OBITUARY NOTICE OF DECEASED MEMBER

Arthur John Hibbert Tomlinson
31 August 1916—17 November 1967

PLATE XXIII

The death of John Tomlinson came as a shock to his numerous friends and colleagues. Many of them had not even realised that he was ill, and few of them knew in August that he had only two or three months to live. In the early part of July he took part in a seminar at the London School of Hygiene and Tropical Medicine. This was sponsored by the World Health Organization for senior bacteriologists from developing countries wishing to study the organisation and management of laboratory services. He was in great form, discussing such subjects as laboratory hazards, the training of technicians, and assessment of the quality and quantity of work done. He conveyed his exuberance to his listeners, and invited them along to his own laboratory on a Saturday morning to demonstrate to them the methods he was using. Only a few days later he began to feel unwell. A tentative diagnosis of hepatitis was rudely shattered when the following month he was operated on at St George's Hospital and found to have extensive metastases in the liver. He came home in the middle of September knowing that his work was finished, yet manifesting in his approach to death the same logical attitude that he had maintained throughout his life. Cheerfully and without fuss he set in order his affairs, sold his house, parted from his treasured alpine plants, discussed topics of scientific interest with his visitors, said goodbye to his friends, and died peacefully in the local hospital on 17 Nov. 1967.

A memorial service was held for him on 1 Feb. 1968 in the parish church of St Marylebone—the church in which Robert Browning was married, and where Sir Henry Souttar, a surgeon from the London Hospital, of outstanding breadth of culture and technical ability, was for many years Vicar’s Warden. The respect he commanded was evident from the large number of scientific and horticultural experts who met together to pay him their last homage. And he, what would he have thought of it all, realist as he was? Probably he would have voiced the cry of the Irish Rugby Captain when the ball had been kicked into a neighbouring stream “Come on boys; never mind the ball, let’s get on with the game”. Or would he, more seriously, have exclaimed with Alaric the Visigoth:

“When I am dead, no pageant train
Shall waste their sorrows at my bier,
Nor worthless pomp of homage vain
Stain it with hypocritic tear”.

If so, he would have been wrong, for our tribute to him was genuine and deserved.

Near his end he said "If you have to write an obituary of me, please say as little as you can". That was true modesty and is what he wished. But for the sake of posterity, as well as for the present, the wishes of the dead cannot always be respected. It is right that his accomplishments should be described, and it is right that something of the nobility of his character should be put on record.

Tomlinson was born in August 1916 in Zanzibar, where his father Thomas Symonds Tomlinson, who was then Assistant Judge in His Britannic Majesty's Court, was later to be made Chief Justice and knighted. He was brought home by sea round the Cape to escape the German submarines in the Mediterranean, and arrived in England in November 1917. His mother settled at Grange-over-Sands in Lancashire, and there she had four further children, all daughters, the last one ten years younger than her firstborn. After five years at a preparatory school in Cheshire, Tomlinson entered Marlborough with a junior scholarship in 1929. I know little about his stay there, but I suspect he was not the picture of the perfect public school boy. He was too unconventional, and too strong-willed to be forced into any narrow groove. However, he did well, played cricket, was captain of the 2nd fifteen at rugger, and left in 1935 with a scholarship in Natural Science for Cambridge.

With a keen mind and a roving disposition it is perhaps surprising that he did not follow his father into the legal branch of the Colonial Service. Probably two reasons account for this. Firstly, his father foresaw that the days of the Colonial Service were numbered and that his son would be wiser to take up some other profession. And in the second place, Tomlinson's early wanderings in the Lake District had instilled into him a love of natural history that predisposed him towards a scientific career.

At Cambridge he was a major scholar at Clare College. He took the first part of the Natural Sciences tripos in 1937, and gained 2nd class honours in the second part in 1938, when he was awarded his B.A. He then moved on to the London Hospital for his clinical work, and qualified M.B., Ch.B. in 1941.

Of this time of his life Dr Hampson writes: "We were close friends as medical students and the image of him that springs at once to mind is of a genial presence, a unique and infectious laugh and a man delighting in conversation. If the company was good the clock would be ignored; to end the talk and go to bed required a positive effort on someone else's part; the initiative would seldom come from him.

