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BY J.H. JONES

Balliol College, Oxford OX1 3BJ, UK

Ewart Ray Herbert Jones, just ‘ERH’ or ‘Prof’ to his laboratory colleagues but ‘Tim’ to his more intimate friends and family, died on 7 May 2002. He made important contributions to the chemistry of polyenes, polyynes, steroids and triterpenes. A leading figure in the profession of chemistry, he steered the Chemical Society and the Royal Institute of Chemistry towards unification, and was first President of the resulting Royal Society of Chemistry.

BIOGRAPHICAL OUTLINE

Jones was born in the north Wales village of Rhostyllen on 16 March 1911, the only son of William Herbert Jones (1878–1924), a solicitor’s clerk in nearby Wrexham, and Mercy Grace (née Ray), his wife (1889–1976). Her father was John Emmanuel Ray (1863–1925), an evangelist missionary who emigrated to the USA in about 1902, leaving her mother (Margaret Ann Ray, formerly Jones, 1864–1927) behind. Tim Jones and his sister were the first of the family to have any secondary education, although their father was a bookish man actively involved with the Workers’ Educational Association.

With a live-in evangelical grandmother, Chapel (thrice on Sundays) and long sermons played a large part in family life. The young Tim did not relish that, but it was associated with much singing and music, which he enjoyed. Between July 1924 and March 1927 his only full sibling (Cecilia Grace Herbert Jones, 1908–26), to whom he was very attached, died a lingering death of tuberculosis; his Ray grandmother, the only grandparent he had known, died; and his father drowned himself, leaving his mother hard-pressed financially. He makes no mention at all of his father’s suicide in his otherwise detailed and reflective autobiographical notes, and generally makes little of what must have been a traumatic and accelerated growing up. In later life, by then religiously indifferent, he felt that the bible-bashing tradition from which his mother sprang could have been more supportive. She took in lodgers to make ends meet, and
married one of them, Keith Wolfenden (1901–91), a young Manchester mathematics graduate. From that marriage he had a supportive step-father and a half-brother, another Keith Wolfenden, who was to be Professor of Information Processing at University College London from 1970 to 1986.

Jones was schooled in Wrexham, first at the Victoria Boys’ School and then at Grove Park School, which had a strong academic tradition. He did well there, with gratefully remembered stimulation from A.R. Lindsay, who taught physics, and from his step-father, who taught mathematics. Entering the University College of North Wales at Bangor in 1929 with scholarships and a teacher-training award, he originally planned to do physics, but found it unexciting. His allegiance soon shifted to chemistry. After his first-class honours degree in 1932, and Diploma in Education a year later, he was invited to stay on by the Head of Department, J.L. (later Sir John) Simonsen FRS, who offered him a two-year studentship of £100 a year for PhD work on terpenes, which he accepted. With much help from George Rowntree Ramage (53)*, and despite the distractions of sport (especially hockey) and falling in love, his research soon took off. At the end of his second PhD year he was awarded a University of Wales Fellowship of £200 a year, which gave him support for his final year and a postdoctoral year. When he finished his PhD in 1936, with an offer from Robert (later Sir Robert) Robinson FRS (PRS 1945–50) at Oxford in his pocket, he went to see I.M. (later Sir Ian) Heilbron FRS at Manchester. Rapport was immediate, and Heilbron offered to make up his University of Wales Fellowship to £400 a year with Rockefeller Foundation money, so Jones never explored the Robinson option fully and went straight to Heilbron. Two years later, when Heilbron was appointed Professor of Organic Chemistry at Imperial College, he accepted a job under him there with ‘no hesitation at all’, despite the prospect of war and London bombing.

He first admired Frances Mary Copp on Friddoedd playing field in Bangor during October 1931. She was then a 19-year-old first-year student from Hawarden, daughter of Charles Edgar Copp (1885–1979), a tin plate worker. They were married in 1937 and had a son and two daughters. It was a profoundly good and happy marriage, and in their testimonials for him, both Heilbron and Simonsen said she was a charming asset to him in his work.

Heilbron became so involved with war work that Jones was effectively in charge of organic chemistry at Imperial College from 1941 to 1945. His own main war work was in the Gas Identification Service, running courses to prepare chemists for civil defence duties—between 1940 and 1944 about 2000 attended these four-day courses in groups of 30–40 at a time. He also undertook secret war research, notably for the Ministry of Supply on the preparation of lachrymators. Against this backdrop, more or less normal academic activity continued. Imperial College in the second half of the war and just afterwards was his golden era for talented research students, among whom at least seven were elected to the Fellowship of The Royal Society: A.W. Johnson (1965), R.A. Raphael (1962), Alfred Spinks (1977), B.C.L. Weedon (1971), Franz Sondheimer (1967) and D.H.R. (later Sir Derek) Barton (1954; Nobel Laureate 1969). T.Y. Shen and Marc Julia, both much honoured abroad for their work, were also with him in this period.

