanch knew how you would use it if you were going to make a machine, but they didn't have at that time the idea of doing it because they just weren't in that field, and they couldn't really afford that. But eventually they did then decide that they'd better set up something and at least try and see how in practice the theory does apply - that's how they got started. I think there is some question whether Aston really ever studied it.

DRP: Or if he studied it, understood it, do you think? Because he was very much an experimentalist; he didn't know very much about maths really.

JH: Aston - I never knew him but he certainly was a pioneer. He made tremendous contributions. They were so outstanding for the times, that there just weren't any other people in it. Of course, Bainbridge came in and he did quite well. But it was dicey, it took a decision then to go into that field. There were other fields to go in that didn't have the deterrent of the prestige of Aston and what he'd done.

DRP: So he acted as a deterrent then, you think? What about Dempster?

JH: Well, Dempster started somewhat the same time, but they were in two somewhat different areas of interest. There were others too, but I would agree with that. You mention Dempster; but Dempster never got into the type of activity that Aston was in until the latter part of the 1930s - almost 1940. I think it's curious, you know. I think such a situation just doesn't happen today. There are so many people in science, and resources available, that any activity that looks the least bit promising, why then many people go in.
At that situation there were fields to go in that didn't seem to be so well in hand as Mass-Spectroscopy at that time. Although the people who were interested in the problems of electrical discharge and gases, as they were for instance at the University of Minnesota, then began to want to know more about the ions that were involved.

DRP: Did you know Walker Bleakney when he was at Minnesota before he came to Princeton?

JH: No.

DRP: Were you working with him during the discovery of deuterium?

JH: Yes, I knew him, but I was not involved in that particular work; but let me see, that was 1932 ...

DRP: Yes, you published in February 1932.

JH: Yes, Urey, Brickwedde and Murphy published in January. I went to Princeton in the fall of 1933 and Walker Bleakney came to Princeton roughly around 1930, I think, as a National Research Fellow, so he had a grant to do graduate research - a grant from the Government; well, from the National Research Council, essentially the Government. So he went to Princeton because K. T. Compton was there. He was a very great man in experimental work, and he was keenly interested in the study of gases - gas discharge phenomena. So Bleakney went there, I think undoubtedly because K. T. Compton was there, and I think K. T. Compton left in about 1932 - I think it was the year before I got there - to become President of MIT. But Bleakney was then with this background - he many have been there a little before 1930, but it was roughly that - and then he was there as a National Research Fellow. And I guess about the following year or so, other people from
Minnesota - other graduate students - came as National Research Fellows
to work along the lines that Bleakney worked.

DRP: People like Wallace Lozier?

JH: Wallace Lozier, P.T. Smith - there's a paper I remember with Smith, Lozier
and Smith and a few others - I don't know, it's sort of like a choo-choo
train!

DRP: They made sure everybody was on that one.

JH: They certainly did. Well, you see, Bleakney had become an instructor.
They may have overlapped as Research Fellows, but I think he became an
instructor in the Physics Department. That didn't mean any particular
field, but naturally he was concerned with the Mass-Spectrometer. I didn'
really get into the laboratory until about 1934; I was busy in my first
year at graduate school, other than being in the laboratory. So by that
time there were two Mass-Spectrometers of that type built.

DRP: They were both built there under Bleakney's supervision at Princeton?

JH: Yes.

DRP: There weren't any when he arrived?

JH: Just H. D. Smyth's machine.

DRP: He'd learned his Mass-Spectrometry from Aston himself hadn't he? As a
National Research Fellow at the Cavendish.

JH: Yes, so he had at that. I don't know what his rank was then, but he was
a Professor at some level. When Bleakney came as a National Research Fellow - I should have mentioned H. D. Smyth too, but it was the combina-
that made this an attractive place, and also a place that would be interes-
in this type of activity, with K. T. Compton and H. D. Smyth. So Bleakney
then was an instructor. P. T. Smith I don't remember. He must have left
before I arrived, because then there was another Smith coming in, Lincoln
G. Smith. (We were on the telephone last night.) Of course he and I
were contemporaries; we both came in the fall of 1933 and started as
graduate students at the same time.

But I was thinking, going back further, about the work on the isotopes of
hydrogen. Bleakney built a solenoid with, I don't know, a 4-or 5-inch
opening that you could slide this glass tube in, with all the electrodes -
beautiful tubing the solenoid, about 4 feet or more long. And all the
electrical leads and the vacuum leads had to come out of the end of that.
The idea was that he built what I call a Dempster-type instrument - 180°
electrical detection in that solenoid. I think that Bleakney made a great
contribution in the type of ion source that was used, and I've always used
a Bleakney-type ion source because heretofore there'd been no good contro-
of the electron beam. Electron beaming had fouled up in the magnetic
field, and so they would accelerate it with rather high energy, and they
would hit electrodes, and you'd get secondary electrons. It was very
difficult to get a nice clean experiment as far as the impact of an electron
on a molecule or atom.

Well, at the University of Minnesota Bleakney did his work on the mercury
ions - his first work on the multiply-charged ions in mercury - and in that
he used a long solenoid and had the filament in a uniform magnetic field.
The electrons then were held in a tight path by the electric field and
went through the electron structure and that didn't hit anything. And it
stayed well under control, and you then proceeded to accelerate
perpendicularly in the direction of the field - perpendicular to the magnetic field. Well, this really was the basis for nice clean experiment on dissociation later by electron impact. So there was that equipment that Bleakney had, and that was available then. It was beautiful for the work that Urey wanted with the measurement of deuterium, nice low pressure, so that the secondary reactions of say an ion with the neutral hydrogen molecule to form H3+ by a secondary reaction were eliminated which would conflict with the HD at mass 3 and deuterium molecule of mass 3 which they did use then for the confirmation of the deuterium, the enrichment.

So that instrument was there, but then H. D. Smyth was interested also in trying to see the products of the deuterium-deuterium reaction. There they wanted to find helium 3, so that was one of the reasons that they built another instrument of this type in another laboratory across the hall, down in the corner of the building. And these were very good experimental people - Lozier, P. T. Smith - so they built another one with a much longer slit. They had to decide beforehand what size the slits were going to be because it was buried inside this glassware sealed in the glass with beautiful tubes with all the taps and the leads coming out the end, the pumping leads coming out the other end, and all inside a magnet that was practically buried in the ground as far as accessibility was concerned. Well, so they built this for that type of experiment. I think that was almost certainly one of his incentives, and also they did some additional work - I think possibly on that instrument - on the multiply-charged ion in mercury - I'm not too clear on that.

So Lozier was there I guess for about a year. P. T. Smith left in 1934 and I think he went to RCA. Lozier was there I think for the first year I got in the laboratory, if it was 1934. I simply stopped in to see Bleakney one day - I think it was his first year as an instructor - and asked to work with him in the laboratory. Linc Smith was at that time
working for Smyth, and that was the laboratory right next door. There was a Mass-Spectrometer there with a PYE magnet from Cambridge. Well, Bleakney did these samples for Urey ...

DRP: Can I get this straight? Urey - had he already discovered deuterium before Bleakney had decided that he was going to investigate hydrogen? Was he in fact confirming Urey's discovery, or were they working more or less simultaneously, but with Urey publishing first? Because there's only a month's difference between Urey's publication - Urey's first letter that he'd discovered it - and the next month Bleakney's. If this was the case then he must have designed, carried out all the experiments, and written them up in a month.

JH: Well, see, I was not closely involved with Bleakney then. As a matter of fact he was an instructor. I don't think he had any graduate courses then, so I knew him in a casual way, as a fledgling graduate student knows someone on the faculty who is not one of his lecturers. My understanding of it is that Bleakney had the best equipment, I would say, in the world there at Princeton to analyse the samples that Urey was enriching at Columbia, or also Brickwedde with the triple point method. So I think that in a way it was a convenient analytical laboratory for the work that Urey was doing.

DRP: Urey was using these samples and analysing them using ordinary spectrograph methods. I'm only trying to see behind the facts. I'm well acquainted on reading the bald facts in the literature that this happened and that happened, etcetera. I'm studying it a bit more closely, and just the business concerning deuterium - how close these two experiments took place. Urey got the Nobel Prize and Bleakney didn't. But if Bleakney was working on the results of Urey - there just didn't seem to be enough time for him to have been working subsequent to Urey so maybe they had to be working
JH: I just wonder if he sat there with a Nobel Prize and he didn't realise the significance of what he'd discovered?

JH: I should have studied last night for this examination today! - I'm joking.

DRP: It's not exactly meant to be an examination.

JH: My recollection is that Bleakney at that time wasn't working on the separation of isotopes. Your point is whether he was able to discover this because he had such sensitive equipment that he could discover it without enrichment of the deuterium in hydrogen - did he in fact discover deuterium in the natural abundance. Urey was working on the electrolytic concentration of the hydrogen isotopes - that was at Columbia. And he also was then in association with Brickwedde who was at the National Bureau of Standards and had the low temperature equipment to do the evaporation of the triple-point for the concentration of the hydrogen isotope. So that was a combination that was really seeking to find the heavy hydrogen. And then of course Bleakney could have - with the equipment that he had - had the possibility of looking for it without enrichment, and I think this is the right viewpoint of what happened there. Now I - having come along just a couple of years later to work in the laboratory, it isn't quite clear to me just what Bleakney was doing at that time. Certainly there was this interesting way that he had developed then of using the triatomic ion, which was in a secondary action. It required the formation of the atomic hydrogen ion by electron impact in his apparatus, and then the reaction of that ion with a neutral molecule to form the HH++ ion. This sort of work was done on that second Mass-Spectrometer, predominantly because that was built for much higher sensitivity than the first one he built. It was built with wide slits and a long slit. So they varied the pressure of the hydrogen that was put into this Mass-Spectrometer tube and used the height of the molecular ion - of the H++ ion - as a measure of the relative pressure in
the Mass-Spectrometer tube. So then he could plot the height of the peak that they got on the triatomic ion - the HHH+ - as a function of the pressure using H++. 

DRP: So it was a rather elegant way of sorting out different ion/molecule reactions?

JH: Yes. And if they could then plot that and distinguish between the HD+ ion which would also be a primary reaction, by plotting it as a function of pressure, you could also separate it from the triatomic ion which was formed by secondary reaction. And by this means of course there was a possibility of very high sensitivity to look for the deuterium. And I suppose that they must have done that then and that perhaps was - probably was, now that we're talking about it - the main reason for building that second instrument which was in being when I went into the laboratory; both instruments were there, in two separate laboratory rooms. So I guess that's it; but my most vivid recollection is of the use of the instrument for Urey's work. Later, and that's why I got confused probably on this, because then - well, it was close to New York and here Urey was working on electrolytic concentration of the isotopes. And he hadn't any means at all comparable with what we had there in Princeton to analyse what he was doing and to indicate what directions he should go to prove his method. It was essential that he knew what he was doing, so one of the first things I did as a graduate student there was to measure those samples that came down from Urey's laboratory.

DRP: Did you build any machines while you were there, apart from repairing them and everything?

JH: Oh, I built a fantastic one. I built a bigger one - you asked how many I ever built - I had an awful time getting pieces.
DRP: It was a difficult business.

JH: Like most things. I was one graduate student. Bleakney, when he became an instructor and Assistant Professor, he learned a lot I think. Well, I had the impression that the beautiful features of this Bleakney ion source were just not realised until I built a few more instruments that just didn't work.

DRP: You built instruments that didn't work?

JH: Oh, terrible.

DRP: How did that happen? Was it just sheer accident; did you design them wrongly, what happened? What were the difficulties involved?

JH: Well, Bleakney was a superb experimental physicist ...

DRP: Did you have to do the glassblowing for it?

JH: Well, I guess I did a lot of glassblowing, but it was more of a utilitarian type of glassblowing. I got to be a very good friend of the glassblower at Princeton!

DRP: I suppose one had to be.

JH: As a matter of fact, when they had the dedication of the new physics laboratory at Princeton a few years ago, I saw this glassblower, Lee Harris, and he took me downstairs to see some glass they had on display in their new building. They had the version of the cross field Mass-Spectrometer on which I got my thesis, and he and I had one thing in common in that display, and it was very interesting to talk to him about it again at that
time, because that was built in the first solenoid that Bleakney built
and it had so many leads in it. I have a picture of it somewhere.

DRP: You have a picture of it?

JH: I have my thesis on it. I started last night to look for some things and
I have some of them, but I don't have that copy in my thesis that I
submitted, but there's a picture in there of that thing that's in the
display. I think it was when, some years afterwards, they moved things
around and they were going to throw that out that Shenstone, who was
(he's still working, he's retired) a professor in physics at Princeton -
A. G. Shenstone - he told me that Bleakney was clearing these things up.
And he took the Mass-Spectrometer out of there some years later and he
insisted that they save that particular structure with the tantalum and
all these plates of glass. It had adjustable slits, and leads coming out
of both ends of the coil, etc. So it was Bleakney then who had it on a
shelf above his desk some years later - many years later - so it must have
been 25 years. Then when they had sort of a museum, they put it in that.

At the National Bureau of Standards there's a display they've got on this
subject. They had this international meeting on precision measurements of
constants - I think it was three years ago in the summer - in August - in
Washington - well, Petersburg. There were some people I knew down there
who have a display, and they took me around to see the megatron that is in
one of these cases that they have in the lobby at the National Bureau of
Standards. Actually it isn't the one with which we did the work - it
looks a little nicer than the one we did the work with.

To return to Mass-Spectrometry and the machines: ours wasn't too bad, but
it was all glass.
DRP: What sort of pumps did you use?

JH: You mean at Princeton? Mercury - mercury and liquid air. As far as I know, the first MS-amplifier was a project that I had in the first year I worked with Bleakney. Let's see, Dubridge's article 'New Scientific Instruments' by Dubridge and Brown - Dubridge was at Washington University in St. Louis then. So Bleakney wanted to have a circuit like that with this particular special tube - an FP 54. Then later Bell Labs came out with D96475 - I think it was something like that. Before that, all the work and the work that I did initially was with a Compton-type electrometer. I don't know whether Bleakney built that, or it was at Princeton. The initial work of Bleakney's with that first instrument was all done with an electrometer. People don't realise how much easier it is with some of these things nowadays. Well, actually years ago it was much easier. In fact, it was with the advent of the consumer radios that the various tubes really became available, because we mostly used commercial products in our circuits. Dempster started just the time I started to work in the laborat

DRP: So how long were you working as a Mass-Spectrometrist? What else did you do?

JH: Well, I stayed until 1938 at Princeton. I completed my work for a doctora in 1937 and I stayed till the following spring. Condon left Princeton the preceding year in 1937 and became Associate Director of Research at Westinghouse Research Laboratory, East Pittsburgh. He talked to me then when he went out there about coming out, and he also said that he tried to get Walker Bleakney to come out. Well, this was an interesting situation for Bleakney because he had been an instructor for a while; I guess he must have been about an Assistant Professor by then. So then Tate about the same time offered him the position as Head of the Physics Department at the University of Minnesota. Tate was the Dean of the School in the
University which the Physics Department was associated with. So Bleakney had these two nice offers, and of course Princeton reacted and he made the decision to stay in Princeton.

Then the following year Condon persuaded the people at Westinghouse to overcome the bad image that they had among scientists, because during the Depression they knocked out so many technical boys. The idea was that they should start some Fellowships, something like the National Research Fellowships, only they'd pay more. Anyway this was fine; there were five appointed in the first year with the possibility of renewal for another year, and five more the following year, so that if they were all renewed - it was only a possibility of renewal - they would have ten new PhDs working in the laboratory, which was quite a departure for Westinghouse. They would also of course supply funds for work that wasn't programmed for any big product - they were just in an area of potential interest, but they were scientists.

DRP: That was a very enlightened policy, wasn't it?

JH: If you knew the background of Westinghouse ... But they had Joe Sleppian - Dr. Sleppian, the outstanding technical person. He had his PhD in mathematics from Harvard and joined Westinghouse in - I don't know - maybe 1916. He made many inventions for Westinghouse, extremely valuable ones: the ion circuit-breaker; the electron tube; some that were before their time; the cross-field multiplier, the cross electric-magnetic field multiplier. He had a patent on that in the 1920s. He had a patent on the idea for a betatron, although it was too early. Sleppian was undoubtedly the person who initiated the idea of getting someone like Condon there. And before Condon went there, there was kind of an agreement that he was going to have considerable support, so he started his Fellowship.
I can say that my two-year Fellowship got to be a little longer, because I wasn't on a fellowship programme, but I spent some time - part-time - on that work and then did some part-time work for Joe Sleppian. They had a project called a K and H generator. That was to get - I guess the nearest thing would be magneto-hydrodynamics now, but it was so early to do this - and this got a lot of money in it. They brought this chap over from Hungary - Carlovitz - and I worked a little bit with him. It was a big project. I had a blast of gas with a big pump at the other end - a big engineering thing that we tried to ionise and then have the ions and the electrons separated by the magnetic field. The source of power - well, I was working for him on that. Then the war came along. Also Westinghouse had some interest in having an analytical Mass-Spectrometer. This got more and more important then during the war because of the synthetic rubber programme and the aviation gas programme. So then I got involved in that, and Condon was the driving force in organising a co-operative programme to apply the Mass-Spectrometer to the analysis of hydrocarbon gases. For that then we got five other corporations interested. We did not have an instrument that could analyse a mixture of hydrocarbons then. We'd been studying them and there were lots of publications, but at that time, no instrument. I have some of them here, like 'Ionisation-Association by Electron Impact of normal-butane, iso-butane and ethane' by Stevenson and myself, and this is 'normal propylchloride', 'tertiary butylchloride' and so on - but that doesn't mean that you can analyse a mixture of them quantitatively. As you well know, all we were interested in was the relative height of the peaks and the assurance that they were primary products of electron impact.

DRP: You had no system of patterns or anything then if you were analysing mixtur

JH: No. Condon had organised a project with Standard Oil of New Jersey - thre of the corporations were corporations of Standard Oil of New Jersey - and
John U. White came from their research group in Elizabeth, New Jersey, to Westinghouse Lab - they sent him. They sent George Walden, who had been a professor at Columbia, and of course many of the people at academic institutions were spread all over the country. He was down in Standard Oil of Louisiana, which is wholly owned by Standard Oil New Jersey. And a chap named Thomas came from Humble Oil Baytown, which is also Standard Oil New Jersey, and Fred Williams came from Hercules Corporation - as it is now, it was Hercules Powder Company then - and then Dave Stevenson after his two years at Westinghouse had gone to Shell Development in Emeryville, California. Well, they sent Dave Stevenson so suddenly - I had five people from these companies that came to apply the Mass-Spectrometer to analytical applications - Jesus, we'd never done anything like that, so it was a fantastic scramble, just terrible, because these people didn't come to work on any instruments, they came to use them. One couldn't relax in that situation at all. I'd been involved a bit because Westinghouse was interested in analytical instruments, so I had a chap named D. J. Grove who was working with me; he was an MA graduate student from MIT and his work was interrupted. There was a very capable chap called Jones; they were mostly chemists - practically all chemists.

They would come during the day time here; they had it pretty nice. They lived at the Pittsburgh Athletic Club and worked during the day. Well, we got a group together to work on the instruments. We didn't even have a suitable recorder available then. I had made a recorder, but you just couldn't buy a decent recorder, so I had made one for this. It looks pretty amateur, but I spent a lot of time trying to make one. So we used that for some of the work with Stevenson, but that was that; you couldn't build that to go to these chemical companies. The chemical companies and oil companies didn't want to go and build instruments, they were going to buy instruments from Westinghouse. We did get a Speedomax recorder from Leeds and Northrup. We then had to build an amplifier that was sensitive
enough. I couldn't get the P54 or the D96475; they were still limited to these for electrometer-type tubes. We finally got some from Bell Labs - special tubes - and as time went on we were able to use some of the Ridley tubes that came out; although they weren't big electrometer tubes, we found some that we were able to use.

Well, we had to do all that and we had to make mixtures of gases; we had a chemist who was a physical chemist, and he was in charge of that part of it. We had to make a very accurate mixture, a very precise mixture to calibrate the instruments. And we had found lots of problems - how difficult it is to really mix them; they really had to be churned up as we put them together because the instrument could tell if they weren't mix. That was quite a scramble. I still have here a report and things from Princeton. Westinghouse was interested in having me go there to work on Mass-Spectroscopy, because at Princeton when I was there we wrote the first paper on hydrocarbons. It was the first paper I ever gave anywhere, and that was on methane. We found that the spectrum of methane was different from what people had published, and that was because of the advantages of the instrument we had. We didn't really measure the primary processes. Now what's his name in Germany published the spectrum of, say, methane and decided it was $\text{CH}_4$ and $\text{CH}_3$ ions and the rest were just secondary products. So my first paper was one that I gave at the Washington meeting of the American Physical Society that showed by pressure studies - I could vary the pressure in the instrument and the peak heights stayed the same for methane - mass 12 and mass 16 - $\text{CH}_4$, $\text{CH}_3$, etc. The paper was published in 1935 - Hipple and Bleakney. As far as I know, that was the first one that clearly gave the methane spectrum as it is essentially now.

Then there was another interesting thing I did at that time. Condon was a wonderful person to have around for a physicist. Condon was a theoretical physicist and really got a lot of pleasure out of working with experimental
physicists and ideas, and so he had an idea which was studied later. I never did publish that one, but it was published in a review article by Linc Smith, and Condon wrote it and Bleakney too I think. It had to do with the study of the Franck-Condon principle, that in hydrogen you have a form of atomic ion as well as a molecular ion, and also similarly for deuterium - the electronic structure is about the same. And Condon had the idea that since deuterium is heavier than hydrogen the zero-point energy has a smaller range. If you think of a potential energy curve, the transition from the ground state to the electronic state - it's hard without going into the actual energy levels, but the ratio of the atomic deuterium ion to the molecular D ion in deuterium would be less than it would in hydrogen of mass 1 ion to the H2+, and he had this idea so I tried it! All I did was put it in Bleakney's instrument, and sure enough, that's what happened.

