Skeletons: A technical autobiography written for instruction and entertainment

Professor Jacques Heyman

Prologue

A book, or a literary essay, or a scientific paper, should exist in some sense as completely detached; neither the author nor the reader should be evident. It is obviously impossible, in an autobiography, to avoid the use of the word 'I', unless the clumsy artifice of writing in the third person is adopted. Thus the statement that I am not French is recorded here without reference to a possible reader, and this is the only time that you, *lecteur*, will be addressed directly.

My origins are to be sought somewhere in Flanders on 11 November 1918. My father, who had fought with the Allies, made a snap decision not to return to Romania, and a friendly English general gave him a laissez-passer to come to England. He arrived penniless, but with, as it turned out, a valuable asset – he spoke fluently four European languages, not counting Romanian.

Plastic theory

What 'plastic theory' means will, I hope, become clear. It applies to the analysis, and design, of engineering structures – a steel frame for a skyscraper, or a concrete shed for a factory – and, as it turns out, for the understanding of a masonry cathedral or a medieval arch bridge. In this sense the theory is universal, although it was developed initially for the design of steelwork. The final master statement of plastic theory, arrived at after half a century or more of work to which many people contributed, may be stated as 'If a structure can stand, then it will'.

This gnomic and slightly anthropomorphic statement can be illustrated with a very simple example. (To be clear, I do not actually think that a steel frame is in some sense a sentient being, but it does display what may be taken for some symptoms of life – it responds in a protective and beneficial way, for example, to disturbances imposed on it by a (hostile) environment.) The example is that of a simple four-legged table, and it will be supposed that the problem facing the structural engineer is the design of those four legs. To determine their size, it is evident that the engineer must calculate the forces in the legs in response to a known weight placed on the table in a known but arbitrary position.

It turns out, technically, that this is a very difficult problem. In very simple (and indeed simplistic) terms, there are only three equations, and four leg forces must be determined. The matter is straightforward for a tripod; the addition of a fourth leg makes the table, in conventional jargon, structurally redundant. In order to solve the problem, further equations must be written which are no longer simple – they are indeed hideously complex. Much effort has been devoted, from the middle of the nineteenth and through the twentieth century, to the solution of these equations, and ingenious (and brilliant) ways have been devised to obtain approximate but remarkably accurate solutions.

With the advent of the high speed computer the equations can be solved as accurately as is needed, and a definite and apparently unequivocal answer can be given for the problem – numerical values can be determined by the computer for the four leg forces. These values are the correct solutions of the complex equations, but they are not in fact the values of the forces in the legs of the actual table. A nearly rigid table placed on the pavement outside a restaurant on a summer's night will, annoyingly for the diners and for the structural analyst, rock – one leg will be clear of the ground. If the leg is clear by only a fraction of a millimetre then the force in that leg is known, pace the computer printout – it must be zero. Moreover it is not known in practice which leg is off the ground; a slight shift of the table will reduce the force in some other leg to zero. A friendly waiter will slice a wine cork diagonally into two wedges; one of these, placed under a leg, will at least make the table more comfortable (anthropomorphically for itself, and also for the diners).

This abstract example is a paradigm for the structural engineer; how can some meaningful statement be made about the forces in the legs (so that they can be designed) if any one of the legs may be clear of the ground, may be supported by a rigid floor, or may be resting on a flexible foundation of unknown properties? The reason that the computer has given wrong answers to the problem is that the program has assumed, or rather the analyst using the program has assumed, that all four legs are in contact with a rigid floor. Since in fact the contact conditions are unknown, the assumption is not relevant to the solution of the real problem; the computer has given answers for a ‘model’ of the table which does not represent reality. But the computer (the analyst) has apparently no alternative to the assumption if a numerical answer is needed, and this again is the situation facing the structural engineer designing, for example, a steel frame; how can a meaningful statement be made if the ‘boundary conditions’ for the frame are unknown, and by their nature unknowable?

However, although the computer solution for the table is wrong, in the sense that it does not describe the actual state of the table, this is merely a consequence of the fact that there is no single calculable ‘actual’ state. What the computer has derived is a *possible* state for the table. The computer has shown that the table *can* be comfortable; the gnomic statement of plastic theory gives assurance that the table *will* be comfortable. This gives the structural engineer the confidence to proceed with the design of the legs.

(It may be noted that the (statically-determinate) three-legged stool does not present similar problems for the designer, or for the diners. The leg forces are determinable immediately, and the stool does not rock. More generally, and also for structures which are redundant (four legs) rather than statically determinate (three legs), an examination of equilibrium forces can always be matched by some geometrical statement.)

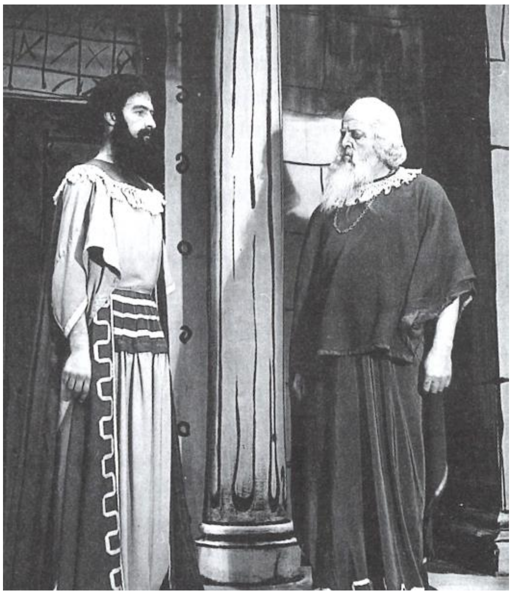
Cambridge

These sophisticated and difficult ideas were not current when I attended my first engineering lectures in Cambridge in 1942. My entry to the University had been through mathematics. I took Part I of the Mathematical Tripos in the summer of 1942 – in fact I was a by-term man, coming into residence in January, and I joined the October 1941 entry during the early years of World War II. I was aged sixteen, which was not a measure of my brilliance (although I was able enough) but was rather due to a series of accidents. I had a good training in my primary school, and I took the entrance examination to nearby Whitgift School in March 1934, two days after my ninth birthday (nine was the minimum age for admission). The examination was for entrance in September, but I did sufficiently well to be allowed to join the First Form at once, and so found myself in September in the Second Form, nearly two years younger than the rest of the class. Further, I continued to do reasonably well, and found myself in the ‘Special’ stream, which compressed three years’ work into two, so that I took the School Certificate examination (later to be called ‘O’-levels) when I was thirteen, at which age I found myself in the Sixth Form, specializing in mathematics. My School Certificate included a paper in Latin, which gave me exemption from ‘matriculation’, a necessary qualification for entry to Cambridge.

The mathematics teaching at Whitgift was outstanding – there were three masters dedicated to the Sixth Form, preparing the students for two years for the Higher School Certificate ('A'-levels). I passed the examination - not outstandingly well – at the age of fifteen, and was qualified to be admitted to university. However, a target was to obtain an Entrance Scholarship to an Oxford or Cambridge College, and Paul Wild and I spent a full third year in the Sixth Form being prepared by the three masters for the examination in the following December.

Cambridge was the university to aim for to read mathematics, and Wild's father had been at Peterhouse, so that was his choice of college. The scholarship examinations were elaborate and complex, and the colleges combined in groups of five or six to set and mark the question papers. I thought it good to part from Paul Wild (although we got on well enough) so I applied to King's College, which was in the same group as Peterhouse – the mathematics masters could prepare us both for the same examination. So it was at King's College that I sat my papers, and where I was interviewed by the College Lecturer in Mathematics, and also by J.T. Shepherd, a renowned Classics scholar who was Provost of the college, and whom I got to know in my post-graduate acting days in Cambridge.

(The Provost mounted Greek plays every three years, performed in Greek. These were notable events. For example, Vaughan Williams composed the incidental music for Aristophanes's *The Wasps*. He also sometimes directed Greek plays in English, and it was in the small part of the Priest of Zeus that I appeared at the Arts Theatre, in a production of Oedipus Rex which was reviewed in the London papers. As it happened, I got a one-line special mention in both *The Times* and *The Sunday Times*.)



Heyman (left) as the priest in Zeus | Photo: J. Heyman

I stayed in Cambridge for a couple of nights in a lodging house half a mile from King's College, and it was the first time that I experienced real night-time cold. There was a very severe winter in 1941, and the fifty-year old house had only coal fires in defence. (In any case, coal was strictly rationed during the War.) When my father married in 1924 he, as a foreigner, installed central heating (then rare in lower middle-class houses) in our newly-built house in Coulsdon. This smallish Surrey township is just south of Croydon, then also in Surrey, but now in London. I was born in a nursing home in Croydon; my mother had been born in Forest Gate, then part of Essex, and now also part of London.

So I walked into the centre of Cambridge to sit my exams at King's. Paul Wild did well enough to obtain admission to Peterhouse; I did not do well enough to obtain a Scholarship at King's. My father told me later that I was so upset by this that he determined to do something about it. I would like to think that he acted purely in my interests, but I suspect that he obtained satisfaction from being in control and for making something difficult happen. And of course it was unacceptable for a son of his not to succeed. My view on this may be coloured by my then increasing dislike, verging on hatred, of my father and of every aspect of his behaviour. In any case, he telephoned King's, and asked whether, although I had not obtained a Scholarship, I could nevertheless be admitted as a Commoner. The reply was that King's was not admitting sixteen-year old children.

My father then telephoned Charles Burkill at Peterhouse; he was a mathematician and Senior Tutor and in charge of admissions, and was later Master of the College. Burkill had read my Scholarship papers, and, over the telephone in mid-December, he admitted me to come into residence some three weeks later, early in January. So it was that I did not escape Paul Wild, but with him spent two terms reading for Part I of the Mathematical Tripos. Since I was well under age for National Service, I was allowed to spend a further two years in Cambridge, and in my third year I was just about the sole member of that cohort. There was no other candidate for the students to elect as President of the Junior Common Room, called in fact the Sex Club to commemorate the sexcentenary of the College's foundation in 1284. I had taken up rowing, and for the same reason I became Captain of Boats.

I spent my first two terms in lodgings – the College rooms had been full since October. My digs were in Tennis Court Road, in a house again without heating, and also without a bathroom. Every morning I would walk in my dressing gown the two hundred yards to the College, where there were facilities in the Birdwood. In the summer of 1942 I moved into a spacious room in the College, which I occupied for over two years. Again, no hot water, or water of any kind; the gyp would bring a small jug of hot water for shaving, and I would mix what was left with the cold water from the ewer, after I had sometimes broken the ice which had formed during the night.

Nor was there heating in the room, apart from an open fire, with no coal to burn. Very shortly after I moved in, I saw a cart in the street loaded with logs; I bought the lot, and stacked them in one of three small rooms opening in the corners of my keeping room – one of these was my bedroom. The logs lasted through two winters. In one of those winters, 1942 or 1943, my parents visited me in Cambridge, and I asked to be excused from the room to visit the Birdwood. On my return, my father asked me what the name signified, and I told him that Field Marshal Lord Birdwood had been Master of the College from 1931-38. On his arrival he was appalled by the lack of lavatories on the staircases and of bathrooms in the rooms; he caused the block with these facilities to be built at the back of the College, and it bore, informally, his name. General Birdwood gave me my laissez-passer said my father.

Engineering at Cambridge

Wild stayed on for a year to read Part II Physics, and after war service he emigrated to Australia, where he had an outstanding career as an astronomer, indeed as the leading radio-astronomer in the country; there exists today the Paul Wild Observatory. Peterhouse, as other colleges, takes pleasure in collecting outstanding alumni, and, late in his career, Wild was made an Honorary Fellow.

I had got into Cambridge on the strength of my (not outstanding) mathematical ability, but my father had always been against my embarking on a mathematical career. He viewed mathematics as merely an entry to more useful professions, and he put great pressure on me to switch from mathematics to engineering. I was not able to resist that pressure, although it caused me distress and worsened still more my relations with my father. I am now grateful to him, since I have come to understand, from a totally different viewpoint, why engineering is different from mathematics and science, even though all use the same language. Mathematicians are interested in deepening their understanding of their subject, just as scientists are for theirs. But engineers use mathematics and science in order to create something, whether it be a jet engine, or a microcircuit, or a skyscraper. Engineers may indeed find that the common stock of knowledge of mathematics and science are insufficient for the needs of a present task, in which case they must abandon the pure rôle of engineer, and become themselves mathematicians or scientists.

So it was that I found myself in the Department of Engineering in October 1942, reading for the Mechanical Sciences Tripos, a slightly pejorative title imposed by the University on a subject which was not, perhaps, quite respectable. In my early days in Cambridge engineering had improved in status, and the creation of the Veterinary School involved students held in even lower esteem than the engineers. In any case, with pressure from electrical engineers and others, mechanical sciences has now been renamed simply as engineering. The Department had been founded in 1875, on the death of Robert Willis, Jacksonian Professor (of Natural Experimental Philosophy), and incidentally probably the greatest ever English architectural historian, when it was realised that he had in fact been teaching engineering for several decades. The subject settled down in the first twenty-five years, and for the first half of the twentieth century the course was almost mandarin. There was a defined body of knowledge, which to be sure changed very slowly, which it was the duty of the Department to teach and of the students to master. The examination papers in each year were exactly the same as those of the last, although they did not present themselves in quite that light to the candidates. Innovations were accommodated. The invention of the transistor was noted, but the device was treated as a part of a conventional electrical circuit, and the examination question merely substituted the word ‘transistor’ for ‘thermionic valve’.

The syllabus was very broad, and correspondingly shallow. Cambridge had, and has, no department of civil engineering, of mechanical engineering, and so on; instead, all candidates, whatever their inclinations, were compelled to study all the subjects taught. The intending civil engineer had to sit a paper involving thermodynamics, and the electrical engineer one on the properties of materials. Such a general education has great practical advantages; when a career is finally chosen, some small knowledge of neighbouring disciplines can be extremely useful. This broad educational programme survives today in the first two years of the engineering course at Cambridge – specialisation is then permitted. In 1944, when I took the Tripos, it was possible to offer two or three special papers in addition to the whole range of compulsory subjects.

Cambridge prides itself on its supervision system. The syllabus is taught by the University, but all undergraduates are required to present themselves once a week for an hour's supervision from one of the Fellows of their college. These Fellows were (and, largely, still are) also University Lecturers; the lectures on thermodynamics would be given by the same ‘don’ who, later, in the afternoon, would see students, individually or in pairs, for their one-hour supervision. However, the lecturer in thermodynamics was expected to supervise, and to teach in the one hour, electrical circuits, mechanics, the theory of structures, and so on. The system led, in the first half of the twentieth century, to enormous inbreeding of the Faculty; in order to teach effectively across the board the dons had themselves to be Cambridge graduates since, apart from Oxford, there were virtually no other general courses of engineering in the U.K.

There was also a curious and perhaps related inbreeding of the Professors (and in the days before World War II most Departments had single Professors who were also Heads of Department, holding office for perhaps twenty years). Ewing held office from 1890-1903, before he departed to be Director of Naval Education at Greenwich; his successor, Bertram Hopkinson, was Head of Department from 1903-1918, before he was killed in an air crash, piloting his own aircraft; and Charles Inglis was Professor from 1919-40. All three were Fellows of King's College.

Charles Inglis

Charles Inglis reached the retiring age in 1940, at a time when there were retiring ages, but he stayed on as a wartime Head of Department until a new election could be made in 1943. He was, then, the professor whose lectures I attended in my second undergraduate year 1942/43. He was a remarkable lecturer, whose discourses were formally structured, and which brooked no interruptions. At the first of his lectures which I attended, someone had the temerity to ask a question – my boy, do not betray your ignorance of the binomial theorem. No one dared ever to ask a question again.

Inglis lectured on mechanics, both theoretical and practical – the vibration of masses on springs, and the balancing of aircraft piston engines. His interests were wide, and his observation keen – on Trumpington Street towards the centre of Cambridge from the Engineering Laboratories, outside the Fitzwilliam Museum, the tarmac had developed a corrugated surface. With the utmost clarity, and in terms that could be understood by a starting class in mechanics, he showed us that a mass on a spring (a car on sprung wheels), running over a sinusoidal surface, could be analysed to give a measure of the force between tyre and road as the car traversed the bumps. Absolutely counter to preconception, this force was a minimum at the peaks of the bumps, and a maximum at the troughs – should a wavy surface develop in the tarmac, it would be unstable, and only grow in depth.

There were separate examination papers in mechanics and in the theory of structures, but in fact, in 1942, mechanics (statics) was dominant. It was not out of place for Inglis to introduce structural examples in his lectures, and one of these was of a simple barred truss. The truss had a superfluous member, which could be removed without collapse of the structure (three legs rather than four). Having reached this point, Inglis was clearly grasping for the words to describe the truss, and after a second or so he stated that it had superabundant ponderosity, a choice of words which has of course remained in my memory for over seventy years. I remarked on the words to an undergraduate a year ahead of me, who had been to Inglis's lectures a year earlier. He told me that Inglis had used the same words then, and that they were accompanied by the same second's pause to find the exact and memorable phrase.

It was then, I think, that I realised that a good lecture could not be given extempore but had to be extensively rehearsed – the lecturer was, in some sense, acting the part of being a lecturer. And rehearsals could involve dress rehearsals; when Inglis gave his celebrated lecture in London, on gyroscopes, complete with the most elaborate demonstrations, it had been presented meticulously to a packed audience in the main lecture theatre of the Cambridge Engineering Laboratories. It is the duty of a professor to instruct and to entertain.

The Theory of Structures

It was not Inglis, but Brian Cooper, a University Lecturer and also Senior Bursar of Clare College, who really awoke my interest in the theory of structures. Some lecturers were poor exponents of their craft – Cooper was brilliant. Like most of his colleagues at that time (not all) he did little or no research to deepen knowledge of his subject, which was, as it happens, thermodynamics and its application to heat engines. But he was a Cambridge polymath, and he made the whole of a subject not his own come alive, with chalk on a blackboard and applied differential equations. He was of course distilling the essence of the engineering approach to the analysis of structures, an approach which had been developing for over a century, and which was applied throughout the world to the design of floor beams, to steel frames for skyscrapers, and to massive bridges made of modern materials rather than the traditional brick or stone masonry. These theories had a self-referential and perfect beauty, and they were fundamentally wrong; they were the wrong tools to tackle the problem of the four-legged table.

Cooper's lectures were listened to with close attention, and he must have experienced something of the thrill of presenting a discourse that had gone well. This thrill is identical to that experienced by an actor on a good night – Marc Antony using Shakespeare's rhetorical skill to rouse the Roman mob to mutiny (a mob of five on the very small makeshift stage in the Hall of Peterhouse), was at the same time rousing the audience to a shared emotional response. There is a palpable interaction between the actor and the audience on such occasions, just as there is between students and a well-rehearsed lecturer. Inglis was right not to tolerate the different sort of interaction that might be generated by interruptions.

Amateur Dramatics in Peterhouse - 'The Revenger's Tragedy': Heyman at the right | Photo: J. Heyman

Wagner

Inglis was appointed to the engineering Chair in 1919, and in that same year another King's engineer, Roy Lubbock, was elected to a Fellowship in Peterhouse, to look after the teaching in the College. He was the first, and for many years the only Fellow in the subject, until my own election in 1948, just before I had completed my Ph.D. Lubbock supervised me when I was an undergraduate, and, again, he was one of those inspirational Cambridge teachers who covered the whole range of compulsory subjects. He had a splendid set of rooms in a building which had scrambled to a finish just as World War II broke out; a building designed by a local architect, H.C. Hughes, whose practice, of a medium size, never really recovered from the war. It was in those rooms that Lubbock, with immense courtesy, carried out his tutorial sessions. And it was in the same rooms that, perhaps once a week, he would play gramophone records to anyone interested – usually half a dozen of us. The records were of course 78 r.p.m., and of shellac; they had to be changed at about four-minute intervals, and the fibre needle sharpened on a rotating sandpaper wheel.

I had become interested in what was then called simply classical music, but is now labelled baroque, romantic and so on, perhaps to distinguish it from present 'music', which is essentially 'popular'. The popular music of the 1930s to which I had been exposed was the kind of entertainment supplied by, for example, the B.B.C. Dance Orchestra – easy to listen to, and essentially anodyne. It was a shock to me when I first heard, on the wireless at the age of thirteen, a Beethoven symphony – everything about the music was totally new to me. One of the first records I bought, and they were terribly expensive, was a recording of the 'Egmont' overture, which fitted conveniently on the two sides of a 78 r.p.m. disc; but there was an active record club at Whitgift School, and I moved on rapidly, before I came to Cambridge, from Beethoven to the even more extraordinary sounds of 'modern' composers like Stravinsky ('The Rite of Spring' had been written only about thirty years earlier). And the Whitgift club introduced me to the whole range of the staples of concerts at that time – music by Mozart, Brahms, Rimsky-Korsakov and Rachmaninov.

Lubbock played us Wagner, very loudly. He had recordings of all the major operas, and would play highlights ('bleeding gobbets') week after week. I had never seen any opera, and indeed it was in Cambridge, when I was seventeen, that 'The Magic Flute' was performed at the Arts Theatre, by Sadlers Wells Opera (now the English National Opera). Cambridge was a centre for much music during the war; the B.B.C. Orchestra had been evacuated from London, and sometimes used the Arts Theatre as a studio, and there were recitals there and in the old music school off Downing Street, before Leslie Martin had given us a miniature Festival Hall in West Road. Peter Pears and Benjamin Britten entertained with Britten's folk song arrangements.

Wagner's operas, and opera in general, have been a major preoccupation for me, but I do not have a clear memory of that first ‘Magic Flute’. I later bought the pre-war recording by Beecham, transferred to 33 r.p.m. vinyl, and it was from this recording that my daughters came to know the work. It was an essential ritual that each of the three, when they reached the age of eight, should be taken to see a live performance. Another ritual was that as they each in turn reached the age of seventeen, they should be taken to Paris for a week by their mother, to be shown the city of her childhood.

Mahler

My wife showed Paris to me as well – I had been there only once before. We lunched together in Cambridge one Friday, and, on an impulse, we found ourselves that afternoon on a flight from Northolt; we were back at work on Monday. It was the first time for me to visit the Hôtel Biron, the one time home and studio of Rodin, and now a gallery for very many of his works. I had known many of the bronzes from photographs, but now saw some of them both in the gallery and also displayed in the garden. At the end of the afternoon, we visited the shop to see whether we could buy a souvenir. We both liked the music of Mahler, and had just seen his portrait bust, with a hand-lettered label giving the name of the composer as Malher. As really an arbitrary request we asked whether we could buy a reproduction. They pulled themselves up to their full height, and made us know that reproductions were out of the question. However, we could, if we liked, buy an original.

Rodin bronzes exist in editions of twelve; after the last has been cast, the mould is broken. Thus the ‘Burghers of Calais’ can be found outside Parliament in London as well as in their proper home. We could, if we liked, have casting number seven of Mahler. The price quoted ended the discussion abruptly.

Rodin’s Mahler | Photo: National Gallery of Art, CC0, via Wikimedia Commons

However, the possibility of owning a Rodin gnawed away for the next year, and we found ourselves negotiating with Mme Goldscheider, the curator of the Musée Rodin. The price was that of a modest new car, and it took every penny I had. The bronze was cast, and a few weeks later we collected him, stuffed him in a duffel bag, and walked him through the customs at Beauvais. I had in fact completed the forms for the importation of a ‘work of art’ into the U.K., including a certificate from the Tate Gallery; the customs at Gatwick had not seen this sort of thing before. The chief officer was woken, and gave no trouble.

This is the only major work of art that I have ever owned (it is now in the possession of one of my daughters). When I went on sabbatical leave later on, I thought to loan it to the Fitzwilliam Museum for safe keeping, but the then Director, Carl Winter, would not accept a modern casting. It is of course true that a casting made while Rodin was alive would have given him the opportunity to work over the final bust. Attitudes have changed; some of the Rodins now exhibited in the Fitzwilliam have the modern date of casting shown on their labels.

John Baker

When John Fleetwood Baker was appointed, in 1943, to the Professorship of Mechanical Sciences and as Head of Department, he found only one research student in the Cambridge engineering laboratories, and he was working in a cupboard under the stairs. The student was Egyptian, caught in the U.K. by the war, who went on to have an extremely successful industrial career, including the presidency of The Institution of Mechanical Engineers. When Baker retired twenty-five years later, as Baron Baker of Windrush, the Department had been totally transformed.

Baker knew Cambridge; he had won a mathematics scholarship to Clare College in 1919, and had discovered that he could, in fact, read engineering. Without really knowing anything about the profession, he had somehow decided to become a civil engineer. He found the Cambridge course to be uninspiring, but he was attracted to structural engineering. Jobs were not easy to find in 1923, and on graduation Baker was unemployed for six months, but he finally took a post at the Royal Aircraft Works, Cardington, to investigate structural problems of airships.

Baker had hardly known that there was an activity called research, but, although he was funded by the Air Ministry, his work was very much like that of a modern Ph.D. student, with an interested supervisor, A.J. Sutton Pippard, the Professor at University College, Cardiff, in the background. This was Baker's ‘apprenticeship’ – his job was to make experiments and do calculations for the stresses in highly complex airship structures. The work, in the absence of electronic computers, was back-breaking. Moreover, Baker found himself dissatisfied with the assumptions behind the conventional elastic calculations – so much so that differences of opinion led to his resignation from Cardington. But Pippard had been impressed, and appointed him as an assistant lecturer at Cardiff. Baker's big chance came in 1930, when he was appointed as Technical Officer to the Steel Structures Research Committee. Pippard and Inglis were both members of the committee.

Steel-framed structures had been in existence since the turn of the century, and by the late 1920s there was a multitude of conflicting building regulations in use in the U.K. and throughout the world. The steel industry set up the S.S.R.C. to try and bring some order into this chaos, and one of the committee's first tasks was to patch together a ‘Code of Practice’ from existing data. This Code was meant to be temporary, but it was accepted at once by the London County Council, and formed the basis of British Standard 449, *The Use of Structural Steel in Building*, a standard whose ghost, nearly ninety years later, is only now being exorcised.

The Committee, in addition to its eminent professors, had leading representatives from the consulting and contracting world, as well as from Government research stations. Such powerful and busy people were prepared to give their full attention, for two hours, roughly once a month, to whatever agenda were tabled before them. It was Baker's job, month by month, to assemble technical information, to write papers developing theory, to collect experimental evidence – in short, to make sure that the work of the Committee was prosecuted. The three volumes recording their findings, published between 1931 and 1936, are a

monument to the profession of structural engineering, and also to Baker, who had done most of the work and who wrote a great deal of the text. His contributions were recognised immediately; he was appointed to the chair of civil engineering at Bristol in 1933, at the age of thirty-two, and he was awarded the Telford Medal of The Institution of Civil Engineers. He continued to work for the Committee until 1936.

Some of the experimental work of the Committee was outstanding. New steel office blocks were being constructed in London in the 1930s, and, for the first time stresses on site were actually measured. The results were, to say the least, disquieting; the quantities observed by Baker on site bore almost no relation to the values calculated by the designers, and on which the design was based. In the Committee's own words, the method of design inherent in the patched together Code of Practice was almost entirely irrational and therefore incapable of refinement.

With hindsight, what was going wrong was the refusal of a steel frame to obey the designer's assumptions – the footing of a heavily loaded column would, unknown to the designer, settle by a few millimetres. Such a trivial movement can have a very large effect on the way a steel frame carries its loads, and hence on the stresses in the members; moreover the defects, as for the four-legged table, are not only unknown – they are unknowable by the designer. The Committee did in fact identify one cause of the anomalies; the way girders were bolted on site to the columns introduced stresses not originally allowed for. A painstaking programme of theoretical and experimental research gave some insight into the problem, and the Committee was satisfied that it had produced a rational and economical method for the design of multi-storey steel frames.

Heureux qui, comme Ulysse, a fait un beau voyage, or like that other, who achieved the Fleece. It must indeed have been very satisfactory, at the age of thirty-five, to have completed a major scientific investigation, to have published the results, to have been awarded the Fleece of the premier gold medal of The Institution of Civil Engineers, to have been appointed as professor in a leading university, and to have given engineers what they needed for the design of the skyscrapers of the future. It must have been all the more upsetting for Baker, at the age of thirty-five, to realise, even as the final report of The Steel Structures Research Committee was being written, that he had essentially wasted his time for the previous eight years. A small steel-framed cinema was under construction in Bristol. The Committee's new method had been tailored for a steel skeleton in its most simple form of regular arrays of beams and columns; it could not deal with the curved girder of the cinema balcony, nor with the light steelwork of the projection room. A rational universal method for steelwork design was as far away as ever.

The eight years were not absolutely wasted, of course. Very much more was known in 1936 about the real behaviour of steel frames than had been known at the start of the investigation. Most importantly, it was seen that an end of a road had been reached. There seemed to be no point in refining still further conventional elastic methods of design, since they would not apply to the real world of the cinema balcony. But there was as yet no signpost to the way forward.