After the war, Heilbron encouraged Jones to seek a chair elsewhere. Nothing came of applications to Bristol University or King’s College, London, but in 1947, at the age of only 36, he was invited to accept the Sir Samuel Hall Professorship of Organic Chemistry at Manchester, unofficially ranked as the third chair in the country. His principal referee was A.R. (later Lord)

* Numbers in this form refer to the bibliography at the end of the text.
Todd FRS (PRS 1975–80), who thought him ‘quite first-class and growing steadily in stature’, with ‘personality as well as chemistry’ who had already shown at Imperial College that he could ‘take responsibility easily and do a thoroughly good job’. Jones quickly established a vigorous team in Manchester, appointing two of his Imperial College students, H.B. Henbest and M.C. Whiting, to his staff. T.G. Halsall, who was already at Manchester, also threw in his lot with the new professor, thus beginning a career-long association. From this time on Jones’s modus operandi was one of delegating the day-to-day supervision of researchers in his large group to lieutenants. It was a successful method, and the Manchester years were very productive. Among his students there was Michael Smith (FRS 1986; Nobel laureate 1993). However, it was not all academic chemistry: Jones also took an active part in university affairs, serving on the small University Council. And throughout his Manchester years he in effect continued his war work through active involvement with the Chemical Defence Advisory Board.

Early in 1954, Robinson reminded the Oxford authorities that he was due to retire from the Waynflete Chair of Chemistry and asked them to get on with appointing his successor so that he could make arrangements for his own future work. Rarely can so much chemical genius have focused on a single appointment. The electoral board comprised, in addition to local dignitaries, Todd, Heilbron, Sir Cyril Hinshelwood FRS (PRS 1955–60), Sir Rudolph Peters FRS and L.E. Sutton FRS. They advertised, but in the time-honoured Oxford way stated that their choice would not necessarily be confined to those who applied. In the event, only F.E. King FRS and W.A. Waters FRS had the temerity to apply, and the electors concentrated on three names of their own: Barton, R.P. (later Sir Patrick) Linstead FRS and Jones. Barton’s brilliance was recognized, but at 36 he was ruled out as being too young and lacking in administrative experience; so he accepted the Regius Chair of Chemistry at Glasgow, having prevaricated pending the outcome at Oxford (Barton 1991). Linstead, in contrast, was thought at 53 to be slowing down scientifically; he was appointed Rector of Imperial College shortly afterwards. There was unanimous agreement that Jones would be a worthy successor to Robinson, and Robinson himself remarked privately before the election was declared ‘of course I am 100% pro-Jones; the runner-up [i.e. Barton] is personally unknown to me but I hear very much that suggests to me that he may be the wrong shape for this hole’.

Robinson’s metaphor was more apt than he intended, for the Dyson Perrins Laboratory was very run-down (Williams 1990). It was not an attractive prospect to Jones, who was domestically and scientifically well settled: only a year earlier he had declined a full professorship at Massachusetts Institute of Technology. At Manchester he had a very up-to-date laboratory and an excellent productive team around him, on a personal salary of £200 a year more than Oxford was offering. He visited Oxford to assess the situation in detail, and set out his conditions for acceptance: major rehabilitation of the laboratory costing £75 000 over three years, increased regular funding, work on an extension costing an estimated £150 000 to begin within two years, appointment of his Manchester Chief Technician George Ryder as Administrator, and appointment of his Manchester colleagues Halsall and Whiting to equivalent posts at Oxford. This alarmed the Oxford establishment, and the electors were asked to reconsider. Was Jones so outstandingly the best that it would be an irreparable loss not to get him? Would either or both of the second and third choices be likely to accept without substantial demands? Did the electors know of any suitable persons who might hold the professorship with credit to the university without requiring any special expenditure? The electors, especially Todd and Heilbron, stood their ground, and the university gave in. Jones accepted the chair in February 1955.
After setting out his demands to the University Registrar, Jones had said:

I much regret having to make this wholesale condemnation of a department with so distinguished a head, but Sir Robert has never been very interested in these matters and during the last fifteen years has made his greatest contributions outside the department. I am firmly of the opinion that if the conditions which I have laid down can be met, then within a period of three to four years it would be possible to have a department, comparable with those that exist elsewhere, of which Oxford need not be ashamed.

He made good this promise, despite the fact that the university did not entirely enter into the spirit of its agreement, and sought to cut financial corners.