Well, later then, before I left Princeton, there was a chap from the University of Lonvin named Delfosse who came over on a C.R.V. Fellowship - that was the Committee for Relief, and it was a Hoover Commission after World War I, using resources that were in Europe to set up these Fellowships, and so he came over on one of those. This was in about 1936 or 1937, and he brought with him some hydrocarbon samples - deuterated hydrocarbons. So then we were able to study the corresponding type of thing - the effect of heavy hydrogen - on the dissociation patterns of these hydrocarbons, and make some inferences from that about the dissociation processes. So we published a paper then on $\text{C}_2\text{H}_2\text{D}_2$. So that was along the same lines, using the same general sort of idea, as this student. At the time Condon had this idea at lunch, you know, and in the afternoon I did it. And then it was mentioned in a review, this review article. Well this was the first time that the deuterated hydrocarbons were studied for these isotopic effects, and now there's a lot of work that's done in this field to get some insights on how these dissociation processes form.
Well, moving on from that digression - I jumped back. The hydrocarbon work was very closely related to what we did later. While I was at Princeton, I finished my final oral exam and I was just round there working in the laboratory until I went to Westinghouse. So there was more and more interest in making some hydrocarbon analyses because people wanted to do various things - looking for oil, you know, and measuring the gas in the ground - there was a lot of that. So it began to look very attractive, I think primarily on the basis of the work that Bleakney had initiated at Princeton. It wasn't so clear at the time that this was going to be the answer for making reproducible mass-spectra with regard to the pressure, because that wasn't the aim at the time, but it did mean that you could superimpose these different hydrocarbons and have a mixture and the peak heights would stay linear with pressure. So this was certainly one of the interests of Westinghouse, not that they were in the chemistry field, but they were looking for possible products. This didn't all work that way. The result was that I had all these people from chemical companies and oil companies there to use the instruments, and we really didn't have an instrument that was set up for that. It was true that there were instruments where you did have this linear response, but that's a long way from a calibrated instrument, an instrument that will maintain calibration for some time, making mixtures and doing it on a routine basis.

So after these people left, then they went and ordered instruments from Westinghouse, and Westinghouse just wasn't organised in any way at all to make a Mass-Spectrometer. The only place that they had any vacuum work in any of the plant was up at Bloomfield, New Jersey, where they made light bulbs. And up there they didn't do any work in electronics, and the only place that they had anything in electronics was in Baltimore, Maryland - that was the radio division. They made military radio equipment during the war. Well, that was really some circus. In the first place, we had a lot to learn, and we could run an instrument for some time and even after these
people left it would be shaken down. The people in Baltimore - that was the place that had responsibility, but they couldn't make the key part of it, which was the tube, so the people in Bloomfield, New Jersey, at the lamp works had to assign someone to be in charge of making the tube for Baltimore. But of course they were just the sub-contractors and the profits were in Baltimore. People in Bloomfield just couldn't lose anything on it, because after all they made it for whatever it cost them. They were accustomed to making things like thousands and thousands of light bulbs. So they could tool-up; they made production lines with beautiful jigs and fixtures to make Mass-Spectrometer tubes. But they'd never made any, so it turned out that they really didn't use them in the end; they had to make then just about the way we make them in the laboratory. But it was an educational exercise on all sides - on my side to know how you operate with industrial divisions. They have certain procedures, and in Baltimore, in the radio division, they're accustomed to making radios that delivered, and they made quite a few of them. You have to have drawings before you do anything, so they made a fantastic number of drawings. The people down there knew just very little about the instrument. They came up and looked at it, you know, diddled around it a little bit. Then they made all the drawings and they tended to do it independently of us at the lab; we were some distance away, about 250 miles or so. Once it goes from the drawing department into production and they start to make it, well, to change anything would be catastrophic. Well, it was great. Anyway, we finally did make the instruments, and then the people who were working for me were out in the field all the time getting these instruments - those who had been on the co-operative programme - getting them to work. It turns out that after the war Westinghouse took a look at this field. They made the decision that they wouldn't make them anymore, although we had gone through the exercise of claiming a new instrument. We had I think a very nice recorder which we devised and we learned a lot - new calibration systems, new tubes, and so on - many-sided improvements on the basis of a market stu
JH: They did a market study, yes. It was an interesting market study; they had this chap who made a study and he turned out to be a physicist. He was a theoretical physicist who decided after the war that he was going to go into industry, so he was in the economics group in Westinghouse. He found that the only companies that had a Mass-Spectrometer after the war - this was in 1946 - were companies that had at least 75 million dollars in sales a year. Furthermore, they weren't soft-drink manufacturers, they were some special category of oil company or chemical company. Well, how many companies in the United States were there like this? Figure that out, how many had a Mass-Spectrometer? Well, at that point there were quite some. I think there were something like 15 or so - Consolidated Engineering had I don't know how many - so you could project from that how many did have them; the market was getting to be saturated.

Soon after that, I didn't see any point in staying around Westinghouse, particularly since Ed Condon had gone down to the National Bureau of Standards and had asked me to go down; so I went down there. One thing we did at Westinghouse too was that we used one of these things that we built on a truck. Oh, it was a real strange animal - I bet I can't find a reference to it. We adapted the instrument - the first one they had on a truck to move around the plant to do things - you didn't move it easily, I tell you. We used it for another application. Westinghouse participated in the centrifuge project for separating isotopes, as well as the project at Oakridge: the calutron - the big Mass-Spectrograph. E. L. Lawrence was a prime mover in that method. Well, that came in operation first before the diffusion came in. So it looked like maybe that might be the way that they did it after the war at that stage. So I got involved in that, but before that on the project that was centred at Standard Oil, New Jersey - that was the centrifuge project, and Westinghouse was involved
in some of the drives for that. There was a little group in our laborator
at Westinghouse that was working on that part of it; they were mechanical
engineers. Well, if that went through, Westinghouse was going to build
the centrifuge. The problems of leaks in that were severe ones, but we
had this Mass-Spectrometer, and I know that at Princeton when I wanted to
look for a leak, why, it was pretty easy to do. Well, I mean you'd see ai
going in, so we used something else. We could use, well, even a hydrocarb
like methane, but we did use helium too. So I suggested they try helium.
We didn't move this equipment to their set-up, but they wanted a demonstra
so we made a demonstration and I wrote a classified report that went into
the atomic energy programme. So that's one other thing that came out of
that, although we were too busy to pursue that one at the time. But then
later we did make, when I was at Admore House later, some leak detectors.
We made a model of a leak detector with a gasket that you could open up
and quickly change the filament. It had a Phillips ion-guage type gas-
discharge and a filament as a source, and Westinghouse made those during
the war for Oakridge, particularly for the diffusion process. Well, I
think that's just about it, you've exhausted me!

DRP: Thank you.

END
DRP: Perhaps we could begin by discussing your early career?

ALV: I was interested in mechanical things and design. My real interest in physics developed from a very simple thing, and that was pulling posts - fence posts - out of old post-holes where they'd been set for a long time. You had to change the fence, and you had to get that post out of there. We had horses as our source of power, and my interest was in arranging some way or other that horses could be used to lift the post out. So just using a fulcrum like this, with the chain coming over the top down to the post here and the horses out here pulling, you could get quite a lift and a pull. That got me interested in how things worked and how you can take component of vectors. Then when I finally got to college. I liked mathematics very well and I thought, well I want something to do with mathematics so I investigated physics and I finally majored in physics.

Then much to my surprise I was offered a graduate assistantship to go and do graduate physics. I was first offered one at Cornell University and that was in 1929. The crash came on the East Coast before it came out here and just a couple of months before I was going to go there, they notified me that I'd been the last one hired and therefore the first one to be released - being impartial that way. So right away I called a friend of mine in the Physics Department in the University of Minnesota here. He happened to talk to the Head of the Physics Department, who at that moment was conferring with John T. Tate, whose name you've heard many times, about the need for one more graduate assistant. So they contacted another person who'd gone there a year ahead of me, and asked him what he knew about me, and the consequence was that before the day was over they had taken me on. So I was very fortunate, I can tell you that. Well, I came here in 1929 to do graduate work in physics with Tate as my major adviser, without knowing
anything about what the graduate fields were that I might be interested in. That's when I became acquainted with this man Walter Bleakney, who was working under Tate. He had become quite interested in collision phenomena, that is, the bombardment of atoms and molecules with controlled electrons, and in looking around I thought, "Well, I've got to do something, what kind of a project can I take on?" The one that Bleakney was working on looked interesting and I asked Bleakney, "Can you answer several questions for me?" "Sure, be happy to." And as it turned out, Bleakney left one year after I arrived here, and I just happened to have asked John Tate, "Could I start working on the old equipment that Bleakney had developed?" which was a Mass-Spectrograph, and they were glad to have me do it, and that's the way I got started in Mass-Spectroscopy.

I was able to carry on some of the work that he had started, plus some of my own. I was using very crude apparatus compared to what Al Nier was then able to design. Al Nier at that time was a an undergraduate student in electrical engineering, but he was quite interested in physics, so he'd been over to Physics quite a bit and I knew who he was. He started graduate work in physics then, while I was still in the Physics Department, and he became interested in Mass-Spectroscopy. Well, with his background in electrical engineering, he was able to design some electronic controls that stabilised the instruments we were working with. I remember I had to do my measurements late at night when things were very quiet, because the streetcar line down on Washington Avenue, way down there three hundred yards away, would upset my apparatus. So I worked from about midnight to about five or six in the morning, something like that, to get the measurements I could make, and my estimates to measure with were very crude compared to what Al was able to design, and also quite unstable. So Al, by using electronic equipment, was able to stabilise - let's say compensate for some of the difficulties, such that he could work around the clock, summer and winter. I couldn't even work in the summer; the moisture content
of the air was so great that it caused trouble with the instruments.

DRP: What specific trouble did that cause? Where did that crop up?

ALV: It ruined the insulation. The insulation was such that you weren't sure that you had steady conditions - aside from the mechanical vibrations that I mentioned, which affected the recording instruments. And he was able to increase the sensitivity of the instruments that way a whole lot. Well, that was his first big advance - applying electronics to the measuring devices we were using, and the compensating controls.

DRP: Did you have to redesign Bleakney's equipment for your own experiments?

ALV: I did. Not drastically, I would say. I used an entirely different scheme.

DRP: Did you change the ion source?

ALV: No, we used the same ion source. A tungsten filament was always our source sometimes we'd have an oxide coated tungsten filament, and that gave an even more copious supply of electrons, but that wasn't too essential.

I was working on Bleakney's old equipment, and eventually another fellow and I designed some new equipment - P. T. Smith, whose name you've found in the literature. He was very helpful to me. He was working on somewhat different lines, but not a whole lot different, and he was quite interested in the results of what I was doing because they fitted in to what he was doing. So, as he was much better at designing equipment than I was, between the two of us we designed a new Mass-Spectrograph which had greater range, was more sensitive, and meant that we could have a better control of the electrons that we were firing down the beam.
DRP: Was that basically the same geometry as the original Bleakney equipment?

ALV: Basically, yes. There was a little difference in the electric and magnetic fields, the design of them and so on - but I haven't given this thing thought for several years.

DRP: That was a 180° machine?

ALV: A 180° machine, yes. Both Bleakney's and mine were. No, wait a minute, I recall now: Bleakney's was different in this respect. Bleakney produced the electron beam coming up out this way. Those electrons collide with atoms and molecules and produce positive ions. The positive ions would be deflected down, pulled down here, into a side chamber. Now, there's a magnetic field and also there's an electric field here. It was arranged such that one urged the ions in one direction and the other in the other direction. When you'd just got them balanced, the balance depended on the mass of the ion, and what Bleakney did was to straighten them out so that they came down here and he would collect them.

DRP: So it was basically an E over M machine?

ALV: That's right. So this was the first instrument I worked with. The new one designed was a 180° machine.

DRP: What kind of work were you engaged on?

ALV: Well, the kind of work that I did then was to try to measure the efficiency of ionisation as a function of the energy of the electron beam itself. That resulted in curves like this (taken on the second machine), where here the onset of ionisation. The appearance potentials was something I was
interested in, but I was also interested in the shape of this curve, where you got the maximum, and how it tailed off, because what Smith was actually measuring was that efficiency as a function of various things, so he was quite interested in what I was doing for that reason. Then I went to measuring something like this: find out the onset of ionisation in order to get the ionisation potentials. I did a lot of that kind of work - ionisation potentials - with that same device, because what I'd do, I'd set on wherever this particular ion appeared and reduce the speed of the electrons to the point where it disappeared. And with that sort of arrangement, I was particularly concentrating on molecular nitrogen to measure the ionisation potential. It had some importance at that time, because with nitrogen being a very important component of fertiliser, there was some thought that how tightly that first electron was bound on had some importance to it. That's the only importance I could attach to it, the rest was just scientific interest, but if anyone asked me why we were doing that, I tried to explain it in practical terms. I'd say, "Well, the Department of Agriculture is interested in nitrogen because it's important in fertiliser". So I was able to finally measure the ionisation potential of molecular nitrogen to a very fine tolerance, with about 1/5th of a volt, something like that. And then that led to measuring ionisation potentials of some other gases. Then, since I was separating them out according to masses and concentrating on just a particular mass and getting the ionisation potential, whereas there might be other masses around, that led to noticing that there are multiple masses on the same atom - isotopes - so it was a natural thing to start looking at those true isotopes and try to measure the relative abundances of them. So I got into that work: measuring the relative abundances of isotopes. The most important work I did there was to get a pretty accurate determination of the relative abundance of carbon 13. I was able to measure nitrogen, nitrogen 14, nitrogen 13; oxygen 16, 17 and 18 had already been pretty well worked
out, but nitrogen had not been worked out and carbon hadn't been worked out yet. Some of the work I did on carbon 13 later became important in using carbon 13 as a tracer, because I was able to measure the relative abundance of it very nicely.

DRP: This again involved a new change in your particular machine?

ALV: No, this machine was flexible enough to do that. The one I showed you right here just has a wide variety of uses. First of all, I could work over a wide variety of masses. Secondly, I could use a wide variety of energies on the electron beam. And then with those two variables you could pick out any one and trace it down, and then you could make some measurement of relative abundances, all with this same machine, because you had them all spread out there and you could do about anything you wanted to on them.

RP: I was talking with Gaylord Harnwell, who of course you know was working about this particular time, and he stressed that he wasn't able to put too great a reliability on his results, simply because he felt he was tinkering with his equipment most of the time, making sure it didn't go wrong rather than performing actual experiments on it. Did you feel that your results stood up?

V: Exceptionally well, I felt. Nothing complex, but I was using pretty stable equipment - I had to, that's all there was to it. The results were reproducible and this same technician that Al Nier was talking about today was very helpful to me. We were very close friends. His name was Rudolph Thorness, but everyone knew him as Buddy. He came there the same day I did, I think it was in 1929. We arrived about the same time and in six months' time we got acquainted. He worked under another master craftsman to get started, a Dane by the way, who was also very instrumental to us. He did a good job helping with the equipment. P. T. Smith was a very good equipment designer and had
lots of good ideas, so the second piece of equipment we built, after workin
on Bleakney's, was an excellent design. Bleakney's had been put together
with sealing wax and so on, and we couldn't get rid of all the impurities.
Of course, he built a much more sophisticated piece of equipment at
Princeton. What he did here was largely on mercury: identifying the isotop
of mercury, measuring something about the efficiency of ionisation and their
appearance potentials. That's what he did here. And - well now, wait a
minute. He did something on hydrogen here because it was something in
connection with hydrogen that Condon got so excited about. This was because
it indicated that Condon's work in connection with the potential energy
curves was real, because Bleakney was able to identify some energy differenc
I'd have to look back on this to find out what he did. So he worked on
hydrogen here, to what extent I guess I couldn't say right now, so you'll
have to ask him about that. But the equipment they used gave very reproduc
results.

RP: How did you pick up your Mass-Spectrometry? Did anybody actually train you,
or did you more or less pick it up as you went along?

LV: I picked it up as I went along. There was no training programme in it. This
man Bleakney was very helpful to me, what little time of overlap we had, but
he left after I'd been there just one year, and I really hadn't got involved
in any experimental work the first year. This man Smith was my mainstay.
He was a couple of years ahead of me in graduate work and he'd been working
on related problems. But he was my mainstay all the way through. Some I
picked up myself, some we picked up together, and he was the better of the
two by quite a bit at seeing the total picture, very helpful. So with help
on this end of it, and this technician's help on the mechanics of it, he came
out with a piece of equipment which was very flexible and did a lot of things.
We produced quite a bit of good work there in a relatively short while.
Andrew Hustrulid got involved in it and then Njer got involved in it. I was
just sort of a link between two very good men - Bleakney and Nier. Hustruli
and I were both just sort of a link between those two very good men. Some
progress was made between the two, but the major progress was those two
fellows.

DRP: How well did they know each other, Bleakney and Nier?

ALV: They didn't even overlap. Bleakney went from here to Princeton, and Nier
was an undergraduate and I doubt that he even knew Bleakney personally when
Bleakney was here. But I did. Nier and I overlapped and it wasn't, however
until I had left that work and went into a little bit of high energy work
that Nier took over the Mass-Spectrograph work. Then he made these advances
that I told you about that just simply opened up the field real wide then.
So he was in a position to do something really great when the opportunity
came. There were - let's see now - some other kinds of things I was
interested in. You see these: these peaks indicate the various ions that
we could separate out. Now, there's some unpublished work on some of the
gases I worked on - carbon monoxide, cyanogen, nitric oxide, acetylene, plus
water vapour which was always present. I did some what I thought was very
interesting work on water vapour which I never took time to write up. That
was concerning negative ions. In monkeying around with this machine, you see
I could reverse any field I wanted to and work on negative ions instead of
positive, so in just monkeying around with sort of reversed fields I began to
get some ions which I identified as negative oxygen ions and negative OH ions.
And I found a very strange thing, that the capture of the incoming electron
was very sharply a function of the energy that incoming electron had, as I
recall. The data's stored away here someplace, I never pursued it. It had
a very sharp energy; the oxygen atom or the OH particle would pick up an
electron of just about 8 volts energy - very sharp - go up there and just
right down again. Well, I studied that for a while, and then got sidetracked
somehow or other.
DRP: Was it just an accident that you got sidetracked, or was it not working out

ALV: Well, I had an accident which sidetracked me, one thing. I'd been working on acetylene. This work I did on negative ions was a by-product, because as I pointed out, water vapour was only present until I got everything all baked out, and I would work on water vapour while I was waiting for the water vapour to disappear. I was working on acetylene, on some of these curves for acetylene. I had done what I wanted to do with that, and decided to change to another gas - I don't know what the next one was I was going to work on. I opened up my apparatus, ran some air through it and so on and so forth, and then started to do the glassblowing necessary to take off the bulb of acetylene - that had acetylene in it - and put another kind of gas there. The way we got the gas into the apparatus was to seal it on to a side tube which was connected to the main apparatus by a long thin capillary tube, very long and very thin. It was only about this long, hanging down inside another tube just as fine as we could draw it. Then we just let the gas come in that way, you see. So I was getting ready to put on something else and I was taking off the acetylene. We used a hot glass rod to crack the glass off, after making a little mark with a file, and so on, and evidently I'd just reduced the concentration of acetylene to the right place where it was a good explosive mixture. This was about ten o'clock at night. I applied the rod and blew glass all over my hair; I had on a pair of spectacles and it cut quite a gash in one glass here and scratched another one quite a bit. That scar there on my forehead was caused by a piece of glass. In fact, I got little pieces of glass out of my face as much as six months later - it had sort of worked its way out. That destroyed some of the glass work that I had and it set me back just about an even six months. I was trying to finish up some work more or less on schedule because I wanted to finish up my thesis to get my degree. I was going to get married some time soon and I wanted to get a job, so on and so forth. So I think that was the accident that took me away from the
DRP: Did anybody take it up again?

ALV: Yes, a fellow by the name of Massey - a Britisher - he did a very fine piece of work on that a few years later and he has a monograph on it which I saw later. It checked very well; that is, where our work did overlap he found exactly the same thing that I had. But Massey at least wrote it up, and I think I could give you a reference to it if I had a little time to find it. Here we are: "The New Age in Physics: Professor Sir Harry Massey, FRS."

DRP: How sensitive were the instruments you were working with?

ALV: Well, we could pin down the ionisation potentials pretty well with this kind of apparatus. We had good enough control over the ion source such that we could pin down ionisation potentials to less than $\frac{1}{10}$th of a volt, something like that, pretty well, and did the measurements which later proved helpful to somebody or other. I failed to mention that for calibrating purposes I used two gases - mercury vapour and argon. Mercury vapour had been pretty well worked out by Bleakney himself for the ionisation potential, and argon, the ionisation potential of argon, had been determined spectroscopically, not with the Mass-Spectrograph but with radiation, and was very accurately known. And the other thing of significance was to show that it was quite possible to measure isotopic abundances with this particular method.

DRP: So did you carry on using P. T. Smith's equipment until you'd finished Mass-Spectrometry? You didn't build any more machines for your own particular work?

LV: No, Nier was the next man to build a machine here. He began to experiment with different ways of using the magnetic fiel
very concentrated small magnetic fields. I'd used large magnetic fields; the solenoid was seven miles of wiring, so heavy it took machinery to lift it up and all that sort of thing. I had to water-cool it. But he started experimenting then with small, very strong magnetic fields, bending them in less than half a circle, and so on. And focussing; he was able to develop focussing devices as well.

DRP: Did you have any contact with the Cavendish Laboratory while you were busy in Mass-Spectrometry?

ALV: Just a little. I read all the things I could about what they were doing, but I've forgotten now who I corresponded with. I never left this laboratory right here except to go to professional meetings now and then.

DRP: You said a minute ago that your accident put you back six months as far as your thesis was concerned. Did you work like fury to catch up, or did you do something completely new to finish up your degree theses?

ALV: I went back to what I planned to do and just rebuilt the apparatus to the point where I could pick up again. But one thing that I did drop then was that negative ion work.

DRP: Would you have included it in your thesis if the accident had not happened?

ALV: No, it would have been something beyond the thesis, although in fact the wor was there.

RP: Because it was something of a different concept.

ALV: Yes. I just didn't pursue it. I have the results in my notebook which I
reported in physics seminars, but then that's a lot different to publication.