"When he came down from Cambridge to work at the London I think we all knew that he would make a place for himself in one of the medical sciences, but it was only on closer acquaintance that we learnt to value his gifts correctly and to realise how far above most of us he stood. He found a good deal of clinical medicine uninspiring in those days. He could be vitriolic about some of the sillier conventions of the times, such as that which required the medical student to attend long sessions in the operating theatre when it was quite likely that he would do no more than catch a glimpse of the surgeon's hands and see
nothing at all of what he was doing. But his interest was aroused immediately whenever what he saw in the wards could be related to some underlying principle. I think he felt that, often enough, no more than lip service was paid to the truth that physiology and pathology are at the heart of medicine, but anyone who behaved as if he really believed this was sure of John’s attention. Even so it was always unlikely that he would choose to follow a career in clinical medicine. It implies no lack of humanity—for no one who knew him would ever think that—to say that his professional drive was predominantly scientific and once the intellectual diagnostic puzzle had been solved and its ramifications explored he was ready for the next thing”.

After the usual house appointments Tomlinson joined the Cambridge laboratory of the Emergency Public Health Laboratory Service (EPHLS) at the end of March 1942. There he came under the inspiring leadership of Alan Downie, who provided for him just the right atmosphere in which his talents could blossom. His first bit of work, done in collaboration with the late Dr Haines of the Ministry of Food’s bacteriological laboratory at Cambridge and thirteen other EPHLS laboratories, was on the bacteriology of spray-dried egg. In 1941 the supply of animal protein to the population was getting very low, and Lord Woolton, on the recommendation of his scientific advisers, decided to import spray-dried egg from the United States and elsewhere. We warned him that this move would not be without danger. In view of the fact that fowls were the biggest reservoir of salmonellae in the animal kingdom, that the egg mixture would inevitably be contaminated from infected faeces on the shells, and that the heat treatment used in its processing would be inadequate to ensure the destruction of all vegetative bacteria, salmonellae would almost certainly be present in the final product. This conclusion was rapidly confirmed. As soon as the ships arrived in Liverpool, batches of dried egg were sent to Cambridge and other EPHLS laboratories to be examined for salmonellae. It was found that, provided large enough samples were cultured, these organisms could be demonstrated in practically every sample. This work provided Tomlinson with valuable experience in the large-scale examination of food substances—an experience that he made ample use of in later years when investigating outbreaks of food poisoning caused by canned peas.

By the end of four years at Cambridge he had developed into an accomplished bacteriologist; and when he moved to the Central laboratory of the Service at Colindale in January 1946 he was able to take control of the whole of the routine work. His direct approach to a problem, planned with almost military precision and carried out with breath-taking efficiency, is illustrated by an incident that Dr Charles Cockburn describes. “Soon after I joined the PHLS”, he writes “Tomlinson invited me to join him in a swabbing expedition to a school where a case of diphtheria had occurred. He had everything fully organised before our departure and we set off with a small team. We arrived at our destination in due course and found ourselves at Strawberry Hill, Twickenham. There in Horace Walpole’s house, now a Roman Catholic school for boys, we set to work in the headmaster’s quarters in rooms seemingly unchanged since Walpole’s day, and swabbed the boys who were marched in
class by class—first using a separate swab for nose and throat and then, when swabs began to run out, a single swab for both areas. Without delay and apparently without a thought of the historical association we were hustled off and on our return immediately sat down to put up the swabs—all this carried out with great expedition and the cheerful bustle which he created around himself.”

Dr J. A. Boycott also gives a picture of him at the time. “I met Tomlinson on the first day that I worked in the routine laboratory at Colindale in December 1946. He was untidy, always in a hurry and worked surrounded by mountains of discarded petri dishes. I was a little put off by his certainty on almost everything bacteriological—‘I wish I were as sure of anything as Tom Macaulay is of everything’—but it was soon clear that this was founded on sound experience and common sense. Among other gifts he had a most reliable memory. He did most things in the laboratory for himself without waiting for help from me or a technician, and when he cocked his head on one side and smiled and said ‘I think we will . . . won’t we’ it meant that he had already planned out the whole job.”