Nearly half a century later, he looked back with pride on the hard line he had taken, amused by the consternation he had caused and the university's attempts to get a cheaper alternative out of the electors—"typical Oxford, typical Oxford", he said. And with a chuckle he recalled 'I was put on the University Chest fairly quickly ... they thought that would be a salutary experience for me'. He was prominent in university finance, buildings and graduate affairs for more than 20 years, and had his tenure as convenor of the Buildings Committee extended for a year after retirement because it was found difficult to replace him. But for all the great service he did the university in those areas, he remained critical of Oxford ways, of the tutorial tradition ('just a kind of coaching') and of the colleges. The aspect of the college system that irked him most was the semi-detached way in which the colleges and university made appointments, which sometimes led to his laboratory staff languishing without college fellowships, or the colleges landing him with people expecting laboratory space. These cases generally worked out in the end, and the difficulties diminished in the second half of his Oxford career as effective arrangements for consultation over joint appointments became established. When a college threw someone in his lap, or twisted his arm (as happened with J.R. Knowles (FRS 1977), for example) he was magnanimous, and gave warm encouragement to the individuals concerned, when lesser men would have been grudging.

He ran the rehabilitated and extended Dyson Perrins Laboratory with smooth efficiency, and was concerned to ensure that its modern instrumentation and research services were as good as anywhere. With junior colleagues there was little consultation, but that did not matter because the autocracy was very benevolent. The studentships funded by UK government, for example, came to him for allocation at his personal discretion; he took pains to distribute them and other resources among his staff fairly. His own research remained vigorous at Oxford, but day-to-day supervision he left to colleagues: Halsall and Whiting, who had come with him from Manchester; G.D. Meakins, who followed a little later (initially to run the infrared service); Viktor Thaller, another slightly later recruit who had also been with him at Manchester; and, for a few years, Gordon Lowe (FRS 1984).

The Waynflete Chair was tied to Magdalen, so Jones was ex officio a Fellow of Magdalen. With Frances he was warmly appreciative of the social aspects of his Fellowship, but he held back from involvement in college affairs. When he retired, he was pleased to be elected to an Honorary Fellowship, but in 1997 he wrote to the President to say, with the greatest possible grace, that he thought it would not be appropriate for him to be accorded a College Memorial Service.

He attached great importance to professorial leadership in undergraduate teaching. 'I have always regarded the Professor of Organic Chemistry's job as putting across organic chemistry', he said in 2001. 'Coming to Oxford I was rather surprised to find there were Professors who didn’t lecture, there were Lecturers that didn’t lecture..., and didn’t regard it as part of their job, which rather disgusted me'. When first appointed at Imperial College, he had lec-
tured to the chemical engineers in organic chemistry; he gave that up after three years and took
on the first-year lectures to chemists. When he went to Manchester, he gave the first-year lec-
tures, and when he came to Oxford he lectured on average twice a week through the year, giv-
ing the introductory first-year lectures on principle. One of his ‘treasured possessions’ was a
letter from ‘my pupil Derek Barton’ referring to his ‘inspiring lectures in organic chemistry at
Imperial College’, and in his autobiography Barton recalled his lectures as ‘perfect’. By 1961
the lectures were still highly polished, but were beginning to be unfashionably heavy on
description and light on mechanistic interpretation. In any case, from the early 1960s, as pub-
lic service made increasing calls on his time, he reduced his lecturing load.

He was a vigorous supporter of the publishing activities of the Chemical Society, urging on
it as early as 1946 the introduction of a vehicle for short communications—counsel that was
not heeded for a decade—and not only giving the lead but providing the name for Chemistry in Britain when it began in 1964. For commercial publishers moving in to compete, like Robert Maxwell, he had only antipathy, and junior colleagues who were tempted by Tetrahedron risked a flea in the ear.

Jones planned for his scientific work to run down naturally as his professorship came to a
close in 1978, but his professional service to chemistry continued for well over a decade after-
wards, including chairmanships of the Executive Committee for the 1985 International Union
of Pure and Applied Chemistry Meeting at Manchester and of the Royal Society of Chemistry
(RSC) Sesquicentenary (1991) Committee, which organized a range of activities, not least a
visit by The Queen to Burlington House. He was an active member of the RSC Awards
Committee until 1997. He also joined the Board of the Oxford-based Anchor Housing
Association, and was Chairman of the Anchor Group from 1979 to 1984. He had been a fre-
cquent visitor to Europe since before the war: in 1936 after finishing his PhD he went on a
month-long cycle tour of Bavaria, Austria and the Dolomites with his step-father, and he was
in France with Frances the day before hostilities began in 1939. From 1950 there was a major
intercontinental trip in most years, often accompanied by Frances, all meticulously recorded
in diaries and photographs. In his retirement, they travelled even more widely and in a more
leisurely way, making several round-the-world trips visiting children and grandchildren in the
USA and Australia. It was mainly for the future interest of his grandchildren that he took up
genealogy, but he enjoyed his archive visits and found the research ‘not a bad substitute for
experimental science of the natural product type’.

