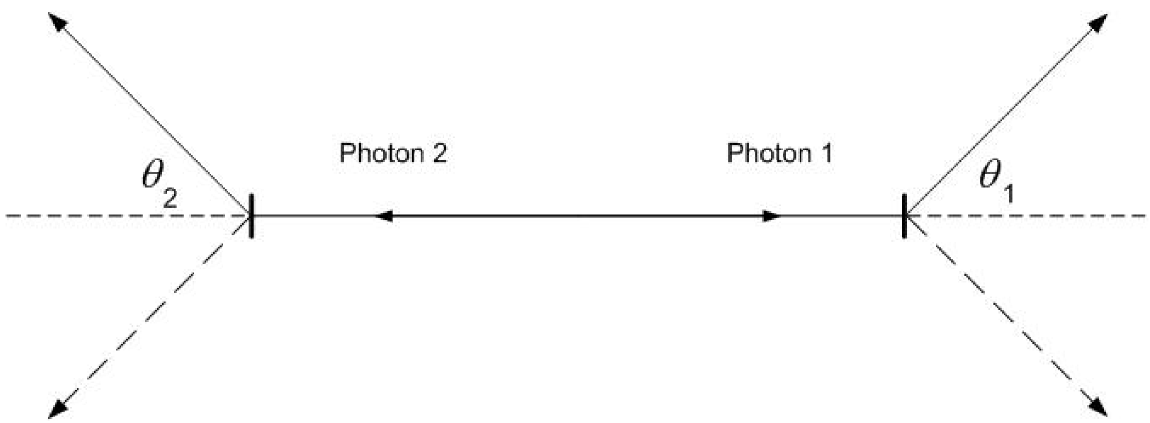
Memoirs of a Theoretical Physicist

J. C. Ward



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Foreword

These are the ***incomplete*** memoirs of John Clive Ward. He was still working on these the first days of January, 2000, which was the last time I spoke to him personally. This was a long and memorable conversation which extended over a few days, with the help of noble red wines, in the hospitable dry summer of Santiago de Chile. It should be emphasized that these writings were left by John in draft form. *Italics*, references, dates (in parentheses) and footnotes have been added in the editorial process. Particular effort was given to maintain John's writing style. For corrections, suggestions, and helpful comments during the editorial process, I am grateful to Dick Dalitz, Bob James, Susan Parulski, Kathy Vaeth, and Paul Wojciechowski.

These memoirs are being published with the authorization and encouragement of his sister Mary Patricia Pavezka, nee Ward. Their publication fulfill a promise to a dear friend and teacher.

F. J. Duarte
Rochester, New York
July, 2004

About the cover: this is a simplified version of the schematics used by J. C. Ward to calculate the polarization correlation of two emitted quanta. This work was part of his doctoral thesis entitled *Some Properties of the Elementary Particles*, at the University of Oxford, submitted in 1949. The results of these calculations were published in M. H. L. Pryce and J. C. Ward, "Angular correlation effects with annhilation radiation," *Nature* **160**, 435 (1947).

Preface

A. J. P. Taylor says somewhere that Bismarck's memoirs should be read as literature not as history. The wise reader will apply this maxim here also. Our selective memories kindly shield us from most of the egregious errors and humiliations of our past.

J. C. Ward

1. Early Life

I was born in East Ham, London, son of Joseph William Ward, minor civil servant employed in the Internal Revenue Department of H. M. Treasury, and Winifred Ward, nee Palmer, housewife and schoolteacher.[1]

My first schools, of which I remember very little, were Chalkwell Elementary and Westcliff High Schools, in the county of Essex, suitably close to my father's place of work in London. Sometime in 1938 my father entered me in a competition for a scholarship at Bishop Stortford College, a minor school in Hertfordshire. Perhaps he saw and advertisement in the local newspaper. I was duly sent off to take the entrance examinations which included a paper on Latin grammar and translations. Despite my complete ignorance of Latin and perhaps to the surprise of my father, I was offered an award. Possibly the school had to fill in a quota to satisfy government regulations concerning state-assisted public (read private) schools.

At the beginning of the next school year, I was therefore sent off to endure the separation from home and family the English middle and upper classes, to which I was now an unwilling recruit, enforced, by strange tribal custom, up on their young.[2]

2. Bishop's Stortford College

The college was one of the many founded in the Victorian times to provide for the needs of the newly established Empire for loyal patriotic servants of Her Majesty, in particular to create willing recruits for the Indian Civil Service. This was explicitly stated in the handbook about the school duly received by my father in the mail.

As I later discovered, these needs were mainly concentrated in the more physical demands of such service, little serious attention being paid to the intellectual side of the process. Cold morning baths, plenty of physical exercise, church service once a day, three times on Sunday. Oddly enough there was a special emphasis on swimming, including water polo, a strange choice for future servants of the Raj. Barely knowing how to swim, I was both literally and figuratively, out of my depth.

My first problem in the school as such was a complete ignorance of Latin and German. I sat at the back of the class with a poor German Jewish refugee, whose perfect command of both languages was not sufficient compensation for his complete lack of English. The Latin master, who doubled as English instructor, was an unpleasant bully and snob. He had the foul habit of requiring all students to learn by heart and recite at his pleasure… large excerpts of Victorian poetry, meanwhile criticizing one's accent, if not suitably genteel for his purposes… The German Master, who also taught French, was a fine fellow, common in the better schools of the U. K., an intellectual teacher for a living, whose real ambition was to obtain a mention in the published results of the New Statesman, which he managed to do quite often, much to the delight of some of the class. The other subjects that I must have studied to satisfy the examiners in the Oxford and Cambridge School Certificate that year leave hardly any trace in my mind. Presumably these included some history, some mathematics, and some science. I vaguely recall scraps of Chaucer and Shakespeare.

At home for the summer holidays, I sat by the radio listening to the news from Poland. I distinctly remember the lugubrious voice of Chamberlain announcing to the world that Britain was now at war with Hitler and Germany. My father was evacuated to new offices in North Wales, and I returned to enter the new world of the Sixth Form, "a heaven for enlightened scholarship and an opportunity to develop my talents to a maximum," to quote the above mentioned handbook.

Since there was no evidence for any talent in the humanities, I found myself in the Science Sixth, a new fangled adventure in the school, which apparently the headmaster was proud of. Why this was so was difficult to understand since the two science masters in charge seemed to know very little of science. Fortunately the school had a remarkably well-stocked library and I began a self-education program in the time I had to myself, which was not much. The whole idea was to keep the boys busy at all times and out of danger of developing bad habits, including that of thinking by oneself. Being already a confirmed outsider, this habit became a lifelong resource.

In 1940 came the German breakthrough in the Ardennes, described to us with clear enthusiasm by the Latin-English bastard, who now added fascism to his already unpleasant personality. In 1941, I had my first shot at the Higher School Certificate, it being explained to me that this was a kind of trial run for a more serious effort in 1942. Apparently I had already been chosen to be an example, for the school, of a possible worthy scholar capable of academic distinction. This included that most desirable of academic achievement, an open scholarship to Oxford or Cambridge. Of the results of this trial run I have no memory at all, but indeed in the next and last year at Stortford I did in fact receive special instruction from the kindly old mathematics teacher, who tried to explain to me some of the mysteries of projective geometry. My first attempt at Cambridge merely received the honorable mention of ***exhibition standard*** but at Oxford my second effort was rewarded with the offer of a ***Postmastership*** at Merton College, the equivalent of an open scholarship. The grateful headmaster awarded the school half day holiday, and I presume, never having checked, that my name is even now still prominent in the list of famous scholars produced by the school, eternalized in gold lettering in the school chapel. There remained the Higher School examinations, where I excelled myself, receiving distinctions in Mathematics, Physics, Chemistry, and *even Latin*, to the surprise and disgust of the Latin master, who had never concealed his contempt for my plebeian origins.

3. Oxford: Wartime and Undergraduate Years

Upon my arrival at Merton, I was interviewed by the senior tutor in mathematics and enrolled in Maths Mods a catchall first year program for would-be physicists or engineers. The entering prospective chemists were somehow exempt from this futile waste of time. The sixth form of British Public Schools, at least for entering science candidates, in those days was equivalent to a full first year at university. This was the result of the intense competition for entrance scholarships to Oxbridge. Consequently, after a quick look at the examination paper, which look very similar to the papers I had just negotiated, I completed my first year at university in a state of absolute idleness. Once a week I would attend a private tutorial with a Mr. Newboult, who seemed to have some knowledge of some esoteric subjects in analysis and linear algebra, but very little was transferred to me. I was introduced to Hardy's Analysis as if to holy book instead of to the book of pedantry it really is. Nevertheless I knew I could scramble through the coming examinations which I duly did, with First Class Honours, and little credit was deserved or felt. I looked forward to my second year when my real studies would begin, or so I thought.

The department of Engineering Science at Oxford was located in a redbrick structure at the end of Parks Road, and had barely received any recognition from the university at all. It was lower in the Oxford pecking order than the other sciences. In two years one was expected to master at least the elements of structural, mechanical, and electrical engineering, an impossible task.

I remember countless hours spent in a drawing office pretending to estimate stresses in simple bridge structures and in an electrical laboratory filled with heavy electric motors and dynamos, and also lectures on applied mechanics that effectively duplicated matters that had already bored me stiff the year before. One thing stays in the memory: A. M. Binnie, the lecturer in mechanical structures, had just written a paper on the stresses due to gravity in symmetrical thin shells. For this purpose he had used some complicated formulae from a book by a Professor Timoshenko, and discovered to his surprise that for a particular shape defined by some particular curve he could actually solve these equations. It seemed that I had already earned some reputation for mathematical manipulation, and was recruited to check his calculations. It did not take me long to realize that the only need for Timoshenkos's formulae was to establish the azimuthal dependence of the stresses, the remaining problem being a simple exercise in trivial mechanics. I had achieved my first piece of research, duly published in the journal of the Royal Aeronautics Society, a joint paper with Mr. Binnie.

