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Contents

Obituary

Jos F. Sturm (1971–2003)

Jos Sturm was born in Rotterdam, The Netherlands, on August 13, 1971.

He graduated in August 1993 in operations research from the Department of Econometrics at the University of Groningen, where he had also been a teaching assistant in statistics and a research assistant in econometrics. From September 1993 to September 1997, he was a PhD student at the Econometric Institute and the Tinbergen Institute of the Erasmus University in Rotterdam, under supervision of Shuzhong Zhang.

Jos spent the academic year 1997/1998 as a post-doctoral fellow at the Communications Research Laboratory (CRL) of McMaster University, Hamilton, Canada, as a TALENT stipend of the Netherlands Organization for Scientific Research (NWO). At McMaster University, he was a member of the ASPC group of Zhi-Quan (Tom) Luo.

After his post-doctoral fellowship at McMaster, he returned to the Netherlands, and from October 1998 to January 2001, he was a lecturer at Maastricht University, at the Department of Quantitative Economics, in the group of Antoon Kolen.

In January 2000, his PhD thesis was awarded the Gijs de Leve prize for the best thesis in operations research in the years 1997-1999 in The Netherlands.

In February 2001, Jos was appointed as an Associate Professor at Tilburg University, and a fellow of CentER (Center for economic research). In July 2001, he was awarded the prestigious Vernieuwingsimpuls (Innovation) grant of the Netherlands Organization for Scientific Research (NWO).

Jos was the editor of the newsletter SIAG/Optimization Views-and-News of the SIAM Activity Group on Optimization (SIAG/OPT), and a council member-at-large of the Mathematical Programming Society (MPS).

His scholarly works include more than 30 papers, and his PhD thesis was published in edited form in the volume 'High Performance Optimization', Frenk et al. eds., Kluwer Academic Press, 2000. He was also the author of a widely used optimization software package called SeDuMi.

He is survived by his wife Changqing and daughter Stefanie.

Articles

R: A Statistical Tool

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1. Introduction

The objective of this article is to illustrate the R statistical system, especially as it may be used by optimization workers. R is an open-source project that was started by Robert Gentleman and Ross Ihaka of the University of Auckland, New Zealand. R follows the spirit and much of the functionality of the S language of Chambers (1998) and colleagues at Bell Laboratories. S has been developed since the 1970s, and is commercialized as S-Plus. There are many differences in the user interface and features between R and S. However, scripts written for one usually run in the other, often with minimal modification. Thus, although what is written here is about R, readers will likely find it applies "mostly" to S, at least concerning computational capabilities.

This article came about as a result of a paper I gave at the McMaster Optimization conference organized by Tamas Terlaky in August 2002. In that paper, I tried to show some of the ways that uncertainty may be introduced into the results of optimization calculations. This uncertainty is essentially a statistical property, so it was natural to use a statistical package to analyze and display results. I chose R because, in talking to optimization researchers, it was clear that many people were unaware of the capabilities of R or S. As sometimes happens with conference talks, the audience paid little attention to "uncertainty", but were very keen to have the URL `http://www.r-project.org`.

2. About R

There are several features of R that should appeal to optimizers.

- R has fairly simple and powerful graphics, including contour and [3D] plots.
- R can be extended fairly easily to incorporate C and Fortran programs.
- R already has some modest optimization functions included. Some of the routines are my own algorithms from *Compact Numerical Methods for Computers*, (Nash, 1979) implemented by Brian Ripley.
- R has a number of useful basic statistical functions that are generally lacking (or badly implemented) in traditional document processing or spreadsheet tools.

My experience is that R installs very quickly and easily using pre-compiled binaries for both Windows, Linux and Macintosh. There is also the source code for those with other systems. In addition, many researchers have contributed "packages" to carry out special computations and extend the R base. These have to be "required" by scripts, a detail that can prove troublesome to novices.