He had always had an historical perspective on organic chemistry and its heroes, was the
prime mover behind J.C. Smith’s two-volume history (Smith 1975) of the Dyson Perrins
Laboratory, and tried to ensure that departmental records and relics, at risk in a working lab-
oratory, would find their way to the Oxford Museum of the History of Science. In early retire-
ment he spent a lot of time collecting data about the careers of doctoral students who passed
through his departments in London, Manchester and Oxford. He also wrote more than his
share of perceptive obituaries. It is fitting that his last formal publication was a historical
reflection (54).

A tall lean upright man, he was not fit enough for the army in 1939, but he had a strong
constitution that bore him through gas exposure during war work, tuberculosis in his thirties,
serious pneumonia in his forties, and major operations in his eighties. Even-tempered and
calm in manner, usually with a friendly smile and a twinkle in his dark eyes, he was resolute
but diplomatic and fair in all his business, a good chairman, a serious student of cricket, and
a keen gardener. Reflecting towards the end of his life, he modestly saw the hand of chance at
many stages of his career, regretted none of the choices he had made, and was grateful. Above all he was grateful to Frances for her support in every possible way as homemaker, companion, and hostess, and for her toleration of his devotion to his work. She died of leukaemia on 22 March 1999, after a six-month illness, during most of which he looked after her at 6 Sandy Lane, Yarnton, their home for nearly 35 years. Tim Jones himself died there on 7 May 2002.

Scientific work

Jones’s first terpene research with Simonsen led naturally into steroids and triterpenes with Heilbron, and Heilbron also introduced him to acetylene chemistry. Practically all of his later work was seeded by his Heilbron years. Heilbron was an early exponent of the value of spectroscopic and chromatographic methods in organic chemistry; that, too, he imbued Jones with. As Heilbron wrote in 1954, Jones was truly one of his ‘scientific children’ and Jones for his part noted, 40 years on, that he was ‘deeply indebted to him’.

His complete scientific works run to over 400 papers, many of them very densely written, with perhaps 150 more from his school on which he did not put his name, although he was closely involved in them. Only an illustrative selection can be cited here, and in many cases the reference given is only a leading reference.

The eponymous selective ‘Jones oxidation’ of secondary alcohols in acetone using the ‘Jones reagent’ (chromic oxide in aqueous sulphuric acid) has been in widespread use ever since it was devised (11).

Early work at Bangor

As part of his PhD work, but conceived and executed while Simonsen was away on a long visit to India, Jones achieved an annelation with angular methyl group formation (1), a conversion that might have been developed further if it had not been almost immediately overshadowed by the Robinson ring extension.

Acetylene chemistry (20)

When Jones joined Heilbron’s group in Manchester, diverse fundamental studies of acetylene chemistry aimed at the eventual synthesis of vitamin A (Heilbron 1948) were in progress. Jones was soon in the lead. Raphael’s book on the subject (Raphael 1955) was dedicated to Heilbron, but Jones’s presentation copy was inscribed by the author ‘From an acetylenic Boswell to his Johnson’. Among the many reactions and procedures of enduring importance developed in the vitamin A connection were: the acid-catalysed ‘anionotropic rearrangement’ of acetylenic and vinylic alcohols (7, 9, 10), a rather general isomerization proceeding through a delocalized carbocation towards greater conjugation (scheme 1); the preparation and use of ethoxyacetylene (17, 30) and ethynylmagnesium bromide (32, 36); and extended Reformatsky reactions (18). An overall strategy for vitamin A (1) was defined (scheme 2) and possible key intermediates were assembled (6, 16), but wartime problems prevented the main project from being carried through. Jones and Heilbron did, however, have the satisfaction of knowing that the first practicable vitamin A synthesis (Isler et al. 1947) that could be applied on an industrial scale was essentially as they had planned, and at critical points depended on chemistry that they had developed. Their work was fully cited and explicitly acknowledged: ‘In einer Reihe von Arbeiten mit modellsubstanzen haben diese Forscher darauf eine sichere Grundlage für die Synthese des Vitamin A geschaffen’.
In 1944, Jones heard via Heilbron that Robinson was in difficulty over the synthesis of \(\delta\)-hexenolactone (2), a suspected selective growth inhibitory factor present in malt. Jones, by now fluent in synthetic methods with acetylenes, could see an easy synthesis, and wrote to Robinson offering his help. The young rising star got a somewhat discouraging reply from the world’s leading organic chemist; ‘Sir Ian tells me that you think you can make hexenolactone, which we believe to be the active principle. My wife and I have spent some time on this: it is very difficult and you are welcome to go ahead and try. We shall be pleased to test the material you obtain: if any!’. Jones sent a 10 g sample, made (12) by his elegantly simple route (scheme 3) a couple of weeks later, and was featured in the popular press (Anon. 1945) as the hero who had made available a potential anti-cancer wonder-drug.

