JOHN CLIVE WARD
1 August 1924 — 6 May 2000
JOHN CLIVE WARD

1 August 1924 — 6 May 2000

Elected FRS 1965

BY NORMAN DOMBEY*

Department of Physics and Astronomy, University of Sussex, Brighton BN1 9QH, UK

John Clive Ward was a theoretical physicist who made important contributions to two of the principal subjects in twentieth-century elementary particle physics: namely, quantum electrodynamics (QED) and electroweak theory. He was an early proponent of the importance of gauge theories in quantum field theory and their use in demonstrating the renormalization of those theories: that is, to remove apparent infinities in calculations. He showed that gauge invariance implies the equality of two seemingly different renormalized quantities in QED, a relationship now called the Ward Identity. This identity can be generalized to more general gauge theories in particle physics and remains a fundamental tool in these theories, which dominate particle theory at the present time. He collaborated with Abdus Salam on the use of gauge theories in strong interactions and in electroweak theory. He also made significant contributions to statistical physics. In 1955 he was recruited by the UK Atomic Weapons Research Establishment at Aldermaston to head the Green Granite section of the theoretical group, which had the task of rederiving the thermonuclear weapon concepts developed by Ulam and Teller in the United States. He spent the years from 1966 to his retirement in 1984 at Macquarie University in Australia.

EARLY YEARS

John Ward was born in 1924 in East Ham, near Barking, just east of London. His parents were Joseph Ward, a civil servant in the Internal Revenue Department of the Treasury, and Winifred

*n.dombey@sussex.ac.uk

This memoir is dedicated to the memory of Kate Pyne and Freeman Dyson, who encouraged me to write the memoir of John Ward but were not able to see it.
Ward, a schoolteacher. He showed early promise in mathematics and won a scholarship to Bishops Stortford College, a private school where he boarded. He says in his memoirs (14)* that ‘the college was founded in the Victorian times to provide for the needs of the newly established Empire . . . in particular to create willing recruits for the Indian Civil Service. This was explicitly stated in the handbook that my father received’. He found little intellectual stimulus in the school until he studied science in the sixth form, where he was able to ignore the ‘two science masters who seemed to know very little of science’. The school, however, had a good science library, which he read voraciously and so began his lifetime habit of self-education and of not interacting with his peers. As he said: ‘being always a confirmed outsider, this habit became a lifelong resource.’ In 1941 he took the entrance examination at Oxford, and was awarded a Postmastership (open scholarship) at Merton College.

OXFORD AND QUANTUM ENTANGLEMENT

Ward studied mathematics in his first year and then transferred to engineering. He found very little in the course to interest him apart from a problem that A. M. Binnie (FRS 1960) was working on about the mechanical stresses due to gravity in symmetrical thin shells. Ward quickly realized that the solution only depended on the azimuthal dependence of the stresses, and Ward and Binnie (1) published the results in the *Aeronautical Journal* of the Royal Aeronautical Society. He obtained first class honours in engineering and in the following year a first in mathematics.

Maurice Pryce (FRS 1951) was appointed to the Wykeham Professorship of Physics in 1946, and Ward became his first graduate student. Pryce suggested that Ward should work on quantum electrodynamics (QED). Paul Dirac FRS (Dirac 1930) had shown that the two gamma rays resulting from the annihilation of a slow positron with an electron at rest would be polarized in perpendicular directions. J. A. Wheeler (ForMemRS 1995; Wheeler 1946) suggested an experiment to test Dirac’s result in which a slow positron beam incident on an electron at rest would create two photons

\[ e^+ + e^- \rightarrow \gamma (1) + \gamma (2), \]

where the photon polarizations could be measured. Two groups were planning to carry out this experiment, so Pryce asked Ward to verify Wheeler’s result that the numbers of photons polarized parallel and perpendicular to the scattering plane were different. Wheeler had calculated that the ratio of photons polarized perpendicular to the scattering plane to photons polarized parallel would have a maximum at scattering angle 74° 30′.

Ward later wrote (14) that Pryce rejected his early attempt to duplicate Wheeler’s results, and pointed out that the state vector describing the two photons with polarization labels \( \alpha \) and \( \beta \) travelling with momentum \( k \) and \( -k \) in an angular momentum state \( J = 0 \) had to be the singlet state

\[ |\alpha, \beta> = -|\beta, \alpha>. \]

That was ‘my first lesson in quantum mechanics and in a sense my last since the rest is mere technique’ (14). More exactly, Ward realized in his thesis (3) that the wave function of

* Numbers in this form refer to the bibliography at the end of the text.
photon 1 and photon 2 had to be symmetric under interchange as they satisfied Bose–Einstein statistics, and therefore the overall wave function is
\[ |1, 2> = (|\alpha, \beta> − |\beta, \alpha>)(|k, −k > − |−k, k>). \]  

[1]

In particular, the ground state wave function of positronium was known since it was just that of a hydrogen atom with proton mass equal to electron mass. Ward then proceeded to calculate the ratio of perpendicular and parallel polarization states for two-photon annihilation of slow positrons incident on electrons using Heitler’s (W. H. Heitler (FRS 1948); Heitler 1944) time-dependent perturbation theory applied to radiation. He showed that Wheeler had not used the correct two-photon wave function \(|1, 2>\) above and therefore had neglected interference terms. Ward’s result was that the maximum ratio occurred for \(82^\circ\) (see figure 1). Pryce and he then published the result in Nature (2). Shortly afterwards, Snyder et al. (1948) published a similar result in the Physical Review, although Ward’s calculation disagreed by a factor of 2 for the expected number of both perpendicular and parallel photon polarizations. Ward was correct.

Ward’s full calculation (3) (equation [2]) showed that the differential cross section for the process in terms of the scattering angles \(\theta_1\) and \(\theta_2\) of photons 1 and 2 and azimuthal angles \(\varphi_1\) and \(\varphi_2\) of their polarizations is proportional to
\[
\frac{(1 − \cos \theta_1)^3(1 − \cos \theta_2)^3 + 2}{(2 − \cos \theta_1)^3(2 − \cos \theta_2)^3} − \frac{\sin^2 \theta_1 \sin^2 \theta_2}{(2 − \cos \theta_1)^2(2 − \cos \theta_2)^2} \cos 2(\varphi_1 − \varphi_2). \]  

[2]

Hanna (1948) in Cambridge and Bleuler & Bradt (1948) in Purdue then published the results of their experiments. Taking account of the specific geometry used, the result of the Purdue experiment for the asymmetry ratio was
\[ e⊥/e∥ = 1.94 ± 0.37, \]
compared with the theoretical value of
\[ e⊥/e∥ = 1.7. \]
This was very satisfactory, although Hanna’s result did not agree so well with the theory. Einstein et al. (1935) had discussed the results of the measurements of the physical properties of two widely separated systems that can nevertheless be correlated. This is clearly the case here since the determination of the polarization of one of the photons gives information on the other’s polarization, even though the measurements can take place an arbitrary distance away. This is now referred to as ‘quantum entanglement’ and it follows from Ward’s two-particle wave function (equation [1]). The physics of quantum entangled states in recent years has brought together an interdisciplinary field of research involving quantum optics, quantum computing and the foundations of quantum theory. The structure of equation [2] with its $[A + B\cos(\varphi_1 - \varphi_2)]$ azimuthal dependence of the photon polarizations is typical of quantum entanglement (Duarte 2019).

Ward’s calculation showed that he was already in the top rank of theoretical physicists working on QED. He had corrected Wheeler’s result, which had won a major prize, had found a mistake of a factor of two in the results of Snyder et al., and had published his results in Nature before completing his doctorate. Yet, instead of submitting this calculation, he decided to work further on QED, where there had been exciting new developments.

