

EXTRACT FROM
A PERSONAL HISTORY OF
H.M. NAUTICAL ALMANAC OFFICE

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PROLOGUE

Personal history to 1930

I, Donald Harry Sadler, was born at Dewsbury, Yorkshire, on 22 August 1908, the second son of James Wright Sadler, a master tailor, and Gertrude Jane (née Needham), formerly a schoolteacher. After primary school I attended the Wheelwright Grammar School from 1919-1926, when I went to Trinity College, Cambridge, with an open Entrance Exhibition in mathematics, and various other scholarships.

At Trinity College, Cambridge

At Trinity my supervisor was J. E. Littlewood, perhaps the greatest pure mathematician of the time; but his supervision of undergraduates was rather superficial and I essentially chose the actual courses for myself.

I duly obtained a first class in part 1 of the Mathematics Tripos, and was awarded a Senior Scholarship on the results of the second-year examination (the “Mays”). I did not, however, get the coveted Yeats and Rouse-Ball Essay Prize; I was beaten into second place by Harold Davenport, who romped away with the prize. [My essay, which I now find quite incomprehensible, was titled “Moving Axes and Differential Geometry of Space-curves and Surfaces”; it was highly commended!] My mathematical interests were varied, but as I had done no physics beyond School-Certificate level I, perhaps wisely, took no course in mathematical physics. I attended Stratton's course on spherical astronomy and Smart's courses on orbit calculation and celestial mechanics, but I was not impressed by the latter — possibly because there were too few students (3 reducing to 2 — the other being F. M. Dean, later Sir Maurice). I also thought that the treatment was too theoretical; I suspect that Smart had never himself computed an orbit from three observations. I also attended Smart's course on practical astronomy at the Observatory, and duly determined its position with a sextant, an artificial horizon and 7-figure logarithms.

In Part 2 of the Tripos, I was awarded a first class with a b* (the highest class) in the voluntary Schedule B; but I did not get the Tyson Medal (for distinction in the field of celestial mechanics), which was not awarded that year. The reason (presumably) was that I failed to answer at all adequately the compulsory question on orbit determination. A study of previous papers had convinced me that Leuschner's method (which had appeared in 3 of the last 4 papers) would not be set again. I disliked the method, and therefore I did not revise my understanding of it, and in the examination I could not reproduce the main argument. [That should have taught me a lesson, but it didn't.]

My scholarship allowed me to stay up for a fourth year, but I could not decide in what branch of mathematics to specialise. I had already realised that, compared to my contemporaries at Trinity (Coxeter, DuVal, Todd and above all Davenport — all of whom became Research Fellows and young F.R.S.s) I was well below the standard of original thought for research in pure mathematics. I was also disappointed at not getting the Tyson Medal and, in any case, I knew insufficient physics to work on general astronomy. I had attended Eddington's lectures on relativity, which I did not fully understand, and on the combination of observations, which gave me much pleasure. I accordingly suggested to Littlewood that I would like to study, and research, in the field of mathematical statistics — then a subject that was almost new. He referred me to G. Udny Yule, the most distinguished statistician of the day, who suggested a line of work

to me; but he was primarily an economic statistician. He demonstrated the first calculating machine that I had seen — a recent acquisition on his part — and suggested that I should look at the periodicities of sunspots; I duly did this with negative results. But he then drew my attention to his own work on spurious correlations between time-series, and asked me to analyse mathematically some of his tentative, descriptive, theories. I tried to extend his ideas of serial correlations, but the progress that I made towards the much later concepts of auto-correlation and the power spectrum was small; the amount of calculation required effectively prevented its practical application. I did not allow my interest in celestial mechanics to lapse, but I was the only student to opt for H. F. Baker's lectures (I think he expected no-one). He talked to me on some dynamical problems in astronomy (variation of latitude, precession, etc) assuming that I knew more of the basic theory than I did — I should have read more and learned more.

Looking for a job

During the year I made tentative efforts to find a job, although this was not easy in 1930. I knew that I could certainly get a post as Assistant Lecturer (at a standard rate of £300 p.a.) at a smaller university or a teaching post at a school, but I was not interested in either. Through the Cambridge University Appointments Board I answered an advertisement by Rowntrees (chocolate manufacturers of York) for a mathematical statistician. I went to York and had a most interesting competitive interview, including one of the first intelligence tests. They offered me the post at a salary that I declined; the following day I got a letter offering me a much higher starting salary, but I turned it down!

Towards the end of the summer I was considering two jobs — an Assistant Lectureship at King's College, London, and a teaching post. Then I received an invitation to attend for interview for a post in H. M. Nautical Almanac Office.

It should be made clear that, although I was 22 (just) and had been at Cambridge for four years, I was extremely inexperienced in almost all practical aspects of life. I was incapable of assessing or appreciating the circumstances of my early years in the Office.