Learning to use the built-in functionality of R is not particularly difficult, though the Internet generation who have a mouse grafted into their hand will likely find its command-line interface **very** awkward. (S-Plus has pull-down menus, though I have found that I am more comfortable with R and its commands.) Generally I like to try out commands then build them into a script that I can run later. It seems that there are always minor changes in the input data or an optional parameter, so that running from a script saves time and effort. Moreover, it documents the computations when I have to come back to revise an article or answer some query about my work.

3. Graphical features of R

Optimization workers are likely to be most interested in R's graphics. Consider the following task, which we will walk through below:

Draw a [3D] perspective plot of the "volcano with terraces" function defined by

$$f(d) = (10 - .5 * d) + sin(2 * d),$$

where

$$d = \sqrt{(x-1)^2 + (y-5)^2}.$$

The "solution" to this problem is presented in Figure 1. As someone who is an occasional user of TEX, I confess to spending much more time learning how to include this graphic in the article than I did learning how to prepare a perspective plot with R.

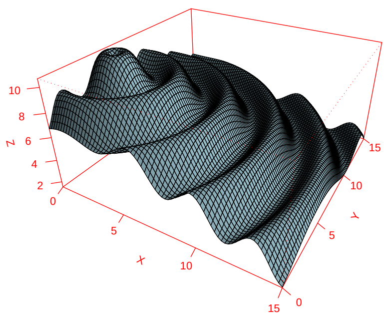

Figure 1: The volcano-with-terraces function.

The script for preparing Figure 1 in R is straightforward:

```
x <- seq(0,15, 0.2) # creates x as
   a vector 0, 0.2, 0.4,...,15
y <- seq(0,15, 0.2) # similarly for
   y
hc <- function(x, y)
d = sqrt((x - 1)^2+(y - 5)^2)
# distance from (1,5)
val = (10 - 0.5*d)+sin(2*d) # hc is
   the terraced volcano function
z <- outer(x, y, hc) # outer()
   creates a [3D] data array
persp(x, y, z, theta = 30, phi
   = 30, expand = 0.5, col =
   "lightblue",
ltheta = 120, shade = 0.75,
   ticktype = "detailed",
xlab = "X", ylab = "Y", zlab = "Z")
# Next line adds the title to the
   graph
title("jnwave function:
   volcano-with-terraces")
```

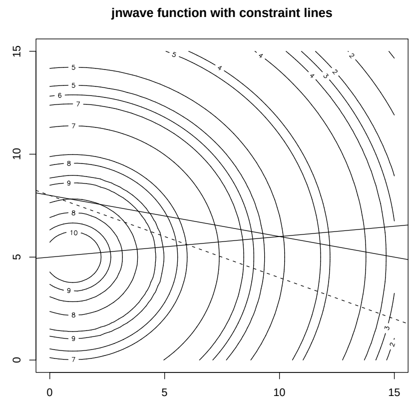

Figure 2: `jnwave` function with constraint lines.

```
# Finally we ‘‘print’’ the graph to
   an Encapsulated PostScript files
dev.print(postscript,
   file="c:/temp/jnwavep01.eps",
   horizontal=FALSE,
onefile=FALSE, paper="special")
```

I wanted to find minima of the "volcano" function subject so some linear constraints, namely,

$$A : y > 5 + 0.1x$$

and

$$B : y < 8 - 0.2x.$$

In addition, I wanted to show how uncertainty in specifying the "slope" of the second line could affect the results. For example, if the line is really

$$B' : y < 8 - 0.4x$$

we will find a very different minimum if we use constraint B' rather then B. This can be illustrated quickly via a contour plot, especially if colour is available. Figure 2 shows this using a dashed line for the modified constraint. It actually looks nicer in colour, and I had less trouble getting colour to function properly.

4. Composite graphics

When comparing different optimization runs or methods, I often want to compare output. Placing graphs side by side or in an array is helpful in picking out the differences. R makes this very easy.

As an example, suppose we wish to apply the Jackknife technique to gauge the stability of the parameters of a logistic growth function estimated by nonlinear least squares. We start with the following weed infestation data.