The Institute of Welding had been interested in the work of the S.S.R.C., and when Baker took up his Chair in Bristol, they provided some modest support to start a research programme; John Roderick, later to be Professor of Civil Engineering at Sydney, was employed as an assistant. Baker himself was enabled to travel to Germany and to visit universities in the wake of a Congress held in Berlin in 1936. One section of that Congress had discussed the inelastic behaviour of steel structures – their behaviour when they were loaded beyond their elastic limits, and were permanently deformed from the elastic to the plastic state until they finally collapsed. Several workers were involved – Baker met Maier-Leibnitz of Stuttgart.

Maier-Leibnitz had contributed to theory, and he had loaded steel beams until they reached plastic collapse. If one statement should be made about Baker's contribution to structural engineering, it is that he realised, immediately and with absolute certainty, that the way ahead lay in the exploitation of plastic theory; moreover he had the drive and determination to develop that theory to the point where it is accepted world wide. It was not to be an easy way ahead; false paths would be followed, and detours had to be made; road blocks had to be cleared with the now customary back-breaking labour. But the signpost was there, and was read correctly by Baker.

The revolutionary idea of plastic theory as a design tool is very simple. We require our buildings to stand up, so that we can use them safely. The conventional designer, the elastic designer, attempts to calculate the actual state of a structure, so that assurance can be given that the building is safe. The plastic designer makes a trivial inversion of the design statement: instead of requiring a building to stand up, it is required not to fall down. The question then asked is how could a building possibly collapse under an overload, and this question involves completely different calculations, involving permanent plastic deformations rather than hypothetical elastic states, which Baker's work for the S.S.R.C. had shown were not in fact observable in practice. And it turns out that plastic calculations give a far more accurate representation of reality; they refer to the actual structure, rather than to the perfect model of the structure that must perforce be analysed by the elastic equations.

The elastic designer's assumptions do, however, seem reasonable, even if they lead to unobservable elastic states; common sense would lead one to believe that a trivial defect cannot really affect the strength of a structure. Common sense is in this instance correct; the paradox is resolved by concluding that the calculation of elastic stresses is not really relevant to the prediction of strength. The strength of a real structure does not depend on an elastic stress reaching some limit at some point in the structure; it is given by the steady development of unacceptably large deformations. It was precisely deformations of this sort that Maier-Leibnitz had observed in his tests on steel beams, and that can be observed in a structure made of any material that an engineer would consider reasonable to use in practice. Glass and cast iron are brittle; what is needed is a material with some minimal ductility. Steel in its plastic range exhibits this ductility, as does reinforced concrete, as designers have appreciated. It is ductility that accommodates imperfections of geometry or of lack of fit. Maier-Leibnitz had found that the collapse loads of his beams were almost exactly reproducible, no matter how roughly he set up his experiments.

In 1936 Baker could see the signpost, but not clearly the path, and the work went desperately slowly. In Bristol he and Roderick made experiments on simple portal frames, and at once realized that the behaviour of compression members – the columns in a skyscraper – would require deep analysis. However, the power of plastic collapse analysis was already amply confirmed by 1939, when World War II intervened.

Air raid precautions

Many in England, including my father, had foreseen the inevitability of war, which was finally declared in September 1939. But as early as September 1938, the time of the Munich crisis, the Government was digging trenches in public parks in London to serve as shelters from the expected air raids. The Anderson shelter, named for Sir John Anderson, the Lord Privy Seal, and Minister responsible for civil defence, was issued to poorer households; it consisted of a pit in the garden surmounted by a curved corrugated steel roof. The shelter was flimsy, and subject to flooding; in any case, it could not survive a direct hit, or even a near miss.

My father improved on the Anderson design by constructing a concrete box, sunk three-quarters into the back garden of our suburban house; the shelter slept six in bunk beds, and was an uncomfortable refuge when the sirens sounded the alarm, as they did increasingly during the duration of the war. In fact the war was 'phoney' during the first winter of 1939/40; and in January 1942 I left London for Cambridge, to sleep in the shelter only during the vacations. In bad winters sirens were followed almost inevitably by the sound of German bombers overhead, and blasts could be heard as bombs exploded within a radius of three or four miles. Equally frightening, from 1944, were the daytime attacks from unmanned 'V2' rockets; their characteristic sputtering noise would suddenly cease, implying that an explosion would follow within a few seconds. I went to live in Chester from the summer of 1944, and it took me a few weeks to understand that if an aircraft noise were heard, it would be from one of ours; German bombers rarely penetrated as far north.

For at least a year before the outbreak of war, the Government had established committees to consider air raid precautions, including the protection of factories and the design of shelters. The Civil Defence Research Committee met for the first time in May 1939 – it consisted of six eminent professors, including Pippard and

Baker. By September, Baker had been appointed Scientific Adviser to the Ministry of Home Security, with a remit to consider the strength of industrial buildings as well as the design of shelters. His work on the plastic approach to structural design convinced him that the secret of resistance to bomb explosions was to provide continuity and ductility, so that enormous amounts of energy could be absorbed without total collapse, or vital damage to the occupants. To help with all this the Government recruited a number of architects, including ‘Hughie’ Hughes, the H.C. Hughes who had created Fen Court for Peterhouse.

The most spectacular early application of plastic theory was to the design of the Morrison shelter, named after Herbert Morrison, then Home Secretary and Minister of Home Security. Over a million shelters were made and erected (often by Boy Scouts), and consumed over a quarter of a million tonnes of scarce steel. The intellectual basis of the design was both simple and elegant, and unattainable, indeed unthinkable, by the conventional elastic designer. The shelter, of the shape and size of a dining table, under which the family could sleep, was required to squash down plastically by no more than 12 inches if the house collapsed. The energy released in the collapse of a house can be estimated with some accuracy; the energy absorbed in the plastic deformation of steel can be calculated almost exactly. Equating the two gives the design of the shelter.

In December 1940 a small party, including Morrison and Baker, took a prototype of the shelter to 10 Downing Street, and explained it to Churchill. The interview was short, but Churchill made a decision: ‘This is the one. Give them to the people. Show them that it is safe. Blow a house up on one. Put a pig in it, put the inventor in it.’

Intellectual property

There is a curious postscript to the story of the Morrison shelter. In 1950, some nine years after its invention, Baker met Morrison again in Cambridge, and told him that he was applying for an award for the design of the shelter. Morrison commented, apparently seriously, ‘but I thought I designed it myself’. It is difficult to know what to make of this exchange. Morrison may well have thought that the idea of a table by day and a bed by night had been his, and that this was the ‘design’ of the shelter. The evidence, in fact, is that Baker had made the design in this sense, as well as making the plastic calculations. The Royal Commission on Awards to Inventors were in no doubt about giving Baker the substantial sum of £3000.

At a deeper level, there is very often the question of attribution to a particular person for an idea or invention. At early meetings of The Royal Society, for example (it was founded in 1660), the possibility of an inverse square law of gravitation was a common subject of discussion. It seems that Robert Hooke was the first to publish this openly, but he could not do the mathematics to prove the matter. Newton, when he heard of the theory (he kept away from meetings of the Royal Society), had no difficulty with the mathematics, which he presented in traditional form rather than his newly-invented method of fluxions – the calculus. Hooke was outraged when the first edition of the *Principia* made no mention of his name. He had learned, the hard way, not to reveal his thoughts to his colleagues. Later, he published such thoughts as indecipherable Latin anagrams, as did the giant Continental geometers – Huygens, Leibniz and James Bernoulli. The objective was to ensure priority of the concept, should some other scientist stumble on the idea, and manage to prove that it was correct.

The sailor in the crow's nest who shouted ‘land ahoy’ did not discover America – the credit is given, without much argument, to Columbus, although there is perhaps a small question of the rôle of the sponsor, Isabella of Castile. Less easy to determine is the rôle of the postdoctoral student, who makes an observation for which the professor receives a Nobel prize. As a much less important example, I wrote my first scientific paper during my research student days under Baker, and with great pride took the draft to him one evening. He read it overnight and the next morning sat me down for an hour to show me how bad it was. With his pen he corrected and rewrote page after page, until he was satisfied (as I was – he had made vast improvements). There, my boy, he said, capping his pen, but not before he had crossed out my name on the title page, and substituted ‘Baker and Heyman’. I felt the outrage that Hooke must have felt. In fact Baker had suggested the problem to me as a contribution to his continuing work on plastic methods of design; he had supervised my research (often vicariously in the form of John Roderick, whom he had brought to Cambridge from Bristol); and he had rewritten the paper in acceptable form (it was immediately published). I cannot now quarrel with Baker having added his name, but I have never added mine to any paper written by one of my research students.

Final-year lectures

Baker was, of course, an experienced author of scientific and technical papers, although this was not known to me when I attended his lectures, on structural theory, in my final undergraduate year at Cambridge. Oxford University Press had published in 1929 his book on differential equations, and in 1936 Pippard and Baker's classic text appeared: *The analysis of engineering structures*. The book gave detailed accounts, chapter by chapter, of the established elastic solutions for steel (and reinforced-concrete) structures. It was too early for the recognition that these solutions referred to idealized constructions, whose behaviour could not be observed in practice, although the second edition of 1943 appended a short section on plastic behaviour. By the time of the fourth edition, in 1968, this chapter had not been greatly modified; in particular, the analysis is ‘mechanical’, and betrayed no knowledge of the general principles given by the basic plastic theorems. These theorems were to be taught to me some six years later at Brown University, and they are fully exposed in my book with Baker of 1969: *Plastic design of frames, vol.1, Fundamentals*.

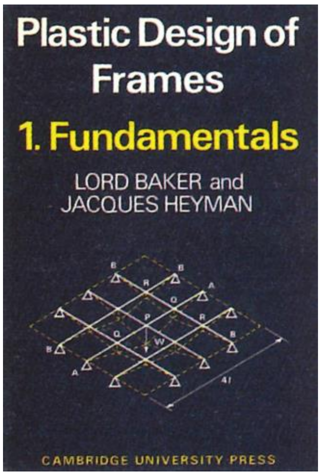


Photo: P. Pattenden

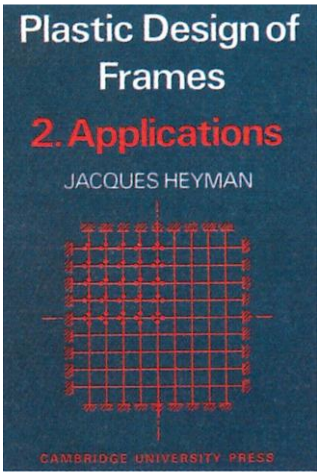


Photo: P. Pattenden

There is an intriguing chapter in *Pippard and Baker* on the behaviour of voussoir arches, the basic construction of medieval masonry bridges; this was one of Pippard's research interests. The analysis is fundamentally elastic, with references going back the best part of a century, although it was to be seen later that plastic ideas can help to explain the observed collapse of such arches.

Baker's lectures to us, third-year engineers, were based on Pippard and Baker, and by the time I graduated I had determined that I should be an (academic) structural engineer. However, the lectures gave no hint that elastic analysis resulted in numerical answers that could not be observed in the real world. On the contrary, very beautiful and powerful elastic theorems had been created in the nineteenth century, from which closed form (symbolic) solutions to the highly complex equations could be found – the forces in the four legs of the table could, apparently, be determined. Equally, ingenious computational techniques had been developed, by which a solution could be found, for example, for a smallish steel frame. This involved hard but enjoyable work, and Baker had us making calculations for an hour or so each week.

Such calculations are of course, or were, the stock-in-trade of school education; repeated working of numerical examples makes the student adept in the solution of similar examples. It might be thought that such an approach to learning is out of place in a university, but it does give students the opportunity to complete successfully ‘mandarin’ courses of engineering. A more respectable excuse for this sort of teaching is that, by repetition, students may see some pattern to emerge reflecting more fundamental principles. It is difficult to teach principles in the abstract – a good teacher will of course advert to those principles, but they will not be grasped at once by an undergraduate class. Only after tedious repetition is it likely that some understanding will emerge.

It is probable that Baker's school type of repetitive learning led me to be placed in the First Class of the Mechanical Sciences Tripos. I had indeed obtained a First in the previous year, but although the examination looked as if it were official, it counted nothing to the award of a degree, which was based solely on the examination in the final year. In the unofficial Preliminary I had, because of the unchanging form of the individual papers – seven of them – been able to estimate with some accuracy my marks in each. I knew as I sat the final paper that I already had a First, and, because the examination was unofficial, the real marks were made known to me and were close to my estimates.

I carried out the same arrogant exercise for the degree-giving Tripos in my final year, and knew as I sat the last paper that I would have to score 200% to obtain a First. When I became Head of Department I found that a filing cabinet in the back room of my office contained examiners' minutes from the late nineteenth century onwards. The records for 1944 showed indeed that the preliminary order of merit placed me reasonably near the top of the Second Class. However, each candidate, in addition to the compulsory papers, could take two or three ‘B’ papers, which were assessed α,β and so on. My answers to Baker's questions (and they were clearly Baker's questions) were good, sufficiently so for a pencilled box to surround my name and drag me up to the First Class.

One of the last things I did in Cambridge, before I left in 1944, was to see the Marlowe Society perform ‘King Lear’ at the Arts Theatre. This society was then firmly in the hands of George Rylands, a Fellow of King's and a noted interpreter of Shakespeare. He sometimes acted himself. He played Oedipus in the Provost's ‘Oedipus Rex’, in which I had the small part, but he produced Lear with his undergraduate cast. Entertainment in the 1940s had not reached the lurid levels of today, and there were two or three nurses from St John's Ambulance at the back of the theatre, ready to treat any extreme reaction of the audience, for example to the putting out of Gloucester's eyes. I had not seen or read the play. My own experience was perhaps something like my first encounter with Beethoven. I had ‘done’ ‘Midsummer Night's Dream’ and ‘Julius Caesar’ at school, and enjoyed the plays at a simple level, but nothing had prepared me for the force of Shakespeare's verse, for Gloucester's torture, or for the pathos of the final recognition of Cordelia by Lear. My children, in due course, came to love Shakespeare; I forbade them to read Lear before they saw it for the first time.

Rylands was a great interpreter; not a serious scholar, but with an immense knowledge of verse and drama. He was a formative influence on John Barton and the developments at Stratford. And, in his own right, he produced in 1944, in London at the Theatre Royal Haymarket, a ‘Hamlet’ in which both Gielgud and Peggy Ashcroft starred.

A little later in the ’40s there were the memorable seasons at the New Theatre in St Martin's Lane, when the repertory company included Olivier, Richardson and Alec Guinness. They appeared, sometimes separately, and sometimes together; they combined as Lear, Gloucester and the Fool.

Jet engines

So I graduated, and the class was addressed, collectively and individually, by C.P. Snow, chairman of the Joint Recruiting Board. It was the Board's task to decide for each of us how we were to be conscripted into War Service. C.P. Snow was, of course, the author of the eleven novels (1940-1970) in the sequence *Strangers and Brothers*, and also the originator of ‘The Two Cultures’, a discourse which I heard him give in the Cambridge Senate House in 1959. The novel *The Masters*, 1951, is a *roman à clef* based upon an election in Christ's College, of which he was a Fellow, and describes a Cambridge which really no longer exists.

Snow told the class that our National Service would be spent in Industry, to which we would be directed, and that, unless we had an uncle who was an admiral, we would not be conscripted into the Armed Forces. Our training would, unlikely as it may seem, be more helpful to the war effort if we capitalised on our rudimentary knowledge of engineering science. This was upsetting for some, and irritating for those of us who had spent six hours every Sunday in term time in the severe discipline offered by the Senior Training Corps, from which we had ‘graduated as Army officers. We were directed to Reserved Occupations, in my case to Salisbury Plain, to assist in the development of military tanks.

In fact, I never went. Someone intervened – I suspect Roy Lubbock – and I was directed to Thornton-le-Moors, between Chester and Liverpool, and I went to live in Chester. Shell Refining and Marketing Company had established an aero-engine research laboratory, staffed by chemists, physicists and engineers. Not all were as junior as me – several of the staff had doctorates (when Ph.D. degrees were scarce), and the output of the laboratory was sophisticated, at a high level of applied science, and, we hoped, of immediate use to the war effort. The work was only marginally on the mechanical engineering of engines, but rather on problems of combustion. Shell was after all part of the petroleum industry, and was concerned with the development of fuels, both for 24-cylinder piston engines for propellor-driven aircraft, and for the rapidly developing jet engines. I did not know at that time that Frank Whittle had been seconded by the Air Force to pursue his work on such engines in Cambridge, and, at the same time, to read the undergraduate course in engineering. It was Roy Lubbock who spotted his arrival, and secured his admission to Peterhouse, and who made sure he got a First-Class degree.

I spent some of my time on the investigation of the correct shape for injection nozzles to ensure optimum combustion, but the work was varied. For example, it was found that I had some rudimentary knowledge of German, and I translated a couple of papers that had somehow fallen into the hands of the laboratory. I also wrote half-a-dozen internal reports, which I cannot think were of much significance, although they were organised as scientific papers suitable for publication – which was out of the question since the work was secret. I was of course not uninterested in the work I was doing, but it was in the wrong field – I still wished to have a career in structural engineering. I knew that Baker in Cambridge was heading a research team, and in 1945, after about a year in Chester and with the War drawing to a close, I went to see him. He received me sympathetically, but there was at the time no possibility for him to take me on, although all his research workers were officially in reserved occupations as their National Service.

Liverpool

During my undergraduate years in Cambridge I had attended, during the summer vacations, several Promenade Concerts held in the Albert Hall. Queen's Hall, the original home of the Proms, and to which I went only once, was destroyed by enemy action in May 1941. The Albert Hall Proms were usually lengthy (programmes were longer then than now, often consisting of two symphonies and a concerto, with shorter pieces as overtures to the two halves), and they were made longer on occasions by the warning notes of the air-raid sirens. Until the all clear was sounded, the audience remained, and the orchestra played. The first concerts I attended were conducted by Henry Wood, and later he was assisted by Basil Cameron and Adrian Boult, and later still by Malcolm Sargent; the soloists were famous and included, for example, Solomon and Moseiwitsch. I continued to go to the Proms in 1945 and 1946, in my holidays away from Chester.

One of my immediate discoveries in 1944 was that one could get by train from Chester to Liverpool through the 1886 Mersey Tunnel, quickly and cheaply. The Liverpool Philharmonic Hall had been completed in June 1939, and was the most modern and luxurious concert hall in England. It is situated in Hope Street. One of the supreme examples of twentieth-century Gothic is also in Hope Street. The Anglican Cathedral Church of Christ was designed by (Sir) Giles Gilbert Scott, son of George Gilbert Scott, who won the design competition in 1903 at the age of twenty-three. The cathedral took twenty-five years to build.

The programmes of the concerts given by the Royal Liverpool Philharmonic Orchestra were both varied and adventurous; conductors included Barbirolli, Beecham, Sargent, Münch and Boult, and soloists came from London – Solomon and Moseiwitsch, and Casals playing the Dvořak cello concerto. The orchestra sometimes appeared in Chester Cathedral, and I went there three or four times – Casals played the Elgar. In Liverpool Peter Pears and Dennis Brain gave an early performance of Britten's ‘Serenade for tenor, horn and strings’. And I heard Bartók's ‘Concerto for Orchestra’ for the first time. In all I must have attended thirty or more such concerts in my two years in Chester, all of high standard, and immensely significant in a war time England starved of music.

The Liverpool orchestra also gave concerts in the Gaumont Cinema in Chester, and it was in 1945, just as the war ended, that I saw in that cinema Olivier's film of ‘Henry V’. It is fashionable now to find fault with Olivier's way of doing things, but I remained in the cinema for a second complete showing of the film, completely overwhelmed by Shakespeare and by a film which was clearly the result of enormous effort and dedication. I was still much attracted by everything connected with the theatre, and I had indeed appeared in two or three productions by the amateur dramatic club of Chester – Noël Coward and the like.

It was in the Spring of 1946 that I received a letter from Roderick in Cambridge, a letter which I was totally unable to understand. I showed it to my father, who told me that Baker was offering me a job. Of course I went to see him, and found that the job was for three years working as a research assistant on the development of plastic theory. I would, effectively, be a salaried research student with Baker as supervisor (in the days when supporting grants for such students were almost non-existent). Of course I took the job, but I was not allowed to register officially as a Research Student; by 1948 Baker thought that I had done enough work for a Ph.D., and I was back-registered to 1946. (I obtained the degree in 1949, news which I heard aboard the *en revanche*U.S.)

Prefrontal lobotomy

My first year back in Cambridge coincided with my brother's last year as a medical student; he obtained his degree, and went on to complete his clinical training at St Thomas's Hospital in London. (Addenbrooke's in Cambridge was not then a major teaching hospital, as it is now.) It was in that year that he was radicalised by an American (not Billy Graham) preaching at the invitation of C.I.C.C.U., the Cambridge Inter-Collegiate Christian Union. He wrote about his conversion to my father (our father, no capitals), who was almost uncontrollably furious. I never saw the letter, but my father telephoned me; about the only thing I could grasp was his objection to phrases, like following Him, in which any reference to Jesus was capitalised. His rage was such that he wrote to Charles Burkill, officially my brother's Tutor, accusing him of negligent ‘parental’ control. Burkill received this with equanimity, but had a discussion with me. There was, after all, nothing that he could do, but he asked me if there was any Jewish element in my father's reaction. This was something I had never considered; there had never been any hint at home, and I answered automatically and without thinking that there was no such cause.

My father was in fact almost certainly Jewish, but the opportunity never arose to discuss the matter. I had essentially left home, and when I returned in 1950 from a year in the U.S., my father was gravely ill with cancer; he died when I was twenty-five. I, of course, am technically not a Jew, since my mother was undoubtedly Gentile, but I am, I think, as Jonathan Miller would say, Jew-*ish*. *En revanche*, my children and grandchildren are technically Jews, but not in the slightest Jewish. There has never been, for several generations, any taint of religious observance, Jewish or Christian, in our households.

My father was clear that a religious conversion could indicate only some malfunction of the brain, and my brother was sent to a succession of doctors for diagnoses. Eventually a doctor was found to administer electric-shock treatment. The treatment did not work, and left my brother with his same fundamentalist beliefs, and, I think, a slightly impaired intelligence. He spent his life as a medical missionary in Africa, in Tanzania and Kenya, and never gave up hope of my salvation. About once a year, at Christmas or my birthday, an improving paperback would come through the post. He died in 2015.

Research at Cambridge

So my research student days began in 1946; the work was heavy, and, in retrospect, partially unnecessary. I saw Baker frequently, but Roderick continuously – he was there as an Assistant Director of Research, with no teaching duties, but concerned full time with the prosecution of Baker's work. He was disliked intensely by the half dozen of us working for him. He was ill-mannered and a bully, but he taught me everything one should know about working in a laboratory – how to make observations and how to record them, how to keep a diary, and how to assemble data. At the end of the war he was clearly of professorial status; engineering departments in all the universities were expanding explosively, and we thought ourselves failures if we had not been appointed to a chair by the age of thirty-two. (I was thirty-nine before I got mine – an abortive and miserable excursion to Keble College and the University of Oxford.) No one in the U.K. would have Roderick, and this was why he went to Australia in 1951, as Professor of Engineering and Dean of the Faculty in Sydney University. His abrasiveness ensured that in due course he became Mr Civil Engineering in Australia, just as Baker, not too abrasively, headed the academic profession in the U.K. I visited Sydney in the mid-1990s, long after Roderick's death. He was remembered very well, and not with affection.

My three-year apprenticeship to the study of structural engineering involved an eight-hour day at a desk or in the lab. This left another eight hours which I filled in various ways. I thought of a problem which lay outside the mainstream of Baker's research, although it was in fact concerned with the plastic behaviour of a simple steel beam. This was before the invention of the electronic calculator, although we had some fast electrical machines, and the problem involved heavy and tedious manual calculation. After six months I had written a paper, which was published in due course.

The French influence

This still left some hours in each day, and I finally got the theatre out of my system. It was in fact a complex process, involving several different, sometimes interlocking, activities. First there was the revelation, after the war in Europe ended in 1945, of what had been going on in occupied France.

Sartre's four-volume novel *Les chemins de la liberté* was appearing. The narrative was informed by a moral climate totally different from anything I had experienced or imagined, and the behaviour of the enormous cast of characters was of compelling interest. Moreover that behaviour was totally dictated by Sartre's existential view of the world. I read with partial understanding his expositions elsewhere of atheist existentialism, a philosophy which I could accept with some enthusiasm. I read also some of the extensive writings of his partner, as we would now say, Simone de Beauvoir. These were suffused with feminism, with which I had no problem, and also with exciting epigrams such as: To the precise extent that you kill time, time kills you. There were many such quotable aphorisms, clearly the constructs of a very able mind. I had not yet seen 'Richard II': I wasted time, and now doth time waste me.

My wife died at the age of fifty-six (she should have died hereafter – tomorrow) when my youngest daughter was still in her teens, and her elder sister only a little older. Beauvoir had written un récit, a book on death called *Une mort très douce*, which revolved round the appalling agony of her own mother's last days. She spoke of her horror to the nurse who had cared for her mother. Not at all, said the nurse, it was a very easy death. I had two copies of the book, and I gave one to each of my two youngest daughters; one had been for six months to the Lycée in Kensington, and she read it in French. Both found it to be of comfort, perhaps even the last sentence: All men must die, but his death is an accident for each individual man, and even if he knows and accepts it, it is a brutal outrage.

It was later that I realised that both Sartre and Beauvoir were extremely clever and really rather unpleasant human beings.

Sartre also wrote plays, of which perhaps the most famous is the one-act 'Huis Clos' (In Camera), in which the closing line is 'Hell is other people'. I got hold of a copy and made a translation, and determined to stage the play, with, of course, myself playing Garcin – there are only two other characters. It was very late into rehearsals that I thought of writing to the publisher, who immediately forbade any performance.

Sartre also joined the number of twentieth-century French authors who sought inspiration in the classical myths, following on the tradition which includes Corneille and Racine. His play 'Les Mouches' follows the pursuit of Orestes by the Furies, and is a free elaboration of the story. By contrast Anouilh's 'Antigone', while not being a translation, follows the story as told by Sophocles, but with twists. For example the whole point of Sophocles is that Antigone wishes to give burial to her brother Polynices, while Creon has ruled that only the other brother, Eteocles, should be honoured by a ritual funeral. The body of Polynices should be left exposed, to be eaten by the dogs. The two brothers had killed each other, and the twist given by Anouilh is that the dogs had already got at the bodies, which were unrecognisable and indistinguishable.

One of the most imaginative of these modern French plays is by Jean Giraudoux – 'La guerre en Troie n'aura pas lieu', which is perhaps existential in its insistence that the ten-year siege of Troy was inevitable. This setting had been explored by Shakespeare; I had recently seen 'Troilus and Cressida', which I thought at the time to be the greatest play he ever wrote. I have retreated from this unsustainable view, but the play is still very great.

One of the most interesting of these reconstructions was by Jean Cocteau, a poet of almost the first class, who ventured into neighbouring artistic fields. He had been active both before and after the Great War, but found it difficult to establish himself at the centre of the exciting new developments in Paris. What must I do, he asked Diaghilev, which elicited the famous reply, *Étonne-moi*. Cocteau did indeed provide scenarios for a couple of Diaghilev's ballets. His play 'La machine infernale' is an astonishing reworking of 'Oedipus Rex', the 'Oedipus Tyrannus' of Sophocles in which I had the small part in the Provost's production, and his film *Orphée* is a commentary on yet another myth. Cocteau's films may well be classed as poetry; the beautiful and surreal *La belle et la bête* is a fairy tale brought to life. All of these benefited enormously from the outstanding actors that Cocteau was able to recruit.

There are other French films, not by Cocteau, which saw the light in England after the end of World War II. *Les visiteurs du soir* gave me my first glimpse of the divine Arletty, and it was she, as Garance, playing with Jean-Louis Barrault, who gave unforgettable performances in *Les enfants du paradis*. *Paradis* is a pun both in English and French; the theatre figures largely in the film, and the 'gods', *paradis*, are the gallery seats farthest from the stage. I thought at the time that the 1945 film, by Marcel Carné and written by Jacques Prévost, was the greatest ever made. I have not retreated from this view.

Greek

In England, a series of paperbacks had been launched by Penguin Classics, who commissioned new translations of ancient texts. E.F. Watling, who for twenty years was a classics master at King Edward VII School in Sheffield, provided both 'Oedipus the King' and 'Oedipus at Colonus'. Very shortly after they were published, we determined (I determined) that 'Colonus' should provide a summer open-air entertainment in the Scholars' Garden of Peterhouse. There were two weeping elms in the garden, whose foliage came to the ground and provided wings to the stage, and served also as large tiring rooms completely hidden from the audience; characters of the play could emerge as needed. This was possibly the only occasion on which I 'dried' (I was playing, of course, the large rôle of Oedipus), but a few steps towards the trees produced, unnoticed by the audience, the required prompt. Watling came down from Sheffield to see the performance.