**Quantum Electrodynamics and the Ward Identity**

The years 1947 and 1948 were especially important for the study of QED. Lamb & Retherford (1947) showed experimentally that the spectrum of the hydrogen atom according to the Dirac equation was not completely correct: there was a small correction to the $2S_{1/2}$ level that removed its degeneracy with the $2P_{1/2}$ state. In addition, the electron magnetic moment had been measured very accurately (Foley & Kusch 1948) and there was a small deviation from the predicted value according to the Dirac equation of 1 Bohr magneton.

In both cases the small corrections to the results obtained from the Dirac equation should have been calculable using second order perturbation theory, but those calculations turned out to give infinite results.

The next year, Julian Schwinger claimed that he could explain these results using a new fully relativistic field theory of electrons, positrons and photons. As he wrote in the introduction to his paper (Schwinger 1948): ‘The objectionable aspects of quantum electrodynamics are encountered in virtual processes involving particles with ultra-relativistic energies. The two basic phenomena of this type are the polarisation of the vacuum and the self-energy of the electron.’

The phrase ‘polarisation of the vacuum’ ‘describes the modification of the properties of an electromagnetic field produced by its interaction with the charge fluctuations of the vacuum ... through the virtual creation and annihilation of electron–positron pairs by the electromagnetic field ... The interaction between the electromagnetic field vacuum fluctuations and an electron ... modifies the properties of the matter field and produces the self-energy of the electron ... the vacuum polarisation effects are equivalent to ascribing a proper mass to the photon ... However the latter quantity must be zero in a proper gauge invariant theory.’ He goes on to show that, although the self-energy of the electron calculated to order $\alpha/2\pi$ was divergent, the physical mass $m$ was obtained when the self-energy was added to the bare or mechanical mass of the electron. Thus $m$ can be taken to be finite.
Similarly, the vacuum polarization process, although seemingly divergent, cannot contribute to the photon mass in a gauge-invariant theory, but does change the electron charge from its bare charge $\hat{e}$ to its physical charge $e$. The transformation from the bare charge and mass of the initial QED Lagrangian to the physical $e$ and $m$ is called ‘renormalization’ and allows finite results to be obtained in calculations, at least to order $\alpha/2\pi$. Using this approach, expressions for the Lamb shift and the anomalous magnetic moment can be obtained that agree very well with experiment.

Ward now attempted to generalize Schwinger’s result on the electron self-energy in an external electromagnetic field to first order in $\alpha/2\pi$ to all orders. In this he was successful, although he warned (3) that ‘it must nevertheless be repeated, and the forthcoming pages will underline this fact, that these results have only a rather formal interest. The detailed calculation of particular processes remains as difficult as ever’.

This problem was extremely difficult and one in which Pryce could not be of much help. Ward persevered, and in April 1949 he submitted his thesis (3): Part I contained his calculation of the angular correlation of polarization states in the interaction $e^- + e^+ \rightarrow \gamma + \gamma$ while Part II described his generalization of Schwinger’s results. The whole thesis ran to only 46 pages.

Rudolf Peierls FRS from Birmingham was external examiner for Ward’s thesis, with Jack de Wet as internal. Although Ward thought they gave him a hard time at his oral examination (he wrote (14) that Peierls ‘declared the thesis unworthy of acceptance’), the written record gives a very different account of Peierls’ views and the examiners are clear (de Wet & Peierls 1949). The report on Part I states: ‘This shows the author’s competence in using methods of modern theory for a complex problem on which wrong statements had previously been published by distinguished people’; and on Part II: ‘While in a subject of such rapid development and on which many experienced people are working intensely one cannot expect a D.Phil. candidate to make a major and lasting contribution, the candidate has succeeded in sorting out the new method from other scanty published information, and has succeeded in putting it in a form not given by Schwinger, which offers considerable advantage’.

Pryce summarized Ward’s doctoral work at Oxford thus: ‘Ward made seminal contributions to field theory even while still a student, but he was so brief in his explanations that people found it difficult to grasp his brilliant ideas’ (Elliott & Sandars 2005).

Ward began his postdoctoral career at Oxford in October 1949, after important new results in QED had been published. In particular, Dyson (1949) conjectured that all the divergences of QED were contained in the mass and charge renormalization to all orders of $\alpha/2\pi$. Dyson had improved Schwinger’s renormalization technique by including the divergences in multiplicative constants, rather than removing them by subtraction. Then, in terms of the renormalized fields $\hat{\psi}(x)$ and $\hat{A}_\mu(x)$, charge $e$ and mass $m$ can take their experimental values, and Dyson conjectured that all orders of the perturbation series in terms of the renormalized charge and mass are finite. The renormalized electron and photon fields are obtained from the original bare electron spinor fields $\hat{\psi}(x)$ and photon fields $\hat{A}_\mu(x)$ by

$$\psi(x) = Z_2^{-1/2}\hat{\psi}(x), \quad [3a]$$

$$A_\mu(x) = Z_3^{-1/2}\hat{A}_\mu(x), \quad [3b]$$

where $Z_2$ and $Z_3$ are divergent quantities corresponding to the Feynman diagrams resulting from the photon and electron self-energies, and are defined by the appropriate integrals in momentum space. They can be evaluated using a high momentum cutoff. $Z_1$ is a similar
divergent quantity arising from the radiative corrections to the $\gamma ee$ vertex, where $\gamma$ refers to a photon and $e$ refers to an electron.

Dyson (1949) then showed that the relation between the bare electron charge $\hat{e}$ and the physical renormalized charge $e$ is

\[ e = Z_1^{-1} Z_2 Z_3^{1/2} \hat{e}. \]  

Adding one-loop processes, that is, adding the electron self-energy term, the vacuum polarization correction to the photon propagator and the correction to the vertex term due to a single photon being exchanged between the initial and final electrons allows the divergent quantities $Z_i$ to be calculated to order $\alpha/2\pi$. The $Z_i$ are needed to obtain finite results when the renormalized fields are used. Dyson conjectured that $Z_1 = Z_2$ to all orders as it does in first order. This result would lead to a simplification of the proof of the renormalizability of higher order terms. Ward set out to prove this result to all orders.

He considered that gauge invariance

\[ \hat{\psi} \rightarrow e^{-i\hat{e}^\Lambda} \hat{\psi}, \quad \hat{A}_\mu \rightarrow \hat{A}_\mu + \partial_\mu \hat{\Lambda} \]  

must apply to the initial Lagrangian involving the bare operators $\hat{\psi}$ and $\hat{A}_\mu$ as the bare photon field described a massless particle. It should also apply to the renormalized operators $\psi$, $A_\mu$ in the renormalized Lagrangian since the mass of the physical photon is zero; that is, the renormalized Lagrangian is invariant under

\[ \psi \rightarrow e^{-ie\Lambda} \psi, \quad A_\mu \rightarrow A_\mu + \partial_\mu \Lambda. \]  

For equations [5a] and [5b] to be simultaneously true, the phases must be equal:

\[ e\Lambda = \hat{e} \hat{\Lambda}. \]  

Using equation [3b] we find $\Lambda = Z_3^{-1/2} \hat{\Lambda}$, and substituting equation [4] into equation [6] we obtain

\[ e\Lambda = Z_1^{-1} Z_2 Z_3^{1/2} \hat{e} Z_3^{-1/2} \hat{\Lambda} e \hat{\Lambda}. \]

Therefore

\[ Z_1 = Z_2. \]  

This is now known as the Ward Identity and is true to all orders in $\alpha/2\pi$. The use of the Ward Identity reduces higher order self-energy terms, which contain so-called overlapping divergences that are difficult to renormalize, to vertex terms that are free of overlapping divergencies and can be renormalized. Dyson’s QED renormalization programme can therefore be carried out to all orders in $\alpha/2\pi$.