DRP: Did it cause any great surprise when you did report them?

ALV: Tate was quite interested in it, and I assumed that he would put somebody else to work on it after I went on to something else, but I don't think this laboratory ever worked on it any further. But several of them knew about that, though. The next thing I heard was that somebody told me that work was being done somewhere else. The results seemed to be consistent with what I had. But I started looking for a job then. In 1934 jobs were not to be had. Physicists were a dime a dozen. There were three of us who got our PhDs here in the Physics Department about the same time, and we were all looking for jobs. There was one came up out in the south west, in some new school that nobody'd ever heard of. We flipped coins here to see who would make the application. I didn't make application, a fellow by the name of Ronald Palmer made application, and he was one of some eighty PhDs who applied for that one job out there; he didn't get it either. So jobs were scarce. After continuing to work there for another year, doing some more work on the Mass-Spectrograph, but also getting to help out on some of the high energy work which was coming into being right then.

Then I was recommended for the job here in the new college that was starting up; the kind of work in which I was interested. I really wasn't interested in going ahead with research work alone. I wanted to teach. I particularly wanted to make known the results of science, and to make science available to the layman and others who could make use of the practical aspects of it. At the college that I head right now, the General College, I had such an opportunity. I took over the job here of providing the kind of science training that we call general education, and trying to make science available to more people. I started in teaching and consequently became too busy to go back to the Physics Department to do much more there, although I continue...
to teach some for them too. So I grew away from the laboratory work. I still keep my roots there in the Physics Department; I know a lot of the people; I go there for seminars. They have a colloquium every Wednesday afternoon, which is a thing of long standing. I still go back there; they send me notices and I go back as often as I can to hear what's going on. A lot of it I can't understand. Whenever something of this kind comes up I can go back for sure, because I know something about it. So I went out of that kind of work just about the time that Al Nier separated the isotope of uranium. I was in the building the night it happened, still doing something or other, but then my main work was someplace else.

DRP: Did you wish when you became an administrator that you would at least like to go back and study these negative ions? Was that something that you desi

ALV: Well, I've always regretted I didn't pursue that further, because it was something new in Mass-Spectrography I think. Others were working on that. I never fretted to the extent that I felt that I would break loose and do it. That became a closed chapter, and meanwhile I think it's been pretty well worked out by somebody else. But it was something that I just stumble on to and that's the way that I found science research being done at that time. Now people talk about research designs and research plans and all that. In my day it was looking around and seeing what you find that you'd like to explore and you explored it.

DRP: What sort of criterion of choice did you use?

ALV: Whether or not we had the equipment with which we might do the job.

DRP: So the technology was asking the questions?

ALV: That's right.
DRP: And you thought you were looking for the fruitful ways in which the particular technology could be applied?

ALV: That's right. I saw a big field opening up; I just saw an almost infinite amount of work that you could do then of a routine nature with a Mass-Spectrograph, and Nier developed it to the place where it becomes a working estimate now, you see, on any kind of gas you want to. But I didn't have much to do with that. I really don't think there's much more.

DRP: Well that's about it; thanks very much.

END
DRP: Can we start by discussing your progress into the field of Mass-Spectrometry? Did you do a physics degree to begin with?

AN: No, I was an undergraduate in Electrical Engineering and I should explain first of all how I got into this. My parents were immigrants and my mother came as a small child - my father came as a young man - they met in this country. They were Germans, and when I was in grade school and in high school I did fairly well in arithmetic - things of this kind - and the feeling was in the family that I should go into Electrical Engineering. I liked to play with batteries and wires and things of this kind, so I got started in this direction, and when I came to university I naturally enrolled in the Electrical Engineering curriculum. The first year is sort of a general year when one takes a mathematics and English course, chemistry and so on - that was the curriculum at the time in 1927.

Then in sophomore year all engineers took a year of physics and mechanics heat, electricity - the standard classical physics of the day - and this was in with how I happened to get into physics because the fall quarter was a course in mechanics and was taught by a man by the name of Henry Ericson, who was head of this department for many years. (And if you are interested in the history of science and the history of physics, there is an excellent history which he wrote. He kept a continuous diary, by the way, of the department from before 1900 up to the time of his retirement in 1938, of all the people, how much they were paid, what courses they taught, etc. There is a copy in the American Institute of Physics.)

Roger Stour is a man interested in the history of science. He was on this faculty and went to Boston University this year, and is coming back this fall. He started the original diary which is in my possession which is
loaned out to Dr. Vaughan, but Xerox copies were made and are in the American Institute of Physics.

Anyhow, Professor Ericson taught the mechanics course and I remember very well - a story hardly anybody knows - in those days they kept enrolment records in class - if you missed so many classes you automatically failed the course and so on - quite different than today as you realise. We had science seats, and so in spite of the fact that I was in a very large class, perhaps 400 students, he knew where everyone sat, and so about the fifth week of the quarter before class one day he came up to where I sat and said he would like to see me - and everybody overheard this around me and wondered what I was guilty of. But it turned out he was interested in having me consider physics as a career, and the reason was - we had weekly tests, and in the first four tests I'd gotten grade 100 - and he was a notably hard grader - so he thought I ought to consider physics. Well, I said I was really planning to go into engineering, but he urged me to reconsider and to encourage me - this I think is a very important point - as was done by many people in those days and I guess still by many people - he gave me a job by the hour working for him as an assistant. He was interested, I believe, in tidal pendulums or something like this, and so I used to measure off charts to see how much this big pendulum had swung around. There was a photographic chart of how this swung and you could tell when the moon went by and things of this kind. So I did this as a student, got other jobs around, and therefore got introduced to the Physics Department through this contact, which you'd never get through formal classes, you see.

When I graduated, however, I decided - well, I took physics courses as an undergraduate, including electricity, and decided that I would go on into industry in electrical engineering. But this was in 1931, in the beginning of the very great depression. Jobs were very hard to find.
and I ended up with a teaching assistantship in electrical engineering, or I might not have had any kind of job, and the professor over there saved me, you might say. He incidentally was also a physicist; his name was Henry Harding, and he was a very good electrical engineer in later years. But he interested me in continuing on there as a graduate student and encouraged me to go back into physics at the same time, because there was not very much advanced work in physics. So I got a Master's in electrical engineering in 1933, came back to the Physics Department, and got my PhD in 1936.

DRP: In what area were you studying?

AN: At that time, the isotopic composition of several elements. I found Potassium 40—perhaps the best thing I did during those years.

DRP: What methods were you using then?

AN: Well, this ties into the earlier work here. You see, in the late 1920s—(There's one more thing I should tell you here, if I might interrupt for a moment. Condon was here. Edward Condon was on the faculty in the late 1920s and he was interested—this was the time of the growth of quantum mechanics—and he was interested in problems of quantum mechanics, of molecules you might say, and he encouraged Walter Bleakney at the time to look into this problem of kinetic energy ions. There is a very good account of this, by the way. When this building was dedicated a number of years ago, Condon was one of the speakers, and I have a tape recording as well as copies of this where Condon tells about this work, and if you like I could lend you this—you might want to copy some excerpts from it.)
But anyhow, this was at that time, and so there was this interest here in the late 1920s. The work under Dr. John Tate - he had a group working on these phenomena of electron impact, and they were interested in such things as determining the absolute number of ions you got per electron per unit density in the gas when you ionise it, and these numbers - by the way, this early work done by Bleakney, and particularly by P. T. Smith is still cited in the literature. When somebody now makes a measurement of cross-sections of ionization and they come out with some number, they report that this agrees with the results of Tate and Smith and Bleakney and so on in 1932; so you can see the quality of the work these people did.

But anyhow, Bleakney built the first crossed field type instrument that showed the multiply-charged mercury ions, (I'm sure you're acquainted with that), and then also the work on the kinetic energy ions in hydrogen and then of course there was Wallace Lozier with the kinetic energy apparatus. Lozier, and later Hanson, who worked in that area on studying things more systematically. So this was a group - the reason I mentioned all this - this was the kind of activity in the very late 1920s and early 1930s.

And I was associated with the Department already as an undergraduate student assisting Mr. Ericson in 1929, so I overlapped all these people and knew all of them, you see. And down this corridor - the alley as it was called - electron impact experiments were going on, so when I came into the Department formally as a teaching assistant in the fall of 1933 the question came up what I should work on for a thesis, and I started to work on discharges. I was kind of interested at that time in other problems. I thought I should apply my electrical engineering background, and so I was interested in low discharge, problems of this kind which had been done years before and were still great mysteries - high frequency discharges in gases and plasma oscillations and things of this kind.

But there wasn't any real interest in the Department. Dr. Tate was not terribly interested in that kind of work; nuclear physics was just coming
in and he thought this was the kind of area we should get into, you see. But isotopes and nuclear physics go very well together; heavy hydrogen was just discovered, you see, and things of this kind.

DRP: Do you recollect any of the happenings concerning the discovery of deuterium - Walter Bleakney was concerned with that?

AN: He was concerned, yes. He could tell you a good deal more about that when you see him, because he you see helped out on some of the experiment verifying the discovery and papers to this effect that were published. I believe he and Lozier, did they perhaps -?

DRP: Yes - about half a dozen people put their names to them.

AN: You know Urey is still alive?

DRP: I'm going to see him.

AN: Good. He is a delightful person.

DRP: I consider other people's opinion important as well as asking the person directly concerned; it all helps to make the picture clear.

We were talking about the people who were in Jack Tate's laboratory, and you were just about to start talking about your first work in Mass-Spectrometry - leading up to building your first machine, presumably.

AN: Yes. Well, it became apparent that it would be interesting to move more into the Mass-Spectrometry direction rather than the discharge of gases. I turned in that direction and one of the new young people in the
Department was a chap by the name of John H. Williams, who was a post-doctoral research assistant to Professor Tate. He had come here in 1933 and one of the first things he did was work with Dr. A. L. Vaughan on some isotope work with the instrument which had earlier been built, I believe by Vaughan, and certainly by P.T. Smith, who played an important part. I would say that P. T. Smith was the man behind this instrument. They had worked together on this, and so I, as a relatively new graduate student, was assigned to work with Williams. But as I said before, this was just the time when nuclear physics was coming in and so Williams pulled off—they had then a 300,000 volt high voltage set, and this was about the time when the work was going on at the Cavendish Laboratory with relatively low voltage things—so this seemed like a logical sort of area to get into. So Williams was pulled off to work in that field and I was left alone in the Mass-Spectrometry area.

The first thing I did was to build an instrument similar to, but slightly larger than, the one which had been used in the work by Smith. It was probably the one used by Smith and Tate on the various multiply-charge ions and appearance potentials in some of the rare gases—oh no—in things like rubidium and potassium I believe, where they actually had vapours present. And so the instrument I had was one which fitted into a solenoid—a larger solenoid than they'd used before—this had a 7" diameter hole in it and so I could have a larger radius instrument. It was a 180° instrument and the analyser was a piece of copper tubing bent in the form of a semi-circle of I suppose maybe 5" diameter, something of this kind. And one of the first things I did with this—there was also this interest at the time in the ionization of gases by electron impact, and I also had the interest in working in that area because, as I remember now, also I worked with Hanson on some experiments to do with HCl and Chlorine, and the Mass-Spectrometer for that work was the same one which had been used by Vaughan and Williams and had been built
by Smith for some other work. So I had these several things going on at the same time, but in any case one of the things that interested me was the possibility of applying Mass-Spectrometry to the study of organic molecules. That had not been done earlier, and I got the first decent mass-spectrum of benzene (and by the way, I could probably find the original notes on this because it was done long before other people had analysed hydrocarbons accurately), and so showed how with benzene you got the peaks 70, 77, and so on, and then the five carbon group and four and so on, but for some reason or other it was never published, although it turned out to be correct and much better than the earlier work that had been published, I believe by Kalman in Germany. But this instrument - the interesting thing about this was it had the resolution to resolve completely mass 78 from 77 and so on, and with the interest in nuclear physics, it immediately became apparent that one ought to look for isotopes and settle some of the abundances of rare ones which had been reported by Aston and by others, and where there was uncertainty whether they existed. And of course with the photographic method you could not get the precision that you could get with the electrical detecting devices. So that's how I happened to move into this area, and the first element I looked at was argon, where there was uncertainty about the amount of argon 38, and showed very nicely that this ratio of 36 to 38 was about 5 to 1.

DRP: Was that in fairly close correspondence with Aston's results or was there a discrepancy - did his machine have such good resolution as yours at that time?

AN: Well, he had better resolution, but he also had a great many impurities because of the discharge, so that he could never be sure when he found a rare isotope whether it was a hydrocarbon impurity or something else.
You see, our instruments were completely of glass, sealed and baked; there was no wax, no grease, nothing like this, so you could really do a much better job, and that was one of the great contributions we could make to this area because of our better technology. Also the fact that we had quite high abundance ratio sensitivity - in this first work I was still using ordinary electrometers - the quadrant electrometer - this was also the time when the electrometer tube came in, so quickly we converted to the use of that, so all my work as a graduate student and the later work was done with, not the FP 54, but the Western Electric equivalent of it.

Well, the argon 38 was the first thing I worked on, I showed how nicely you could do this, and then there was the great argument about the radioactivity of potassium - which isotope was responsible, was there potassium 40, which had been suggested - so I set out to look for that, and indeed in the summer or spring of 1935, I found potassium 40. Brewer, by the way, had been working in the same area at the same time, and shortly after that confirmed the abundance of it - as if you talk to him you'll learn that.

Well, then I went to work on other elements: zinc, cadmium, and something else - I've forgotten what else - and that really was the end. I finished then and got my degree in the spring of 1936, and left immediately for Harvard, where I was a post-doctoral fellow for two years with Bainbridge. I came back here on the faculty as Assistant Professor in the fall of 1938.

DRP: What work were you doing with Bainbridge - were you doing Mass-Spectrometry with him?
AN: Well, yes. He provided me with anything I wanted - this was a marvellous thing at the time. Money was very scarce for research equipment, but they had funds available, and Bainbridge was most generous in giving me anything I wanted, which was quite an unusual thing. And I worked independently; he was very helpful to me in every way, but never interfered with what I did. He just put everything at my disposal, which was most amazing, and of course I had two very productive years there. I also met individuals who were very very helpful there, particularly Professor Gregory Baxter, who was one of the great atomic weight chemists who had a marvellous collection of lead samples, both common lead and radiogenic lead, essentially the world's collection, and he put all of these at my disposal. He converted all of them to lead iodide, because it was a vapour that could readily be used.

There was also at that time in the neighbourhood Professor Alfred Lane, a geologist, who lived near Harvard Square, and who had been a professor at Tafts College in another suburb nearby. He was active on the National Research Council on what was then called the Committee on Geological Time, and they encouraged anyone they could to work in the area of geochronology, and he was very helpful to me in getting other samples. Baxter, as I said, put at my disposal, and converted for me, the various lead that he had into lead iodide for my use, and he was such a good chemist that the samples were unbelievably pure, so I had the very best chemicals to work with. He became so interested in the work he used to come by everyday. He taught a class at Radcliffe College, which was on the other side of the campus. He'd have to go to chemistry past physics on the way, and he'd drop by before and after the class to see what the latest project was. And as I have described it, I had at my disposal a full professor at Harvard as a research assistant to make chemical samples for me, and he was so interested in helping me, a young person
at the time, that it gave enormous impetus to the work. He was nearing
the retirement age; he had spent his life in atomic weight work. He
realised that Mass-Spectrometry would take over some day on this. In
fact, he commented to me as our work proceeded and we got better and
better results, he was glad that he was nearing retirement age. When
I started the work on lead, one of the things he had was a whole load
of common lead samples, and they had always been found to have, within
what they considered their error of measurement, the same atomic weight -
207.21 I believe was the number - and they came out always within one
part in that second decimal place. If somebody came out different than
that, it was generally assumed that he had done a bad job. Well, he put
at my disposal a number of these different samples - I remember I believe
there were four originally - and I ran them. To everyone's amazement, I
found rather large variations in the isotopic abundance. This was quite
a shock, and I knew the results were real, but no-one would believe me.

DRP: You could place that much reliance on the results?

AN: Oh yes, they were that good; and the variations you see in the isotope
abundance ratios were like ten per cent or thereabouts, and later on some
were even larger. This, as I said, created great consternation. But
the interesting thing about it was that in spite of the fact that these
abundances varied so, you always came out with about the same atomic
weight, because what was varying was that the 206 and 208 when compared
with 204, the rare isotope, went up and down about the same amount, and
with the atomic weight about half way in between it was concealing the
difference. So we had very large differences. And so Baxter, since he
didn't trust me at first on this, because this just didn't make sense,
would give these to me as unknowns to try me out, and of course he tried
all kinds of combinations to check me and I always came out with the
answers, until he finally had to conclude I was right. Of course, it did not invalidate any of the chemical experiments because they'd always found the same atomic weight, it was just something that had been concealed there.

Well, by coincidence, at the same time, Arthur Holmes at the University of Edinburgh - who was also interested in geological age measurements - had just written a paper which had been published in 'Economic Geology', in which he argued that because of the constancy of the atomic weight of lead, lead could not have had a magnetic origin, because he says if this were so, then it should have varying atomic weights because it would then be contaminated with various amounts of radiogenic lead from the decay of uranium and thorium. This came out within days of the time when I made this discovery of the variations. The economic geologists at Harvard of course were terribly disturbed when his paper came out. Professor Gratton, an economic geologist who had been teaching other things in economic geology, found this paper very distasteful, and so when he learned of my discoveries - which wasn't very long - he and I became great friends too, because I had the key information which he needed to disprove what Holmes had said. I met Holmes years later in 1954. I visited him in Edinburgh on a short vacation (he lived only a few years after that and I did not have the pleasure of meeting him again), and so we talked about the old days. (By the way, Holmes and I became great friends by correspondence during the war.) Of course, after he published his paper, I immediately wrote to him pointing out what I had found. The error he had made was not really an error - who would have dreamed that you could have such a coincidence - and so his arguments were perfectly right, if you allow for the coincidence that the atomic weight doesn't change. Well, as you know, everybody knows that Holmes had immediately seized upon this and actually developed this field, and showed how the variation in common lead could be used
systematically to show that the earth was older than two billion years. It's something that I should have pointed out at the time, and did indirectly, that some of the samples appeared to be older than two billion years, but I was hardly the person to challenge the great geologist of the time, so I missed this opportunity of contributing something new. Then of course many other people went and got into the field later, using the variations in common lead, so that you finally come down to this value of 4.6 that is now more or less accepted by everybody as the age of the earth and the solar system as we know it.

DRP: I see in the literature that you collaborated at least once with a chap named Jerry Pickavance. Did you work a great deal with him?

AN: Not really, I met him, but he actually worked with Mark Inghram - this was of course at the time in connection with the uranium work. We were all in the same building - the Nash Building which had been a warehouse for the Nash Motor Company, and which had been converted into a laboratory for the Manhattan Project. We were all there, but we actually didn't see much of each other, because we were on different parts of the Project then.

DRP: You of course were on uranium separation?

AN: Yes. I was in charge of the development of Mass-Spectrometers for the entire gaseous diffusion project, although the instruments were later used for other things. The helium leak detectors were developed here at Minnesota, then by the Kellex Corporation. The instruments used for all the uranium analysis through the entire project were developed here. We built more Mass-Spectrometers here from 1941-43 than probably had ever been built before. We built seven uranium instruments; I think something like ten hydrogen/deuterium analysis instruments; four helium
leak detectors; and a number of other miscellaneous instruments. So we became the biggest manufacturers of Mass-Spectrometers at the time. Of course, most of the instruments were then built by the General Electric Company, which got the contracts for the uranium instruments; for the leak detectors, which were produced by the hundreds; Mass-Spectrometers by the many dozens (which were used in the electromagnetic separation plant as well as the gaseous diffusion plant); and for the miscellaneous ones for the centrifuge work at the University of Virginia. And some of these original instruments are still in existence, by the way - I don't know if they're still used, but they still exist. Unfortunately, we don't have any of this equipment because it was all secret and taken away from us when our Project closed; it went to other places, and then when the war ended that's where these things were, so we have nothing to show for this. We still have some instruments around somewhat similar to parts of Mass-Spectrometers. I can show you one of those if you are interested in seeing it, but most of the original things have disappeared since.

DRP: During the Manhattan Project, did you see the use and development of the Mass-Spectrometer going according to plan: the goal was there and you followed the right path?

AN: Yes. Actually it turned out to be a very fruitful programme. If you follow the different things that were done, I think it really turned out quite well. For instance, take the helium leak detector. The original ones we built here had a glass spectrometer tube. We had enclosed permanent magnets; the envelope surrounding the Mass-Spectromete was all glass. We made one concession, if I remember correctly: the filament was waxed in. We had a ground joint with wax around the outside that was the one concession we made to having an ultra-clean system.
We had mercury diffusion pumps on those; when they went into commercial production, they all became metal. We built the first all-metal spectrometer tube for this as a sample for General Electric and they copied it, and those early leak detectors were just about direct copies of ours. They had, I believe, oil diffusion pumps, but they may have had mercury diffusion pumps on them. There was a liquid nitorgen trap on them, of course, and then there were metal valves and so on. I don't think, given the time span, you could have done much better. The instruments were operated, since they were portable, by two 6-volt automobile batteries, which of course later on were replaced by other things, but that was very convenient then. Two 45-volt dry batteries ran the acceleration supply, if I remember correctly, and so it was a completely portable unit. It was big and ponderous, but still it was portable, so they could have these in the foundries of the Crane Plumbing Company that made valves for the Manhattan Project, and various other places - on the test floors at the Oakridge Diffusion Plant and other places. So while they were unwieldy, they did work and were amazingly reliable.