All this suggested to me that I should write my own 'Oedipus at Colonus', which I did as a short one-acter. After years of wandering, blind, and led by his daughter Antigone, Oedipus comes to Athens, where he is well received by Theseus. In some versions of the story, Oedipus is finally killed by the Furies; in Sophocles, Oedipus dies in the presence, only, of Theseus, who thus knows the holy site of Oedipus's grave, which, as a place of veneration, will be of enormous benefit to Athens. It is implicit in Sophocles that Oedipus is more valuable to Theseus dead rather than alive.

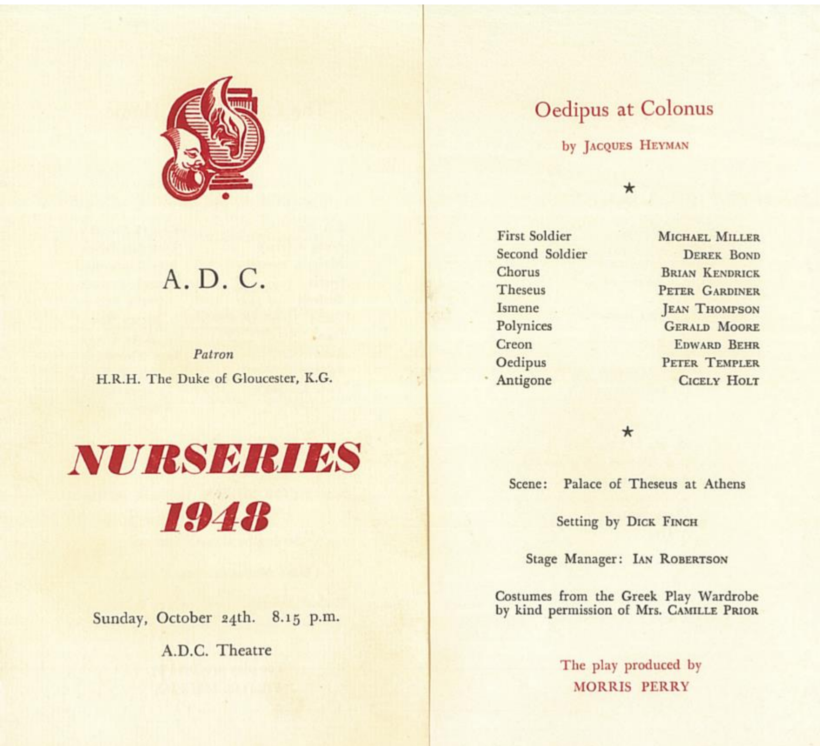


Photo: J. Heyman

Deaths in Greek tragedies take place off stage, and are reported by the Messenger. In Sophocles, the Messenger reports that Oedipus vanished. The twist I gave was for the Messenger to sow doubts in the hearers' minds as to how exactly Oedipus died – did he fall or was he pushed?

The play was performed at the A.D.C. theatre together with two other one-acters – a Eugene O'Neill, and the mechanicals' mounting of Pyramus and Thisbe in 'M.N.D.' (John Barton played Peter Quince). These were 'Nurseries' in which aspiring undergraduates had a first chance to show their talents. My 'Colonus' was well if politely received; a few days later George Rylands told me that he liked what I had tried to do (with no indication that I had succeeded).

The Cambridge phenomenon

The work I did under the direction of Baker (Roderick) was on the collapse of portal frames, and this gave rise to my first paper ('Baker and Heyman'). The frames consisted of two columns rigidly connected (welded) to a horizontal beam, thus forming a simple square or rectangular structure firmly connected to the ground. Maier-Leibnitz's beams collapsed by the formation of plastic hinges when, under increasing load, the structures passed from an elastic state to the development of ductile 'mechanisms'. The portal frames collapsed in a similar way, but with a more complex pattern of plastic hinges, depending on the precise way the frames were loaded (simulated snow on the roof, wind against the side). The calculation of the values of the collapse loads was a matter of simple mechanics, and no great theory was needed. The object of the tests was to determine whether these collapse loads could be observed in practice. They could, with an almost indecent accuracy. Thus the resulting paper, although basically simple, was well worth publishing; it supported Baker's conviction that plastic theory would provide the way forward to the design of steel-framed buildings.

But designers would have to be convinced, and Baker knew that the practical world would be suspicious of laboratory tests on tiny structures – my frames measured six inches by six inches. In 1946 the Welding Research Council, which had supported Baker's work since the 1936 days in Bristol (and who were, through the University, paying my salary), established itself as the British Welding Research Association, supported by the steel industry, and bought twenty-six acres of land at Little Abington, about seven miles from Cambridge. The site included the manor house, and the price paid was the ridiculously small sum of £3850. The establishment of such a research institution, having close contacts with Baker and the Engineering Department of the University, was a very early example of what became known as the Cambridge Phenomenon.

The manor house was large, with a basement, a raised ground floor, and a first floor, the *piano nobile*. Above were the attic rooms, inhabited by the servants, and to which there was no staircase, access being by ladder. A small amount of renovation work was put in hand, the ground floor providing offices, and the first floor two or three spacious apartments. When I first knew the house, in about 1947, one of these flats was occupied by Richard and Katy Weck, Czech refugees, who became extremely good friends. Richard had completed a Ph.D. as a mature student in Cambridge, and was now in charge of the research being proposed by the B.W.R.A. Indeed, he moved at various times between posts at Abington and a University Lectureship in Cambridge, before finally moving back to Little Abington as Director of the Welding Institute, as it came to be called.

They were joined in Little Abington by (Antonin) Tony Bartl, Katy's brother, an artist with very little hope of any income. He survived on small commissions – designing letterheads and selling an occasional painting. I have several of these, including a half-length portrait in oils of myself, which I bought for my parents; they did not like it. His work was not cheerful, as might perhaps be expected from a refugee from the War, but they were expertly executed. Baker gave him two chances in Cambridge, both murals, the first in 1949 for the new workshops block for the Engineering Laboratories, which was the first new building in the University after the War; it was opened by Field Marshal Smuts. It was seeing Tony Bartl working on this that made me realise the depth of his knowledge and skill; the mural, a short history of engineering, was a fresco painted in the traditional way on wet plaster, with egg whites in the colours. In the same way he took the opportunity to learn the technique of designing and executing mosaics; when the Wecks built themselves a house at the entrance to the Welding Institute, he provided an external two-metre high panel composed of hundreds of tesserae. (I provided an extremely slender and almost invisible steel skeleton for the house, which gives large uninterrupted living spaces.)

Tony Bartl's fresco in the Engineering Laboratories, Cambridge | Photo: Cambridge University Department of Engineering

The second mural is also very large, and is in oil paint on wood; it is in the entrance lobby of the next new building on the Engineering site, now called after Baker, and completed in 1952. The Baker Building was opened by Prince Philip, and Terence Cuneo recorded the event in one of his busy conversation pieces, in which a score of staff are recognisable among the very many depicted. Both of Bartl's murals still exist. He married while in Cambridge, and after a short while went to teach at the Lincoln School of Art, where he remained for the rest of his life.

Croquet

The other large flat in Abington Hall was occupied by Nicol Gross, a Romanian like my father, and who like my father entered England in an irregular way. He had spent the first part of the War in Cairo, working under P.C. Vellacott, who was Director of Public Relations, Middle East. Vellacott had been Senior Tutor of Peterhouse in the 1920s, then Headmaster of Harrow School, and then again at Peterhouse as Master of the College. It was he who admitted me as a freshman in January 1942, on one of his intermittent visits to Cambridge during the War. He arranged for Gross, an engineer, to be allowed to come to England.

Baker took on Gross as a student, working on the strength of metal tubes. He was successful in his Ph.D. work, and was immediately employed by the B.W.R.A. at Abington; they were interested in the very difficult problem of welded connexions in pipelines. Gross had been junior tennis champion of Romania, and a tennis court soon appeared at Abington Hall. There we had enjoyable games, myself totally outclassed by Gross and by a former Cambridge tennis blue, Bernard Neal. Neal was also doing research with Baker, a year ahead of me. Nicol Gross stayed for a few years with the B.W.R.A., but he was a man of immense energy and extraordinary physical presence; he departed for a career at the top levels in Industry. He died suddenly at the age of about forty.

Bernard Neal and I taught each other to play croquet on the Fellows' lawn at Peterhouse; after the first game he always beat me. Indeed he soon became the croquet champion of England, and represented the country in international matches – which in practice meant playing against New Zealand. He had played at Wimbledon, and was therefore elected as a (life) member of the All England Lawn Tennis Club, where he became an extremely influential member of the governing committee. I owe it to him that on three or four occasions I found myself sitting in the Royal Box on Centre Court.

On one of those days there was absolutely no play, because of rain, and it was Neal who went to Australia to see how a tennis court could be closed against the weather. The Centre Court now has a retractable roof, and another is soon to be installed on No 1 Court. He also persuaded the committee to revert to the nineteenth-century form of the Club's name, and it is now known as The All England Lawn Tennis and Croquet Club. After the tennis tournaments in June/July, half a dozen of the outer courts become croquet lawns. Neal's career and mine, while very different in detail, had much in common; his final appointment was as Professor and Head of the Civil Engineering Department of Imperial College.

It was at Abington that Baker arranged for full-scale tests to be made on portal frames, to confirm both the theory of plastic behaviour, and my experiments on miniature frames. In reality the frames were about half full size, but they were welded up from typical rolled steel joists and erected on practical foundations. The tests were, by their nature, expensive, and required enormous manpower, supplied by many research students not directly concerned with Baker's work, and by some final-year undergraduates. Steel skips were suspended from the rafters of the frames, and each of these was loaded, up to a total of five tonnes, by means of lengths of caterpillar track links (all placed by hand), and finally by adding weighed amounts of water until collapse occurred. Similar skips were used to apply horizontal side loads simulating the effect of wind. Some behaviour was noted that had not been seen in the miniature tests, but the accuracy of plastic theory to predict collapse loads was amply confirmed.

In 1947 the B.W.R.A. made the first of many developments in the grounds of Abington Hall; a fatigue testing laboratory was needed, and the concept of a single, relatively vast, steel-framed shed was proposed. One of the students in Baker's team was M.R. Horne, a couple of years ahead of me in his work for the Ph.D., and he made the design of the steelwork. The steel frames were pitched-roof rather than the rectangular form that had been tested, but Horne had no difficulty in making a straightforward simple plastic design, using standard rolled-steel sections rigidly welded together on site. (Site welding was uncommon in 1947.) Every such design has of course to be approved by the Local Authority, and approval would have been virtually automatic if the design had been made in accordance with British Standard 449, The Use of Structural Steel in Building, the Code which Baker's S.S.R.C. had cobbled together some fifteen years earlier. Unfortunately it was only in 1948 that the Code was to be amended by the addition of a single sentence permitting plastic design (but giving no guidance as to how it was to be applied). Horne had no alternative but to submit elastic calculations, purporting to support a design which had actually been proportioned by plastic methods.

Grand opera

In 1919 my father found employment in Bermondsey with a firm of leather brokers. He knew nothing about leather; the firm was keen to trade with Europe after the Great War, and they needed my father's knowledge of languages to deal with their correspondence. He took himself for six months to night classes at the Leathersellers Technical College, and found himself increasingly trusted by his employers. So much so that he was sent to France to try to sell some of the leather stocked in the warehouse in Bermondsey; he succeeded in the first week of the month he had been allowed. By 1924 he was earning enough to get married and buy the house just being built in Coulsdon. A couple of years later he established, in Bermondsey, his own business as a leather broker. He was, in a modest way, extremely successful, and he survived the complete destruction, by an air raid near the end of World War II, of his premises in London.

From the start of his business in the 1920s my father concentrated on European markets – France, Germany, Italy, Portugal, Spain. He bought raw hides from South America, tried to predict what next year's fashions would dictate for shoes, gloves and handbags, and would arrange for the hides to be processed and dyed accordingly. He would then, once and sometimes twice a year, take a four-week trip to Europe, where he was in touch with the manufacturers catering for the fashion houses. I was always very upset, at the age of seven or so, to see him off at London, although I loved the splendour of the boat train. No-one told me that the tapping of the wheels was to detect any signs of metal fatigue.

World War II put an end to these trips, and he started another company trading in different classes of leather for the British market, but he resumed his European visits as soon as possible. In 1948 he took my mother and myself with him, and this was really the first time that I went abroad. In the early 1930s we had had a couple of seaside holidays in Belgium, but in the middle of that decade he built a four-bedroom house in Felpham, just outside Bognor Regis on the Sussex coast. This provided seaside holidays, not only in summer.

The 1948 trip took in Lugano for a few days, and business meetings in both Switzerland and Italy. At one of these meetings I saw with a shock (at the age of twenty-three, my father behaving deferentially; until then he had in my eyes been totally authoritarian, not only at home, but in public. At a restaurant, for example, he would almost always insist on a table other than the one offered. In Lugano he rejected outright the hotel which he had booked, and he swiftly found us another. It might have been a sort of game to him – to impose his will on someone or something.

We went to Milan, and naturally to La Scala – my first European opera house. I go to La Scala once every forty-seven years – that is, I went again in 1995 to see ‘La Damnation de Faust’. This was a curious experience; there are arias in the opera, but not in the conventional Italian sense. Berlioz leaves no room for applause, and the audience was obviously bemused. The reception was polite enough, but the idiom was clearly unfamiliar to most of those present. ‘Aida’ in 1948 was very different – a grand opera given a grand production. On both occasions we sat in one of the boxes comprising the whole of the horseshoe of the auditorium. The ambulatory has a series of doors, each giving access to a box, and those on the other side to the box's private retiring room. I had not heard ‘Aida’ before; I was overwhelmed.

The end of student days

On my return to Cambridge I found that I had been elected, by competition, to a three-year Fellowship at Peterhouse, with leave to absent myself for a year. The appointment was actually to a Bye-Fellowship, and Peterhouse was one of the first colleges to offer what are now called Junior Research Fellowships. Candidates submit a programme of study, together with samples of the results of their research; in my case I submitted work which would form part of my Ph.D. thesis in a year's time.

The stipend of a research fellowship was, and still is, small, but certainly sufficient for a bachelor, particularly since there was a free apartment (with service) consisting of a large living room, a bedroom and bathroom, and seven free meals a week, usually dinner in the evening. This enviable style of living, perhaps designed to dissuade young scholars from embarking on the time-consuming chores of matrimony, was really an echo from before World War II, although something still survives.

So I prepared my Ph.D. thesis. It recorded a great deal of work, experimental and theoretical, and its physical production was laborious; before the Xerox machine, every equation had to be written by hand into four copies (two for the examiners, one for the Engineering library, and one for me). The examiners were kind enough to pass me; had Cambridge been a European university, where it is the custom to grade Ph.D.s as *summa cum laude, cum laude*, or plain pass (or, of course, fail), I guess I would have been placed in the second class. My oral examination had taken place a few days before I left England for my year's absence from my Fellowship.

In 1947 Baker had met, at a conference, William Prager, who was working in the field (among others) of plasticity; he had graduated in engineering from the Institute of Technology in Darmstadt, and was a powerful self-taught mathematician. He occupied important posts in Karlsruhe and then Göttingen, but in 1933 he fell foul of the Nazis, and he went to Istanbul, together with other eminent German scientists, to help create a modern system of technical education in Turkey.

Prager's reputation grew steadily, and just after the start of World War II (the U.S. was not yet involved), Brown University invited him to set up and head a new Division of Applied Mathematics, specializing in several fields, including the theory of structures, but above all in the theory of plasticity. Prager accepted, but it was not easy in World War II for a German to travel by obvious routes. His journey was by air, train and ship, and took in Baghdad, Karachi, Bombay, the Cape of Good Hope and finally New York, not too far from Providence, Rhode Island. Brown University had taught engineering for many years, but, apart from some input by Hardy Cross between 1909 and 1918, the Division was of the 'gum-boot' variety, teaching practice with only minimal theory, and with no sign of research activity. By judicious appointments at the professorial level, Prager later totally transformed the Division of Engineering, which began to work closely with his own professors in Applied Mathematics.

At their meeting, Prager and Baker took an immediate dislike to each other; they had different patterns of thought and different approaches to the prosecution of research. However, they were both working in the field of plasticity, and each realized that the other had much to offer – Baker his wealth of practical experience of structural engineering, and Prager his grasp of the power of mathematics. They agreed to exchange postdoctoral workers, with visits planned for a year at a time and funded, of course, from the U.S.

Brown University

Prager had a magnificient and foolproof way of raising research funds. He would take on a young postdoc, paid through the University but in reality from Prager's 'slush funds'. The young worker was free to work as he or she pleased, receiving close but not oppressive advice from Prager. After say three months this relationship bore fruit, and the postdoc had assembled some work into a publishable paper. Prager would then cast about for a suitable sponsor and tell them that a young recruit to his Division had made a promising start in such and such a field, and could they provide funds to see a successful conclusion to the work? Since the paper had already been written, Prager was able, within a few months, to satisfy the sponsor – he always delivered what he had promised – and to replenish his own funds.

Bernard Neal was the first to join Prager under the scheme; he completed his Ph.D. a year before me, and sailed for Providence in the autumn of 1948. He had been married for a few years, and his hosts at Brown had arranged accommodation for him. A year later I myself experienced American hospitality, and in due course I tried to put in practice the lessons it had taught me. As a trivial example, a stranger moving into a new city will without question be entertained to a meal on the first day, and also on the second and third days, until the businesses of shopping and self-catering have been mastered. In Neal's case he found himself in the care of a very, very quiet and very clever professor of engineering, with a wife and two children. The Baker/Prager agreement bore early fruit, and Neal and Symonds wrote several important papers together, exploring almost totally new ground in the field of plastic design. In fact, the collaboration was so obviously rewarding that Paul Symonds accompanied Neal when he returned to Cambridge after a year.

I was the second postdoc to visit Brown, making use of my permitted year's leave of absence from my Fellowship at Peterhouse. I arrived before Neal had left, and Paul Symonds's wife displayed that American talent for hospitality by taking me round department stores to buy necessary bedding, towels and so on; Prager had arranged for me to have a room in a graduate house.

He had done more than that; he met me off the 'Mauretania' at the dock in New York. In 1949 it was absolutely forbidden for a citizen of the U.K. to hold foreign currency – Prager met me with a bundle of dollars. Bevin and Stafford Cripps had travelled with me on the 'Mauretania' – they first class and I steerage. Daily press conferences gave the reassuring information that there was no question of the stability of the pound sterling; within half an hour of docking, the pound bought 2.80 rather than 4 dollars.

My own work at Brown during the year was on two topics suggested by Prager, both concerned with the plastic design of structural frames of far greater complexity than the simple sheds that I had studied for my Ph.D. thesis. It turns out, as a technical matter, that plastic ideas offer a far more direct attack on practical design, that is, on the assignment of specific steelwork for the beams and columns of a steel skyscraper; a conventional elastic approach very often becomes a matter of trial and error. Moreover alternative designs, equally able to carry specified loads, could be generated, and a problem which I tackled, with some success, was the assignment of material to give the lightest possible structure. Prager read my draft report, and decided that the Office of Naval Research in Washington, through which much Government money for research was channelled, would be interested in the work. In the Prager way, a contract was arranged, money came to Brown University, and in due course a report, actually already written, was delivered. There was a slight hitch in its presentation; I did not have clearance to visit a U.S. military office, and Prager himself went to deliver my paper and explain its contents.

The second topic was more intractable, but again I made some headway; it was a discussion of the way in which a three-dimensional structure might behave. The structures studied by the Steel Structures Research Committee in the 1930s consisted of a series of parallel arrays of beams and columns, which are, each of them, essentially two-dimensional. An example of a structure working in three dimensions is that of a simple grillage of floor beams – steel girders running north/south and east/west but loaded transversely, so that deformation occurs out of the plane of the grillage. This seemingly straightforward problem leads to complexities which are not evident with the skyscraper.

In both of these topics I was using techniques I had learned from Baker, but they were now slowly infused by ideas from Brown. Baker's approach, and therefore mine, was to try to imagine how a structure might behave when it was loaded beyond the elastic limit, and some ductile yielding occurred. Where were these plastic regions located? How could numerical information be deduced to lead to practical designs? The analysis was essentially ‘mechanical’, applying laws of statical equilibrium (of course, correctly) without any basic underlying principles.

Gvozdev

The basic theorems of plasticity, which would give the underlying principles, had been stated and proved in Russia in 1936; they were published obscurely in 1938, and were totally disregarded. They were picked up again in 1946, when the theorems were republished, without proofs, and again in Russian. This time they were noted, by Prager, who understood immediately their relevance and power. The theorems are so simple that they seem, simultaneously, to be both obvious and extremely unlikely. Once they had been stated, there was no difficulty with their proofs, just as Newton, having been told that the inverse square law might be used to explain planetary motion, found no difficulty with the mathematics, whether the arguments were from classical geometry or from his newly invented method of fluxions. Prager and Greenberg proved the plastic theorems, and gave full acknowledgement to their originator, Gvozdev; I think I would have understood their arguments while I was in the sixth form at Whitgift School.

The engineering analysis of some object, a structure, an electronic circuit, or a jet engine, is not possible without some simplification; the engineer analyses a *model* of the object, rather than the object itself. Thus for a steel-framed building, for example, the dimensions will in general refer to the centre-lines of the beams and columns; the material will be assumed to be perfectly ductile (plastic); elastic distortion of the structure will be small, so that calculations may be made with reference to the original specified dimensions; none of the members will buckle or otherwise become unstable, and so on. These assumptions are in fact perfectly reasonable, and enable the discussion of a large range of engineering structures, but care must be taken to ensure that, in practice, the assumptions are satisfied.

For such an idealised structure, then, the three theorems may be stated, not exactly in Gvozdev's terms, but interpreted anthropomorphically for a structure of the type considered here, and exemplified for example by the steel frames tested by Baker in the 1930s.

Theorem I. If the engineer can think of a way in which a structure is comfortable under given loading, then the structure can never collapse under that loading. (If the engineer can think of a satisfactory state, then the structure itself can also think of a satisfactory state, but which is unlikely to be that constructed by the engineer.) This is the safe theorem.

Theorem II. If the engineer can think of a way in which a structure might collapse under given loading, then the structure cannot in fact carry that loading; it will collapse at lower loads, or, at the very best, at the loads calculated by the engineer. This is the unsafe theorem.

Theorem III. If the engineer calculates the greatest possible values of the loads that satisfy Theorem I, and the least possible values of the loads that cause collapse (Theorem II), then these two values are equal. This is the uniqueness theorem.

The safe theorem has, implicitly, been understood through the centuries and millennia by every building constructor. Prager's proof of the theorem gives those constructors the assurance needed for a comfortable night's sleep. Indeed the fact that a Greek temple, a Gothic cathedral or a skyscraper stands after its completion is itself the proof that such buildings are themselves comfortable under their loads.

Prager had just announced the theorems as I arrived at Brown University in 1949, and they provided the theoretical backbone for the work Baker had been doing for a decade. The theorems were easy enough to state, and seemingly to understand, but they are highly sophisticated for the layman, and also for the engineer whose education has been based on the elastic behaviour of structures, even when modified by the possibility of plastic yield. Their importance was clear at once, but it took several years for them to become ingrained in my approach to analysis and design. However, I returned to England after a year at Brown with at least some understanding that Baker's absolute faith in plastic theory was supported by mathematics as well as by experiments in the laboratory.

I had enjoyed my year at Brown. Providence has changed very much since 1950; at that time the University was pleasant enough, and there were a few houses of some distinction a little way down the hill. But downtown was dreary, and there was very little in the way of 'culture'. A visiting company did put on 'Rigoletto', with Lawrence Tibbett, but otherwise I went to Boston, only an hour away, for occasional concerts. And I saw my first musical, which happened to be 'Kiss me Kate'. I went to Boston also to see the Red Sox at Fenway Park, at a time when the legendary Ted Williams was still playing. The other sport in which I became interested was American football; Brown was part of the Ivy League, and there were often matches at the weekends.

What was memorable during the year was the food. The United Kingdom, in 1949, long after the end of World War II, still had severe rationing of basic provisions, and exotic fruits, many nuts, cheeses other than cheddar, had not yet reappeared. There were no shortages in the U.S. in 1949, and when I returned to England in 1950 there were very few shortages in the U.K.

What also changed during the year was my relationship with other students in Cambridge. In the post-war years up to 1949 the student population was composed of a thorough mix of undergraduates from school, of research students like myself, and of demobbed members of the Armed Forces, returning to complete their degrees. In the college supervision system I found myself teaching men (there were no women) who were older than me in age, and vastly older in maturity and experience. We were all students together. After my year away I found that I was no longer really a member of that group; I had stopped being a student and was regarded as a don.

The academic ladder

On my return to Cambridge Baker offered me a junior lectureship (called a Demonstratorship), an appointment for five years. However, I still had a year to run of my Fellowship at Peterhouse, and a job in the Department of Engineering was not compatible with the research I was supposed to be doing to justify the Fellowship. I would have to give up my comfortable life style in the college. I turned down Baker's offer, knowing that it would be repeated after a year.

It was not. Perhaps Baker had been offended at my rejection of his offer a year earlier, but in any case he had a love/hate relationship with Cambridge colleges. He would appoint promising young men (there was only one woman, a distinguished materials scientist, on the Engineering staff) and, when they had tenure after five years, some would be seduced away to time-consuming College posts, such as Tutor or Bursar. In 1950 most College Bursars, who looked after their College's finances, also held University posts, and there were only three full-time professionals. Of the others, several were Lecturers in Engineering, who made appearances in the Department only to give their lectures. Nowadays almost every College has a professional, often a youngish middle-aged man who has had experience in the City. For whatever reason, Baker made it clear to me that after the year away the offer of a University post no longer held.

As it happened, Baker had need of me, and it was Richard Weck who persuaded him to call me in, to give me a dressing down, and to renew his offer. Baker's 1936 Code had become, almost unchanged, British Standard 449, which controlled the design of structural steelwork in the U.K.; permission would not be given for construction unless it could be shown that the design satisfied this Code. The Code was concerned entirely with the calculation of elastic stresses, but it was in 1948 that Baker persuaded the British Standards Institution to add a single sentence to the revised edition of the Code, permitting the use of plastic methods. We in Cambridge thought we knew how to use the new theory, but no one else did.

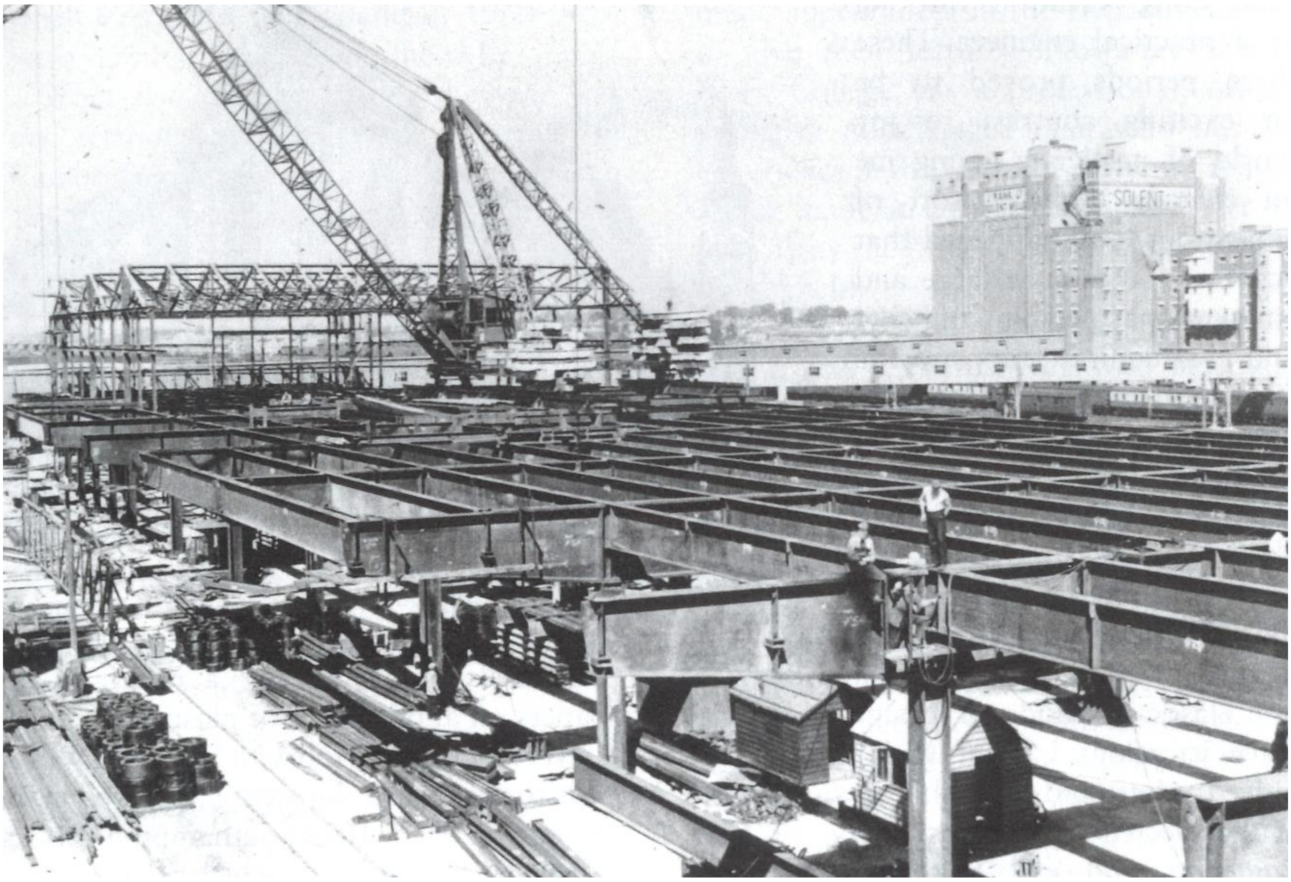
Baker devised a nine-month taught course in plastic design methods, of a kind unusual in Cambridge at that time. The students would be seconded from the staffs of consulting engineers, contractors and others who wished to use the new method. About a dozen students each year attended the course, and they were taught by a staff of three, whose duties were dedicated to these students. Richard Weck was in charge, and two other lecturers were needed. Michael Horne was one, and Baker offered me the other post, subject to a condition. Baker was always insistent that his staff should have had experience of Industry, and he fought a losing battle to try to maintain this requirement. A losing battle because after two years with a manufacturer, a consultant or a contractor, the young qualified engineer could not accept the severe drop in income that a return to university would entail.