Year	Weed Density
1	5.308
2	7.24
3	9.638
4	12.866
5	17.069
6	23.192
7	31.443
8	38.558
9	50.156
10	62.948
11	75.995
12	91.972

We want to model this data with the growth function

$$model \cong b_1/(1 + b_2 exp(-b_3 year)).$$

We will fit the parameters by minimizing the sum of squares of the deviations between the data *weeds* and *model*. To obtain a measure of uncertainty in the parameters, however, we will estimate the parameters 13 times – once with all the data, then omitting each of the 12 observations in turn. We would then like to examine b_1, b_2, b_3 and the sum of squares for each of the 13 sets of input data. And we would like the graphs to be displayed together. This is fairly easy with R. We simply set up the graphical window first with the command

```
par(mfrow=c(2,2))
```

After this, each of four "plot" commands will be sent to a separate portion of the plotting window, along with its related commands for titles and legends. The result is in Figure 3.

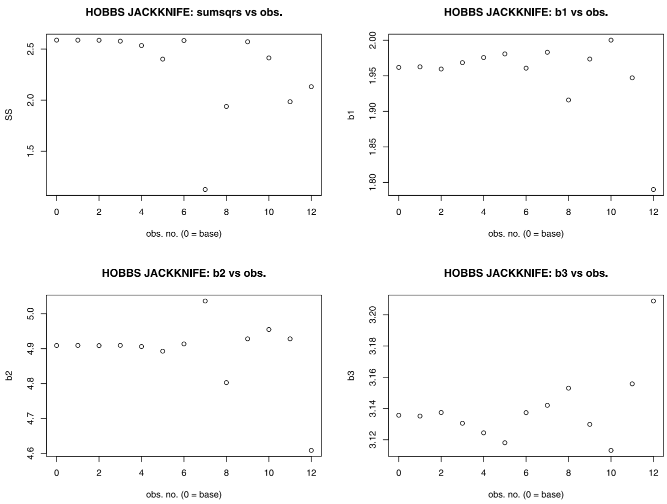

Figure 3: Hobbs weed infestation Jackknife estimates.

5. Help in learning R

There are now a number of books and online resources for learning R or S. In book form, I like Venables and Ripley (1994). Though R is not mentioned, versions of all the scripts exist for R. There are some differences in the binary formats used for R and S, but plain text is supposed to be equivalent. Venables and Ripley include an introductory chapter to the S language, followed by a chapter on graphics that gives the commands to prepare some of the more interesting outputs, then a chapter on writing your own scripts. The rest of the book covers various statistical techniques. Thus users can access the base material quickly, and then pick and choose more specialized tools.

Online (there are links on the r-project site), I like John Maindonald's tutorial (`http://wwwmaths.anu.edu.au/~johnm/r/usingR.pdf`). I walked through the examples while reading the material, and I found a printout of the tutorial helpful, along with scripts downloaded from Maindonald's site (`http://wwwmaths.anu.edu.au/~johnm`). The quick reference sheet by Jonathan Baron (`http://www.psych.upenn.edu/~baron/refcard.pdf`) can be helpful. There are other documents, some in languages other than English. All this is in addition to the official R documentation, which I find more useful for reference.

Another approach, which has wider application, is Rweb by Jeff Banfield, which has been described in an online article (`http://www.jstatsoft.org/v04/i01/Rweb/Rweb.html`) and for which the software can be downloaded (`http://www.math.montana.edu/Rweb/Resources.html`). This uses a web-server to provide example scripts for common statistical calculations. The user pastes a script into a submission box and launches the calculation. The results (text and graphic) are presented via the client's web browser. I found it straightforward to install Rweb on our Faculty's experimental server. You can try it there (`http://courses.gestion.uottawa.ca/Rweb`) or at Banfield's own site (`http://rweb.stat.umn.edu/Rweb`). A warning: it can be slow as our server is a machine that was intercepted on its way to disposal.