To my surprise at the end of my second year I was not called up for national service. I had been informed two years earlier that the most I could expect out of Oxford was a two year stay in residence, before being expected to contribute in some appropriate way to the war effort. Since most of my school friends were long since in the armed forces, I could hardly complain. For some reason it had been decided that my whole class would now be allowed the extra year needed to graduate. The landings in Normandy and the following collapse of the German forces in France had led to a general expectation that the war would be over in a few months. Whatever the reason I was able to stay on and to conclude my studies with the regulation First-Class B.A. in Engineering Science. Without I hope sounding arrogant, I felt that I had learnt very little so far and knew that my education was very deficient indeed.

My fate was anyway now in the hands of higher authorities. The war was now indeed over and the decision was made by C. P. Snow, controller of technical manpower, that I was to be allowed to stay on to do research in the Engineering School, the last thing I wanted to do. Perhaps at Cambridge, but I could hardly put this to Snow in the presence of Professor A. Thom, newly appointed, from whom the request of my services had clearly come. For some reason I was asked to start research on air bearings but no supervision was offered, and of course my knowledge of the subject was zero. As a way-out guess fifty years later I surmise this subject had been lifted from a list of possible topics of interest to the government. The plans for an enrichment plant using gas bearings were in the works possibly. In any event, I went through the motions of starting an experimental program, but had already decided I would attempt mathematical finals at the end of the next academic year. I had studied the exam papers and they did not look too difficult. Of course mathematics at Oxford at the time was in a parlous state. There were two real mathematicians, J. H. C. Whitehead and E. C. Titchmarsh, both professors, and the syllabus such as it was could be easily mastered at least superficially by an accomplished examinee like myself. A few years later this would be an entirely different matter. J. H. C. Whitehead managed somehow to reform the syllabus, much to the disgust of college tutors, who were now expected to have acquaintance with subjects such as group theory and topology. In the meantime I listened to the unintelligible lectures of Whitehead with fascination, and became friends with G. C. G. Dalton, a New Zealander, who was completing a D.Phil. in the Engineering School, and with his wife Catherine, of whom there will be more to tell in the future. In due course I sat the required exams and received my *First Class Honours*. J. H. C. Whitehead was kind enough to say that I had thoroughly floored the examiners, and Alexander Thom, a dignified and gentlemanly Scot said that he was glad that he had at least backed a winner.

What to do now? The Ministry of National Service continued to show no interest in me, and I saw no reason to awaken them from their slumbers. Engineering and mathematics had lost all possible charm for me, at least in Oxford. There remained Clarendon Laboratory, which has achieved some reputation in low temperature physics. Experimental work was out of the questions, not being a physics graduate, but theoretical work was a possibility, given some financial support. Fortunately, Merton College was the beneficiary of the Harmsworth Trust, which made funds available to suitable graduates from Merton for further study. "Suitable" was defined as a good academic record and of unblemished Protestant and Anglo-Saxon, presumably including early

Norman, descent. My father with the help of several church registers, was easily able to satisfy the college authorities in this respect, DNA-wise that is. Maurice Pryce had just been appointed to new established chair of theoretical physics, and I applied for a position of his first graduate student, knowing very little of the subject. As usual, I proposed to learn on the job so to speak. Such is the arrogance of youth.

4. Oxford 1946-1949

The conditions of life for a penurious outsider at Oxford in 1946 were no good. The American troops who had flooded the streets and pubs in 1942-1944 had now been replaced by demobilized warriors of the British Empire, whose education, unlike mine, had been so tragically interrupted by the war. Lodging and food were in short supply. Even potatoes, freely available during the war, were rationed in 1946. Ten men to one female meant that girlfriends were rare to the point of extinction. Those too proud to participate in the inevitable mad competition for even a few hours of female company were relegated to the dustbin of seedy hours in the local overcrowded pubs drinking watered so-called beer, commiserating with their equally frustrated acquaintances, previous heroic paratroopers, bomber pilots, and the like. Since I had endured these conditions also for all my years in Oxford, I could only too easily empathize with my newly found friends, many of whom were Rhodes Scholars or otherwise dislocated colonials far from home. I shared their contempt for the absurdities of class distinctions endemic to British and particularly Oxford life at the time.

My first introduction to theoretical physics, on the other hand, was a great success. Maurice Pryce had learnt from his friends in Cambridge that an attempt was to be made to verify an old prediction of Dirac that the two equal and opposite gamma rays emitted up on the annihilation of positrons in matter were polarized mutually perpendicularly. Unknown, at least to me at the time, was the fact that J. A. Wheeler of Princeton had already done some calculations on the subject, and even received a medal from the New York Academy of Sciences for his work. I proposed to duplicate Wheeler's efforts. When presented to Pryce, my work was smartly rejected. "You cannot apply Aristotelian logic here," said Maurice. With remarkable insight, and extraordinary perspicacity, he presented me with the statement: "the state of the photons is described by $(|X\rangle|Y\rangle - |Y\rangle|X\rangle)$." *This was my first lesson in quantum mechanics, and in a very real sense my last, since all the rest is mere technique, which can be learnt from books. The inner mysteries of quantum mechanics require a willingness to extend one's mental processes into a strange world of phantom possibilities, endlessly branching into more and more abstruse chains of coupled logical networks, endlessly extending themselves forward and even backwards in time.*

I went away and recalculated, using my newly acquired understanding, and resubmitted my results. After the correction of a slight error, again due to the eagle eye of Maurice, it was possible to write up a short account for publication. To my surprise, when I said that both names should appear as authors, Maurice Pryce initially refused. "You told me how to do it," I said, which was true. A short note[3] giving our results, but no derivation, duly appeared in Nature, sometime in early 1947. There will be a lot more to be said about this episode when I discuss the strange matter of my D. Phil. thesis. Apart from this work I was left pretty much alone by Maurice Pryce who was finding out by himself how difficult is the life of someone who is presumed capable of supervising students for advanced degrees in theoretical physics, where ideas are essential and extremely rare. Indeed the whole notion of directed research in the subject is absurd. Maurice suggested I look at some paper he had written prewar on the definition of photon position. I decided I could make nothing of it whatsoever.

I had now spent nearly five years at Oxford without hope of security and indeed seemed to have reached a dead end. In desperation I replied to an advertisement for a lectureship in mathematics at the University of Sydney, and was somewhat surprised to receive, by return mail, an offer of an appointment. Here, it seemed to me was my chance. I could escape from my present impossible circumstances, I could eat a decent meal again, and I might even develop some social life away from the Oxford life that both repelled me and fascinated me. Being still young, adventurous, and naive, it seemed to me that another piece of paper, another label to my name was not worth the candle.

My journey started in Southampton, where a suspicious custom officer looked with surly eyes through a book donated to me by a loving maiden aunt[4] as a going away present. Perhaps he thought that I was on the run from the law, which in a sense I was, not having completed my national service. Apparently there was no law at the time to prevent my leaving, and I was disdainfully allowed to board the ship. The first port of call was Las Palmas in the Canary Islands where all the passengers quickly disembarked and made haste to the best restaurant in town. There we feasted on a magnificent menu, finishing up with crepes Suzette and Spanish brandy. There were about seven of us and as many waiters. I still remember that meal with affection. The restaurant is now long gone, replaced by a horrible tourist trap of a hotel for packaged vacations. From there we went round the Cape and then directly to Freemantle and Sydney. Again my first act up on arrival at Sydney Cove was to indulge in a huge steak with two eggs on top, the standard menu for a Sydney resident those days. Somehow I found my way to the University of Sydney and to St. Andrews College, from which had come the offer of employment. It did not take me long to discover that I was not really a lecturer at the university at all, but a tutor in the College, with some tenuous connection to the university at best. There was nothing to do but to wait out the year before returning to the U. K. In the meantime I spent many hours on the splendid Sydney beaches, gave a course of lectures in the mathematics department and made several fine friends, including Freddie Chong, later Professor of Mathematics at Macquarie, and Dick Makinson.[5] I was able with pride many years later, to rescue Dick Makinson, from the clutches of a certain professor, and bring him to Macquarie where he became an Associate Professor of Physics.

5. Back in Oxford

I can remember very little of the trip back to Oxford. When I had informed St. Andrews of my intended resignation, they demanded that I refund the cost of the fare out to Sydney. I agreed to do so in installments as best I could, but of course I had first to find the funds for my return, and for the subsequent year at Oxford. The Harmsworth scholarship was no longer available. How I got by I cannot now recall. I suppose I had saved some small amount. I remember tutoring rather ineffectually some engineering students. I do remember that I avoided calling for help from my longsuffering parents, and that I successfully paid off the debt collectors from St. Andrews. The last letter went something like this: "Dear Mr. Ward, thank you for the last installment. Please note that we are still owed Three Pounds, Seven Shillings, and Twopence.