An interesting thing was that one of the problems we had was that since it was known that these were instruments right on the forefront of technology at the time, there were a lot of bright young people working with them who were supposed to do maintenance, but they wanted to improve them all the time. For instance, the feedback amplifiers we used (which were perfectly good - they worked beautifully well), they would want to add stages of amplification. For instance, one came in that was damaged or something had happened, it may have needed maintenance; they would want to rebuild the circuit. And so we had a great deal of trouble in the early days. We finally insisted that the maintenance be put in the charge of people who were not that bright, but could follow
instructions, whereupon the 'down' time dropped in a precipitous manner, and I think they finally got things so that they would be in operation 95% of the time, which at the time was really quite good. The uranium instruments went into production, and while there were the usual problems in the early days, the GEC in the end did a pretty good job in getting these going. They were used by the dozens: as I say, in Oakridge for the uranium analysis, both at the Y 12 plant, which was the electromagnetic plant, and specially the gaseous diffusion plant, which as you know became the backbone of the whole effort later on. The circuits that were used were almost the identical ones that were developed here. The 6\" instrument - the 6\" x 60\° angle - became the standard instrument. The ion source we had developed here originally with the multiple electrodes became the standard for this. The system we'd worked on, whereby you had no more electrons than necessary hitting any given surface; so that you didn't decompose any UF6 to UF4 to form insulating coatings, that was followed quite well. As time went on, they learned more and more what the tricks were in working with UF6 (which was not easy, by the way); and there was a lot of maintenance on those instruments but the people in charge of these things finally got quite good at this, with the result that the down time on those was remarkably low, considering the difficulty of working with UF6.

DRP: You designed an extremely successful machine?

AN: That was quite a successful machine. It had to have high resolution to separate the uranium isotopes, and of course as time went on there in the plant, the question arose of the enrichment of the U234 as well as the U235; and then later on, when they began to feed in products that had been in reactors for a time - there were other isotopes to worry about - it worked out quite well.
The biggest development I worked on in those later years: I left the University of Minnesota in the summer of 1943 to take charge of the Kellex Laboratory in New York City, in the Nash Building as I said before, and our biggest project was the development of what was called the line recorder, which was the first on-line Mass-Spectrometer for monitoring the impurities in the Oakridge gaseous diffusion plant. And there we had to do a certain amount of inventing new techniques. The reason for having the Mass-Spectrometer on this was to watch for impurity building up in the gaseous diffusion plant which would kill the plant—either destroy the barriers or the platinum. For instance, if it developed a leak due to air getting in or a refrigerator pipe breaking somewhere, this of course could fill the entire plant with impurities, so it would have to be shut down.

As a matter of fact, it happened once: in the spring of 1945 the entire plant was shut down because the people in charge of operating refused to believe the Mass-Spectrometers. By that time we had them on the plant. This is an interesting story in itself - I don't know if you want to hear it? These are things that are never published anywhere as you know. But at that time the lower part of the plant was operating and was producing the feed material which was given to the electromagnetic plant. You see, if you could start with a higher base, this of course would help enormously in getting large amounts of material, and the feeling of some of the people - our idea in operating the plant was to have the Mass-Spectrometers sprinkled throughout the plant at various stages - I've forgotten whether there was one Mass-Spectrometer every 100 stages or so of the plant. And then there would be slave recorders run from the strip chart recorders at the instruments - slave recorders in a master control room - so the operator could see the readings on each of the Mass-Spectrometers in each part of the plant simultaneously.
And this was before the days of the electronic recorders; these were the mechanical balancing type which were made in this country by Leeds and Northrup, so they were rather slow. Also these were multi-point recorders so that you could check - I've forgotten the exact number, but I believe these were 16-point recorders, so you could follow as many as 16 component by jumping from peak to peak on these. We didn't use all 16; I think we used something like 8, and then duplicated some of the signals such as nitrogen, which were interested in monitoring more than other things.

Then one day in the spring of 1945, a major leak developed some place in the lower part of the plant. The Mass-Spectrometers showed this, because the air or nitrogen leaking in - I've forgotten which it was - of course it went and worked its way up the plant, so you could see recorder after recorder showing this wave of nitrogen filling up the plant, coming along just like a wave of water, as though there was a flood. The so-called practical people running the operations refused to believe the Spectrometers but began to shut valves.

DRP: They knew that there was a leak?

AN: They knew there was a leak, but they didn't know what to do about it. Whereas the man in charge of the instruments, he nearly went crazy because he knew exactly where the leak was, but they wouldn't believe him. It was obviously in the first building that showed, because you see the light gas did not go down in the plant, it only went up; so you could just trace it back to which was the first Mass-Spectrometer to show it. He told them exactly where the leak was, but they wouldn't believe him. By the time they got around to this, however, the entire plant had filled with air, so that the entire production of uranium 235 was shut down until they straightened this thing out. Well, by sheer
coincidence I had come into town that very day - into Oakridge - when all this happened, and my superior, the man who I think was not the President but the General Manager of the Kellex Corporation, also came to town that day. You see, the plant was being operated by Union Carbide personnel, but the Kellex people still had charge of it as the builders of the plant, so there was this mixed-up relationship. So we told our story to Mr. Baker, who was an electrical engineer himself and appreciated these new instruments, and was also in a position of high authority. The records on this were the sort of thing you could publish in a text-book on how you look for leaks in a big plant, because the records told exactly what had happened; yet no-one had been willing to believe. Well, I can assure you, after that had happened the man who had been crying in the wilderness when the first accident happened had a great deal more authority than he had had up to that time. After the instruments found their rightful place in the Project, and were very useful for diagnosing many troubles which were taking place. This was perhaps the most interesting development.

DRP: And after that AEI used your designs?

AN: Well, I don't know the exact history of that. There was the double focussing instrument that came later, and I don't know if you want to hear about this. I think we shipped two Mass-Spectrometer tubes to England -

DRP: - and one got broken?

AN: Yes, I believe this is right; I never saw them again. In fact, I was supposed to go - and I'd already had my shots and everything else -
I was supposed to go, and this would have been about 1942 I suspect, or early 1943.

DRP: These were the tubes that you shipped to Liverpool?

AN: To Liverpool, yes; and I was supposed to go with them, but this was all cancelled suddenly, because I guess the decision was to concentrate the work in this country - that's my understanding. Then Pickavance came over, and other people came over of course. But the 6" x 60° instrument became known during this period. Many people saw these instruments in use for the uranium work, and they were used for other things. So I guess the single-focussing instrument became recognised as a possible instrument for a variety of uses, and General Electric were making them. And I suspect that this is how a number of different companies got into this in seeing how this could be done.

The double-focussing instrument is another interesting story. In the late 1940s when I got back here - of course I got back here in October 1945 - we had no equipment. We had lost everything during the war. Everything had been given away or sent away, and other people as a matter of fact had some of my Spectrometers from the war. Because these things were still secret as they were tested for projects, they ended up being classified and we had nothing. So I found myself competi with people who had the most advanced instruments available: namely, ones that we had built and developed; and we had nothing, so we had to start from scratch, which we did.

We built a number of 6" instruments of our own for a variety of uses - we looked at isotopes and some elements - but then we became interested in the possibility of measuring atomic masses directly, where it was
obvious you had to have higher resolution and the kinds of properties
the double-focussing instruments of Aston's or Dempster's had. I saw
what I thought, and what turned out to be, the breakthrough on this:
photographic recording had gone about as far as it could, so why not do
this work electrically, where you could tell instantly what was happenin
But this required a degree of stability that was not known in electronic
circuits generally used at the time. Then the other thing that I thought
ought to be done was to look into the possibility of getting higher-order
focussing, at least for angle. You theoretically ought to be able to do
this if, instead of having a photographic plate, you concentrate on one
position, i.e. having a single slit. I put a student to work on this -
Edgar Johnson - and we published a paper in 1952 based on work he had
done for his Master's degree in about 1948 or 1949 in which we showed
this. Johnson never went beyond a Master's degree, and yet he was one
of the brightest people I ever knew. He still works here in town; he
works for Minnesota Mining. I believe he's worked on advanced copying
machines or something, and then some process of his revolutionised the
copying machine business - at least, Minnesota Mining's part of it.

But anyhow, to return to double-focussing: he was very good at carrying
out these difficult calculations, the kind which I always had trouble
with. I always managed to change a plus sign to a minus sign, or leave
off an exponential somewhere, like those kind of terrible calculations
that Hintenberger was so good at, or Awald; they were such suckers for
punishment at this sort of thing. I never had the patience, but Johnson
did, and he carried out this calculation. In fact, I think he did the
first calculation of an electrostatic analyser, a cylindrical
electrostatic analyser which appears in this paper. The first order
term had been done earlier, but nobody had bothered to carry through the
calculations to higher orders, which then showed how you could put a
magnetic and electrostatic analyser together to cancel out the alpha-squared term. Now this was before the days of computers, you understand. Now of course people do this with computers, and one could design a new instrument every three seconds.

So this was the formula which was incorporated in this double-focussing instrument which we began to use about 1949, and I had a series of students on this. And where you could get the resolution, you could have a wider angle opening and still maintain the resolution, and then by using a second Mass-Spectrometer tube as a monitor you could cancel out the variations, either of the high voltage or the magnetic fields. And so these instruments were quite successful for this purpose. John Beynon, who was with Imperial Chemical, was of course interested in this work at about this time, and we became acquainted, and I guess he worked pretty closely with AEI and the development of the instrument they had there. This was before the time of the use of these high resolution Spectrometers for gas analysis purposes or chemical structure measurement. In fact, when we first entered this, in 1949 or so, I believe, in justify this he said that if it wasn't useful for mass measurements, if we couldn't do well enough to compete with the photographic people, it would always be useful for chemical analysis purposes. We never got around to exploiting this ourselves, but I contributed a paper to this effect (which most people aren't aware of) in 1954 - the paper published in the Proceedings of the Bunsengesselschaft 1954, which may have come out in 1955; In was reprinted in 'Science' in this country. I pointed out how you might be able with a high resolution machine to use it for organic analysis, where you separate out different compounds by their exact weights. And that's buried in that paper somewhere. I don't know if anyone's ever seen it; I'm sure John Beynon read it, but I don't know who else did. By the way, I have reprints of practically all of these things.
DRP: I'd be extremely interested to see those concerning the designs of the various instruments and your early work on the instruments.

AN: Yes, well, I have them. We never published very much on the designs because we felt Dr. Tate, who was editor of the 'Physical Review' for twenty-four years as you probably realise and as editor here was very cost-conscious, always pruned papers down that came in. We were expected to set an example on this and I'm afraid, and I think rightfully so, our papers were almost always too brief. I think this was one of the criticisms of the 'Physical Review' at the time; they never gave enough details on instrument design, helpful as it might be to people. But this was his influence as editor. He was really at heart a theoretical physicist, although he was not formally a theoretical physicist, so he didn't feel too much experimental detail was in order; he was much more interested in the result. He has a son by the way, of the same name - John T. Tate - who is a professor of mathematics at Harvard, who's a brilliant mathematician.

DRP: Well, do you think we've more or less covered your progress through Mass-Spectrometry up until some time after the Second World War from the factual point of view?

AN: Yes.

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You can believe this or not, but when I was a graduate student I had completed all my work by the winter, anyway before spring (school normally ends in June), and I had an appointment as an assistant, for which I was awarded the National Research Council fellowship which started that next fall. So I was sort of done, I think, around January or even earlier, before I got my degree, and a number of us - including
Andrew Hustrulid, who worked in the field and was a graduate student along with me - built an apparatus for separating potassium by evaporation. Following the technique I believe originated by Hevesey, who had some years before evaporated something like a kilogram down to a gram, that sort of thing, to see if the isotopes' abundance had changed, we did a joint experiment with a chap by the name of Donald Hall. He was one of, I believe, Bill Libby's graduate students from California who came here as an instructor, and he was an expert on counting - beta counting, low energy beta counting - and we did the joint experiment. We separated the potassium, and actually enriched th 41 relative to the 39, and also the 40, and the idea was that Hall would measure the increase in the activity. From that you could conclude whether it was the 40 or 41 that was responsible for that. Well, everything seemed to work, but results were never published. I don't know what ever came of this experiment. They continued, I believe, after I left in 1936, but we had actually found that we had enriched the potassium something like 6% of 41. So the effect should have been measurable, but nothing ever came of it.

DRP: Were you acquainted with Brewer's work?

AN: Yes.

DRP: He cribbed from nature, if you like, for his work on electrolytic separation through a membrane for potassium isotopes; he subsequently wanted to incorporate that in a process for uranium separation. Are you acquainted with his work at all?

AN: Yes. That was of course a little later. That was more in war time, I believe. I was acquainted with it, but I wasn't really associated with
You may be interested in the controversy over the discovery of potassium 40. I had found the evidence for this in the late spring or early summer of 1935, and prepared a paper on this which was published in the 'Physical Review'. Professor Tate, who was my adviser at Minnesota and also editor of the 'Physical Review', was at Columbia University teaching during summer 1935, and the paper was prepared for publication; he made some minor changes in it, and it was published I believe in the fall of 1935. In any case, while this was in the press, and it was too late to withdraw it, a letter to the editor came to the 'Physical Review' from Keith Brewer, who was then at the Department of Agriculture I believe, and who had been working on potassium using thermal ion sources with a magnetic Spectrometer. The letter indicated that potassium 40, if it existed, had an abundance less than some number which was appreciably smaller than the abundance I had set for it in my work. Naturally there was great consternation, because there was the thought that perhaps I had made a mistake and measured an impurity when I reported this amount of 1 in 8600 for the abundance of potassium 40 relative to potassium 39. In any case, Professor Tate communicated with Brewer, who remeasured his work, did it over again, and he verified that the abundance was something around 8000. If I remember correctly, in his publication which he then presented later, it came out to be 1 in 8300, closely paralleling the result that we had given.

DRP: What was his first abundance ratio that he gave in the letter?

AN: I don't know, but I believe it was something like less than 1 in 30,000 or 1 in 50,000, which was a number so far removed from 1 in 8600 that there had to be some gross discrepancy. Apparently what had happened
was that his resolution was not quite as good as mine, and he had read
the background wrong or something of this kind - a mistake that would be
easy to make - and indeed it was a mistake which he acknowledged he had
made and that came from misinterpretation of the spectrum.

DRP: It wasn't due to a sampling error, or anything of that nature?

AN: No, it was nothing of that kind; it was simply an error in interpreting
the shape of the peaks.

DRP: So he missed out a bump altogether.

AN: He missed on it, that's right.

We're talking now up to World War II really aren't we on this? Well, I
think by 1930 the field had developed well in certain directions. There
had been mainly the high resolution instruments such as Aston's; the
kind of things Dempster had done, things had been developed in that
direction, and this had to do largely with looking for isotopes,
measuring their masses, that kind of a thing. The possibility, however,
of using a Mass-Spectrometer for other purposes was not so evident; not
that this wasn't thought of, but it wasn't considered a very useful tool.
I think that the important breakthroughs that took place - and they
weren't really breakthroughs, they were more the application of other
sciences or technologies to the area - were these:

First of all, the use of better vacuum techniques; clearly, in the 1920s,
the development of the mercury diffusion pumps, the getting rid of all
the wax and the grease, things like this. As I said, all the work in
this laboratory in the early 1930s and late 1920s was done with systems
that had no stopcocks and everything was sealed off; we were all expert
glassblowers - you had to be to be in the field. As a matter of fact, in
this day, an interesting thing in the physics building here, with
150 graduate students and a faculty of about 50, I believe there are
only two or three of us who can blow glass, and I still blow glass when
necessary, although we have professional glassblowers on the campus.
Well, the development of vacuum techniques was an important step.

Then I think the important development was the work here by Dr. Tate
and his students in using the electron beam collimated by a magnetic
field so that you had a well-defined ionising beam. You see, their
original interest in this was merely to determine appearance potentials
with a tube which just had a pair of plates on either side of the
electron beam. This was the classic work of P. T. Smith. Then it was
of course a simple matter, after Bleakney's work with his original
cross-field instrument, to then hang an analyser on the side to draw the
ions out and put them through a magnetic analyser. I think that develop-
ment of the well-collimated electron beam, where you then knew the
definite geometry of the ionising electrons, made possible the realitvely
high resolution, because your ions all originated in a region of equal
potential, or very near equal potential. Now I think this was a very
important development which was just at the right time, and which I certain
made use of in the instrument which I used for the potassium work and
the lead work later on. As you know, Harold Washburn commented that he
followed this instrument which I had at Harvard in the original commercial
instruments because it was such a successful thing. But I think the
thing that made it all possible were the developments by Bleakney, Smith;
these people who really the only workers in that field. You see, the
other people that had done electron impact work had generally sent
electrons along the line that ions were drawn out, so you had an ill-define
region which may have given you more intensity, but a terrible energy spread. So having a definite energy spread made possible the whole field of Spectrometry, which led into the isotope work which we did, the gas analysis work of Washburn and these other people later on.

Then of course I think the third big factor that was important in the field was the development of electronics. First of all the electronic amplifier, then getting away from batteries, then inverse feedback devices so you could have stabilised high voltage power supplies. In my original work I had a storage battery, a high-voltage storage battery with little glass cells; I think they were 1 amperhour or 2 amphotours capacity, and you had a whole pile of these stacked up. Matter of fact, when I was at Harvard, my high-voltage power supply from which I could get up to 2400 volts in 2-volt steps was part of their 100,000 volt battery which Duane had used in the 1920s and early 1930s. He was one of the people who was interested in measuring the ratio of H to E and so on from X-ray onset potential or something of this kind, and they had this marvellous 100,000 volt battery at Harvard which they had dried up and stored parts of it, and they fixed up 2400 volts of it for me, which I used for acceleration. This included quite a bit of gadgetry, but now you just throw a switch and you get something to a hundredth of a percent or a thousandth of a percent of the voltage you want.

So I think that all these things working together really were the big developments that made Mass-Spectrometry possible of the kind we saw after the war. You see, we didn't even have recorders during the war - electronic recorders - the only recorders that were available to us at the beginning of the war that were practical devices were these ones that were made by Leeds and Northrup, which were mechanical recorders. You had a galvanometer element, and it was all balanced; something bit on a
needle and turned a cam, which in turn readjusted itself (it was self-balancing); so you didn't even have decent recorders at the time. But of course with the war the electronic recorders came out, particularly the Brown instrument ones at the time, and then Leeds and Northrup came out with better ones, so you then had the strip chart recorder available; but this was part of the whole electronic revolution.

So I think these are the three big things that made Mass-Spectrometry of the kind we have seen, and from them you can develop any amount you want; and now of course we have all computerised devices and so on that go with it.

DRP: If we consider the early developmental years of Mass-Spectrometry from 1910 up to the war, Mass-Spectrometry was almost purely the physicists' domain. What type of persons do you think were the ones who were most interested in Mass-Spectrometry? You had an electrical engineering background, Aston had a fairly technological background. How about the other ones that were in the field; for instance, Henry Smyth?

AN: I think they were genuine physicists. They probably did not, however, have the engineering type of interest in these things. I think this is why some of us carried these things further, like the use of modern vacuum technology, electronics, and so on, although Henry Smyth of course was a little earlier in this - he still had grease joints, things of this kind. Of course, I think the people here that did the important things - Bleakney and Smith, for instance, who I regard as the real people who opened up the field - were purely physicists. I believe their undergraduate degrees were as physics majors, which was kind of rare in those days, at least in American universities.
DRP: There was a certain amount of resistance over letting Mass-Spectrometry as a technique get into the hands of chemists and biologists. Would you like to comment on the gradual changeover from its being purely a physicist's domain to being more general; is it attributable only to the industrial developments, or do you think theoretically it was pounced upon before?

AN: Well, I don't know if there was any deliberate separation. I think that people were acquainted with certain kinds of techniques and if they knew about those they would apply them to research in their field. I would today no doubt be doing physical chemistry - well maybe not today, but certainly would have in the 1930s if nuclear physics hadn't come along - I'm sure I would have been doing the kind of thing that John Beynon was doing, because this was what I really started to do and could probably have done well. I had the best Mass-Spectrometer in the world, and if you recall, I obtained the first benzene spectrum, for instance; and yet I dropped this because of nuclear physics - you just can't do everything. I would have pursued this and done more on appearance potentials and probably would have pursued all this sort of work on studying complicated molecules, rearrangements, all the kinds of things that people talk about now.

DRP: It is interesting to see in Aston's time, when the full evidence for isotopes was becoming available, that nevertheless ion molecule reactions were pretty much of a mystery at that particular time. Isotopes were regarded by the physicists as an OK subject to be studied with the Mass-Spectrometer, but although ion molecule reactions had been discovered at a similar time, they were not investigated for a considerable period of time.
AN: Well, there wasn't any field really such as chemical physics; I think this is the difference. There were chemists of the time who were physical chemists; certainly there were people such as Glöckler here, and Melyin Calvin, who as you know won a Nobel Prize with some of his work on photosynthesis. He was a student of Glöckler's working on something on appearance potentials, where they just had a simple sort of Franck and Hertz tube, as a graduate student in physical chemistry. He later on went into organic chemistry, of course, but there were relatively few people doing what you now call chemical physics.

DRP: What about gas kinetics?

AN: Gas kinetics, yes. That was in a field that wasn't started and people were not skilled. I think the main thing is that you simply weren't skilled. The chemists were sort of more 'wet' chemists, if you wish, and if you couldn't do it with a test tube, and so on, you didn't do it at all. They were relatively unacquainted with vacuum techniques, for instance, which you had to know to play in this game; and also electronic

Now I've co-operated with a lot of people from other disciplines. One of the most notable ones is Harland Wood, who is an outstanding biochemist, and one of the things I'm rather proud of is how I helped him and his colleagues at Iowa State University show how certain bacteria could incorporate inorganic carbon. He'd been very famous for this, and probably should have won a Nobel Prize for it. We had separated the carbon, which I had separated by thermal diffusion. Right after I came here I began working on the separation of isotopes by thermal diffusion and at one time had the world's supply of carbon 13, or most of it, because we had made it here, and we also had the Spectrometer for doing the analysis. This was before the war, you see, and there weren't many people operating Mass-Spectrometers that could do isotope analyses, so we helped them. We gave them the separated carbon on which he let his little bugs grow, and then
they took the molecules apart that they created, and showed in which part of the molecule - whether it was in the COOH end or something - the carbon 13 was planted. He did some beautiful experiments of this kind.