After just under two years at Colindale he was offered the directorship of the public health laboratory at Bradford which the PHLS had taken over from the city corporation. I pointed out to him that this was bound to be a difficult assignment. Local feeling would be antagonistic to the intrusion of a national body, and what aggravated the position was that the retiring director had been given permission to stay on for a time. Tomlinson was only 31 at this stage and had had practically no administrative experience. He did not underestimate the difficulties, but after mature reflection decided to accept the offer. He took charge of the laboratory on 1 Oct. 1947. Being brought up as a Lancastrian, he quickly summed up the character of his new Yorkshire colleagues and, by his open, direct and transparently honest approach, rapidly gained their confidence. With great tact he managed to keep on good terms with the previous director and, out of the kindness of his heart, even allowed him to stay on longer than he had been promised.

During the five years he was at Bradford, Tomlinson built up an excellent laboratory and in the process acquired administrative experience. He took over the bacteriology of the Children’s Hospital, the Infectious Diseases Hospital and the Venereal Diseases Clinic; made a useful comparison of the New York State VDRL flocculation test with the routine Wassermann test; acted as chairman of a working party set up to devise a simple method for the bacteriological grading of ice-cream; and, following the example of his well known predecessor Dr Eurich* of Bradford, he learnt how to cultivate and recognise colonies of the anthrax bacillus. The skill he acquired in this art and the wide knowledge he gained of the disease itself led to his being appointed by the Minister of Labour and National Service in 1957 to the small select Committee of Inquiry on Anthrax.

At Bradford he was so busy that my frequent appeals to him to prepare a thesis for his M.D. degree met with little response. In fact over ten more years

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* See Dr Eurich’s biography written by his daughter, Margaret Bligh (James Clarke & Co., Ltd, London, 1960).
were to pass before I finally convinced him that he could not expect to receive
the full financial remuneration to which his ability entitled him until he had
taken a higher degree.

The success he made of the Bradford laboratory marked him out for rapid
promotion; and when it became clear that, with the kind offices of Sir Allen
Daley who was then Chief Medical Officer to the London County Council,
the PHLS was to be allowed to open a laboratory in County Hall I turned to
Tomlinson as one of the few younger men who had the vigour, resoluteness and
organising power to undertake what would probably prove to be an unusually
arduous task. It was a difficult decision for him to make. He and his wife—
a Cambridge graduate—whom he had married in 1945, had settled down at
Bradford and were not anxious for another move so soon. The laboratory was
running smoothly. His family home at Grange-over-Sands, where his father,
who had now retired, was a Justice of the Peace and a County Alderman, was
not far away; he had refused an attractive offer of a lectureship at one of our
foremost universities; and the prospect of having to start again right at the
beginning, which the opening of an entirely new laboratory would entail, was not
inviting. However, he was not one to shirk responsibility and, after due
consideration, he agreed to the proposal. It was a year before the new
laboratory was ready for occupation and it was not till late in 1952 that he
moved south.

The realisation proved worse than the expectation. For months practically
nothing happened. What little bacteriological work the metropolitan boroughs
required continued to be sent to various hospital and commercial laboratories.
Tomlinson and his small staff found themselves sitting in the laboratory with
practically nothing to do. After the humming activity of the Bradford days
and all the friendly associations he had had with the surrounding medical
officers of health and general practitioners, the inactivity and loneliness of the
London laboratory were hard to bear. Disappointed as he was, he never
complained, but rather thought of ways in which he could make known the
service that his laboratory was only too ready to offer. I did what
I

I
could to
fortify him during this period; and both Sir Allen Daley and his chief epidemi-
ologist, Dr Ian Taylor, proved very good friends to him. At last things began to
move. Everyone who came into touch with him realised something of his merit.
One by one the metropolitan boroughs started sending their work to him, and
practitioners increasingly sought his guidance and advice. Within about two
years the laboratory was getting really busy; Dr Joan Davies, who was to be
his faithful assistant and colleague to the end, had joined him; and other
entrants to the PHLS were being sent to him to receive a training such as they
never forgot.

Of this period Dr Boycott writes: "Time spent in a call at County Hall
was never wasted. It seemed to be one of the most inconvenient of laboratories,
but it hummed with activity. All the doors were open, all the benches were
overcrowded, there was no room for anything and no privacy. I was there
when he received a batch of tinned peas from the City of London. There
were, I think, 400 tins, but Tomlinson took this in his stride. Within a few
minutes the tins were lined up on the bench and while one technician knocked holes in them with a hammer and sterile 5-inch nails another followed with tubes of broth and a tin of pasteur pipettes. It is my recollection that the whole lot were sampled within 90 minutes”.