My D.Phil. thesis had still to be written and submitted. I expected no particular difficulty here. After the appearance of my note in Nature with Maurice Pryce, a much longer article[6] with a derivation of our result had appeared in the Physical Review, including a factor of two error, by Snyder, Pasternack, and Hornbostel, all highly reputable U. S. physicists. Experiments were underway at Columbia by Madame C. S. Wu to check our answer experimentally. Since Wheeler had omitted the important correlation terms in his result, our prediction was of a much larger effect, soon to be indeed observed.[7] Similar work was also started in Cambridge where R. H. Dalitz also made quite a name for himself by deriving independently our result.[8] I naturally thought therefore that my thesis would be accepted without difficulty. This would certainly have been the case if the external examiner had been N. Kemmer from Cambridge, as had initially been intended. Nick Kemmer was fully informed about this work, having been the leader of a seminar in Cambridge where the derivation of our result had been unsuccessfully attempted, until Dick Dalitz had managed to do so.[9] For some no doubt trivial reason, Kemmer was unable to make the trip to Oxford, and his place was taken by R. E. Peierls, who declared the thesis unworthy of acceptance. Outside the examination chamber, he privately suggested that the standard consolation prize of a B.Sc. topped up with an entry into his own empire in Birmingham, an offer which perhaps I could hardly be expected to refuse. But refuse I did. Fortunately, the internal examiner J. de Witt put on a good show of determination that the degree be awarded. R. E. Peierls retired hurt from the contest. Nevertheless, any hope that I may have had of staying on in Oxford was, at least for the moment, now out of sight.

Facing up to reality, I applied for a job at Rolls Royce in Rugby, and was offered a position as trainee engineer, starting from the ground up. Perhaps I should have accepted, since it was now quite evident to me that theoretical physics, as a profession, was an impossible ambition. The fates had already decided that I stay in Oxford. Maurice Pryce had arranged for me to receive a senior D. S. I. R. award for two years. I was still wondering why the Ministry of National Service had apparently given up on me. The temptation to rescue my academic career was just too much. I decided to stay on.

Normally this would have been a suicidal decision, but my astounding luck, which was to come to my aid many times in the future, asserted itself. I could not have chosen a better time to dive into the deep waters of the exciting new theory of quantum electrodynamics. Willis Lamb had just announced his brilliant experiment on the hydrogen atom fine structure, Hans Bethe had just reasonably accurately calculated the required radiative corrections to the standard theory, and above all Dick Feynman had unveiled his new relativistic formalism. F. J. Dyson then demonstrated the relation of this to more familiar treatments. More important from my point of view was a subsequent paper, which claimed to show that all divergences in the theory were contained in the renormalized mass and charge of the electron, an astounding result. In particular Dyson had conjectured that the infinities associated with certain graphs would cancel.[10] It so happened that I had already developed a technique capable of proving this, and was delighted to be able to publish a short letter[11] in the Physical Review, containing what is now called ***Ward's Identity***.[12] I had managed this by a careful study of Feynman's treatment of what would be now called the gauge invariance problem, a problem that was not properly understood, at least in the general case, until the early seventies. There remained the complicated matter of overlapping, so-called ***b*** divergences. Dyson said "the reader will verify for himself" at this point in the text, which was a clear challenge for anyone reading his paper. How many readers attempted this I have no idea, but I do know that I succeeded only after immense effort. While struggling with this apparently insuperable problem, from which my academic life was now hanging by a thread, I made the terrible error of falling permanently, and quite inappropriately, in love. Probably this was inevitable after seven years in Oxford. I might have escaped this disaster if I had been more realistic. My excuse is that realism at this moment in my life would have been suicidal. I knew only too well the risks I had taken by accepting the temporary situation in Oxford, which was quite properly described as "Kafkesque." There were no jobs available. Many distinguished refugees had lived for decades on temporary grants. No one at Clarendon with the exception of Maurice Pryce knew anything or cared anything about my work. Lindemann…was away most of the time serving as Churchill's scientific advisor. He would come down to Oxford on rare occasions… on one of these rare visits he asked me: "This fellow Pryce we've just hired, is he any good?"… I had lost the only love of my life and was in great danger of being ejected from Oxford at the end of my D. S. I. R. grant .

I sat down to explain to Dyson what I thought I now understood about his theorem. My only hope was now that I could escape to a better part of the twentieth century. Another odd episode occurred shortly afterwards. Experiments at Cambridge at the Mond Laboratory had just shown that the velocity of second sound in liquid helium near zero approached the value predicted by Landau, namely $c/\sqrt{3}$. None of the experts, mainly

Dutchmen, seemed to understand how this result was obtained. It so happened that I had been reading in the Physical Review a paper by Teller and de Hoffmann about relativistic shocks, where they showed that the same result was true, substituting the velocity of light for that of sound of course. I published a little note to this effect.[13] To my surprise, when I attended a meeting in Holland it became clear that no one had understood. I made a speech on my own behalf amidst a deafening silence. There was one bright spot in the generalized darkness: ***H. A. Kramers was staring at me as if at a ghost***. We corresponded later to great effect. I had made my first contact with a real physicist. No, I should say the second. Maurice Pryce was the first. From now, things began to look up. I was invited to give a talk in Cambridge about renormalization, and made my first acquaintance with Abdus Salam, who was working on the same problem. I slept in a four- poster bed in Trinity College, said to have been slept in by ***Elizabeth I***, which I considered a great honor, despite the mattress, that seemed to date from the same period. I also found good friends in Nick Kemmer and P. T. Matthews, a useful asset in battles to come. Nevertheless my situation at Oxford remained as insecure as ever. Maurice Pryce had taken off for Princeton as visiting professor, a well deserved reward for his work with me on quantum mechanics. P. T. Matthews and Abdus Salam had left for the Institute of Advanced Studies, also in Princeton, as visiting members for the academic year 1950-1951. My D. S. I. R. grant was coming towards its end, and I had no idea what to try next. This requirement for continuous achievement had become a nightmare. There was nothing else to do except to write to the Institute in Princeton, and to hope, with the aid of my newly acquired friends, to gain a few more years respite. Of course writing at the end of the nineties, it is incumbent on me to remark that this style of life has become commonplace in areas of the so-called Post-Doc generation. I had now to wait until I heard from the Institute, one way or another. At long last, after some prompting from Maurice, the precious letter arrived, offering a membership of the Institute for the year 1951-1952 at the princely salary of $3000 per year. I felt that I had finally entered into the sacred temple.

My arrival at Princeton is engraved in my memory…here had trod the feet of Einstein, Weyl, and others. A room was found for me in a boarding house downtown, and an office assigned at the Institute. For the rest I was on my own, normal procedure no doubt. After some discussions with others, it appeared that far from being expected to sit at the feet of the more learned, I was regarded as a visiting expert. Oppenheimer inquired as to whether I had a position to return to Oxford, but made no comment when I said "no, I have not." I now understood the comments that Dyson had made to me earlier. He compared the Institute to the sanitarium in Thomas Mann's ***Magic Mountain***, where the inhabitants await the unknown fate. This was true for all except the permanent members who were few, or those with permanent jobs to return to, professors with tenure from some other institution. The majority was in the same situation as myself. In the meantime we were expected to produce. Otherwise the future was not bright.

About this time I sent a report to England on my work on renormalization, as required by the terms of my D. S. I. R. grant. I received back a letter thanking me for the report, which was described as to be “well worthy of study by the experts in the field” and requesting the repayment of Thirty Pounds, Seven Schillings, and Threepence. It seemed that I had left England two weeks early. I replied that I was surely entitled to a two weeks holiday after having produced such a valuable document. To my eternal disgrace, I also included a check for the full amount demanded.

I lay in bed, listening to two elderly ladies counting the receipts from the Sunday collection, and contemplated my fate. When, if ever, would this constant need to produce rabbits out of a distinctly limited hat ever end? Princeton was far lonelier than Oxford, with no companions, no pubs, and no girls. Even Oxford had a few women that I could admire at a distance, and countless pubs to drown down my sorrows in. Not so Princeton, which like most U. S. cities made sure that the outsider had nowhere to sit down to relax and to make trouble for the worthy taxpayer citizens. Not even a decent coffee shop.

Back at the Institute I listened with incredulity to some outrageously silly seminars, received with apparent equanimity by Oppenheimer and others. The drill was, it seemed, to just wait. One’s turn would come. My disillusion was complete, and drastic measures were called for, as usual.

Rudi Kompfner, an old friend from Oxford, inventor of the traveling wave tube, had just perforce migrated to Bell Laboratories, in Summit, New Jersey, where he had discovered the backward wave oscillator. For some reason, he was visiting Princeton, and he suggested to me that I join his group there. I said that I would consider doing so if allowed to do experimental work.

Meanwhile, my invaluable luck had come to my rescue again. There was a seminar on the Ising model, a classical problem in statistical mechanics. I made the usual comment, at least for me, that a combinatorial solution to the two-dimensional case should be possible, given that Onsager’s algebraic and extremely opaque solution already existed. I hit up on the concept that a suitable determinant might be constructed that would do the required counting. It did not take long to indeed find such an expression, and I showed it to Mark Kac, who was visiting from Cornell. He greeted me with enthusiasm the next day having calculated the not particularly difficult final result. “It nearly works” he said to me. After the immediate correction of a few odd errors, it became quite clear that it did indeed give the right answer, but only because of a sophisticated theorem in the theory of two-dimensional graphs. Nevertheless, it was also clear that a solution to the three-dimensional problem along the same lines was quite hopeless. I gave a seminar on the method[14] a few weeks later, and felt the warm approach of Hendrick Kramers, now also a visiting member, descending up on me. He was one of the world’s experts in this rather abstract problem. By a strange twist of fate, I had already impressed him by my work on charge renormalization, *of which he was the inventor*, and of course my work on liquid helium.

