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A. G. OGSTON COMMENTS—How did I come to make my original discovery? My undergraduate training was in pure chemistry, with a strong preference for physical chemistry, and my first research was on the conductivities of dilute electrolyte solutions in Sir Harold Hartley's laboratory in Balliol. It was my interest in electrochemistry that soon led me (opportunist that I am) into the biochemical field: first with Sir Rudolph Peters, then with E. R. Holiday and J. M. Gulland, to work on the constitutions and electrochemical properties of some biological compounds; then to work (still as a physical chemist) on immune proteins, under E. R. Holiday and J. R. Marrack, at the London Hospital; finally back to Oxford for a second degree course in Physiology and to appointment at my old College (Balliol) as Tutor, mainly teaching Physiology and Biochemistry to medical students.

Some time in 1948, when spending a spare hour looking through journals (mainly with a view to advising my pupils what to read), I came across a paper[1] which seemed to show that aminomalonic acid could not be an intermediate in the conversion of serine to glycine. I read it with interest and, initially, with consent. Suddenly, something happened. One moment I thought: 'That's neat.'; the next: 'But it's wrong.'; the next: 'Citrate!' It may have taken five seconds, perhaps less. A day or two later I sent off my letter to *Nature*[2].

Why did I leave it (almost[3,4]) at that? Until I received, a few days after publication of my note, an excited letter from Sir Hans Krebs (then at Sheffield), I had little notion of the size of iceberg beneath that tip, regarding my idea as no more than an amusing piece of stereochemical logic and its consequences for asymmetric synthesis as merely an obvious corollary. And I was, by then, deeply involved with other lines of research which, if they have proved to be less sensational, have been satisfying in requiring far greater intellectual effort. 'Three-point attachment' was a gift, out of the blue, for which I have never felt able to claim much credit.

Yet I had, I suppose, unwittingly prepared myself for the three-dimensional visualisation that it required. As an undergraduate student I had enjoyed (and had been excellently taught) the intricacies of stereochemistry. During the war, on daily walks between my digs and place of work, I had amused myself by building, in my head, possible models (all quite wrong[5]) of protein structure. And I was, of course, well aware of the problem, at that time, of the place of citrate in the Krebs tricarboxylic acid cycle.

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articles

Is there a chronometer hidden deep in the Sun?

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No support is found for the conventional view of the sunspot cycle, that there exists a large random walk in the phase of the cycle. Instead, both sunspots and the [D/H] solar/terrestrial weather indicator seem to be paced by an accurate clock inside the Sun.

IT has long been believed that "the sunspot disturbances, like the eruptions of a geyser, are inherently only roughly periodic" [1]. Observations show a large variation in the ~11 yr half-cycle period, "the observed intervals ranging all the way from 7.3 to 17.1 yr" [1]. Kiepenheuer[2] has described the sunspot cycle as follows: "It was previously believed that the sunspot cycle resulted from the superposition of different periodic cycles.... Since then it has become clear that the rise and fall in the number of spots is due to a number of practically independent individual processes. Thus the idea of a true periodic phenomenon was dropped in favour of the so-called 'eruption hypothesis'. On this hypothesis, each cycle represents an independent eruption of the Sun which takes about 11 yr to die down". This conception of an irregular sunspot cycle, implying a random walk in the phase of the cycle, seems to agree with the Babcock theory and with subsequent modifications of the theory. According to the Babcock theory the poloidal magnetic field remnant of one half-cycle (~11 yr) provides the seed field

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