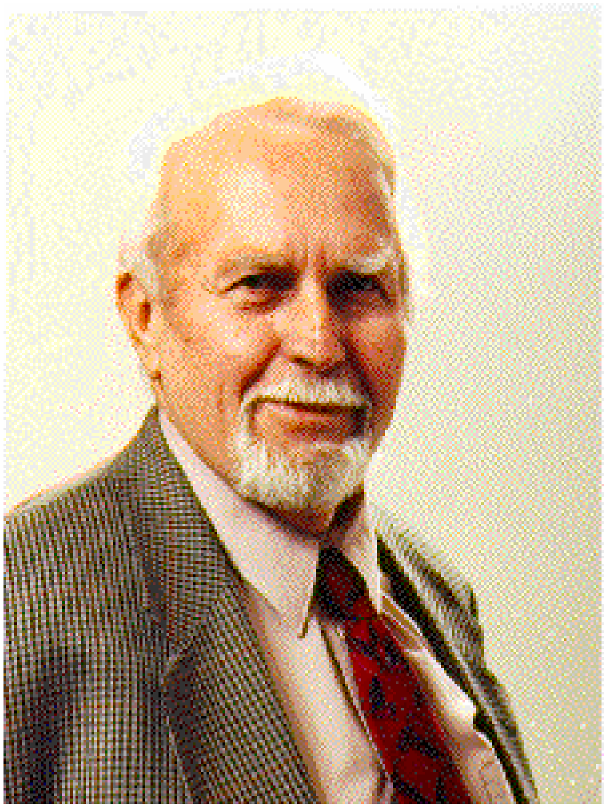

Professor Olgierd (Olek) Cecil Zienkiewicz (1921 – 2009)

From Wikipedia, the free encyclopedia,
http://en.wikipedia.org/wiki/Olgierd_Zienkiewicz

Olgierd Cecil Zienkiewicz, CBE, FREng, FRS was a British academic of Polish descent, mathematician, and civil engineer. He was born in Caterham, England. He was one of the early pioneers of the finite element method. Since his first paper in 1947 dealing with numerical approximation to the stress analysis of dams, he published nearly 600 papers and wrote or edited more than 25 books.

Early education:
His school education took place in Poland, where his father was a judge of the Katowice district. He and his family moved to the UK due to the World War II. Zienkiewicz studied in the early 1940s at Imperial College London for an undergraduate BSc (Hons) degree in Civil Engineering which he obtained in 1943 with first class honours. Then, after being offered a scholarship, he stayed for two more years at Imperial College to carry out research on dams under the supervision of Professors A. J. S. Pippard and Sir R. V. Southwell. He was awarded the PhD degree in 1945 with his thesis title "Classical theories of gravity dam design in the light of modern analytical methods".

Contributions to science:
Zienkiewicz was notable for having recognized the general potential for using the finite element method to resolve problems in areas outside the area of solid mechanics. The idea behind finite elements design is to

develop tools based in computational mechanics schemes that can be useful to designers, not solely for research purposes. His books on the Finite Element Method were the first to present the subject and to this day remain the standard reference texts. He also founded the first journal dealing with computational mechanics in 1968 (International Journal for Numerical Methods in Engineering), which is still the major journal for the field of Numerical Computations.

International recognition:

The international range of Zienkiewicz' academic experiences has been geographically diverse. He became a lecturer at the Department of Engineering, University of Edinburgh, UK (1949–1957) before becoming Professor of Structural and Civil Engineering at Northwestern University, Illinois, USA (1957–1961). From 1961 to 1988 he was Head of the Department of Civil Engineering at Swansea University. He was latterly Professor Emeritus of this institution. Other teaching positions have included:

1. International Centre for Numerical Methods in Engineering at CIMNE, Barcelona, Spain—Professor of Numerical Methods in Engineering
2. Polytechnic University of Catalonia at Barcelona, Spain—UNESCO Chair of Numerical Methods in Engineering
3. University of Texas, Austin—Joe C. Walter Chair of Engineering.

Honours:

Zienkiewicz received honorary degrees from Ireland, Belgium, Norway, Sweden, China, Poland, Scotland, Wales, France, England, Italy, Portugal, Hungary and the United States.

