Francis Peyton Rous, 1879-1970

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FRANCIS PEYTON ROUS
1879-1970

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Before PEYTON ROUS died at the age of ninety he had been acclaimed as an outstanding leader in the field of pathology and particularly in cancer research. Before he received the well-merited award of a Nobel prize in 1966, he had worked unremittingly for more than fifty years, piling fact upon solid fact; yet for many of those years his outstanding discovery of a virus causing a tumour in fowls was disregarded. Rous, however, was throughout his working life constantly excited by every new fact he brought to light and this made his life a truly satisfying one.

There is no evidence of any scientific achievement among his forebears. His great-grandfather had come to America from Henham in Suffolk in the early 1800s. His father, Charles Rous, was a grain broker in Baltimore; he had married Frances Anderson Wood from Virginia.

Peyton was born on 5 October 1879. When he was eleven, with two younger sisters, their father died. Their mother, though she had been brought up in Texas, remained in Baltimore, though with little money, so that her children could have the best possible education. So Peyton was taught at a public school and a public high school in Baltimore and from there he obtained a scholarship to Johns Hopkins University and Medical School. While an undergraduate he earned some money—in fact five dollars—by writing articles for a Baltimore newspaper on 'The Flower of the Month'. His ambition was to be a naturalist.

During his second year an infection acquired during an autopsy led to a tuberculous inflammation of an axillary gland; this required excision. So he was sent for a year to work as a cowboy on a ranch in Texas. He was described at this period as being small, delicate, eager and immature.

On his return to Baltimore he was a member of the last class which studied under William Osler. Later, Rous held a medical internship at Hopkins. He was perhaps rather overwhelmed by the galaxy of medical talent there. Anyway he soon realized that clinical medicine was not his métier and he obtained an assistantship in pathology under A. S. Warthin at Ann Arbor, Michigan. Here he obtained a sound grounding in pathology, receiving a salary of $900 a year. His pathological training was further developed by a year under Schmorl in Dresden. Unfortunately on his return home he developed pulmonary tuberculosis and was sent to the Adirondacks for a
period to recover. This had the desired effect and the tubercle bacillus troubled him no more. A few papers published during his early years dealt with cerebrospinal fluid (1, 2) and lymphocytes (4, 5, 6).

Soon after his return to work in Baltimore, there came an opportunity to work at the Rockefeller Institute in New York. Rous went to see Simon Flexner and was told that the work would be in the field of cancer research. He accepted, despite some discouragement. One of his old chiefs, Warthin, had earlier told him that accepting a Rockefeller fellowship meant receiving tainted money. The other, W. H. Welch, had told him: 'Whatever you do, don't commit yourself to the cancer problem.'

**Fowl tumours**

In 1909, soon after Rous had begun work at the Rockefeller Institute, there was brought in for examination a Plymouth Rock hen bearing a 'large irregularly globular mass' projecting from the breast. Some to whom it was shown evinced no interest, but Rous seized upon it avidly. A biopsy revealed that it was a spindle-celled sarcoma. Minced material was injected into young Plymouth Rocks of related 'pure blood'; a similar sarcoma developed in one of them and was subsequently further transmitted in series. This was possible at first only in genetically related birds but later transplants 'took' in fowls of other breeds. This was the first record of a propagable bird tumour, and it is noteworthy that in his first paper about it (15), Rous wonders whether 'our conception of tumor behavior . . . has been too narrow' and mentions that no attempt had been made to transmit with cell-free material. He adds: 'there is no reason to suspect that the neoplasm will differ (in this respect) from the better-known tumors of mammals'.

Little more than a year had passed, however, before he could record that the neoplasm had greatly increased in malignancy on passage and that cell-free filtrates would give rise to tumours (17, 22). This was a wholly unexpected finding. True, Ellerman and Bang had, not long before, transmitted a fowl leukaemia with filtrates, but the discovery was stillborn and excited no great interest, as leukaemias were at that time not thought to be related to cancers. Rous's finding was, of course, in flat contradiction to views then current about cancer, and for years afterwards pathologists refused to consider that the discovery was relevant to the problem of cancer. One pathologist told Rous: 'Look here, young man, that can't be a cancer if you found its cause.' Some argued, like this, that the growths were not really cancers; others that the infective agent was not a virus or else that tumour cells had somehow penetrated the filters.

