A Memoir concerning the Londonderry Laboratory for Radiochemistry at the University of Durham

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Introduction

The Department of Chemistry of the University of Durham is, by any standards, one of the best in the country; many of its academic staff have enviable international reputations, and its graduates carry the stamp of the Department's achievements with them. These are statements which can be made with confidence in 1995, but to have suggested anything to this effect in 1945 would have produced surprise in Durham, and derisive laughter elsewhere. Tempora mutanter, nos et mutamus in illis (included to show that I have not forgotten my Latin) is true both for chemists and for their Departments, and it therefore seems appropriate to examine the roots of these changes, and the lives of some of those who initiated them.

On a number of occasions in the early 1980's, Prof. Ken Wade and I spent pleasant times together in various locations, including Windsor and Durham, and the idea gradually formed between us that I should write something about the early history of the Department, given that I am, by chance, apparently the only product of the immediate post-war years to have followed an academic career in chemistry, and might therefore have some appreciation of the problems which have been solved in the course of the Department's growth. Having agreed to the proposition, I followed the usual academic tradition of doing nothing about it until my
conscience nagged me once too often. In October 1988, I arranged to spend a week in Durham, and in fact devoted much of the time to reading papers in the University Library, about the foundation of the Londonderry Laboratory for Radiochemistry,1,2 and talking to various people. The information obtained in this, and other ways,3,4 matured and fermented in my mind over a difficult period in my personal life, so that the offer of a Visiting Professorship in the Department in 1995 made me determined to complete this project. In passing, let me note that to be given the opportunity to re-visit the place at which one's scientific life began is not something that comes to many of us, especially when this involves an invitation to return to the Department which I entered in 1946 in a state of untutored naiveté which still causes me amazement and cynical amusement, and my time here in 1995 has been a joy for many reasons.

I have tried, in what follows, to identify the important dates and people concerned with the development of the Department between 1924 and 1953. There can be no concealing the fact that this is a personal approach, and one that advances personal, idiosyncratic judgements about people, and about chemistry. For this reason, I have appended a brief expurgated autobiographical sketch which may explain and expose my own background and experience, and hence allow any interested reader to make his/her own adjustments to what is in some cases personal opinion.

The Origins of the Department of Chemistry
I have been told that the Durham Cathedral Library had at one time a fine collection of scientific books, but decided to opt out of the field when the rate of publication became too high for the budget; the story supposedly relates to the seventeenth century, and true or apocryphal, it certainly has a familiar ring to twentieth century academics. In practical terms, science clearly was of little importance in Durham until the establishment of a separate site at the corner of South Road and Stockton Road in 1924,5 although in his obituary notice of Arthur Holmes, K C Dunham6a notes that the teaching of mineralogy by J F W Johnston in the period 1832-1856 marked the beginning of the teaching of science in Durham. The
decision was subsequently taken to centre science and engineering in Newcastle-upon-Tyne, at Armstrong (later King's) College, and it was not until 1924 that the revival of science in Durham in 1924 lead to the appointment of J I O Masson as Professor of Chemistry, and Head of the Department of Science. From the obituary by Haworth and Lamberton, Masson emerges as a complex and fascinating character. The son of an eminent chemist (Sir David Orme Masson, FRS (1858-1937); Professor of Chemistry, University of Melbourne, 1886-1923), and a member of a large family of distinguished academics, his career included periods at the University of Edinburgh and University College London, and wartime service in the RNVR and Woolwich Arsenal. His research spanned inorganic, physical and organic chemistry, and while in Durham he published about 15 papers on intermolecular forces in gas mixtures, nitration, iodination of aromatic compounds, and iodine-oxygen compounds, displaying a breadth of interests which no contemporary granting council would tolerate.

While this obituary suggests that Masson was not widely popular in Durham, he nevertheless played a crucial role in the establishment and development of science in Durham. Amongst other key decisions, he insisted that separate Honours and Pass degree programmes be offered, and initiated a tradition of requiring very high standards for the First Class. His interactions with the Royal Commission of the University of Durham in 1932-1933 seemingly influenced the future of the University as a whole, and this no doubt was in part responsible for his translation to Sheffield as Vice-Chancellor in 1938. In terms of the views expressed about Masson's popularity in Durham, and his reserved character, the arrival of Paneth, with his very real charm, his urban outlook and his wide scholarship must have done much to convince those in other fields of the possibility that scientists can be both human and humane.