Ward’s paper entitled ‘An identity in quantum electrodynamics’ (4) was written in his typical style: it had just one reference (to Dyson), and eight equations, and it took up less than half a page of the Physical Review.

Dyson told me* more than 60 years later that ‘the Ward papers on overlapping divergences demonstrated the deep connection between gauge invariance and renormalisability, which was another major step on the road to the standard model, namely, the modern gauge theory of weak, electromagnetic and strong interactions. Ward did not make QED, but he transformed

* Copies of key correspondence relating to this memoir are available on request from the author.
John Clive Ward

QED so that it fitted into the context of modern gauge field theory’. Subsequently his work was
generalized to more general gauge theories by Takahashi (1957), Taylor (1971) and Slavnov
(1972).

SALAM AND GAUGE THEORIES

Abdus Salam (FRS 1959) from Pakistan and Richard Dalitz (FRS 1960) from Australia
were doctoral students of Nicholas Kemmer (FRS 1956) at Cambridge when Ward was a
doctoral student at Oxford, and both became lifelong friends with him. Dalitz carried out
an independent calculation of the Hanna experiment, which agreed with Ward, while Salam
(1952) wrote a PhD thesis on the renormalization of QED including higher order overlapping
terms in perturbation theory. After Oxford, Ward went on to the Institute of Advanced Studies
at Princeton, where Dyson worked, to continue his work on renormalization, as did Salam.

Salam subsequently returned to the UK and, after a spell back in Cambridge, was appointed
professor of theoretical physics at Imperial College, while Ward stayed in the US supported
by postdoctoral appointments for several years. Salam’s student, Ronald Shaw, wrote a thesis
(Shaw 1954) about what would happen if charged massless photons existed as well as a neutral
massless photon and formed an isovector triplet of particles $A_\mu$ coupled to the isovector current
\[ i \bar{\psi} \gamma_\mu \tau \psi, \]
and $\tau$ is the isovector formed of Pauli spin matrices. So, instead of equation [5b], the equations
of gauge invariance now become
\[ \psi \rightarrow e^{-i\tau \cdot A/\Lambda} \psi \quad A_\mu \rightarrow A_\mu + \partial_\mu \Lambda + \tau \times A_\mu, \quad [8] \]
and $\Lambda$ is an arbitrary isovector.

Yang & Mills (1954) published essentially the same analysis a short time later,
emphasizing that a theory of this type with a conserved isotopic spin current and the
isovector gauge invariance of equation [8] would lead to the conservation of isotopic spin.
Mathematicians would call the isotopic invariance of the theory based on the Pauli spin
matrices $\tau_x, \tau_y, \tau_z$ the Lie group SU(2) generated by the Lie algebra composed of $\tau_x,
\tau_y, \tau_z$.

The work of Yang, Mills and Shaw initiated a revolution in theoretical particle physics.
Sakurai (1960) assumed that SU(2) could be applied to strong interactions, and used it to
describe the properties of the newly-discovered spin-1 particles $\rho^+, \rho^0, \rho^-$, which thus formed
an isotopic vector. They were assumed to be the massless gauge particles of Yang, Mills and
Shaw, but somehow had acquired mass. In electromagnetic and weak interactions, Schwinger
(1957), and then his student Glashow (1961), thought that they could bring together QED and
weak interactions by basing an electroweak theory on Yang, Mills and Shaw’s innovations,
where the gauge theory was based on SU(2) for weak interactions, while keeping QED
invariant under the one-dimensional unitary group U(1). If there were a common electroweak
coupling, the new electroweak spin-1 particles $W^+, W^0, W^-$ would have the very high mass
of about 90 GeV: this was determined by the ratio of electric charge $e$ to $G^{1/2}$, where $G$ is the
Fermi coupling of nuclear $\beta$-decay.

Ward joined Salam in an attempt to investigate gauge theories based on Lie groups applied
to both strong interactions and electroweak interactions. They started with electroweak theory
in 1959 with a paper ‘Weak and electromagnetic interactions’ (9). This was modelled on Yang, Mills and Shaw where the basic isovector triplet now referred simply to \((W^+, W^0, W^-)\), where \(W^+\) and \(W^-\) mediated weak interactions and \(W^0 = \gamma\) was a photon. But there was no explanation of how weak interactions violated parity while electromagnetic interactions conserved it. Nor was there a reason for the large mass difference between theWs and the photon.

In 1961 they published three papers (10, 11, 12) on a gauge theory of strong interactions, which included both isotopic spin and strangeness so that they could include the \(K\). They also varied the Lie group. The special unitary group SU(3) in three dimensions and the symplectic group in four dimensions Sp(4) were both considered. The vector mesons in the theories were then determined by the gauge group: octets arise naturally in SU(3) and decuplets in Sp(4). The octet of vector mesons would consist of the isovector particles \((\rho^+, \rho^0, \rho^-)\) and isoscalar \(\omega^0\) (as Sakurai had also assumed) and, in addition, four strange vector mesons with the same strangeness and isospins as the kaons.

Salam’s student Ne’eman was working along similar lines using SU(3) and obtained the same octet of vector gauge particles as Salam and Ward, and in addition put the neutron and proton in an SU(3) octet with the same quantum numbers as the vector meson octet (Ne’eman 1961). He was thus able to include the strange isotopic baryon triplet \((\Sigma^+, \Sigma^0, \Sigma^-)\) and the strange isoscalar \(\Lambda\) in the octet.

Salam and Ward’s attention returned to electroweak theory in 1964. They began with their 1959 work (9), but, instead of the triplet \((W^+, \gamma, W^-)\), they took an approach in which the triplet \((W^+, W^0, W^-)\) represented the vector mesons of the weak interactions while the field \(A_0\) represented the photon. Then, in order to obtain a single electroweak theory, they allowed the field of the neutral weak vector meson \(W^0\) to mix with the field \(A_0\), obtaining

\[
A = A_0 \cos \theta + W^0 \sin \theta \quad Z = -A_0 \sin \theta + W^0 \cos \theta,
\]

[9]

with electroweak mixing angle \(\theta\), where now \(Z\) is a new vector meson which becomes the quantum for a new electrically neutral weak interaction, while \(A\) is the photon field after mixing. So this model, based on the gauge group SU(2) \(\times\) U(1), predicted a neutral weak current and the relationship between the electromagnetic and weak charges is

\[
e = g \sin \theta,
\]

[10]

where \(g\) is the coupling constant for the \(\bar{\nu} e^- W^+\) and the \(\bar{\nu} \mu^- W^+\) vertices: \(\bar{\nu}\) is an anti-neutrino and \(e^-\) and \(\mu^-\) are an electron and muon respectively (Salam and Ward assumed that the coupling of the W to the electron and neutrino was the same as its coupling to the muon and neutrino). Thus, the Fermi coupling \(G\) for muon decay into an electron, neutrino and anti-neutrino due to heavy \(W^+\) exchange is given by \(G\):

\[
G = \frac{g^2}{\sqrt{2} \cdot 8M_W^2}.
\]

[11]

Therefore, using equations [10] and [11], the W mass is given by

\[
M_W = e/(2^{5/4} \sin \theta G^{1/2}) = (37.3/\sin \theta) \text{GeV}.
\]
It is surprising, however, that their paper (13) was published because it only reproduced the results of Glashow three years earlier. Furthermore, the authors did not note that the W mass must be at least 37.3 GeV and would be determined once $\sin \theta$ was measured.