Dr Charles Oberling later wrote as follows concerning this period in cancer research: 'Tumor pathology was then completely under the spell of the German school of pathologic anatomy which, probably as an aftermath

* The numbers in parentheses refer to the numbered items in the Bibliography at the end of this memoir.
of the antagonism between Robert Koch and Virchow, was utterly opposed to any theory of an infectious origin of cancer. So were others, like Jensen in Denmark, who for many years had tried to transmit tumors with cell-free extracts... Reporting the consistent failure of all his attempts, he concluded that his time had not been completely wasted inasmuch as these experiments had at least plainly demonstrated that there could be no question of an infectious origin of cancer. And suddenly, in opposition to all these dignified and bearded Herren Professoren who firmly believed what they said, rose the voice of a young American who claimed to have transmitted by a cell-free filtrate a neoplasm—a chicken sarcoma. Of course this could not be true, and for years they did not even try to repeat his experiments. But Rous, working with J. B. Murphy and W. H. Tytler, and later with Linda Lange, steadily pursued his studies of the tumour and built up a convincing body of evidence. It was shown that there was nothing in the general behaviour of the tumour to show that it differed from other cancers, save in its possessing a filterable cause (23, 24, 25). The tumour could, as could other tumours, invade tissues and by penetrating blood-vessels, liberate cells into the bloodstream. Metastases accordingly occurred quite commonly, and these were apparently the result of lodgement in capillaries of cancer cells, as with mammalian growths; they were not caused by immediate infection of new cells by a virus. Further study of the agent showed that it had the properties of a virus and before long Rous was referring to it as a virus (26). Rous’s colleague, Murphy, did not agree with such a conclusion and coined for the agent the term ‘transmissible mutagen’; but he soon parted company from Rous and pursued independently other studies of cancer.

Before long many other fowl tumours had become available for study. Several could be transplanted and a number were shown to have filterable causes. One of these was an osteo-chondrosarcoma; filtrates of this reproduced the same histological picture in other birds (30). Two other tumours were ‘sarcomas of intracanalicular pattern’, described also as ‘rifted with blood-sinuses’ (35). It was found that growths could be produced in the embryonic membranes of fertile eggs and in the body of a chick embryo itself by injecting either tumour cells or filtrates (28). This was the first use of the chick embryo, now so widely used, for this type of research.

Studies revealed that immunity was of two kinds, resistance to transplantation of the tumour cells and immunity against the virus (33). The two forms of resistance were found together in the occasional birds in which the tumour regressed, but they could exist independently of each other. Incidentally, Rous had, earlier, been among the first to show that much published work on transplanted tumours was based on a fallacy. It was not realized that the immunity being studied and the ‘cures’ being reported were nothing to do with cancer as such, but only with transplantation of foreign cells and resistance against them (14). Rous’s last paper on fowl tumours for many years described production of antisera to the virus by immunizing geese and rabbits (60).
Rous made very important contributions to cancer research in later years; but undoubtedly his big discovery of the fowl tumour virus was something quite revolutionary. The ‘Rous sarcoma’ has been studied all over the world and these studies are continuing to yield tremendous and often unexpected dividends. Rous himself always referred to it as ‘chicken tumor No. 1’ and disapproved of use of his name to designate filterable fowl tumours of various origins.

It is remarkable in retrospect that the importance of his discoveries was disregarded for so many years; the tumour was considered as merely a pathological curiosity. After some years, even his chief, Simon Flexner, began to feel that Rous was not getting anywhere. He himself was, of course, well aware that he had discovered something fundamental and was naturally sad and disappointed that others could not see it too. A colleague writes that he was nevertheless ‘miraculously unembittered’. He was greatly pleased when workers in Britain, chief among them W. E. Gye, took up the study of the fowl tumours in 1925, and re-awakened interest in them. He later acknowledged: ‘I was saved by an Englishman.’ Gye’s main hypothesis, relating the Rous virus to cancer in general, proved to be wrong; nevertheless knowledge was carried some steps further forward, and interest in the Rous sarcoma was kept alive.