Amongst those appointed in this period were A Holmes (Geology, see later) and J E P Wagstaff (Physics), and in Chemistry A K Macbeth (Reader 1924-28) H J E Dobson (lecturer 1924-26) and G H Christie (1926-1995); W C Gibby apparently replaced Dobson in 1927, and W A Waters (1928-1945) followed Macbeth in 1928. Macbeth (1889-1957)
subsequently had a distinguished career in Australia, as Professor of Chemistry at the University of Adelaide. Christie’s contributions to the University have been sympathetically summarised by Coates. Gibby’s interests in surface chemistry involved the use of techniques for measuring the surface area of dyestuffs using surface pressure (Langmuir) techniques. Of those who worked as research students with Gibby, the person I came to know well was C C Addison (1913-1994), who was Reader and then Professor of Inorganic Chemistry at Nottingham in my time there, elected Fellow of the Royal Society in 1968, and President of the Royal Society of Chemistry (1976-1977). By C C A’s own account, Gibby paid little or no attention to any of these achievements. As I note elsewhere, my own interests in inorganic chemistry were prompted by what was to happen later at Nottingham, yet another example of the circuitous nature of chemistry. Others who spent periods of time in the Department before moving elsewhere include J C E Simpson (1939-1945), an organic chemist of distinction, and W A Crawford, who preceded G Kohnstam in 1949-1950.

The Durham University Library archives contain a striking photograph of the undeveloped site in 1921, with the Elvet Colliery standing alone, and later views of the one-storey building which was the origin of the Dawson Building, and thence of the complex of buildings which are now in existence. The top floor was built in the late 30’s, but not put into general use until about 1948, having been used for RAF Short Course students during the war, so that all science teaching and research had been carried out in miserably small space until that time. To see the Dawson building being refurbished (Autumn 1995) is to bring vividly to mind the immense difficulties of the pre-war and immediately post-war years.

In that period, I believe, the most respected of the Science Departments was Geology, where L R Wager was Professor, having succeeded the equally eminent A Holmes, who had gone to the Regius Chair at Edinburgh. When Wager went to Oxford in 1950, he was succeeded by Kingsley Dunham; others who went on to great success were F H Stewart (Edinburgh), E A Vincent (Oxford) and G M Brown, a Durham undergraduate and eventually Director of the British Geological Survey. The Departments of Physics, Botany, Zoology, Geography
and Mathematics all suffered from the common problems of heavy teaching loads and the lack of adequate research space, research funding, technical assistance, and graduate research students. As a student, it was easy to be critical of the general absence of research and scholarship; as an older and wiser academic, one can readily sympathise with the frustrations which must have been the daily lot of once enthusiastic and ambitious researchers.

As a first-year student, I was of course completely unaware of this historical background, but one of my first relevant memories is that during the terrible winter of early 1947, when snow lay on the ground throughout most of the term, two new buildings were being erected at the south end of the Science (Dawson) Building. Interestingly, I remember watching these on occasion from a room in which Paneth lectured on the joys of the Periodic Table (with special emphasis on both Lothar Meyer and Mendeleev). These 'huts' were to house the Londonderry Laboratory for Radiochemistry, officially opened in June 1947 by Lord Portal, the Director of whatever name then covered the UK Government's atomic energy programme. Two more huts were added later (ca 1950); Graham Martin\(^2\) has recounted in delightful detail the joys and hardships of those days, with the insight of one intimately involved in the minutiae of administration (something which I came to realise he enjoyed immensely, despite his protestations). The unit was named after the then Chancellor of the University, who covenanted money for a new development to be identified by the University, and Paneth's determination to see the growth of radiochemistry was no doubt a major factor in the selection of this particular project as the use for these funds.