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Euclidean Distance Matrices and the Molecular Conformation Problem

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1. Introduction

In molecular conformation theory [7] and in multidimensional scaling in statistics [6, 17], one is usually interested in the following problem known as the *graph realizability problem* (GRP). Given an edge-weighted undirected incomplete simple graph $G = (V, E, \omega)$ with node set $V = \{v_1, v_2, \ldots, v_n\}$, edge set $E \subset V \times V$, and weights $\omega : E \rightarrow \mathbb{R}^+$, the GRP is the problem of determining whether or not G is realizable in some Euclidean space. A graph $G = (V, E, \omega)$ is said to be realizable in $\mathbb{R}^r$ if and only if there exist points $p^1, p^2, \ldots, p^n$ in $\mathbb{R}^r$ such that $\|p^i - p^j\|^2 = \omega_{ij}$ for all edges $(v_i, v_j) \in E$, where $\| \, . \, \|$ is the Euclidean norm.

In the context of molecular conformation theory, graph G represents a molecule with V, the set of nodes of G, representing the atoms. It is possible, using nuclear magnetic resonance [22, 24], to determine some of the pair-wise distances of the atoms. If the distance between atoms i and j is known, then edge (v_i, v_j) is created in graph G with weight ω_{ij} equal to the square of this distance. Naturally, the missing edges of G correspond to those unknown interatomic distances. Thus the shape of this molecule, which is important in determining its chemical and biological properties, can be found by solving the GRP. In this case we want the graph to be realizable in $\mathbb{R}^3$.

Euclidean distance matrices (EDMs) provide a useful tool in the study of the GRP. In fact, the GRP can be formulated as a Euclidean distance matrix completion problem [5]. Other optimization methods for the GRP are discussed in [14, 18, 21].

An $n \times n$ matrix $D = (d_{ij})$ is said to be a *Euclidean distance matrix (EDM)* if and only if there exist n points $p^1, p^2, \ldots, p^n$ in some Euclidean space

$\mathbb{R}^r$, such that $\|p^i - p^j\|^2 = d_{ij}$ for all $i, j = 1, \ldots, n$. The dimension of the smallest Euclidean space containing points $p^1, p^2, \ldots, p^n$ is called the *embedding dimension* of D. Given graph $G = (V, E, \omega)$, define an $n \times n$ partial symmetric matrix $A_G = (a_{ij})$ with some entries specified (or fixed) and the rest unspecified (or free) such that

$$a_{ij} := \begin{cases} \omega_{ij} & \text{if and only if } (v_i, v_j) \in E, \\ \text{free} & \text{otherwise .} \end{cases} \tag{1}$$

An $n \times n$ matrix D_1 is said to be an EDM completion of A_G if and only if D_1 is EDM and $d_{1ij} = a_{ij}$ for all fixed elements of A_G. Thus it immediately follows that $G = (V, E, \omega)$ is realizable in $\mathbb{R}^r$ if and only if A_G has an EDM completion of embedding dimension r.

In Section 2 we present some of the properties of EDMs and we show how the GRP can be formulated as a semidefinite programming problem. In Section 3, we study the problem of determining whether or not a given EDM completion is unique. Finally in Section 4 we present an interesting connection between the set of EDMs and the set of correlation matrices.

2. Euclidean distance matrices

A necessary and sufficient condition for a symmetric matrix D with zero diagonal to be a EDM was given by Schoenberg [20]. The following is a slightly modified version of Schoenberg result due to Gower [12].

Let M be the subspace orthogonal to e, the vector of all ones, i.e.,

$$M := \{e\}^{\perp} = \{x \in \mathbb{R}^n : e^T x = 0\}.$$

Let J be the orthogonal projection on M, i.e., $J := I_n - ee^T/n$ where I_n is the identity matrix of order n. Then we have the following result.

