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A Recollection of Gene Golub and solving normal equations by his QR approach and the ABS-EGERVARY approach

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A RECOLLECTION OF GENE GOLUB

AND SOLVING NORMAL EQUATIONS

BY HIS QR APPROACH

AND THE ABS-EGERVARY APPROACH

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1. Personal recollections

My first meeting with Gene Golub took place when he was invited, in the spring of 1971, to give a talk at the Department of Computer Science of the University of Essex. I was there for a stage of 6 months working on Quasi-Newton methods under the guidance of Charles Broyden, then very famous for having introduced such methods for solving nonlinear equations and for having discovered a method, known as the BFGS method since it was independently found also by Fletcher, Goldfarb and Shanno, that was more stable than the DFP method. I am not sure of the argument presented by Golub, whose talk was quite technical and probably relating to the QR method, and whose American accent was curious for me, immersed in the British local accent. Broyden invited also in that period dr Greenstadt, then working with IBM, who presented a talk on Quasi-Newton methods. I was fortunate to attend another talk of Greenstadt at the Computer Science Department in Stanford, during the weekly talks organized by Golub; it was 1976 and my second visit in Stanford. I was able to make important additions to the material he presented, so that we wrote a joint paper that appeared in Numerische Mathematik, dealing with optimally conditioned Quasi-Newton methods for nonlinear equations.

In September 1973 I obtained leave from CISE, a nuclear research center near Milano where I worked from 1969 to 1976, to spend 6 months at

Stanford, in the Department of Economic Engineering directed by David Luenberger, author of the book I still consider the most elegant and philosophical on nonlinear optimization and linear programming. There I worked with his brilliant ex student Shmuel Oren, who had written an exciting PhD Thesis on scaled Quasi Newton methods. It was a special time in America and in Stanford, Watergate had begun and at the campus by paying 25 cents you could throw a ball to the effigy of Nixon. Joan Baez came to sing, Angela Davis gave a course on lesbianism as a lethal attack to capitalism, AIDS was unknown but preparing in the bath houses of San Francisco where some people went to have up a hundred intercourses a day (Golub later told me he lost a statistician friend to such a disease). I was able to get two remarkable results in Quasi-Newton methods, namely a strict bound to the condition number of their matrices and invariance to nonlinear scaling. The first result is still my most quoted result, albeit it is not really very deep, and because of it I was quoted by Bulirsch and Stoer as one of the three Italians in their book (the others being Fibonacci and Volterra). I attended the weekly seminars organized by Golub (Luenberger was very busy in private consulting), gave one on my results, went to the monthly meetings in Polya's home, who was at that time about 90. I was invited at the end of the lessons to a Chinese restaurant with the people of the Computer Science Department, a dinner ending with a soup served in an emptied huge melon.

In November 73 I was at the Madison conference on Generalized Inverses, where Golub attacked the old school of linear algebra, in particular prof Albert, to whom I am indebted for a theorem I discovered in his book, which has been central in the development of ABS methods. The whether was grey, the Yom Kippur war started, and I began a very long trip in US by bus (7000 km) and by air to Panama, where I gave a talk on Quasi-Newton methods to an audience of over 100 persons!!!

Then Golub took a sabbatical at ETH (invited by Marti, whom I met at his home), during which time I invited him to give a talk at CISE, and he stayed a few days at my home. We had a train visit to Venice and he told these were his best days of his sabbatical in Europe.

In 76 I came back to Stanford staying at his department then located in Serra House, a small building among bushes and quails. I was given the room where Householder used to come once a month to do referee work. He was a quite, austere man not easy to talk to. During that time I wrote the quoted paper with Greenstadt and spent hours in the math library reading dozens of books (including the 36 by Bellmam, each one a permutation of the sentences of the other…) and hundred of papers. A Brazilian friend sold me his huge car for 100 dollars and I had to pay much more for insurance and change of the

antipollution devices. It was normal to work in Serra House until 11 p.m. and on the way back to May Avenue where I had a room near the house of Cavalli Sforza (his daughter Violetta studied Russian with me) I used to visit the Music Department to play piano. Once at a late night a student of Gene, John Bolstadt, gave me his FORTRAN program implementing the Golub-Pereira nonlinear structured least squares method. Back to CISE this code turned out to be exactly what was needed for my last work there, namely determining the amounts of pollutants in the atmosphere, based upon certain measurements made by a CISE produced electronic device. The code has always worked very well and may still be in use! Bolstadt was not one of the best scholars among Golub students (better were Chan, Luk, Overton…they became famous and work now in top places) but we must thank him for his programming work (on many pages of grey paper!).

In 79 I came back for only June and July. The department had moved to a new building where rooms were very small and often without natural light. One was beginning to use the big SLAC computer via long distance connection sending program and data via teletype. There was a price for the use of processor's time and Gene gave me several minutes to use.

In 80 I was now at Bergamo University and invited Golub for a CREST course in optimization, where he spoke on least squares, other participants including Kuhn, Dixon, Mc Keown, Wedin... He wanted the bank to pay him immediately his fee and got angry when they told him of new regulations that money could be sent abroad only by bank order.

I organized a NATO ASI in optimization at Il Ciocco in 85 and then one on linear system solvers in 87. Initially I put him in the list of the second ASI and asked if he would be a coordinator, but I never got answers. He was on sabbatical and quite depressed on the future of linear algebra, as he told me when we met at a conference in Valencia. My first list was not accepted since they required certain specialized areas to be introduced, which I did (by inviting Hackbush, Meurant…on the suggestion of Stoer). Golub wrote then a letter to the NATO ASI direction saying that he was "astounded" to note in the list almost only Europeans, one of the Americans, Viktor Pan, not being for him a proper American. But the original list had many Americans and my choice was forced and supported by Stoer advice. And NATO ASIs do not have a minimum requirement for nationality of teachers.