AN ITINERANT PHYSICIST AND STATISTICAL PHYSICS

From 1951 to 1966, Ward worked at the Institute for Advanced Study at Princeton (1951–52, 1954–55, 1960–61), the Bell Laboratories (1952–54), the University of Adelaide (1954), the University of Maryland (1956–57), the University of Miami (1957–59), the Carnegie Institute of Technology (1959–60) and Johns Hopkins University (1961–66). He was thus continually in the United States, except for 1955 when he was back in the UK and 1954 in Adelaide, where he had accepted a position at the university but left after a few weeks to go back to the Institute of Advanced Study at Princeton.

His work on QED and gauge theories is discussed above, and yet he worked on various aspects of many-body theory and statistical physics as well. For this work, I will rely on Roger Elliott’s (FRS 1976) recollections. Elliott knew Ward as a fellow graduate student, and he told me that Ward wrote papers (5) and (6) about second sound in liquid helium with Wilks. Subsequently, Ward showed the enormous range of his versatility by making at least two significant contributions in the area of statistical physics: with Mark Kac (7) he obtained Onsager’s solution to the two-dimensional Ising model in a more direct way using combinatorial mathematics, with another remarkable mathematical tour de force; and working with Montroll, he showed that the state of the electron gas that had been extensively studied at absolute zero could be extended by what was effectively a form of Debye–Hückel theory. In this work (8), he was also probably the first to use the periodicity of properties in inverse temperature. Either of these examples would have ensured him an enviable position among statistical physicists, but they remained for him a side interest.

MACQUARIE

Throughout his itinerant years, Ward was uneasy with American physics graduate schools. According to his memoirs, they were established so that ‘physicists would be created by a production line of suitable professors, financed by the splendid generosity of government institutions. . . . This resulted in an absurd inflation of theoretical physics in particular, aided and abetted by publishers of innumerable semi-fraudulent science journals’.

Dalitz and Salam often urged him to return to the UK when vacancies arose.* The necessity of publishable papers was not as extreme in Britain as in the US, but it was probably just a matter of time before Britain caught up, and so he did not take up these opportunities. He did, however, allow his name to go forward to be a candidate for a Fellowship of the Royal Society, to which he was elected in 1965. This helped him in seeking a professorship outside the US and Britain.

* Salam’s correspondence with Ward is held in the Salam archive of the Salam International Centre for Theoretical Physics in Trieste, Italy; Dalitz’s correspondence is held in the Radcliffe Science Library, Oxford, UK. Unfortunately, at the present time neither is publicly available.
In 1966 Freddie Chong, an old friend who had just been appointed professor of mathematics at Macquarie University in Sydney, persuaded him to move to Macquarie, which was a new university.

Unfortunately for Chong and Ward, Macquarie in 1966 was run by non-scientists, and most students were teacher trainees studying for a BA degree. Physics was a new subject there, and Ward was in charge of developing courses and recruiting faculty. He enjoyed undergraduate teaching and found that he much preferred this to the supervision of endless graduate students in the search for ‘golden eggs’, as he termed research achievements. He also introduced a Master’s course for local physics teachers, based on the Feynman Lectures, that was very successful. He supported Duarte and the science students who campaigned for a BSc degree to be awarded as well as the BA, and this goal was finally attained in 1979.

Ward remained at Macquarie from 1966 until his retirement in 1984. Yet he did not publish in a recognized physics journal while he was there. His interest in particle theory continued, but he insisted that the publication of any new paper of his must meet the standards of his previous contributions to physics.

According to Duarte (2009): ‘at Macquarie he [Ward] became known for his forceful defence of science, high academic standards, and for his uncompromising honesty. In this regard, he openly and vigorously supported the student science reform movement that permanently changed the degree structure of the university. This transformative innovation strengthened significantly the structure of the sciences at Macquarie.’

**Fermi, Ulam and Teller**

In the 1980s, I met several of the British, American and Soviet physicists involved in nuclear weapon development while I was a member of the British Pugwash Group, which supported arms control. I also discussed Ward’s time at Aldermaston with him. This section consequently has a rather more personal approach than the others.

After the Second World War, QED and quantum field theory became the principal research interests of theoretical physicists. Robert Oppenheimer (ForMemRS 1962), the director of the Manhattan Project, moved into civilian life as the director of the Institute of Advanced Study at Princeton. He convened conferences in 1947 and 1948 on the subject of ‘Foundations of quantum mechanics’, in particular on progress in QED in the light of the new experiments on the Lamb shift and the anomalous magnetic moment of the electron. The first conference was held in Shelter Island, New York, and the second in the Pocono Mountains in Pennsylvania (Schweber 1994). The guest lists for both were very restricted. Among the discussion leaders were Oppenheimer, Hans Bethe (ForMemRS 1957), Victor Weisskopf, Richard Feynman (ForMemRS 1965), Robert Serber, Enrico Fermi (ForMemRS 1950) and Edward Teller, all of whom had played important roles at Los Alamos in the Manhattan Project during the war (Hawkins 1983).

After the war there was much discussion between physicists led by Oppenheimer, who considered that fission weapons were sufficient in any future war, and physicists led by Edward Teller, who considered that thermonuclear weapons based on the fusion of light elements were needed. Fission weapons typically had an explosive yield measured in kilotons of TNT, while the yield of a fusion weapon would be measured in megatons. President Truman decided the question on 31 January 1950, when he announced that he had directed the US Atomic
Energy Commission ‘to continue its work on all forms of atomic weapons including the so-called hydrogen or superbomb’; Rhodes (1995) gives a full account of the debate between Oppenheimer and Teller, and Truman’s eventual decision.

The major physics problem involved in a hydrogen bomb is that the energy release arises from the fusion of deuterium and tritium:

\[ D + D \rightarrow ^3\text{He} + n + 3.3 \text{ MeV}, \]
\[ D + D \rightarrow ^3\text{H} + p + 4.0 \text{ MeV}, \]
\[ D + T \rightarrow ^4\text{He} + n + 17.6 \text{ MeV}. \]

D and T are positively charged and so, in order to fuse, they have to surmount the Coulomb barrier to get within a nuclear radius \( r_D \) or so of each other. The height of the Coulomb barrier when the two nuclei encounter each other is then about \( \alpha/r_D \), where \( \alpha \) is the fine structure constant; so the barrier height is many keV. Deuterium or tritium must therefore be heated to a temperature of tens of millions of degrees to have a chance of extracting explosive energy from fusion. This can only be achieved by exploding a fission weapon. Furthermore, at such high temperatures all the matter is completely ionized and every electron and positive ion will radiate energy away.

In August 1945 Enrico Fermi gave a series of lectures at Los Alamos on the current state of work on the Super, a possible thermonuclear weapon. Members of the British Mission still at Los Alamos were present. Philip Moon (FRS 1947) took notes, which he passed on to the authorities in London, while Klaus Fuchs, who was subsequently convicted as a Soviet spy, passed his notes on to Moscow; both sets of notes are now available (Moon 1945; Goncharov & Maksimenko 2008). They show that Fermi’s conclusions were that, for ignition, it would be necessary to heat the D and T ions to a higher temperature than that of the radiation and that therefore thermal equilibrium of the matter and radiation was not possible, and that compression of the heavy hydrogen fuel made no difference.

Throughout 1950 Ulam and Everett calculated the dynamics of deuterium plus various amounts of tritium at the temperature attainable using a fission weapon. They found that the Super would fizzle out unless unrealistically large amounts of tritium were used.

In 1951 everything changed: Ulam & Teller (1951) wrote their report, which laid the foundation for modern H-bombs. On 1 November 1952 there was a successful test of Ulam & Teller’s ideas when a device, codenamed Mike, was exploded in the Marshall Islands, yielding 10.4 megatons of energy. Mike weighed 82 tons and hence was not a deliverable weapon, but was proof that the Ulam–Teller (U-T) concept worked (Rhodes 1995).

