BIOGRAPHICAL MEMOIRS

John Fleetwood Baker, Baron Baker of Windrush, 19 March - 9 September 1985

J. Heyman

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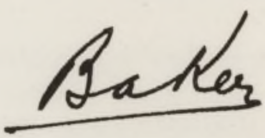

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Elected F.R.S. 1956

By J. Heyman

Introduction

John Fleetwood Baker returned to Cambridge in 1943, as Professor of Mechanical Sciences and Head of the Department of Engineering, and as a Professorial Fellow of Clare College, where he had been an undergraduate. In the conflict of interests, sometimes trivial, sometimes painful, between 'University' and 'College', Baker was clear, at least officially, as to where his duty lay. His staff was large—Engineering forms one tenth of the University of Cambridge, and consumes proportionately more than one tenth of its resources—and his staff were in demand, as men of affairs, for various College offices. He pretended to deplore this, and would occasionally, in the Departmental Common Room, rail against the College system, which seduced his lecturers away from their tasks of teaching and research to the time-consuming offices of Bursar or Tutor. In fact, when a young lecturer—and all his lecturers were, to Baker, young—when a young lecturer would summon the courage to confess that a College was proposing an appointment of this sort, Baker was not altogether displeased. He took immense pains with the appointment of his staff; he was quick to terminate employment of those who did not meet his high standards, in a way that is not possible in these more bureaucratic times; and he gave unstinting support to those he thought of as his team. If one of these should catch a College's eye, it was only confirmation of the qualities of those in his Department.

In 1943 the Engineering Department was staffed by 24 lecturers, and there was one other chair. In 1986 there were 120 teaching staff, of whom 15 were professors, and this growth was fostered by Baker during his 25-year tenure of the Headship. Of course the expansion was part of the

general explosion of the physical sciences in the period after World War II, and Baker found himself paying a price for his own severe criteria for appointment to his staff. The function of a lecturer was to teach, and, because it was engineering that was being taught, the candidate had to have served his time in industry so that he could inform his teaching with the real blood of engineering rather than the dry dust of academe. Above all, the candidate should be willing and able to do research. Members of staff who met these criteria were what Cambridge needed, and Baker treasured them; they were also what the expanding universities needed, and as fast as Baker recruited his staff, so they were promoted to chairs round the country and abroad. Indeed, Baker was consulted in these matters, and there were, in the 1950s and 1960s, few appointments made in the engineering departments of this country without Baker's advice.

Baker took immediate steps in 1943 to provide facilities for research in his Department. By 1944 and 1945, when a return to normal life after the war could be foreseen, a complete review of the teaching programme was made. As a result the two-part Tripos came into being in 1946, and remained virtually unchanged throughout Baker's tenure of the chair. Baker was on the one hand following the academic precepts of his predecessor, Inglis, and on the other he was revitalizing the syllabus to accommodate the increasing demands of technological change.

In all this, as indeed in all innovative action in Cambridge, Baker had to carry the Faculty with him. The University is the most democratic of institutions and appears to have no levers of power readily accessible. Its Faculty Boards, however, respond to leadership, and the fact that a revised Tripos structure, for example, had to be argued step by step through the Board ensured that, at the finish, the new venture would be embraced with enthusiasm.

An example is the extraordinary statement of needs prepared by Baker and agreed to by the Faculty Board very early in 1944, four or five months after Baker had taken office. In addition to the complete review of the Tripos, the Faculty Board's report proposed the introduction of post-graduate courses, an increased emphasis on research, and increased facilities for research students. The report proposed a near doubling of the teaching staff. The report proposed new laboratories, new workshops and new lecture rooms. It was in fact a blueprint for the development, on an unprecedented scale, of teaching and research in engineering in Cambridge. Baker saw to it that every single objective, however seemingly unattainable, was in fact amply attained.

The construction of the first postwar buildings shows clearly how Baker went about things. The 1944 programme had been submitted to the University, but it was known that no government funds would be available for building in educational establishments until 1947. However, by 1945 Baker had persuaded the University to appoint an architect. As a result, when resources became unexpectedly available in 1946, the Engineering Department, alone in the University, had plans ready, and

building of the new workshops started immediately. In the same way, complete proposals for the whole site were ready by 1948, and a year later the main new block was started, the block to be opened by H.R.H. The Duke of Edinburgh in 1952, and known now as Baker Building.