He was elected to a number of learned societies,[4] including:

Royal Society
Royal Academy of Engineering, 1979
United States National Academy of Engineering (foreign member)
Polish Academy of Science
Italian National Academy of Sciences
Chinese Academy of Sciences

He has been the recipient of many honors, awards, and medals. including

Commander of the British Empire
Royal Medal (Royal Society)
Carl Friedrich Gauss Medal (West German Academy of Science)
Nathan Newmark Medal (American Society of Civil Engineers)
Newton Gauss Medal (International Association for Computational Mechanics)
Gold Medal (Institution for Mathematics and its Applications)
Gold Medal (Institution of Structural Engineers)
Timoshenko Medal (American Society of Mechanical Engineers)
Prince Philip Medal (Royal Academy of Engineering),
Zienkiewicz has been listed as an ISI Highly Cited Author in Engineering by the ISI Web of Knowledge, Thomson Scientific Company.

He was instrumental in setting up the association of computational mechanics in engineering (ACME) for the United Kingdom in 1992 and was the honorary president for the association for the rest of his life.

1998 Timoshenko Medal Lecture by Olgierd C. Zienkiewicz

AS I REMEMBER

by O. C. Zienkiewicz, University of Wales, Swansea

The text of the Timoshenko Medal Acceptance Speech delivered at the Applied Mechanics Dinner of the 1998 IMECE in Anaheim, California.

1. Introduction

It is a great pleasure and honour to be included in the distinguished list of recipients of the Stephen Timoshenko Medal. I would like to take this opportunity to thank the American Society of Mechanical Engineers and the various friends I have in it who must have been responsible for my selection.

Because of my age and my long involvement in the field I know personally, or have known, more than half of the previous recipients of this award. Indeed, the very first recipient and namesake of the award, Stephen Timoshenko, was one of these. We met in 1960 at Northwestern University when he visited one of his early doctoral students, much distinguished in the field, Professor Nick Hetenyi. Both of these acquaintances are now gone, having worked long and contributed much to the subject of applied mechanics. In the long list of recipients, now departed, I find my own Ph.D. supervisor, Sir Richard Southwe1l, and an old adviser and friend, Professor William Prager. Amongst those no longer here is another friend, James Lighthill. Though he received his medal as early as 1963, he was still healthy and fit this year. But many may not know that it is only a few months ago that he met his end – trying to swim round the Island of Sark in the Channel islands, a feat much younger men would not attempt and which he, using his knowledge of the tides, previously accomplished more than once. I salute his courage and achievements.

2. Timoshenko: teaching and research

Though my first personal encounter with Stephen was in 1960, he was well known to me by that time. In my Ph.D. studies, which started in 1943, his book on Theory of Elasticity became my bible. At the outset of my study with Professors Pippard and Southwell I had to acquaint myself with the earlier numerical solutions produced in 1910 by J. F. Richardson. As that work used the Airy stress function to formulate the solution, some introduction to elasticity was clearly necessary. I had many gaps in my knowledge having just completed the very brief, two-year, wartime degree at Imperial College. Therefore, after some unsuccessful encounters with various texts I followed the recommendation of a senior colleague and invested in Timoshenko’s famous book, which today still holds a privileged place in my library. That text solved my problem completely. In the first two chapters I found all that was needed and it was only his excellent presentation which made me read further.

This episode - and indeed later contact with the works of Timoshenko - made two important impressions on me. First, I realised that even quite complex ideas could be presented in a lucid form. This was most helpful to me later when I was compiling my own first book on the finite element method. Of course this was some 20 years later, but I have always tried to follow the master by avoiding the alternative process, very popular among some scientific writers. They follow the maxim quite probably coined by a German philosopher, which simply said: “Warum einfach machen wenn man auch kompliziert sein kann.”

Second, which perhaps took me longer to realise, was the fact that good teaching cannot be practised properly without underlying research. Certainly the example of Timoshenko provided an example for me when I became a young teacher.

The conflict between the two directions of teaching and research, still much discussed in Academia, was originally the subject that I wanted to discuss in this talk - but, enough has been said on this matter. It was after reading Timoshenko's autobiography that I changed my mind and in true "plagiaristic" spirit I adopted his title for the present talk – "As I Remember". This will allow me (1) to reminisce a little on my own origins and (2) to discuss the development of my own research and how this led to my present involvement with the finite element method.