In Britain there was a similar debate about whether or not to attempt to build a hydrogen bomb. The UK had first tested an atomic bomb in October 1952, but, after the successful Mike test in 1952 and especially the Soviet announcement of a successful H-bomb test yielding 400 kilotons in August 1953, pressure was growing in military and political circles for Britain to build an H-bomb. Churchill was prime minister at the time, and he was heavily influenced by his scientific advisor, Lord Cherwell (FRS), who was head of the Oxford Physics Department. In 1953 ‘Cherwell, in fact, had already been press[ing Penney [FRS; the head of Britain’s nuclear weapon programme] to start work on the superbomb’ (Penney & Macklen 1988). The government decided in July 1954 to build the H-bomb, and the Atomic Energy Weapons Research Establishment (AWRE) at Aldermaston in Berkshire became the main site for the H-bomb work. No one at Aldermaston knew what Ulam and Teller had
done, since the US McMahon Act forbade any transmission of nuclear information to other countries.

Ward writes in his memoirs (14) that one day he received a letter from Francis Simon (FRS), the head of the Oxford Low Temperature Group, suggesting that he return to Oxford. Cherwell had brought Simon, an old acquaintance who was Jewish, to Oxford in 1933 after Hitler took power. In his governmental role, in 1953 Cherwell wanted to attract theoretical physicists who might be able to help Penney rediscover the U-T concept. Who better than an Oxford graduate with a worldwide reputation in QED, just like the American nuclear weapon physicists? Simon was clearly acting on Cherwell’s behalf. In his memoirs, Ward says that Hans Kramers had been impressed by Ward’s work on both renormalization and the Ising model, and had written to Simon saying that Simon should try to attract him back to Britain. Dalitz searched for Kramers’ letter in Simons’ papers, but could not find it. I think it is more likely that Simon was writing on behalf of Cherwell and that Kramers had nothing to do with Simon’s letter.

After the government’s decision to proceed, William Cook (FRS 1962) joined Penney as his deputy with responsibility for designing the H-bomb. Cook began work at Aldermaston in September 1954 and immediately advertised for theoretical physicists. Ward applied and, according to his memoirs (14), Cook told him that the matter was urgent and that his presence was very much desired. Ward returned to the UK and began work at Aldermaston in June 1955. Ward writes in his memoirs that a few days after he arrived, ‘there was a formal meeting, chaired by Penney, of about 20 senior staff. He declared that I would be in charge of Green Granite, the codename for the development of a U-T device’. A few days later he was called to Penney’s office with Keith Roberts, a theoretical physicist on Aldermaston’s staff, and told what was currently known of U-T at Aldermaston, namely that the device involved two stages and that neutron shielding was involved. That was it. It was up to Ward, with Roberts’ assistance, to rediscover what Ulam and Teller had worked out.

Everything about the British H-bomb was still classified in the 1980s. I was interested in nuclear history, and knew that Ulam and Teller were the originators of thermonuclear weapons in the USA and that Sakharov and Zeldovich were responsible for Soviet thermonuclear weapons. I wondered who their counterparts were in the UK.

In 1991 I was reading US nuclear weapons: the secret history (Hansen 1988) when I came across a footnote quoting Ward: ‘To my amazement, when (in 1955) I reached Aldermaston . . . I was assigned the improbable job of uncovering the secret of the Ulam-Teller invention . . . an act of genius far beyond the talents of the personnel at Aldermaston, a fact well known to both Cook and Penney.’ Hansen said that Ward had written a letter to Prime Minister Thatcher claiming that he had succeeded in his task and that his work should be recognized (see Appendix). I had met Ward in the summer of 1964 when we were both visiting Brookhaven. Maurice Pryce visited Sussex occasionally in the 1990s, and I knew he had been Ward’s doctorate supervisor. He confirmed that Ward had been employed at Aldermaston, so I wrote to Ward at Macquarie on 30 April 1991 asking whether he ever came to Europe since I would like to discuss his time at Aldermaston. At a seminar on British post-war nuclear policy the previous year, I had asked former senior Aldermaston officials for the names of the British equivalent of Ulam and Teller and they all insisted that everything was done collaboratively.

Five months later I received a reply from Canada. Ward had retired and left Macquarie. He said that he remembered me and that ‘Anyone who believes that radiation implosion could be
invented by committee should be locked up. Quite simply I was drafted in to tell them how to do it, and Penney refused to listen’. He said that he would be in Portugal in December. In fact he delayed his visit to Portugal to April 1992, but I received a phone call that autumn saying that he was in Calais and could I come to see him? So, the following day I set out by train and boat to Calais. We both recognized each other and he told me his Aldermaston story, which was basically what he subsequently wrote in his memoirs (14).

In April 1992 I went to see him in Cascais, a pleasant coastal town near Lisbon. Ward wanted to tell his story more widely, and I had arranged via a colleague at Lisbon University for a journalist on Publico, a Lisbon newspaper, to interview him. I wrote ‘40 years of hydrogen bombs’ to provide an introduction, which was translated as ‘A bomba de hidrogenio 40 anos depois’ (Dombey 1992). When I arrived at Cascais, I showed Ward my piece. I had written that Ulam ‘was struck by the idea of the two-stage process and realized that the electromagnetic radiation from the primary explosion could be reflected by a heavy, naturally occurring metal such as uranium on to the secondary’. Ward became very agitated. ‘Not reflected’, he shouted. I thought for a bit and couldn’t understand why not. Then I said tentatively ‘black-body radiation’. He calmed down and nodded.

Ward told his story in the interview and then the military historian Eric Grove and I followed it up in the London Review of Books in an article called ‘Britain’s thermonuclear bluff’ (Dombey & Grove 1992). As a result, when Aldermaston’s historian, Lorna Arnold, published her official account, Britain and the H-bomb (Arnold 2001), Appendix 5 was devoted to Ward’s claim to have re-invented U-T. Arnold inspected the written record for 1955 and confirmed that Ward had been group leader of ‘new devices’ and that Roberts helped him, even though ‘he [Ward] had, as he said, worked almost entirely alone’. Ward did not publish a paper while he was at Aldermaston. Roberts, however, had written a paper just after Ward had left Aldermaston, which referred to Ward and must have included his results (Roberts 1955).

In 1993 Ward came to Britain, where I introduced him to Lorna Arnold. He didn’t say anything new, but drew some sketches of bombs that he gave to her. In her book, Arnold says that ‘it seemed clear from his answers and the sketch that the Ward concept, whatever its intrinsic value, had not been the basis of the various Grapple devices (a fact he could not have known without access to later British work)’.

Kate Pyne, Lorna Arnold’s successor, was interested in a further study of Ward’s contribution. She wrote to me on 29 July 2008 saying that ‘I really don’t know what it was that Professor Ward claimed to have found or discovered all those years ago’. Then, on 20 June 2015, I received an email from her saying that ‘I don’t wish to denigrate Lorna Arnold’s memory, but I didn’t agree with her about relegating Dr Ward to a relatively short Appendix in Britain and the H-bomb. I’m sure a more appropriate tribute could be written so I’m glad to hear that you’ve contacted the RS’.