In such matters Baker always thought of himself as a lucky man, one on whom chance had bestowed favours. Baker was a good mathematician; his first book was on differential equations, and he had a keen interest in probability, in the distribution of floor loads in office buildings, for example, or the wind forces on skyscrapers. He knew that of 10000 aspiring and equally talented newsboys in the U.S.A., only one would by chance become a millionaire. He knew that the Gothic cathedrals we see are the ones that have not fallen down. In the same way he reckoned that at various turning points in his life chance had intervened to nudge him this way rather than that, that he had been available for a particular post at a particular time when perhaps many others could have filled it as well. This modest view, and modesty is perhaps not the first attribute that springs to mind when trying to describe John Baker, this modest view of the role of chance must of course have some substance, but there is an enormous mass of evidence, such as the 1944 Faculty Board blueprint, which makes it difficult to accept that Baker would not have got on whatever the odds.

Early life

The Bakers came from the Cotswold village of Windrush and, for three generations at least, most of the men were professional gardeners, several of some distinction. Baker's great-grandfather, Thomas Baker, of Windrush, was head gardener to Lord Sherbourne, and Thomas's elder son, George, was head gardener to Lord Revelstoke in charge of the gardens at Membland near Plymouth (George's grandson later occupied the same position).

Thomas's younger son John (1838–1924) was Baker's grandfather, and it was John's elder son Joseph William (1872–1958) who married Emily Caroline Fleetwood (1874–1952), and who were Baker's parents. Grandfather John was Secretary and Director of R. P. Kerr and Co. Ltd, Seedsmen, of Liverpool; father Joseph William had a short career in business and then turned full time to etching and water-colour painting.

Baker was born on 19 March 1901 at Liscard, Cheshire, and he grew up in the Wirral in a happy, comfortable and hospitable middle-class home. His father's interests centred on his painting and his artist friends; his mother was more practical. There seem to have been no scientific or technical contacts, and neither parent appeared ambitious for Baker or his older sister—it was tacitly assumed, however, that they would do something with their lives. They were given the best possible education and the parents made some sacrifices to this end.

Baker went at the age of 6 to a girls' school (Sea Bank High School)

but was already acquainted with some of the syllabus; his sister had started at the same school two years earlier and taught him day by day what she had learnt. A year later he went to a private school (Liscard High School) where the standard of teaching was high, if unorthodox, and Baker progressed sufficiently well to win an entrance scholarship of £40 a year to Rossall School. This was in December 1914, but Baker joined the school in the summer term of 1915.

Discipline at home and at his previous schools had been easy, and Baker found Rossall something of a shock. Intellectual and ethical standards were high, but there was nothing gentle or gracious about the school, particularly with the increasing rigours of World War I. Teaching suffered from the departure of masters to the army, but the standards in mathematics remained high, and Baker apparently received an excellent grounding in the subject. Indeed, in his penultimate year he took all the mathematics prizes and thought himself to be a real mathematician; certainly he was awarded, in 1919, a £60 Open Mathematical Scholarship to Clare College. However, in that final year at school, after winning his scholarship and before starting at Cambridge, he was overtaken by a younger boy whose brilliance convinced Baker that his future did not lie in mathematics.

He somehow determined to be a civil engineer, although neither his family nor the school apparently knew much about the profession. His father was advised that the route was not through a university but through a pupillage, but he was determined that his son should go to Cambridge. It was only after his arrival at Clare College that Baker found that, as a Scholar in Mathematics, he would in fact be allowed to read Mechanical Sciences; he took the Qualifying Examination (in mathematics and mechanics) and joined the newly devised first-year course.

In October 1920 the Engineering Department was crowded with returned veterans of the war, but the new course seemed to be directed more to training newly returned lecturers. In any case the work was not demanding, and Baker found time to read widely in other subjects, particularly French literature. (A language, as well as divinity, Latin and history, had been required subjects at Rossall right through the school.) The teaching at Cambridge was much as it is now; some lecturers were brilliant (for example, Baker's professor, Inglis), and some were dull. Baker took firsts in all three years; he was good in all subjects, but especially in the theory of structures.