In my case Baker told me that I must, for the next three years, spend three months of each Long Vacation working as a practical engineer. These three periods proved to be an exciting contrast to my world of academe, giving me an entirely different sort of education. It so happened that Scott and Wilson, a large and eminent partnership of civil and structural consultants (they had, for example, recently designed the engineering structure of the Festival Hall), wished to know something about the plastic design of steelwork. They would give me the experience I required, and in turn I would tell them what I knew of non-elastic methods of design. After the three vacations I spent with them I was indeed wiser, and was able in due course to be elected to the Institution of Civil Engineers and call myself a Chartered Engineer; I could sign drawings which would be accepted by Local Authorities. Indeed, in later years I made many such designs, providing the structural skeletons for architects' projects.

Consulting engineers in London were, in 1951, largely housed in Victoria Street, just a few steps away from Westminster Abbey, and from Great George Street, the home of the Institution of Civil Engineers. The Institution had meetings on Tuesdays at 5 p.m., and on Tuesdays the consultants' offices would close early, and we all walked down to Great George Street, to have tea and to listen to the presentation of some engineering topic. The paper on which the presentation was based would be published in due course, together with Discussion, in the *Proceedings* of the Institution, and would perhaps describe an advance in theory, but more usually the completion of some major engineering project. The authors were leading engineers of their time, informing colleagues of a new achievement, and the Institution had been having such meetings for over a century. In due course the Civils published half a dozen of my papers on applications of plasticity.

My first summer job with Scott and Wilson was to work with the section involved in the design of Shed 102, Southampton Docks. This was the title under which my paper was later published by the Civils, and it disguises a very large steelwork construction. Shed 102 was the terminal building for the Union Castle Line, from which a passenger ship sailed to Cape Town every Thursday at exactly 12 noon. The passengers would arrive by train from London, and proceed to embarkation through the ground floor of the shed. The floor above was designed to carry heavy cargo, and consisted of a grillage of girders, of which the main members were 6 ft deep and welded from steel plate. The ground floor was to be kept as open as possible, so that the main girders spanned 108 ft, with one internal prop obstructing an otherwise open space. Scott and Wilson had decided to use the new plastic method to proportion the girders, and wished to learn the technique. It turned out that the design could proceed very simply, in marked contrast to conventional elastic calculations.

But first the section leader in the office wished to test my abilities, and he set me a child's problem to determine the size of a steel beam to be used in a flooring system. The calculations were done in thirty seconds or so, and after fifteen minutes I told the section leader that the required member was an 8 by 4 (the size in inches of a standard rolled steel joist). He did not bother to look at my calculations and merely told me that I was wrong, as indeed I was when I found my simple numerical error; he knew from his experience that the section would be either 10 inches or 12 inches deep, and he wished to know which.



Shed 102 Southampton Docks

Things went better later on, especially after I had suggested, with great diffidence but correctly, that there was a flaw in the fundamental arrangement of the steelwork of Shed 102; rigid connexions were needed, but had not been provided, at the tops of the columns. The plastic calculations were straightforward, and I was able to determine the dimensions of the girders of the main structure. These values did not offend the section leader.

The ‘safe theorem’ had not yet been absorbed into my fundamental ways of engineering thought. I had designed steelwork for a structure which would collapse in a certain way – of course with an appropriate factor of safety, which had been chosen to have the value of 2; the loading would have to be doubled for collapse to occur. But was there some other more critical mode of collapse, of which I had not thought? I did indeed spend sleepless nights (not literally) until I realized that my design was one with which the structure would be comfortable, and that therefore there could not possibly be a more dangerous condition.

The plastic design was accepted, was sent out to tender, and an experienced contractor appointed (with whom I worked later on other jobs). Construction started conveniently two years after the design, and I spent my third summer with Scott and Wilson as a junior site engineer at Southampton. The intervening summer had involved three months in Victoria Street, working with another section designing reinforced concrete. The original design for the Festival Hall had been for steel frames, but steel was still not freely available at the time of construction; the calculations had to be redone for reinforced concrete, and this was the way the concert hall had been built in the late 1940s. The experienced architectural eye can detect this change of material in the present Hall; concrete sections are deeper than steel, and some main members slightly betray their presence. I had nothing to contribute to the store of expert knowledge held by Scott and Wilson, and, on the contrary, much to learn – in particular that my understanding of reinforced concrete that I had been taught in Cambridge, while completely correct in theory, was of only remote background service to the jobs in hand.

A year later, at Southampton, one of my duties was to check the delivery of steelwork, which involved the verification of the weights entered on the delivery notes. I had to master (which I never did) the table: 14 lb make a stone, 2 stones make one quarter, 4 quarters one hundredweight, and 20 cwt make one ton – which therefore consists of 2240 lb. At least the U.S. ton is 2000 lb. (Curiously, the metric tonne of 1000 kilograms is very close to the British ton.) But my main task was to oversee the erection of the steelwork, and above all to check the site welding. This was long before the days of health and safety, and even of hard hats, and I was very grateful for the care and kindness with which the steelwork erectors treated me. They knew I would be scared walking in the open on a skeletal framework, and indeed so I was for about a month. After that I could manage to walk the tightrope of the upper flange of a steel girder without too much difficulty.

It was the site welding that was all important; the integrity of the whole structure depended on the quality of the connexions between the members. I had a bulky and difficult to use device to test the soundness of a weld; the echo from a sonic pulse would be distorted if the weld were faulty – if it had an air gap or a slag inclusion. I was suspicious of several such welds, but did not have the confidence to condemn the work. Eventually, however, I thought I was completely sure that some work would have to be redone. The matter was not simple; the contractor was outraged that his work should be questioned, and refused to take me seriously. However the weld in question was finally cut open, and found to contain a very large slag inclusion, caused by hasty work from the welders. After that there were no more problems; I had only to appear on site with my apparatus for more care to be taken.

Construction was completed during my three months in Southampton. This was the first plastic design of such heavy steelwork, and we proof tested the main members by applying the full design loads. For the main girders this involved hanging from cradles some 390 tons of scrap steel, consisting of batches of 30 ft and 45 ft lengths of bull head rails, topped up with submarine net sinkers weighing 1000 or 2000 lb each. The work was very similar to that of the full-scale tests at Abington, but on an enormously greater scale. The weather was appalling, and the labour force was almost mutinous. I would of course have wished to double the loading, which would, according to the plastic calculations, have caused the collapse of Shed 102. As it was, I measured stresses at various points in the girders, using the same ingenious vibrating-wire gauges that Baker had employed some twenty years earlier in his tests on buildings in London for the S.S.R.C. It was this whole enterprise, design and testing, that was presented as a paper to the Civils.

The steel skeleton

In the nine months of each of these three years when I was in Cambridge, I continued to nibble away at smallish problems of plastic design of steelwork, and several papers were accepted for publication. However, I knew that, although the work in the papers was original, no real advances were being made. But much of my effort was devoted to the nine-month course, and I had my first real experience of giving a full course of lectures, in this case to a dozen students. I was, of course, scared of this duty, but I found that, since the class was so small, it was possible after a time to present the material in an interactive way, which would have been anathema to Inglis. It was later, when I found myself a member of the main teaching staff, that I faced the task of lecturing to two or three hundred students, and I began to appreciate the pleasure of making formal presentations as free as possible from flaws.

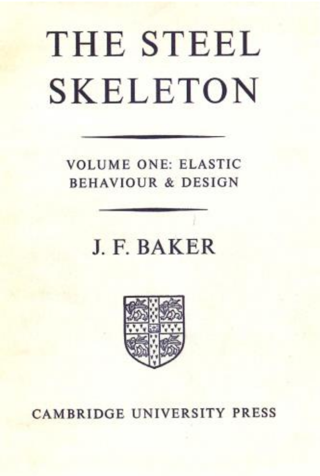
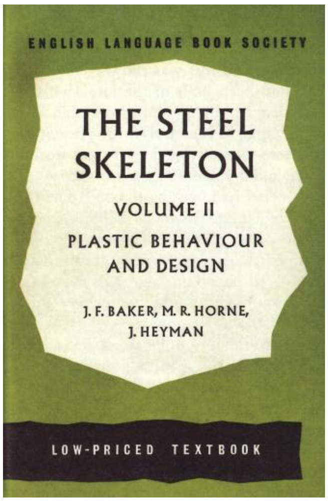


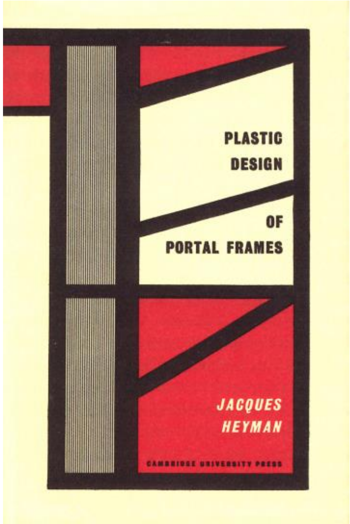
Photo: P. Pattenden



Photos: P. Pattenden

However, some of my energy was directed elsewhere. Baker had taken sabbatical leave in 1952 in order to write his magnum opus, The *Steel Skeleton*. He must have known, as an experienced author, of the intense labour involved in writing a book; nevertheless, he was disappointed when he returned from leave to find that he had written only volume 1: *Elastic behaviour and design*. This book is partly historical, and covers Baker's work with the Steel Structures Research Committee, as well as reviewing the methods current before 1936 for the design of structural steelwork. The load on the Head of a large Department is heavy, and Baker knew that he could not possibly undertake volume 2: *Plastic behaviour and design*. My own experience, when I held the Headship for ten years before my retirement in 1992, amply confirmed that of Baker. The best I could do was to maintain three files for three books that I wished to write some day; I wrote them after my retirement, and indeed a further two.

Baker had drafted the first two chapters of his volume 2, but the book finally had seventeen chapters and two substantial appendices. He asked Horne and myself to co-author the book, which we did, and, as it turned out, without any squabbles. Horne and I each took responsibility for half of the chapters; one would make a draft, to be amended by the other, and an agreed version submitted to Baker for his final comments. Of course we wrote with pen on paper; a typed manuscript, with carbon copies, would be produced on a manual typewriter by Baker's secretary, and there followed the final laborious task of entering by hand the mathematical equations. Cambridge University Press published volume 2 in 1956; volume 1 appeared in 1954. They also published, a year later, a short book of my own on the way to use plastic theory to design simple factory buildings.



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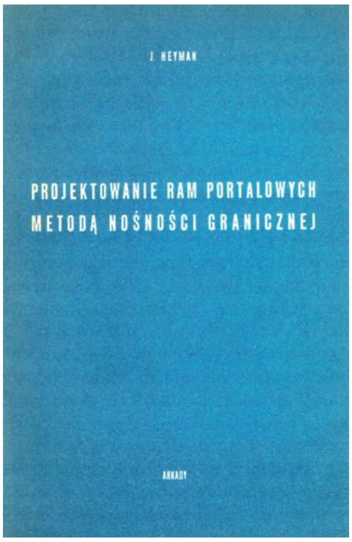


Photo: P. Pattenden

Covent Garden

And it was in 1956 that I made my second visit to Covent Garden to see my second Wagner opera. I had vacated my Bye-Fellowship at Peterhouse in 1951 on my appointment as a University Lecturer; in 1955 Roy Lubbock secured my appointment to a permanent Fellowship in the College, actually renewable every five years – but re-election was automatic. For nearly thirty years Lubbock had been the first and only engineering Fellow in Peterhouse; I was the second. I was not yet married, so that I moved back into College to occupy a bachelor's set of rooms.

A group of four or five of us, including a senior research student, bought tickets for the amphitheatre of the Royal Opera House. My first visit had been in person three years earlier to try and get seats for a sold-out 'Die Meistersinger'. The box office, to their surprise, found that they had indeed two seats in the front row of the Grand Tier, at five guineas each. That sum, which is about the equivalent of today's price, was outrageous for a struggling young academic; well, I can't do them for less than three, said the booking clerk. I took them. The aftermath of World War II had in fact left bargains elsewhere; for example, no restaurant could charge more than five shillings for a meal. The Savoy Hotel, not too far from Covent Garden, charged that sum for dinner in the Grill Room, together with a cover charge of ten shillings, a total that could be contemplated by the young academic.

The seats in the amphitheatre for 'Das Rheingold' were spartan compared with the luxury of the Grand Tier; they were benches with no armrests as divisions. They were also very far from the stage, but the view was completely uninterrupted, and the sound was, and is, magnificent. When, later on, I started to go regularly to Covent Garden, the amphitheatre seats, now no longer benches, were my favourites. As I began to have a little more money, I gradually moved down in the House to the stalls.

Performances of these two Wagner operas were outstanding. With hindsight, my complaint would be that their presentation was without surtitles, which were of course not yet current in international opera houses. Wagner is much given to lengthy narrative, with the score brilliantly orchestrated so that every word is audible and comprehensible; but the words are in German.

Sabbatical leave

Shortly after 'Meistersinger' I had completed six years as a University Lecturer, and I arranged to spend a Sabbatical year back at Brown University with Prager. With his usual skill he arranged for me to be paid without initial funding, in the expectation that someone would be interested in any work that I might complete. He had appreciated, at a very early stage, the power of the computer, and, after discussion, we agreed to work on computer applications to plastic design. This was not a matter of using the number-crunching power of computers, but of the development of logical criteria by which, for example, a steel frame might be designed. I remember a eureka moment when I discovered one of these criteria, and I was waiting at Prager's office at eight o'clock the next morning. He had already been up since five, doing a couple of hours work at home before driving into the University.

I had bought a car soon after I arrived, and on one afternoon I drove through a red light while some fool went through the intersection on a green light. I think perhaps that I was unconscious for a few seconds; when I realised where I was there were already two policemen asking me what had happened. I told them, and once or twice later in the year I would see a patrolman nudging another, saying that I was the guy who said he had gone through a red light. After I left hospital, a couple of these patrolmen took me to a private appearance before a judge, who fined me the tiny sum of ten dollars – mainly, I think, because I had been completely honest.

I was not seriously damaged, although the car was, and it had to be replaced and insured by a different insurance company. I had bruised a kidney, and was under observation for a couple of days. One of the professors at Brown lent me the seven volumes of Proust, and I had read five by the time I was back at work; it took me a further two years to finish the last two. I had read *War and Peace* during the evenings of my three-month residency at Southampton.

I had always known Prager to be a kind man. On the third day he kidnapped me from the hospital, and took me to his home to be looked after for five days by himself and his wife. This was before Christmas, soon after the start of the academic year, and I took pains not to let news reach my mother, who I knew would suffer some distress. I had not appreciated the working of the grapevine of the academic world – it was not long before I had to write reassuring letters back to London, where she was living after the death of my father.

The following summer I took a six-week holiday. My mother came over to Providence, and we set off in my new car (not a patch on the one written off) to make the grand tour, driving from Rhode Island to California by the northern route (there is an awful lot of the Dakotas, where in the heat the car's coolant often boiled) and returning through the southern states to the east coast. We paid the conventional visits, to Niagara, the Grand Canyon, Yellowstone, the redwood forests, and we were not disappointed. Motels were easy to find, and meals were no problem. My mother loved travelling. She had spent an extended honeymoon in 1924 in Germany, at the height of hyperinflation, which my father no doubt combined with business, and, after his death, she found an off-season way of being a passenger in a cruise ship going from England to Australia and New Zealand, calling at exotic places like Ceylon. She had four brothers, two of whom she had not seen since the end of World War I; Cyril had settled in Australia, and Arthur was sheep farming in New Zealand. On our return from Providence to the U.K. she flew back to England, but both of my visits had been by ship; on the first, in 1949, it was not possible to fly the Atlantic commercially. For this last journey I upgraded to first class, which did a little to relieve the tedium of a six-day voyage.

My mother died at the age of ninety-three in my flat in Hampstead. I had bought this two years before my retirement, and used it at the weekends as a base for my attendance at the opera. Covent Garden was about twenty-five minutes away by car, as was St Martin's Lane for the English National Opera, and, with careful timing, there was no difficulty in parking virtually outside the opera houses. Return to Hampstead took about twenty minutes. My mother enjoyed her return to London, in which she had been born and schooled, and from which she had been parted for most of her life, and she enjoyed coming with me occasionally to the theatre, and to concerts and operas. Her health deteriorated slowly; there was some but not excessive dementia. The flat was large enough to accommodate a succession of splendid young girls from Australia, New Zealand, South Africa, who cared for my mother for her last eighteen months. My only complaint was that they stayed for only six or eight weeks, but the agency, without fail, found replacements. District Nurses, who were then operating fully, paid daily visits, and of course the doctor came in. All in all, it was a very easy death.

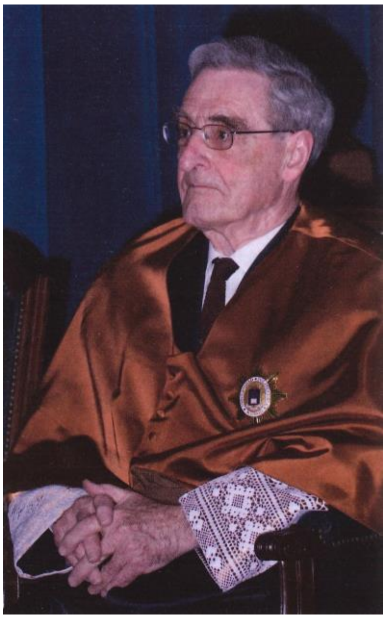
The middle-age syndrome

Back in Cambridge in 1958 I saw the publication of a piece of work which strayed from the path of structural into that of mechanical engineering – indeed, the paper was published by the Institution of Mechanical Engineers. The work was concerned with the design of turbine discs. These discs carry on their rims sets of numerous turbine blades, and for reasons of strength, the discs taper from their thickest at the hub towards the rim. The profile of such a disc was determined by a mixture of design experience coupled with an elastic analysis of the stresses caused by rotation. This analysis was, as usual, highly complex; plastic ideas should be easier to use. The 'philosophy' was the same as that used for steel frames; the disc was not analysed for the stresses it would experience, but would be designed to burst at a factored overspeed. Moreover, since the plastic calculations were so much simpler than the elastic, it became possible to do away with 'experience' in determining disc profiles. The plastic method generated the proper shape for discs without need for guesswork modified by subsequent analysis.

I had started this work before I went to Brown, but had put it aside. I was sure that the mathematics was correct, and that it was in fact elegant, but when I inserted numbers to give real designs, the answers were obviously nonsensical. The problem lay in my ignorance of the facts of mechanical engineering – I had been putting in the wrong numbers. When I corrected this, profiles were generated which, satisfyingly, were not dissimilar to those produced by conventional experience, but which had been obtained directly rather than by trial and error.

The Mechanicals gave me a prize for this paper. I have not received many such awards, although I do have a gold medal from the Civils. Similarly, I cannot complain about my lack of honorary degrees, of which I have two. The first was from the University of Sussex, given quite early on (Baker said: you're starting young, my boy), and appeared to me to be a mistake. The citation at the ceremony seemed to have been written for someone else – I could hardly relate it at all to myself.

The other degree was given by the Polytechnic University of Madrid, and was only the second to be proposed by the old School of Architecture – the first recipient was Felix Candela. It has a flamboyant gown, with faint references to the uniform of a matador, and a splendid hat whose glory may be seen if my name is presented to Google. This connexion with Spain came about much later.



Heyman, Madrid | Photo: J. Heyman

Also on my return from Brown I got married, just before Christmas 1958. Housing in Cambridge at that time, as now, was very expensive, but we managed to scrape together enough to make the down payment on a flat just being built in a block of twelve behind the Evelyn Nursing Home (as it then was. Two of my children were born there). The flats were a new venture by Span Developments, and were designed as affordable for young professional first-time buyers. (Major blocks were built at Blackheath, London, and elsewhere.) Money was very short, and for two years we lived on bare boards, sanded and sealed by myself, until we could afford some minimal floor covering. As children began to arrive the flat became too small, but by chance the one next door became vacant. We bought that and knocked the two flats together, so that for twenty years we lived in an apartment which included two living rooms, a study converted from one of the kitchens, four bedrooms and two bathrooms; this accommodated us, three children, and a succession of au pair girls – some of whom, after their year's stay, became family friends. The au pairs had been preceded by a girl who lived locally, and who came in for a full day. When she left to get married, her mother, Mrs Jackson (and 'Mum' to my children) took over her duties, and continued to come in every day for fifteen years. Newton Road still had gas lamps, and Mr Jackson, as the last lamplighter in Cambridge, came by every day. Most of the au pairs were European (Danish and French), but one was an English girl from the Fens, who was in fact a qualified nanny.

I had resumed my duties for the one-year course on plastic design, and gradually moved into mainstream teaching. At first these lectures were on advanced topics given to small groups of students specializing in their final year in structural engineering. Again, I much liked the opportunity of explaining rather difficult matters to a small class, where questions could be asked. When, however, when I was able to decide which lectures I should give, rather than be a member of a team covering a multitude of topics, I chose to give formal ('Inglis') lectures to 200-300 students. Finally, in the last years before I retired, I monopolised the teaching of structural engineering to the first-year class.

And I continued to write on plastic problems, although I became more and more dissatisfied with what I was doing. I knew that the really basic problems of the new technique had been solved, and the backing theory had been established. I was making new, but really very small advances in my published papers. The old work was being recycled, with tiny technical additions. This is, of course, a perennial problem when working in an established discipline; there is satisfaction to be found in exercising one's expertise in creating something new, but it is only a marginal satisfaction.

Intermezzo con variazioni

A parallel may be found in the development of music in the sixteenth century. Writing for a five-part choir is a difficult technique to be mastered, and William Byrd had composed Masses for four and five voices which are technical and artistic masterpieces. However, the Italian Striggio had composed a motet for forty voices, and this was a challenge not to be refused by Thomas Tallis. His 'Spem in alium' is for eight choirs of five voices each. All forty singers are singing the same words, but not in unison – the phrases have different lengths, as well as different harmonies and rhythms. The result is a gorgeous wall of sound, in which the words are totally indistinguishable. There are three chinks in the wall; it must happen, by chance (but certainly by design), that all eight choirs will draw breath at the same time. This happens about half way through the twenty-minute motet, and again towards the end; the effect of the resumption of singing after a split second of silence can be overwhelming. But in all this Tallis is not really advancing his art – he is showing superlative skill in refashioning an accepted musical form.

There was a strong proposal in the Council of Trent, held between 1545 and 1563, to stop composers doing this sort of thing. The decoration of the words of the Mass with ever more elaborate counterpoint made the words unintelligible compared with earlier plainsong chant. The Pope proposed the banning of all such polyphony, and a return to earlier forms of the sung Mass. However Palestrina was given a chance to compose a new work which would satisfy both the musicians and the Church.

In Hans Pfitzner's opera 'Palestrina', his young pupil, the seventeen-year old Silla, is not happy with what he has been taught, which seems to him to be stale and unimaginative. By the end of the opera he has escaped to Florence, where he hopes that something new might be found. In the meantime Palestrina composes his Mass, on stage in what passes for a single night, and with a sort of automatic writing, which perhaps symbolizes the fixed form of the musical rules being used. The Mass was accepted by the Pope.

It was not in Florence but in Venice, a few years later, that Silla would have found what he was looking for. Monteverdi made a seemingly simple innovation that totally changed the face of sung music. His early madrigals were for solo voices, perhaps five or six (and of course doubled or trebled if necessary), singing together, and with the voices providing the words, the tunes and the harmonies - all this done with enormous skill. Then Monteverdi added a backing group, the continuo, consisting of harpsichord, cello and theorbo. This group could look after melody, rhythm and harmony, leaving the voices free to articulate the words; indeed, a solo voice could tell the story. Opera was born.

Variation 1

The Italians call the theorbo by the name *chittarone*, big guitar. The suffix *-one* magnifies the name; *minestra* is soup, *minestrone* is big soup. The basic member of the string family is the viola, able to cover most of the range of the human sung voice. Musicians found they needed a higher pitched instrument, and so the violin was devised – *violino*, the small viola. They also needed a lower pitched instrument, and this led to the *violone*, the big viola, now obsolete and replaced by the double bass. But this in turn was too low in pitch, and a smaller instrument was developed, the *violoncello*, a small big viola. For over two centuries the standard string section of a symphony orchestra has consisted of a choir of five voices – first and second violins, violas, cellos and double basses.

Variation 2

All these instruments have four strings, and those of the violin, viola and cello are tuned in fifths. The violin has strings sounding G, D, A and E; the viola has C, G, D and A, and the cello the same but an octave lower. The characteristic of the interval of the fifth is that the upper note vibrates 3 times to 2 of the lower, so that every second beat of the lower note is afforced by a beat of the upper – the result is pleasing to the ear. On the piano the white notes F, C, G, D, A, E, B have these intervals of a fifth; if the series is continued the five black notes, F♯, C♯, A♭, E♭, B♭ are reached, until finally the sequence returns to the base note (the tonic) F, but seven octaves above the starting F.

Or rather, it does not quite return. In the sequence the pitch is raised twelve times by the factor 1.5, and 1.5 to the power 12 is approximately 129.7 ... From the base note F the F above vibrates at twice the frequency, and 2 to the power 7 is equal to 128. If the viola were tuned, not in perfect fifths, but with strings which vibrated with ratios of about 1.498 instead of 1.5, then perfect octaves would be recovered, and this gives the notion of equal temperament, in which the laws of physics are violated by a tiny amount, normally unnoticeable to the ear. Bach's Preludes and Fugues showed that a well-tempered clavier could play in seeming tune no matter which base note was chosen.

The Second Viennese School rejected this musical heritage, and at the same time, unwittingly, ignored the laws of physics. Schönberg's 12-tone serial scale gives equal importance to every note; any note can be followed or combined with any other, ignoring the fact that, because of their physical frequencies, some combinations sound well and some do not. His pupils Berg and Webern adopted this new approach to music. Webern in particular was totally dismissive of conventional structures. If, according to accepted ways of composing, the sounding of two or three notes would be followed, automatically and predictably, by a sequence of other notes (returning to the tonic, for example), then Webern omits that sequence. It makes his music difficult to follow and to understand.

Schönberg himself suffered from triskaidekaphobia, to the extent that he deliberately misspelt Aaron in his opera ‘Moses und Aron’, so that the title should have only twelve letters rather than the dreaded thirteen. His malady perhaps reinforced his fixation on the twelve-tone system.

Variation 3

Labelling the white notes on the piano C, D, E and so on is a matter of convention. Playing the white notes in sequence from a base note C gives the major diatonic scale, in which the intervals between notes are a tone, tone, semitone, tone, tone, tone, semitone, but of course major diatonic scales with these intervals can be constructed starting from any note, inserting black notes as needed.

A ring of eight bells in a largish church is usually tuned to such a major diatonic scale, doh ray mi… A few very large churches have rings of twelve bells, in which the sequence of eight, C, D...C, is supplemented by four more bells sounding D, E, F, G. Such a large number of bells gives rise to an enormous number of combinations in change ringing; the number is already high for a ring of eight. Eight bell-ringers can of course find themselves in a church with twelve bells, in which case they will ring only eight of the twelve, normally the lighter top end, starting at G of the basic eight and ending on G of the added four. This leads to a musical difficulty if the diatonic scale is to be preserved; the required tone and semitone intervals can only be achieved if the penultimate F is replaced by a bell sounding F♯. Such a bell is often provided, so that the four supplementary bells in fact number five, and a ring of twelve is in fact a ring of thirteen, with the top F sounded if all twelve are rung, but F♯ sounded if only the top eight are rung. Schönberg would be appalled.