Dr Boycott goes on. “I sometimes used to stay a night with the Tomlinsons at Weybridge. It began with a drive from County Hall, which reduced me to a jelly. On reaching his garage—which I think he built himself—he shot in without much reduction of pace and with about six inches to spare on each side. We then made a tour of the garden—again too fast—and ate a more than ample meal. It was what my wife would call a ‘warm’ house where intense family affection and pride were masked, quite ineffectively, by leg-pulling and very candid comments—on all sides. I think everyone in the house found life very interesting. I was as envious of the parents as I was of the children. And then we sat down to gossip in front of the fire. Tomlinson was kind. You know better than I do how he would take some assistant who had not been a success elsewhere and accepting his faults would train him to be useful and reliable and give him back his self-confidence. But in private he did not hide his contempt—I do not think the word is too strong—for many of his colleagues in and out of the Service. They might be pleasant fellows, they had done nothing disastrous, but they were in his words ‘asses’ though this view did not prevent him from giving them all the help he could.”

Dr Hampson emphasises the same point. “He was direct and outspoken and a great detector and hater of humbug, but he was never unkind. He was sometimes impatient with a dullard and was said not to suffer fools gladly. This was true only if those in question, in addition to being foolish, were being opinionatedly or pretentiously so. Then it could be true indeed, but his scorn was, one came to see, always for the foolishness and never for the man behind it. ‘Not a clue; not a single clue, but a nice chap.’ One can almost hear him saying it.”

At County Hall there commenced a series of fruitful studies on Sonne dysentery which occupied him intermittently for the rest of his life. This disease was causing major disturbance in the schools and child population generally. A working party was set up to study it, and Tomlinson was made chairman. He threw his whole energies into the investigation and in 1955 was able to issue a report in which two important, though unhelpful, conclusions were reached. First, daily doses of phthalylsulphathiazole given to the children and their teachers had no apparent effect in preventing the spread of the disease; infections were as frequent in the treated as in the control series. And secondly, this failure was not due to the development of resistant strains of dysentery bacilli.

The control of Sonne dysentery presented a baffling problem, which brought the work of the committee to a standstill. It is not surprising therefore that Tomlinson’s later work on this subject, was concerned more with a study of the organisms themselves than with the epidemiology of the disease. With Taylor, Heimer and Lea he studied the fluorescent antibody technique for the rapid demonstration of Sonne’s bacillus in the stools, judging its reliability by
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simultaneous culture of the organisms. In about three-quarters of the samples examined the two methods agreed. About 15 per cent. were positive by culture only, which was not surprising in view of the larger amount of faeces examined by the cultural method. And in a rather smaller percentage the stools were positive by the antibody method but negative by culture. Reasons were advanced for believing that these were false positive results. The general conclusion was that the fluorescent antibody technique was useful in an epidemic when numerous specimens were likely to be positive, but unreliable for the diagnosis of sporadic cases.

With Farrant he determined the colicine type and antibiotic resistance of Sonne dysentery bacilli cultivated from nearly 10,000 separate incidents in London between 1956 and 1965. The general tendency was noted for strains to become increasingly resistant to antibiotics during these years, though this was not an uninterrupted process. Strains appeared having a pattern of colicine production and antibiotic resistance that was new to the area surveyed. The suspicion he entertained that these were the result of transfer of characters from other intestinal organisms was confirmed in one of the last pieces of work he did. Joan Davies and he studied the transfer of resistance to Escherichia coli K12, and the reverse. Transferable resistance, which they were able to distinguish from chromosomal resistance, was found to be associated with the colicine type. They devised a multiple inoculator by which they were able semi-automatically to test large numbers of dysentery strains for antibiotic resistance, for colicine type, and for drug resistance transfer. In this way they showed that transferable antibiotic resistance occurred among the strains infecting 13 per cent. of the families they studied, and that 5 per cent. of convalescents and contacts excreted organisms differing from the original infecting strain by a single transferable resistance determinant. These later studies appealed to the academic side of his nature, and called forth at the same time his inventive ingenuity.