He was not inspired by his three years at Cambridge. Engineering appeared to be an immensely complex profession, where experience was to be gained by starting modestly at the bottom in some industrial organization; the knowledge that would lead one eventually to the top would be gathered only slowly. There appeared to be no research on view in the Department at Cambridge, and Baker was scarcely aware that such an activity existed. He looked, then, for an industrial appointment,

perhaps with a chemical firm such as Brunner Mond (ICI was not yet in existence), to set him on the right road. In 1923, however, jobs were difficult to find, and Baker was unemployed for six months (he refused some good administrative posts because he was determined to become an engineer). Finally, he accepted on 1 January 1924 a temporary post funded by the Air Ministry to investigate structural problems of airships.

Work on airships

Baker was appointed as assistant to Professor A. J. Sutton Pippard, who was then at University College, Cardiff. (Pippard later moved to the Professorship of Civil Engineering at Imperial College.) His job was to design and build a braced tubular framework hexagonal in cross section (i.e. a primitive airship structure), to load it, to observe the strains and so to check the Airship Stressing Panel's design rules. The conditions of work were about those of a modern Ph.D. student; Baker was left much to his own resources at Cardiff, but with an interested supervisor, Sutton Pippard, in the background.

The work went well and Baker transferred to the Royal Aircraft Works, Cardington, in January 1925 as a regular member of the design staff. Some of the work was experimental, some theoretical. Baker was responsible particularly for the stress calculations, notably for the main transverse space frames. The structures were exceedingly complex and, in a technical sense, highly redundant; strain energy methods were used to make the elastic calculations, and these calculations were very heavy in an age not yet blessed by the electronic computer. Moreover, structural theory itself had not been developed sufficiently to be applied with confidence to the problems being tackled, and Baker had a difference of opinion with R. V. Southwell, consultant to the project, about the validity of some of the assumptions. Specifically, Baker questioned the applicability of St Venant's assumption, and asked to be allowed to make tests to verify the matter.

Permission was refused, and Baker resigned to make his own tests. However, he had to earn a living, and he returned to Cardiff in 1926 as an assistant lecturer under Pippard. (The two retained a close personal and academic relationship through the years; Pippard & Baker, *The analysis of engineering structures* (2)*, first published in 1936, has run through many editions.) Baker used his spare time to carry out tests on his primitive structure, and the results were published by the Royal Aeronautical Society (12, 13). These papers give a first indication of the intense work Baker was prepared to put into his pursuit of knowledge. The mathematical work was heavy, and the experiments painstaking; moreover they showed that, for the particular structure under

* Numbers given in this form refer to entries in the bibliography at the end of the text.

investigation, Baker was correct. However, the difference with Southwell did not prevent Baker's return to Cardington each vacation to work at the R.A.W.

The objective of the whole exercise was the determination of elastic stresses, and the criterion for success of the design of the structure was that it would be known to be in a safe state of stress under service conditions. The team engaged in the investigation worked long and hard, and problems of seeming drudgery were enlivened by the enthusiasm of those involved. A lesson was taught here that was not forgotten by Baker—principles and theory are not enough, but must be supported by tedious and sometimes back-breaking work. His own teams, later on, were made to serve an apprenticeship of hard problem-solving, and these later teams, just as the team at Cardington, were inspired by the excitement of the whole operation. This pattern continued until 1928, the year of his marriage, when Baker was due to take up the post of Technical Officer to the Steel Structures Research Committee (S.S.R.C.), of which both Inglis and Pippard were members. However, at Christmas of that year he fell seriously ill with a lung infection, euphemistically called Vincent's Angina, but almost certainly tuberculosis. There was no improvement after three months and the next six months were spent in a sanatorium in north Wales. Baker was discharged when he was no longer able to pay the fees, and was told he would never be able to work again. Necessity drove him back to work in January 1930, and by the middle of that year he was shouldering the full responsibilities of Technical Officer to the S.S.R.C. Very few knew, in later life, that Baker had been written off in his twenties.