Variation 4

The usual kind of bell-frame installed in a church tower ensures that a bell is rung either in the north/south or in the east/west direction; the precise arrangement of the bell pits is influenced by the number of bells and the size of tower. In change ringing, in which each bell is swung through a full circle of just over 360°, the forces exerted on the tower can be considerable – as a rough guide, a bell will exert a sideways force of about three times its own weight. In the year 2000 the uncompleted tower of Brisbane Cathedral in New South Wales lacked bells, and I was asked whether the central tower could be completed, and whether it would be safe to install a ring of twelve.

John Loughborough Pearson, the architect of Truro Cathedral, had designed several churches in the southern hemisphere; his very large parish church of St Matthew's in Auckland is particularly fine. His plans for Brisbane were approved at the end of the nineteenth century, and construction began in 1906 under the direction of Pearson's son. In true Gothic tradition, construction had not been completed a century later, and I paid two visits to advise not only on the tower, but also on defects which had developed in parts of the fabric constructed a century earlier in the first building campaign. These defects, which involved cracking of the high vaults, were to be expected, and were essentially benign.

Pearson was a master of medieval styles of construction. Brisbane Cathedral is very large – if you are in the Lady Chapel, you cannot see the south transept. This allowed Pearson to build in different styles in different parts of the cathedral – Romanesque, early and late Gothic, quadripartite and sexpartite vaults – all perfectly executed. Brisbane Cathedral is one of the most beautiful ‘medieval’ churches that I know.

I had done a little mathematics using simple tools reminiscent of Inglis's analysis of a car running over a ridged road surface. If we could establish the basic natural period of vibration of the tower, and if we could estimate the total mass that would be vibrating, then a one-line calculation would establish the stiffness of the tower as a whole. The calculations would be rough, but if a force of 3 tonnes exerted by the ringing of a 1-tonne bell caused a calculated deflexion of 2mm, then the bells would go easily; if the deflexion were 20mm, then the ringers would have trouble. A colleague from Cambridge, an Australian on sabbatical leave, happened to be in Brisbane, and he organized a gang of ten tough workmen to heave sideways on a rope dangling from the top of the tower. The resulting oscillation of the tower was very small, but the frequency was determined very accurately. The mass of masonry was quickly estimated. It would be perfectly possible to install bells.

The tower was completed by adding a further few metres to its height to house a bell chamber, with a ringing chamber below. In the event there was no bell frame; the bells were bolted directly to a new concrete slab forming the floor of the bell chamber, and they were installed not north/south and east/west, but radially, so that they delivered their forces to the centre of the tower.

Treading water

Richard Weck had built up an impressive research unit investigating problems of metal fatigue. In one of the laboratories of the Engineering Department very large machines operated twenty-four hours a day imposing literally millions of cycles of loading on test specimens. Weck was about to leave the University for the Welding Institute at Little Abington, where indeed he had designed and built the house at the entrance to the drive to Abington Hall, for which I had provided a slender and almost invisible steel skeleton to create a large open ground-floor space (and Tony Bartl had designed the mosaic by the front door). He tried to persuade me to take over the fatigue enterprise, but, despite my disenchantment with my own research, I could not envisage any sort of intellectual challenge that the work might involve.

Circumstances intervened, and I consented to be driven along a different path. The Senior Bursar of Peterhouse was Joe Sanders, a University Lecturer and distinguished New Testament scholar who was also Dean of the College, responsible for the College Chapel. He was parish priest for a church a few miles from Cambridge, and he was a family man. He was a pluralist if there ever was one, and he was very pleasant company; we used to play bridge together, in College or in his parish. At the turn of the year 1961/62 he died suddenly. I had thought that my views in general, and actions in particular, had not endeared me to the College; I was very surprised when the Master, (Sir) Herbert Butterfield, telephoned me at home very shortly after Christmas Day (there was snow on the ground) and summoned me to the Lodge, where he offered me the job of Senior Bursar. Strictly, the appointment lay in the hands of the Fellows meeting as the Governing Body, but it is one of the many unwritten chores of a Head of House to make proposals which would be acceptable to the Fellows. I accepted at once; it would give me something to do, if only in an amateur way.

My acceptance did not threaten my marriage, but it was hated by my wife. There was much love in the family, but we could not agree on two matters; she could not stand Wagner, and the college system in Cambridge was total anathema. This anti-establishment view dated no doubt from her communist childhood in pre-war Paris, where at the age of twelve she was selling *l'Huma* in the streets. But it was also coloured by her perception of the way she, and women in general, were treated in Cambridge. She was a good immunologist, well qualified for a university post, but she had to survive on a succession of three-year research contracts, moving between different laboratories. It is perhaps no surprise that our political views were at opposite poles, but we both believed firmly that it was our duty to vote in elections. On polling day we would walk hand in hand to the polling station and fill in our papers, knowing that our votes would cancel each other out.

Eventually, the perfect permanent appointment for her research arose in London in the laboratories of the Royal Marsden Hospital. The work was intensely satisfying for her, and it involved a complete upheaval in our lives. We bought a terrace house in Islington (in which she died) when my younger daughters were still at school in Cambridge; they lived with me during term time, and mainly in London with their mother during school holidays. And of course we were always united at weekends, in Cambridge or London. These arrangements sound bizarre, but in fact they worked extremely well, and the weekly reunions were indeed exciting for all of us.

The house was only a few minutes by car from the Coliseum in St Martin's Lane, and very often on a Saturday night we would all go to the English National Opera, where the purchase of the cheapest seats in the Balcony qualified us as children of the gods. Access to the Balcony was at that time (now no longer) down a side alley, and it was several years before my daughters discovered the main entrance to the theatre. (Covent Garden had the same separation between the Amphitheatre and the expensive areas of the House.)

After my wife's death I kept the Islington house for a few years, but then moved a few miles to a spacious flat that I bought in Hampstead.

College and Department

So in 1962 I joined the ranks of amateur bursars – Trinity, St John's and Gonville and Caius, all wealthy colleges, had the three professionals. I sought advice from the College's stockbrokers on matters concerned with investments in stocks and shares (we were severely restricted at that time by rules laid down by the Ministry of Agriculture and Fisheries), and from the College's land agents with regard to the substantial number of farms as well as commercial and residential property owned by Peterhouse. It would be more true to say that, instead of seeking, I received active advice. I had few grounds for rejecting such advice, although it is part of the Cambridge ethos (and, come to that, of parliamentary ethos), that an amateur can make judgements which are unclouded compared with those made by better informed experts. In any case, we changed our professional advisers from time to time.

The Bursary was on the first floor of an eighteenth-century block erected in one of the College's, usually short lived, attempts to add to its amenities. There was a large main room, serving as an office and reception area, and also a delicious small room, beautifully decorated, and irresistibly compelling for academic thought. I did not resist, and spent my first few weeks writing a small book concerned with the elastic, as well as plastic, analysis and design of the type of skeletal structure with which I had been concerned. The book had been commissioned by Robert Maxwell, as had a huge number of others from a multitude of authors. His money-spinning technique was, in its own way, as foolproof as Prager's – he did not mind if some of the commissions were flops; they had been filtered through a consultant editor, and some would be best-sellers. My own sold in the middle range, and it was translated into Spanish.

This took me a couple of months, before bursarial work really started. The work was not heavy, and not tedious. I knew how to read accounts, and I had been forced to understand some financial matters after my father's death. (I still keep track of my own domestic expenditure by means of double-entry book keeping.) I had no problem with the firm of Chartered Accountants who visited annually after mid-summer to prepare the financial statements required by the University. And I enjoyed learning, in no doubt a very superficial way, about farming. Several times a year the College's estate agents took me with them, for a day or so at a time, to visit College property, mostly in East Anglia, but also as far distant as Devon. Once a year many of the tenant farmers made a point of attending the Audit Lunch, arriving in their Rolls Royces to hand over in person a cheque for the rent of their land.

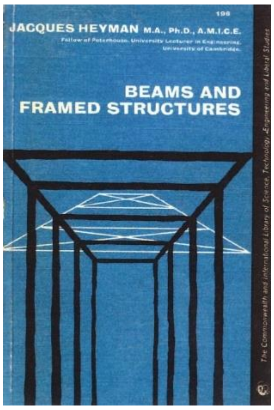


Photo: P. Pattenden

Photo: P. Pattenden

I wrote only one paper during my three-year (as it turned out) tenure as Bursar. When Baker came back to Cambridge in 1943 he set about a total reform of all aspects of the teaching of engineering in his Department. The ‘mandarin’ syllabus was revised, and with the revision came the opportunity to escape the inbreeding of the teaching staff, and to appoint lecturers who had graduated from other universities, although it was to be many years before the University of Cambridge would allow such a graduate to be addressed by the title of Doctor. Above all, and in the face of the severe post-war shortages, Baker embarked on a major redevelopment of the engineering site. The first fruit of this programme was the very large six-storey building, housing nearly all the teaching staff, new laboratories, common rooms, administrative and secretarial offices, and study rooms for research students. This building, now called Baker Building, was opened by the Duke of Edinburgh in 1952, an occasion commemorated in the very busy oil painting by Terence Cuneo. The building was, within ten years, already in need of extension, and I was concerned with the plastic structural design of two of the three added wings.

The South wing had been completed in 1958 by a conventional elastic design, and housed facilities for the Professor of Aeronautical Engineering, including a wind tunnel rising through two floors. Two years earlier a four-storey centre wing had been constructed. The steel framing for this was very simple, but it was probably the first multi-storey building to be designed by plastic methods; moreover it was site welded, although, despite Shed 102 and the Fatigue Laboratory at Abington, this was still not common at the time. Michael Horne and I did the calculations, and supervised the erection of the steelwork.

The much more substantial North Wing was completed in 1964, and for this design Roger Johnson and I worked in conjunction with a large London firm of consulting engineers, R.T. James and partners. (I had no idea at the time, and would really have had no interest, that R.T. James was consulting engineer to the Dean and Chapter of Westminster Abbey.) Johnson was a specialist in the design and behaviour of reinforced concrete, and the North wing was not only designed plastically, but was a composite design, in which the concrete floor slabs also acted as top flanges for the supporting steelwork. It was this co-authored work which was presented to the Civils in 1965.

Oxford

Before this, in the summer of 1964, I at last achieved, at the age of 39, my appointment as a professor, long after my colleagues, Neal and Horne, had gone to other universities. The steel industry had established a new, second, professorship at the University of Oxford, and Baker, whose influence was paramount both in the world of steel and the world of academe, had secured my appointment to the new chair. I went over to Oxford to discuss my (modest) needs, which were for a simple new extension to the Engineering Laboratories to house experimental work on steel structures, together with funds necessary to run the new facility. My requests were agreed by the Professor, the Head of Department. During the next few weeks he became increasingly evasive about his negotiations with the University, citing difficulties with the Vice-Chancellor. Eventually, in September, I sought an interview with the V.-C., and it turned out that he had not been presented with my plans, and that there was no money to satisfy them. I resigned on the spot.

I had also to resign from Keble College. A Professorship in Oxford is often tied to a Fellowship in a specific College, and I had been duly elected at Keble. I was looking forward to being associated with a college having such splendid polychrome brick buildings, created by the Pre-Raphaelite Butterfield – my tastes then, and now, were not shared by many. Pevsner, for example, found Butterfield's buildings actively ugly. The College is not rich, but it does possess Holman Hunt's painting The *Light of the World*.

I had, in the summer, written to the Secretary-General of the University of Cambridge (one of the three great Officers, the other two being the Registrary and the Treasurer), tendering my resignation as a University Lecturer. At the 1 October, then, I had no job in either Oxford or Cambridge. There was a splendid Secretary of the Faculty Board of Engineering, a Lecturer who, having given up research, instead of becoming a College Bursar, administered the academic activities of the Department. In a way that I hope might still be possible today, Stephen Harris went to the Old Schools and explained the situation to the Secretary-General, who, having listened, took my letter of resignation from his filing cabinet, and tore it up. This was twenty years before George Orwell's *1984*, but I was still, therefore, a University Lecturer, having never resigned.

Unfortunately, my ‘vacant’ post had already been advertised, and an appointment made. In a way, again, that might not be possible today, the University agreed to treat me and to pay me as if nothing had happened; a few weeks later a post became vacant elsewhere, and the funding was regularised.

I had also vacated the College Bursarship. Peterhouse moved swiftly, and within a very short time had persuaded a Fellow, (Sir) Tony Wrigley, to take the office. He was a demographer of distinction, who later became Professor of Economic History, Master of a Cambridge College, a Fellow of Oxford's All Soul's, and President of the British Academy.

I was not sorry to lose this College Office, since it was at this time that I at last found a different application for plastic theory, a discovery that was to totally change my life.

Masonry

I was about eleven or twelve when I saw Gloucester Cathedral during a week spent at a Boy Scouts' summer camp. I think I had always been astonished by large churches and cathedrals, but in a very superficial way, not thinking at all about their design and construction. So it was for some not very definite reason that I decided at the age of forty to find out something about the architecture and engineering of masonry. I found it very difficult to make a start. The year 1964 was well before the days of Google, and I was not used to the mechanics of library searches. It took a long time, but eventually I found a book by John Fitchen: *The Construction of Gothic Cathedrals*, published in 1961 by Oxford University Press; the book has the subtitle: *A Study of medieval Vault Erection*. Fitchen was an American professor of architecture at Colgate University in Hamilton, upper New York State. The book is of large format, has over 300 pages with lavish illustrations, and is a work of major scholarship. Very small criticisms can be made, but, apart from the enormous amount of information and explanation in the text, the exhaustive twenty-page bibliography gave me the encouragement to begin to learn about masonry.

There was, for example, a reference to Paul Frankl, a Czech architect and architectural historian, who had arrived in the U.S. in 1938, and eventually settled in the Institute of Advanced Study at Princeton. That university published his massive historical work *The Gothic* in 1960, a year before Fitchen. Frankl died in 1962. His book deals with topics not covered by Fitchen – for example, the greatly important expertise held in Milan in 1399-1400 shortly after the start of the building of the cathedral. Between them these two books gave me some of the keys to the doors that I wished to unlock.

Above all, Fitchen talks of the work of Viollet-le-Duc, the nineteenth century French architect who became responsible for major restoration and rebuilding projects – for example Notre Dame in Paris and the whole of the town of Carcassonne. In addition to his practical involvement with construction, he wrote an extraordinary number of books, most of which are in the University Library at Cambridge. Many are now on my own shelves, but in those days (and not now) nineteenth-century books in the Library were on open stacks, and could be borrowed. So I took home, one by one, each of the ten volumes of the *Dictionnaire raisonné de l'architecture française du XI^e^ au XVI^e^ Siècle* (strictly nine volumes, the tenth being an elaborate index). There are well over 5000 pages in the nine volumes, and almost every page has an elaborate illustration engraved from Viollet's drawings; I read them, avidly, in bed. Some articles are longer than others in this alphabetical dictionary; ‘construction’ is in an early volume, and has been reprinted as a self-standing book. But the articles are varied; the entry for ‘*échelle*’ for example is short, and the fundamental message is that ‘the scale of the dog kennel is the dog’. Others are seemingly discursive, like the article on pigeon houses. In some typical pages Viollet sets the discussion in a historical context – only nobles were allowed to keep pigeons, which were an important source of food. The larger houses were cylindrical towers of masonry, containing inside up to two thousand nesting holes for the birds; a revolving internal timber gantry allowed the pigeon master to inspect the nests. The whole point of these elaborate constructions is that a pigeon does not leave its nest until it is fully grown, and it was the master's task to wring a bird's neck a day or so before it flew, so that its muscles were tender and not toughened by flight. In the U.S. these birds are called squabs.

These are trivia, which I found absorbing, but of course I read the books for their information on masonry construction. I am ashamed to say that I had never heard of Viollet-le-Duc, who has not been surpassed as a French architectural historian. His greatness was recognised at the time and he undertook massive enterprises for the State, but at the same time he was not an Establishment figure. He had refused to study at the *École des Beaux-Arts*, and in return that school held him at arm's length. This is reminiscent of the slightly earlier treatment in Paris of Berlioz. Everyone knew that if a state funeral had to be organised, and appropriate new music to be written, or the chorus at the Opéra to be trained, then there was only one man for the job. The Establishment at the *Conservatoire*, Cherubini and later Auber, refused to allow their concert hall to be used by Berlioz, just as the *Opéra* would not produce *Les Troyens*, which in fact Berlioz never heard performed completely.

Viollet had an astonishing understanding of the action of a masonry structure as a whole, and of its elements, such as high stone vaults and flying buttresses. This understanding is that of an architect; Viollet does almost no ‘engineering’ calculations. Fitchen led me to the works of some nineteenth century German writers who had made such calculations; for example Ungewitter published extensive tables from which it is possible to calculate the outward thrust generated by a masonry vault, and so to design the flying buttresses to contain that thrust.

As another aspect of masonry construction, French engineers had made great advances in the analysis and design of arches – bridges constructed from wedge-shaped voussoirs, assembled on (timber) centering which could be removed once the keystone was in place and the arch complete. Some of these bridges are very large, and indeed a definitive ‘catalogue’ published early in the twentieth century lists only those arches which have spans of more than forty metres. Eight bridges are noted for the U.K., and include two over the Thames, at Putney and Kew. Of the rest, one of the largest, at over forty-five metres, is by Telford at Over, which by his own account was based on Perronet's earlier design for Neuilly, just outside Paris.

From about 1700, and throughout the eighteenth century, the *Académie* offered prizes for the determination of the thrust generated by a masonry arch, so that the abutments could be designed, and for the best shape of the arch and the required depth of the voussoirs. The problems were attacked by a mixture of experiment – models were made – and the application of mechanical laws of statical equilibrium.

With hindsight, it is these laws which are the proper tools for the analysis of masonry, and which are basic to structural engineering, no matter what material (steel, aluminium, concrete, timber) is used. With their use efficient and accurate designs could be made for the large span masonry bridges constructed in the nineteenth century. All this was polluted in 1826 by Navier, who introduced equations of elasticity into structural analysis.

Navier had observed, correctly, that it was not possible to calculate the response of a structure to external loading by the use of equilibrium equations alone – there were not enough equations. He therefore introduced extra information – for the four-legged stool, a ‘Navier’ solution requires the elastic properties of the legs, and possibly the flexibility of the seat (for a plastic stool). He had not noted, and 200 years later it may still not be considered, that a ‘Navier’ solution is for a model of a structure that in many cases does not represent reality – in the case of the stool, any one of the legs, unknowable to the analyst, may be off the floor. The German experiments that came to Baker's attention in 1936 showed that, at least for continuous steel beams, their collapse loads were totally independent of such trivial imperfections – that is, of the way a structure was actually supported by the environment. It was actually a big step to apply these observations, and above all, ways of thought, to the analysis of masonry.

Drucker, head of the Engineering Division of Brown University, and a close colleague of Prager, made this step. He suggested to one of his Ph.D. students that the simple voussoir arch might repay attention in the light of the plastic theorems which had been enunciated and proved. Unlike the British Ph.D., which involves a three-year research project, the American degree consists largely of examined coursework, with perhaps less than a year spent on research. Kooharian, within the year, established that plastic ideas could indeed illuminate the behaviour of masonry. His paper was published in 1953.

My experimental work on miniature steel frames, and, later, the full-scale tests, were concerned with the maximum loads a structure could carry. Collapse of these frames, as the applied loading was steadily increased, occurred by the generation of so-called ‘plastic hinges’, which formed by local ductile yielding of the steel. For a simple rectangular frame, three or four such hinges would transform an originally viable structure into a mechanism capable of large deformations. By the master theorem, this was an ‘unsafe’ approach to the analysis of structures, but since my frames were so simple, the experiments provided unique values of the collapse loads.

Kooharian found a similar transformation of a masonry voussoir arch into a mechanism of collapse; at the maximum load, hinges would form in the extrados (the upper surface of the arch) by contact between voussoirs only just being maintained at a point in the surface. Similarly, hinges could form in the intrados, and the arrangement of such hinges was identical with that of the steel frames; a few hinges would transform the viable masonry arch into a mechanism of collapse.

This approach was in direct contrast to earlier exhaustive nineteenth-century French work, which had been concerned with the statics of the arch and with ways of determining ‘comfortable’ states – in short, with the unwitting application of the safe theorem.

Mediterranean codes

These ways of thought run counter to those of a modern structural engineer. There were, for example, no calculations leading to the values of stresses, which are essential parameters in steel or concrete design. Although the collapse of masonry arches indicated that plastic theory, developed for steel frames, could give a new view of the action of masonry, the basic theorems had not in fact been ‘translated’ from the one material to the other.

The question of stresses was resolved by observation of the structures under discussion, both cathedrals and large span bridges. Astonishingly to the modern engineer, the stresses in these structures are extraordinarily low compared with the capacity of the material. The four crossing piers carrying a 10,000-tonne tower in a major cathedral are stressed to less than ten per cent of the capacity of the material; important structural elements, such as flying buttresses, are stressed to perhaps one hundredth of that limit, and high stone vaults to one thousandth. The hinges which can develop in a masonry arch do not involve fracture of the stone, but are caused by very small movements in the interfaces between the voussoirs, which are filled with either no mortar or with very weak material.

Two formal assumptions can then be made which make it legitimate to use plastic theory to explain the behaviour of masonry. First, the stresses in compression, as in a column in a cathedral, are so low that there is no question of crushing of the material. This assumption is, of course, questionable – it implies that stress levels are so low that the material is effectively infinitely strong. By contrast, the observation of hinges leads to the idea that masonry does not depend on its tensile strength; individual stones may be strong in tension, but two adjacent stones can be freely pulled apart. A key postulate, then, stated in slightly incomplete terms, is that masonry is a ‘unilateral’ material, unable to take tension but infinitely strong in compression. This is a view of masonry which admits the validity of the basic plastic theorems.

Ancient and medieval builders knew nothing of this, but one cannot build without rules, and Frankl's historical work provides another essential clue. An early reference may be found in the book of Ezekiel, about 600 B.C. Chapters 40, 41 and 42 record over several pages the sizes of gateways, courts, vestibules, cells, pilasters and so on, for the construction of a great temple. Part of a building manual seems to have been incorporated in the Old Testament. From Ezekiel 40, 3 and 5: 'I saw a man holding a cord of linen thread and a measuring rod...The length of the rod was...six cubits, reckoning by the long cubit which was one cubit and a hand's breadth.'

The dimensions which are given in the manual are in cubits and palms. The Hebrew cubit, about 17.7 inches or 450 mm, the length of the forearm from the tip of the middle finger to the elbow, was divided into six palms; the 'royal' cubit – a cubit and a palm - was therefore about 20.7 inches or 525 mm, very close to the Greek standard of seven palms. What the master builder – the architect – was holding was the 'reat measure', without which work could not proceed on an ancient (or medieval) building site. This particular measure was six cubits in length (somewhat over three metres), marked with a subdivision of palms and further subdivided into fingers; it could be used to establish major dimensions of the work as well as smaller individual dimensions, merely by using the numbers listed so diligently in the books of Ezekiel. When such numbers had been recorded, they could be transferred to the site by means of the great measure.

The great measure, the rod, was a convenient practical tool. The British measures, learned by school children in the U.K. even after World War II, were

(3 barley corns = 1 inch)

12 inches = 1 foot

3 feet = 1 yard

5½ yards = 1 rod, pole or perch

40 rods = 1 furlong

8 furlongs = 1 mile

Barley corns had perhaps disappeared as units by the time of World War I – and the seventeenth-century subdivision of the inch into twelve lines (*lignes* in France) had been overtaken for scientific work by metric standards. As noted in Ezekiel, the rod could be used for establishing large leading dimensions (as, in England, 4 rods = 1 chain = 1 cricket pitch), and also subdivided for detailed work. The essential feature of the great measure on a building site was that it was part of the building. It was not an absolute 'yardstick'; if it were cut slightly smaller, then a slightly smaller building would result from the same building plan. The same arguments could apply to a sculptor creating a human figure. The figure can be made to any scale, but, whatever size is chosen, the ratio of any part of the sculpture, say the head, to any other part, say the hand, will be the same. Once the dimension of a single component of the statue has been fixed – the foot, for example – all other components can be expressed in terms of that foot. The foot is the unit of measure.

The essential clue provided by Frankl is, then, that design of ancient buildings proceeded by the application of numerical rules, and these in turn led to geometrical shapes which had no reference to the modern engineer's 'stresses and strains'. The 'translation' of plastic theory from steel to stone requires the examination of the pattern of forces within a masonry structure, and the geometry of this pattern must conform to the geometry of the structure as built. This geometry was generated by the great measure and the associated rules of proportion.

Vitruvius (c. 30 B.C.), writing five centuries after Ezekiel, makes *ordinatio* the first of his six principles of the theory of architecture, and it becomes clear that ordinatio is nothing other than the great measure, the rod made up of modules (*quantitas* in Vitruvius) by which the whole building may be measured and constructed. He then develops his theory of proportions in terms of modules taken from the great measure. For example, if the diameter of a column in an aerostyle temple has a diameter of one unit, then the intercolumniation, the distance between the two central columns, should be four units, and the height of the columns should be eight units. Further, for a column of the Ionic order, the height of both the base and the capital should be half a unit, and there are further and more subtle subdivisions for finer details of the work. The way the ground plan should be laid out echoes Ezekiel. Vitruvius was not lost in the 'Dark Ages' – on the contrary, his manual was read throughout the medieval period, and was copied again and again for use in monastic schools, and above all, in the masonic lodges. The hand of Vitruvius is clearly visible in the fragments of Gothic lodge books that have survived, for example in the sketchbook of Villard de Honnecourt, compiled about 1235.

There is a curious mathematical problem associated with the great measure. The measure, subdivided into palms, and further divided as finely as one pleases, should (it might be thought) provide lengths which can be transferred on the building site to elements of any conceivable dimension. This is not so, and the fact was recognized by Greek mathematicians. The irrational numbers, of which the square root of 2 is an example from an infinity of such numbers, cannot be so measured – there is no mark that can be made on the great measure to represent √2, but both Vitruvius and Villard show how such a length can be constructed. The matter, viewed through present-day eyes, is of no practical consequence, but it is intellectually fascinating. Pythagoras's simple and beautiful proof of the irrationality of √2 has survived over two thousand years.

The stone skeleton

So I had two ways into the study of masonry. Technically, it was clear that a structural analysis could be made within the precepts of plastic theory. Architecturally, I had access to a wealth of information about the feats of structural engineering achieved by ancient and medieval builders. I put all this together, and in 1966 I published a long, indeed over long, paper on the action of structural masonry. The paper gave a broad, and necessarily shallow, view of, for example, Gothic quadripartite vaults and flying buttresses, Byzantine domes, and the overall structural action of a great cathedral. General principles were also flamboyantly stated – that the cracked state is in fact the natural state of masonry, and that cracks indicate merely that the building has acted in a way to find a comfortable state in response to small unpredictable movements imposed by the environment (which, anthropomorphically, I came to call a hostile environment). Two statements stemmed from the plastic safe theorem:

(a) If, on striking the centering for a flying buttress, that buttress stands for 5 minutes, then it will stand for 500 years.

(b) If the foundations of a masonry structure are liable to small movements, such movements will never, of themselves, promote collapse of the structure.

In both cases, of course, the structure has found a comfortable state of equilibrium.

The paper was called 'The Stone Skeleton', unashamedly echoing the title of Baker's *The Steel Skeleton*, of which I had co-authored the second volume. Not that Baker had a prescriptive right to the word 'skeleton'; it was in common use to describe a structural frame. Unfortunately, I was so taken with the resonances of the word that one of my books, written after my retirement and published in 1995, collecting together much of the work I did in the intervening twenty-five years, was also called *The Stone Skeleton*. This is perhaps my most highly regarded book.