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German wartime advances in acetylene – nickel carbonyl chemistry prompted Jones, with Whiting, to initiate a programme that ran for a decade or so on the Reppe carbonylation, involving the study of reactions such as those exemplified in schemes 4 and 5 (23, 33). The chemistry of the latter is a general synthesis of allenic esters; allenes were an ancillary interest over a long period, and later work included the first proof (39) of the absolute configuration of any allene.
It was a natural extension of studies aimed at the synthesis of polyenes to investigate, also with Whiting, the synthesis and properties (especially spectroscopic) of the polyynes (22, 41). The longest stick of bare carbon they made (38) was the decaacetylene (3). Remarkably, although such compounds are notoriously unstable in the condensed phase, 3 was obtained crystalline. Its ultraviolet (UV) spectrum shows no less than 18 maxima; it was a matter for wry comment that its highest \( \epsilon \) value was a disappointment at just under the million mark.

**Natural polyacetylenes (46, 52)**

Naturally occurring acetylenic compounds were rare curiosities when Jones began his career, and even in 1950 only a dozen or so were known; around 1000 were known by the end of the century. They first attracted his attention as interesting synthetic targets, such as matricaria ester (4), one of the first to be isolated and characterized (scheme 6) (34).

However, synthetic target-shooting widened into searches for more examples. Screening promising plants and fungi was greatly facilitated by the characteristic UV spectra and high UV absorbances associated with compounds of this class. A very diverse and numerous array of naturally occurring polyynes, ene-yynes and modified (especially heterocyclic) derivatives were identified through this screening. Jones and Thaller also contributed to the characterization and synthesis of wyerone (5), a systemic fungicide produced by broad-bean shoots when challenged by infection (47).

The biosynthesis of the natural polyacetylenes also intrigued Jones, and with Thaller he devised efficient syntheses of a range of specifically labelled biosynthetic intermediates. Much of the synthetic methodology employed was novel and ingenious, such as the route to polyacetylenic C\(_{18}\) derivatives containing ‘skipped’ ene-yne unsaturation (scheme 7) (49). The elucidation of the means by which the triple bonds were formed eluded them, but they managed to elucidate the pathways to a series of acetylenic metabolites in fungal cultures, notably (51) the nitrile acid diatretyne 2 (6).

\[
\begin{align*}
\text{MeCH}_2\text{CH}_2\text{CH}_2\text{C} & \equiv \text{CCO}_2\text{H} & \text{MeCH}_2\text{CH}_2\text{CH}_2\text{CO}_2\text{H} \\
\end{align*}
\]

Scheme 4. A typical Reppe reaction. Conditions: Ni(CO)\(_4\), acetic acid, aq. ethanol.

\[
\begin{align*}
\text{Cl} & \quad \text{Me}_2\text{CC} \equiv \text{CH} & \quad \text{Me}_2\text{C} \equiv \text{CCO}_2\text{Et} \\
\end{align*}
\]

Scheme 5. Reppe allene synthesis. Conditions: Ni(CO)\(_4\), acetic acid, aq. ethanol.

\[
\text{Me}_3\text{C} \equiv \text{C} \equiv \text{CMe}_3
\]

\[
\text{MeCH}_2\text{CH} \equiv \text{CCH}_2\text{OH}\quad \text{HCOCH} \equiv \text{CCH}_2\text{OH} \quad \text{MeCH}_2\text{CH} \equiv \text{CCH} \equiv \text{CHCO}_2\text{Me}
\]

Largely as a result of the work of Leopold Ruzicka (ForMemRS 1942), by the mid-1930s the partial structures of many pentacyclic triterpenes were known, and most could be assigned to groups with common features, but no complete structure had been established. Heilbron had for some years been interested in lupeol (7), which was first isolated from lupin seeds but is widely distributed, and Jones entered the field with a proof (5) that it had an exocyclic carbon–carbon double bond, and made the incidental observation that there were regularities in the rotatory power of lupeol derivatives. This led him to suggest to Barton during the dark days of the war, when experimental work was inhibited, that he might make a systematic library study of all the rotation data on triterpenes he could find in the literature. From this came the method of molecular rotation differences (8), a generalization that said that in a group of closely related compounds, functional modification would shift the molecular rotation by about the same amount. The groupings of the pentacyclic triterpenes were mostly confirmed, and the finger of suspicion was pointed at a number of structures. But Jones’s main early contribution to the field, greatly admired by both Vladimir Prelog (ForMemRS 1962) and Ruzicka (Prelog & Jeger 1980), was the correlation of two major groups, the \( \Delta^5 \)-amyrin (8) and lupeol families, by the demonstration that both parents could be converted to 9, proving that they shared common stereochemistry and functionality in at least three rings (scheme 8) (21).