I suppose that by now I should have seen that a university appointment was in order. But here a curious paradox arose. By this time the universities of any reputation, or those that so aspire had invented the graduate school. Physicists would be created by a production line of suitable professors, financed by the splendid generosity of government institutions. The same students could then also be used as cheap instructors for undergraduate courses, leaving the professors, with the help of his contract, to spend his time directing research, and publishing more papers. This resulted in an absurd inflation of theoretical physics in particular, aided and abetted by publishers of innumerable semi-fraudulent "science journals." I would be joining an ever-growing army of unfortunate ex-graduate students, now professors, and be competing for ideas as best I could. This was quite an impossible choice. Mark Kac suggested Vassar as a possibility, and he may well have been correct, but I opted instead for Bell Labs and Rudi Kompfner.

Returning to the U. K. to collect my U. S. visa, I visited Oxford, and lectured on the Ising model as now understood (1952). "Was not this an enormous risk?" said Maurice. "No more than anything else" I replied.

6. Bell Labs

My ambition was to qualify now as an experimentalist. But I had ignored one vital fact. Rudi, a fine fellow in many ways, had one incurable defect. A former architect from Vienna, he was filled with grand ideas, some of which were technically impossible, or at least very difficult to realize in practice at the time. He had been successful once with the traveling wave tube by ignoring the advice of so-called experts. This led on occasion to excessive optimism. He conceived the notion of detecting noise from an electron beam externally to the vacuum envelope, and I was entrusted with the task of building a suitable tube for the purpose. I now can judge that this was a hopeless enterprise, for the simple reason that the coupling to any outside movable detector has to be far more robust than is physically possible. At that time however I was neophyte in the electronic tube business, and spent many fruitless months detecting noise from every other source but the beam itself. I managed to break a few delicate tubes in the process. If I had been more assertive, I could have said to Rudi that this was nonsense. I might even branched out on my own. Molecular oscillators were on the horizon, for example.

Meanwhile, Summit[15] was for a lonely bachelor even worse than Princeton. The Institute, however inhospitable, did provide some kind of social life. In Summit there was none. It was just possible to commute to New York, as several hardy bachelors did. I discovered my mistake much too late.

One day I received a letter, much to my surprise, from Francis Simon. He inquired whether I was enjoying life at Bell Labs. Naturally, I responded by describing my sad situation, and added, not unreasonably, that I would indeed like to return to Oxford…To my horror, there now arrived another letter from Simon stating that he regretted that he did not actually have any position to offer at the moment. I could of course have an ICI fellowship, a five year appointment, with possible renewal. This would put me at a par with several other more distinguished physicists at Clarendon. All this hoping desperately that some Oxford college would offer a fellowship. But the colleges themselves decided these matters for their own peculiar reasons, sometimes with the advice of Linderman, now Lord Cherwell. I could not accept.

Why had Simon, whom I hardly knew, written to an ex-student long overseas, in such a friendly fashion? The reason I only discovered by accident many years later. It seems that Kramers had written to Simon a letter urging in the strongest possible terms my return to Oxford. This is highly possible given my affinity with Kramers. The uninformed reader will not know this, but ***Kramers was the uncrowned king of Dutch physics.*** Why he never got a Nobel Prize I cannot understand. A heavy smoker, he died of lung cancer shortly afterwards.

What was I to do? There remained an attractive possibility. H. S. Green had written to me from Adelaide, offering me a senior lectureship at his department. I knew very little about Green except that he had written a huge amount with Max Born in Edinburgh. I did know that Adelaide was the most pleasant of Australian cities. There was a sense of quiet dignity that had appealed to me tremendously. I was not the only person to think this way. The Adelaide Festival of the Arts is now indeed world famous. Even the auto racing circuit regards Adelaide as their favorite town.

7. To Adelaide and Back

To characterize H. S. Green is impossible. I have never met any one like him. It is not that he was without talent. He had plenty, in a bizarre fashion…Green was unique in his complete disinterest in the outside world. He had absolute confidence on anything that he came up with…My duty, it appeared, was to listen with enthusiasm to every idea and to applaud at regular intervals. Even the Institute did not require this particular service. I sincerely tried for what seemed like an eternity, more likely a few weeks, to act as a responsible audience, and then gave up. Later back at the Institute, Bram Pais said to me "I could have told you about Green." He then described how Wolfgang Pauli had once entered his office quivering… "For heavens sake"…said Pauli…"protect me from that green monster."

Accident prone is the technical description for someone who continuously drifts from one disaster to another. That my disasters were more original than most perhaps implies a more ingenious unconscious. A letter to Princeton resulted in an offer of another year's membership. My Shangri-La was not to be, at least not yet.

Back at Princeton, I had no ideas for my future. Until now, my life had been a crazy sequence of hasty improvisations. As I said to Bram at the time, I was just plain tired. I needed a wife and a family, things that other people had. I was dead tired of the constant need to prove myself over and over.

Pais introduced the notion of strangeness which called for an algebraic formulation. Bram and I exchanged ideas, but strangeness, charm, top, bottom, are still deep mysteries, forty-five years later. A new appointment had to be found. Oppenheimer was kind enough to strongly recommend me for a readership that Nick Kemmer had available at Edinburgh, where he had just been made professor, upon the retirement of Max Born. Nick, for whom I had nothing but the highest respect, warmly received this suggestion. But here was the usual rub. How could I accept such an appointment from a friend whom I respected, when I had no ideas whatsoever? "A paradox, a paradox, a most ingenious paradox." [16] I had responded in the same manner to Maurice Pryce. He was leaving Oxford for a post at Bristol, and suggested that I now apply to return to take his place. No doubt he had the support of Simon and possibly of Lord Cherwell. I replied that the only way I could consider such possibility was if the offer could be considered an "honor." It was evidently neither an offer nor an honor. I had also been offered a modest position at the University of Michigan, with George Uhlenbeck, whom I also held in great esteem, with therefore the same dilemma as with Nick Kemmer.

The British Atomic Weapons Research Establishment at Aldermaston was at the time advertising for theoretical physicists at, for Britain, reasonable salaries. I sent an inquiry out of curiosity, and received in return an invitation to an interview as soon as I was back in the U. K. At least this would be a challenge. My other motives I will discuss later when I debate the thorny question of the morality of nuclear weapons. Sufficient for now is the statement that I duly appeared at Aldersmaston, and was offered a position at half the amount advertised. When I bluntly said that this was unacceptable, William Cook quickly came up with the required offer. I said I would consider the matter, and was given a phone number to call when I had made up my mind. Indeed I decided to accept the offer from Michigan and wrote a letter of acceptance. Then I called Cook and told him of my decision. The conversation went something like this:

Ward: Do I understand from you that the matter is urgent?
Cook: Oh no, I would never presume to coerce you in this way.
Ward: Let me get this straight: you regard the matter as urgent, and my presence is very much desired?
Cook: Yes indeed.
Ward: I will think the matter over again and will let you know in due course.

It is always pleasant to be flattered. But I also had my own reasons for taking a second look: natural curiosity, a wish to return to England, and a need for a reasonably well paid job.

I now know the reasons for Cook's response. Churchill had instructed A.W.R.E. to build an H-bomb "to qualify for a seat at the table." Penney had said that Aldermaston had neither the expertise nor manpower for such as task. It was certainly true that the problems were daunting. In particular, the Ulam-Teller design was the most carefully guarded secret of all U. S. secrets. I called Cook and told him I would come after all. My excuses were sent to Uhlenbeck. There was very little I could say to excuse my behavior, without revealing the true reasons.

Many people would have said then, and many will say the same fifty years later, that only madmen would occupy themselves with such terrible matters. This is one view. There is the other view that says that only by making war too dangerous will we have peace, and this has always been my opinion. The truth lies somewhere between these two extremes. Small wars and not too much danger appears to be the rule in 1999. I also believed that, in 1955, the danger lay in a war in Europe, and that an independent deterrent was essential, the more powerful the better. It is nowadays a common place that weapons from small individual terrorist groups are the real danger.

8. Aldermaston

A few days after my arrival (about mid June, 1955), there was a formal meeting, chaired by Penney, of about twenty senior staff. He declared that I would be in charge of *Green Granite*, the code name for the development of a Ulam-Teller device. I was assigned a small office, with a secretary adjoining. That was all, no staff, no instruction, no advice, as to what I was expected to do next. This was most odd to put it mildly. A copy of the classical super design appeared mysteriously on my desk. For a few days I was left to meditate alone.

K. V. Roberts and I were summoned to Penney's office, where he proceeded to tell us what he knew about the makeup of the weapon. This was very little. He knew that there were two separate fissile assemblies, and that neutron shielding was somehow involved. This was effectively it. I was now on my own. Assistance from Roberts was implied but left unstated.

There followed three or four futile months, when I drew up and discarded endless schemes. There was a brilliant invention to be made. That was clear from the reports in the press and from the Oppenheimer papers. Even the simplest calculation was missing. One day I asked Keith Roberts where I could find a report on the α values of a Sakharov sandwich. Keith replied that there was no such information available. I commented that this was quite a simple calculation to perform, as indeed it is, for the infinite case. Keith went away and in the remarkable time of a few days came back with the data.

One day I had the idea of radiation implosion. As in all the ideas that have ever popped up in my head, there is no way I can trace the source. I think that one just goes through a mechanical process of trial and error. "I now know five thousand ways how not to produce a light bulb" was Edison's reply when asked what he had done lately. Perhaps I still remembered the results of Teller and de Hoffman concerning radiative shocks. I tried the idea on Keith, whose eyes glowed with enthusiasm. I drew up a sketch of a primary and showed it to Pike. We agreed that it would not be difficult to modify some existing arrangements. Pike went away, and as I was to discover quite soon afterwards, and used some already existing blueprints to incorporate the necessary changes.