Well, why I mention all this is that Wood was a very, very good experimenter - though his field was really biochemistry - and with great effort he built a Mass-Spectrometer down there. We built the Spectromete tube, and he had students help him on other parts of it. He even built a thermal diffusion column to make his own carbon. This was all in the early days; well, it was just before World War II as far as we were concerned, 1940 or thereabouts. And they had great difficulty in building and keeping this stuff running, yet he was one of the most skilled, diligent and hard-working people I have ever known. And so the techniques of operating this kind of equipment was really another discipline; I think this is part of it. And then of course - you ask about development I think the thing that has made Mass-Spectrometry possible for so many people is the commercial developments: the fact that you don't have to be an expert at Mass-Spectrometry. And this is of course in some ways a regret to me, if you wish, because I used to be such an expert on the things. We were the only people that could build these things; we could then control what work was done on the Mass-Spectrometer, but now with World War II, with our own production putting people into business, if you wish; through this Mass-Spectrometry indirectly became available to a lot of other people, which was really very good. I say that, but I mean it was really very good, and with so many companies now that you can buy these from, it means that you can apply this to other fields where you don't have to be an expert at this rather limited, exacting sort of a discipline.

END
DRP: Perhaps we could begin by discussing your early work, leading up to your discovery of Deuterium?

HU: Well, I was at Columbia and thought I was going to do molecular spectra. I didn't do a very good job of it. I hadn't had enough training in physics to do it, and I didn't do a very good job at setting up my spectrograph, and it wasn't a very good spectrograph anyway. And so this didn't get along very well. Then the idea came to - well - out here in California, Professor Birge and Donald Menzel published a paper in which they predict the existence of heavy hydrogen. It came about this way. Aston in England had determined the atomic weight of hydrogen relative to oxygen, of course it was hydrogen 1 relative to oxygen 16, but he didn't know the isotopic abundances and he got a value. Then the chemists - I think it was Professor Lamb at Harvard - determined the atomic weight of hydrogen relative to oxygen, but in this he was determining the ratio of the mixture of isotopes of hydrogen relative to the mixture of oxygen isotopes, and didn't know it. And then Giauque at California, at Berkeley, discovered the oxygen isotopes. Then course there had to be - well in the first place, the chemical and the physical determinations agreed. Well then, if they agreed, and you had heavy isotopes of oxygen, you had to have a heavy isotope of hydrogen, and Birge and Menzel used this data to predict its existence and predicted it to one part in 5000. Now then, this was such a low concentration of an isotope that we had no means of finding it, so we felt we had to concentrate it in some way. My colleague or my assistant, George Murphy, and I worked out a theory that indicated that there should be a difference in the vapor pressures of the hydrogens. We estimated how much it was, and then Ferdinand Brickwedde in Washington distilled some hydrogen and got a concentrated residue, and we looked for it and found it to the extent of about one part in 5000 - no, about one part in 1000, where we expected to find one part in 200. Now the mistakes that were made along this route a
interesting. First of all, Aston made a mistake; one of the few mistakes he ever made.

DRP: By deciding that oxygen was monoisotopic?

HU: No, he was looking at the oxygen 16 isotope - the hydrogen isotope - I've forgotten the details of why it was wrong, but it was wrong. In the second place, the chemists purified their hydrogen by electrolysing water, and they didn't know that this fractionated the isotopes. The two effects cancelled each other, and the prediction of Birge and Menzel came out right. Now then, we made a mistake. Brickwedde in Washington had just taken down his electrolytic cells with which he made hydrogen and thrown away all the beautiful water that had an increased concentration of hydrogen - of deuterium - in it. And he put in nice fresh clean water, electrolysed it, and of course got hydrogen that was depleted in deuterium by about a factor of five. Then we distilled the hydrogen and we concentrated it down from 25,000 to about 1000. That is what happened. And then we were able to find it. So that there were a whole series of little mistakes that went into this, and the prediction was based upon two mistakes that cancelled each other. Well of course we looked for it by means of the spectrum, and at Columbia University there is still the spectra that we took at that time as a sort of a relic of this. It is in the Chemistry Department muse. If we hadn't found this, somebody else would have found it in a year or so I think it was about two years later that the people at Berkeley running their big machines found an effect that was due to deuterium in natural hydrogen, and they would have immediately come out with some sort of a discovery of it.

DRP: How do you think Walter Bleakney's discovery of it a little bit later fits in with that? He ran an enriched sample.
HU: Oh yes, he ran this with a Mass-Spectrometer and confirmed our results beautifully.

DRP: He took a sample from you?

HU: Oh yes, he took his sample from us, and showed that it was there. Well, this is pretty much the story of it. But today of course we would have done the Bleakney stunt immediately with exceedingly beautiful instrument which would automatically measure it, you know, overnight while we were sleeping. That's the kind of machines these young people have made. I was born too soon; I don't understand them at all. Well, it was very exciting, and then this led on to our work on the fractionation of the chemical isotopes of carbon, hydrogen, nitrogen; I guess we did oxygen to a little bit. But I dropped out of all of this.

DRP: Were you concerned with looking for tritium after you discovered deuterium?

HU: No, I wasn't concerned with this at all. Of course, it had to be made by radioactive processes, and I really have never worked in radioactivity at all. I've always been interested in other things.

DRP: Because Bleakney tried to look for tritium after he discovered deuterium.

HU: I think so, yes. He couldn't find it of course - it's radioactive. Well I often think about this, and we thought that heavy hydrogen would be useful scientifically to investigate chemical tracer techniques, and also it was the next complicated nucleus after hydrogen, after the proton, and would be useful in that way, but we had no idea that it would have any commercial importance at all, and now of course it is part of this terrib. bomb. It's used in Canada for producing power; I guess in the United Sta too to some extent. It's one of the ways of producing power. And it
just may be that fusion will work some day, and if so it is a source of power for a long period of time. The methods that are being used at the present time are really using tritium - I mean lithium - as a fuel. Lithium of course won't last as long as deuterium, and I am told that at that time it will certainly be possible to take over with pure fusion of deuterium. The commercial uses please me very much, and the military uses just scare me to death, that's the truth.

DRP: Have you any other recollections concerning your discovery of deuterium? Things that might not normally appear in the literature, for instance. You were talking a minute ago about various mistakes before the initial discovery. Would you like to expand at all on this? How do you think these mistakes were resolved in the end?

HU: Well, Aston repeated his work and found his mistake, and the discovery of this concentration of heavy hydrogen by electrolysis was made by Washburn in Washington, and then it was used by Lewis and by me to produce high concentrations of heavy water. The chemical mistake came out because of Washburn discovering that the isotopes of hydrogen were fractionated in electrolysis, and Lamb had used electrolysis as a way of purifying his stuff. Apparently it was connected with this.

Now, what this led on to with me was the development of chemical methods for separating isotopes. We then extended our calculations to show that hydrogen in equilibrium in water, the hydrogen would contain less deuterium than the water by about a factor of three. But experiments on electrolysis showed a factor of six or eight, and this meant that the kinetics of the thing was also fractionated, and of course having discovered this, then we went on to calculate methods for fractionating the other isotopes. My student, T. Ivan Taylor at Columbia University, has continued this very
effectively and is quarrelling with the Government, of course, as to who
can use this and who can't. I never patented any of my things, and the
reason that I didn't patent them is that I found it was a damn nuisance;
you'd have to work with the Patent Office, and work and work. I wanted
to be a scientist, not an engineer. I just didn't want to fool with this
Money was of secondary importance to me; I didn't care who used it. But
T. Ivan Taylor paid some attention to this, and immediately had some trou
with the Government about it.

Well, that I think was an interesting development: the fact that isotopi
substances have slightly different thermodynamic and chemical properties.
And of course we began to develop the use of Mass-Spectrometers in our
laboratory for this purpose, and these were the old Mass-Spectrometers wi
a little galvanometer sitting over there on the wall, and you looking at
it and shifting the voltage on the thing.

DRP: You started developing these?

HU: Yes. Well, I was only copying others.

DRP: This is when - about 1934 or 1935?

HU: Yes, somewhere around then.

DRP: Whose were you copying? Do you remember?

HU: Well, Bleakney at Princeton and Nier were developing these things, and I
had a nice effective physicist working with me by the name of Fox. What
was his first name? I can't think. He died in California since I've been
here and I went to the funeral. A very nice person, and he helped us out a
great deal. I owe much to him and to all these people who have helped me all these years.

But I think that ended our contact with Mass-Spectrometers, because I went on to Chicago. We continued to use Mass-Spectrometers to understand the fractionation of the isotopes of other elements, and in fact we developed next the thermometer, the isotope thermometer. We just found that the shells of animals laid down a hundred million years ago contained different ratios of the oxygen isotopes, and we could relate this to temperature; my post-doctoral Fellow in Florida, by the name of Cesare Emigliani, an Italian, has continued this and done a marvellous job on it. I don't know whether you know the name. Well, he went down in the Carribean and took samples as he went down, and when he got through he plotted a curve of temperature: every 30,000 years there was a dip for 4000,000 years, and there had been some ten or twelve ice ages in this time. We today are at the top of a curve - maximum temperature. The question is: which way will we go now? Will we go down and will we have another ice age? Will the activities of people make the earth get colder, so that we will have a dreadful ice age, or will they make it get hotter, so that we'll all wish to move nearer to the North Pole. I don't know. Or will it have no effect at all? This is what is worrying Emigliani. But he continued to use Mass-Spectrometers there.

DRP: When you were using them yourself, did you find they were a fairly reliable kind of instrument to use for research?

HU: They certainly weren't very reliable at that time, and they have become much more reliable; precision has gone up enormously. I don't know what our boys out here are getting on these isotope measurements they are making on materials at the moment, but they are enormously better and they've
become automatic, so that all sorts of tricks have been developed by these young people working on them. And of course Al Nier is, I think, or of the very best people in the field. And these others I mentioned; Sam Epstein keeps working on things like that up at Cal Tech, and Lowenstaurrn did some very good work with me while we were at Chicago, an then Emigliani has continued, and T. Ivan Taylor at Columbia has continue to work on this. But during the war we also got some chemical separation going. We developed very good methods for making heavy hydrogen, and this was done by Jerome Speebach. He did the most important work in connectio with it. And we also separated the boron isotopes. I can't think of the man's name who did that; he was there in the neighbourhood of Chicago and we continued separating isotopes there. Then I went on to other things besides isotopes; working on the abundance of the elements in the sun. I am in a very curious position. I published a paper giving the meteoric abundances. People generally supposed that there's a column of them there, that that is correct; and the people working on the sun get somewhat different values. What a job it is.

END
DRP: Perhaps we could begin by discussing your early career?

WB: Yes. I got my Bachelor's degree at a little college in the state of Washington called Whitman College. When I finished there I had more the idea of going into engineering, and I had the good fortune to get a fellowship at Harvard University, where I entered the Harvard Engineerin School. But being a liberal arts student up to that point, I had to go back and take a lot of undergraduate engineering courses in order to satisfy their requirements, and I spent the year there. That taste of engineering convinced me that I would prefer to be in physics, but I had no financial means and no way to go ahead there; although I talked to th people in the Physics Department, there didn't appear to be a way for me to finance it. So I went to the University of Minnesota where I got a teaching assistantship, as they call it, and I began my graduate work in physics.

DRP: Under whom was that, at Minnesota at that time?

WB: For the first couple of years it wasn't under any particular person, but the dominant experimental physicist in that department at that time was John T. Tate, who was well-known in the Physical Society and was editor of the 'Physical Review'. He had been a student of James Franck in Germany. In fact, he got his Doctor's degree under James Franck, I believe; I don't know what year that would have been. As a consequence he was interested in somewhat the same fields that James Franck was interested in, which included collision phenomena, kinetic theory, excitation of atoms and molecules by electron impact, and that kind of thing. People talked a lot in those days about the Franck-Hertz experiment, and so naturally I was influenced in the same direction.
Tate - you know he is no longer alive - was a wonderful teacher, but I
would say he was not a man with great imagination, and although his
students liked him very much, he left it pretty much up to them to
decide what they would do and how they would do it. They didn't get too
much help from him, which in many respects was good, because I know that
in most places the professors helped their students a great deal, you
know, and really spoon-fed them. But there we were on our own pretty
much, and it was sink or swim.

DRP: So you were free to choose your own work in fact?

WB: Yes, but always within the facilities available. So I tried for one whol
year an experiment which turned out to be a complete waste of time - I
got absolutely nothing out of it. This was after I'd been there for two
years and was ready to concentrate on thesis work. And of course during
this time I had read a great deal. I had been interested in the work of
K.T. Compton and the Mass-Spectrometry work of such people as Dempster,
Smyth, and Hogness; people who were doing work not so much on the
measurement of masses, but on the other aspects of what you could learn
from Mass-Spectrometry. And I was impressed by the fact that all of that
work seemed rather fuzzy; it was hard to draw conclusions from it. I
decided that if one could work at much lower pressure, things would be a
lot clearer. So I started out to do that. I built a little Mass-
Spectrometer; it was a kind of an odd thing, but it was shaped partly by
the fact that - Well, let me say that I think if I could claim to make
any contributions in those years at Minnesota and shortly after, it would
be that I went, I believe for the first time, to studies at very low
pressure. Secondary processes were practically eliminated, so that the
result you got you could be sure was the result of a single collision.
And in order to do that you had to make a Mass-Spectrometer which could
be baked out and you could get a high vacuum. Nobody had ever done that. All of the experiments, like those of Smyth and Dempster and Hogness, and even Aston, they never had a good vacuum I'm sure. Now for Aston's purposes it didn't matter so much, because he was interested in measuring the masses, and what happened in the source was of not so much interest to him. But to the people interested in the kinetics, this was of prime importance. And they knew that there were some doubly-charged ions, but it wasn't very clear whether that resulted from a single impact or from multiple impacts, or whatever. So that was the first thing then: to get to low pressure. But to go to low pressure, that meant that you'd have trouble getting enough intensity, and so I had the idea that I could improve the intensity by just simply making the slits longer and collecting over a longer path. But then when you do that, you no longer can use an iron magnet, because the fringing field is too great, so therefore I was driven to the use of a solenoid. But solenoids don't give a very high magnetic field, and therefore resolution would be poor, or else you had to have a giant solenoid which was not available to me. So I wound a solenoid, and the magnetic field that I could get was so low that I thought I wouldn't be able to bend the ions around in the usual 180 degrees, and so I just shot them straight up between two plates with a counter-balancing electric field. Now that had poor focussing properties, but it worked for the purpose that I was after; it worked surprisingly well just surprisingly well. So I think the low vacuum plus the type of source where you had an electron beam held by a magnetic field and pulling the ions out - that ion source was a change which has spread throughout the field.

DRP: That was an important breakthrough really.

WB: Yes. There are sort of two breakthroughs - one was the ion source, and
the other the high vacuum.

DRP: What pumps were you using on that first machine?

WB: Mercury diffusion pumps with liquid-air traps. The oil pump had not yet come into use - it did shortly after - and oil pumps themselves are not so good because you do get layers of - I don't know - monomolecular layers of something, that charge up and distort the field. That's still a trouble, even with the best equipment nowadays, but not as bad. So it was with great astonishment that Tate and I saw what had come out of this - these multiply-charged ions - so clearly. And we were even able to measure the electron energy necessary to produce them, so that the ionisation potential could be at least roughly determined. And those indicate quite clearly, in comparing them with spectroscopic data, that those multiply-charged ions had to occur as a single impact, which was hardly believable at first.

DRP: So that was your first experiment?

WB: Yes, that was my first experiment, and it spawned a lot of others which were not exactly, but somewhat, related to the Mass-Spectrometer. For example, Edward Condon, who was there for a year just about that time, and I (although I had finished my thesis on mercury vapour, I then stayed almost a year beyond that time to exploit a few other things) started with molecular hydrogen. Condon at that time was full of ideas about molecules and shortly before he had announced what became known as the Franck-Condon principle, which indicated something of what should happen in molecules. He made a prediction of what I should find in hydrogen, which was pretty well borne out, but it led also to some other things that he had not predicted; these ions that jump off of molecules with kinetic energy, for
example. If you bombard a hydrogen molecule, it's possible with a single electron impact to knock both electrons away and here are the two protons sitting there. Of course they spring apart with an energy which you—which even I can calculate. And lo and behold I found them.

DRP: On the same machine?

WB: On the same machine. In fact, I thought that the first paper on hydrogen to me was the most pleasing work of all, because it elucidated so many nice things. Well, we even found negative ions from some of these molecules and these became of quite a bit of interest to the physical chemists, and suggested to Lozier—Wallace Lozier—to build a special piece of equipment using the same kind of an ion source, but not bothering with mass separations.

DRP: To study the negative ions?

WB: Yes—and he could get much more intensity this way and so could determine the critical potentials; and he did some very nice work on that—it was suggested as a result of my first experiment. Then we went on to do some work with the rare gases, and that sort of sums up my work at Minnesota. There had not been any Mass-Spectrometry at Minnesota prior to that time. But the tradition grew with Nier, who did such magnificent work. You know, he was an undergraduate student when I was a graduate student and I had him—you know I was an assistant—I had him in my freshmen's laboratory.

DRP: So then you moved to Princeton?

WB: Then I went to Princeton. Well, I stayed at Minnesota one year past the PhD; let me say, past the time when I had the thesis all in order. Now I didn't actually take the degree; I was too anxious to go ahead and do
this other work. I spent about one extra year there, and then I got what was called a National Research Fellowship when I went to Princeton to work with K. T. Compton. By the time I got to Princeton, K. T. Compton had left. He had gone to MIT as the new President of MIT, and he was very nice; he said I could come to MIT and work with him. But he advised against it. He said the facilities there were not good, although he hoped they would be in the course of time, and he thought I would be better off if I just stayed in Princeton. I think he was probably right; and so then I continued to work there.

DRP: Did you take any machines from Minnesota to Princeton, or did you construct new ones - start again?

WB: I started again from scratch. And now I knew a little bit more about it, and I was able to build a little bit bigger and better solenoid so we could use the 180° method and so improve the focussing properties. I even improved the vacuum as well, because the instrument in Minnesota could only partially be baked out - it had a long tube with a kind of a brass plate on the end. But they had a good glassblower in Princeton, and I was able to get the whole thing sealed up and baked, and then this pushed the sensitivity way up enormously. It was just about that time I started work on some other molecules and continued some further work on hydrogen, when Harold Urey made his discovery of the deuterium isotope. Now when that happened of course that led to a lot of excitement. His first evidence for that isotope was not regarded by everybody as fool-proof. You know, it was a faint companion line taken on a grating with which you had to take a long exposure. This brought out a lot of ghosts, and a lot of spectroscopists said "If you expose long enough you can get anything".
DRP: It depends whose side you're on.

WB: Yes. And so Urey asked me if it would be possible to check his results on my Mass-Spectrometer.

DRP: By the way - as far as hydrogen was concerned, were you of the opinion, before Urey's discovery, that there should exist isotopes of hydrogen?

WB: No, I hadn't felt too much about that. I thought that Urey's arguments were very good. Because of the difference in the atomic weight as determined by Aston and Bainbridge, and the molecular weight as determined by the chemists - this discrepancy is what led Urey to seek the isotope. But the funny thing is that later on it turned out that that discrepancy was an error anyhow, and he was led to it for the wrong reason. The abundance of the isotope was not right to explain this discrepancy, and when both the Mass-Spectrometer values were improved and the chemical was improved -

DRP: So you would agree that Urey's first discovery was the result of an experimental error?

WB: Yes. But nevertheless he was very anxious to get confirmation, so he some enriched samples. He couldn't see anything on natural hydrogen. was through the enrichment by evaporating liquid hydrogen that he observed it in the first place. And so he brought some of that enriched sample down to Princeton and I ran it through the Mass-Spectrometer.

DRP: Straight away? Did you have to modify your machine?

WB: No, I was all set up perfectly.
DRP: That was quite a coincidence!

WB: I had already been studying some hydrogen for other purposes.

DRP: You were studying kinetics still?

WB: Yes. I had tried to make an improved measurement of the ionisation potential of molecular hydrogen. Everybody thought I wouldn't be able to do it for the following reason. When you bombard hydrogen - molecular hydrogen gas - and look at the mass spectrum, you always see some mass 3s, and mine was no exception. It's true the mass 3 was pretty weak, but it was there. Therefore how was I to distinguish between HD and \( \text{H}_3^+ \)? I said, well it should show up because the \( \text{H}_3^+ \) is the result of a secondary collision, and therefore its intensity should go with the square of the pressure, whereas the HD, if there is one, should go linearly with the pressure. So we put it through on that basis, and I just measured the ratio of the big \( \text{H}_2 \) which I took as a measure of the pressure, and then looked at the mass 3 peak, and took the ratio as a function of pressure, and plotted that out. Then it was clear - it stood out like a sore thumb - that there was an extra linear contribution.

DRP: Because you had a positive intercept?

WB: A positive intercept. It was quite a big intercept, and that encouraged me to even look at ordinary hydrogen to see if I could detect an intercept and sure enough I could. So had I had this knowledge, I could have looked for the isotope in the first place, but I thought that even if it was there it would be too weak, since I hadn't seen any evidence. On the other hand, I hadn't concentrated on this point. So after doing Urey's sample I went back and did some ordinary hydrogen, and I found a little intercept there that indicated an abundance of about one part in 35,000.
Now, you may ask why it was so weak, because we later came to know that it's much more abundant than that. Well, the reason again is a funny one. I was using electrolytic hydrogen, and it was later discovered that that impoverished it.

DPR: So you were fighting against all odds.