The Steel Structures Research Committee

Steel-framed structures had been in existence since the turn of the century, and by the late 1920s there was a multitude of building regulations in use throughout the world. The steel industry set up the Steel Structures Research Committee to try and bring some order into this chaos, and one of their first acts was to patch together a 'Code of Practice' from existing data. The Code was meant to be a temporary expedient, but it was accepted at once by the London County Council, and formed the basis of British Standard 449, a standard whose ghost, nearly 60 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 the government research stations. Such a high-powered committee can only make progress if it is properly serviced, and it was Baker's job, month by month, to assemble technical information, to write papers developing theory, to oversee the collection of experimental evidence—in short, to make sure the work of the Committee was prosecuted. The three volumes recording the findings of the Committee, published between 1931 and

1936, are a monument to the profession of structural engineering and to some of its eminent members, among whom may be singled out, perhaps invidiously, Oscar Faber and Professor Batho. Above all, however, the volumes are a monument to Baker, who wrote a great deal of the text, either by himself or in collaboration with others (14–17, 20–22, 25–32). Baker's contributions were recognized immediately; he was appointed to the chair of civil engineering at Bristol in 1933, at the age of 32, and he was awarded the Telford gold medal. He continued to work for the Committee for another 3 years.

Some of the experimental work of the Committee was outstanding, in scale and in its results. New steel office blocks were being constructed in the 1930s; for the first time, stresses on site were actually measured. The results were, to say the least, disquieting; the quantities observed on site bore almost no relation to the quantities being calculated by the designers. The Committee rapidly concluded that 'the method of design inherent in the [patched together] Code of Practice [the Code still in use today] was almost entirely irrational and therefore incapable of refinement'.

The Committee were intent, however, on making progress. They discovered what was wrong with the design process to produce such a marked difference between predicted and observed behaviour. They launched a painstaking programme of theoretical and practical research to determine the real behaviour of a steel structure, and it is this work that is recorded in the three monumental volumes. Finally, in their *Recommendations* of 1936, the Committee proposed a rational and economical method for the design of large-scale multistorey steel frames.

Heureux qui, comme Ulysse, a fait un beau voyage. It must be very satisfactory, at the age of 35, to have completed a major scientific investigation, to have published the results, to have been awarded the premier gold medal of the Institution of Civil Engineers, to have been appointed a professor in a major university, and to have given engineers a new way of designing the steelwork for the skyscrapers of the future. The new method was, to be sure, complex, at least when measured against the utter simplicity of the irrational Code of Practice, but it would be sure to make its way in time because of its rationality and its inherent economy.

It must have been all the more upsetting for Baker, at the age of 35, to realize, even as the final report of the Steel Structures Research Committee was being written, that he had essentially wasted his time for the last 8 years. A small steel-framed cinema was under construction in Bristol. The Committee's new method could deal with the steel skeleton in its most simple form of regular arrays of columns and horizontal beams, that is it could deal with great office blocks and with skyscrapers. It could not deal with the curved girder of the cinema balcony, or with the light steelwork of the projection room. What had been produced was a method for handling one particular type of structure—a rational universal method for steelwork design was as far away as ever.

The 8 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. And, most importantly, it was seen that an end of a road had been reached. There was no point in refining still further elastic methods of design; the real world of the cinema balcony was too complex for there to be any hope for traditional design methods. But there was as yet no signpost to the way forward.

Plastic theory

The Institute of Welding had been interested in the work of the Steel Structures Research Committee, and Baker received support for his later work from the Welding Research Council and, from 1946, from the newly formed British Welding Research Association. Indeed, the establishment in 1946 of a research institution at Abington, seven miles from Cambridge, having close contacts with Baker and the Engineering Department, is a very early example of what is now known as the Cambridge Phenomenon.

In 1936 only very small sums of money were available, but the Institute of Welding provided funds to employ an assistant at Bristol, John Roderick, later to be Professor of Civil Engineering at Sydney. Further, Baker was enabled to travel in Germany and to visit universities in the wake of a Congress held in Berlin. One section of that Congress had discussed the inelastic behaviour of steel structures—their behaviour when they were loaded beyond their elastic limits, and 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 experimental beams until they reached plastic collapse. If one statement should be made about Baker's contribution to structural theory, it is that he realized, 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 by now customary back-breaking labour. But the signpost was there, and was read correctly by Baker—his life's work had begun.

The revolutionary idea of plastic theory 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 the structure, so that he can assure himself that the building is safe. The plastic designer makes a trivial inversion of the design statement: instead of requiring a building to stand up, he requires it not to fall down. The question the plastic designer asks is then not how a building comports itself under its loading, but rather how it could possibly collapse under an overload. This collapse concept involves completely different calculations, referring to permanent plastic deformations rather than hypo-

thetical elastic states (which the Steel Structures Research Committee found were not even observable in practice). And it turns out, as a technical matter, that plastic calculations give a far more accurate representation of reality than elastic calculations.