As luck would have it, there was an important meeting held shortly afterwards. How important can be measured by the presence, among numerous senior staff, of a splendidly outfitted admiral, in full ceremonial dress, sitting to attention in true naval fashion. This had to be a committee established to review progress, if any. I cannot remember anything of the first hour or so of the meeting, but presumed it dragged on in an inconclusive way until Cook was forced to put the following question forward: "does anyone have any ideas on how it might be done?" After a few embarrassing moments of silence, I went to the blackboard and sketched out my proposals. I was surprised to see Pike leave the room and return with some rolls of blueprints which he unrolled on the long table. Everyone crowded around to see his first glimpse of a primary. I then drew what I discovered many years later was an accurate picture of the remaining parts, except for shielding and other minor precautions. Cook was quick on the uptake, and made very pertinent inquiries about shielding problems. I explained how important it was to move the energy fast to the other end, and emphasized the need for compression. There was then a great hush. Evidently, it was now Penney's turn. I spell his words verbatim: "this is too much like a piece of clockwork. If this were wartime, we might consider something along the lines of these waveguides of yours." Cook said, rather softly I remember: "this should be looked into." The meeting was then promptly concluded. Several weeks later Keith told me that there had been another meeting in my absence. I have often wondered what happened this time, but unfortunately Keith died of cancer before it occurred me to ask. He did tell me once in Cambridge that "we did a lot of work after you left"… he went to say that they were "bang on." This was after the successful test at Christmas Island in 1957-1958. This achievement, if true, had outdistanced all others, who, particularly the Americans, had vastly more computing power at their disposal.

There followed a month or two of absolute silence. I took off a few weeks to drive Robert Graves down to Barcelona. I described the situation to my old friend Cliff Dalton, Robert's son in law, and now advisor to Harwell on the fast reactor project. He suggested that I join him in Australia where he had just been appointed Chief Engineer of the Australian Atomic Energy Authority. I told him to proceed with the paper work. Fortunately for me, apparently the head of the establishment vetoed the idea.

There also appeared in my mail an offer of employment for an electronics company in California. Before leaving the States I had asked a friend of mine at Stanford to find me a job. I had not yet given up the idea of working once again in the electron tube business. My reentry permit visa to the U.S. was about to expire. I announced my immediate departure for California, and hoped, the good life. “Don’t stand in his way” were the instructions Cook gave to Corner. He was not pleased.[17]

9. Back in California and on to Maryland

I was back in the States, but not back in my feet. It turned out that I had been hired to replace an unfortunate engineer, who had just had a nervous breakdown, and quit the company. The reason is that he had been given a job of preparing for the production of the company’s new line of backward wave oscillators. Probably my year with Rudi had led them to believe that I knew some of the trade secrets. In fact, of course, I knew hardly anything about trade secrets, and there was nothing and nobody to assist me in this endeavor. My usual luck held. I negotiated a visiting professorship with Elliot Montroll at the University of Maryland (1956-1957). Obviously, however, my problems were not over. I had just exchanged one impossible job for another. I had no idea how I should spend my time. There were still many universities and colleges in the U. S. that went, cap in hand, to the meeting of the American Physical Society in the hope of finding faculty for their physics departments. And then there were industrial employers trying to fulfill their requirements under government contracts for their quota of “senior scientists,” namely any one over thirty with a Ph.D. My situation could hardly be called precarious, but I did not want to embarrass Elliot, who was probably paying my salary out of his own government contract. As it turned out I was an excellent investment.

One day I was invited to give a seminar in the physics department, which was undergoing an enormous expansion. This was much to the concern of the older faculty who saw, quite rightly, that the new emphasis on graduate students and paper writing, could only lead to no good. In addition one of the more unpleasant new recruits hoped to reveal my failure to produce more rabbits out of my distinctly diminished hat.

I had a few weeks to prepare for this imminent trial by combat. Brueckner and Gell-Mann had just published a paper on the ground state energy of an electron gas, and I was convinced that I could generalize this to the case of finite temperatures. I hoped to use a concept of periodicity in $b = (1/kT)$ space, which, as far as I know, I was the first person to notice. I got as far as being able to prepare a reasonable hand-waving lecture, which is all I had in mind for the moment.

Elliot was of course present when I made my presentation. I gleefully observed the astonishment in the eyes of the gentleman who had so kindly invited me. I later received a phone call from Elliot, who excitedly said to me that the high-temperature limit of the theory I had just sketched out had to be the classical Debye-Huckel theory of electrolytes, as treated in the, believe it or not, Mayer-Montroll ring diagram method. It was an extraordinary piece of luck for me that Elliot was one of the few people that knew about this classical work in statistical mechanics. The next few weeks were crucial to my personal morale. *We had managed not only to produce a definitive extension of a previously purely classical theory, but also to establish the rules for diagrammatic treatment of problems in quantum statistical mechanics, rules that are now the bread and butter of modern calculations.*[18] Mark Kac said to me shortly afterwards: "You have done it again." "Yes," I said and added, "it is very difficult to do anything new nowadays. I thought that I would never do anything again." I might have said "almost impossible." I can think of only two other examples , as far as pure technique is concerned: the work of Fadeev and Popov on the quantization of non-Abelian gauge theories and presumably the work of Veltman and t'Hooft on the calculations in electro-weak theories. My knowledge of the last is insufficient to make a real assessment. Now that a Nobel Prize is involved (1999) I might have to do some homework.

Quite soon after this triumph, the experiment of Mrs. C. S. Wu *et al.* at Columbia, acting upon the suggestion of Yang and Lee, definitely established the non-conservation of parity in weak interactions, surprising everyone. I wrote a note to Abdus, telling him of the result, adding that Einstein must be spinning in his grave, clockwise presumably. With my newly refreshed morale, I thought it time to start thinking again about particle physics. The first order of business was *b* decay, since new options were now open, even the old Fermi theory of vector interactions, now presumably V-A.[19] It happened that Marshak was visiting Maryland to lecture at the time, and I asked him whether this was possible. Marshak's response was remarkable. First he asked me why I had asked the question, and I responded in the manner that no doubt Fermi would have, that it would be nice to have something like the electrodynamic coupling. He then spelled out for me four results that would have to change before this could be the case, three of which were unpublished results soon to be discredited. Marshak proceeded to discuss the matter with several colleagues. As it turns out, subsequently Gell-Mann and Feynman wrote a paper on this coupling . At a meeting of the American Physical Society in New York, shortly afterwards, where Feynman gave one of his typically brilliant lectures on the subject, Marshak grabbed the microphone, and in tears said "I was first, I was first." Dick, as usual …strictly honest, said… "All I know is that I was last."

With the coming of V-A, it was clear to several people (including Sakharov[20] and Bludman), that the correct formulation of Fermi's intuition must include some kind of gauge theory, of which electrodynamics was the only known example. Oscar Klein in 1939 had invented general gauge theories as classical fields. He had the misfortune to publish in the Acta Physica Polonica late in that year. Frank Yang rediscovered them after the war, as did a student of Abdus Salam, a Dr. Shaw. When I remarked to Abdus in 1957 that there must be a generalization of the Abelian theory to the non-Abelian case, Abdus at once said that this was in fact the subject of a student's Ph.D. thesis.

It was remarkable how little most physicists knew of Lie groups, of their representations, and of the connections to gauge theory at the time. The rotation group was the only one that was standard information. "What's a Lie group?" was a question put to Abdus by a famous physicist in 1960. The intrinsic difficulties of the quantum theory aspect may have been the reason for this neglect. Even electrodynamics was essentially fudged, as far as gauge invariance was concerned. This I knew from my experience with ***Ward's Identity*** problems.

My attitude towards what would now be called high energy theory differed strongly from other practitioners. Many seemed to regard the subject as a kind of glorified Klondike gold rush, staking their claims as best they could, and keeping their cards close to their chests, to mix metaphors. I perversely refused to play the game. Instead, I would openly discuss the problems with anyone who was interested, and in particular of course with Abdus. He and I were old friends, despite the fact that our temperaments were directly opposite. He would publish anything and hope for the best. I would not normally publish unless I was sure of the product. Strangely enough, sometimes he would also put my name on papers, if we had discussed the problem, without asking my permission.

The currents that were known at the time were the electrodynamic current, and the charge transferring weak current, which applied only to the left-handed part of the electron, muon, and neutron. This meant that the associated Lie algebra was a very unpleasant animal. My natural instinct was to delay everything until a better, more complete understanding could be reached. Abdus went ahead anyway and in 1958 published a premature effort with me (*nolens volens*) in which he tried to avoid neutral currents. I would certainly have objected to this particular disclosure. This was done right[21, 22] in 1964, but by this time Glashow had already (1961) published his paper, for which he shared the Nobel Prize in due course with Abdus and Weinberg.[23] One of the telegrams Abdus received read "widely admired richly deserved." What you lose at the swings you gain at the roundabouts, as they say in the circus. This conflict between premature publication and the fear of being scooped was now endemic. The more expert players developed a technique of the two way bet, to avoid this problem. Obscure journals could be used to prove priority if need be and conveniently forgotten otherwise.

The mysteries were deepened by the evident conflict between weak and strong physics. The identity of the electron and muon currents with the nuclear weak interactions surely linked both systems in some way. On the other hand, the existence of strangeness, soon to be accompanied by charm, seemed to also require Lie group structure.

Sometime in 1960 a student appeared on Abdus doorstep, in search of a subject for his proposed Ph.D. project. Abdus suggested that he look at all the second order groups, and see whether there was a reasonable fit anywhere. It was at about this time, that new resonances, now to be dubbed new particles, were appearing from the ends of the new super-high-energy accelerators. It did not take long for the student to return, with a newly minted paper on his hand, to say to Abdus: "$SU(3)$ fits!" "We all know that, but where are the triplets?" replied Abdus. This was not technically correct. $SU(3)$ definitely did not fit, if one included the weak currents, the only known currents, in the general picture. Only by considering the strong interactions on their own might $SU(3)$ be an acceptable symmetry, with dire consequences to the chances of ever understanding the weak currents.