Studies of Blood and Liver

Meanwhile Rous himself turned for some years to other subjects. It was now the beginning of the first World War and there were matters of immediate practical importance to be dealt with. Working with J. R. Turner he studied the preservation of living blood cells in vitro. They found that cells of various species differed in their resistance to mechanical injury and that they could be protected if $\frac{1}{3}$ per cent of gelatine was added to the suspending Locke’s solution (44, 46). This, however, was not the whole answer to the problem of long-term preservation. It was finally found that ‘in a mixture of three parts of human blood, two parts of isotonic citrate solution and five parts of isotonic dextrose solution, the cells remain intact for about four weeks’. Rabbit cells so preserved could be kept for two weeks and successfully used to transfuse those animals (47). The ‘Rous-Turner solution’ is to this day the basis of suspending solutions for use in many human transfusions. Rous continued his studies of blood cells in collaboration with O. H. Robertson (51, 52, 53) and before long, in 1917, Robertson was operating the world’s first blood bank in Belgium near the front line.

A by-product of this work was a description of a method for obtaining suspensions of living cells from the fixed tissues (48); this was achieved by the use of trypsin and the technique is of great practical value today to tissue culture workers and others.

Rous’s work on blood preservation and destruction led naturally to the next phase of his activities, the study of the liver and gall-bladder. He
transfused rabbits by day-to-day injection of 10 ml, later 15 ml, of homologous blood. The plethoric condition thus induced was accompanied by the deposition in the liver and elsewhere of haemosiderin (57). The condition closely resembled haemochromatosis of man. It was found that a feature of this was the excretion of haemosiderin by the kidney (58). Finding deposits of the pigment in the urine led him, as he records in a letter, to the emission of 'loud whoops at intervals'. It appeared reasonable that search for such urinary deposits might prove useful for the diagnosis of haemochromatosis and even of pernicious anaemia.

For the next three years he collaborated in studies of the liver with P. D. McMaster. They could obstruct the bile ducts from three-quarters of the liver of a dog without evidence of accumulation of bile pigments (67). Previous work on the bile had doubtful value because operative procedures were followed by infection and by permitting surgically treated dogs to lick the bile coming from artificial fistulae. Rous and McMaster perfected a technique for permanent sterile draining of bile ducts and collection of bile and thus avoided previous errors (75). Bile from different parts of the liver was similar in pigment content. Collection of all bile led, as expected, to fall in pigment content, but this was not progressive—an indication, confirmed by later work, that much bile was re-absorbed from the gut and utilized afresh. This entero-hepatic circulation was the subject of a later paper (81). But the most important work concerned the functions of the gall-bladder. This could no longer be considered as a purely passive receptacle for storing bile; it was an important organ for concentrating bile, being capable of reducing its volume by nine-tenths within twenty-four hours (68). This ability is the basis of tests in use today, involving the recognition of the success or failure of the gall-bladder to concentrate dyes; this can be visualized by X-rays. Thus disease of the gall-bladder or presence of gall-stones may be revealed and decisions reached as to the desirability of operation.

Further studies on bile now became possible, including the effects of diet, exercise and liver disease (77, 93); in particular it was shown that bilirubin had its sole origin in the breakdown of haemoglobin (83). A number of papers dealt with the conditions necessary for the formation of gall-stones (75, 82, 85-87).

Rous's next field of enquiry concerned the relative reaction of living tissues: on this he and H. P. Gilding wrote a series of thirteen papers. I well recall a moment in 1924 when Rous's first papers about this had just appeared in print; Baird Hastings came joyfully into the sitting-room of the Rockefeller hospital exclaiming: 'Now I can speak. He's scooped the lot!' Rous had indeed devised a technique whereby vital dyes, particularly phenol red and brom cresol purple, could be given to living rats; it was thereby disclosed how the pH of the tissues varied from one part of the animal to another (88, 89). It was shown how local ischaemia caused by various functional conditions could increase the acidity of the tissues,
particularly the muscles. The pH of the blood was commonly unaffected, or in certain circumstances might be more alkaline while the outlying tissues became more acid (95).

This work led on to studies of the gradient of vascular permeability and a series of four papers with Gilding & F. Smith (110-113). They showed that permeability increased along the course of certain capillaries so that the opportunities for benefit from blood-borne fluids became equal for all tissues.