I saw him a few times later, giving talks in his department on ABS methods. He considered them initially as only of theoretical interest, which was also Stoer's opinion, and considered them unstable in practice: a wrong judgement, based on the absolute neglect in the linear algebra community of

the Egervary rank reduction process in linear algebra. Next year it is 50 years since Egervary's tragic death and I hope to organize a meeting to his memory (he was a giant in linear algebra, celestial mechanics, combinatorial optimization).

My last talk in Stanford was in 2002 and attracted lot of attention by the audience. About that time Chu, Wunderlic and Golub published a paper on the Egervary process, inviting to more analysis and use of it. They failed to realize that we had done exactly this in 400 and more papers and in two books, since they only looked at the UNSCALED ABS CLASS, not at the SCALED CLASS!!! A problem possibly originating from the fact that most published papers nowadays are never read (a paper in math is read on average by 1.6 people, one in physics by 0.1…. the History of Astronomy by Schiaparelli in the library of Merate Observatory was bought in 1917 and no one has yet cut the pages!) . I sent to Chu a letter of 7 pages showing that ALL results given in that paper had been published within the ABS field, but never got any answer. Exactly what happens today when I send letters to newspapers or magazines, including the so called catholic Avvenire, about the incredible errors in physics and history in Odifreddi' books, not to say of his curious statements that mathematics has died in Italy because Italians are catholic: the letters are never published (I have seen only a mutilated letter by Israel, angry that Odifreddi defines Israel a Nazi state…).

Overall I say the following about Gene

- **He has produced important results based upon enormous knowledge of linear algebra and the most extensive net of contacts in mathematics with other researchers; his book with Van Loan will be a must for generations**
- **He was very charismatic and generally very open, cordial and helpful (he accompanied me twice from Palo Alto to SF Airport)**
- **Sometimes he got a temper without real reason and so contributed to some splitting in the community of linear algebra people**
- **He has to be considered one of the great mathematicians of the 20th century in linear algebra and numerical analysis, whose place as a leader will be hard to be taken by another.**

ON THE NORMAL EQUATIONS:

THE GOLUB-HOUSEHOLDER APPROACH

AND THE ABS-EGERVARY APPROACH

Gene Golub, whose recent demise has deprived the mathematics community of perhaps its greatest leader, has worked with many problems, but his initial activity dealt with applications of the QR procedure of Householder to linear least squares, i.e. to the normal equations of Gauss of the first order. He was involved also in the so called American Datum, i.e. the determination of the topographical data (latitude, longitude, elevation over sea level) of North America, a problem of rather large size, about 4 million variables, the largest at that moment, some 20 years ago, which required use of nonlinear least squares on measurements then taken still in the traditional way, as at the time of Gauss, or of our Cosimo De Giorgi.

The normal equations of the second type use a matrix $A = (m,n)$ with $m>n$, rank not necessarily full. The problem to be solved has the form, with x and b vectors of size n and m

(1) $$A^TA x = A^T b$$

The above system is always compatible and if A has full rank we can use Cholesky method, which however requires the formation of the matrix A^TA, which is expensive and whose condition number is generally higher than that of A. By using Householder transformations one can transform A in a matrix whose upper n by n part is an upper triangular matrix R, the part below is zero, and one is then reduced to solve the triangular system, with d suitably defined

$$Rx = d$$

The above method is convenient in terms of complexity and conditioning.

The above normal equations of the second kind can be solved also by ABS procedure without having to form the product A^TA, in a way that apparently was never considered in the literature, apart from a comment by

Golub that some PhD student had considered them in his thesis (never retrieved the paper). This is based upon the observation that the above system is equivalent to the following extended system

$$Ax = y$$

(2)

$$A^Ty = b$$

Since (1) is compatible, then (2) must be solvable. Thus y, the right hand side of the first overdetermined subsystem, must lie in the column space of A, i.e. in the row space of A^T. But the second subsystem is underdetermined, hence it has one and just one solution in the row space, which is the solution of least Euclidean norm (a fact I learned from Albert's book!). Such a solution is exactly the one computed by the ABS method called the Huang method or implicit Gram-Schmidt method, started by the zero vector (or more generally any multiple of the first row of A^T). Once y is so computed, the ABS procedure solves the first subsystem by identifying and removing the m-n dependent equations (or even more if A is rank deficient, full rank of A being unimportant in ABS methods). Therefore we obtain a procedure that avoids formation of the normal matrix and that has proved to be also very stable in extensive experiments by Bodon and Luksan.

The Gauss normal equations of the second type arise e.g. in the primal-dual interior point method. They have the form

(3) $$AA^Tx = b$$

and can be solved by ABS methods in a similar way as for those of the first type. Experiments show that the ABS solution may be many orders more accurate in residual error than that by standard methods. Again it is quite incredible that such a process escaped the authors of more than 5000 papers in the IP field!

Of course open research fields are how to exploit structure of sparsity. We now that structure is usually respected in the ABS methods. About general sparsity there are few results, one important by the Chinese Zeng Li, and this is a field where people could give many contributions. Young people, not me since my interests lie now in less technical but more fundamental areas.

Redazione

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