The reason that calculated elastic states cannot be observed in practice is that an engineering structure is highly sensitive to very small imperfections. A slight mismatch of geometry, whether in manufacture or in setting out, will lead to large strains (and hence large associated stresses) of the whole structure. The elastic designer must make the assumption that his structure is perfect, but his calculated stresses then refer only to that perfect structure, and not to any real construction.

The elastic designer's assumptions do, however, seem reasonable, even if they lead to apparently unobservable elastic states; common sense would lead one to believe that a trivial defect cannot really affect the strength of the 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 practical structure does not depend on an elastic stress reaching some limit at one 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 of steel beams, and that can be observed in any structure made of a material that an engineer would consider reasonable to be used in practice. Glass and cast iron are brittle, and are unsuitable structural materials; what is needed is a material with some minimal ductility. Steel in its plastic range exhibits this ductility; so does reinforced concrete, as modern designers have appreciated. It is ductility that accommodates small practical imperfections of geometry or lack of fit; Maier-Leibnitz had found that the collapse loads of his beams were always almost exactly reproducible, no matter how roughly he set up his experiments.

These statements are made with hindsight. 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 would require much work. However, the power of plastic collapse analysis was already amply confirmed by 1939, when World War II intervened.

Air raid precautions

Baker left Bristol at the outbreak of war to become Scientific Adviser to the Ministry of Home Security, at Princes Risborough. He established there the Design and Development section of the Research and Experiments Department, where his first concern, during the 'phoney war', was to consider the protection of production by the correction of the design of wartime factories. These were subject to the phenomenon of 'spreading

collapse' from initially slight bomb damage. Further, almost all the standard forms of air raid shelter were unsatisfactory. The exception was the Anderson domestic shelter, which was structurally sound but liable to flooding in the winter.

Baker realized that the plastic approach gave the key to the design of all structures subjected to bomb explosions. The secret was to provide the structure with continuity and ductility, so that enormous amounts of energy could be absorbed without collapse or damage to the occupants. With these principles, new types of standard shelters, including brick surface, were designed, and trench linings and basement strutting could be proportioned. The whole of this story, from 1939–43, has been told by Baker in *Enterprise versus bureaucracy* (6).

The most spectacular application of plastic theory was to the Morrison shelter; over a million were made and over a quarter million tons of steel were used. The intellectual basis of this 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.

There is a curious postscript to the story of the Morrison shelter. Some nine years after its invention, in 1950, Baker met Morrison 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, now, 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 is, in fact, that Baker had made the 'design' in this sense, as well as making the plastic calculations. And there is a third stage in any engineering project, which Baker was at pains to teach every student at Cambridge. The engineer's job does not stop after he has had the idea, nor yet after he has made the calculations—the engineer must ensure that his design is actually built. There were, in the event, formidable difficulties, in wartime Britain, in actually constructing the Morrison shelter. In 1950, shortly after Baker's conversation with Morrison, the Royal Commission on Awards to Inventors was in no doubt about agreeing to Baker's claim, and he was awarded the sum of £3000.

Cambridge

From 1943 onwards, at Cambridge, Baker started building his team for the prosecution of research on plastic theory. There was more money for the work, for research students, for research workers on contract, for

designers to be seconded to Cambridge from industry, and progress was, by prewar standards, rapid. By 1948, British Standard 449 (the 20-year-old patched-up Code of Practice) was modified by the insertion of a single clause to permit plastic design. No guidance was given, but for the first time a designer was permitted to stop thinking about how a structure might stand, and, instead, to design it to collapse under a calculable overload.

Immediately, designers in Britain started using the new method, encouraged by the Cambridge team who themselves made calculations for many early buildings. From 1948 onwards the Welding Institute at Abington became a showcase for examples of plastic design, and three separate examples may be seen in the new buildings of the Cambridge Engineering Laboratories. Baker himself lectured in the United States in 1949 on plastic design methods and, despite a somewhat poor reception, the U.S. steel industry thought it worthwhile to send someone to Cambridge for a few months; as a result, the method took firm hold in America, and subsequently around the world.

