work at the Courtauld Institute of Biochemistry in London for three years, and in 1949 came to the Chester Beatty Research Institute to apply his physical chemical knowledge and techniques to the study of DNA. This began an interesting and exciting 17 years, during which he studied many aspects of the chemistry of the components of the cell nucleus.

It was in 1953 that I first met J.A.V., and being a very junior member of the technical staff at that time I had little to do with him in an official capacity. However, within six months he was telling me in considerable detail why my geraniums were dying, and the need for repotting them at frequent intervals. Although I believe he was basically a shy man, he was friendly with all members of his staff, and never hesitated to allow the students a day off for the Varsity Rugby match at Twickenham.

He will be remembered also, I am sure, by his friends at the Institute, for his absent-mindedness. I have seen him return from an academic board meeting with two hats, one on his head and one in his hand, hotly pursued by the rightful owner of the second hat. On another occasion, deep in conversation with a colleague in a café, he complained about not receiving any change, only to find it in his coffee when he stirred it!

The stories about J.A.V. are legion. His lack of pens and pencils, the gunfight he caused in Chicago by losing his brief-case, and his very, very long pauses on the telephone before he answered "Butler here". He was in all ways the epitome of the "absent-minded professor".

We will also remember well his method of crossing the Fulham Road. Head down, deep in thought, shoulders slightly bent, straight across, ignoring all traffic. A screech of car brakes was often the sign that J.A.V. was crossing the road.

He was however, clear and precise about his research. Although he began by applying his knowledge of physical chemistry to the structure of DNA, he soon became interested in the wider aspects of DNA control and his interests lead him into the fields of DNA synthesis, RNA synthesis and the functions of the chromosomal proteins. This was, of course, in the fifties when molecular biology was in its infancy, and most people chose to forget that DNA was associated with an equal amount of protein.

He felt that the best strategic approach to the cancer problem was to understand the fundamental aspects of gene control and he directed his department to this end. However, he was keenly aware that this was a long term approach and once confided to me that he was sad we could do so little of immediate value.

Apart from his work within the Institute, his scientific interests were wide and varied. He edited *Progress in Biophysics and Molecular Biology* for over 25 years and his publications, apart from his research work, varied from a text book on chemical thermodynamics to a very successful series of books, written for the layman, on various aspects of cell and molecular biology.

He was appointed to the newly-established chair of Physical Chemistry at the Institute in 1952, and was elected a Fellow of the Royal Society in 1956.

After his retirement in 1966 he still took an active interest in the work and was often to be seen at scientific meetings. He frequently called in to see us and to hear about the latest work. His last visit was early this year, when true to form he left his brief-case in my room, fortunately remembering it before he left the building.

He was widely read, keenly interested in the arts and an amateur painter of some merit.

He married Margaret Lois Hope in 1929, and as all his friends who visited his home knew well, he had a very happy and contented domestic life. His three children, two sons and one daughter, all followed his lead and have made careers in various other scientific disciplines.

E. W. Johns

F. H. Ludlam

F. H. LUDLAM, Professor of Meteorology and Head of the Atmospheric Physics Group at Imperial College, London, died on 3 June 1977 aged 57 years. Frank Ludlam studied clouds. His descriptions of the movement of air and water vapour and rain through clouds were so intense and graphic that one began to suspect that he had some supernatural power to actually identify himself with a cloud.

He saw so much. He would come into a lecture with perhaps 20 slides then spend all his time talking about the first, frequently drawing back from removing it because some new feature had just caught his attention.

Having made fundamental contributions to the microphysics of clouds during the 1940's while working at the Meteorological Office, he moved to Imperial College and away from the study of the details of processes towards the study of the organisation and dynamical structure of clouds. This philosophy culminated in his now classical description of the motion of air in severe thunderstorms. Like all grand ideas this one could, in retrospect, have been seen coming for years, but Ludlam had it. He discarded what he called 'the haystack theories' of storms. These were illustrated by diagrams, and thinking, with arrows all over the place. He replaced it by a streamline theory showing where the air and water came from and where it went to. He took a certain delight in the fact that, working on a tiny budget, he made a very satisfying advance in a subject of great economic importance.

Subsequently his interest shifted progressively to phenomena of larger scale. For his inaugural lecture in 1966 he showed that weather systems could be seen in a new perspective by examining them relative to axes moving with the system: again almost identifying himself with the phenomenon. This was his way of using the abstract concepts of pattern movement as distinct from pattern development developed by his penetrating contemporary, Eady. But whereas Eady's concepts were based on distinction of mechanism and arose from mathematical elegance and clarity, Ludlam made them lead to pictorial and functional beauty.

No man showed better the false distinction between artist and scientist. To him good science made a pleasing picture. He delighted in the impish gesture. Few present at a lecture to the Royal Meteorological Society (on three-dimensional data analysis) will forget the impact made when he projected a slide of a Rodin nude (very female) in the middle of a masterly description of the techniques of synoptic analysis.

In his magnum opus entitled *Cloud Physics* to be published by the Pennsylvania State University Press, he summarises his views on atmospheric phenomena from a few microns in scale to 40,000 km. He was concerned with the notion that conventional physicists, wedded to the controlled experiment, did not understand the essence of meteorology as he saw it. What he did see was an immensely elaborate system with complex feedback. The essence was the feedback and the uncontrollability of the meteorological experiment. He was the privileged onlooker. He even took lightly some of his beautiful and fundamental work on cloud microphysics and said it did not really matter if it were wrong: the atmosphere would find some way of fulfilling a more over-riding purpose whatever the details of the processes.

Such an integrating philosophy is likely to be fundamental for the study of complex systems like the atmosphere and we are privileged to have had so talented an exponent of it.

John Green