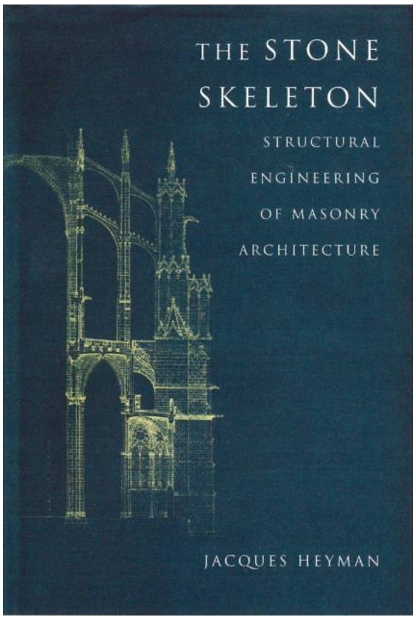


Photo: P. Pattenden

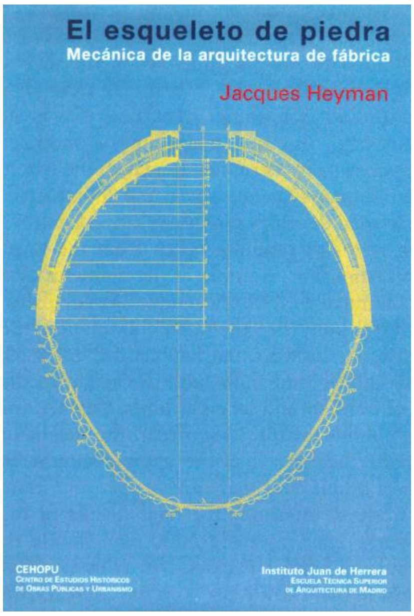


Photo: P. Pattenden

Just after the paper was published I received an unexpected invitation from Harvard to spend a Term as Visiting Professor. At Brown University in 1957/58 I had given lectures in a course of structural engineering – three lectures a week for thirteen weeks, and an immense education for me in the organization and presentation of a technical subject. One of the students in the class had wound up at Harvard in the Graduate School of Design – that is, the architecture school. He had read The Stone Skeleton, and invited me, at short notice, to come for a term to give a course of lectures. I was due for sabbatical leave, but it was only about two weeks before Christmas that I completed all the formalities to fly to Cambridge Mass in early January.

The pay at Harvard was on a scale totally new to me. I stayed for three months, with my wife and three daughters with ages ranging from two to fourteen, together with our English nanny, rented a very comfortable house, bought a car, and returned with enough money to buy a piano. We had never had a piano in my childhood house in Coulsdon, something I have always regretted. My daughters never played with distinction, but they did achieve some proficiency; above all, they enjoyed the music. The piano has now been played by grandchildren. The nanny, the extremely competent girl from the Fens, was so smitten by the experience that within a year or two of our return she had emigrated to the U.S. to pursue a lucrative career of looking after children.

As for the lectures, they went down well enough, and were designed according to the brief I had been given. But they were technically difficult, and beyond the needs of the class of architecture students. Nevertheless, they mastered the details – students of architecture are among the brightest I have taught. And the lesson taught to me was that a teacher should be very careful in the selection of his material and in the depth of his discourses.

James Ackermann, a distinguished architectural historian, offered me a carrel in his department, and I spent an intense and productive three months starting to flesh out some of the topics I had sketched in *The Stone Skeleton*. The Widener library is magnificent, and it was possible to borrow books, date stamped as having been consulted fifty years earlier, and to take them back to my monkish carrel. There I began to write a series of papers on medieval construction, which were published over the next six or seven years.

There were mathematical papers for technical journals, exploring the application of the very beautiful equations of equilibrium to masonry domes, to spires and to fan vaults. These equations had been used by engineers to establish the so-called shell theory, by which thin concrete roofs could be analysed and designed; the concrete shells, now out of fashion, have thicknesses which are, in proportion, considerably less than that of a hen's egg. The essential feature of these equations, which seem frighteningly to involve partial differentials, is that in fact they solve very easily, and give unequivocal results which are independent of the boundary conditions. The solutions equate to those for the four-legged table, and do not require the pollution of an examination of elastic behaviour.

There was a series of papers on the simple masonry arch, which explored questions of strengthening existing medieval bridges. The size of trucks allowed to operate in the U.K. had recently been increased to forty-two tonnes, and county engineers, who were responsible for non-arterial roads, were concerned with the safety of their old as well as their new bridges. I managed to establish rules which could help these engineers, and I used them later myself in two case studies: the very small span of Clare College Bridge in Cambridge, which at that time was open to passenger cars, and Telford's bridge over the the masonry arch

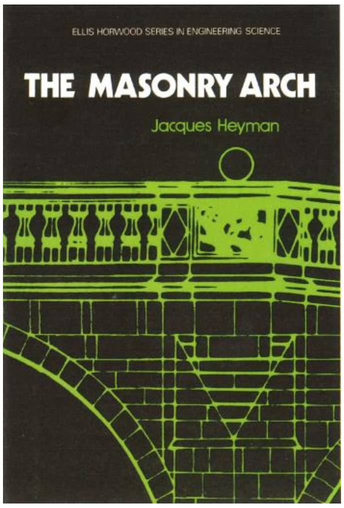


Photo: P. Pattenden

Over which spans 45 m, and was carrying the 42-tonne trucks between England and Wales. The Institution of Civil Engineers published my papers on these studies, and gave me a gold medal. Some eight years later the Institution also published an account of the work I did to strengthen a bridge over the Medway in Kent.

Clare College Bridge, 1635 | Photo: P. Pattenden

Telford's Bridge at Over, 1826

There was also some mainly historical work, published in non-technical journals. For example, the history of Beauvais Cathedral included the fact that after the completion of the first phase of construction in 1272, the cathedral then stood quietly for twelve years, until on a quiet night in 1284 the cathedral quietly collapsed. This contradicts the flamboyant theorem (if a masonry structure stands for 5 minutes then it will stand for 500 years), and the reason for collapse must almost certainly be sought in some geotechnical event. It is a reminder to the engineer that the simplifying assumptions necessary for an analysis may not represent reality – in this case, perhaps soil movements were on a scale with which the essentially flexible masonry construction could not cope. My paper on Beauvais made much use of the material stored in the wonderful Widener library.

A more violent contradiction of the 5-minute theorem is evidenced by the collapse on 12 February, 1322 of the central tower of Ely Cathedral; the tower had been standing for two centuries. This collapse was almost certainly due to geotechnical causes. A year earlier work had started on the very large Lady Chapel to the north of the choir; excavations removed soil on one side of the tower but not on the other, and the water table (the level of standing water in the soil) would also certainly have been changed. The precise cause of collapse, as for Beauvais, will never be known, but it is easy to construct possible soil states for which the tower would have been unstable.

I continued to write other papers on ancient construction. For example, 'On the rubber vaults of the Middle Ages' was published in the *Gazette des Beaux-Arts*, and took as its starting point a statement of Choisy, quoted by Frankl. Choisy wrote, in 1899, that 'the ribbed vault is, as it were, flexible and deformable; the points of support can settle, the piers lean, it will follow their movements'. Choisy was an engineer of the *Ponts et Chaussées*, and his *Histoire de l'architecture* gives, for really the first time, a comprehensive overview of the mechanics of masonry structures. Frankl offers as comment: 'When an expert like Choisy said that, it is no wonder that whole generations of architects and art scholars calmly continued to live and theorize under Viollet-le-Duc's influence, although they all must have known that Gothic transverse arches, ribs and vaults were not made of rubber.'

But Choisy was merely articulating the understanding of structural action that every engineer has always known, perhaps unconsciously – if a structure can stand, then it will. Gothic masonry is indeed flexible; it responds to attack from the environment by deforming and developing harmless cracks. Poleni, in the mid-eighteenth century, had this understanding of the cracks in Michelangelo's dome of St Peter's; I wrote later about the work he did in Rome.

Ely Cathedral

My *Beaux-Arts* paper was published in 1968; *The Stone Skeleton* had appeared in 1966. Two or three years later these papers had been noted by Donovan Purcell, an architect who had been a Lecturer in Cambridge, who had set up an architectural practice (Purcell Miller and Tritton – now simply Purcell), and was the Surveyor of the Fabric of Ely Cathedral.

The lightning conductors in the cathedral were inspected professionally every five years, and in 1971 the steeplejack reported that some tapes on the Great West Tower were detached, and that in places some of the masonry was fragmented or loose. Purcell immediately commissioned a survey of all four faces of the tower, and by 1972 it was clear that there was much unsound masonry, and that considerable damage was being caused to otherwise sound stone by the rusting of external iron ties and strapwork (which had been installed a century earlier by George Gilbert Scott). Purcell recommended that the stonework of the whole tower should be overhauled. This would not be possible without the provision of very expensive scaffolding, and while this was in position it would provide an opportunity for any necessary strengthening works.

Donovan Purcell needed engineering advice. It may be noted that architects acting as Surveyors for cathedrals have immense experience, and are fully capable of tackling most of the problems of repair and restoration. Occasionally, however, the work is on such a scale that it can be best prosecuted by an architect together with an engineer working under his or her direction. (There are at least four English cathedrals whose Surveyors have been women, and the Dombaumeister of Cologne Cathedral was better addressed as Dombaumeisterin.) So Purcell came to me, the expert on masonry, to give him advice on the repair and restoration of the Great West Tower of Ely Cathedral.

I knew nothing about masonry. I had done some mathematics to establish a basis for understanding the action of stone blocks piled on each other, and I had become interested in the history of the engineering analysis of this action. Moreover I realised immediately that if I were to act, effectively, as the engineer in charge of a major restoration, I did not have the staff and office capacity to provide surveys, drawings, reports and so on. However I had kept in touch with R.T. James and Partners, who had provided precisely these vital functions in the contract for the building of the plastic composite framework for the extension to Baker Building. Johnson and I had made the calculations and sketches, but it was R.T. James who made the final drawings, negotiated with the Local Authority, drew up the contract and supervised the construction, working all the time with the architect employed by the University.

So it was that I found myself working on the repair of the Great West Tower of Ely Cathedral, the first of many similar (but smaller-scale) involvements at Ely, and at other cathedrals and many parish churches in England.

The Tower project lasted for two years, and I made formal weekly visits to attend site meetings, and often drove the sixteen miles from Cambridge at other times. I came to hate those journeys across the Fens, especially in some exceptionally cold weather, but my spirits recovered at about half way, when the cathedral could be seen in the distance, and I realized that the tower was still standing. For I had not yet grasped fully the implications of my theoretical work; my trivial interventions into the fabric of the tower could never compromise the essential stability of the whole construction.

For example, both Purcell and I needed to know the actual composition of the walls of the 65 m tower, which were, in the lower portions, more than 1½ m thick. It turned out that the 1610 mm consisted of surface limestone ashlar about 180 mm thick, and an inner limestone skin of about 150 mm; between the two there was a rubble fill consisting of small fragments of stone, pebbles and mortar, with a significant proportion of complete voids.

This twelfth-century ‘standard’ construction was determined by drilling and removing a 50 mm diameter core from the whole thickness, and it was in this operation that, not having complete comfort of the safe theorem, I had serious self-induced fears of the consequences of my intervention. Of course, the tower never noticed that it was being attacked by what was really very simple apparatus. In modern concrete usage cores are taken to monitor the quality of the material actually poured in the construction, and the concrete laboratory of Cambridge University's Engineering Department had the tools needed. Surprisingly little power was required to drive the diamond-tipped coring cylinder, and the small electric drill could be easily handled. It was a matter of some delight to me that one of the technicians from the laboratory who carried out the work had the good medieval name of Christopher Mason.

At the same time scaffolding was being erected, and R.T. James were preparing drawings for our use and that of the contractor. The general contractor was Rattee and Kett, a large firm based in Cambridge. In the mid nineteenth century both Mr Rattee and Mr Kett had sizeable businesses in Cambridge, and they merged to form a single firm to carry out the work of George Gilbert Scott's repair and restoration of the cathedral.

A decade later, in the 1870s, Rattee and Kett were working in Peterhouse. The College had decided to reconstruct the Fellows' public rooms, and George Gilbert Scott Junior led a Pre-Raphaelite team which included William Morris, Burne Jones and Ford Madox Brown. The design brief was comprehensive, and included not only extensive alterations to the buildings themselves, but also the decoration and furnishing of the new public spaces. The dining chairs, each subtly different from the others, were crafted by Rattee and Kett, and are in use today. The cost of all this reduced the College to extreme, if temporary, straits.

When Rattee and Kett had completed Scott's works at Ely, they made general repairs to the cathedral as and when needed, and at the time of the work on the Great West Tower they had been working in the cathedral for over a century. Their knowledge of the fabric was unrivalled, and was invaluable throughout the contract. If some problem arose during our two years' work on the tower, then the Surveyor, as architect, myself, as engineer, and Vic Franklin, as contractor, would be found on the scaffold examining some difficulty that needed solution. It did not take long for each of the three of us to confess that we did not know exactly how to deal with the problem, but we were determined to find the solution. And, invariably, we did, or at least we found a solution, and the conclusion was stronger because it had been arrived at collectively. Right, we will do it this way – and the bill will be sent to the Dean.

Not surprisingly the Dean came to require some ‘peer review’, as the academics would say, and his solution was to employ a Chartered Quantity Surveyor. So soon, instead of three on the scaffold discussing a problem, there were four, and the original three appreciated a helpful extra opinion. However, the contractor perceived that decisions might not always be to his advantage, so he employed a second Quantity Surveyor, and now there were five of us. In fact this worked well, and to the advantage of the Cathedral.

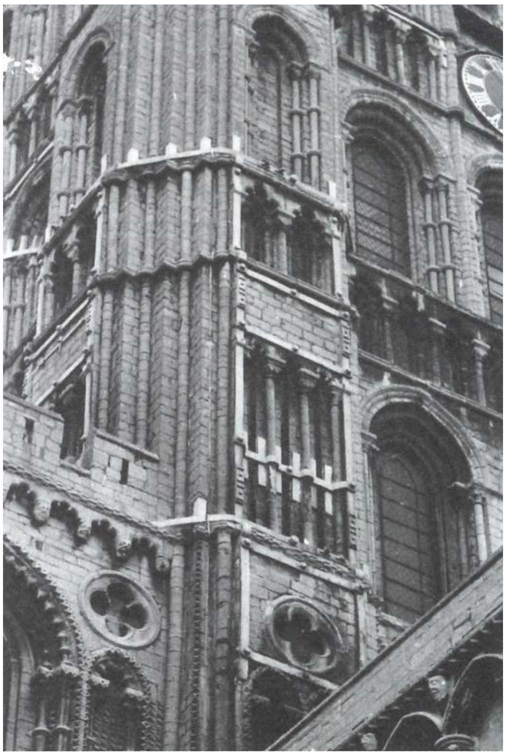
And also to my personal advantage. I was learning very fast the business of the practical engineering of masonry, absorbing what I was taught by Donovan Purcell and by Rattee and Kett. Purcell had worked in many large churches, as had the Quantity Surveyors, and the practical knowledge of Ely Cathedral possessed by Rattee and Kett was invaluable. My experience with R&K was repeated on several other occasions; when I gave some advice on problems in King's College Chapel, I found that they had worked there extensively themselves. Distressingly, Purcell died unexpectedly about a year into the work at Ely, and the Dean and Chapter had to find a new Surveyor. There was no compulsion at that time, as there is now, to make an appointment, but it was clear that an architect was needed to continue the repairs to the tower. And there was no compulsion to appoint one of Purcell's colleagues, but Purcell Miller and Tritton had by this time a complete overview of the whole operation, and Peter Miller proved to have the skills of Purcell himself. Indeed in calmer times, when Miller retired, his junior partner Jane Kennedy was appointed against stiff competition, and she has served the Cathedral with distinction for three decades.

I was also learning the administration of (in today's terms) a multi-million pound contract, of the importance of a skilled workforce, of the vital rôle played by the master mason in charge of that force – there is now a stone bust of John Bradford let into the parapet at the very top of the West Tower.

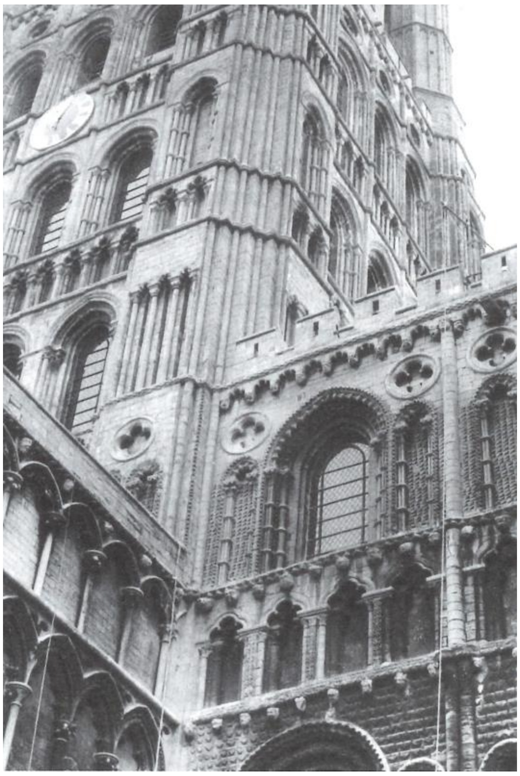
The problems of medieval towers are straightforward. The four walls of a tower, constructed in the usual way with inner and outer skins of good masonry and with a rubble fill between the skins, tend to increase in thickness. A rise in temperature may cause an expansion of the masonry, leading to voids appearing in the rubble fill. The passage of an ox-cart close to the tower will cause slight vibrations, and small particles, dust from the rubble fill, will fall into the voids; the solid skins, on a decrease in temperature, cannot return to their original positions. A further rise in temperature will cause new voids to open, and the cycle is repeated. There is, in fact, a kind of ratchet mechanism; there is a one-way drift in the wall thickness.

However, the inner skins of the walls of a square tower cannot move inwards; the four corners provide mutual support for the four inner faces. The 'drift' in thickness must result in the outer skins of the tower walls moving out. The remedy has been known for very many years; at the tops of many church towers may be seen iron ties, lying within the tower, which connect together the four corners, and betray their presence by the external pattress plates to which they are connected.

This remedy was very well known to Scott, and he used it several times – for example, at St Albans. At Ely, three sets of iron ties were installed in a major engineering intervention. Just above the floored belfry, at about two-thirds of the height of the tower, Scott introduced diagonal wrought-iron ties (5 in by 1¼ in) connecting opposite corners of the tower. At two lower levels the inner skin of the walls is cut away and replaced by arcaded piers, while the outer skin remains; between the two there are walkways connecting the four corner stairs. It was in these walkways that Scott introduced 2½ in diameter iron ties, passing at each end through the outer walls. But the ties had to connect to something; instead of impossibly large pattress plates, extensive external strapwork embraced each of the corners, forming what Purcell called a corset (Purcell hated this bracing, which was indeed visually very obtrusive). The corset was, in general, formed of rectangular section wrought iron, typically about 1 in thick. This whole reinforcement was a triumph of Victorian engineering design and execution; it was in the hands of R. Reynolds Rowe, who reported on the repairs to the Royal Institute of British Architects in 1875, in the same way that papers were presented to the Institution of Civil Engineers.



Left: Ely Cathedral – the ‘iron corset’ to the West Tower | Photo: J. Heyman



The same after removal of the iron | Photo: J. Heyman

Scott's whole concept for the strengthening of the tower was exactly right, but the wrought iron let him down. Some of the nominal 1 in strapwork was found, in 1972, to be 2 in thick – the extra inch was rust, and it was this virtually irresistible increase which was causing so much damage to the masonry. We resolved to undo all of Scott's work, and the external faces of the tower are now clear of all the unsightly reinforcement.

But the intrinsic problem of the tower remained; the continued drift of the thickness of the walls had to be prevented, and the four corners had to be located permanently in place. For the first defect, I borrowed technology from modern reinforced-concrete design. At three levels of the tower, a horizontal concrete ring beam was created by drilling literally hundreds of small holes, into each of which a stainless steel rod was inserted, and then fixed in place by pumping in Portland cement. These ‘reinforced-concrete’ ring beams stabilize the two skins of the tower walls.

In addition, I cut Scott's ties (and removed the external iron strapping), and replaced them by substantial new ties, this time in stainless steel, to perform the same essential task as the wrought-iron ties of Scott's intervention. This was my first essay in the strengthening of a large Romanesque/Gothic construction. With hindsight, the introduction of invisible concrete ring beams and the replacement of ties was a sort of belt and braces solution of what was evidently a chronic problem. Much later I cut Scott's ties in the south-west transept of the cathedral, and used only the reinforced-concrete technology, without replacing the ties.

Photo: J. Heyman

This work may be criticized by the precepts of the Venice Charter of 1964. A group of architects and conservationists had met and drawn up guidelines for the preservation of ancient structures, which paid attention to both the structural and aesthetic needs of these buildings. On the whole these guidelines are eminently sensible, although there is one particular recommendation (which can be traced back to Ruskin) about which present-day architects are divided, and with which I profoundly disagree. Effectively, the recommendation is to the effect that if an intervention is made to repair a building, then that intervention must be immediately obvious. This has led to unsightly intrusions; a new stone in an old wall is made deliberately not to match those surrounding, and the mortar round its edges does not match the mortar elsewhere. I had, on the contrary, taken pains (for example in the construction of the reinforced-concrete ring beams) to make the work on the West Tower of Ely Cathedral completely invisible, even on close inspection. Later, when I reinforced the four crossing piers at Worcester Cathedral by the same technique of stainless-steel reinforcement and grouting with strong cement, the work was done so carefully (by an Italian firm, Fondedile) that, even at ground level, the presence of hundreds of holes cannot be seen.

Morever, at both Ely and Worcester, a further recommendation of the Venice Charter, added later, is grossly violated. Any repair work to an ancient structure must not only be immediately detectable by the observer – it must be reversible. My introduction of steel reinforcement, grouted with cement, is clearly irremovable. I have no difficulty with this.

Westminster Abbey

Donovan Purcell had noted my attempts to scratch the surface of the engineering theory of masonry – small scratches, not very deep, but I was perhaps the only engineer in England prepared to offer advice on the repair of old structures. Advice, that is, based upon what was really a new way of looking at the action of structural masonry (and indeed, of the behaviour of medieval timber roofs – plastic theory is a universal theory). Large experienced firms of consulting engineers were of course prepared to give sound advice on these matters, based on their deep knowledge of conventional structural design. R.T. James was available for Westminster Abbey, and Ove Arup devised and controlled the execution of a large-scale underpinning of York Minster, together with the stabilization of the east end of the church. I visited York while the work was going on, and met the engineer in charge, a Dane, Poul Beckmann, whom I got to know more closely later; his experience and insight taught me much.

But it was Donovan Purcell's membership of the Cathedral Architects' Association that led to the opening of many doors for me. The sixty or so architects in charge of the fabric of cathedrals in the U.K. had created a forum, several years before, for the exchange of information and experience; the internet has now made such exchanges very easy. In addition the Association met (and still meets) for an annual conference, usually for two or three days in a cathedral city, with a few lectures and excursions to local churches of interest. In between the annual meetings, the Association usually arranges a Spring visit for a few days to a cathedral city in Europe – for example, Cologne, Milan, several churches in Sweden, and so on; perhaps some twenty members attend these visits. I had in due course been made an extraordinary member of the Association, and our most adventurous excursion was to Mexico City, where I had by accident established some connexions.

It was at one of the annual meetings that I presented what I knew about the mechanics of church architecture, and it led to my involvement, over the next forty years, with some dozen cathedrals and perhaps fifty churches, some small, some large. Because of my obsession with the safe theorem, whose fundamental conclusion is to leave well alone, my interventions were usually on a small scale, and my advice was often, acceptably, to do nothing. Exceptions were the repairs at Ely, and, later, major works in other parts of the cathedral, and at Worcester Cathedral, where in addition to the strengthening of the crossing piers, a medieval high vault was repaired and in part rebuilt. Elsewhere, work on a smaller scale was done at St Albans, Peterborough, Exeter, Lincoln and other cathedrals.

However it was in 1969, before work had started at Ely, that I first became involved with Westminster Abbey, an involvement that was to continue for thirty years. Some gently heated correspondence in *The Times* had started two years earlier, in 1967. To understand the questions at issue, it must always be remembered that, until the passing of the Cathedrals Measure in 1990, cathedrals (and Royal Peculiars like Westminster Abbey) were subject to no form of planning control (unlike churches, even large churches). The reason that Purcell and I could discuss problems on the scaffold and determine courses of action was that, if the Dean of Ely accepted our advice (and he had little alternative) then no further permission was needed – Deans had the freehold of their cathedrals, and could do as they liked.

Pevsner's first letter to *The Times*, on 3 February 1967, drew attention to the destruction of the medieval timber roof of the nave of Westminster Abbey, which had, on the advice of the Surveyor to the Fabric, been taken down and rebuilt, with no survey having been made; the same fate had overtaken the roof of the south transept.

The Dean of Westminster replied five days later in a long letter clearly based on advice from the Surveyor, Stephen Dykes Bower. The letter points to the dangerous structural condition of the roofs in question, and defends the work on the grounds of safety, as well as creating a reduction of fire risk (all cathedrals now have fire breaks down the lengths of their roofs, as they did not in 1967); the replacement of medieval timbers by new was done on grounds of economy.

Pevsner was not satisfied, and his short letter to *The Times* a few days later exposes his real concern, namely that the programme of work was not 'transparent' (to use modern jargon) and had not been subject to expert and detailed advice. This evinced a long statement from Dykes Bower himself, who was clearly offended at being thought inexpert in what he was doing, and he raises (whether knowingly or not) some of the precepts of repair and restoration stated in the Venice Charter. Experts (an archaeologist, an entomologist), and the Secretary of the Ancient Monuments Society, also contributed to the correspondence, and various cross-currents could be detected.

Battle was being joined with several principals involved, who sometimes formed unlikely alliances. At bottom, the architect, engineer, entomologist, were the professional 'players', and the 'gentlemen', the amateurs, were the art historians, the archaeologists and the self-appointed guardians of the heritage. The essential difference between the two groups is that the architect (the Surveyor), with his qualified advisers, is responsible not only for the aesthetic well-being of the building, but also for its physical safety.

I was talking with Dykes Bower once about Abbey matters, when he suddenly set me a searching question. Supposing, he said, that six days before the coronation of Queen Elizabeth II in 1953 I had come into the Abbey, and a verger had reported a fallen piece of stone, about the size of an orange, exactly on the spot where the Coronation Chair was to stand. What would you have done, knowing that in six days' time the organ and brass would be shaking the windows, and massed choirs would be shouting Vivat Regina? I replied that I would climb as high as possible, to the triforium and above to the clerestory, and search the high vault minutely with powerful field glasses. I would, of course, find nothing. It would perhaps not be necessary to kill the verger, but he would be sworn by an awful oath to secrecy. I would then do and say nothing. Quite right, said Dykes Bower.

Such Surveyors are contemptuous of what they regard as ignorant interference. For their part, the 'gentlemen' (and the term may in this instance be widened to include professors) are equally outraged that the clergy, advised by their professionals, could proceed almost as they pleased in their churches.

There was more correspondence in *The Times* during the next few days. The Secretary of the Ancient Monuments Society pointed out that the 'ecclesiastical exemption' from planning laws made cathedrals ineligible for Government grants towards the cost of repairs. The cathedrals had fought for their autonomy over a long period, and they gave in only in 1990, after a century of successful resistance. They were well aware that their exemption cut them off from public money, and they had been willing to pay this price. For example, my work on the West Tower of Ely Cathedral had been funded by an extensive private appeal.

It was a combination of circumstances – above all, the huge cost of repairs – which forced the cathedrals to accept the 1990 Cathedrals Measure. In 1967, when the correspondence about the roofs of Westminster Abbey was about to come to a peaceful, if temporary, conclusion, the 'amenity societies', such as the Ancient Monuments Society, found the current situation indefensible.

One of the last letters of the February correspondence of 1967 was written by Lord Euston (later to be the Duke of Grafton), acting as chairman of another amenity society, the Society for the Protection of Ancient Buildings. He regretted that his professional members had not been able to inspect the roofs which had been destroyed, but reported that the Surveyor had promised the opportunity to inspect the remaining medieval roofs of the north transept and the apse. These matters did not in fact come quite out of the blue. Over a year earlier the Marquess of Salisbury, as Chairman of the Royal Commission of Historical Monuments (England) had written to the Dean of Westminster to plead for the retention of the thirteenth-century roof over the apse.

In the calm which ensued after *The Times* correspondence, the Dean and Chapter felt able to resume the repair work to the Abbey. However, a year later, in June 1968, a Petition addressed to the Queen was written on the headed paper of the Royal Commission on Historical Monuments (England) and signed by both Lord Salisbury and Lord Euston. The Petition noted that work had been resumed on the remaining medieval roofs, and asked the Queen to appoint a small advisory committee to look into the position regarding these roofs.

The ultimate authority for a cathedral is the Bishop in whose diocese the cathedral is situated; the Bishop is the Visitor, to whom appeals may be made. Westminster Abbey is not a cathedral – it is a Royal Peculiar, and its Visitor is the Queen. The Petition which asked the Queen to intervene was never sent; its existence was sufficient to cause the Dean and Chapter to follow its advice. No work had yet been done to the apse roof.

The Dean and Chapter instituted something very close to a medieval expertise in order to decide on the best course of action to take in the repair of the apse roof. With advice from The Royal Institute of British Architects and the Institution of Civil Engineers they appointed four experts to report on the matter – two architects and two engineers. I was one of the engineers; the other was a partner of R.T. James, the consultants for the Abbey. One of the architects was, at one time or another, Surveyor to three major cathedrals, and had carried out extensive repairs to these and other churches; the other architect also had broad relevant experience, and was, in a sense, representing the Society for the Protection of Ancient Buildings.

The four experts were not allowed to meet before submitting their independent reports; indeed, they were initially not given the names of the others in the group. This contrasts sharply with the meeting in Milan in 1400 of ten Masters from all over Europe; disputes had arisen about the recently started building of the cathedral. The extensive and detailed minutes of the discussions give invaluable insights into the theory and practice of medieval architecture.