Kate Pyne died from a pulmonary embolism about 90 minutes after she sent me the email. This rather long section is my attempt to fulfil her wishes. Her own discussion of Ward’s contribution is contained in her posthumous thesis at King’s College London, where chapter 5 is called ‘In the matter of John Clive Ward: work on thermonuclear warheads in Britain between June and December 1955’ (Pyne 2016). Unfortunately, the thesis is kept at Aldermaston under lock and key. Kate clearly did find progress as a result of Ward’s presence at Aldermaston, so I will give my answer to what he had discovered using readily accessible material and some elementary physics.
Freeman Dyson explains the basis of the U-T concept in his biographical memoir of Teller (Dyson 2007):

In 1950 electronic computers were able to simulate in a rough fashion the Classical Super design for a hydrogen bomb and showed that it did not work . . . For eight years his [Teller’s] thoughts had been fixed on the Classical Super, which required deuterium to burn at low density, so that radiation could escape from the burning region and not come to thermal equilibrium with the matter [see Fermi’s lecture notes, discussed earlier]. The idea was to achieve a runaway burn with temperature of the matter remaining much higher than that of the radiation. The computers showed that runaway burn did not work. So Teller began to look seriously at the opposite situation, with deuterium at high density and the radiation trapped in thermal equilibrium with the matter. Teller found that at high density, deuterium could burn well in thermal equilibrium with the matter . . . Stanislaus Ulam at Los Alamos thought of a similar arrangement at the same time, and so the idea became known as the Teller–Ulam design.

Teller had shown in his article in Encyclopedia Americana (Teller 1976) that the primary and secondary are inside a uranium-238 container. In addition, Dyson’s note shows that the crucial ideas of the U-T concept are: (i) a physically-separated primary and secondary; (ii) thermal equilibrium between radiation and matter; and (iii) high compression of the secondary. In the minutes of the meetings between US and UK physicists in 1958, after the UK had demonstrated that it could explode H-bombs, it was reported that the UK had tested two-stage radiation implosion weapons (US Atomic Energy Commission 1958). Ward claimed in letters to Dalitz and me that he had re-invented U-T concept (see Appendix) and in particular radiation implosion.

Penney was aware in 1955 that a two-stage device should be used, so Ward was not responsible for that idea. That compression of the deuterium (or deuterium plus tritium) fuel was necessary was known to him: in his memoirs he remembers that at an important meeting at Aldermaston he ‘emphasized the need for compression’ (14). He also wrote to Dalitz in May 1997: ‘After my resignation it was realised my ideas about radiation implosion, compression and subsequent heating were all correct’. The need for high compression for ignition in inertial fusion experiments is demonstrated in The physics of inertial fusion, where the compression necessary to ignite 1 mg of DT fuel is shown to be 1500 (Atzeni & Meyer-ter-Vehn 2004). That compression helps ignition is not difficult to understand since compression will push the deuterium or tritium nucleus up the Coulomb potential barrier so that less energy and thus a lower temperature is needed for fusion.

When Ward insisted that I must not say that the uranium container reflected the radiation from the primary on to the secondary, and that black-body radiation was the mechanism, it could only mean that he envisaged that the radiation was in equilibrium with the matter inside the container. Physics undergraduates are taught how to calculate electromagnetic radiation pressure, and at room temperature it is negligible. But the Stefan–Boltzmann law shows that the relation between the radiation pressure $S$ and the equilibrium temperature $T$ is

$$S = AT^4 W \text{ cm}^{-2}, \quad [12]$$

where $A$ is a constant. The temperature reached by an A-bomb is about $50 \times 10^6$ K, so that the pressure arising from the radiation at that temperature in equilibrium would dwarf any pressure arising from matter. Using radiation to compress the secondary to ignite the heavy hydrogen is called ‘radiation implosion’. Ward was clearly considering the situation where
radiation and matter were in equilibrium when he was talking to me, and that must have been
the result of his work in 1955. Atzeni & Meyer-ter-Vehn (2004) state that the radiation flux on a
target in a cavity of gold increases as $T^4$ when ‘multiple absorption and re-emission processes
lead to a thermal distribution of photons in the cavity described by black-body radiation’ in
inertial fusion (this is known as a hohlraum configuration). The use of radiation to obtain
extreme pressures and therefore extreme compression followed from the thermal equilibrium
of radiation and matter that Ward introduced, just as Ulam and Teller had done four years
earlier.

Ward did not re-invent the complete U-T concept, but it seems to me that he did do what
was asked of him; namely, that given the requirement of a two-stage device, he realized that the
two stages had to be in a heavy metal container, that the U-T concept was based on thermal
equilibrium between matter and radiation, and that this allowed the matter to be extremely
compressed by radiation, thereby providing the conditions for fusion to take place. Thus, Ward
re-invented radiation implosion at Aldermaston. That was his crucial contribution.

Just as Ulam and Teller had done before him, Ward realized that to make progress, Fermi’s
ideas for the Super had to be discarded. Roberts may have helped him, but given Ward’s
characteristic intensely personal working habits, it is much more likely that Ward had the
ideas of both thermal equilibrium and extreme compression and asked Roberts to do various
associated calculations such as the calculation of uranium opacity referred to in his memoirs.
Roberts then reported on their work just after Ward had left Aldermaston.

Richard Moore, Kate Pyne’s successor at Aldermaston, told me that Ward’s contribution
was reviewed at length by official historian Lorna Arnold, who concluded firmly that Ward’s
ideas were ‘not the basis of the British H-bomb’. Her successor Kate Pyne reviewed the evidence
available to Arnold again in a later study, which suggested that Ward might ‘possibly’ have
contributed one of the key concepts, radiation implosion, to the design process. However, others
were also working on these concepts and, when Ward left AWRE, the path to Britain’s H-bomb
was still not yet clear. The weight of evidence from all of the relevant minutes, papers and
drawings led Pyne to conclude that, far from relying on a single dramatic insight, H-bomb science
was a fundamentally collaborative process involving a team of people working on a wide range
of ideas and calculations.

I do not agree with these conclusions. I hope that I have demonstrated that Ward did have
a ‘single dramatic insight’; namely, that matter and radiation could be in thermal equilibrium
and that, in that event, radiation pressure was proportional to the fourth power of temperature.
That, in turn, meant that radiation implosion of the secondary was the dominant process in its
detonation.

That there was a subsequent ‘collaborative process involving a team of people working on
a wide range of ideas and calculations’ is not disputed, nor did Ward design the British H-
bombs tested in the Grapple series. The principal designers of those were Keith Roberts and
Bryan Taylor (FRS 1970), just as the principal designer of Mike was Richard Garwin (Broad
2001). But both the British and US H-bombs depended on thermal equilibrium, radiation
implosion and high compression. Ward was responsible for introducing thermal equilibrium
and radiation implosion into the British programme, thereby abandoning Fermi’s ideas, just as
Ulam and Teller were in the United States.
John Ward ended up as an embittered man. Dyson told me that ‘he became obsessed with the lack of recognition of his achievements. At the end of his life he was a tragic figure, isolated by his own querulous complaints’. Frank Duarte, who spoke to Ward frequently towards the end of his life, told me that ‘I agree with Freeman Dyson that John was bitter about his lack of recognition. However from the late 1970s to late 1990s time appears to have done some healing and tempered that bitterness’.

His Macquarie pension may not have been bad, but Ward would not have got much income from his short periods at American universities. In addition, he thought that he should be financially compensated for his work at Aldermaston. He even persuaded Salam to write to Mrs Thatcher (Salam 1985), saying that ‘I strongly feel that, at the time of his need, Her Majesty’s Government might make a monetary gesture by either giving him a supplement of his pension or by some suitable appointment where his scientific talents can still be used . . . having collaborated with Professor Ward myself and knowing his calibre in fundamental Particle Physics, I would believe that he did indeed reinvent the process which was subsequently used by the Aldermaston Laboratory in building the British nuclear deterrent. Professor Ward received no recognition for his work by the British Government’. Not surprisingly, the government refused.