10. More Travels

Despite the success of quantum statistics with Elliot, my future plans seemed as murky as ever. Elliot offered me a post at Maryland, but I was as determined as ever to find a job that required no constant miracle working. I went to a New York meeting of the A. P. S. and looked into the job market in secondary institutions, my rule of thumb being that only these would not be demanding golden eggs, or requiring graduate student supervision. I decided upon the University of Miami, Florida. Naturally one had to expect heavy teaching loads, but this trade off seemed well worth it. And so it was for a year or so. In the fall of 1958, I took leave to accompany Elliot to Europe paid by his contract as usual. When I returned there were big headlines in the newspaper: "University Physics Department Flunks Students." Sputnik had given the department enough courage to require an increased effort from the student body. This created a divergence with the administration. A poor assistant professor was denied tenure. He forthwith attempted to commit suicide. Soon after I was denied tenure too. Somehow my friend Walter Khon heard about this situation. He was in the process of leaving Carnegie for La Jolla, and he arranged for me to be offered a post as his replacement, at a fancy salary. No doubt he thought he was doing me a good turn. I had no choice but to accept.

I knew on the day of my arrival in Pittsburgh (1959) that I had made a terrible mistake. Although I had once visited Manchester and Liverpool in England, nothing could compare with the bleak horror of downtown Pittsburg as it then was. When term started I was confronted with a class of about twenty graduate students, and many faculty members, all eager to climb in this new bandwagon of statistical physics. I plodded through everything I knew, and some things I half knew, as best I could, feeling a complete fraud. I was actually inviting these poor souls to follow in my footsteps. I had disregarded the primary directive of Ward's law: ***better at a fifth rate institution with no ambitions than at a second rate ambitious one.***

At this point the reader will reasonably suppose that my guardian angel had moved into overdrive. The Institute inquired whether I would care to spend the academic year 1960-1961 in Princeton, now for the third time, and of course I accepted, intending to make as graceful an exit from Pittsburgh as possible. True to form, my angel then arranged for Ted Berlin, another old friend, to leave The Johns Hopkins University for the Rockefeller Institute, now to be transformed into the Rockefeller University, and I was asked to take his place at Hopkins. In those days Hopkins was a placid institution with an excellent reputation for not trying to keep up with the times. The physics department in particular specialized in old-fashion spectroscopy under R. H. Dieke and had other worthy faculty members, such as Rasetti. Ted assured me that I would find the atmosphere suitable for my special needs. This was certainly true, at least for the few years before I quit the U. S. for good.

About 1962 the accelerators were producing the famous decuplet, which directly implied some form of $SU(3)$ for strong interactions. But where were the triplets? And how was it that the weak currents were identical only for $SU(2)$ and then only for the left currents? Not to worry. The purely experimental facts were converted into a brilliant theoretical achievement, using methods more appropriate to the old days of robber barons than to the formerly sedate world of theoretical physics. Nevertheless, it was immediately clear that somehow all strongly interacting particles were constructed of so-called quarks, which are normally confined. In fact, of course, apart from the evident triplet structure, the first concept of $SU(3)$ was quickly replaced by the $SU(3)$ of color. Something less of a mystery is the present Standard Model, still enveloped in the enigmas of containment and families. Only singlet color systems are allowed to freely propagate. Why this is so is still a mystery to me. If there is someone outside there who understands these facts, I wish he would explain it to the public.

There was a gap of some thirty years between the birth of gauge theory and general acceptance. *Strange theories with strange names…eminently forgettable and now forgotten, were the common currency of high energy physics.* I found myself at Hopkins seemingly condemned to a life of negativism. There were many reasons that gauge theories were not taken seriously. One reason of course is the terrible bandwagon mentality of the time. More respectable reasons were the lack of experiments on the natural currents and a definitive understanding of the so-called Higg's fields and associated Goldstone bosons. Somewhere in Eddington's brilliant treatise of general relativity *he points out the difference between coordinate transformations and real physical phenomena.* The same could have been said in the case of gauge theory. Indeed there is a close mathematical connection between the two subjects. This misunderstanding of Goldstone bosons did not help. I was more concerned with the lack of any rigorous perturbation theory. In electrodynamics, the appearance of non-transverse photons had long been written off as a tiresome detail to be suitably forgotten. The non-Abelian case was really no different, only technically much harder. But *dynamical variables are what count in physics, not coordinate or gauge transformations*.

In 1963 Peierls offered me a readership in Oxford, as a result of urgings from Dick Dalitz and others, which for obvious reasons I had to refuse. I had been a candidate since 1959 for a fellowship of the Royal Society, and considered that, equipped with this prestigious title, my chances for a return to the real academic life, still available in odd parts of the world, would be excellent. This was achieved in 1965, with the help of many friends, already fellows.

11. Shangri-La for the Last Time

My election to the Royal Society opened up opportunities just in time. There had been a futile attempt at marriage and Dieke had died of a heart attack, leaving me vulnerable to criticism from the younger generation for the insufficient lying of golden eggs. The success of gauge theories was yet to come. I also knew that if I did not move soon, it would be too late. In a current copy of Nature I saw an advertisement for a professor of mathematics in Wellington, New Zealand. Now was the time to test the power of the FRS in the market place. I put in an application and was duly offered the appointment. As soon as I arrived in Wellington, I took a trip across the Tasman to see Catherine Dalton, who had written to me about the death of Cliff and her terrible difficulties trying to live without pension or insurance money. In Sydney I contacted my old friend Freddie Chong, who surprised me asking why I had chosen New Zealand instead of Australia. He said that he had just been appointed Professor of Mathematics at Macquarie University and what a pity it was that I had not chosen to join him there. I replied that this was the first time I had heard of it, but perhaps it was not too late. Certainly my first choice would have been Sydney if properly informed. It was not difficult to arrange the transfer that had many advantages for me. It was a new institution, and therefore, I assumed, would be progressive in its methods. Finally, it was on the North Shore of Sydney, one of the best locations in one of the most attractive cities in the world. I decided that this would be my last stand, come what may.

When I arrived at Macquarie it was possible to imagine the future institution as a possible equal to all but the great universities of the world. All that was required was vision on the part of the administration. Alas, for many years my hopes were sorely threatened by different policies, already embedded in the political undergrowth. The education establishment quickly demonstrated its dominance on the basic structure of the institution, and very nearly succeeded in monopolizing the control of admission policies and standards. The first indication of this was an extraordinary proliferation of courses offered by the schools of education and psychology. Within a few years most of the undergraduate students at Macquarie were teacher trainees, most of whom were studying anything but science. Something had to be done to prevent Macquarie becoming just another teachers college.

Shortly after my arrival in Australia, I received a letter from Dick Dalitz. He said that he knew that I had turned down an offer from Oxford and why, but now there was a position vacant at Cambridge. Was I interested?

There was no higher honor available in the British academic system, if an honor it truly was. The salary was ridiculously low, which I suppose enabled me to conclude that there was an element of honor involved. Here we have an interesting difference between the U. S. and Europe: in the U. S. the higher the salary the higher the honor, in Europe is sometimes the reverse. It is all a matter of what is expected from the recipient. In the U. S. one might be expected to lay golden eggs for a decent salary, or at least to maintain a suitable high publication rate. The European system is more civilized …or rather it used to be. Low salaries with security, but no paper counting, was the norm. But even in Cambridge in 1967 it was not clear that this system had survived. I had no ambition to either supervise students or to face criticisms for not doing so. Maurice Pryce once told me the story of his first encounter as a would-be graduate student, with Dirac:

Pryce: "I would very much like to be accepted as a student of yours"
Dirac: "Oh, I am very sorry but I don't think I need help with my problems at the moment."

But then I was not one of the inventors of quantum mechanics… I was indeed very tempted but felt I could achieve something more useful staying at Macquarie.[24]

The education establishment had only one interest: the supply of suitable docile science teachers. High-school courses were controlled by a syllabus carefully designed to be taught from textbooks under the control, and copyrighted, by a certain professor. Although many schools had long given up attempting to use these books in the classroom, there were still large stocks that schools had paid for and were loath to give up. As a result many teachers had voted with their feet, either giving up teaching altogether, or by converting themselves to teach other subjects, such as mathematics. The crux of the matter was that the books, and the syllabus, required the teachers to attempt to teach all the science subjects, regardless of training background. This was in accord with the strange doctrine, common enough in modern educational circles, that anyone can teach anything. The result, predictably, was that they were teaching nothing at all. My guess is that the university was supposed to contribute compliant graduates to supply the system. A student of sociology once commented that very little had changed from the time New South Wales was indeed a prison colony. A fundamental conflict had to be resolved.

Fortunately for me, the initial development of the physics teaching at Macquarie was left entirely in my hands, as was the recruiting of staff and planning of accessory services such as workshop facilities. It was possible, with a minimum of duplicity, to design courses well up to the level of other Australian universities, and yet still cater to the needs of the teacher trainees.

There is a common misconception about the teaching of science in schools. It is supposed that to do that successfully, it is not necessary to have teachers equipped with anything more than the absolute minimum of instruction in the sciences. Nothing could be further from the truth. As an experienced university professor, I would dread having to teach science in the schools. Questions that are reasonably easy to answer at the college level demand far more expertise from the poor school teacher, who must answer in the language of the pupils or not at all. *Popular science is often an oxymoron*, and the temptation to bluff it out is always present. *"I don't understand this either" is often the correct reply*. School science textbooks cannot possibly replace the informed teacher, informed particularly as to his own limitations.