Decisive work, with Halsall as before, first (29) on \( \psi \)-taraxasterol (10), and later (42) on hydroxyhopanone (11), followed. And, for a finale, lupeol (9) was converted by rational degradation into 12, which was shown to be enantiomeric with 13 obtained by partial synthesis from 3\( \beta \)-hydroxy-5\( \alpha \)-pregnan-11,20-dione (14). This was the first correlation of steroidal and pentacyclic triterpene stereochemistry (scheme 9) (43, 44).

The tetracyclic triterpenes are also a numerous complex class (31) that Jones engaged with before the war, especially the so-called polyacetylenic acids A, B and C obtained from the birch shelf fungus parasite, which were described in 1940 (3). Their study had then to be set aside but was resumed in peacetime (25, 26, 28), when acid B was shown to be a mixture, but acids A and C were established as 15 and 16. Cephalosporin P, (17), an antibiotic with similar features, was a later interest (45).
By the time Jones went to Manchester in 1936, it was known that vitamin D3 was derived from cholesterol, and his introduction to steroids with Heilbron began by seeking a better route to its provitamin, 7-dehydrocholesterol. The initial bromination studies of cholesterol-derived ketones that were involved (see, for example, (2)), although failing in their primary objective, led to some interesting UV observations on conformationally locked \(/c522\)-haloketones, and Jones later recalled the shifts in absorption maxima observed with the various isomers, might have led us to recognize that we were observing the effects of axial and equatorial substituents if it had occurred to us to make some efficient models, as Barton did so effectively 12 years later. It looked so obvious when the data were re-interpreted by R.C. Cookson in 1954 [Cookson 1954].
A 30% yield conversion of cholesterol to provitamin D₃, achieved in collaboration with Glaxo, with which Jones had a long consultancy relationship, was reported from Imperial College after the war (13). Also at Imperial College, Jones helped to determine the structure of zymosterol, the first natural sterol to lack a 5:6-double bond (4).

When Jones left London for Manchester in 1948, he was deeply involved in work on acetylene and triterpene chemistry and had no plans to continue with steroid studies. However, the discovery that cortisone was an effective therapy for rheumatoid arthritis stimulated a worldwide demand, and he joined in the search for good synthetic routes. Ergosterol (18), from yeast grown on raw sugar, seemed an obvious starting material, and with Henbest he developed a
10-stage route from it to the 5α-hydroxy-11:20-diketone (19), and thence to 20, cortisone acetate (scheme 10) (24, 27). Many others were similarly engaged, however, and he was later self-effacing, seeing this very elegant work as an academic exercise.

The cortisone–ergosterol work at Manchester led him to become interested in steroid ring B stereochemistry; with Meakins he clarified the situation regarding ergosterol and its photoisomers, establishing (35) that iso-pyrocalciferol is 9β-ergosterol and that pyrocalciferol is 9α-lumisterol.

Meakins continued as lieutenant in the last scientific programme initiated by Jones, on the microbiological hydroxylation of steroids (50). This, too, sprang from cortisone. The central problem in the derivation of precious cortisone from more accessible steroids was the oxidation of the 11-position. Murray & Peterson (1952) had shown that 11-hydroxylation of progesterone could be achieved microbiologically. Jones recognized that it was desirable to widen and define the scope of the approach by the systematic screening of large numbers of simple steroids as substrates for a range of fungi in culture. Techniques of chromatographic and structural analysis then available were not equal to the task, but by the 1960s it was practicable to embark on such an exercise, and in the last decade of his tenure of the Waynflete chair there was a steady stream of heavily data-laden papers. The principal aim was to map the relationship between functionalities presumed to be binding sites and the sites of hydroxylation. Typical were the results (48) obtained with 5α-androstanones and Calonectria decora, the 3-ketone being 12,15-dihydroxylated (21) whereas the 17-isomer gave the 1,6-dihydroxy compound (22; scheme 11).

These and other substrates demonstrated a regular pattern of equatorial dihydroxylation at CH groups about 4 Å apart. Several such generalizations were made, and much was discov-
Ewart Ray Herbert Jones

Erked about the directive effects of halo- and oxy-substituents, but it was essentially a fact-gathering campaign, and Jones realized that full understanding would need the molecular enzymology to be thrashed out. He had hopes that the programme would continue in that direction, and was disappointed that it was terminated when he retired.