Various studies were made in conjunction with workers in other institutions. For instance, with Salaman he experimented on the protection of mouse colonies against ectromelia by the use of glycerinated vaccinia sheep lymph, and showed that tumour formation was not significantly inhibited by this procedure.

With Albert and Rees he worked on the bactericidal power of 2-mercapto-pyridine-N-oxide, and found that this could be ascribed, like that of 8-hydroxyquinoline, to its ability to form cyclic complexes with the ions of heavy metals, i.e., to chelate. Such a property rendered it suitable for application to intact mucous membranes.

In his early days at Cambridge he had noticed that nearly half of the staphylococcal strains he isolated from cases of impetigo contagiosa in young servicemen had the property of inhibiting the growth of diphtheria bacilli. Later, in collaboration with Parker at Manchester, he found that three-quarters of the impetigo strains isolated from children not only had this inhibitory effect but belonged to a single otherwise uncommon phage type—type 71—and were resistant to penicillin. Another property possessed by the impetigo strains was
that of causing precipitation in serum agar, manifested by a zone of opacity around colonies of the organisms. This observation formed the subject of a joint letter with Parker to *Nature*.

In 1957 Professor Reid at the London School of Hygiene and Tropical Medicine had published a paper in which, as the result of a survey, he brought evidence to show that pulmonary tuberculosis was commoner among pathologists than among other groups of persons of the same age and sex. He concluded that for workers in a laboratory and post-mortem room tuberculosis must be regarded as an occupational hazard. How did infection occur? This is what started Tomlinson thinking. In most laboratories screw-capped bottles were being used for the cultivation of tubercle bacilli. Working with various organisms and using a slit sampler, Tomlinson showed that when a culture bottle was opened a cloud of organisms was quite often liberated into the air. This was the result of organisms multiplying on the moist rim of the bottle that had been contaminated during inoculation. Dry rims were seldom found to be contaminated. Analysing the constitution of the cloud, Lidwell at Colindale found that 30–50 per cent. of particles carrying bacteria were 4 \( \mu \text{m} \) or less in diameter—just the size in fact to be inhaled directly into the lung. Other dangers were noted, and in 1958 Tomlinson, as chairman of a committee appointed to look into laboratory hazards, was primarily responsible for the publication of a report in which precautions were laid down for avoidance of infection in the diagnostic laboratory, the post-mortem room and the animal house.

On a very different subject Tomlinson carried out a valuable investigation in collaboration with the Fruit and Vegetable Canning and Quick Freezing Research Association at Chipping Camden on the cause of food poisoning from the eating of canned peas. For some years past occasional outbreaks had occurred in different parts of the country associated with the presence of an enterotoxin-forming strain of *Staphylococcus aureus*, and quite recently there had been an unusual number of such outbreaks. The industry was aware that during processing of food in the autoclave the seams of the cans were liable to gape slightly, and that during the subsequent cooling of the cans water was liable to be sucked in through the gaps thus left. The leaks were often only temporary and closed spontaneously. Provided the cooling water was pure, little or no harm resulted; but if bacteria gained access to the contents then food was likely to be spoiled. With what are technically known as garden peas, contamination led to the production of gas, which led to the blowing and consequent rejection of the can; but with processed peas no gas production was evident. It was cans of these peas that had been responsible for the food-poisoning outbreaks. Gaping of the seams and leakage were most likely to occur in the big 6-lb cans, thus explaining why the outbreaks were confined to schools and canteens in which there was communal feeding. Inquiry into recent outbreaks revealed that most of them had followed the consumption of canned peas from a single factory. Swabbing of the nose and throat of the employees led to the detection of a woman who was carrying the same phage type of *Staph. aureus* as was responsible for the various outbreaks. The woman was subject to repeated boils on her arms. Her duty was to take the cans out
of the cooling water. In this process her arms were immersed in the water, which was thus contaminated with staphylococci, and by this means the organisms gained entrance to the occasional leaky can. Removal of the woman was followed by the cessation of outbreaks. During this investigation Tomlinson examined no fewer than 3071 cans of peas. His results showed the importance of using water of drinking standard for cooling, and of the mechanical removal and avoidance of handling of all cans while they are still wet.