Baker had, in twelve years, from 1936 to 1948, brought the work to a point where it could be used in practice. Progress may have seemed slow, and the war years did not help, but it was an extraordinary achievement to have moved from an idea glimpsed in the 1930s to an officially permitted method a decade later. Baker and his team, and any engineer who cared to learn the technique, could calculate the collapse load of a steel frame, and so provide an economical and safe plastic design for the structure. However, each structure, or structural type, was tackled *sui generis*; there were no supporting principles against which the design was being calculated. Baker, in 1948, did not yet know of the fundamental plastic theorems.

A few months before this Baker met, at a conference, W. Prager. Prager, a refugee from Germany, had made his way to Brown University in Providence RI in the early years of the war. He was an engineer by training and a powerful self-taught mathematician, and he was aware (from Russian sources) of the mathematical theorems of plasticity. Prager and Baker were very different men, and never established a close relationship; at their first meeting, however, they realized that each had much to offer the other. Baker had a wealth of practical knowledge and Prager could supply the framework of mathematical support. They agreed to establish exchanges between their universities for young members of staff, and from 1948 for a few years visits of one or two years' duration were arranged across the Atlantic.

Baker had always tackled his problems head on. The assault on the mountain took place on a broad front, with painstaking uniform moves forward. If the work was held up at one point, then other work was delayed until the block had been cleared. Prager, on the other hand, would pursue any important road that went in the right direction, and would leave unscaled peaks behind him, unsolved problems, perhaps to be returned

to later. It would sometimes happen that, viewed from behind, the unscaled peak was seen to be irrelevant to the progress of the work or, alternatively, a previously hidden path to the summit would be revealed.

The cooperation between Cambridge and Brown universities was very fruitful; there was important joint work (for example on the repeated loading of structures); the Cambridge team, once in possession of the underlying principles, could begin to establish a rounded account of the engineering theory of plasticity; and the American workers developed a powerful mathematical framework for that theory, which they pushed forward in areas of practical importance.

During this period in which Baker was doing the work with which his name is inseparably linked, he was also reforming the teaching at Cambridge, expanding the teaching staff and establishing the new laboratories. He was also writing. In the 1950s, with plastic theory firmly established, he published a massive two-volume account of the history of the steel skeleton, covering the period of the Steel Structures Research Committee through to the development of plastic design rules. In the late 1960s, with plastic theory a central part of undergraduate courses, he devised a two-volume text on the plastic design of frames for the use of students.

During this period, also, he served on various national bodies. From 1953 to 1963 he was a member of the University Grants Committee. He was a member of Council of the Institution of Civil Engineers. He took part in an inquiry for the Ministry of Transport into the feasibility of adding a road above the rail track to Benjamin Baker's Forth Bridge, and in another inquiry for the Central Electricity Generating Board into the cause of the collapse of the Tyne crossing towers. In Cambridge, from the 1940s through to the 1970s, he played a full part in administration, serving on the General Board and on the Faculty Boards of other departments.

Baker had an extraordinary energy, but he could not have done all he did without the protection of his family. Baker's own door at the University was always open, but Fiona shielded him from the burdens of domestic arrangements, and made sure that his energies were conserved for whatever he wanted to do. Baker indeed led a full social life, but he was not always easy with strangers. He could be tactless—complaints in the Common Room about levels of surtax did not always receive sympathy from struggling junior lecturers. He was sometimes prickly, and he sometimes had a quick temper, and the family, and friends, had occasionally to smooth over some temporary misunderstanding. Baker relied very much on his home; children and grandchildren were frequent visitors, and he and Fiona entertained and welcomed visits from a wide circle of friends and colleagues.

Public honours were perhaps slightly slow in coming. The O.B.E. was awarded in 1941, and it was to be 20 years later that he was knighted.

Academic honours, however, arrived early. The gold medal of the Institution of Civil Engineers was followed by many others; he played a charming game in wondering whether he could acquire gold medals faster than grandchildren. He had more honorary doctorates than he could remember. He was made a Fellow of the Royal Society in 1956. Finally, in 1977, he was made a life peer.

Baker enjoyed being a lord. He liked the break of a few days each month in London, and the atmosphere of Westminster, and he played a major part in several memorable debates. He liked also to visit for a few days the structural firm of which he was a director, which maintained his contact with the real world of engineering, and from which he returned refreshed to Cambridge and to Fiona. Happy indeed is he, who, like Ulysses, has made a splendid voyage, or like that other who achieved the fleece, and then returned to home and family, full of wisdom, to live out his life in peace.