Other areas

As perusal of the full bibliography will confirm, Jones’s excursions into topics outwith the areas so fleetingly surveyed above were few and relatively unsustained. But there was a series in the *Journal of the Chemical Society* (15) arising from wartime work on halolachrymators with D.H. Hey (FRS 1955), and another with E.A. Braude (14) on organic spectroscopy largely for its own sake, and a few papers (19) related to plant growth factors, such as Erxleben’s alleged auxin-b which was subsequently exposed (Vliegenthart & Vliegenthart 1966) as an elaborate fraud. And, finally, there were two papers (37, 40), with Lowe, on the structure, chemistry and biosynthesis of the complex fungal antibiotic tricothecin.

PUBLIC SERVICE

Throughout the 1960s, Jones was prominent in the committees of the Department of Scientific and Industrial Research and its successor the Science Research Council (SRC), and in late 1964 he was invited to be chairman of the newly formed Council for Science Policy. He declined, but wondered 25 years later whether the distribution of science funding might have been somewhat different if he had taken the helm. As it was, in 1967, together with Sir Ronald Nyholm FRS he protested at a proposal to allocate more than 40% of the next 10 years’ SRC funds to a European 300 GeV accelerator. There were, they wrote,

> other scientific fields which, cultivated and nurtured as nuclear physics has been in recent years, would yield still richer harvests…. We should urgently be seeking opportunities of investing comparable, and if possible larger, sums in projects which offer some prospect of material advantage to the community and which at the same time serve to train useful scientists.

Challenged by the physicists to say what exciting new area of chemistry worthy of a major initiative he had in mind, enzyme chemistry was his prime suggestion; he had been briefed by Knowles. The outcome was that an SRC Enzyme Panel was set up with Jones as chairman and Knowles as technical secretary. The Panel’s report led to major funding of the field, notably at Oxford, where the Oxford Enzyme Group flourished for nearly 20 years under the successive chairmanships of Sir Rex Richards FRS and Sir David (later Lord) Phillips FRS (Alton 1990).

FIRST PRESIDENT OF THE RSC

The Chemical Society (CS) and the Royal Institute of Chemistry (RIC), founded in 1841 and 1877 respectively, were both chartered bodies for chemists, but the CS was established as a learned society, to which any chemist (however defined and qualified) might belong, whereas the RIC was set up as a professional organization, membership being itself a professional qualification. There was perhaps a little of the Victorian snobbery between gentry and trade involved. By the mid-twentieth century both organizations were strong, and dwarfed the other
UK chemical associations. There was some overlap of membership, but the two remained distinct in ethos: the CS academic, with emphasis on its library and prestigious journals (which were proliferating), in the grandeur of Burlington House; the RIC in humbler Russell Square, concerned with qualifications, professional status, salaries—and indeed acquiring the characteristics of a trade union in small steps. It was recognized that it would be very desirable to have a single body as voice for the profession and subject of chemistry, but there were many obstacles—both legal and philosophical—and influential objectors. By 1971, agreement on the ‘amalgamation’ of the CS, the RIC, the Faraday Society and the Society of Analytical Chemistry (the Society for Chemical Industry remaining aloof) had been achieved, but in truth the process was less than the word implies, with the two main bodies continuing to do their own thing while working in close cooperation, but retaining their charters and integrity. Jones, who had been a faithful member of both organizations from early in his career, and had been President of the CS from 1964 to 1966, was President of the RIC from 1970 to 1972 when amalgamation took place. In getting that far, his running mate had been, as with the ‘small science versus big science’ controversy within the SRC, Sir Ronald Nyholm. Nyholm’s death in 1972 left Jones as leader in the further development of the relationship between the CS and the RIC. The next step was the formation of a committee to explore a complete merger in which each would give up its identity and charter, and the two would coalesce. Jones chaired this Unification Committee, and between 1975 and 1979 steered things with masterly diplomacy through many difficulties to the emergence of the RSC, which was granted its charter in mid-1980. He himself was elected to be its first President from 1980 to 1982, crowning his distinguished career.

PRINCIPAL APPOINTMENTS, DEGREES, HONOURS AND DISTINCTIONS

1929 Price Davies Scholarship, University College of North Wales (1929–32)
1932 BSc, first-class honours in chemistry, Wales
1933 First-class Diploma in Education, Wales
1935 University Fellowship, Wales (1935–37)
1936 PhD, Wales
1938 Assistant lecturer, Imperial College, London (Lecturer 1941–45, Reader and Assistant Professor 1945–47)
1940 Meldola Medal, RIC
1942 Elected FRIC
1943 DSc, Manchester
1947 Sir Samuel Hall Professor of Organic Chemistry, University of Manchester (1947–55)
1949 Tilden Lecturer, CS
1950 Elected FRS (Council 1969–71)
1952 Arthur D. Little Visiting Professor of Chemistry, Massachusetts Institute of Technology
1955 Waynflete Professor of Chemistry and Fellow of Magdalen College, University of Oxford (1955–78); MA Oxon.
1956 Curator of the University Chest, Oxford (1956–69)
1957 Karl Folkers Lecturer, Universities of Illinois and Wisconsin
1959 Pedler Lecturer, CS
Ewart Ray Herbert Jones