WB: Quite. Well, after I got these results, there was a meeting of the Physical Society in Boston, and so I put in a little extract that was the first confirmation of Urey's result. I remember that day very well, because people were very much interested in this question, and my paper was next to the last on the programme. You know how that is at meetings people begin to disappear, and the last man usually has nobody to talk to. Well this was doubly embarrassing to the last man on this particular occasion, because I was next to the last, and there was so much interest in this problem that everybody was there. Everybody was very attentive; it was so quiet you could hear a pin drop, and I gave my paper, whereupon everybody moved out. And this poor fellow who had to give the last paper he must have felt pretty embarrassed; I felt embarrassed for him, I know. But I think after that everybody was convinced, there was no question. And Urey, and also Hugh Taylor at Princeton—as soon as the method of electrolysis for concentrating isotopes was discovered, Hugh Taylor set up quite a big array of cells in Princeton, and so for a while he had the biggest source of deuterium in the world, and I did some experiments with him. He had a graduate student named Gould, and Gould attended to the chemical side and I analysed things on the Mass-Spectrometer. There was a lot of uncertainty in those days, you know, about what happened to this isotope, because people said that it could interchange. In fact, I believe that there was a sample of enriched water sent to Oliphant in England. I believe it was Oliphant. It was in the form of gas—hydrogen...
and they stored it over water. When they came to use it, it seemed to a lot weaker than they expected, and it was thought that it could interchange the light and heavy isotopes, and eventually disappear on you. This raised all kinds of questions: what happened if you had stopcock grease or any hydrocarbon around? You might lose everything you had. We did experiments on sealed-up bulbs that had been kept for a long time for about a month. One of these bulbs was lined on the inside with stopcock grease, and different kinds of stopcock grease. And with some we put water in the bulb, and would shake it every day, and after a month we would test it. We did all sorts of things like that, and we got no interchange at all.

But we did get interchange when you used certain catalysts and, you know Hugh Taylor was supposed to be a great expert. He was a great expert - one of the leading chemists on the subject of catalysis - and it was rather funny because he predicted that if we put in a mixture of almost pure DD and HH and absorbed them on two catalysts in particular (one was activated charcoal and the other one was - not being a chemist I don't even remember - one of those metallic things) - in one case he predicted that if we absorbed it on there, and then pumped it off and analysed it we would find that there would be HD molecules formed. We tried this and his prediction was completely wrong. Then he predicted that we would get a like result on this other catalyst, and we did that, and he was wrong again. We got complete equilibration. I don't know, it was a lot of fun then because the experiments could be done fairly rapidly; you didn't work for two or three years on a complicated system to get a result, so it was quite an interesting period. And Urey himself was so keen about analysis by means of the Mass-Spectrometer that he was very anxious to get a Mass-Spectrometer himself, which he eventually did. Lozier came to Princeton also, and after he'd finished his fellowship
there, went to work for Urey building a Mass-Spectrometer, and then he went off to do other work. But it was a long time - years - before the people at Columbia succeeded in making that Mass-Spectrometer work properly. I don't know, they had all kinds of difficulties. But apparently it was not easy to make those things work. I know one commercial company that hired a physicist with a PhD to build them a Mass-Spectrometer, and he never got it to work. It just never worked - he never got a bean. It's incredible.

DRP: So, after your confirmation of Urey's results on deuterium, did you think there might be another isotope?

WB: Yes. In fact, we thought at one time that we had found it; not in hydrogen, not in natural hydrogen, but in a discharge tube that was set up by other and ran at - I don't remember - many kilovolts. Then they took samples out and we analysed it in our Mass-Spectrometer, and again using the study as a function of pressure, tried to find a linear component of mass 4 or higher. We thought we found a positive result, and that apparently was a spurious effect. We published that, to our regret. After we realised that it must be wrong, we tried to do further experiments to understand why. We thought that what we found was that there must have been a hydrocarbon gas out of which you could knock off as a unit, and therefore it would go directly with the pressure, something of mass higher than 4. That seemed to be the explanation, but it was a very, very faint thing and so it was pretty hard to pin down.

DRP: Was anybody else looking for tritium?

WB: Yes.

DRP: Was Urey looking for tritium at that time?
WB: Yes.

DRP: But not on the Mass-Spectrometer though; using ordinary spectroscopic methods?

WB: Well he possibly might have, I don't recall now. He may have brought samples to us, but he at least tried chemical methods to see if there was some enrichment that would show it up.

DRP: Was Aston at this time concerned with the isotopes of hydrogen? Were you in correspondence with him?

WB: No, I was not. He was of course getting along in years by then and he pretty much dropped out of the picture, I think partly because he never really improved his apparatus and new techniques were coming into use, but he was trying to use his old method still - which was magnificent in its day.

DRP: One could argue that he built a good machine to begin with and then couldn't bear to alter it, I suppose. So you stayed at Princeton; for how much of that time were you concerned with Mass-Spectrometry?

WB: Up until World War II - a couple of years into World War II - then I was dragged off into military research of one form or another. I made a couple of trips - one was to England, that was the only time I was ever in England; I spent a couple of months there in 1942.

DRP: Where did you go? Were you concerned with the Manhattan Project?

WB: No, I was concerned mainly with civil defence. Nominaly it was civil
defence, but actually the military people were interested in military
offence and defence. We studied the effects of impact and explosion on
damage to structures, and I spent a lot of time at Princes Risborough,
where there was a station devoted to this sort of thing. There was a lot
of dirty work, proving ground work, and we did a lot of model work at
Princeton on explosions. In fact, we did the experimental work at Prince
with little one-pound charges of TNT, studying the effect above ground
that led to the decision at what heights to burst the atom-bomb. Von
Noyman was the theoretical man who was so much interested in this, and we
did this work at his instigation.

DRP: Just as a matter of interest, did you ever meet Barnes Wallis?

WB: No, I don't think I ever crossed paths with him. But this work led into
study of shock waves and it was an on-going thing after the war. I'd
started this shock tube work, which is now off in a totally different
direction, and I suppose that perhaps I made better and more contributions
in that field than I did in Mass-Spectrometry. Certainly I have a lot more
students, and now there are shock tubes all over the country. We built
the first one at Princeton, and that field has grown enormously. I just
had a letter the other day; they're going to have an International Shock
Tube Conference at Stanford University next year, and they want me to be
on some kind of an advisory committee, but I begged off - I've retired now

DRP: You'd rather play golf?

WB: Yes! But to go back to the Mass-Spectrometry field and one or two aspects
of things I was interested in. I got the idea at one time of what I called
a cross-field Mass-Spectrometer. In fact we built a little one, and it
was Hipple who was my graduate student who worked on that. That was a very
interesting instrument. It had rather unusual properties, and we got a
patent on that: Princeton University had a working arrangement with the company whose business was the administration of patents, and they took it, and their attorneys and what-not worked it through to get the patent on a cross-field Mass-Spectrometer. Well, I didn't think that anybody would ever go to that complication - to make a Mass-Spectrometer commercially - and therefore I thought the patent was only of academic interest, and I was right because nothing happened after they got the patent. You know, the life of a patent is about 17 years in this country I believe. Well, for 15 years nothing happened. Then, all of a sudden, this Research Corporation, organised by a man named Cottrell, succeeded in licensing this patent to a company in Pasadena - Consolidated Electrodynamics, they called it. They did a remarkable engineering job on it and produced a little instrument that was just a little tiny thing and I think the radius of curvature or length of path of the ions was on a matter of a few centimetres; yet it had the most surprising resolving power and intensity. They marketed that things; I don’t know where they used it, I guess chemical companies, manufacturers.

DRP: What year would this be? In the 1950s?

WB: 1955 - 57, in that area.

DRP: Because Westinghouse was already manufacturing larger machines.

BL: Larger machines, yes. Apparently so - I never even saw one of these things. I saw their papers where it was written up. And I don’t know where they used it, but they must have sold some because Princeton University and Research Corporation - I forget whether together they collected $80,000, or whether that was Princeton’s share, in royalties. But this only lasted, you see, for a year or a year-and-a-half, and then the patent ran out. So it was 15 years ahead of its time. Now I don’t
know whether they still make them or not.

So the cross-field instrument looked very interesting, and Hippie to build a large one at the Bureau of Standards. I think that would been successful, except that - well - there were organisational difficulties at the Bureau of Standards about that time. And Condo I remember he got into a little trouble with the security people an what-not, and then Hippie left, and so the whole thing died. Nobody would do it now again because Linc Smith, who was also a student of has now produced a magnificent instrument. It's too bad that there a way of supporting that work, which I understand is now suffering if the fact that there is no way to support it. He was making measurements When I left Princeton he already had measurements that were good to about a very few parts in $10^9$ - nearly one part in $10^9$ for the mass the proton, relative of course to the oxygen 16, which amounts to, energy-wise, about one electron volt. It is so good that he has to account of the chemical binding energy - you know, when the hydrogen bound, it loses mass. Now you can turn this around and say he could measure the chemical binding energy, except that it is of the order magnitude of errors. So the cross-field I think is out for high mass determinations, and has been superceded by a still better method.

There was one other little contribution that I made, and I was led to by concern over a question which troubled many people who used Mass-Spectrometers, and that is if you want to use the instrument as a method of analysis to determine quantitatively the composition of something other, then you always ask yourself,"Does the instrument itself discriminate in any way?". We always suspected that it did to some extent, and it was quite clear that the discrimination was very large in some cases. If the mass differences were very large, where the peaks wer
apart, then it could be demonstrated that there was some discrimination and in these commercial instruments this was especially true. And I think it was sort of common practice to try to calibrate the known samples and take account of this effect.

Well, it was a consideration of this question, plus the fact that in reading the old papers of Aston and Dempster and Bainbridge, I noticed that each man made a calculation and came out with a formula of how the mass he observed depended on the fields that he applied. And I noticed that they all came out to be the same; that is to say, the function was the same. The mass always varied directly as the electric field and inversely as the magnetic, or vice versa. And I thought, when I came to make my Mass-Spectrometer at the University of Minnesota, that there must be something more general about this. So I made up what I called a theorem, which in the beginning I wasn't able to prove, but I was convinced it was a good theorem, which indicated how the mass should vary with the fields you apply. I showed this to Condon and he was quite interested and pleased with the idea. In fact, he showed me how to prove it. Well, after that I found a much easier way to prove it; in fact it is so simple that you can do it in just a few lines. I couldn't find that anybody had done this before; it seemed so simple that I couldn't believe that it wasn't already known, so one time when I was writing a sort of review article for what was called the American Physics Teacher I guess it's in that list somewhere -

DRP: What would the date of it be? In the 1930s still? In Minnesota?

WB: No, it would be after then, in Princeton. It was a review article - no a review article in the sense of being highly technical or to do with research, but more for elementary physics students.
DRP: I have a note of it here. It was the American Physical Teacher Review: 'The Mass-Spectrograph and its uses'.

WB: That's right. So I never had the courage to publish this little idea because I thought somebody would jump on me, you know, because I hadn't made a proper search of the literature. And so when I wrote this review article, I apologetically included this as a little appendix at the very end. Well, it's been rather surprising that of all the Mass-Spectrometists that there were, few had apparently thought about this in this general sort of way, and that little theorem had a big influence. About ten years after it was published people began to quote it and not it, and I even had a letter from the leading German Mass-Spectrometist.

DRP: Jo Mattauch?

WB: Mattauch, yes. I had a letter asking, "What's this about?". He apparently had been hearing about it from others, and he couldn't track it down. And so he asked me for a reprint and it turned out I didn't have a reprint any longer, so I made a Xerox copy and sent it to him.

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Well, I'll get this article and just show you what a ridiculously simple thing it is, and yet now it's quoted by quite a few people. When I went on a visit to see Nier one time - it was only a few years ago - I went down in his laboratory where I knew several people. Professor Johnson was there and several graduate students, and Nier introduced me and he said, "You know, the man with the theorem." Apparently Bleakney's Theorem is well known now among the students there.

DRP: During the 1930s when you were working on Mass-Spectrometry, did you ever consider that it would be an important tool for analysis to be used, fo
instance, by chemists? Or did you think that it was just a physicist's piece of equipment to be used only for the type of research which you were engaged in?

WB: I didn't realise it at the beginning, but I came to realise it after I was at Princeton, and Lincoln Smith made the first analysis of - he was then a student - he made the first analysis of a hydrocarbon - methane, which is the simplest of all. And then I began to see that it had tremendous possibilities. And in fact there was - we were always looking for money, of course, to spend on research, and I don't recall how it got started - but there was an idea, I believe it was through this Research Corporation that administered the patent, that the oil companies might be interested. So there was a big operator from Texas who came to see me; I think his name was Block, and he walked in with high boots and his Tex hat, and he was intrigued and thought that possibly this instrument might be used to locate oil. He said that you could make all kinds of seismic measurements which told you about the structures, but they didn't tell you whether there was any oil there or not; if you could detect some hydrocarbons from samples of earth, then that would be much better. So he went home and sent me mason jars full of earth, and I was supposed to look for hydrocarbons in those. Well, that is a difficult thing, and the results were I think rather inconclusive; but nevertheless it looked like you might, if you followed it up - you know, there were clues there. And so we tried to get several of the oil companies to divvy in a little money to support this research, and nothing very much happened. We got a little bit of money, and then I realised that these companies were very jealous of each other, and were starting research programmes of their own in their own laboratories. I never did find out how well they made out. But it soon became evident that this was a growing field of analysis, a getting a catalogue of all the various molecules and so on was going to
be important. But about that time I was really out of it.

DRP: Do you think the development of the machine itself was slow? Do you think that it was fraught with mistakes all the way along? How have you seen its progress? There have been fairly few people concerned, relatively with Mass-Spectrometry, at least up until the war.

WB: There isn't any doubt that if one had been wise enough to foresee all the possibilities, one would have known that it was important. If one could have put more manpower into it, like they do nowadays, and made a big project out of it, it could have been done faster and better and so on. It was done on a shoestring by physicists just experimenting, and on that basis I think you wouldn't expect it to grow any faster, because there are certain technologies that were not available. This has been important. How to secure a real good vacuum didn't develop too fast.

DRP: It's interesting to go back into the 1920s when Aston first categorically maintained that there were isotopes of practically every substance and not just radioactive ones. At that time ion molecule reactions were regarded as getting in the way rather than being something to study for their own inherent interest. Isotopes were the OK subject to study, so progress was concerned with elucidation of all the various isotopes, but ion molecule reaction studies for their own inherent interest were sort of swept to one side. How you you think one could account for that? Because isotopes are physics and ion molecule reactions are chemical physics?

WB: I don't know the answer to that.

DRP: It was just an accident, would you think?
WB: It was an accident, yes. When I was a graduate student, the big interest in physics was spectroscopy, ordinary optical spectroscopy, and there weren't too many people interested in these things. I did have (in those early years at Princeton) an awful lot of requests from everywhere for analyses of samples from people trying to separate something by diffusion or - by what they used to separate isotopes - by thermal diffusion; thermal diffusion, where you had thermal gradients and not just ordinary straight diffusion. When that method was being developed people would try things like that on say neon, but then they couldn't find out what the result was. And they would bottle it up and send it to me and ask if I could look at it. And I did more of this than I suppose I should have, because I was easy-going, I was a soft touch. A lot of these experiments I was pretty sure didn't amount to much, but I tried to do as many as I could.

DRP: Were you ever tempted to go into industry at all?

WB: Only once, and that was after Condon left the Bureau of Standards and became Director of Research at Westinghouse. He offered me a job there. He's quite a convincing talker and he had me considering it seriously, but in the end I decided not to go.

DRP: For what reason? Did you prefer to work academically?

WB: Yes. I had a couple of other offers at that time, but Princeton University treated me so well - it was a magnificent place to work. You were absolutely on your own, and the facilities were good. I'm sorry to say a lot of things can no longer be done. For example, no doors were ever locked there in those early years. The stockroom was wide open and you could just walk in and take what you wanted. Anytime, twenty-four hour
a day, you could work all night if you wanted to; Saturdays, Sundays, you could do anything and nobody bothered. Nowadays of course they can tolerate that, they'd be robbed blind, and so they have to have all kinds of rules and restrictions and so on. So it was a magnificent place to work, and although when I went there the prospects of advancement didn't look very good, because the department was pretty well filled with relatively young men and offhand it didn't look very promising for me, it was during the depression when jobs were not very abundant, and so I was pretty happy to have anything. In the end it worked out all right; they never gave me a good excuse to leave. I was there you know for thirty-nine years.
DRP: Perhaps we could begin by talking about some of the early episodes in your career.

KTB: Well, I was a member of a family where two uncles were in engineering, and that may have headed me in the direction of engineering. And then I later changed to physics, but I went to Massachusetts Institute of Technology, and they had a five-year course there in which one spent half one's time with a commercial company and half at MIT. I elected the General Electric Company option, which meant we'd work at Linn or Pittsfield or Skennectady. Finally I was fortunate enough to get to Skennectady and meet some of the people there in the research laboratory, and I decided that research was much more fun than electrical engineering. So I talked to the people there at the lab, particularly Howard Jones who was a chemist, and Ernest Charlé who was a chemist, and Hull and Duchmann who were physicists. Whitney was a chemist in the laboratory, Coolidge was there, and they thought that Princeton was a great place to go to, where Carl Compton was a consultant for them. So a close friend of mine, Tom Killian, and I went to Princeton. In the summer I would go back to General Electric Company and work there some of the time; two of the summers I went to Europe. Anyhow, while I was at the GE I got interested in photoelectric work, and through the photoelectric work got interested in the caesium and rubidium and alkaline metals, and it was apparent that one of those was missing - element 87 was missing - and so at Princeton I selected a thesis topic myself. I decided I would try to find element 87, and if it did exist in the earth's crust, it shouldn't be too hard to find. The ores they used for getting their caesium was from all over the world, but mainly from Maine, where the ores have all the known alkali metals. And so I tried all sorts of tricks: fractional crystallization, solubility things, solubility in organic salts too, and then analysed those things on a 180° type spectrometer, the simplest type.
DRP: That you built?

KTB: Yes.

DRP: Would you like to describe the building of that?

KTB: Yes. This was pretty much following the Classen's Beta-ray spectrometer, that the ions were thermally emitted from a hot filament coated with a powder of the separated stuff, or in some cases simply using one of the ores of some of the alkali metals from Maine which had everything in it: lithium, sodium, titanium, rubidium, caesium, and hopefully it might have some element 87. And you could, by positive ray analysis or by positive ion analysis, push the sensitivity so that, say, one part in $10^7$ or better could be determined. Nothing like that showed up. There were peculiar things which did show up, such as molecular sodium, an ion at 46. Sodium had a low work function and was ionisable thermally, so that got me interested in that type of Mass-Spectroscopy. And then I was going to get my degree in June 1929, and Compton thought it would be a good idea to have some post-doctoral research work, so I applied for a National Research Council Fellowship. In the process I went through a lot of possible problems, and one was trying to check relativity using an electron deflection method. Another was to design a new type of Mass-Spectrometer. Anyhow, I decided that was probably the more interesting thing and more likely to give some positive results, and so I went ahead on that basis, and got a National Research Council Fellowship. I looked around at three places, and the Bartol Research Foundation at Swathmore, Pennsylvania, seemed to have the best facilities and would be a nice place to work. The other two places - people who had been there had mean things to say about them. I went to the Bartol and was there two years as a National Research Council Fellow, and then continued two years as a Bartol Foundation Research Fellow.
DRP: And during this time you were building an instrument?

KTB: Yes, during that time I built a Mass-Spectrograph really - though Aston objected to the term and wrote a letter to 'Nature' saying that there was only one type of Mass-Spectrograph, and that was his, with his sequence of fields. This was essentially a sequence of opposed electric and magnetic fields - a beam velocity selector followed by magnetic 180° focussing.

DRP: Did any difficulties crop up during construction? This was quite early on in the building of these machines; would you like to talk a bit about problems involved?

KTB: Well, I've been fortunate in having had some experience at the General Electric Company laboratory. My friend Tom Killian was working with Dr. Langmuir. One summer I worked with Ernest Charlton when he did the work on caesium, oxygen and silver photo-cell, and another summer I worked with Duchmann. And Duchmann, Langmuir and Hull, their vacuum technique was the best of anybody else's. You know, Langmuir developed the mercury diffusion pump and Duchmann was an authority on high vacuum and measurement methods, and on, so that this was very useful as regards going into anything requiring equipment of that sort. The Bartol Foundation was well-equipped, had a good shop, a good glassblower, and so on, so that went very well and there were no great difficulties there. And they had sufficient money at the time - could buy only a desk computer today, I guess, for the same amount of money but it was enough to do things, and build a large magnet, and get the high voltage equipment - electrometers and things like that - that you needed. So that went fairly well.

Then I got interested in the equivalents of mass and energy, and so that was one of the first things I worked on, I think in 1932. It was just after Cockcroft and Walton's work on their lithium 7 plus proton went to alpha...
particle and lithium 6 plus. Every hydrogen went to two alpha particles. So I concentrated on the light elements - deuterium, light hydrogen, helium, lithium, boron, and so on. The heavy hydrogen thing broke at that time, it was important to get the mass of it as accurately as it could be done, that Professor Lewis at the University of California sent me a few drops heavy water, and Urey sent me some of his stuff which was concentrated 1 in 300, something like that. Well, a year later, Lewis' material was concentrated to 40% or 50% deuterium, and with that it was possible to compare deuterium with the hydrogen 1 molecule, and compare deuterium with helium. Then the next step, of course, was to get the lithium, and then you had the whole there, as far as getting an experimental check of the equivalence of mass and energy. At the same time, Dr. Von Hevesey, who died a few years ago, was in Germany. He was very kind in supplying things. He and Hertz were separating neon, and I got some of their neon, so that you could measure the mass beyond 21, which you couldn't get at otherwise. Normally it was hard to get it with such low abundance, but they'd enriched neon at 22 to the intensity as neon 20 with a factor of 10, and so on, then 21 came up to it. But people were very kind in supplying rare isotopes which you could work in the normal limits; and then I guess the heavy hydrogen was 1931 or 32.

DRP: Urey discovered it in 1932. What was your opinion before this discovery? you think that hydrogen might have another isotope, a heavy isotope?

KTB: There had been some talk earlier by J. J. Thompson of a hydrogen 3, even if his first parabola experiments he pushed around. With that he thought the might be, but he was very careful, as you know, until he had complete proof. There never was any proof, and it wasn't there at the time. And certainly if you read his papers on neon 22, again he was ultra-conservative in his opinion, although he had pretty good evidence from his parabola pictures.
DRP: Which was Aston's eminence, really.