It is necessary to underscore the strange and unique relationship during this period between the Durham Colleges (as the official name had it), King's College Newcastle, and the University of Durham Medical School (also at Newcastle). At the student level, one was aware of the tripartite nature of the University of Durham since sports were organised separately in each Division, whereas University teams were chosen to represent the whole federation. Even in athletics this caused occasional problems, and I hate to think about the selection of a Rugby XV. Again as students, we were aware of some of the problems caused
by different emphases, different syllabi and very different research interests in Durham and Newcastle, but the main difficulties clearly arose from the difference in mass; Durham in 1948 probably had 900 students, KC and UDMS together nearer 2500. If this was not enough in itself, science and engineering were long and well-established in Newcastle, in contrast to Durham, and all of this resulted in an apparent sense of rivalry, rather than collegiality, between the Divisions. To match Paneth in Durham, one would have found in 1951 G R Clemo, P L Robinson, J Weiss and J K N Wynne-Jones, all with high reputations in British chemistry. Meetings of the whole federated Department must have been extremely difficult, given the conditions of the post-war period, and the inherent structural dissimilarities.

The separation of Newcastle and Durham into autonomous institutions, which took place in 1963, must have come as a welcome relief to all concerned. Once placed on its own feet, and with the necessity of surviving as such, science in the University of Durham clearly flourished, and the relative standing of the two Departments of Chemistry is now very different from that of 45 years ago. This is not the place to recount the subsequent growth and rise in reputation of the Department of Chemistry, and in any case I played no role in those developments. The only relevant comment is that the activities of Paneth and the radiochemical group certainly provided the basis for that growth; equally, the efforts of the successive members of the Department have been responsible for what has happened since 1953.

**F A Paneth**

Paneth's life and work has been carefully reviewed by Emelus,\(^{13}\) who identified a number of remarkable achievements, any one of which would make most chemists proud. In my estimation, these include the first essential use of radioactive isotopes as tracers; the first identification of free radicals in the gas phase; the discovery of gaseous hydrides of several of the heavy elements; the determination of helium in the atmosphere and stratosphere; and the
application of this technique to the age determination of minerals and meteorites, to half-life measurements, and related problems.

After an initial foray into organic chemistry, Paneth began his studies of the new science of radioactivity with Stefan Meyer in Vienna in 1910. Other mentors included F Soddy (Glasgow) and E Rutherford (Manchester); his subsequent progress through the European (pan-Germanic) University system took him to Prague, Hamburg, Berlin and Königsberg. His eminence as a chemist can be judged by the many recognitions which he received in his life time. He was appointed to the directorship of the Chemical Institute in Königsberg in 1929, a singular event at a time when such positions in Germany normally went to organic chemists. He knew on personal terms all the early workers in radioactivity; evidence of this is found in a photograph of a group of eminent scientists (probably about 1929). He was nominated in 1938, along with Hevesy, for the Nobel Prize. He became a Fellow of the Royal Society in 1947. He was the Chairman of the International Commission on Radioactive Standards in 1948-1951. All of this must have been heady stuff in a University city unaccustomed to such international eminence, all to be taken with the urbane civilised manner of a man of genuine culture and outlook. One can only wonder about the way in which the rest of the University establishment regarded him. Whatever the response to this thought, there can be no doubt that his very presence in the city put Durham on a scientific map from which it would have otherwise been omitted.

His teaching, insofar as he gave lectures to undergraduates, was based on a literate and expert use of the English language, which made him a delight to listen to, but the chemical content which I retained was slight, and I believe that others would share this view. In matters to do with radioactivity, the wisdom and the experience were immediately apparent; in other areas, and especially inorganic chemistry, the Periodic Table could and did form a basis for almost everything. I shall comment elsewhere on his interest in inorganic chemistry at a time when many regarded the subject as moribund. As a research supervisor, he was generally an éminence grise, leaving the daily (or monthly) supervision to others. The selection of topics
was certainly something in which he was involved, but beyond that his interest was essentially in the results. One outcome of this, and without arguing about the details, was that in my opinion the training and supervision of graduate research students at the time was lax and poorly done, and that a tighter system would have produced more and better results than was in fact the case.