Alas, Fiona died in 1979. Baker was afflicted with irritating disabilities to his eyes, but he continued to receive his friends at home, to lunch in his College and to dine on occasion. He was active to the last, and on the night before he collapsed he had been at a College function in honour of a colleague. Despite the miseries of the last few years, and some fits of depression, he was still buoyant—he had, after all, made a great voyage, he had achieved his fleece, and he knew it to be gold.

It is just possible that even Baker did not know how pure the gold was. He had constructed a theory, the plastic theory, as he thought to discuss the behaviour of steel frames. It turns out that the theory is universal, and can be applied to any material that is used for building. The new theory of general structural design is applicable to the medieval cathedral and to the skyscraper. The simple intellectual inversion, which seems a truism, was finally proved to be absolute.

If the designer can find *any* state for a building for which it is not at collapse under given loads, then the building is bound by the iron laws of mathematics to be safe under those loads. This is the master theorem of simple plastic theory, a theorem used, consciously or unwittingly, by every structural designer.

Appointments

1924	Temporary post, Air Ministry.
1925	Technical Assistant, Design Department, Royal Aircraft Works.
1926	Assistant Lecturer, University College, Cardiff.
1928	Scientific Assistant, Building Research Station.
1930–36	Technical Officer, Steel Structures Research Committee.
1933–43	Professor of Civil Engineering, Bristol University.
1939–43	Scientific Adviser, in charge of Design and Development Section, Ministry of Home Security, Air Raid Precautions Department.
1939–48	Member, Civil Defence Research Committee.

1943–68	Professor of Mechanical Sciences and Head of Department of Engineering, University of Cambridge.
1943–85	Fellow of Clare College, Cambridge.
1945–46	Scientific Advisory Committee, Ministry of Works.
1947–56	Advisory Council, Military College of Science.
1948–63	Consultant, Naval Constructional Research Establishment, Rosyth.
1953–63	University Grants Committee.
1958–64	Chairman, Naval Educational Advisory Committee.
1962–74	Director, Technical Development Capital Ltd.
1963–71	Director, John Brown and Co. Ltd.
1964–74	Director, Cambridge Fender and Engineering Co. Ltd.
1975–76	President, British Association for the Advancement of Science.
1978	IDC Group Ltd: Director of Research and Development. IDC Consultants Ltd: Deputy Chairman. IDC Project Management Consultants Ltd: Director.

Professional affiliations

Institution of Civil Engineers: Fellow. Member of Council 1947–56. Vice-President 1969–71.
Institution of Structural Engineers: Fellow. Member of Council 1936–39. Chairman, Western Counties Branch 1935–39.
Welding Institute: Fellow. Member of Council. Chairman, Research Board. President 1971–73.
Fellowship of Engineering: Founder Fellow 1976.
Royal Institute of British Architects: Hon. ARIBA.
Institution of Mechanical Engineers: Hon. F.I.Mech.E.
Hon.F. South African Inst. C.E.

Honours

1931	O.B.E.
1961	Knight Bachelor
1977	Created life peer (Baron Baker of Windrush)

Medals and prizes

Royal Society:	1972	Royal Medal
Institution of Civil Engineers:	1932	Telford Gold Medal
	1936, 1953	Telford Premium
	1937	Howard Quinquennial Medal and Prize
	1952	Ewing Medal
Institution of Structural Engineers:	1951	Silver Medal
	1953	Gold Medal
	1957	Research Medal
Welding Institute:	1977	Brooker Medal

Honorary degrees

1953	D.Sc.	(Leeds)	1966	D.Sc.	(Aston)
1958	D.Sc.	(Manchester)	1968	D.Sc.	(Leicester)
1960	D.Eng.	(Liverpool)	1974	D.Sc.	(Salford)
1962	LL.D.	(Glasgow)	1976	D.Sc.	(Cranfield)
1963	D.Sc.	(Edinburgh)		D.Sc.	(Lancaster)
1964	D.S.	(Ghent)	1978	D.Sc.	(Bristol)

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(16) Methods of stress analysis. *First report, Steel Structures Research Cttee*, p. 179. London: H.M.S.O.

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