KTB: Yes. And then he had the suspicion that neon, which had I suppose an atomic weight of 20.2, must have about 10% of 22 in it. And he got a line in his parabola equipment showing there was something of mass 22, but it might have been $\frac{Ne}{Lo}H_2$, that sort of thing - which he certainly was tro with, and Aston was too. So, as I remember, he asked Aston to see what h could do towards concentrating this. So Aston tried sort of fractional distillation, I think, and then absorption on charcoal, and evaporation, got some small shift in the increased intensity of the 22. Hertz I think started about the same time - his counter-diffusion method in mercury vap and so on. Well, that was really what got Aston started, I think, as he assisting Thompson, was he not? That's an interesting bit of history: A had devised his thing, I guess with R. H. Fowler's help, and Thompson did believe it at that time. Wasn't there a paper in the Royal Society in wh Thompson attacks Aston's data, saying the damn thing won't work?

DRP: Did you know Aston personally?

KTB: Yes.

DRP: Did he come to see you?

KTB: No. The situation there was that after two years on a National Research Council Fellowship and two years as a Bartol Research Fellow, I applied for a Guggenheim Scholarship to go to the Cavendish, and I got it. The thing awarded and I got Rutherford's OK to come and went over there about the middle of July 1933. But prior to that time there had been a Physical Society meeting in Chicago, and I met Aston at that time, and then Cockroft was on this side at the same time. I met him at the Bartol; he came through and saw him later. And I guess I saw Aston again at some eclipse up in Southern
DRP: Yes, he was keen on photographing eclipses, wasn't he?

KTB: Yes. He was with some team, and my wife and I drove up, and like everybody else we missed it because of clouds over the particular site there. Some people went dashing across the countryside, that sort of thing. But that's when I met him, and then of course I had some correspondence. Then I got over to England about the end of July. Cockcroft had very kindly offered my wife and me his house until he returned later - he was touring the United States in a very thorough fashion - and so we went and lived there until he returned. Then we went to a place - I forget the name of the road there - in Cambridge. It happened to be the place where J. J. Thompson had lived when he was a young man, and we took lodgings there. And when our son arrived, living in lodgings was too much, and so we got a house in Grantchester Road.

Well, I went to the Cavendish and brought over my own equipment, which Rutherford helped get through without paying the full price for the equipment. And he got a high-voltage power supply which amazed me; the Cavendish used special rectifier tubes which one of the British manufacturing firms made especially for them, and this thing came through in two weeks costing essentially nothing, compared with the American equivalent which you could get off the shelf from some place three months later. When I saw Cockcroft at the Bart, I'd built up a coincidence outfit and a Geiger counter arrangement. He asked me if I'd bring that over and I said "sure", and also American vacuum tubes which were a shilling apiece at the time. Well, the British equivalents were a pound, so I had a suitcase full of vacuum tubes and Geiger counters, which they didn't have. Of all things - I mean with Geiger having worked there and invented the point counter, and later with Mulligan, the wire counter, they didn't have any there.

And so I brought this thing over for Cockcroft's use, and the rascal didn't use it, you see, until after artificial radioactivity was discovered, and th
was December 1934 I guess. He suddenly remembered that I had this thing and borrowed it for a lecture demonstration. As soon as they'd heard about Curie-Joliot, then Rutherford, Chadwick and Ellis got together and prepared a polonium source and went to work on it; maybe it was Radium C, but I think it was a polonium source. They went to work on boron, nitrogen, aluminium and at the same time Rutherford and Oliphant, and Cockroft and Walton thought of producing carbon. Let's see, they were going to bombard boron I guess with protons and get 12 and 11, and they did immediately. I mean that same day everybody came out with this radioactivity stuff. Then about a week later, two weeks later, Cockroft reported on his experiments using high energy protons, and borrowed the machine to demonstrate radioactivity - to bombbar the stuff when someone ran over with it - the half-life, whatever it was, ten minutes or something, for the particular thing they were playing with the time.

Well, I was up in the attic section of the Cavendish where Gott was, who was working with Wilson, and - let's see - Dee was working with Cockroft and Walton, Gott was working on thunderstorm ideas, and that was very pleasant. C. T. R. Wilson would come up almost daily and drop in and say hello; I was a great admirer of his. I worked up there, but not successfully. I had some trouble with the charging up of the plates and I didn't lick that before I had to leave. I think Rutherford felt that it was sort of ambitious of me to try to get set up in this, a year, fifteen months, ahead, but he was willing to support it, and I was grateful to him for that.

Well, after that I had the opportunity to go several places here in the United States. I thought that Harvard was a better place for research than the other two places, and so in the spring I spent most of my time designing equipment to use here - another type of spectrograph with $120^\circ$ electrical focussing and $60^\circ$ magnetic focussing. I made up all the drawings there, sent them here to Harvard to friends of mine who saw them through the shop
and so on. When I got back in September I had a lot of equipment already put together here, so I didn't lose much time in getting back into business with one which did work right.

I took an oath at that time that I'd never use oil pumps again, so I had mercury pumps and liquid-air traps and all that nuisance, but still it did the job perfectly well. So I continued in measuring masses at the Bartol. I'd started off in measuring relative abundances, and this had got me in conflict with Aston because he'd had a lot of hydrogen in his discharge to.

There were a lot of things like lead; lead is 204-6-7-8, and I think he had hints of something at 205, 209, in mercury the same way, and that forms hydrides easily, and zinc and germanium and others. So I thought I'd look at these independently, which I did here you know, and I couldn't find the hydride, but I could make them if I had enough hydrogen in the discharge to. And then I switched from that to mass-measurements, particularly the light elements which hadn't been done, or where there was some question of the correctness of the value, or where a more accurate value was needed because there was so much disintegration - low-energy disintegration work going on with the Van de Graaf machines and low-energy cyclotrons.

I concentrated on that until 1940, and then of course the war was under way (at least, the phoney war was under way), and so in October - no, I think in September 1940, Cockroft, Colonel Wallis, E. G. Bowen and Sir Henry Tizard came over on a mission to get the microwave radar started in long-range navigation, and I guess Wallis was also interested in anti-aircraft gun production in the United States, and so on. Cockroft cabled me from Halifax on the way - I was up in Hampshire - and asked me to meet him at the rail station here in Boston. I met him there, and he said that he was coming back, and would I arrange for him to go through the Watertown Arsenal and talk to Bridgeman and the others. He came back late September I think.
Then in early October things were boiling here as regards the National Defence Research Council, and so on, and having been closely associated with Carl Compton and Van Buche at MIT, they asked me—no, it was Lawrence who saw me at first. Lawrence came to Harvard; he'd been here several times because we were building a cyclotron, and I'd known him a good many years. He suggested taking a walk around Cambridge, and once we'd started on the walk, he told me about radar. He was recruiting for starting the radiative laboratory. Then within a week Compton called and said, "How about going to Washington?", and I went down to Washington and saw the first radar in action, and it was quite a thrill. They had a research laboratory, and you could see the planes coming into the field, and they had quite good range display. I think they could do something like 40 or 50 miles a time, and they had a funny double sweep; the plane would go from left to right, then go into a double sweep, and then back right-left until it landed, and you could look out the window and see the damn thing.

So I was teaching two courses at the time, and I was meant to help locate a place to put up the microwave laboratory. A place in Washington, of course was suggested, and a building someplace in the vicinity of New York; final we decided to start at MIT. As Compton had control of the place, he could make room available, and he was the Head of the Physics section of the NDR. So I spent a lot of time at the Tech looking over the laboratories to decide where we could go. Ultimately it meant building a lot of new buildings. We started out in rooms like this one; the next one belonged to somebody else so you couldn't have it; the next one you could have; and so very shortly they started to build new buildings. And that November there was a nuclear physics meeting at MIT, about November 1st, and a lot of those people never went home again; they were just recruited then to join this radiation laboratory to work on radar. Now the impetus for this thing came from E. G. Bowen and Tizard, Bowen being an experienced man in the microwave field and on the development of the AI and Coastal Command search radar, a
so on. I think he stayed over on this side, and I think Fowler and Darwi at various times were the representatives of the British scientific mission in Washington. So the radiation lab started really in November 1940, and things were going pretty well by January.

Then it was realised that there ought to be better liaison with the British because they were so far ahead of us, and the microwave thing was their big thing. I was selected to go over at the technical level; February 1st I was meant to go — no, it was the end of February. So in February I spent my time touring around the concerns in the United States working on radar: Bell Labs, Sperry, General Electric Company, Westinghouse, and anybody who had anything in the field, to get their latest ideas and find out where they were, and then I went to the Government Laboratories in Washington.

Early in March I went over for two months to exchange this information with the British group, and we got information back by however they sent stuff: by cable, or in many cases by military cruisers, destroyers, etc. I had quite an exciting, interesting time; I saw Cockroft again, and I saw Aston. I guess about the first day I was there there was meeting of the Maud Committee at the Royal Society building; there was also another scientific meeting of some sort where Aston was present, so I had a chance to see him again, and then I went upstairs to this other meeting. I didn't get to Cambridge. I wanted to get to Cambridge, because although I wasn't in on the uranium development this side, I was the first Fellow in nuclear physics over there and the first scientific representative from the United States at least at the technical level, because Wilson and Conant had gone over to set up a mission and embassy. Fred Hobday and Caroll Wilson had gone over to set up after some agreement, I suppose between Hopkins and Rutherford, that they would have this exchange, and they worked fast and started people going over. I went over with Warren Weaver, who was on the mathematics panel this side — computers and gun directors.
I was radar, but the Signal Corps over here found I was going and they gave me and gave me a list of questions they wanted the answers to. I thought they were all pretty damn delicate questions, but it was amazing the cooperation I got - I suppose that Churchill had told people lower down, "B gosh, we're going to have this collaboration and you'd better play the game right" - but they were very good about it. I went to some place, some signal laboratory, which was amazing. It was a huge estate with a fence around it with all sorts of handsome-looking stags roaming the grounds, rooms one-fourth the size of this, with two steps up, two steps down, and there's even a research laboratory in this - not a castle, but a manor of some sort. Well, they were very co-operative, and I got all the information that the Signal Corps wanted; things like the ground-to-air, tank-to-air, air-to-tank, tank-to-troops frequencies and so on, which I thought was very hot stuff. Well, apparently the British had more sense than I did, because they simply gave the stuff in the clear - to hell with codes and secret wavelengths. By the time anyone could act on the information, their boys could act first. But there were a lot of reports they wanted, and so on. Most of the time was spent going round the different radar laboratories seeing what was going on there along the south coast, Swanage, Christchurch. I remember there was at Watchet a searchlight-director laboratory where Kempton was - I went there. And then at Bristol I think Ellis was working on proximity fuses and TR boxes, and that sort of thing. Then of course Swanage and Christchurch laboratories.

DRP: So for all the wartime period you were concerned really with radar?

KTB: Up until June-July 1943. In June Durbridge asked if I would go down to Curacao to the refineries which were supplying a lot of the aviation gas, and they were worried that German submarines could come and navigate offshore there and not be spotted, rise, and raise hell for the refineries. Durbridge
asked if I'd go down to Curacao and look over the situation to make recommendations for coastal defence radar: where you'd put it, what the problems were, and so on. And so I went down to New York and got shots for yellow fever and so on.

In the meantime, Oppenheimer suggested that I come down to Washington and see him, with the possibility of going to some mysterious project out Wes. So I went down to see Oppenheimer with Bob Barker, and he said, "Well, come on out and see this place", so I went out about the first week of July, and then learned what it was all about. I decided I would go out there and Bob Barker had already decided to go. I think the point was that by June 1943 we - well, you certainly had your ships well supplied with almost hand-made sets. The anti-submarine things they put on destroyers - we had to get them out in a hurry, and by June 1943 our stuff was pretty well in production. The GL3, which did such work with the V1s, was completed. We had good air interception. People were beginning to get odd things which were not very basic, like special radars to get mortar shells, and so on, so you could tell where the shell was fired from, and coastal defence radars, which we didn't do a thing of the past.

So I guess the one big thing left was the air early-warning system. This was something which I was in charge of for a while. It had been an idea of Oliver's; using huge phased antennae which would give you enormous range horizontal, so you could get high precision in direction; but vertically I was somewhat of a fan. I remember going to Florida and flying along the coast to find a hill - the highest hills are 300 feet high. We wanted to get a hill so we could put one there and try it out, and we located an orange grove which we took over. And we took our carrier-type - ESM-type and we put that on a hill in Orlando, and it messed up a mock raid on Orlando Air Base, and so we got them before they really turned to come to the air base miles and miles away. But that wasn't in time for the invas...
the other thing was Oliver's scheme, which was just too late, I think, to be of much use, and that was something you put on the wings of aircraft, with phased antennae which would give you very high precision. But already you had high precision with this thing called the Hrs, a 3-centimetre ra which gave you a beautiful map of railroad tracks and streets and everyt
For any blind bombing and so on it was the answer, and you didn't need t other huge thing. Anyhow, Barker convinced me that all the radars which were basic types had been completed and were in operation and in quantum production. Anything anyone was going to do from then on probably would get into the war, and the ones we had could be altered (as indeed they w so that they would be applied to other jobs. So I took off for Los Alam and never got to see the Dutch Indies. I was out in Los Alamos almost until the end of November 1945; I finished writing my volume of the ser which came out on Los Alamos and was limited to an edition of ten copies. When I got that thing together, I took off and went to Berkeley.

DRP: I would be interested in hearing some of the details of your work at Los Alamos.

KTB: I went out to Los Alamos in July, started work in August, and then my family came out in September. My job there was - well, I started off wit a lot of funny things, but the main job was to test the first bomb: to s up a test site, set up the instruments, and find out what the yield was. If it didn't yield what had happened we had to find what was wrong - some poison or something we hadn't considered, or maybe all the errors in the individual experiments happened to be minus instead of plus, plus and min you know. So we had an enormous number of individual experiments which went into the things. The Hiroshima type where you mechanically bring together the uranium - fire a chunk into another chunk - everyone felt th' would work, and it was not going to be tested. I think the first one
certainly was a Hiroshima bomb. With the second type, the implosion type, nobody had an awful lot of confidence. And it was felt that you should test it to see whether it did work as planned; there wasn't as much leeway, and so on. So over a year ahead of the test - I think it was around February 1944 - I guess I was partly with Kistie and partly with Parsons on the Hiroshima bomb, we did tests on that. You know, radar measurements of the velocity of ejection of the chunk of stuff and that sort of thing, how we could a gun could do in throwing the stuff around. In February Oppenheimer asked if I would take over the responsibility of the test - the plan, and instruments, and finding a location, and so on. I had some people there and I recruited others, and I was fortunate in having Phil Moon. The British group came over about that time, I think, and Ernie Tedderton and Phil M and Tuck and - who was the one who was a Communist spy?

DRP: Not George Eltonton?

KTB: No, Eltonton was at Berkeley. This was a solid state theorist, a very exceptional theorist who later went back to England after the war and was working with Skinner, and he had been a spy for the Russians all of this time. Well, his name will come back to me - Fuchs, that's it. Well, he was there, and Parles - I forget all of them. But I had known Phil Moon the Cavendish and he came and worked with me. Tedderton did later on the fast circuitry work. So I started that early looking for a place; you couldn't actually hunt for places at that season, but in May the choice was a four-cornered state district which was pretty barren or Mojave Desert which the United States Government used pretty much as a tank try-out place and they had a big airfield there and all the surrounding country, or the sand barrier beach off Brownsville or Corpus Christi which runs from Texas into Mexico, or the sand dunes at Colorado (in Colorado there's a big desert area of sand), and the other place was around Socorro which was barren but occupied - that is, there were a lot of ranches there, and there are today
but it takes twenty to thirty acres to support one steer there, it's that barren.

And so we started looking around and flying around. I got maps of every Western state, because I couldn't specify where the area was without giving something away maybe - you want to keep these two places separate, Los Alamos and the test site. We had a lot of trips into this area and the Southwestern area, which is north of Mexico and near the uranium area, and so on. Well, fortunately for the Indians at that time, the Secretary of the Interior said that we could not take any land which had one Indian on it. He was not going to disturb the Indians. Well, you get up in the San Juan country, Indian will get a quarter section, put a home upon it, and then for 50 cents a year he could get twenty acres of land or something from the Government for grazing, so he really owned homestead a quarter section, and the rest of it he could use, but we couldn't touch that country. And we had difficulty with that, and finally Texas looked too far away, because we had to get back and forth - we had constant contact with the base - and so finally when we got down to the other region, this looked like it, particularly as an area there about thirty miles by sixty miles had been taken over by the Army as an air-to-air gunnery range and practice bombing range. Before they went to the Pacific, the boys would have a final examination. They had to start from some place a couple of thousand miles away, locate this place, drop their practice bombs and get out, get home safely. And so we finally found a flat section of that. Partly because of the fallout considerations, we had to be where there wasn't anybody, but the ranchers were still in there though. They'd been pushed out by the Government and their cattle had been bought. They'd been guaranteed an income as long as they were out of their range, and yet they stayed there. But when we went down and got a little area eighteen by twenty-four miles - we selected that area - then the security boys came in and got them out of there. The soldiers were very much of a help to them. They'd never eaten range beef before and they
thought they were going to get something good to supplement their menu. So they let fly at these poor Herefords - and cried halt! three times. These things were so stringy you couldn't even hamburger them, you know, they quit that pretty soon.

Well, we got that area, and the rest of it was getting a camp there, getting roads in, getting a water supply - we had to truck in water finally. The desert water was beautiful stuff - drill a well two or three hundred feet and get ice-cold water which in a tank was a beautiful blue colour, an opalescent sort of blue - yet it was almost solid. It had a saturation value of calcium sulphate and calcium carbonate in it. Fine for swimming but you couldn't do anything else with it, and so they had to bring in a lot of water by tank truck for drinking and washing.

So the camp was set up - the first buildings were there - in December 1945, and very quickly grew. We didn't know when the test would but finally it was quite clear that it couldn't be earlier than June, and probably would have to be July before we could get enough plutonium to get and so we went ahead on the basis that it would be sometime in July. The finally we were pretty ready. We had a mock-up run to see what was wrong with the organisation and communications and so on in May with a hundred tons of TNT as a scaled charge - it was scaled so that we could check our instruments and so on. And also - this is something where certainly history is sour - also we were perfectly aware of fallout and also knew that this would be a good way of detecting if some other nation set one off in the atmosphere.

And so this first test - the thing was a big cube of TNT interlaced with very much faster stuff (Torpex) and things to blow it up at one time as closely as possible within milliseconds, and interlaced in this thing was a huge plastic pipe which was filled with a good many thousand curies of
fission products which we got from Hanford. And there was a chemist - Herb Anderson - and it was his idea. This is where I think he's been gyped, because it was his idea. Oppenheimer said, "Can you think of some way of detecting atomic explosions at long range?", and Herb Anderson did a few calculations on how many curies of stuff would be emitted and possible pollution and so on. It was clear that if you could pick up so of the dust in the atmosphere you could tell if there had been an explos you could also tell how efficient it was, and also tell what they'd used And so Anderson had charge of this thing and he put in this pipe and spi it with this stuff from a reactor - some of the slugs from Hanford - and this was the first detection of fallout. But there have been any number awards given to people in the United States, any number of generals and admirals who've claimed they thought of this first - well, it was Anderson by God. I don't know quite what to do about it - well, I did write to o Senator.

Well, that thing went off, and then we found things that were sour; the roads, which were dusty - people stayed with their cars, if the cars bro down; accounting for people, and so on. And the main thing was that the roads were lousy, and then we had to have about 25-30 miles of roads put before the bomb test to get in the cameras. I have some pictures showin the dust.

Well finally things got more tight. We had to draw a deadline - no new experiments two weeks before the test. We didn't want any wires changed becuase of the pick-up problem. No-one could bring in a new experiment i they changed wires around. We had to make tests then and the pick-up was bad on the signal wires with some crucial experiment. And then about two weeks ahead we did have a real ding, and that was that Trueman was going Potsdam and, by gosh, we ought to know whether the thing worked or not, a least while he was there, and July 15th I guess was the first day. And w
had our meteorologist Jack Hubbard who was very good, and General Groves he'd been in on the invasion, the D-Day stuff, you know; he'd been there with Holson who was the top meteorologist for Eisenhower, he'd been there too. And he did very well I thought, considering how finicky the weather in that section - nothing happens and then all of a sudden you get a cloudburst. So he was using the same system everyone uses today - the principle of similitude. You look for a pattern ten years ago and try to match it up with the situation you've got today, which is hard to do because the data wasn't that good out in that area. Anyhow, he did very well.

And so the first day was the 15th. Well, that was sour weather-wise, pl we really couldn't get the stuff there; some people hadn't made their first tests; and so it was postponed until the 16th, and various people came to me and asked if we could postpone it another week or something. Well you couldn't, because the boys had been working day and night for a month, and a good share of them for the last two weeks had been working themselves to death, and the let-down would have been something - they w have got this going again in two weeks, I'm sure, and they couldn't have extended another week. So we picked the 16th hell or high water if the weather wasn't completely stinking, because there were reasons why you couldn't have rain and so on. Well anyhow, it went off. We wanted it to go off around two in the morning, but there were clouds, there were thunderstorms, and so we waited until the last minute, which was 5.30. Well, we got that right because these seismometer boys had to see if they could detect the shock. The security boys had alerted all the earthquake labs to watch for something. And they told them the time it happened an the distance, and they had sort of a first show at trying to detect test Of course, this was above ground; this was about 100 feet above ground. That's where my radar experience came in. We wanted to get it above gro so we wouldn't get too much fallout picked up from the ground. And I kn that Blair-Knox made radar towers 200 feet high, and if we got the botto
hundred feet it would be good and sturdy, and so we got a Blair-Knox tow. We had Jumbo there, which had originally been to contain the thing. If it had fizzled, why you could have at least recovered the plutonium. that was still in the plans there until about two or three weeks ahead, and I said no. You couldn't tell very much if you had that thing there, and if it had gone off it would have spent most of its energy vaporising iron, and this was a couple of hundred tons of iron - steel - so that was cancelled. I guess it was more than two weeks ahead, but it was pretty darn close - we were both playing games - the tower and the container.