**Radiochemistry in Durham**

The history of any science is, in the simplest terms, the history of the development of the ideas of the men and women who conceived of the problems, did the experiments, worked out the results, and wrote the papers. Nothing is so fundamental to an understanding of the nature of science as the acceptance of the vital human factor in the subject, and yet ironically little or no attention is given to this essential humanity in the teaching of science. Too often, we teach results, without emphasising that each result depended on an involvement of human energy, physical and/or mental, and of the human spirit of people for whom at the time the production of that result was an achievement to be savoured with the pride of accomplishment.

Few subjects illustrate the theme of human involvement as clearly as that of radioactivity, and this field has the additional attraction that its origins are few in number, and readily identified. Following Becquerel, the names most strongly associated with the early years are of course Marie and Pierre Curie, whose story has been told on the personal and the scientific level in several places. The developments in the period 1900-1940 have also been well delineated; one of the fascinations of the story is that until about 1935, the number of workers was small, and their contacts of an intimate (and sometimes familial) nature.¹⁶

When one examines the growth of the sciences, and in our case of chemistry in Durham, it is therefore necessary to come down to the life and times of the people involved. This is especially the case when one focuses on the period 1939-1953, for there was undoubtedly in Durham only one area of chemical research worthy of mention on the national or
international level, and hence only one scientific leader who with his assistants held the key to success or failure. The field was radiochemistry, and the leader was F A Paneth*. One purpose of this memoir is therefore to focus on what was done in those years, on the long-term impact of radiochemistry on science in Durham, and on what happened (and why) after Paneth's retirement in 1953.

We have seen that when Paneth came to Durham in early 1939, he inherited a small Department of generally undistinguished stature, in the scientifically less important and smaller division of a provincial university. The physical and financial resources were poor, and the number of potential research assistants much smaller than had been the case at Imperial College, or in Königsberg. Whatever his reasons for accepting the position of Head, he accepted the challenge of building a bigger and better Department, only to have any chance of immediate success dashed by the outbreak of war. Factors too numerous to list completely, but including scientific manpower control, shortages of everything, the call-up of students into the armed forces, the impact of RAF Short Courses, the internment of family and colleagues as enemy aliens, and the general tenor of life in war-time Britain, all added together to make pure research both unrequited and impossible, and it seems that no work on radiochemistry was done in Durham before about 1945.

Paneth spent the period between 1943 and 1945 in Montreal as head of the Chemistry Division of joint British-Canadian Atomic Energy project. There are differing views of his contributions to this project (compare Emelius,¹³ p. 229, and Martin,² p. 5-6), but apart from anything else, the contacts which were established were important when the Durham radiochemistry effort eventually did get under way. Friends in high places in the UK atomic

* These statements are not meant to denigrate the work on organofluorine chemistry by W K R Musgrave. At the time under review, Musgrave began in 1948 with one student, followed by two more in 1949, but the publications from this expanding group had made little impact by 1953. This changed markedly, of course, in subsequent years.
energy world (Cockcroft, Spence, Akers), added to those in the chemical industry (Applebey, Fleck), were of tremendous help in the difficult post-war years, when shortages were an accepted and constant fact of life.

The rate at which things changed can be judged by the size of the group to be seen in a photograph taken in July 1951. In addition to Paneth, there are two lecturers in radiochemistry (G R Martin, K F Chackett) a university demonstrator (R Mercer) an ICI Fellow (L Tordai), a staff physicist (E Martin), two foreign visitors (Aeschbacher, Purkayastha), seven PhD students (plus two others absent) and three technical/secretarial staff. A few PhD’s had already been completed (Durham, Hollis, Robson, Sutton, Wilson) and two senior people (Glueckauf, Morgan) had left earlier to go to AERE and AWRE respectively. Such growth is the reward for administrative, as well as scientific effort, and the main driving force can only have been Paneth himself. Apart from this development of the clearly academic side, there had been at least one training programme (in 1947) for scientists at the then recently initiated AERE Harwell, clear evidence of Martin’s contention (p. 8) that Durham had the largest concentration of radiochemical expertise in any British university at the time. Indeed, it is difficult to think of anywhere else other than Maddock’s group in Cambridge where the subject had any significance in British academic life.