At about this time he carried out experiments which have been described as a 'high peak in lab. virtuosity'. He wished to obtain suspensions of the Kupffer cells of the liver which have the property of taking up particles of foreign matter from the circulation. He therefore gave suspensions of magnetic iron particles intravenously to rabbits and dogs and after a few days washed out the liver and from the cell-suspensions so obtained sorted out the iron-bearing Kupffer cells with an electro-magnet (118).

**Rabbit warts**

Then came a development which, almost overnight, changed the whole course of Rous's work and was finally of much significance for cancer research. A junior colleague, Richard Shope, working at the Rockefeller Princeton laboratories, had discovered warts on wild cottontail rabbits, and these proved to be due to a virus. This was readily transmissible to domestic rabbits, but it could not, in them, be easily passed in series. This matter and other aspects of the warts were profitably under study by Shope, when he found evidence that one of the warts in a domestic rabbit had acquired the characters of malignancy. Now Shope was more of a field virologist than a dedicated pathologist; so he offered his material to Rous. Rous was most delighted and grateful. He was back in cancer research where his heart lay. He wrote that he was once more sailing free on the broad ocean. The Shope papilloma and the cancers derived therefrom continued to occupy him for nearly twenty years. His chief collaborators in the investigations were J. W. Beard, John G. Kidd and W. F. Friedewald. About fifty papers about these tumours were published, nearly all in the *Journal of Experimental Medicine*. They were freely illustrated with excellent photographs and microphotographs; many of them also with silhouettes drawn to scale to show the progress of the growths. Descriptions were detailed, for Rous liked to have his observations fully documented, and the accounts were often full of vivid imagery.

Rous's early discovery of the fowl sarcoma was, of course, outstanding and was followed up by work which consolidated knowledge of the facts. But the studies of the rabbit tumours, carried out over many years, were, if less dramatic, yet equally important in the long run, for they helped in the formation of Rous's ideas as to the development of cancer, ideas which are now part of our basic knowledge of the disease.

The first papers (117, 120-122) showed convincingly that the papillomata
had the characters of a true tumour. When warts were removed and planted into the muscles or elsewhere within the same animal, they grew progressively, invading surrounding tissues and leading, not infrequently, to death. Secondary nodules were found in lungs and lymph-nodes. Virus could be recovered from the growths in the case of cottontails, usually not in domestic rabbits. The papillomata occurring on the skin could also be induced to show malignant characters. The dye, Scharlach R, injected beneath them, caused them to become invasive and to penetrate blood vessels. If they were covered with a layer of collodion so that they could not grow outwards in their usual manner, they grew downwards both by expansive growth and by invasion. As with the fowl sarcomata, they behaved in all respects like the generality of mammalian growths except in having an extrinsic cause. Not even this differential criterion applied to those growths in tame rabbits from which no virus could be isolated.

The warts which became malignant spontaneously were observed with meticulous care (125). While the original papillomata behaved in a rather uniform manner, the subsequent changes were diverse. They progressed from bad to worse by a series of steps, and these differed in various parts of the tumour. There might even be regression at one place with increasing invasiveness elsewhere. Some growths differed little histologically from their original papillomatous state, yet could give rise to metastases; others were highly anaplastic.

Rabbits bearing the derived carcinomata had virus-neutralizing antibodies in their sera, just as had those with warts only. But, as Rous had shown for other viruses (116), such antibodies were commonly powerless to destroy viruses residing within cells; these served to protect them from its action.

The cancers were, in general, not transplantable to fresh hosts, as is not surprising, seeing that inbred animals were not available. They did, however, go in series when inoculated into suckling rabbits and in these they were often remarkably malignant and invasive (169). It was thought possible that with such very active growths it should be possible to demonstrate infectious virus, yet such proved not to be the case. A transplantable tumour was, however, obtained from one cancer, and, by 1940, had been passed serially through fourteen rabbits (143). It was called V2, but when that designation had become associated, at the end of the war, with the German rockets used to attack Britain, the name was changed to VX2. Though no virus was directly demonstrable in the transplanted tumours, the rabbits bearing them regularly had complement-fixing antibodies in their sera. Rous naturally speculated as to whether the virus was present in a masked condition yet still having a causative role, or whether it was a mere passenger, as other viruses in other cancers often are.