3. As I remember - the linking of life's strands

Timoshenko's autobiography was written in Paris in 1963 and was translated into English with his help five years later when he reached the ripe old age of 90. Reading this book was a most interesting experience, especially when I realised that our own life's strands were interlinked and even intersected on many occasions. Timoshenko was born in the Ukraine in 1878, five years after the birth of my father. The places of their birth, as far as I can trace from the available atlases, were about 100 miles apart and each a similar distance from Kiev. Both of them were citizens of Imperial Russia at the time of their birth, but their nationalities were different. Timoshenko was basically Ukrainian and my father was Polish - both facts quite well recognized at that time when nationalities and citizenships were separate entities.

Though my father was a lawyer and Timoshenko an engineer, it is interesting to speculate whether their paths at one time or another crossed. Certainly, for a limited period both of them lived in Kiev and it is also certain that during later years both were much involved with St. Petersburg where, after the Revolution, the first liberal, provisional government in Russia was formed under the premiership of Kerenski.

It was during that provisional government time that the divergence of their paths occurred. My father, perhaps because of his English wife and knowledge of the English language, was chosen to be the Consul in England of Kerenski's provisional Russian government. However, my father was stranded and started a new life when the Bolshevik revolution erupted in Russia. It was in England that both my sister and I were born. Timoshenko, on the other hand, left Russia by a completely different route. This led him later, via Turkey and Serbia to Zagreb in Croatia where he became a professor at the Technical University for some time, before moving finally to the U.S.A. in 1922.

Those who read his autobiography will find full details of the adventures of his life at that time, and the story of his rise to fame in the American continent and indeed in Europe. He first established his position firmly as an engineer and teacher at Westinghouse, then became a professor at the University of Michigan in Ann Arbor in 1927. Finally, in 1936 he reached Stanford University. The Chair he held there became his last permanent employment although he finished his life in Switzerland - a country he had much loved in his younger days.

So how did our life strands interlink again? Well, I have already mentioned the importance of his text in producing my doctoral work under Professors Pippard and Southwell. It is through the work of the latter that the connection will arise again. Professor (later Sir) Richard Southwell was, at the time of my doctorate studies, leading a research team concerned with the solution of finite difference equations in elasticity for various problems of realistic application. Indeed many of these problems were concerned with the war effort and therefore confidential. Others were not - like my own analysis of a dam - though the methodology was not publishable during the war. Even the proceedings of the Royal Society were at that time “confidential”. It was then that I acquired a general interest, not only in elasticity, but also in fluid mechanics, which to Sir Richard presented just one more problem to be dealt with by a general numerical procedure.

It happened that Southwell was one of Timoshenko’s guests at the University of Michigan as early as 1935. This in turn led to a later encounter after the war in 1949 and again at Ann Arbor. At Timoshenko’s invitation both were involved in a summer course and this meeting was to be more important. Certainly Timoshenko was always the engineer, and being at that stage engaged in the quantitative solution of problems, he was much impressed by the generality which was established by using numerical, finite difference solutions. I believe this caused him to write an extensive appendix when the second edition of his book on Theory of Elasticity, now co-authored by J. N. Goodier, appeared in 1951. This appendix included a full description of Southwell’s procedures and solutions. He remained at all times a protagonist of numerical solutions, and it was here that our interests began to overlap.

4. The engineering beginnings of numerical analysis

The first finite difference solutions of equations of elasticity dated back to the work of Runge in 1908 and Richardson in 1910. The latter indeed solved the problem of stress distribution in a gravity dam, a subject of much interest at the time in view of the construction of the Aswan Dam in Egypt. Indeed, during the same period, inconsistencies and difficulties of using standard, “cantilever” approximations were realised and a true elastic solution was obviously needed to settle the controversy.

As Southwell’s relaxation methods were available, Professor Pippard - my doctoral Supervisor - set me the goal of providing a very accurate answer to the above question. I was eventually successful and in 1945 I duly handed in my thesis solving that problem, as well as others on meshes very much finer than those initially used by Richardson. The success was due to the use of relaxation methods, but why were they so successful and where-in lay their magic?

It is my belief that the ideas introduced by Southwell, which were of considerable importance, could be summarised as:

The recognition that the finite difference equations could be made equivalent to an analogous discrete structural system, and

The solution of the structural discrete system could be performed most efficiently by an iterative process.