In terms of scientific achievements, a good starting point is a document written by Paneth in March 1945 to W A Akers, then the Director of Atomic Energy Research for the UK., and on secondment I presume from ICI. At the time, Paneth was still in Montreal, and possibly Akers was there also, since this looks like an internal memorandum. The document is a well-argued and persuasive presentation of the case for a laboratory of radiochemistry in Durham in the post-war period, and for government support of the work, and makes the argument in terms of both the pure and the applied importance of the field. Paneth also stresses the opportunities presented by the impending expansion of the Chemistry Department in Durham, and the wish of the authorities to attract research in one or two specialised areas not represented in many other English universities. I have not seen a copy
of any reply from Akers to this memorandum, but the subsequent history of the growth of the laboratory suggests a sympathetic reception. The argument for specialisation is especially significant and proper, given that a university of Durham's then size cannot afford to spread its limited resources too widely. This policy surely paid dividends eventually not only for chemistry, but for all of the other sciences in Durham.

The scientific theme of this document opens with an accurate appreciation of the crucial part played by chemists in the discovery and subsequent understanding of nuclear fission. Paneth notes with understandable regret that radiochemistry had not achieved any stature in pre-war academic research in Britain, in sharp contrast to the eminence of nuclear physics.* He goes on to argue that the handling of radioactive isotopes requires specialised training, and underlines the interesting chemical problems associated with the low concentrations sometimes involved (eg radiocolloids, electrochemistry). The then relatively new area of radiation chemistry is also identified, and it is worth noting how much more rapidly this area grew compared with radiochemistry in the post-war years. It is interesting to contrast these proposals with what actually took place in Durham in the period 1945-1953, and subsequently. In manpower terms, the largest effort went into microanalysis of He and Ne, with applications in measuring long half-lives (\( ^{238}\text{U}, ^{226}\text{Ra} \)), in determining the geological age of meteorites, and in analysing gas samples from the upper atmospheric obtained from US rocket flights. I think it fair to say that the half-life measurements produced little or nothing of publishable quality. The upper atmosphere studies lead to nine papers over five years, with another dozen on various aspects of the meteorite work in the same period. Paneth's early and decisive work on hydrides saw no revival in Durham, but studies of gaseous free radicals using tracer methods were carried out by R W Durham, A Hollis and H C Sutton, and a number of papers were published. The lack of a substantial effort in this area

* It is reasonable to lay much of the blame for this situation on F Soddy, who did little of significance scientifically after about 1920. Fleck discusses the life of Soddy, and Paneth offers a sympathetic comment which is included in ref. 15.
is difficult to understand, given the interest elsewhere in gas phase and solution thermolytic and photolytic decomposition reactions; I return to this point below. Nor did Paneth's continuing fascination and respect for the Periodic Table as the basis for understanding inorganic chemistry lead to any chemical research on (say) the heavy elements, whose chemistry offered many challenges at that time; even if one excludes neptunium and plutonium as being unavailable, the chemistry of uranium, thorium, polonium, bismuth and lead then had many lacunae which could surely have been the starting point for challenging inorganic and/or radiochemical problems.

The object of this analysis is to try to understand why Paneth's undiminished personal reputation did not carry that of the Durham Department into higher esteem. Many of the graduates of the Radiochemistry Laboratory went on to scientific careers of distinction, which shows what was being achieved in research training, but the Department itself did not move into national prominence until long after Paneth's departure, and of course as the result of work in areas completely unrelated to radiochemistry. I believe this is to be traced to the gulf between the main lines of research in Durham and those in other leading chemical centres in Britain. Age determinations of meteorites and studies of the upper atmosphere are extremely valuable in geo/cosmochemistry and meteorology, but can have had little impact in other Departments of Chemistry, or in industry, or indeed on the development of atomic energy in Britain. There can be no doubt, however, about Paneth's continuing stature at the international level; one witness to this is the stream of eminent scientists who chose to visit him in Durham, although few I think were "mainstream" chemists.20