1960 Andrews Lecturer, University of New South Wales
1961 Member, Council for Scientific and Industrial Research (Chairman, Research Grants Committee 1961–65)
Upjohn Lecturer, University of California in Los Angeles
1962 Fritzche Award, American Chemical Society
1963 Knight Bachelor
Member, Advisory Council on Science Policy
1964 President, CS (1964–66)
1965 Member, Science Research Council (Chairman University Science and Technology Board 1965–69)
Hon. DSc, Birmingham University
1966 Davy Medal, Royal Society
Hon. DSc, Nottingham University
1967 Fellow, Imperial College, London
Foreign Member, American Academy of Arts and Sciences
Reuben Benjamin Sandin Lecturer, University of Alberta
Hon. DSc, University of New South Wales
1969 Hon. DSc, University of Sussex
1970 President, RIC (1970–72)
1971 Hon. DSc, Salford and Wales universities
1972 Hon. LLD, Manchester University
1973 CS Award for Services to the Society
1974 CS Award for Natural Product Chemistry
1975 Chairman, Joint Unification Committee of the CS and the RIC (1975–80)
1978 Hon. Fellow, Magdalen College, University of Oxford
Emeritus Professor, University of Oxford
Hon. DSc, East Anglia and Ulster universities
Robert Robinson Lecturer, CS
1979 Chairman, Anchor Group (1979–84)
1980 First President, RSC (1980–82)
1985 Dalton Lecturer, RSC
2000 Hon. Fellow, RSC

ACKNOWLEDGEMENTS

I am especially grateful to E.R.H.J.’s daughter, Celia Cohen, and half-brother, Keith Wolfenden, and to his close collaborators, particularly Mark Whiting and Viktor Thaller, for information and encouragement. Celia Cohen kindly arranged for me to have access to E.R.H.J.’s personal and genealogical papers, and to borrow a complete set of his scientific publications for the duration of my work.

E.R.H.J. did not respond to Royal Society requests for personal information until 45 years after his election, when, with apology, he deposited some detailed autobiographical notes that he had begun for family interest. These notes were amplified by a long taped interview that he gave in the academic year 2000–01 to Matthew J. D’Aubyn, whose Chemistry Part II thesis, ‘The history of the Dyson Perrins Laboratory 1955–1978: the era of Sir Ewart Jones’ (University of Oxford, 2001), was also very helpful. An edited transcript of the interview is on file at The Royal Society. I am grateful to Mr D’Aubyn for making his notes, tapes and other raw material freely available to me. E.R.H.J.’s written and oral evidence to the Oxford University Commission of Enquiry (the ‘Franks Commission’,...
1964–66) was revealing, as were his notes on the origin of the Oxford Enzyme Group (Bodleian Library, MS.Eng.c.2662/A.1.). Ms Hilary McEwan, Assistant Archivist at Imperial College, provided copies of papers concerning E.R.H.J.’s promotion there in 1945 and resignation in 1947. Dr James Peters, University Archivist at the John Rylands Library in Manchester, provided valuable material including extracts from the Vice-Chancellor’s Archive relating to E.R.H.J.’s appointment at Manchester in 1947. I was given access to E.R.H.J.’s personal file in Oxford University Archives, and also to the file that includes the papers concerning his election and negotiations with the university before his acceptance (OUA UR 6/CHE/3.File 1). Histories of the RIC and the RSC (Russell et al. 1977; Whiffen & Hey 1991) were valuable sources of background information on the journey towards unification of the two, the later stages of which can be traced in Chemistry in Britain (1976–82).

Many other people too numerous to list individually took the trouble to respond to enquiries, but thanks are offered to them all.

The photograph (ca. 1963) is reproduced by courtesy of the Library and Information Centre of the RSC (LIC number AR0565). The RSC also has an impressionistic 1990 portrait by Humphrey Ocean, and the National Portrait Gallery has a preliminary pencil drawing (NPG 6131) for this portrait.

REFERENCES TO OTHER AUTHORS


Anon. 1945 Growth inhibitor; hexenolactone oil may be a powerful ally in the war against deadly diseases. News Rev., 28 June, 20.


The following publications are those referred to directly in the text. A full bibliography appears on the accompanying microfiche, numbered as in the second column. A photocopy is available from The Royal Society’s Library at cost.

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Ewart Ray Herbert Jones

Biographical Memoirs