With this in mind, and in imitation of Freddie Chong, who had already started a similar course for mathematics, I decided we would offer a M.Sc. for physics teachers, a refresher, or to be more accurate, a retraining course. The candidates would be required to attend for three years, of their own time, during school time, one morning a week (Saturday), and to submit written assignments each week.

Since this was one of the truly novel and useful innovation at Macquarie at that time, which I sincerely believe should be copied wholesale elsewhere, I will spell out in some detail the procedures we adopted. *The Feynman Lectures on Physics*, Volumes I to III, too original for sole use in standard university courses, are admirable as surveys of fundamental principles. I decided to make use of them as a basis for the lectures. I was most fortunate to have been able to recruit Dick Makinson as an Associate Professor, and we agreed to alternate each week, with the following timetable:

09:00 am.: Lecture by Professor X or Y
10:00 am.: Tutorial by Professor Y or X
11:00 am.: Tea break
11:15 am.: Other activities (until 01:00 pm.)

Students were expected to prepare for the forthcoming lecture by reading the appropriate sections of Feynman.

Each week assignments for the following week and marked assignments for the previous week were handed out, and assignments for the week handed in. The tutorial (by Y or X) was then dedicated to a discussion of the lecture (by X or Y), with no holds barred (!), and then a discussion of the previous week's assignment problems from the admirable problems book associated with the Feynman's texts. The last two hours were spent in various sessions of workshop practice, simple computer programming, electronics, remedial mathematics, and special individual projects, chosen by the students themselves, if possible as useful tools to their regular school teaching duties.

At the end of each school year there was an exam. This worked very well. There was an attrition rate of about 30% in the first year and none after that. We finished with about

40 graduates, and celebrated with a going away party, at which many declared that they had the times of their lives. Not so some of the wives, who said that sometimes their husbands would disappear completely for the week when a particularly challenging assignment was due.

This Special Masters Program for Physics Teachers (S.M.P.P.T.) worked so well that we repeated it again after a year of intermission. At the end of the second series, Dick and I decided that we had run out of candidates. We had at least refurbished some seventy schools in the Sydney metropolitan area. I have often wondered why similar courses are not offered in other places. They are not expensive, and are far more valuable than summer crash courses as offered sometimes to teachers elsewhere.

Another problem to be faced was the reform of the schools curriculum, and in particular to return, if possible, to the period of offering separate sciences. There was a syllabus committee for so-called Senior Science, packed in favor of the status quo. The reforming of this committee was a time consuming task, but finally we managed to get a few members appointed who represented our point of view. To have any chance of success we also had to have available viable alternatives in the way of textbooks and teaching equipment. For physics, fortunately, there was a local publisher prepared to back us by producing an Australian version of the Harvard Physics text, which in effect meant replacing all the maps of the U. S. by maps of Australia. The associated equipment needs were brilliantly met by several members of the S.M.P.P.T., who designed inexpensive and effective substitutes for the rather expensive Harvard Physics course, and who formed their own company to produce enough apparatus for the trials the New South Wales Education Department had insisted upon. In this way we thoroughly outfoxed the bureaucrats, who had believed that the obstacles strewn in our path were quite sufficient. At the end of the so-called trials they had no choice but to allow the introduction of this and similar courses in the other sciences as an alternative to the combined course.

12. Physics at Macquarie

The establishment of a physics department from scratch was not easy in 1967. I was very fortunate that Claude Curnow, an experienced teacher of first year students at the University of New South Wales, was available to start the first year of teaching, giving me one year to establish general principles for the next two and three year sequences. I decided that we would use the ***Feynman Lectures on Physics*** as an overall recipe, with more standard texts as routine backups.[25] Early in the second year, Physics 251 required a definite level of achievement…less able students were discouraged from continuing. It was then possible to effectively guarantee a reasonable smooth passage on to third year. Assessment wise we used the following device: all courses required students to submit weekly assignments and later model solutions were handed out for further study. The final examinations, then always contained some questions that were similar in subject matter.[26] We added subtle twists for the better students. Students quickly realized that determined study of all handouts was a good idea. I make no apology for this system. It worked very well.

Elmer Laisk was an absolute treasure, formally at the University of New South Wales, ran a splendid experimental physics course for the third year. Elmer should have been appointed professor at the University of Hamburg after the war despite being an Estonian. Somehow he was reduced to coming to Australia as a refugee, and was employed in a lowly capacity at the University of New South Wales. He had an encyclopedic knowledge of classical physics, and delighted to demonstrate how to obtain results in the laboratory without the use of expensive equipment. We could not have done without him. We were also able to introduce an electronic sequence, under the leadership of Ronald Aitchison, which complemented the physics courses, and gave more weight to the employment prospects of our graduates.

At the end of the fourth or fifth year after starting from scratch, *we had undergraduate degree courses running that I considered second to none in Australia.* But at this time we began to suffer from a need of good students mostly due to the B.Sc. problem described in the next chapter. It was now time to worry about graduate studies and research by staff. Good research in physics requires high quality staff and often far more money than is available in Australia. I would have much preferred a first rate undergraduate school to a second rate “research university.” Fortunately it was possible to combine the best of both worlds, by concentrating upon the field of laser development.

Under Jim Piper, just out from Oxford, in a surprisingly short time, Macquarie quickly achieved world class status in narrow-linewidth dye laser oscillators.[27] The Macquarie Centre for Lasers and Applications was then established. At the time of writing (1999) Macquarie is leading the world in some aspects of electro optics. Quite appropriately, Jim Piper succeeded me as professor upon my retirement (nominally in 1984, but effectively in 1980, since in the period 1980-1984 I spent half the year in California at UC Irvine).

13. Macquarie B.A. and B.Sc.

A problem for serious science teaching at Macquarie was the decision made early in the planning of the university to offer only a B.A. degree. My guess is that this was a deliberate attempt on the part of the education establishment to solve a long running squabble with the other two universities in Sydney. Teacher trainees would graduate with B.Sc. degrees whilst being financed as prospective teachers, and would then jump ship to better paying jobs elsewhere. A B.Sc. degree was more valuable in the Australian marketplace, and as soon as Macquarie started producing graduates, there arose quite spontaneously from the students themselves a demand that they be allowed to graduate as Bachelor of Science. This demand was strongly opposed by the administration and the education establishment, who might have suspected an attempt to undermine their stewardship of the scholarship holders, now nearly all channeled to Macquarie. In reality, the demand came almost exclusively from students with no connections to the teacher-training program at Macquarie.[28] The teacher trainees were already circumscribed by their enrollment in a combined B.A. Dip.Ed. program evidently designed to restrict their choices. Macquarie was the only university in Australia to offer education as an undergraduate subject with a large number of education credits. All credits counted the same amount towards the final degree and there was heavy encouragement on the teacher trainees to comprise their programs with education courses.

The sciences revolted[29] under the guidance of several student activists, Frank Duarte in particular, and a movement called ***Students for a Science Degree*** (***SSD***) was formed. This functioned as a political entity which publicized the problem in various ways and exerted heavy influence in the result of student elections. The extraordinary drift of Macquarie towards teacher training, to the near exclusion of all else, had also worried the federal authorities for some time. We were fortunate that Duarte had *somehow* established close links to the Federal Government,[30] which was now the source of all funds.

One day a reporter from one of the Sydney newspapers called me and asked me for an interview. He had heard about the Macquarie problems, and wished to hear my side of the story. I spoke quite freely to him[31] and explained how the university had suffered from the undue influence of the education establishment. Next day there were provocative headlines in the press.[32]

There was a special meeting of the University Council held to decide what to do. I received a letter … suggesting that I resign. I explained *that I had not said* that the university was disgraceful and scandalous. I had said that the actions of the education establishment, in building huge education and psychology departments by using their influence, were indeed so, which is an entirely different matter. I also enclosed a letter to the editor of the newspaper in which I said that a B.Sc. degree in the near future was a possibility.

A B.Sc. degree was announced, soon afterwards, following a meeting of the academic senate.[33] Later I discovered that the university had been “encouraged” by elements of the Federal Government to go along with the reform. Internally circulated papers complained about “intimidation” which I accepted as a compliment. We had now neutralized the education establishment, and for ever altered the future of Macquarie, ***no small achievement.***[34, 35]

Photographs

John C. Ward in the 1960s.

John C. Ward on the 30th of December, 1999.

References and Notes

1. John Clive Ward was born the 1st of August, 1924. His sister, Mary Patricia, recalls that he spoke for the first time at the age about four…in a complete sentence.
2. His sister wrote the following account about this period: "I was partly responsible for having John sent to Bishop's Stotford where he was so unhappy. He was doing well at the Westcliff High School and had a good friend, called John Porter, who was like himself perhaps a bit of a loner. They spent hours playing with a small sailing boat my father had bought second hand. The boat was really only meant for rivers and ponds but my father couldn't have known that. The boys sailed in the Thames Estuary where we lived and nearly drowned once when the boat overturned and their rubber boots filled with water.

   John was happy at his public grammar school, but when I was seventeen I spent a lot of time in the school library during spare periods and found, in a scholarship magazine, an advertisement for scholarships at Bishop's Stortford College. It caught my eye because our father had decided that he was tired of traveling up to London each day to his job in Somerset House and thought that a town called Bishop's Stortford in Hertfordshire would be a good place to move to. The train journey from there was only half an hour while the trip from Leigh-on-Sea took nearly two hours by steam train. He had to catch the 8:00 am train in order to get to his office by 10:00 am (an old joke pointed out that civil servants, like the fountains of Trafalgar Square, only played from 10 to 4).