The experience he gained of the canning industry proved useful to him at the time of the big outbreak of typhoid fever at Aberdeen in 1964. The epidemiological evidence indicated strongly that the source of infection was a large can of corned beef that had been cut up and served on the shop counter. The manufacturers could not believe that a can could be infected from contaminated cooling water without showing subsequent blowing. Tomlinson, however, found that typhoid bacilli introduced in small numbers into a can of corned beef were able to multiply without giving rise to gas formation or to any change in the appearance of the food. Of course, the chances of one or two typhoid bacilli entering a leak in a can during the cooling process from sewage-contaminated water without being accompanied by gas-producing organisms were remote, but the records of the few previous years provided strong evidence that this did indeed occur from time to time.

During the early part of the war outbreaks of diphtheria in schools among the native children and those who had been evacuated from the towns were not infrequent. Experience of the Oxford public health laboratory showed that these outbreaks could be rapidly controlled by swabbing the nose and throat of the entire school population to pick out healthy carriers, combined with active and passive immunisation. These results were obtained in an unvaccinated community. When diphtheria broke out among a fairly well immunised child population in south London fifteen to twenty years later, the problem of control was not so easy. It was studied intensively by Tomlinson, Ian Taylor and Joan Davies. They found among the contacts of the cases a large number of healthy carriers. This was quite different from the picture of diphtheria in an unimmunised community, where the number was usually quite small. It appeared that the continuance of the outbreaks depended on the presence of these carriers, and that the problem of control therefore resolved itself largely into their detection, segregation and treatment.

Probably the greatest of Tomlinson's contributions were those he made to the study of poliomyelitis. At a time when the Virus Reference Laboratory at Colindale was practically the only laboratory in the country examining routine faecal specimens for the poliovirus, he showed how it was possible for an ordinary laboratory to isolate this virus by use of a simple tissue culture method. Neither the growth medium nor the maintenance medium that he devised required anything but reagents that were easy and inexpensive to obtain. The result was that within a year about twenty public health laboratories had followed his example and were giving valuable service to practitioners in the diagnosis of poliomyelitis and other virus diseases. The rabbit serum used in the maintenance medium was later replaced by 5 per cent. liver digest and the
HeLa cells by monkey kidney cells. The final medium, which he described in his thesis of 1961, consisted of lactalbumin and liver digest in Hanks' solution without serum. With this medium it was possible to isolate poliovirus from faeces with considerable assurance.

Tomlinson and Joan Davies took part in the PHLS field trial of live attenuated poliovirus and collated and analysed the results obtained by the various laboratories. Three different dosage schedules were tried: (a) a single dose of trivalent vaccine, (b) three doses of trivalent vaccine at 4-wk intervals, and (c) three doses of monovalent vaccine at 4-wk intervals in the order type 1, type 3 and type 2. The infants, 6–12 mth old, were bled before the first dose, and again 4 wk and 6 mth after the last dose, and the sera titrated for neutralising antibodies. Faecal examinations were made twice weekly for 4 wk after each dose; and later a proportion of the infants were challenged to see whether immunity had developed towards implantation of the Sabin virus strains in the gut. The second schedule proved the best, all the infants forming neutralising antibodies to all three virus types, as against 97 per cent. in the first schedule and 99 per cent. in the third. The investigation showed also that in a household the vaccinal strains rapidly infected the non-immune contacts; that infants and children excreted the virus in the faeces in greater quantity than did adults; and that the gut immunity to reimplantation was much higher after Sabin than after Salk vaccine.

In connexion with the nation-wide field trial, Tomlinson carried out a small trial of his own in which he studied the development of neutralising antibodies, and particularly the time of appearance of the different vaccinal types in the faeces and the duration of their excretion. In contrast to the results of the main trial, this smaller trial showed that the best antibody response was after three doses of monovalent vaccine. The superiority, however, over the schedule in which three doses of trivalent vaccine were given was not great. For good immunity to develop it was found that any given vaccinal type must multiply in the gut and be eliminated in the faeces for at least 10 days, this period being counted from a week after oral administration. These investigations contributed greatly to our knowledge of the chronological development of immunity after the Sabin vaccine, and of the technique best suited for demonstrating both antibody production and faecal excretion.