As is well known, discrete structural systems, which provide the basic work for all civil, aeronautical and structural engineers, can be formulated using either the so-called “displacement” method or the “force” method. The first of these methods is obvious and direct, though it is well known that the second (the force method) is also useful in many simple cases of redundant structures for which it provides an economical and elegant solution. It is difficult to say who first formulated the direct displacement (or direct stiffness) approach. Certainly the method was well known at the beginning of this century and certainly it was included in the education of engineers in the 1930’s. In this approach stiffness coefficients were obtained for each element of the structure and the system equation was obtained by a simple addition of such coefficients. Matrix ideas were useful in this process and certainly provided a shorthand. They were not, however, essential to the understanding or indeed to the solution of the equations. Fraser, Duncan and Collar in the 1930’s appeared to be the first to use matrices for such problems in structural engineering in the aeronautical industry.

The procedures used in the direct stiffness approach were precisely the same for many other engineering systems, typically those that occurred in the solution of pipe network systems or electric networks. In each of these exactly the same formulation applied and in all cases the procedures were the same. It is therefore worthwhile to talk about a standard discrete system in this context and we observe in the literature a rapid diffusion of ideas from one area of application to another.

Southwell's method of relaxation used for iterative solution of structures, or similar problems formulated in a discrete system, was a procedure he named "systematic relaxation of constraints" in 1934. In this process, each "nodal" displacement or similar system quantity was first assumed to be fixed in an arbitrary position by imaginary constraints (which he often described as "jacks"). On "relaxing" of such a constraint by removing the "jack", the load was transferred to adjacent nodes and the node in question then was displaced by an amount which was easily calculable. Obviously, in the continuing process of constraint relaxation the load transfer in the structure would ultimately lead to correct solutions when all the load was thus transferred to the supports.

Mathematically, of course, the procedure was carried out in a sequence similar to that of the Gauss-Seidel iteration, but in a manner guided by the user. However, the physical interpretation of the process made it very understandable and such methods as moving a whole group of nodes simultaneously etc. could be used effectually by an intelligent operator to accelerate convergence.

The "structural" relaxation procedure of the Southwell type was apparently used as early as 1922 in Zagreb by a man called Calisev, (viz. Timoshenko). However, much more important was the development of the so-called "method of moment distribution" by Hardy Cross in the U.S.A. in 1932. This preceded the Southwell process by only two years but the Hardy Cross Method gained fame internationally and became the standard process for solution of framed buildings, etc. in the 1930's and 1940's.

It is of interest to make a marginal remark that there is a good reason for the success of the Hardy Cross moment distribution method vis-à-vis the Southwell relaxation method then applied to tension bar structures. The "carry over" factor in bending computations was one half rather than unity in bar structures and this of course led to a very much more rapid convergence.

When Southwell entered the area of finite difference computations he generally endowed the discrete equations with a structural interpretation. Thus the Poisson equation, which was well known could represent the deformation of stretched membrane, became in the finite difference net the deformation of a string net with given tensions. The string net being a simple structure could of course be solved by precisely thesame procedures as Southwell applied earlier to actual discrete structures and thus the Systematic Relaxation Constraints of 1934 could be used again.

It is of interest to note that such a physical interpretation of finite difference equations, when used for elasticity, was simultaneously and independently derived in the U.S.A. The conditions of wartime secrecy and the resulting restrictions on exchange of documents prevented Southwell's work with this being widely known there. However, two important developments were derived in the U.S.A. The first one was arrived at by Hrenikoff who in 1941 established a so-called framework analogy to the finite difference equations of elasticity, and the second was arrived at by McHenry who in 1943 presented the lattice analogy. Clearly engineers liked this physical manner of interpreting equations which also simplified boundary conditions which were now purely physical. These analogue procedures were the precursor of the concept of finite elements.

In the classic paper of 1956, Turner, Clough, Martin and Topp presented the idea of dividing the real continuum directly into elements of arbitrary shape and directly establishing their stiffness. This became known as the method of Finite Elements only in 1960, following a paper presented by Ray Clough. In the original work very explicit physical models were used, thus completely avoiding the writing down of either finite difference or differential equations.