In recognising Paneth as the undoubted leader of the Londonderry Laboratory for Radiochemistry, one should also ask 'what of the others?'. Graham Martin had a brilliant mind, with wide interests in chemistry, but throughout his life he appeared to be disinterested in carrying a research project to timely publication. Work on free radicals, on boron isotope separation, and on fission induced by fast neutrons each showed great promise, but all suffered from the drawbacks just noted. The construction, virtually by
hand, of a linear accelerator to produce high energy monoenergetic neutrons was itself a substantial achievement, but leads to the Johnsonian thought that the wonder was not that it was done, but rather that it was attempted at all, given the rather meagre resources available. Tordai's work on self-diffusion can be seen in the same light. K F Chackett was interested in the chemistry associated with nuclear reactions, I believe, but in any case his efforts in Durham centred on the helium analysis, as did in the main those of his successor S J Thomson.

There is an interesting flurry of correspondence in the Paneth papers\(^1\) for 1947, regarding the provision of various isotopic tracers of P, Fe, Co, Zn, etc, for the Medical Research Council. Chackett and Morgan were heavily involved in working up targets which had been bombarded in the US (Berkeley, Carnegie Institute?), and it seems possible from the timing that this also involved the services of Harwell staff sent to Durham for training. This apparently singular venture into such radiochemistry illustrates the talent and expertise available in Durham, and raises the question as to what might have been done in the wider use of radioactive tracers in chemistry. The type of work done then, and later, in other laboratories on such topics as separation procedures, electrochemistry, homogeneous and heterogeneous exchange reactions, diffusion processes and reaction mechanisms, to name but a few, found no interest in Durham, and again emphasises the failure to apply expert and scarce knowledge to potentially fruitful fields of chemistry. An examination of a Special Chemical Society Publication\(^{21}\) in 1949 shows quite clearly what was going on elsewhere, so that these present comments are not written with the sole benefit of hindsight.

An insight into this lack of chemical tracer work may perhaps be found in a letter from Paneth\(^1\) to von Hevesy (1945. 02.05), following a congratulatory cable to the latter on the award of the Nobel Prize for 1943. Paneth says, *inter alia*, that he entirely agrees with the view of some people in Sweden that the biological applications of isotopic indicators are "the application". He adds "Only after artificial radioelements, and isotopes of non-radioactive elements, were introduced by you in the study of biological reactions did the real fruitfulness
of the indicator method become obvious". Given this attitude, it is not surprising, though perhaps disappointing, that the use of tracer methods in chemistry were of such little consequence in the Londonderry Laboratory.

As Paneth's retirement date (1953) approached, the University obviously was faced with a dilemma. In the absence of someone of Paneth's stature with interests in radiochemistry to assume to the Department Headship, where lay the future of the Londonderry Laboratory for Radiochemistry? My own knowledge suggests that J S Anderson and R W Spence were approached, but in the event the Chair went to G E Coates, a distinguished worker in the rapidly expanding field of organometallic chemistry, and the progress of the Department since that time speaks to the wisdom of the selection. One can only imagine Coates' difficulties as the new Head in coping with the problem of an entrenched sub-Department, with its own budget, its own technical staff, and with scientific interests completely different from his (and from those of the organofluorine chemists). It is not surprising that when Martin went to Canterbury in 1964 (S J Thomson having returned to Glasgow in 1957), no attempt was made to fill the vacant Readership in Radiochemistry, and the Londonderry Laboratory was allowed to wither on the vine. As of this writing (October 1995) the huts of 1946-1947 still stand, a lasting tribute to that most permanent of university institutions in all parts of the world, the temporary building.

**Concluding comment**

Leaving aside the local peculiarities and personalities of the Durham story, one might ask why radiochemistry has virtually passed from the scene in chemistry departments in most western universities, to the extent that in many places it even forms no part of the undergraduate syllabus. There are, I believe, several parts to the answer. One is that the need for people competent in micro-techniques can be filled now, as it was at the early days of the development of atomic energy, without specialist prior training in radiochemistry, and another is that atomic energy agencies have developed their own training establishments. On a different tack, the instrument manufacturers learned long ago how to make user-friendly