A trial he made in conjunction with Joan Davies and with the manufacturers of a quadruple vaccine—DPT poliovaccine—in which infants were given three doses administered at 2, 3 and 4 mth of age yielded disappointing results. Maternal antibody interfered with the response to the poliovirus antigens, and when the infants were later challenged with live Sabin vaccine strains, they manifested no increase in resistance over unvaccinated infants. These results were communicated at Stockholm to the 9th Symposium of the European Association against Poliomyelitis and Allied Diseases.

So much for Tomlinson’s scientific work. As a man his character was full of interest. The extracts I have already quoted from Dr Hampson, Dr Boycott and Dr Cockburn are sufficient to reveal his kindliness, his clarity of thought, his love of discussion, and his dynamic activity. I used to see him mainly at
the meetings of the Staff Committee. These were held five times a year to enable
the directors of the sixty laboratories in the PHLS to meet and discuss technical
and administrative problems and indirectly to educate each other. Tomlinson
took a prominent part in these meetings. His loud voice and distinctive laugh
left no doubt, wherever one was, that he was there too. It was a wonderful
laugh, spontaneous, explosive, sudden in its beginning and its end, expressing
the amusement or the joy of a person who knew no inhibitions. It was not a
timid laugh, a forced or an affected laugh; not an ostentatious laugh; not the
laugh of vulgarity or an ingratiating laugh. It was a pure natural laugh which
it did one good to hear. If Touchstone had classified laughs as he did retorts,
I think he would probably have dubbed Tomlinson's laugh the "laugh
hilarious".

In any discussion he could generally be counted upon to be the odd man out.
After a few members had expressed their views on a particular proposition,
Tomlinson would burst out with a statement so preposterous that half the
meeting was shocked into incredulity, while the other half struggled to under-
stand what he really meant. He had an active imaginative mind that leapt
forward from one summit to another, leaving his companions to puzzle out
the intermediate pathway. It was not everyone that could follow him directly.
Confident of himself as he was, and happiest when he was playing with ideas,
he had a certain natural diffidence about him, and he was completely devoid of
personal pride.

In his Lancet obituary Dr J. W. Howie wrote. "On any subject he professed
to understand, his views were worth having. They were not always correct, as
he readily admitted. But they came forth as an instantaneous reaction to any
question or challenge. Always they were stimulating. Often they were brilliant.
Invariably they were expressed with clarity, vigour, and glorious individuality.
. . . In the end, he was seldom found to be far wrong; usually he was dead right."

"When he took over the laboratory at County Hall", Dr Cockburn writes,
"we began what became an invariable custom after the staff meetings—
adjournment to a Soho restaurant, armed with a bottle of wine purchased en
route, where we ate spaghetti and talked all evening.

"He was very fond of anecdotes which he told with great glee, and he
was fond of repeating 'words of wisdom' from his Cambridge teachers. 'I
rather think so-and-so (a widower of our acquaintance) is about to get married
again', he said one evening. 'Why do you think so?' 'Well he has bought a
new set of dentures, and as Professor . . . used to say, believe me, when a widower
gets a new set of dentures he is almost certainly about to remarry.' Then came
his high-pitched and (to his neighbours) startling chuckle.

"He discussed the PHLS, its achievements and its shortcomings, its director
and our colleagues, and its programme. He generally expressed views that were
superficially rather unfavourable. This came not from a dislike of the Service
or the staff, but from his sense of the importance of the PHLS and of his pride
in it. Of all the members of the Staff Committee he was the most consistently
loyal to the Service and to its work.

"He was a great talker but he never talked scandal. . . . He was the least
malicious person I have known and though he had his prejudices they lay
entirely on the surface. He was always willing to modify them if a reasonable
case was made out.”

In 1963 at the invitation of the Department of Technical Co-operation he
visited Iran to report and advise on the public health laboratories there, and
particularly on the necessity for the large amount of expensive equipment for
which they had asked. His visit was a great success, and it was not surprising
that three years later he was called upon, with an American epidemiologist
Colonel Livermore, to go out again and prepare a report on immunisation
against Communicable Disease in the area of the Central Treaty Organization
(CENTO). Together they visited the three countries concerned—Turkey, Iran
and Pakistan. Their report contained information on the collection of health
statistics, the operation of existing laboratories, the extent of immunisation,
and an assessment of the local capacity to produce vaccines and antiserum. They
also attended a meeting at Ankara to discuss a report on the control of cholera.