Nor did he receive what he considered to be his due as a result of his work on QED and electroweak theory. Salam won the 1979 Nobel Prize with Sheldon Glashow and Steven Weinberg (ForMemRS 1981) for the prediction of neutral weak currents, not Ward, although Ward collaborated with Salam on their 1964 paper predicting neutral currents and also their original 1959 paper on the subject. Sakharov (1989) regarded him as one of the ‘titans of modern physics’ with Feynman, Schwinger, Tomonaga, Dyson and Wick for his work on QED, and Dyson told me: ‘Ward and I had an approximately equal share in the evolution of QED into its modern shape’. Despite this, Schweber, in his history of QED (Schweber 1994), devotes 100 pages to Dyson and a single page reference to Ward.

In his review of Monk’s biography of Oppenheimer, Freeman Dyson says that Oppenheimer never made any revolutionary discoveries in science, although he was capable of doing so; he was too interested in the mainstream and the fashionable (Dyson 2013). Ward was the opposite: he was interested in neither the mainstream nor what was fashionable. He spurned opportunities to hold positions at prestigious universities in the US and UK and settled down at Macquarie. He did, however, make three revolutionary advances in physics that, unusually, can be simply described by short equations. First we have his wave equation for two entangled photons,

\[ |1, 2\rangle = (|\alpha, \beta\rangle - |\beta, \alpha\rangle)(|k, -k\rangle - |-k, k\rangle), \]

[1]

describing the two-photon wave function for \( J = 0 \) where the photons are travelling in opposite directions, which leads to the first known derivation of the expression for the polarization correlation of the two photons in a quantum entanglement situation.

Second we have his Identity,

\[ Z_1 = Z_2, \]

[7]

which, with its generalizations, shows the deep connection between gauge invariance and renormalization in modern quantum field theory.
Third we have the Stefan–Boltzmann expression for the radiation pressure at temperature $T$ arising from the thermal equilibrium of radiation and matter

$$S = AT^4 \text{ W cm}^{-2}, \quad [12]$$

which Ward showed led to radiation implosion at high temperature and hence provided the conditions for thermonuclear fusion of deuterium and tritium nuclei.

**Prizes**

Ward received the Guthrie Medal of the Institute of Physics in 1981, the Dannie Heineman Prize of the American Physical Society in 1982 and the Hughes Medal of the Royal Society in 1983.

**Acknowledgements**

I should like to thank Frank Duarte, Freeman Dyson, Roger Elliott, Peter Knight, Chris Llewellyn-Smith, Kate Pyne, Martin Rees, Adam Roberts, Carey Sublette, John Taylor, Dmitri Vassiliev and two others who prefer to remain anonymous for their help. The portrait was taken by Walter Bird in ca 1965 and is © Godfrey Argent Studio.

**Supplementary material**

Copies of correspondence referred to in this memoir are available on request from the author. This includes the correspondence between Ward and Dalitz, and my correspondence relating to research for this memoir. An extended draft version of this memoir, including the complete Thatcher correspondence and my correspondence with Freeman Dyson, Kate Pyne and Ward himself, is available at https://arxiv.org/ftp/arxiv/papers/2007/2007.16199.pdf.

**Appendix**

On 13 May 1985 Ward wrote a letter to Prime Minister Margaret Thatcher saying that he wished to put on record ‘a personal account of events of some thirty years ago’. That personal account (below) was included in the letter. It is the document referred to by Hansen (1988).
1955

In the spring of 1955 advertisements were prominently displayed for theoretical physicists to join the staff at Aldermaston at, by U.K. standards, quite attractive salaries.

Wishing to return to the U.K., and with a marriage in prospect, after further enquiries and negotiations, I was offered a position but decided to refuse, intending to take up either a possible position in Cambridge, or to return to the U.S.

When I telephoned William Cook to tell him of this decision, he was so upset that I said I would come if the matter was sufficiently urgent. He said it was indeed most urgent. Going to Aldermaston under these conditions clearly involved an extreme professional and personal risk and indeed as it turned out sacrifice. (The marriage did not take place.)

I now know why Cook was so anxious for me to come. He had seen a letter Kramers had written to Simon urging my return to Oxford in most determined language. Cherwell had used this letter improperly for his own purposes.

To my amazement when I reached Aldermaston, I was assigned the improbable job of uncovering the secret of the ULAM-TELLER invention, an idea of genius far beyond the talents of the personnel at Aldermaston, a fact well-known to both Cook and Penney.

Under great stress, with no assistance whatsoever, I came up with the correct scheme within six months, minor modifications excepted. When presented at a subsequent meeting, a crucial one judging by the full-dress uniform of the visiting Admiral, my proposal, the only one offered, was peremptorily rejected by Penney, who declared the matter not to be urgent anyway! I was supported barely pro-forma, if at all, by Cook. Afterwards Penney demonstrated his complete lack of understanding of the problem in a private talk with Cook and myself. I was not invited to subsequent meetings held to discuss the project.

I therefore quite correctly and naturally resigned forthwith, and returned to the U.S. taking the first job I could get. My personal and profession survival of this trauma was something of a miracle.

1985

Last year I retired at 60, believing it not too late to contribute my remaining talents to the recalcitrant problems of modern physics and expecting to be able to travel more widely than possible otherwise. I now find that I am afflicted with chronic high blood pressure. The prognosis is at the moment uncertain. Clearly this is a result of past stress, and I see no reason to exclude from this my Aldermaston experience,

SYDNEY, MAY 11, 1985.
REFERENCES TO OTHER AUTHORS

Arnold, L. M. 2001 *Britain and the H-bomb*. Basingstoke, UK: Palgrave Macmillan UK.

Atzeni, S. and Meyer-ter-Vehn, J. 2004 *The physics of inertial fusion: beam plasma interaction, hydrodynamics, hot dense matter*. Oxford, UK: Oxford University Press.

Bleuler, E. & Bradt, H. L. 1948 Correlation between the states of polarization of the two quanta of annihilation radiation. *Phys. Rev.* 73, 1398. (doi:10.1103/PhysRev.73.1398)

Broad, W. J. Who built the H-bomb? Debate revives. *New York Times*, 24 April 2001. Section F, p 1.

de Wet, J. & Peierls, R. E. 1949 *Report on J. C. Ward to the Board of the Faculty of Physical Sciences*. Oxford, UK: University of Oxford.

Dirac, P. M. 1930 On the annihilation of electrons and protons. *Camb. Phil. Soc. Proc.* 30, 361–375. (doi:10.1017/S0305004100016091)

Dombey, N. 1992 *A bomba de hidrogenio 40 anos depois* [40 years of hydrogen bombs]. *Publico* 4 April 1992. [In Portuguese.]

Dombey, N. & Grove, E. 1992 Britain’s thermonuclear bluff. *Lond. Rev. Books* 14 (20), 8–9.

Duarte, F. J. 2009 The man behind an identity in quantum electrodynamics. *Austral. Phys.* 46, 171–175.

Duarte, F. J. 2019 *Fundamentals of quantum entanglement*. London, UK: Institute of Physics.

Dyson, F. J 1949 The S matrix in quantum electrodynamics. *Phys. Rev.* 75, 1736–1755. (doi:10.1103/PhysRev.75.1736)

Dyson, F. J. 2007 *Edward Teller 1908–2003: a biographical memoir*. Washington, DC: National Academy of Sciences.

Dyson, F. 2013 *Oppenheimer: the shape of genius*. New York Rev. 15 August 2013.