The four reports on the Westminster roof were simply written but were, of necessity, technical. All four experts found that extensive repairs were needed to the apse roof (a few years later I published in a journal of architectural history an account of the medieval timber work, stressing the defects in both design and execution). The two engineers were in favour of dismantling the roof and replacing it with new work; the two architects were in favour of retaining as much as possible of the thirteenth-century timber, with joints and cavities filled with epoxy resin, together with other strengthening measures.

Dykes Bower summarized all this at length for the Dean, and at the end of ten foolscap pages he concluded with a series of observations that lie at the heart of conservation and repair.

‘That there are two approaches to this problem will now be clear. The care of old buildings induces a bias of inclination that is properly and almost inevitably towards preserving whatever is of worth, towards losing nothing that can fittingly be kept. Nowhere is it likely to be more impelling than at Westminster Abbey. The very sense of continuity, however, instinct in corporate bodies and which engenders reverence for the past, also must take thought for the future. In what condition is the building to be handed on? What will be its structural safety? Should potential sources of trouble be bequeathed to successors?

‘Such questions demand a balance of sentiment and realism. Has the roof over the Presbytery and Apse, in spite of structural shortcomings, such outstanding historical worth as to justify resort to any means to retain it? Can these means be accepted as reliable for at least one or two centuries? Will the cost of preserving it, not easy to assess, vindicate the result? When three layers of soft wood are placed over its trusses, its oak members are largely filled with epoxy resin, its joints are made good by the addition of mild steel plates, the oak wall plates and tie beams replaced by reinforced concrete, how far will it really be the roof as left by the original builders? If it is the stressed skin that will be chiefly holding it, will it have been preserved or mummified?

‘Would a new oak structure, also on a foundation of concrete ties and wall plates but conforming to the original design be, aesthetically and historically, rational or the reverse? Is it wrong in the present age to do what has been done before, with only such changes as technical advances direct? May greater convenience for ease of inspection and upkeep, as well as freedom of movement for fire-fighting in an emergency, be an advantage of practical benefit? Is structural simplicity preferable to the medley of struts and supports that, as a result of repairs, have confused the existing roof?’

Many of these questions are, in reality, unanswerable.

Perhaps it might have been expected that four experts, commissioned to write four independent reports, would submit four differing views of the way to treat the roof. Dykes Bower certainly gave no clear guidance to the Dean, and the Chapter were compelled to appoint a fifth expert, a very distinguished architect, to make an assessment of the four reports. Effectively, he came down on the side of the architects – the thirteenth-century timbers were to be retained, but there was to be restrained use of epoxy resin. A concrete ring-beam was needed, and steel plates were to be used as necessary. This was the way the work was done in the next few months.

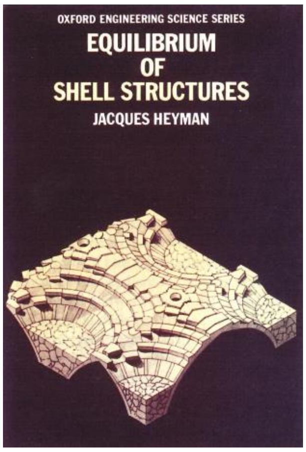
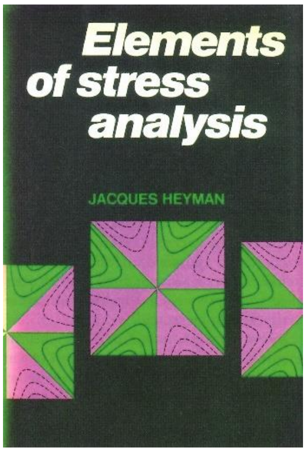


Photo: P. Pattenden



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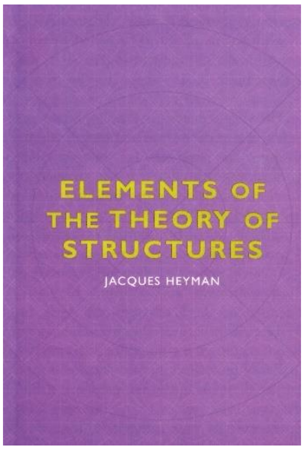


Photo: P. Pattenden

Thus, some two years after Pevsner's letter to *The Times*, the Dean was able to make final repairs to the apse roof, keeping in place most of the original timbers. The local problem had been dealt with, but this still left the ‘intolerable’ situation of ‘Ecclesiastical Exemption’; the cathedrals of England, and a *fortiori* the Royal Peculiar of Westminster Abbey, were not subject to the usual State controls imposed on scheduled monuments.

Some twenty years earlier the Council for the Care of Churches had established the Cathedrals Advisory Committee in an attempt to persuade cathedrals to, at least, receive advice on their problems. Lord Euston was an early member, and, as the Duke of Grafton, he was later to chair the Committee. As implied by its name the Committee had no powers of enforcement. It had as members three or four Deans or other clergy, two or three architects, an archaeologist, an engineer, and an art historian, to a total of about twenty. During its nearly forty years of existence it was in fact increasingly consulted by Deans and their Surveyors, and it established a strong store of information and experience. It was some ten years later, in 1981, that I joined the Committee, which became, under the Cathedrals Measure of 1990, the Cathedrals Fabric Commission for England, on which I served for a further decade.

Dykes Bower was succeeded as Surveyor at Westminster Abbey by Peter Foster, and the Dean and Chapter took the opportunity to establish an Architectural Advisory Panel, with a membership reflecting that of the Cathedrals Advisory Committee, although rather smaller in number. In future, should controversial matters arise such as the reconstruction of timber roofs, they would be minuted as having been discussed by the Panel, although it sometimes proved difficult to time discussions so that effective decisions could be made. I was a member of the Panel for twenty-five years. Membership of the Cathedrals Advisory Committee and of the Westminster Abbey Panel of course changed slowly, although there was always some common membership. Meetings of both bodies embraced lunch – sandwiches for the Committee and, for the Abbey Panel, a three-course meal with wine in the splendour of the Jerusalem Chamber, near ‘another chamber’ where, in 1413, Shakespeare's Prince Hal tried on the crown as his father Henry IV lay dying.

Fan vaults

While I was becoming increasingly involved in the worlds both of structural masonry and, ironically, of the Church, I was writing papers and books. The repairs to the Great West Tower of Ely Cathedral were reported to the Institution of Civil Engineers, and published records of this sort of work were repeated during the next twenty or thirty years; repairs to a medieval bridge, to a cathedral, to a small church, quite often threw up new problems. For example, repairs to the great Dean's Eye of Lincoln Cathedral led to a study of the way a masonry rose window, subject to a wind load of perhaps 10 tonnes, manages to carry the resulting forces, an understanding essential for its repair; or repairs to ‘cantilever’ stairs, in which the outer edge of each tread seems to be totally unsupported, led to the conclusion that the inner edge tends to twist in the wall, and that this torque must be resisted; and an explanation was found for the characteristic and harmless cracking of high Gothic vaults (and aisle vaults); or, back at Westminster Abbey, the cracking and displacement of stones in the fantastic fan vault of Henry VII Chapel required explanation.

Apart from smallish fan vaults at Sherborne Abbey, about mid-fifteenth century, and examples of small aisle vaults, such as the cloisters at Gloucester, about 1400, only four major fan vaults were ever built, all in the first dozen years of the sixteenth century, and all by the same group of masters. Robert and William Vertue completed the vault at Bath Abbey, about 9 m span, in 1503; they were responsible for Henry VII Chapel, over 10 m span, built 1502-09; William Vertue visited Cambridge several times to consult with John Wastell, who completed the vault of King's College Chapel in 1512, the largest of the four at a span of over 12 m; and finally Wastell vaulted the 8 m Lady Chapel of Peterborough Cathedral.

The fans of a fan vault spring from the north and south walls of a church, and meet at the highest point of the roof. Their form is that of a half cone – an ice-cream cone, but with a fundamental difference. The sides of the cone of a fan vault are not straight, but are themselves curved, as is, for example, the bell of a French horn; the ‘cone’ of the fan is strictly a conoid. This kind of surface is fundamentally different from that of a domical shell. In technical jargon, a dome has positive Gaussian curvature – the curvature of the shell of the inner dome of St Paul's Cathedral is of the same ‘sign’ in the N/S and E/W directions. By contrast, a fan vault has negative Gaussian curvature, like a saddle; the saddle curves upwards along the backbone of the horse, but downwards to accommodate the rider's legs.

The mathematical equations to determine the stresses in a dome and in a fan vault are the same, but the positive and negative Gaussian curvatures lead to some extraordinary differences in the solutions. For example, the mathematical conclusion is that a dome should be very easy to build. Brunelleschi was not believed in 1407 when he declared that he could build the dome of Santa Maria del Fiore without elaborate centering; but when he was finally commissioned in 1418 he showed that it could be done. It can be imagined that a single course of the dome, built from small pieces of masonry that require minimal support as they placed in position, will, when the course is completed, be self-supporting; the ring will sustain itself against falling inward. That ring will form the base for the next course of the dome, and so on; there is even no need to close the dome, and an eye may be left, as in the Pantheon in Rome. This behaviour is confirmed by the mathematics; all the forces in the masonry in the construction process are compressive, compacting the stones together.

By contrast, the mathematics for the fan vault shows that tensile forces during construction are inevitable, pulling the work apart. The whole construction, and every stone, must be supported by centering, which can only be removed when the fans from opposite sides of the church meet at the centre. This is completely analogous to the construction of a two-dimensional arch; the centering can only be removed when the keystone is in place.

Construction scaffolding, in the sixteenth century and now, is extremely expensive – in fact, at King's College John Wastell had to be helped financially. In addition, the double curvature of the stones in a fan vault imply very great expenditure on the cutting of the masonry. Henry VII Chapel and King's College had royal money, but the large fan vault was inevitably doomed to extinction on the grounds of cost.

Peter Foster was succeeded as Surveyor at Westminster by Donald Buttress, and among his many other activities, he made a thorough restoration of Henry VII Chapel. Complete internal scaffolding enabled inspection at every level, and I recorded defects such as cracks in the masonry, stones which had slipped against their neighbours, and small changes in the overall width of the Chapel – as in all such buildings, the external buttresses had given way slightly, leading to a slightly increased span of the vault. All this could be related to the by now well established pathology of Gothic and Romanesque vaults, and it was immediately clear why certain stones at Westminster had slipped, and not others.

History of Civil Engineering

I believed I was beginning to understand the behaviour of stone construction, and I was certainly taking great pleasure in being involved in the repair of ancient buildings, and in my involvement with bodies concerned with these matters. I had worked so far in two fields – the plastic analysis of steel structures, and the application of plastic principles to masonry. A third field was about to be opened.

I had known Andrew Schofield for many years, since he had been a research student in the soil mechanics group in Cambridge (now dignified by the label geotechnical engineering). He pointed out to me that Charles Coulomb had published in 1773 a paper laying the foundations of the whole new science of soil mechanics. Every text book on the subject quoted Coulomb on the first page, and it was clear that every author of these books had never read the original paper. Schofield proposed that we should translate this paper from the French, and publish it in a soil mechanics journal, together with appropriate footnotes. We started on this enterprise, but he was then immediately appointed to a Chair in Manchester where, with enormous energy, he established laboratory machines for the centrifugal testing of soil – models of earthworks could be subjected to gravity forces a hundred times (at least) of those on Earth, and time-dependent behaviour was correspondingly speeded up.

He had no time for the Coulomb project, but I was hooked. I made the translation, and my wife corrected the grosser errors. And I supplied footnotes, a commentary on Coulomb's text. Coulomb is remembered, rightly, as a physicist – the unit of electric charge is named after him. But he was in fact a military engineer, an officer in the Army who had graduated from the engineering school at Mézières. This *École du Génie* was, effectively, a graduate school, and indeed, after the establishment of the *École Polytechnique* in 1794, engineering students had a common basic training in Paris and then went on to an *École d'Application; Artillerie, Mines, Ponts et Chaussées*, and so on.

Coulomb entered Mézières in 1760, and graduated two years later. He was posted to Brest, from which in 1764 a ship was sailing to Martinique, for the purpose of strengthening the fortifications against possible renewed attack by the English. The assigned engineering officer fell ill, and Coulomb was drafted in his place; he stayed for nine years on the island, and his health was permanently ruined. He found that he needed expertise in four fields of civil engineering, and that what he had been taught at Mézières was of limited help. He attempted his own solutions to the problems, and it was his *Essai*, containing the section on soil mechanics, that was presented to the *Académie* in 1773. The four topics tackled in the *Essai* were the 'classic' civil engineering problems of the eighteenth century: the strength of beams, the strength of columns, the thrust of soil, and the thrust of arches.

The title of the paper was 'On an application of the rules of maximum and minimum to some statical problems, relevant to architecture'; he was applying the century-old calculus of Newton/Leibniz to engineering construction. The beams and columns of a framed building require rules, based on theory and experiment, for their design; Coulomb wished to determine the breaking strengths of both elements. It is ironic that his discussion of beams was directed towards the determination of breaking loads, but that his theory has come to be regarded as laying the foundations of the elastic theory of bending.

La poussée des voûtes and *la poussée des terres* are analogous problems; in both cases their study is directed towards the determination of those thrusts. For a masonry arch, say a bridge across a river, masonry abutments must be established on the banks of the river to ensure that the bridge does not collapse. My study of Telford's bridge at Over (the gold-medal paper) had started from the observation that the 45 m bridge had sagged at midspan, and it was obvious that the abutments had been inadequate. In fact Telford had himself noted, and much regretted, that the western pier had moved; he also noted that the movement had ceased, and pronounced the bridge to be safe – an unwitting recognition of the 5-minute theorem, or in this case the generation theorem. The geotechnical consolidation of the clay soil had been accomplished within a very short time.

The sag of Telford's bridge was the result of 'hinges' forming between the voussoirs of the arch, normally a safe behaviour until a limit is reached when the sort of mechanism proposed by Kooharian develops. Coulomb's outstanding contribution to the theory of arches lay in his examination of the way in which such hinges might form. He does not examine the *actual* state of a masonry arch; rather, he observes that, from a study of hinge formation, it is possible to find limits within which the arch thrust, the force exerted on the river abutments, must lie. An arch developing a force outside those limits would not be stable – it would collapse; hence the words maximum and minimum in the title of the paper.

The calculation of the thrust of soil was directed to the design of retaining walls, for which empirical and effective rules had been in existence for half a century or more. If a trench with vertical sides is cut in a clay soil, a depth will be reached at which the sides fall in (perhaps killing the digger – such deaths do still occur). To prevent this collapse, the faces of the trench must be lined and propped apart; for fortifications, which was the design problem faced by Coulomb, a retaining wall is needed. A theoretical analysis will provide rational rather than empirical rules, and Coulomb made a huge advance in determining the behaviour of soil behind a wall. His contribution was not the first in the field, but the *Essai* may indeed rightly be considered to lay the foundations of the science of soil mechanics.

I made footnotes in an orderly way for each of the four problems treated by Coulomb, trying to find antecedents and tracing developments forward to recognisably modern analysis. It was this work that forced me to dig deeply into the development of structural theory; I had always been interested in this history, but it led to a third major preoccupation (after plastic design of steel frames, and the application to masonry). The footnotes to the Coulomb paper ran to four times the (substantial) length of the original Essai; I had written a 200-page book, and it was published as such, superbly, by Cambridge University Press (of which I was not yet a Syndic – I served for eleven years on the 'board of directors'). *Coulomb's Memoir on Statics* is not my best book, but, together with *The Stone Skeleton*, it is the one most often cited.

Coulomb was published in 1972. I consider my best book to be *Structural Analysis; a historical approach*, published in 1998, five years after I had retired from the Chair in Cambridge. The book treats the three main aspects of structural theory, as indeed do all such books, namely strength, stiffness and stability. This alliterative package does in fact cover the preoccupations of the structural engineer; a structure must be strong enough to carry safely the loads that may be applied – wind, snow, earthquakes, the passage of a train; its deflexions must be small enough not to inconvenience its users – wind on a skyscraper must not cause seasickness, a gantry supporting an overhead cable must ensure that the electric train can continue to receive power; an individual element, perhaps a steel column in a factory building, must not suddenly buckle under the load it is required to carry.

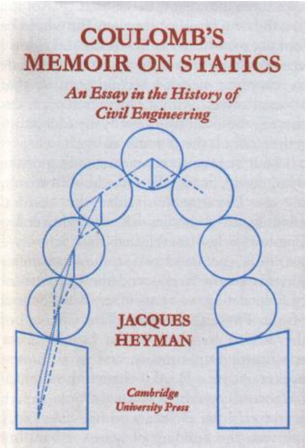


Photo: P. Pattenden

Photo: P. Pattenden

These topics are treated in my historical approach to structural analysis in an unashamedly Whig manner. For example, the analytical study of the breaking of beams starts with Galileo, and moves through the eighteenth and nineteenth centuries until the truth, as it is now seen, is achieved, with interesting but irrelevant byways ignored. The history is not total, but considers only those developments which lead from ignorance to enlightenment.

Head of Department

I became, by accident, Head of the Department of Engineering of Cambridge University in 1983. The University had abandoned the concept of single chair departments, where the professors held office until their retirement, perhaps for twenty or twenty-five years (Inglis, Baker). Instead, departmental heads were limited to five years in office, acting effectively as chairs of committees.

In 1983 there were thirty-two professors in the Engineering Department, half-a-dozen having been elected to established chairs, and the rest promoted from the career grade of Lecturer (or Reader). (I was the first *ad hominem* Professor of Engineering in Cambridge.) It was clear that one of the thirty-two should be appointed for five years to the newly vacated headship. I am not so clear myself, although successive heads since 1983 have all been appointed from within. I do not go all the way with Timoshenko, who stated that such an appointment should always be made from outside, but it would be good to adopt the traditional Harvard appointment process, in which tenured appointments were invariably open for universal competition, with a current holder of such a post having to compete against all comers.

(Timoshenko was a Russian emigré of immense distinction, who held various appointments in universities in the U.S: he was the brother of the General Timoshenko who served with equal distinction in the Russian army during World War II. Stephen (Stefan) Timoshenko had made brilliant contributions in the early 1900s to problems of elastic buckling, and, in the 1930s, he wrote definitive and influential texts on stability, on the theory of elasticity, on plates and shells, and on vibrations, all in the field of structural engineering. He also wrote an extensive history of the subject, with which my own studies of the ‘historical approach’ cannot begin to compete in depth and detail, but Timoshenko's work is confined to elastic solutions, ignoring the insights to be given later by the findings of plastic theory.

It is believed that a Welsh engineer called Timothy Jenkins settled in Russia in the nineteenth century to help establish the railway system.)

A committee was established which worked for six months to determine which of the thirty-two professors should be the next Head of Department. The first stage was easy; sixteen could be rejected out of hand. It was more difficult to reduce the remaining sixteen to eight. After this the real work began, and eventually a short list of four was established, any one of whom would have been a good appointment. Unfortunately it was only at this stage that the committee found that two out of the four had no intention whatever of accepting the appointment – they were distinguished in research, and were completely unwilling to waste their time in the administration of a department which, by numbers of students and staff, was ten per cent of the University, and, by the measure of resources, much larger than that.

This left Arthur Shercliff and myself; the committee recommended, rightly, Shercliff for appointment. He was ten years younger than me, although, I think, still too old to give energetic direction to a very large establishment. So Shercliff became the new Head; he died after two months in office. The committee was unwilling to relive the previous six months, so that I found myself Head by default. I thought, and still think, that five years is too short a time for such an appointment. The first year would be spent in determining the structure, financial and otherwise, of a large Department; in the next three years it might be possible to take some initiatives; in the final year of the five the new Head of Department would be looking over his shoulder at his successor. I insisted that a commitment should be made to reappoint me for a second term, so that I found myself with a ten-year tenure, cut short in the event by a year by my obligatory retirement on the grounds of age.

I had, in fact, some understanding of the relationship, financially and administratively, of a Department with the central administrative bodies of the University. Cambridge had established four Schools – two in Sciences and two in the Humanities (there are now five) – and the Department of Engineering was a member of the School of the Physical Sciences. The Council of the School consisted of the Heads of the constituent Departments, and as an honest broker not yet a Head, I chaired the Council for several years. I was taught by a superb Secretary, John Payne, who had previously overseen the building of a new laboratory on the West Cambridge campus for the Department of Physics, and moved them from their traditional home in the Cavendish Laboratory in the centre of Cambridge. John Payne showed me how to run committees, and how to prepare and master the business of the meetings twice a Term of the full Council. I learned how to negotiate with the administrative officers of the University in 'The Old Schools', and on occasion to represent our needs to the University Grants Committee. So that although the detailed operation of the Engineering Department was still not fully known to me, I did at least have some knowledge of the structure of the University as a whole.

My membership of the School coincided with a period of great financial stringency in the universities throughout the U.K. Cambridge was not exempt, and much of the business of the School was concerned with dividing up the reduced resources made available to us. For example, permission had to be sought to refill a vacant post, and the Heads of Department (robber barons, John Payne called them) had to agree among themselves, in committee and in the monthly full meetings, a prioritised list of needs. I believe we made some progress. At the start of my chairmanship, we were regarded by the Old Schools as merely an advisory body – decisions would be made by the Needs Committee of the General Board of the University. Later the University came to recognise us as competent to make our own decisions.

I was correct in thinking that I was too old for the Headship of the Engineering Department. I had imposed myself for nine years on the Department, and no inspiring leadership would result. However, money was still extremely short, and I felt obliged to launch a public appeal; glossy brochures were produced, and I visited C.E.O.s of several large enterprises. But money was short in Industry too; for whatever the reason, nothing came of these efforts. And I met some ambivalence in the attitude of Industry to universities, and in particular to Cambridge. Cambridge was perceived as unwilling to engage with the problems of the real world; professors and research minded lecturers could not be persuaded to tackle problems which were seen as of great importance by the engineering profession.

This view of academic engineering staff in Cambridge has some substance. The teaching staff is large – more than sufficient to give lectures to classes of 300 or more – with the result that the standard of lecturing is very high, and the lecturers not involved in mainstream teaching can give specialist lectures to final year students, and have more time to pursue their research. It does however arise that there is a teaching need in a particular area, perhaps due to the departure of a lecturer to a chair elsewhere. A post is advertised, and applicants are interviewed; appointment is made on the quality of the publications record of the successful candidate, whose field may not be precisely in the area required. When appointed, the new member of staff will naturally pursue their own line of research, which will, with luck, prove successful, and perhaps distinguished. Such a person is fully occupied, not only in research but also in teaching for colleges, and fulfilling other duties required in a large department, and will respond with a marked lack of interest to any project suggested by Industry. Cambridge engineers will be perceived once again to be indifferent to the needs of the outside world.

There are notable exceptions to this bleak view of the Department's relation with Industry. Some large companies are aware of the needs of academe, and are prepared to provide funds for research in a general area rather than for the solution of narrowly specified problems. Substantial support for equipment, research students and technicians has resulted in activities involving groups of academic staff. But, in general, the Cambridge Engineering Department is a collection of distinguished *prime donne* (or, more numerously, *primi uomini*), who may sometimes form small research units, or who may work beneficially with colleagues, but who are substantially alone. Firm direction from above by a Head of Department is impossible; the Faculty is a great ship, which responds only feebly to commands from the bridge. Its strength lies in the quality of the work done by the crew, and it is here that a Head of Department can really be of help. I had access to large sums of money not scrutinised either internally or externally; if a member of staff (a Fellow of the Royal Society, say) had an important project for which, for one reason or another, funding was not available, then that project could be enabled. I regarded my tenure of the Headship as giving the possibility of enablement, of ensuring that a promising project could be fulfilled.

During the nine years I found that such decisions, coupled with the routine chores of running the Department (and I had an outstanding Secretary of Department, Colonel Hutton) left little time for scholarly work. I continued to be involved with the repair of cathedrals and churches, but I had little time to pursue other interests.

The Cathedrals Measure

It was in the 1980s that the enormous costs of repairing cathedrals forced the Deans to throw in the towel. In exchange for eligibility to receive Government grants, they finally surrendered their autonomy. The mechanism for bringing this about was complex and arcane; the first move was to gain approval from the General Synod of the Church of England. The composition of the then form of the Synod had been established in 1970; it had 580 members, divided into the House of Bishops, the House of Clergy and the House of Laity, and was presided over by the two Archbishops. The Synod has the power to frame statute law on any matter concerning the Church of England – once a Measure has been agreed by the Synod it is laid before both Houses of Parliament, who may ignore it (in which case it becomes law by default) or who may debate it, and accept or reject it, but they can make no amendments.

The Care of Cathedrals Measure became law in March 1991. In summary, the administrative body of a cathedral church (that is, in general, the Dean and Chapter) has to obtain permission to carry out any works or additions to the church (or any building within the precincts that is regarded as part of the cathedral) which would affect its architectural, archaeological, artistic or historic character. Further, possessions vested in the chapter cannot be disposed of by sale or loan without permission. All this is regulated by the Cathedrals Fabric Commission for England, a body of twenty-four members in prescribed categories – I was one of the two engineers. The Commission meets about once a month to consider submissions from the cathedrals.

Submissions are in fact routed through the individual Fabric Advisory Committees established for each cathedral, and which meet perhaps three or four times a year. They are small bodies consisting of three to five members nominated by the Chapter, and an equal number nominated by the main Commission. Deans and their Canons attend meetings of their Fabric Advisory Committee, and present and discuss their proposals, but they cannot vote. There are complexities in the working of the Measure – defining exactly the precincts of a cathedral, for example – and there are some laborious tasks, such as creating an inventory of the (literally) thousands of possessions of a cathedral, but procedures are straightforward. If the Chapter wished to carry out work that materially affected the interests of the cathedral, then it would need the approval of the Fabric Advisory Committee. A permanent alteration needs the approval of the full Commission. On a doubtful matter the local committee might refer the question to the Commission; the Commission might consider the liturgical and aesthetic consequences of the proposal to be sufficiently serious to send a delegation to the cathedral, or they might ask the Dean and the Chairman of the Fabric Committee to make their case in London. As the Engineer for Ely Cathedral for some thirty-five years I attended informally meetings of the Fabric Committee (the Surveyor and Archaeologist are required by the Measure to formally attend these meetings), and after my retirement as Engineer I became a full member for a further ten years. At meetings in London of the full Commission I was sent out of the room when Ely matters were discussed.

In return for this surrender of autonomy by the Chapters, cathedrals became eligible for grants of Government money to help with repairs; large sums were set aside (of course reduced after a few years – five years is the lifetime of a Government), but in fact since 1991 any cathedral with a worthy and well-prepared programme of work has been able to put this in hand with the aid of substantial grants (helped also by the creation of the Millennium Fund).

So, some twenty-five years after Pevsner had written to *The Times*, he had achieved the result he wanted, although indeed he had been dead for a decade. But this still left the untidy anomaly of the Royal Peculiars, which retained ecclesiastical exemption, but were subject neither to the controls exercised by the Church itself over the parish churches, nor to the provisions of the Care of Cathedrals Measure. In 1992 the Government attacked the concept of ecclesiastical exemption, saying that in future it would be confined to those buildings in respect of which there were acceptable internal procedures for relevant works. All other buildings were to be brought within normal local planning authority controls. Westminster Abbey was asked to choose between three alternative courses of action. First, they could agree to be subject to normal listed-building control, exercised by the Local Authority. Second, they could agree to be subject to the Care of Cathedrals Measure. Or third, they could institute some method of control which might be acceptable to Government. The Abbey wished to be neither an ancient monument, nor a cathedral. They chose the third alternative; they transformed their Architectural Advisory Panel into the Westminster Abbey Fabric Commission, modelled closely on the Cathedrals Fabric Commission for England.

I helped to draft the Constitution for the Abbey Commission, and indeed it follows closely the provisions of the Cathedrals Measure. There are however two differences. There is no small Fabric Committee, and the Abbey Commission deals with small matters as well as large. Second, the Dean is Chairman, and four of the twenty members are Canons; all may vote. Despite this partial retention of control, the Abbey continues to enjoy ecclesiastical exemption.

Petra

From very early in our marriage we took two foreign holidays every year – one in winter by ourselves, and, when the children arrived, one with them in summer. Both were short two-week breaks. I had learned to ski in New England, when I was at Brown University; or rather, I had become proficient enough not to scare myself when faced with a steep slope. The age of thirty is too old to master the art. So for several years we went to Switzerland, Italy, France – unfortunately, because of university terms, too early at the turn of the year for good snow, and too late at Easter to catch the last of the season. This went on for several years, and was highly enjoyable; for some reason, when I happened to be forty, we could not go, and we went to Greece for the first time. We instantly gave up skiing.