This last subject particularly seized Tomlinson’s interest, and just before he
fell ill he explained to me the reasons why he had come to believe that cholera
was really a food-poisoning disease. Arrangements by then had been made for
him to go on secondment to Dacca to study the behaviour of cholera in the
field and to test the correctness of his views. Unfortunately this visit was not
to be, and his fertile mind was precluded from the study of a problem that still
baffles bacteriologists and epidemiologists alike.

Beyond his scientific interests, Tomlinson had many diversions. He was
a good shot, enjoyed sailing and rough walking, and in his early days was a
better-than-average bridge player and an expert on the dance floor. For his
holidays he would go off with his family to the Shetlands, or less often to
Norway, Iceland or the Faeroes, where he found relief from life in the pleasures
of walking, bird-watching and looking for plants. His absorbing hobby,
however, was horticulture. This may seem too grand a word, but he was far
more than a mere gardener. His speciality was alpine plants and, in particular,
bulbs. He joined the Alpine Garden Society in 1955, quickly became a member
of the Committee, played an active part in preparing for the 1961 conference,
was chairman for a time of the East Surrey group, formulated the basic
organisation of the Seed Scheme, took over the care of the Slide Library, was a
popular lecturer, and contributed the article on “Propagation” to the Society’s

Not content to sit at home tending his seedlings, it was with the greatest joy
that he received an invitation from Brian Mathew to accompany him on an
expedition to south-east Turkey to hunt for rare plants. He was given six
weeks’ extra leave and the two set off together with Helen, Tomlinson’s wife,
and Margaret Briggs, now Mrs Brian Mathew, on 26 Apr. 1965. Tomlinson
had bought a Land Rover with a long wheel base and a Martin Walter conver-
sion. His family named it “Symbiosis”. It served not only as a conveyance,
but for the sleeping accommodation of all four at night. Their route took them
through Jugoslavia, where they found the spring flowers entrancing. Making
their way down the coast, they passed through Greece, reached Ankara and
then headed south-east to Gaziantep across the Taurus mountains. From there they went east-north-east to the almost mythical Lake Van—an area that had not been visited by botanists for a very long time. There is no space to describe their various adventures or the discoveries they made; these are told in Brian Mathew's account of the expedition in the *Journal of the Royal Horticultural Society* (1966, 91, 334, 383). They came back safely to Ankara, where Helen and Margaret left them to fly home with the plants they had collected. After renewed exploration in the Taurus area, Brian Mathew and Tomlinson finally left Turkey, took the road through Bulgaria, and arrived home on 2 July, after being away for nearly ten weeks.

And now we must leave him. "Open and full of laughter as he was", says Dr Cockburn, "he never discussed his personal affairs, financial or otherwise, and in spite of his friendly approach to all whom he met he was at heart rather a solitary person who preferred to go much of the way alone, knowing what he wanted but appreciating also just how much of what he wanted was attainable, then making the best of it cheerfully and constructively."

Tomlinson belonged to no church organisation, but the whole tenor of his life, his manifest sense of purpose, and the defensive guard he placed around his inner self leave me in little doubt that he was essentially a religious man to whom religion meant more than mere verbal conformity or belief.

Again, to quote from Dr Cockburn, "He loved living and enjoyed to the full his diverse interests, and this makes all the greater the courage with which he faced the inevitable consequences of his illness. He was sincere, upright, modest, practical, with a lively mind, wide culture and fine intelligence. Like so many good microbiologists he was interested in all aspects of biology, and the breadth of his interests and the arresting methods of expressing his ideas made him one of the most stimulating colleagues and companions."

And as Dr Hampson says, to him may be paid the tribute that is earned by so few: he was a whole man and his integrity was absolute.

It is hardly necessary for me to say how much I am indebted to Dr Frank Hampson, Dr John Boycott, Dr Charles Cockburn and Dr Joan Davies for the written appreciations they sent me of Dr Tomlinson. To Mrs Tomlinson, too, I owe my thanks for details about his early life; to Mr Michael Upward for information on his horticultural interests; and to Dr James Howie for permission to quote from his *Lancet* obituary.

G. S. Wilson

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