Much later the finite element method was to become based on the use of variational or weighted residual procedures of Galerkin type applied directly to the differential equations used to model rationally the elements of a continuum. Though most engineers applied this initially to the elasticity equations, it must be remarked that Courant did this much earlier in 1943 - i.e. precisely when Southwell, Hrenikoff and McHenry were active. In his work he showed that such direct procedures could be used for the Poisson equation. Courant introduced what is essentially the same linear triangle as that derived in 1956. Being, however, a mathematician he did not see the need nor did he have the desire to seek a physical interpretation.This perhaps accounts for the fact that his work was only unearthed several years after the mainstream engineers had been happily using the finite element procedure for solving their structural problems.

5. Is F.E.M.'s success due to its intuitive appeal or its generality?
There is no doubt that it is the intuitive appeal of the finite element process which makes it so popular today. When in the late 1950s I met Ray Clough and for the first time encountered his idea of splitting a continuum into "physical lumps", the procedure did not appeal to me. Surely it all could be done more precisely and conveniently with finite processes I learned from Southwell and used successfully over many years? We did, however, agree that one problem remained which needed solution. That problem was the analysis of shells of arbitrary shape as these were much encountered at the time in the design of arch dams and in architectural flights of fancy.

The twin difficulties of the finite difference approach that I had been using were: (a) deciding which set of governing equations to use. Here the choice was wide with many authors contributing different approximations, and (b) establishing analytically the co-ordinates of an arbitrary shell in which the governing equations were to be approximated.

Both Ray and I agreed that here the finite element approach could well be used, employing as elements flat facets of a triangular or rectangular shape with the former of course being needed for arbitrary shells. In such a manner both difficulties could be simultaneously avoided.

For such a finite element formulation the "inplane" stiffnesses were already well established and surely the corresponding bending stiffnesses based on the Kirchhoff thin plate theory could easily be added.

It was on these problems that Ray's and my own research students spent much time during the early 1960's. By 1965 both of the groups were successful and found suitable formulations for triangular plates. Two years later they established the possibility, and indeed were successful in solving arbitrarily shaped shells. The thin shell modelling by flat facets proved convergent despite a few variational crimes committed on the way, but both the problem and its solution were overtaken by events which occurred in parallel.

My colleague, Bruce Irons, and myself also developed in 1965 the first three-dimensional solution using higher order elements, which could be curved by an isoparametric mapping to fit almost any shape. Clearly, by making such elements thin, any curved shell or plate could be modelled without introducing the super-human efforts needed to establish Cl continuity or the necessity of introducing thin plate and shell theory assumptions.

This development resulted in the fact that by the end of the decade the thin plate and shell problem disappeared, being today largely of historical interest. But many difficulties still were encountered in establishing a robust formulation. I shall not dwell on them except to say that by the mid-1980s all of these were overcome.

Did intuition or mathematics drive the second development to success? Who knows? But without the proper and precise use of mathematics the present case of dealing with thin wall structures would not exist. Further, the developments of rational approaches to such new fields as those of fluid mechanics, electro-magnetism, etc. would not be possible. Which way should we direct our research now ?

6. Which way now?

It is recognised by many that the finite element process of today is but a particular form of the weighted residual approach. The latter was classified well by Stephen Crandall in his excellent book of 1955, though the fundamentals were established by Galerkin somewhat earlier in 1915. (This occurred I believe in St. Petersburg and must have coincided with the time Stephen Timoshenko was there!)

The difference between the various approximation procedures that are today still used is that of the specific trial or weighting functions that are employed. Much of the research done today centres on finding better, newer, and more efficient designs.

Von Karman said:

"The Scientist studies what is, the engineer creates what has never been."

Surely this requires more efficient analysis procedures to design what "never has been".

Charles H. Duell, commissioner of U.S. Office of Patents in 1899 mentioned at the time that

"Everything that can be invented has been invented."

I do not share this pessimistic view and I think we shall see many exciting developments in the coming years. It is evident that both applied mechanicians and mathematicians will continue to contribute to the numerical analysis field.

However, I have reservations about making predictions for the future, especially since in public speeches these may lead to such mistakes as the famous one of Thomas Watson, Chairman of IBM in 1943. His prediction was that

"I think there is a world market for maybe five computers."

. . . . probably more than a trivial miscalculation.

This could only be rivaled, however, by a statement by the famous British scientist, Lord Kelvin, who was the President of the Royal Society in 1895 and apparently said:

“Heavier-than-air flying machines are impossible.”

Perhaps silence on matters of predicting the future is golden – and here I shall rest.