   So, John was entered in the scholarship exam. The scholarships ranged from four pounds to one hundred pounds. I remembered pointing out at the time, that if John won the four pound scholarship it would cover his day boy fees when we moved to Bishop's Strotford. We didn't think he had much hope because the exam included Latin and Scripture and he knew nothing about those subjects. So he wrote the exams and cheerfully reported that he had failed because he couldn't do the Latin and had certainly failed the Scripture. So, that was the end, we all thought, and in any case my father had changed his mind and decided that we would stay in Leigh-on-Sea. But a letter then arrived asking my father to send his son to Bishop's Strotford for a weekend of interviews. After that, unfortunately for poor John, my father had another letter offering the top scholarship of 100 pounds. This sum was a lot of money in 1938. My father's salary was not much than 400 pounds (per year) and we only had our relatively prosperous life, and my father's golf, because my mother was always in continuous demand as a substitute teacher. So my father was not about to turn down a 100 pound scholarship in spite of John's pleas and I am sure he thought that it would be a wonderful thing for John to have public school, that is private school, education.

   John was offered the top scholarship because he wrote a perfect math paper. He was very unhappy at school because he was not particularly good at sports and hated being away from home. My mother said she was about to take him out of the school but asked him to stick it out for the year. He seemed to settle down after that but never really fitted in. His big consolation was his music. He had music lessons at school and glowing reports from the music teacher. He spent most of his free time playing the school organ. Then the war came and the young male teachers were "called up" and old retired teachers were recruited. Much of their knowledge, specially in science, was out of date so John really taught himself. I have always felt guilty that I was the initial cause of John being sent away to school. John had forgotten this though and of course it was my father's decision in the end. (***Note***: besides his early experience with the school organ, John became an accomplished pianist, and French horn player).

3. M. H. L. Pryce and J. C. Ward, "Angular correlation effects with annhilation radiation," *Nature* **160**, 435 (1947).
4. This was aunt Rose, his father's sister, who had given John a humorous book about life in Australia.
5. Author of papers in solid-state-physics such as R. E. B. Makinson, *Proc. Cambridge Phil. Soc.* **34**, 474 (1938).
6. H. S. Snyder, S. Pasternack, and J. Hornbostel, "Angular correlation of scattered annihilation radiation," *Phys. Rev.* **73**, 440-448 (1948).
7. C.S. Wu and I. Shaknov, "The angular correlation of scattered annihilation radiation," *Phys. Rev.* **77**, 136 (1950).
8. R. H. Dalitz, theoretical physics seminar delivered at The University of Cambridge (October, 1947).
9. The reader should be aware that this physics is directly applicable to problems of photon entanglement and quantum cryptography, see for example, R. H. Dalitz and F. J. Duarte, *Physics Today* **53** (10), 99-100 (2000).
10. F. J. Dyson, "The S matrix in quantum electrodynamics," *Phys. Rev.* **75**, 1736-1755 (1949).
11. J. C. Ward, "An identity in quantum electrodynamics," *Phys. Rev.* **78**, 182 (1950).
12. A computer literature search reveals that, as of January 2004, some 1197 papers have been published with the subject *Ward Identity* either in their title or abstract. This number becomes 2623 for papers published with the subject *Ward Identities* either in their title or abstract.
13. J. C. Ward and J. Wilks, "The velocity of second sound in liquid helium near the absolute zero," *Phil. Mag.* **42**, 314-316 (1951); J. C. Ward and J. Wilks, "Second sound and the thermo-mechanical effect," *Phil. Mag.* **43**, 48-50 (1952).
14. M. Kac and J. C. Ward, "A combinatorial solution of the two-dimensional Ising model," *Phys. Rev.* **88**, 1332-1337 (1952).
15. Summit, New Jersey, one of the locations of Bell Laboratories.
16. From *The Pirates of Penzance* of W. S. Gilbert and A. S. Sullivan.
17. For an additional perspective on the events at Aldersmaston see N. Dombey and E. Grove, "Britain's thermonuclear bluff," *The London Review of Books*, 22nd of October, 1992; for an alternative perspective see L. Arnold, *Britain and the H-Bomb* (Palgrave, New York, 2001).

18. E. W. Montroll and J. C. Ward, "Quantum statistics of interacting particles; general theory and some remarks on properties of an electron gas," *Phys. Fluids* **1**, 55-72 (1958).
19. Vector-axial vector.
20. Sakharov classified John Ward as one of the ***titans*** of quantum electrodynamics alongside Dyson, Feynman, Schwinger, and Tomonaga. See A. Sakharov, *Memoirs* (Knopf, New York, 1990).
21. A. Salam and J. C. Ward, "Electromagnetic and weak interactions," *Phys. Lett.* **13**, 168-171 (1964); A. Salam and J. C. Ward, "Gauge theory of elementary interactions," *Phys. Rev.* **136** B, 763-768 (1964)
22. A description of this work, for general audiences, is given in G. 't Hooft, "Gauge theories of the forces between elementary particles," *Sci. Amer.* **242**, 104-138 (1980).
23. In his 1979 Nobel Lecture, S. L. Glashow refers to a paper by A. Salam and J. C. Ward, "On a gauge theory of elementary interactions," *Nuovo Cimento* 19, 166-170 (1961), as "a remarkable portent of the $SU(3) \times SU(2) \times U(1)$ model which is accepted today."
24. Whilst at Macquarie John Ward did not supervise any graduate students despite determined efforts, to be accepted as such, by some mathematics students. This issue did not arise relative to physics students, since the physicists were mainly interested in experimental physics. At the post-graduate level his only physics interaction with students was provided via informal discussions and a course called *Topics in Physics* which was offered to Honours students, and in a voluntary format, to post-graduate students in laser physics.
25. Among these texts: C. Kittel, *Introduction to Solid State Physics*, 4th Ed. (Wiley, New York, 1971); P. L. Lorrain and D. Corson, *Electromagnetic Field and Waves*, 2nd Ed. (Freeman, San Francisco, 1962).
26. Final three-hour exams accounted for 100% of the assessment.
27. F. J. Duarte and J. A. Piper, "Dispersion theory of multiple-prism beam expander for pulsed dye lasers," *Opt. Commun.* **43**, 303-307 (1982); F. J. Duarte and J. A. Piper, "Narrow linewidth high prf copper laser-pumped dye-laser oscillators," *Appl. Opt.* **23**, 1391-1394 (1984).
28. It is important to point out that by the mid to late 1970s only a small minority of science students were involved in the education program. Also, the Macquarie academic model allowed graduation, in a given discipline, with a required number of core courses. The rest of the credits had a free format. This meant that a physics student could graduate with its physics core and with the rest of the credits being composed entirely of mathematics and even more physics courses. Thus, for many, the liberal system was applied to yield uniquely hardened physics degrees.
29. See, for example, "2000 seek introduction of science degree at Macquarie," *The Sydney Morning Herald*, 9th of November, 1977.
30. At the time the Australian Federal Government was in the hands of the Liberal Party and the National Party, which are conservative parties. The Prime Minister was Malcolm Fraser and the Minister of Education was Senator John L. Carrick. John

Ward was neither right wing nor left wing, he was an independent thinker. Dick Makinson, on the other hand, for example, was clearly of left wing leanings. However, both strongly supported the *SSD* student reform movement which was often accused by the opposition as being "ultra conservative" and even "elitist." For Ward and Makinson what was right for physics and what was right for Macquarie overcame possible ideological barriers. The *SSD* helped to organize an alliance with various non-left-wing groups and parties into what became known as the *Moderate Student Alliance*. This alliance was victorious in a series of elections at a time when Australian student politics was heavily dominated by the left.

31. This reporter was Greg Sheridan from ***The Bulletin***. In one of his subsequent articles he described the events as "…the battle for that degree was a nasty, bitter, bureaucratic struggle." See G. Sheridan, "Australian physicist wins Guthrie Medal," *The Bulletin* **101**(5239), 49-50 (1980).
32. At this stage it is instructive to provide a limited description of a more official version of these events as published in the book by B. Mansfield and M. Hutchinson, *Liberality of Opportunity: A History of Macquarie University 1964-1989* (Hale and Iremonger, Sydney, 1992): "The venom of the attack was, however, something new and formed a part of the new wave of activism on the part of science students and staff alike." And…"Never one with patience either for due process or the social sciences, Ward was vocal in his denunciation of the trivia that filled up Senate agendas while important issues, like new degree regulations… were passed over. Suitably then, it was a close student associate of Ward's, Physics Ph. D. student Frank Duarte, who began to mobilize student opinion in favor of a change."

    Here, it is appropriate to indicate that the science movement had a broad base of support among science faculty and that John's support, although important, was not unique. The following description, written in 1998, illustrates this perspective: "Beyond the issue of the name of the degree it was understood early in the movement that such change in the degree structure would have a significant and profound effect in the future direction of Macquarie University… Thus, this incipient student movement soon gained the strong support of leading academics within the sciences. Among the professors supporting the science movement were J. C. Ward, R. E. Aitchison, R. E. B. Makinson, E. Laisk, J. Hawke, B. Gray, and R. H. Vernon." See ***www.opticsjournal.com/BSc.htm***
33. See, for example, "Macquarie University approves B.Sc. degree," *The Sydney Morning Herald*, 13th of September, 1979.
34. John felt a particular sense of fulfillment about his participation in the events leading to this achievement. Later, when referring to this period he would smile subtly, his eyes lighting up, as he articulated some of the finer points.
35. John died, following a trip to the South Pacific, in Vancouver Island, Canada, the 6th of May, 2000. A biographical article, including references to articles on his life and a list of his physics papers, is published in ***http://www.opticsjournal.com/ward.htm***

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