Einstein, A., Podolsky, B. & Rosen, N. 1935 Can quantum-mechanical description of physical reality be considered complete? *Phys. Rev.* 47, 777–780. (doi:10.1103/PhysRev.47.777)

Elliott, R. J. & Sanders, J. H. 2005 Maurice Henry Lecorney Pryce. *Biogr. Mems Fell. R. Soc.* 51, 355–366. (doi:10.1098/rsbm.2005.0023)

Foley, H. M. & Kusch, P. 1948 On the intrinsic moment of the electron. *Phys. Rev.* 73, 412. (doi:10.1103/PhysRev.73.412)

Goncharov, G. A. & Maksimenko, P. P. 2008 *Vodorodnaya bomba 1945–56* [Hydrogen bomb 1945–56], book 1, vol. III of documents and materials. Sarov, Russia: Atomic Project USSR.

Glashow, S. 1961 Partial-symmetries of weak interactions. *Nucl. Phys.* 22, 579. (doi:10.1016/0029-5582(61)90469-2)

Hanna, R. C. 1948 Polarization of annihilation radiation. *Nature* 162, 332. (doi:10.1038/162332a0)

Hansen, C. 1988 *US nuclear weapons: the secret history*. New York, NY: Orion Books.

Hawkins, D. 1983 *Project Y: the Los Alamos story* (History of modern physics, 1800–1950). College Park, MD: American Institute of Physics.

Heitler, W. 1944 *The quantum theory of radiation*. Oxford, UK: Oxford University Press.

Lamb, W. E. & Retherford, R. C. 1947 Fine structure of the hydrogen atom by a microwave method. *Phys. Rev.* 72, 241–244. (doi:10.1103/PhysRev.72.241)

Moon, P. B. 1945 Summary of notes on lectures by E. Fermi, G. P. Thomson CSAC75/5/80. Trinity College Library, Cambridge, UK.

Ne’eman, Y. 1961 Derivation of strong interactions from a gauge invariance. *Nucl. Phys.* 26, 222–229. (doi:10.1016/0029-5582(61)90134-1)

Penney, W. G. & Macklen, V. H. B. 1988 William Richard Joseph Cook. *Biogr. Mems Fell. R. Soc.* 34, 43–61. (doi:10.1098/rsbm.1988.0003)

Pyne, K. 2016 ‘Turning on the light’: the history of thermonuclear warhead principles in the United Kingdom. PhD thesis, Kings College London.

Rhodes, R. 1995 *Dark sun: the making of the hydrogen bomb*. New York, NY: Simon & Schuster.

Roberts, K. 1955 *An elementary theory of detonations* (TPN 123/55, December). Aldermaston, UK: AWRE.

Sakharov, A. D. 1989 *Vospominyeniya* [Memories]. [In Russian.] See https://protivpytok.org/dissidenty-sssr/saxarov-a-d/saxarov-a-d-vospominaniya.

Sakurai, J. J. 1960 Theory of strong interactions. *Ann. Phys.* 11, 1–48. (doi:10.1016/0003-4916(60)90126-3)

Salam, A. 1952 Developments in quantum theory of fields. PhD thesis, University of Cambridge.

Salam, A. 1985 *Letter to Mrs M Thatcher*, August 3 1985. ICTP Trieste.
Schweber, S. S. 1994 *QED and the men who made it*. Princeton, NJ: Princeton University Press.

Schwinger, J. 1948 Quantum electrodynamics I: a covariant formalism. *Phys. Rev.* 74, 1439–1457. (doi:10.1103/PhysRev.74.1439)

Schwinger, J. 1957 A theory of the fundamental interactions. *Ann. Phys.* 2, 407. (doi:10.1006/0003-4916(57)90015-5)

Shaw, R. 1954 The problem of particle types and other contributions to the theory of elementary particles. PhD thesis, University of Cambridge.

Slavnov, A. A. 1972 Ward Identities and gauge theories. *Teor. Mat. Fiz.* 10, 153.

Snyder, H. S., Pasternack, S. & Hornbostel, J. 1948 Angular correlation of scattered annihilation radiation. *Phys. Rev.* 73, 440–448. (doi:10.1103/PhysRev.73.440)

Takahashi, Y. 1957 On the generalised Ward Identity. *Nuovo Cim.* VI, 372.

Taylor, J. C. 1971 Ward Identities and charge renormalization of the Yang–Mills field. *Nucl. Phys B* 33, 436–444. (doi:10.1016/0550-3213(71)90297-5)

Teller, E. 1976 The hydrogen bomb. *Encyclopedia Americana*, vol. XIV, pp. XIV, 654–656. New York, NY: Grolier.

Ulam, S. M. & Teller, E. 1951 *On heterocatalytic detonations I: hydrodynamic lenses and radiation mirrors*, Report no. LAMS1225. Santa Fe, NM: Los Alamos Scientific Laboratory.

US Atomic Energy Commission. 1958 *Quarterly progress report to the Joint Committee on Atomic Energy. Part III: weapons*, July–September. Washington, DC: USAEC.

Wheeler, J. A. 1946 Polyelectrons. *Ann. N.Y. Acad. Sci.* 48, 219–238. (doi:10.1111/j.1749-6632.1946.tb31764.x)

Yang, C. N. & Mills, R. L. 1954 Conservation of isotopic spin and isotopic gauge invariance. *Phys. Rev.* 96, 191–195. (doi:10.1103/PhysRev.96.191)

---

**Bibliography**

The following publications are those referred to directly in the text. A full bibliography is available as electronic supplementary material at http://dx.doi.org/10.1098/rsbm.2020.0023.

1. 1944 (With A. M. Binnie) Stresses due to internal hydrostatic pressure in thin-walled vessels of stream-line form. *Aeronautical J.* 48, 538–543. (doi:10.1017/S0368393100120085)

2. 1947 (With M. H. L. Pryce) Angular correlation effects with annihilation radiation. *Nature* 160, 435. (doi:10.1038/160435a0)

3. 1949 Some properties of the elementary particles. DPhil thesis, University of Oxford.

4. 1950 An identity in quantum electrodynamics. *Phys. Rev.* 78, 182.

5. 1951 (With J. Wilks) The velocity of second sound in liquid helium near the absolute zero. *Lond. Edin. Dublin Phil. Mag. J. Sci.* 42, 314–316.

6. 1952 (With J. Wilks) III. Second sound and the thermo-mechanical effect at very low temperatures. *Lond. Edin. Dubin Phil. Mag. J. Sci.* 43, 48–50.

7. 1952 (With M. Kac) A combinatorial solution of the two-dimensional Ising model. *Phys. Rev.* 88, 1332–1337. (doi:10.1103/PhysRev.88.1332)

8. 1958 (With E. W. Montroll) Quantum statistics of interacting particles: general theory and some remarks on properties of an electron gas. *Phys. Fluids* 1, 55–72. (doi:10.1063/1.1724337)

9. 1959 (With A. Salam) Weak and electromagnetic interactions. *Nuovo Cim.* 11, 568–577. (doi:10.1007/BF02726525)

10. 1961 (With A. Salam) On a gauge theory of elementary interactions. *Nuovo Cim.* 19, 165–170. (doi:10.1007/BF02812723)

11. 1961 (With A. Salam) Vector field associated with the unitary theory of the Sakata model. *Nuovo Cim.* 20, 419–421. (doi:10.1007/BF02781760)

12. 1961 (With A. Salam) On the symplectic symmetry. *Nuovo Cim.* 20, 1228–1230. (doi:10.1007/BF02732536)

13. 1964 (With A. Salam) Electromagnetic and weak interactions. *Phys. Lett.* 13, 168–171. (doi:10.1016/0031-9163(64)90711-5)

14. 2004 Memoirs of a theoretical physicist. *Optics J.* See https://www.opticsjournal.com/JCWard.pdf.