It seems to me to be odd writing an appreciation of an old friend, Allan Elliott; odd because although I am only about 3½ years older then he is I have usually felt much older and never dreamed that I would still be around, and asked to write something about him when he attained his 80th birthday, Allan’s youthful vigor has always amazed me and just now, though I have not seen him for a couple of years, I would not be surprised if he will still be here at the turn of the century, welcoming in the year 2000.

I think that I first met Allan on Downing street, Cambridge, UK, near Tennis Court Road, sometime in 1927 when he spoke to me about his idea of transferring his activities from the field of organic chemistry, under Mills who had a mighty reputation, to the comparatively new field of biochemistry, under Hopkins who had a still mightier one. I believe I said that I approved the move—after all, I had done much the same about six years before—but that, first, he should seek the advice of Hopkins who would manage things in his usual tactful manner. Not long after that, I saw Allan either with Hopkins or with Malcolm Dixon, at work in the then-popular area of biochemistry, biological oxidations—particularly the study of peroxidases. Soon after that I lost sight of him altogether, as I moved from Cambridge to a place nobody seemed to have ever heard of apart from a few psychiatrists and their numerous patients. Allan, at that time, was friendly with a group of bright young men that formed part of Hopkins’ establishment. Then I heard that he had gone to the USA. His activities are described in his short autobiography in the Bulletin of the Canadian Biochemical Society (1980).

When I came to Canada in 1947, to take up appointments in McGill University and the Montreal General Hospital, I found that my newly established laboratory was scarcely a block away from the Montreal Neurological Institute and that the biochemist there (since 1944) was Allan.
I met him frequently, often in the McGill Faculty Club, sometimes in his lab, and sometimes in mine. I think that he thoroughly enjoyed his work at the MNI. He had numerous associates to whom he has given much of the credit of their joint work. He greatly admired and was devoted to, the head of MNI—Wilder Penfield—who had treated him with much kindness and who seemed to understand what Allan was doing with his brain preparations. Therein, Allan was lucky, surrounded by highly capable brain surgeons, neurologists and physiologists; a sympathetic and considerate atmosphere in which to work. How different from my own, way back in 1930, when I had to work under the sceptical eyes of the psychiatrists, who would watch me in the lab, now and again, when they had the time, with amused tolerance—but this attitude began to change some time later, as the new ideas began to dawn upon them.

I am inclined to think that Allan and I, with the distant but considerable assistance of I. H. Page, really put Neurochemistry on the scientific map when we published "Neurochemistry" in 1955 with the invaluable cooperation of about 30 experts in the rapidly developing field. This book, together with the launching of the Journal of Neurochemistry, which occurred at about the same time, paved the way for major advances in the field. Neurochemical societies began to be formed, helped on by the highly articulate and highly intelligent neurochemical "statesmen" who supervised the growth of the comparatively new science. At that milestone, 1955, Allan had been in the field just over 10 years and a little later he edited the Canadian Journal of Biochemistry. We were in 1955 already old hands, busy, with a rapidly growing number of near and also distant colleagues, helping to lay the foundations on which modern neurochemistry stands.

I recall being much interested in the demonstration by Allan and his group that water is absorbed by brain slices when they are incubated in a physiological electrolyte-balanced medium. This observation formed the basis of much further work and was taken up by other laboratories—including my own—and there is still great interest in it, not only to neurochemists but to all students of neuroscience. I think, however, Allan's most important contribution was that of his small group in the demonstration of the neurophysiological significance of GABA. I believe this to be a very considerable contribution to neuroscience and I am glad that Allan played a role in its discovery.

I have read most of Allan's publications and I do not believe that we ever clashed, though we worked quite independently of each other. Although I did not always agree with his conclusions, I certainly accepted, without question, all his results. He was always very attentive to every detail of his experimental work.