Well, then we knew it was successful. We had Fermi's experiment which gave the magnitude, fairly well actually, and we had the cameras which also could be used to give magnitude, and we had blast gauges, and so on; so right away we knew that it was around 20,000 tons equivalent TNT. It was until the end of July we went back and recovered other instruments for neutron yield, gamma-ray yield, which were buried underground around the thing. It was still hot enough there so that we had to take turns digging these things up - there was enough beta activity in the ground there. We carefully had the ground analysed. They had done a pretty good job, but they hadn't gotten the rare earths out of it; they hadn't told us there were rare earths there - large $\alpha$-sections, some of them - and they only had the rare earths, which is quite a bit of radioactivity. They hadn't realised how compressed the ground was; it pushed things down nine feet and produced a type of wave guess which is normally not seen in any earthquake. So after that was over, the thing was to evaluate all these tests and then write them up, and write up the difficulties and what to watch for, and so on. That was completed around 20th November I think.

Lawrence asked me to come to Berkeley with him, and Harvard didn't start second term until January, so I went to Berkeley until the end of December. They were still
here. My plans before the war were working with the cyclotron, and our cyclotron had been picked up and taken to Los Alamos, and we debated whether we would bring the cyclotron back here which they would have don Bob Wilson, who's now at Potavier, was coming to Harvard and he'd been running the cyclotron out there and he had a very good crew of young men who were really undergraduates who come out here and wanted to continue their studies and eventually graduate. So that these men offered to come back, and we had that pretty much set, and then we decided that we really wanted more energy. Berkeley had their huge cyclotron and still no-one w
sure it would work, you see, because - it was an 184-inch and Lawrence wa
trying a brute force method of getting it going - putting half a million volts on the 'D's and getting the thing round fast and cutting down on the phase losses as the mass increased, and so on. And at the same time we had Ed McMillan, who with Weschler had independently thought of phase focussing, and so for a while Lawrence was following both paths. Finally I believe - McMillan - they went ahead on phase focussing - the synchrotron principle. We decided locally to go to a proton-synchrotron, where we change the frequency and take care of the drop in the magnetic field and the increased mass of the proton. So Bob Wilson and I designed a buildin and then we designed a machine, and this unfortunately was limited. We didn't wind our own coils - we were limited by the biggest coil we could from Pittsfield Works and the General Electric Company here, and that me. we had something around 90 inches, which was good for finding about 150 o 160 MeV.

Bob stayed at Berkeley, and worked on proton scattering and chasing thruth the design of the tank. Here we worked on a half-scale model of the RF s. We managed to get back and forth so we could keep in touch, and then he c here in the fall of 1946. So that machine was completed in about three y I guess. He's still here, but he's retired to medical work with a big electron accelerator and is working on that.
DRP: So what happened after the war was over?

KTB: What happened was that I had been working — I designed the first cyclotron and helped design the second, and was interested in that, but then Segre I was also still interested in isotope separation, but that was killed. I mean, I had isotope separation schemes; in fact, one for separating uranium I worked on in 1940, and I thought of this idea for separating — you know the idea of uranium 235 — only one part in 140 and you've got to get the stuff — and I talked to Stockowski and Bob Wilson who were interested. So I gave them the equipment I had for the separation and set that up in the Chemistry Department. I set up a little Mass-Spectro here to analyze how well we were doing, and we worked with argon as the simplest thing to work with, and then that could be extrapolated to uranium. And this worked out very well, and I remember George, after we had some results, went to Washington to try to sell this to someone in Washington as a way of getting uranium 235. He got down there and he was very close to Conant, and they had introductions to big shots in Washington. Finally I guess he ran into the Navy, and they said, yes we're doing things like this and this is very interesting and well taken care of — very hush-hush stuff — you fellows just forget about and go back. So we quit on that; that was in the spring of 1940. We got started in the winter and sometime in April, I think, George got this thing — get the hell out of this business.

Let's see, where were we? After the war and how I got back into Mass-Spectroscopy. Well, there were very few masses which had been measured at that time really. I mean, sure people went up to sulphur, and I guess Dempster and Duckworth had done some of the heavier ones — rare earths and so on, and Aston had done tin and things, but not within a factor of a hundred, just the accuracy that people wanted at the time. Then I went back into the business with the idea that I would get high currents and high accuracy if I went to a larger machine, so that for the same slit size I
could get currents as good as or better than before and considerably hi
resolution. In fact, I could finally push resolution up to one part in
300,000, which was very good. I think Aston was working around one in
and one in 5000 or something. I was working here before the war around
one in 12,000, the best I could do, but we soon got up to 100,000 to
300,000. Well then also I guess my mind got back because Segré asked:
could write a chapter for experimental nuclear physics on Mass-Spectrom
and I guess this is one way of recovering from thinking about weapons f
five years. To be forced to do some physics and turn my mind off from
counter-measures, atomic bombs, long-range detectors, and so on. So th
I started building the thing, and finally got it going. I was running
until two years ago, and then with Don Kerr - this machine takes two
people to run it. One fellow has to be there eight hours a day full ti
depending on which term it is - duties and so on - the other puts in a
quarter to half time. It's essential though to have two people on it.
tried for a while running it by myself with a technical assistant, but
doesn't go. Any business like that, where you're working on precision
measurements, you've got to be damned sure that nothing is changed, or
you change it, that you've done it and know what happens each way, and s
on. And you have to run the thing all the time too - this is impossible
So I stopped that and went into some radioactivity stuff - changing dec
rates, changing physical state, chemical state of radioactive substances
which the electrons - where you have electron capture, or where you have
meta-stable state isomer, where you get transition by ejection, coupling
nuclear energy to the electron-ejection of the electron. There you can
play around a little bit with the decay, and you get some idea of the
density of electrons produced. Now I am working on a crazy neutrino
experiment with not much hope of success, but it's worth a try. I mean,
somebody's got to do it and I thought I'd do it - why not?

END
INTERVIEW WITH JOHN PAUL BLEWETT 27th July 1972 at Brookhaven National Laboratory

DRP: Perhaps we could begin by discussing the development of your career, from when you started at college to when you first started being engaged in Mass-Spectrometry.

JPB: I did a year of graduate work at the University of Toronto before I went to Princeton in 1933, where I started off to work with Harry Smyth. He was interested in the products and the processes of ionisation in gases, for which we were using Mass-Spectrometers to make studies. And at the time Bleakney had two people working with him; two of the more senior of his students were Philip T. Smith and Wallace Lozier. One of the main things he was doing was looking to see if he could find an isotope of hydrogen, namely deuterium. So he and Smith and Lozier had built a Mass-Spectrometer which didn't have much sensitivity as far as masses were concerned, but it had quite high sensitivity otherwise.

DRP: So this is what they were engaged on when you first went to Princeton? You say that they had built the machine to specifically look for an isotope of hydrogen?

JPB: Yes. Unfortunately, it wasn't quite sensitive enough and they didn't make it. It was found by - I guess it was Urey that discovered it. Well, I started off to do some work on molecules and I put together - I inherited a Dempster-type of Mass-Spectrometer which had been put tog by a man named Stuckelberg, who was there at that time, but he'd gone back to Switzerland when I arrived. He was quite a character (I'm not sure whether he's still alive or not) - a military type; he loved to ride on horseback and he was a cavalry-man as I remember. I didn't ever meet him because he left before I got there, but he was quite well known. I
believe he went back to the University of Geneva where I think at one
time he was. Head of the Physics Department, I'm not sure. So I started
off looking at the carbon disulphide molecule - I seemed to specialise
in very smelly things there. The purpose of this was to do as Smyth
had previously done with carbon dioxide, to see if we could tell from the
products of ionisation how the molecule was put together, and we were
able to show quite a bit. We got CS^{++} and CS^{+}, but we didn't ever get
any S^{++}, from which we concluded that the thing was a straight-line
molecule with a curve in the middle. And the same thing had been shown
for carbon dioxide.

So that was that, and then I got involved in several things. I started
to do a thesis on bromine. Several phenomena had been discovered in
bromine by the nuclear physics people, in particular the Russians. And
so people began to wonder whether there were other isotopes of bromine
besides the two that were known, which had mass 79 and 81 as I remember.
So I rebuilt the Mass-Spectrometer with some sensitivity to mass and
started to look for new isotopes. I didn't find any - at least, I don't
think there are any others actually - and it turned out that these things
which had been observed were isomers which weren't appreciated then but
are now, so everything hangs together alright.

DRP: How long did it take you to rebuild the Mass-Spectrometer? What particular sorts of problems did you come across in building it?

JPB: I changed the vacuum chamber so that the magnet poles were the walls of the chamber to make some more space. The thing had formerly been all built of glass. It was a Dempster-type Mass-Spectrometer, and I put a brass lid on it to hold the vacuum. It worked fairly well at first, but then it began to behave very strangely and I took it apart to see what
had happened, and I found a sort of brown fur all over the poles, which had been caused by some bromine reaction. It is very active stuff and it's very smelly, so I went and copper-plated the poles. Copper-platin steel is a fairly complicated process as it has to be done in a cyanide bath, and so I had this cyanide bath in my office and the whole thing was very lethal. Then, I remember, I had it all set up with some sort of electrometer as a detector with a fairly long light arm and the scale in front of my eyes. And then I had controls on all the various voltages of this thing, which went up to the order of a thousand volts. On the side were rheostats and every now and again I got a several hundred volt shock off this thing, but somehow I survived alright.

DRP: What pumps were you using for that?

JPB: I was using mercury diffusion pumps, all in glass. Although I did a lot of the glassblowing myself, I didn't make the pumps; we had a glassblower who was expert enough for that. I couldn't make ring-seals that big, I never learned how, but a lot of the other glassblowing I did myself. That was a very exasperating process, especially when the whole thing was set up on a table. Some of the glass was above the table and more of it was down below, and - have you ever done any glassblowing? - well, when you're sealing one-inch pipes together what happens usually with this stuff - Pyrex or some other kind - a hole will form in the joint that you're trying to make and when you try to seal it up with a piece of can glass, the hole immediately runs around to the back side of the thing where you can't get at it. So I trained myself holding the torch with the flame pointing at myself, and I perpetually had all my front hair burned off, so that was fairly exciting.

There was also a lot of interest in beryllium, as to whether beryllium...
was stable or not. By this time a man had moved in across the hall from me, a man named Milo Samson and he was also interested in Mass-Spectro. He built, at Bleakney's suggestion, a big Mass-Spectrometer with permane magnets which therefore was pretty stable. And he and I collaborated on several things. We worked on beryllium, and Ruby Sherr' was of course on this too, and also Roman Schmolokowski was there - he's very deep in solid state physics now. So we got ourselves a bar of beryllium from Ra Metals of New York. We didn't know anything about beryllium being letha in those days, or about the hazards of inhaling its vapour, and so on, so sawed up this bar with a hacksaw and then heated it up good and hot to evaporate the beryllium, so we could look for beryllium 8; but it wasn't there.

Then I got interested in filament sources of positive ions and fooled around with that for a bit. It was well known that there were some sorts of mine like spodumine, that would give off ions of lithium, and it turns out that practically anything, when you heat it up, will give off ions of sodium a potassium. If you scrape up the dirt off the floor and heat it, you get sodium ions given off. So I tried a lot of other things. I've tried aluminium oxide and got both positive and negative aluminium ions out of and also barium oxide, and Samson played quite a bit with barium. We discovered a couple of new isotopes, and we looked for, and found, a stron isotope too I think - I've forgotten the details now, but it's all in the literature. Then I got interested in doing some chemistry by bombarding gas with ions. There had been quite a lot of work done on that on the West Coast. So I got the thing set up, all built with a filament that you could swing through large angles and do some scattering, but I didn't get it finished quite as fast as I thought I would, but I got my PhD and finally got out. I got a fellowship to go to England, and there I met As. That had been one of my ambitions, and I have some comments to make about
Aston, but I probably should make them off the record.

DRP: You think so? Well, then you went to the Cavendish to work?

JPB: Yes, I spent a year at the Cavendish. There I got involved in measuring regeneration energy relations for alpha particles.

DRP: You weren't working directly with Aston?

JPB: No, I wasn't working with Aston. He turned out to be something of a disappointment. I gathered that the reason he was working with isotopes was because old J. J. Thompson many years ago had suggested that he did the same thing for ions that he, J.J., had done for electrons. Old Aston got going at that and he just kept on steadily ticking off the elements for which he got Nobel Prizes, or at least one. He didn't work very hard He was independently wealthy, and so he spent half of his time in Switzerland.

DRP: Photographing eclipses and so on.

JPB: Yes. There was a big dispute going on between him and Bainbridge about the mass of carbon, I think it was, and there were a lot of interesting discussions. It was a very interesting place to be in those days. Rutherford was still alive, and Fowler was still alive, and Dirac was still there. I ran into Dirac just last spring, much to my surprise, in Florida where he's living now. I guess that's about the extent of my history in Mass-Spectrometry.

DRP: So when you came back from England, you didn't carry on in this field?
JPB: No. I went to the General Electric Company then, and I had a theory that although you might never win a Nobel Prize by doing it, you would keep interesting if you changed your field once every five years, and I did it for about 20 years, so I never went back to Mass-Spectrometry. I got into thermionics and properties of oxide-coated cathodes, and then I got into microwaves and worked on radar during the war and radar counter-measures. And then I came here and got mixed up with accelerators and I've never been really able to get clear of it. Although I'm changing now—I'm working on superconductivity.

DRP: Do you think it takes a special type of physicist to be able to change from one field to another, or do you think it's just a question of willpower?

JPB: Well, I think anybody can do it if they really want to, it's just that you have to be willing to be anonymous for a few years. We've got involved in superconductivity here. I'm running a conference on magnet-technology in October, and we have a project on superconducting power lines which we've now got financed to the extent of half a million dollars from the National Science Foundation. We were just having a session with a visitor who's from Canada giving us a lecture on how to design high-voltage splitters for power lines.

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SG: All that I shall be relating will be coloured with my personal opinions, so you must realise that this is of course a highly biased account. I not believe in impartial histories of physics - absolutely not - so please have it on record on the tape as a highly biased story.

The origin of the laboratory here was to make it possible for projects were too large or too expensive for universities at the time when the laboratory was created. Of course, it isn't true any longer. These we not only the usual nuclear projects like large accelerators and reactors but electron impact experiments too.

Bleakney and Hipple had an idea for a Mass-Spectrograph which sounded very good. Namely, if you have a crossed electric and magnetic field, then have automatic focussing. No matter what speed the ions come out, no matter what they do, they travel in cycloids and they focus in different points depending on the mass. Moreover it's a linear scale. But the resolution is very small, so you need a magnet and a good magnetic field very uniform, and a good electric field of course, which is enormous. You need an enormous magnet. In fact, a small prototype was built, I think the Bureau of Standards, by Hipple, who showed that at least in principle it could be done. And this laboratory wanted to build a large magnet, you know, in line with the original plans of the laboratory. Linc Smith, I think, was hired to be in charge of that project.

I came to the laboratory early in September 1948. Linc Smith had been on an extended vacation, and hadn't come back or wasn't starting his job. I listened to a few lectures about it on a Sunday afternoon. The lab was small and there were six people; Bainbridge was coming, Bleakney was here and I listened to their project. Now among other things during the war
I had been on radar, and when I came home and looked at that project, I thought that nowadays one can do that simply, because we have learned how to measure time very accurately. So instead of having this enormous mag with a hundred cycles until the mass has spread sufficiently you can let them go around and measure the time, and then you get resolution in time which at the moment is so much easier to measure than resolution in space.

So I went to the Director of the lab, Phil Morse it was at the time, and said that I had this idea that one ought to do it by measuring the time that it goes around in the magnetic field instead of doing it with cross electric field and measuring the space, and he said OK. So I went around the lab and I found there was a magnet available which had been used as model, a small model for a cyclotron, which I could have to play with. Happened to have two young fellows working with me, one of whom I had worked with before - Paul Richards and Earl Hayes - and we set it up in Bill Gowan's lab because I had no lab at the time - I was still classed as a theorist. And indeed, just one day when the trustees were here fortunately, we got results on the scope where the isotopes - I think we used neon or argon in the first time - were very nicely separated on the scope by several microseconds. We got the idea we could let them go around a few more times, but then you needed a little wider magnet because the main handicap was the geometry rather than anything else.

You have a source here at the front, and they go around and they hit the back of the source. Well, a big magnet - how do you get it? You don't get it; you look around and ask, "Has somebody got one?". Indeed, before the war, Ed Condon, when he was at Westinghouse - long before the war - Hipple, who was also then at Westinghouse, had built a magnet which was really the way a theorist would build a magnet. There is a problem some in Whittaker about how you can get a homogenous field inside a sphere.
take a hollow sphere and you put windings around it, and you put them a just the right angle and the right number of windings, and if you do th right, then you get - inside the sphere - a nice homogenous field. Now that magnet was standing somewhere in the attic of Westinghouse, and we asked whether we could borrow it. They said we could with pleasure, an so we borrowed that hollow sphere to have a nice magnetic field which h some space in it, you see. It didn't have to be a strong magnetic fie we used around 600 gauss, or oersteds, you say nowadays. And in order have uniform current going through the windings, we were fortunate that the lab for other reasons currently had a whole set of submarine batte They are just like automobile batteries, only they are as large as this desk each you know, and we had a room full of them. So we used that as our current supply, because we didn't want to be dependent on the varyi current of the Long Island Lighting Company, which is absolutely imposs even after you rectify it!

We built a vacuum chamber in that thing, and a source. We made some contribution to a new type of source, as I shall mention. Actually then in order to measure the time.... I had been on radar, and the people her at the Electronics Instrumentation Department were also old radar gradua and I said to them, "I need the thing"; the same kind you use in radar when you measure time differences by coincidence courses. "Oh yes," the said, "that's easy. What times do you need?". I said, "Well, they go around a thousand microseconds with the heavy elements." And they did i I didn't know it was in the books, I just knew how to look through the scope and twiddle the dials. And we were able to measure these times around and match pulses to about a hundredth of a microsecond, and that quite good. We could let the ions go round about ten to twelve times an measure the first time around and the last time around - the tenth or eleventh, because the time that they left the source was very hard to
know, you never know what part of the source. As I said, geometry was rather important, and the source has a certain size, and the pulse always indicated it was about 3 millimetres long when it went round, so a little cloud of 3 millimetres went around, gave a nice pulse on the scope and then went round again, that was the principle.

Now - they go around - I think it came out to be about a microsecond for a heavy mass - and we could measure to a hundredth of a microsecond with precision, you know, maybe plus or minus a hundred. Well with hydrogen of course that's nonsense, because it goes around in a hundredth of a microsecond and that's nothing, but this was meant for the heavy elements you see. It's a linear scale, and we got very fine results which I remember in the literature, and a book by Seglé and Bainbridge I think has the best data for some of the heavy elements; for xenon, bismuth, those were the ones we played with. There of course a hundredth of a microsecond is a very nice precision, at that time at least, and I am sure that this technique could have been improved, because in the meantime pulsing and timing techniques have been improved. But it was primitive; we used an magnet which wasn't always very homogenous. In order to get the pulses into the detector of the right intensity we had to put shims in here and there, and they changed once in a while.

We discovered, because the ions are very slow, that probably surface deposits on the vacuum can make a difference, and after we had worked for a week the pulses got blurred. Then we had to clean the can; we never had any spots on it, but when we cleaned the can, just with elbow grease, it worked again beautifully: very sharp pulses for a while, you know, coming exactly at the place where you wanted. At the end of the week you had to move the source a little bit in order to get the pulses in; it was obviously a deflection due to certain surface potentials - undetectable
We coated the can with gold because we were told it might be a little better. We did so, and it helped a little bit—instead of one week, we could work ten days, or something.

It worked well, and then at that spot we also tried to do something else to see whether we could get excitation effect from organic molecules. Because it was all so easy—you had it all on the scope—you could take easy pictures with Polaroid cameras. And we could change the electron excitation of the molecules without any trouble and could look at—even could follow—one slow chemical reaction. We were able to show it. But at that time the project was taken away from me, so that was the end of that. It's a pity; I always hoped that somebody else would pick it up again, but you know how difficult it is to get somebody interested if he didn't think of it himself. The 'NIH'—'not invented here'—syndrome is very strong among physicists, so I've never been able to get anybody else interested.

So then Linc Smith changed the principle entirely. He went back to measuring frequencies instead of measuring time, which is a modification of some other ideas which existed already. He was very successful in principle and in practice for the lighter elements, which probably are more important, I don't know. He got some results first with my instrument with mainly lighter elements. His main trouble was his perfectionism, was never satisfied. I had to go out there with all the results we had got with several more elements—he always shook his head. It's a pity and I'm sorry; I should have stayed with them instead of getting into administration.

So that's the story of my Mass-Spectrograph. It has been written up in the literature here and there. I probably have some prints about it, and
I even was able to find out certain details about the scattering of these ions as a result. Much more can be done with that kind of instrument, because it's so versatile.

What happened then later? Oh yes, then industry got interested. They came round, talent scouts from industry, and they said, "That's a nice thing; it may be very useful in industry", and they planned to build one. But that was just the time when you could not get alnico - that was scar. They wanted to do it, of course, with a permanent magnet, which, for industrial processes, for control, would be sufficient. You didn't have go ten times around you know; if it went twice around it would be enough. And they couldn't get a magnet. They couldn't wait, so they developed a linear Mass-Spectrograph by just playing with the source to get it focussed a little better. The got the linear Mass-Spectrogram; it was Bendix that did that. Except that they kept me for twenty years as a consultant on the payroll, which was very nice - that's all I got out of it. They just fired me last year after all that time. And then finally they got a magnet delivered - a nice magnet - and they didn't use it, so I said, "Can I have it?". I made a joke - "Send it to me airmail", you know - and then one day I came into the lab and the stockroom said, or the receiving room said, "You know, there's an instrument for you by special air freight. There is no voucher, there is no purchase order, nothing. What is it?". It was a personal present to me of their magnet. It came just a couple of weeks too late. I tried to keep my technician interested instead of changing, you know, but since I was kicked out it didn't work. My technician went over to meteorology, where he still is, and the magnet I gave to a small college where a pal of mine was working. Since it was my personal property, nobody had anything to say about it any more. I asked the Bendix people, "Surely you can do with it what you want?". "We don't want it, it's in the way." So now a small college
where I gave a lecture, where a former student of mine was working, got that back. That's the story of my life. Being miscast as a theorist and then into administration - which is bad - and I can tell you about administration off the record.

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