This state of affairs persisted during transplantations carried out during more than three and a half years. Then, very surprisingly, after a further four and a half years, rabbits of the forty-sixth and subsequent transplant generations were found to be fully susceptible to the papilloma virus and to
have no antibodies against it (171). Was the virus only a passenger after all? Or had it become more closely integrated with the cell than ever? Concurrent work with polyoma virus in other laboratories rendered this explanation not too fanciful. Rous records: ‘In one respect a small gain can be registered . . . the cancer . . . has come to resemble the classical malignant tumors in giving no recognizable sign of the nature of its actuating cause.’ Other transplantable cancers were also obtained and from some of these the immunizing viral antigen did not disappear as it did from VX2.

**Virus-warts and tar-warts**

It was natural for Rous to compare the virus-induced warts with those elicited by chemical carcinogens (140) and evidence was soon uncovered of remarkable synergism between the papilloma virus and tar. Twice weekly tarring of the ears of rabbits led after some months to the appearance of numerous warts, but these normally regressed when tarring was stopped. When papilloma virus was given intravenously to rabbits so treated, the virus was localized to the tarred skin. Moreover, instead of the usual virus-warts of similar character, there arose ‘a considerable variety of tumours, both benign and malignant’, and these ‘exhibited their peculiar character from the first’. The change to malignancy was ‘telescopied into a few days instead of taking months or years’. It was concluded that the virus was unchanged but behaved abnormally when it infected changed cells (137, 139). When tar warts were minced and injected into the same rabbits intramuscularly, they failed to progress; but when the mince was soaked in papilloma virus before the injection many of them grew as virus papillomas and a few as cancers (142).

Some years later experiments of a different type were carried out. The skins of rabbits were rendered hyperplastic by painting with turpentine; they were then scarified, inoculated with papilloma virus and covered with a dressing containing a chemical carcinogen, either methyl-cholanthrene or 9:10-dimethyl-1:2-benzanthracene. The dressing was left on for five to seven days. The resulting papillomas were fewer and poorer than those on rabbits receiving virus only. After some months, however, most of them rather suddenly became carcinomatous, as the controls did not. ‘The virus and the hydrocarbons acted jointly and in their carcinogenic capacities’ (167).

Before discussing what conclusions Rous drew from all this work, it is well to describe some experiments carried out in mice. Rous & W. E. Smith (155) minced up the skin of embryo mice and inoculated the mince into thigh muscles of adult mice along with Scharlach R and methyl-cholanthrene in olive oil. Cysts were formed and the epidermal cells in the cyst lining were well exposed to the carcinogen. Papillomas and carcinomas arose within a few weeks and some of the latter were transplantable to syngeneic mice. It was thus shown that embryonic skin, and later, stomach epithelium were susceptible to malignant change. It was thought ‘impossible to suppose’ that a virus was concerned. Another carcinogen, urethane, was found capable
of inducing pulmonary adenomata in newborn mice when the drug was
given to their mothers in the latter half of pregnancy (163). A decade later,
papillomas were induced in skins of mice with tar and transferred to the
subcutaneous tissues of suckling and weanling mice. They proved to be
serially transplantable in syngeneic mice, often showing malignant changes
but growing so large as to kill the mice even when they did not so change
(174). It was concluded that, as with virus-warts in rabbits, it is local
defences which in the ordinary way stop superficial warts from progressing
indefinitely. Experiments in collaboration with K. R. Dumbell (173),
afforded evidence that application of chemical carcinogens to existing
spontaneous tumours did nothing to enhance their malignancy.

Nature of cancer

Rous came to some very important conclusions about the nature of cancer,
largely as a result of his work with the warts elicited by viruses and by
tarring. It became clear that the tar warts and cancers derived from them
were true tumours; yet the neoplastic change did not ensure that they had
a permanent competitive advantage over normal cells in all circumstances.
Thus, when the stimulating effect of tar-application ceased, they commonly
regressed and the skin apparently returned to normal. Yet there were still
present latent neoplastic cells which further stimulation could activate.
Cells could even acquire a neoplastic character without ever revealing the
fact by developing into a tumour. This was revealed by experiments in
which tar was applied to the ears of rabbits for a shorter period than is
necessary to elicit growths. Holes were then punched in the ears under
anaesthesia, to stimulate the cell-proliferation of wound-healing. This pro­
cedure evoked the appearance of tumours and brought out ‘the need for a
sharp distinction between the forces which induce neoplastic change and
those which determine, or prevent, its realization as a tumor’. So was
reasoning led on to a paper on ‘conditional neoplasms and subthreshold
neoplastic states’ (144). The neoplastic change is thought to ‘endow cells
with powers which they can exert only in favoring conditions’.