We spent time in Athens, and explored the Acropolis and the museums, but by Greece we really meant the islands, which had antiquities and also beaches – Crete, Aegina, Lesbos, Santorini, Rhodes, Kos; and other Mediterranean centres – Cyprus, Sicily, Morocco. All this was at a time when these places were not really on the tourist routes, but it was possible to get cheap flights and to hire cars. The last time we had such a holiday was when we spent a week in Jordan. I had been asked by U.N.E.S.C.O. to visit, with an architect, the city of Shibam, then in the People's Democratic Republic of Yemen before the country was unified into a single state. After my visit I flew to Amman, and joined my wife and hired the usual car.

Shibam, which is now a U.N.E.S.C.O. World Heritage Site, has existed for nearly two millennia; it is a walled city of 7000 inhabitants situated on a plateau about 2 m above the level of the Wadi Hadramawt. The wadi floods periodically, and there is no possibility of expansion of the city; residential development has taken the form of skyscrapers up to eleven storeys high. These 30 m skyscrapers are built from bricks made of sun-dried mud reinforced with straw – bricks cannot be made without straw.

The construction of these buildings is massive, and, just as for Gothic cathedrals built of stone, the stresses in the dried mud are extremely low – there is no question of crushing of the material. The great enemy is, of course, water. The roofs must be tight, and the external walls are whitewashed annually with a mixture of lime and wood ash called *ramad* in order to repel rain. The imposition of communist state control had had unfortunate consequences for these houses. First, no one person was allowed to own more than one dwelling; if someone had three, then two were taken over by the state, which neglected the annual weatherproofing. Second, the state had installed running water carried in pipes under the dirt streets, delivered to each of the houses. Pipes always leak, and the first sign of trouble was often the transformation of a corner of one of the houses into mud.

Nor, for some reason, were the ‘old builders’ allowed to exercise their trade. I spent an evening with them, discussing, through an interpreter, their problems. We understood each other immediately. The behaviour of mud brick walls is exactly the same as that of ashlar stone masonry – cracks due to settlement form in the same way, and can be treated by similar remedial work, stainless-steel ‘stitches’ at Ely Cathedral, and thin timber rods in the mud walls of Shibam. Unfortunately the visit of the architect and myself did more immediate harm than good; the visit of ‘experts’ implied that outside financial support would soon arrive, and owners were reluctant to spend their own money on continued maintenance. But U.N.E.S.C.O. has no funds for these purposes; it can merely point to examples of World Heritage, and encourage the arousal of world interest. In the meantime Shibam was in the same severe financial straits as before.

My wife was waiting in Amman, and we made the usual tourist visits. At Petra the local villagers were waiting in a line, each with a horse. I proceeded in a stately way through the gorge, with the villager keeping a tight hold on the bridle; my wife's escort could see at once that she knew how to ride, and let her manage by herself. She was three months from her death, and it was clear to anyone who saw her what state she was in. We have partially lost this sensitivity, but the Jordanians, and above all the children, knew what they were seeing. And, indeed, on our return to England she wished to visit Harrod's sale, which had become something of a twice-yearly fixture for us; the reactions of some of the shoppers were in fact as sensitive as those of the Bedouins of Jordan. Almost her final wish was to see *Don Giovanni* at Covent Garden, and I duly went to the Box Office at 10 a.m. on the day of a performance which had been sold out for weeks. They did not laugh at me, but invited me to become third in a small queue waiting for returned tickets. And, sure enough, a young man soon came in with two seats in the Grand Tier, where I had sat so long ago to see *Die Meistersinger*. The French couple who headed the queue could not believe the price of the seats; the second hopeful believed, but had only a cheque book. I happened to have cash, and since then, despite the advent of credit cards, I have always carried in my wallet money sufficient to buy two Covent Garden stalls. In the Grand Tier we sat next to royalty, and Prince Charles was a little way along, well placed to throw flowers at Kiri Te Kanawa. Colin Davis conducted a good performance.

Panorama - Bonassola | Photo: J. Heyman

In the summer we went for two weeks to Italy, and for the children, for about ten years, this was the end of the school holidays. We had found, through friends, a small village, Bonassola, on the Gulf of Genoa. The region is mountainous and, when we first knew it, the only access by car from the inland Via Aurelia was by a poorly-made track which hairpinned down to the sea. That was the end of the road; the car could only be turned round for the half-hour drive back inland. The next settlement east was the larger town of Levanto, and then came the *cinque terre*, Monterosso, Vernazza, Corniglia, Manarola and Riomaggiore, before the port of La Spezia was reached. Each of the five villages had been established at the mouths of streams cutting ravines in the hills; they could not be reached by car, and access from one village to the next was possible by boat, or by foot on the mule tracks which climbed into the hills and then down again. They were fishing villages, and for a few hundred metres upstream there were vines and olive trees.

The five villages could also be reached by train. In the nineteenth century a heroic feat of railway engineering, of which Timothy Jenkins would have been proud, had been achieved to allow the Paris/Rome express to run round the Gulf of Genoa a few metres from the sea. Most of the track was in tunnels through the mountains, emerging briefly at Bonassola, Levanto, and each of the *cinque terre*. Some years we drove, and made a leisurely journey part of the holiday, stopping overnight where we could in Italy, Switzerland, France and Germany. But we also sometimes took couchettes from Paris to a main station on the Ligurian coast, where we changed to a local diretto stopping at all the villages. The local train tried to keep to time, but the line through the tunnels was single-track, and very often we waited in sidings in one of the villages until the express had gone through.

In the Cinque Terre | Photo: J. Heyman

We knew no Italian on our first visit to Bonassola – but we knew that if our carefully spoken English was not understood, we could always fall back on French. In fact the locals spoke only Italian, Genovese dialect. In the autumn we registered at the Tech in Cambridge (now the Anglia Ruskin University), putting ourselves down, with confidence, for the second-year Italian class. Our arrogance was rewarded; the superb teacher, Signor Petoello, a retired Lector of the University, had no confidence in the first-year teacher, and in the first three lessons he rehearsed the whole substance of the previous year's course. We were diligent students, and by the end of the year we had mastered the language; we had no vocabulary, but a good understanding of archaic Dante.

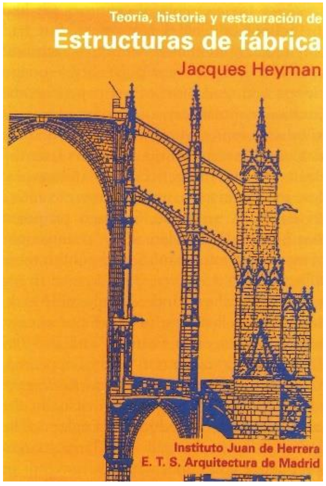
At the end of the year Petoello entertained the class of a dozen at his home, and played us gramophone records. It was the first time that I had heard *Nessun dorma*, and the entire class demanded an immediate replay.

Santiago Huerta Fernandez

The brief visit to Jordan was the last holiday we took together. Our two breaks in each year were usually spent in Italy or Greece, although we went on occasion to the U.S. (New York and Martha's Vineyard). We had no inclination to visit Iberia – we never considered going to either Spain or Portugal. The conferences I attended were at that time held in Italy or France, and sometimes in Germany or England. There were, and are, a few hundred people in the world interested in technical aspects of the design and construction of old buildings, and about a hundred of these would attend a three-day programme of lectures and presentations. I did not really like these meetings, but my attendance renewed my friendship with many people. And I got to visit some of the larger opera houses of Europe.

It was at one of these conferences in the last decade of the twentieth century that I met Santiago Huerta. He introduced himself as a young professor at the School of Architecture of the Polytechnic University of Madrid. The School itself had been in existence for a couple of centuries (as had the School of Civil Engineering), but had only recently combined with other technical schools to form the new University. Santiago Huerta had read most of my papers on the mechanics of masonry, and professed himself to be an admirer of my work. This is some sort of understatement; a few years later the Dean of the very large School of Architecture described Santiago to me as my representative on Earth. In any case, Santiago proposed that he should translate some twnty-eight of my papers into Spanish, and publish these collected into a volume. This seemed to me a monumental and indeed impossible task, but Santiago is a man of enormous energy, with no hostages to Fortune, and in 1995 the volume *Estructuras de fábrica* appeared, a year before *Arches, Vaults and Buttresses*, which contains twenty-six of virtually the same papers, was produced in England. The Spanish version outsold by a large margin the English collection, which is now out of print; by contrast Estructuras de fábrica is now Vol. 1 of a two-volume work, since Huerta in 2015 made further translations of twenty-seven of my papers written between 1993 and 2014. At the same time his press published in English a volume containing twenty-five of these papers.

Santiago Huerta is the greatest scholar of technical architectural history that I know. He is an architect, and he has a complete grasp of the application of structural mechanics to the analysis of ancient structures, and of the history of the development of structural engineering. He has invented the new discipline of ‘Construction History’, which covers not only architectural design and structural analysis, but also the politics and economics of building works, the use of machines and labour, techniques of construction, the properties of materials and so on. Any conference held today, and there is at least one a year, will have sessions on most of these topics, and will be attended by delegates from all over the world. Above all, Santiago Huerta will deliver a keynote lecture.



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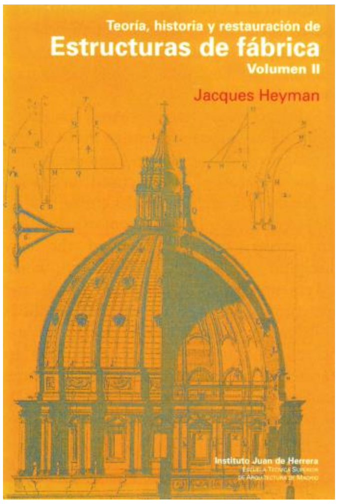

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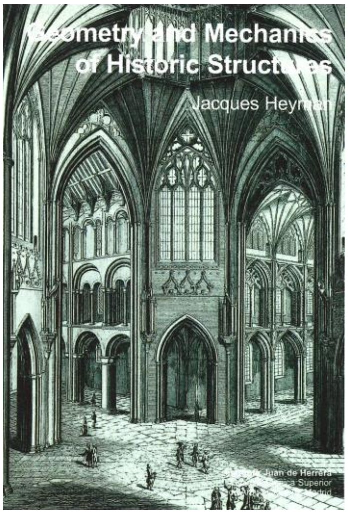

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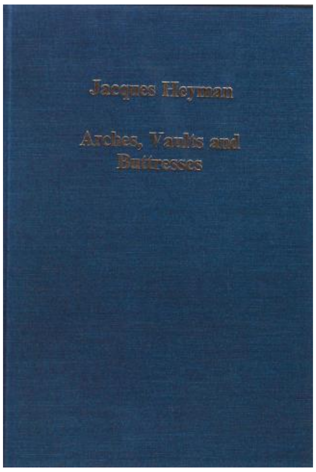


Photo: P. Pattenden

And, to fulfil his Dean's pronouncement, Santiago Huerta has invented me. He has translated into Spanish six of my books (and a seventh is in the press), and these have again sold better than the English originals. It is due to Huerta that my name is known (to a rather small circle of scholars) in many European countries, in contexts other than solely in the repair of churches.

Santiago has not confined his extraordinary activity to publishing and to academic research. He has taught to graduate classes in Madrid the theory and practice of the analysis and repair of ancient structures, and he acts as consulting architect to some of the major cathedrals and churches in Spain. And indeed, Santiago showed me something of his country, from the high vaults of Segovia Cathedral covered six inches deep in pigeon guano, to the tremendous cathedral of Palma de Mallorca, enriched at the east end by the fantastic decorations of Gaudí, to Gaudí's own work in Barcelona of the Sagrada Familia. Robert Hooke, in 1675, had published a key statement of the equilibrium of masonry: As hangs the flexible chain, so, but inverted, will stand the rigid arch. Gaudí's incredibly complex arrangements of hanging chains, preserved and reconstructed in the crypt of the Sagrada Familia, show that Gaudí understood completely that these chains, inverted, would give the incredibly complex architecture of his cathedral. I came to wish that I had visited Spain earlier in my life.

Gaudi’s Sagrada Familia, Barcelona | Photo: Alvesgaspar, CC BY-SA 4.0, via Wikimedia Commons

It was of course Santiago Huerta who secured my Doctorate *Honoris Causa* at his university. The one-hour ceremony can, extraordinarily, be seen on the internet, and includes my thirty-minute discourse. I was allowed to keep the ceremonial robes, but have not yet had the courage to wear this fancy dress on a Scarlet Day – although Cambridge decided half a century ago to recognise degrees from other universities (they had always recognised those from Oxford).

Scholium

I have spent my life in academe, but quite early on, while still a Lecturer, I became a Chartered Civil Engineer. My brief excursions into Industry, with Shell, and in Victoria Street with Scott and Wilson, qualified me for acceptance as a Member (at that time Associate Member – there has been a grade drift, and I am now a Fellow) of the Institution of Civil Engineers. I was able to make designs (of steelwork and indeed of reinforced-concrete structures) which could be accepted by Local Authorities. For small buildings (and I made several designs for laboratories and offices for the Welding Institute) I would make drawings, accept tenders from contractors, and oversee construction. For larger projects, the final design would be passed to an established firm of consulting engineers, as for example the plastic designs for buildings at the Engineering Laboratories at Cambridge.

This way of working carried over to my involvement with masonry. My very large first project, the repair of the Great West Tower of Ely Cathedral, relied heavily on the expertise and organisational capacity of the London consultants R.T. James and Partners. Work to small medieval bridges, however, was designed and overseen by myself alone, although in general overall responsibility lay with an architect. And certainly for repairs to a cathedral, I would work closely with the Cathedral Architect to whom I was giving advice.

To ensure the confidence of my clients (for example, Deans and Chapters), I found it necessary to carry Professional Indemnity Insurance. The premiums were heavy, corresponding to the total sum insured – several million pounds, a total in fact rather smaller than the damage I could do by giving poor advice, although this was never tested. On my eightieth birthday I decided to retire, and found that I could pay a single enhanced premium to cover all my previous work – a sort of Papal Indulgence to absolve all my past sins, on condition that I committed no new ones.

I very much missed this involvement with the practical world – both for its compelling interest, and also because I found that, no matter how small the project, some problem arose that I had not seen before, which led both to interesting work of repair, and also, often, to a published academic paper. I did not leave completely the world of masonry – I stayed for another decade on various committees, prepared from time to time to address for an hour or so the agenda laid on the table.

In the University (Cambridge, with brief but significant excursions to Brown and Harvard) my research had been in four related but distinct areas. I was Baker's last student, and, as I have related, I worked on problems connected with the design of steelwork, from cowsheds (I did a simple farm building for the Queen at Sandringham) to skyscrapers, at a time when Baker had overcome the hurdles of acceptability of the method. The research was meaningful, but of the nature of extensions to an established technology, rather than of innovative advances. It is ironic that in Germany, for example, I am regarded as having been, with Baker, the creator of the new method, whereas in reality the credit is entirely due to Baker. It is ironic in another sense, in that Baker had not yet learned of the fundamental theorems which had been exposed by Prager. Baker's greatness lies in the fact that he had grasped absolutely the importance of plastic ideas, and that he had the determination and drive to impose those ideas on the structural engineering profession.

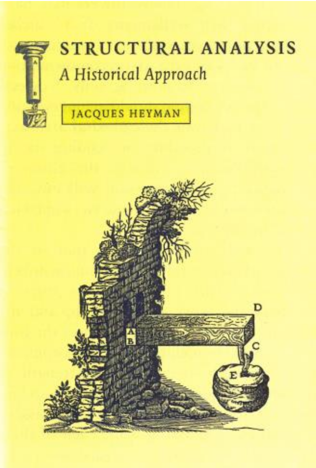
My second area of research was, of course, the application of these new ideas to the analysis and repair of ancient structures, particularly those constructed from masonry. I really do believe that my papers, and my book *The Stone Skeleton*, have had a profound influence on cathedral architects, and on others concerned with the care of old buildings. Cracks are no longer frightening, but serve as clues as to what might be their cause; deformed arcades are testaments to movements that have ceased long ago; piers supporting massive towers may have completed their settlements of 12 inches within a dozen years of their construction.

In all such cases the first recommendation for action is to do nothing; only if defects seem to be developing should an intervention be necessary. It was the cathedral architects who were responsible for sending me to Lambeth Palace to receive the Cross of St Augustine from Rowan Williams; the honour ranks, I would guess, somewhat below the M.B.E.

At Lambeth Palace, 2005 | Photo: J. Heyman

My third interest became that of the history of structural engineering. Schofield had inspired this interest by his proposal to study the work of Coulomb, and my commentary on his *Essai* explores the four concerns of the eighteenth-century engineer – the strength of beams, the strength of columns, the design of (masonry) arches, and the forces engendered by a mass of soil against a retaining wall. Each of these topics is treated in a separate section in my book on Coulomb; his contribution is evaluated, and antecedents are traced forward until recognisably modern statements are reached. This sort of study is, as I have said, pure Whig history – the steps in the intellectual chain are followed until the known final result is reached, with interesting but irrelevant excursions into side issues totally ignored.

There have been several masterly treatises on the general history of the theory of structures, and all suffer from this same Whig approach. In these accounts the known final result is the evolution of a method of analysis and design which gives an elastic state of a structure, a state which I now know to be but one of an infinite number of possible states, and moreover one that can never be observed in reality. So I wrote my own Whig history, *Structural analysis: a historical approach*, which is based on the rejection of elastic behaviour as a tool of analysis, but instead explores the use of equilibrium (or plastic theory) as a tool of design.



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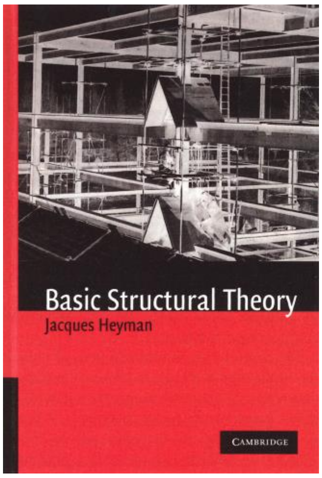

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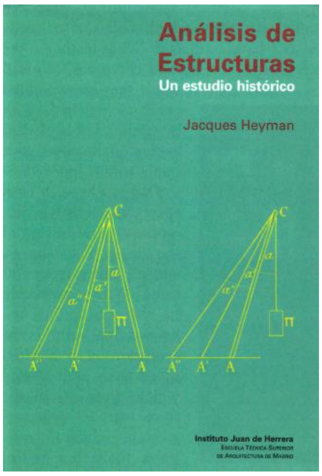

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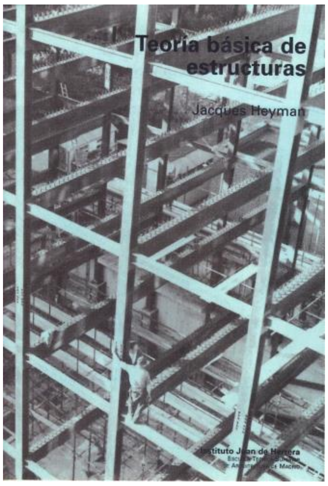


Photo: P. Pattenden

And, finally, I have tried to record my own understanding of my subject in my book *Basic Structural Theory*. Elastic ideas no longer form the main toolkit for the overall design of an engineering structure. They are necessary for the prediction of unstable behaviour – buckling of a steel column for example – or for the estimation of the deformations of a building frame, but they are of no use in predicting the ultimate strength of a structure. For that plastic theory is necessary.

Envoi

The purpose of a cowshed is to provide protection for the cows, and the design of the structure of the shed will ensure that it does not leak, that it is not too hot or cold, and so on. Above all, it must be strong enough not to fall down under gale force winds or under heavy snow. The aim of the theory of structures is the analysis of the shed to ensure that the shed is indeed fit for purpose. To this end it seems reasonable to enquire into the values of the internal forces in the shed engendered, for example, by snow on the roof. Once these forces are known, that is, once the *state* of the structure has been calculated, it would seem a simple matter to ensure that the steel joists, the reinforced-concrete columns, are strong enough to ensure safety.

It has been made clear throughout this essay that the question: What is the state of a structure? cannot be answered and therefore cannot be asked – there is an ephemeral state here-and-now for the four-legged table, but this is one only of an infinite number of such states. In Donald Rumsfeld's classification this is an example of a known unknown – we know that we do not know the values of the forces in the four legs of the table. But the problem is much worse than that envisaged by Rumsfeld; it would seem implicit in his statement that once it has been realised that something specific is unknown, then diligent enquiry will reveal the truth. But in the case of the structural analysis of the table, the unknown is unknowable; unique and definite values cannot be assigned to the forces in the table legs. (Nevertheless, the use of the so-called plastic theory will provide a way to ensure that the cows are comfortable.)

It is of interest that modern philosophical thought seems to have run up against the same barrier of unknowability, but in this case there is nothing analogous to the plastic escape route available to the structural engineer. Bryan Magee, for example, reaches the conclusion that there are no answers to the fundamental questions pursued by philosophers for hundreds if not thousands of years. He regards Locke as *the* empiricist among philosophers, Hume as *the* sceptic, and Schopenhauer as *the* pessimist; Magee would wish to be known as *the* agnostic, acknowledging in the fullest way our ignorance of the answers to the greatest questions. His simple analogy is the complete impossibility of explaining colour to someone blind from birth.

These ‘philosophical’ considerations may seem unimportant for the world of structural engineering, but there is a very real pedagogic problem. How is the discipline of the Theory of Structures to be taught to an undergraduate class? For two centuries universities have established courses of instruction which lead to statements about the state of a given structure in response to given loading. How are students to be warned that such calculations lead to states which are certainly possible, but which can in reality not be observed?

The problem is made more difficult by the fact that official building Codes betray no doubts about the existence of unique states; the internal forces in a structure must be calculated, and they must satisfy relevant requirements of the Code. Structural design in practice depends, implicitly, on elastic analysis; numerical values are generated which are available to satisfy the various Clauses of the Code. This is the real world in which new graduates will find themselves, and they must be equipped with mastery of the traditional methods of calculation. The teaching of structural engineering is almost compelled to start from considerations of elastic behaviour, which will, in any case, be needed eventually for the estimation of deflexions and for the analysis of unstable behaviour (the buckling of columns, for example). But this classical approach will lead to analyses which give poor indications of the strength of the cowshed.

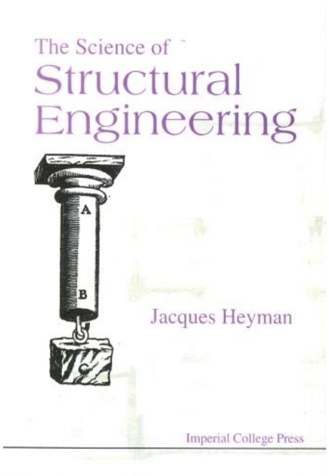
I do not know how these doubts can, or indeed should be, conveyed to students. And, in any case, the problem is very much wider. The subject of structural engineering (or of any other engineering course in a university) will be taught with the intention of exposing its scientific basis; there will be little room to discuss matters of practical importance. Professional training is acquired after leaving the university, in the traditional master/pupil relationship in Industry. The examinations set by the university have only marginal relevance to the acquisition of this practical expertise; the syllabus is ‘mandarin’, and the students are required to jump through a set of hoops, very much like those which had been jumped to get to the university at all. The student's brightness has already been established, and there is really no need of confirmation.

These considerations also arise in other professional schools. In debates as to whether or not universities are the proper places for vocational training, it is often unnoticed that, for engineering at least, many universities do not actually give vocational training. A university is a home of scholarship, of learning and research, and its teaching is inevitably directed to the creation of scholars who will carry on these activities. This is clear in such subjects as History or English; some of the top quarter of students who get first-class degrees may in turn eventually teach in universities, having first spent some time in research, and a few may teach in schools, but by far the greatest number of graduates in these subjects will enter professions where little direct use is made of what they have been taught.

This pattern is also true of the engineering schools in universities, but with one difference. Engineering courses serve to determine the next generation of engineering scholars. Some of those who pass the examinations, the top quarter of the students, may go on to do research, and may hope one day to advance the science of engineering. The other three quarters, who, to judge harshly, have failed the tests – that is, the typical products of the university – go on, in the case of engineering, to practise engineering. This is the apparent difference between a professional school and an arts school such as history.

Of course, three quarters of the students have not really failed the tests – it is the university examinations which have failed, by not being directed to assess the abilities of the intending professional engineer. There is an urgent need to reform both the teaching and the examination of engineering students, and there are probably several ways of doing this. One such move was made in Cambridge in 1992, just after my retirement (although I had steered the changes through the University); the traditional three-year course, of the kind which leads to a Bachelor's degree, was replaced by a four-year course. The Bachelor's degree is still awarded after three years, but successful completion of the fourth year leads to the degree of Master of Engineering. However, these changes were mainly in response to external pressures; students were less well prepared at school than in the past, and Europe did not recognize three years at a university as proper professional training. The first two years of the new course are still broad, with students being examined in a wide range of subjects.

The second two, however, permit specialization, with the mastery of fairly deep theory; the aim is to give the potential scholar and also the potential professional engineer a deeper understanding of some branch of engineering science.



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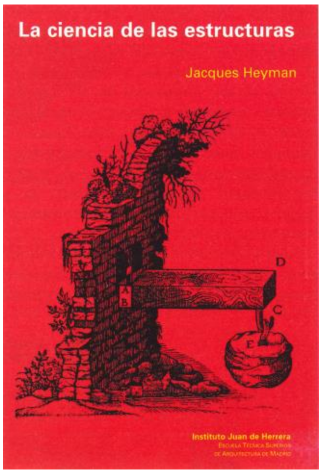

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There is indeed a body of knowledge called engineering science which is different from the knowledge required to practise as an engineer, but they overlap and one is essential to the other. The science of engineering must be taught in universities, but the evidence seems to be that there is a large number of clever students who do not, one way or another, really wish to master this science. This is a view from within a university, and is perhaps contentious.

Viewed from outside, it seems that Industry needs a large number of clever engineers, but not necessarily engineers deeply versed in the science of their subject. Attempts to instil this deep science, of which little or no use will be made, are wasteful. The basic engineering knowledge to serve the intending professional engineer can be taught in two years. The teaching programme for such a two-year course requires very careful planning, with every precaution being taken to maintain the students' awareness of the relevance of what they are being taught, but at the end of the two years they will have a firm foundation on which to base the practical skills learned in Industry. It is from the students in the second two years of a four-year course that the next generation of 'scholars' will come.

Lessons can be learned from history. The medieval professional route, for a stonemason for example, was from apprentice to journeyman to the career grade of master. A few of these masters could progress further by being taken into the ‘office’, where they learned the deeper ‘theory’ of their trade, and they could finally take charge of a major project. There is a case for following such a programme for the training of today's engineers. The two-year basic university course is the ‘apprenticeship’; the period spent in Industry acquiring relevant practical skills is the ‘journeyman’ phase; acceptance by one of the Institutions as a Chartered Engineer marks the attainment of the career grade. If they so wished, some Chartered Engineers could return to University for a second two years, where they would master the advanced theory so necessary for a deeper understanding of the professional work in which they are engaged.

Such a radical programme would require a massive upheaval in the tertiary education system in the UK, and there would be many objections. Certainly some of these would be voiced by students; if a four-year course leading to a Master's degree were on offer, there might be reluctance to settle for an ‘inferior’ two-year course. However, if the University decided that two years would qualify for a Bachelor's degree, and if the engineering profession would accept that such a degree, followed by appropriate practical training, would lead to Chartered status, then very many students might decide to enter the real world as soon as possible. They could return later for two years at university to prepare them for leadership of their profession.

Until his retirement Jacques Heyman was Professor and Head of the Department of Engineering at the University of Cambridge. He is the author of fourteen books and co-author of two others, and has written numerous articles on the plastic design of steel structures, masonry construction, general structural theory, and the history of structural engineering. He is a Fellow of the Society of Antiquaries, of the Institution of Civil Engineers and of the Royal Academy of Engineering. He has been the Consulting Engineer for the repair of a large number of English cathedrals and churches. Professor Heyman was for many years a member of the Architectural Advisory Panel for Westminster Abbey, and of the Cathedrals Fabric Commission for England.

Text of the original version published in the *Peterhouse Annual Record*, 2014/2015, pp1-92, which also includes further images.

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1. Books

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12 *Teoría, Historia y Restauración de Estructuras de fábrica*, Madrid (Juan de Herrera), 1995. (A collection of 28 articles on masonry construction translated into Spanish.)

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2 Tests on miniature portal frames, *The Structural Engineer*, Vol.28, no.6, p.139, June 1950 (jointly with J.F. Baker).

3 Extensions of the simple plastic theory to take account of the strain-hardening range. *Proc. I. Mech. E.*, Vol.165, p.189, 1951, (jointly with J.W. Roderick).

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5 The plastic design of beams and plane frames for minimum material consumption. *Quart. Appl. Math*., Vol.8, no.4, p.373, January 1951.

6 Elasto-plastic stresses in transversely-loaded beams, Vol.173, *Engineering*, pp.359, 389. March 21 and 28, 1952.

7 The limit design of a transversely-loaded square grid, *Journ. Appl. Mech*., Vol.19, no.2, p.153, June 1952.

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