Three years later came Rous’s classical paper, with Friedewald, on
‘initiating and promoting elements in tumor production’ (152). Chemical
carcinogens can initiate, but promote only as long as they are repeatedly
renewed, and other things may substitute for them as promoting agents.
Some viruses may merely initiate: those which both promote and initiate,
perhaps combining these activities into one operation, are called by Rous
‘do-alls’. He repeatedly draws distinctions between chemicals which start
something and then disappear from the scene, and agents such as the virus
causing fowl tumours which persist: the latter are evidently continuing causes
of malignant growth and Rous refers to them as the only direct or proximate
causes of tumours which we know. In contrast are the ‘tumors of unknown
cause elicited by tarring’ (140). When, after persisting in a masked state
for many years, the papilloma virus apparently disappeared from the
VX2 carcinomata, Rous concluded that the virus must, for a long time, have been only a casual passenger (171). He might have thought otherwise today, now that a virus genome has been recognized in certain tumours although infectivity has been lost. Perhaps the wart virus was, after all, more than an initiator.

Though he was so essentially a laboratory worker, Rous was familiar with the clinical aspect of tumours and frequently draws analogies between his own findings and clinical experiences in the field. Thus, warts arising very late after tarring are compared with deferred cancer in man (166).

**Background of the work**

This account of Rous's life-work would be incomplete without reference to his family background and methods of work. In his earlier years at the Rockefeller Institute he enjoyed the company of a house-full of young doctors. Then in 1915, when he was thirty-six, he married Marion Eckford de Kay. This was at a time when the first flush of excitement over the fowl tumours was past and he was perhaps disappointed that its significance was not understood or appreciated. The Rous's had three daughters, and enjoyment of family life was of great importance to him. He used to take two months' holiday in the summer; fishing was a favourite hobby. After World War II a summer home was acquired at West Cornwall, Connecticut, and there he could enjoy gardening as well as fishing.

He was a tremendous worker and would bring back home piles of manuscripts to study in the evenings and even to take away on holidays; much of this concerned the *Journal of Experimental Medicine*. Often he slept badly and would keep by his bedside a pencil torch and note-pad on which he could write down ideas which came to him in the night.

Dr John Kidd was a close associate and friend over many years. He has written that he was impressed by Rous's naturalist's attitude to the phenomena he was studying. During the work on the Shope papilloma 'he would look close at the growth with his naked eye'; palpation followed and then 'as you watched him you soon perceived that the growth was an evolving biological entity ... and you were led to wonder how it had come to be what it was, and to speculate on what the future might hold'. Rous would often draw free-hand sketches of the tumours, or would dictate to his secretary 'elaborate, precise, vivid descriptions of the growths'. If of sufficient importance, the rabbit would be taken in a laboratory truck to the illustration department to be photographed with meticulous attention to detail. Equal care would be lavished on any microphotographs deemed necessary.

Writing demanded equal care and he would commonly spend a morning from 10 to 1.30 concentrating and writing steadily. If a talk was to be given, he would trouble himself greatly to make sure that it came up to the standards he had set himself. After an afternoon's work in the laboratory he would, at 6.30, walk the fifteen or so blocks to his home, and this walking he kept up until he had reached a great age.
Another of his assistants, Dr Stanfield Rogers, wrote: ‘A man of many
facets, he was the most interesting and delightful human being I have
encountered. At the same time he could be the most difficult—this latter
was perhaps his most valuable aspect to young people he was bringing along
into research. Almost obsessed with new discovery, he had little use for
anything less. This feeling was somehow “transduced” to his associates
though none have been his productive equal. Perhaps the most characteristic
of this giant of a man was something he told me when he was about eighty
years of age: “If I were only sixty I would learn biochemistry.”’ Dr James
Stuart Henderson was his associate during the latter part of his working life.
He has written that: ‘His unique mastery of the awesome volume of facts
about carcinogenesis, both chemical and viral, was unchallenged. His skill
in predicting and depicting order in it was unerring.’

Rous spent sixty years at the Rockefeller Institute, now the Rockefeller
University. He was made a full member in 1920 and a member Emeritus
in 1945. After that, his ostensible retirement, he continued to work in the
laboratory and about sixty of his 300 or so papers date from that ‘retirement’
period. In 1926-1927 he spent a sabbatical year at Trinity Hall, Cambridge,
where Professor H. R. Dean, the Master, was an old friend, and he made a
number of return visits there. During his later years Rous derived great
pleasure and interest and was, moreover, of much help to others in his
capacity as adviser to the Sloan-Kettering Institute and other bodies
concerned with cancer research.

Besides his insatiable curiosity about the facts of nature, Rous was keenly
interested in people, always wanting to know more about them. Any
anecdote about his friends or their families would be greeted by ‘Now, isn’t
that interesting?’ In consequence, he and Marion always seemed to have
stored away a wealth of information about scientific people, whether
American or British or from elsewhere.

Soon after he came to the Rockefeller Institute, Flexner called upon
him to be his assistant in editing the Journal of Experimental Medicine. He
was appointed co-editor in 1921 and served as editor for forty-six years in
all, and up to six weeks from his death. The ‘J.E.M.’ was part of his life.
For most of that time he read every word of papers submitted for publica­
tion. Dr O. H. Robertson wrote: ‘That he gave much time and thought to
his editorial duties many of us, whose returned manuscripts were covered
with innumerable notes in his fine but ultimately decipherable handwriting,
can testify.’ Rous’s own literary style was as different as possible from the
often dull and stereotyped scientific prose of today. His descriptions are
vivid, bedecked with apt comparisons and his discussions and conclusions
imaginative. He had some pet phrases: ‘with result in . . .’ recurs frequently
and it seemed at times that a touch of Rous could be perceived in not a few
papers in the J.E.M. by other authors.

The bibliographical list at the end of this memoir includes the most
important of the accounts of his scientific work; there are also not a few
general articles on the problem of cancer (135, 146, 147, 149, 150, 159, 170, 172, 175, 177, 179, 184, 185, 190). Rous also wrote obituary notices of colleagues and contemporaries (131, 164, 176, 178, 186), notably Simon Flexner (165) and Karl Landsteiner (161), and there are a few short, rather light-hearted, articles bearing such intriguing titles as ‘The lamentable decline in self-satisfaction’ (160), ‘The disagreement amongst doctors’ (181), ‘Henry James and the mouse’ (182).

In his later years Rous received many honours as the following list testifies:
Honorary degrees from the Universities of Cambridge, Michigan, Yale, Birmingham, McGill, Chicago, and Zurich.
Member of National Academy of Sciences, Washington.
Member of American Philosophical Society.
Foreign Member of the Royal Society.
Member of Royal Danish Academy of Sciences.
National Medal of Science.
Cleveland Medal of American Cancer Society.
Gold-headed cane of the Association of Pathologists and Bacteriologists.
United Nations Prize.
Gold medal of the Royal Society of Medicine.
Albert Lasker Award.
Landsteiner award of the American Society of Blood Banks.
Distinguished Service award of the American Cancer Society.
Kovalenko award of the National Academy of Science.
Ehrlich-Darmstaedter Prize.
Kober Medal of the Association of American Physicians.
Benter Medal and award of the University of Texas.
Walker Prize of Royal College of Surgeons.
John Scott Medal and award of the City of Philadelphia.
Finally, in 1966, came the Nobel Prize for Medicine, which he shared with Charles Huggins.

Rous died of abdominal cancer after a fairly short illness, not long after his ninetieth birthday. He is survived by his widow and three daughters. The eldest, Marion, is the wife of Alan Hodgkin, P.R.S. The others are Ellen de Kay and Phoebe, widow of Thomas Wilson. There are six grand-children.

In his youth Rous had red hair, as befits his name, but he sometimes referred to himself as ‘the last of the red Rouses’. He had not, however, the brisk temper sometimes associated with red hair. He did not always suffer fools gladly, but was invariably kind and courteous to his colleagues and co-workers.

I am most grateful to Mrs Marion Rous for help in preparing this memoir, and for the same reason to several colleagues mentioned in the text.

The photograph has been supplied by Mrs Rous and the Rockefeller University.

C. H. Andrewes
Francis Peyton Rous

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