Theorem 2..1 *A matrix D with zero diagonal is EDM if and only if the matrix JDJ is negative semidefinite. Furthermore, the embedding dimension of D is given by the rank of JDJ.*

Let D be a EDM and let rank$(JDJ) = r$. Then, the points $p^1, p^2, \ldots, p^n$ that generate D are given by the rows of the $n \times r$ matrix P where $-\frac{1}{2}JDJ := PP^T$. Note that the centroid of the points p^i, $i = 1, \ldots, n$ coincides with the origin. This follows since $P^T e = 0$ which is implied by the definition of J namely $Je = 0$.

An alternative statement of Theorem 2..1 which is more convenient for our purposes is given in the next theorem [3, 8]. Let $\mathcal{S}_n$ denote the space of symmetric matrices of order n and let $\mathcal{S}_H$ denote the subspace of $\mathcal{S}_n$ defined as

$$\mathcal{S}_H = \{B \in \mathcal{S}_n : \operatorname{diag} B = 0\}.$$

Let V be the $n \times (n-1)$ matrix whose columns form an orthonormal basis of M; that is, V satisfies:

$$V^T e = 0\ , \quad V^T V = I_{n-1}\ . \tag{2}$$

Note that $J = VV^T$. Now define the linear operators $\mathcal{K}_V : \mathcal{S}_{n-1} \to \mathcal{S}_H$ and $\mathcal{T}_V : \mathcal{S}_H \to \mathcal{S}_{n-1}$ such that

$$\begin{aligned} \mathcal{K}_V(X) = {} & \operatorname{diag}(VXV^T)e^T + e(\operatorname{diag}(VXV^T))^T \\ & - 2\, VXV^T, \\ \mathcal{T}_V(D) = {} & -\tfrac{1}{2} V^T D V\ . \end{aligned}$$

Theorem 2..2 *[3] The following statements are equivalent:*

1. *$D \in \mathcal{S}_H$ is a EDM.*
2. *$\mathcal{T}_V(D)$ is positive semidefinite.*
3. *$D = \mathcal{K}_V(X)$ for some positive semidefinite matrix X in $\mathcal{S}_{n-1}$.*

Let A_G be the partial matrix associated with $G = (V, E, \omega)$. Without any loss of generality we can set the free elements of A_G initially to zero. Let H be the adjacency matrix of graph G. Then graph G has a realization in $\mathbb{R}^r$ for some positive integer $r \le n-1$ if and only if the optimal objective function of the following semidefinite programming problem is equal to zero.

$$\begin{array}{ll} \min & \mu(X) = ||H \circ (\mathcal{K}_V(X) - A_G)||_F^2 \\ \text{subject to} & \\ & X \succeq 0, \end{array} \tag{3}$$

where $||\ .\ ||_F$ is the Frobenius norm defined as $||A||_F^2 = \operatorname{trace} A^T A$, $\circ$ denotes the Hadamard product, and $X \succeq 0$ means that the matrix X is symmetric positive semidefinite. If X^* is the optimal solution of problem (3) such that $\mu(X^*) = 0$, then $D_1 = \mathcal{K}_V(X^*)$ is an EDM completion of A_G. Note that we could answer the question whether graph G has a realization in $\mathbb{R}^k$ for a given integer k simply by adding to problem (3) the side constraint

$$\text{rank } X = k.$$

Unfortunately the feasible region of problem (3) with this extra constraint is not convex. In fact, it was shown by Saxe [19] that given an edge-weighted graph G and an integer k, the problem of determining whether or not G is realizable in $\mathbb{R}^k$ is NP-hard. Where as the GRP, i.e., the problem of determining whether or not G is realizable in some Euclidean space is still open [15], the GRP can be solved "approximately" in polynomial time since problem (3) can be solved using interior point algorithms for semidefinite programming [23]. For polynomial instances of the GRP see [16].

A primal-dual interior point algorithm for problem (3) was given in [3] and numerical tests were also presented. Note that by taking H to be the matrix of all ones, problem (3) can be used to find the closest Euclidean distance matrix, in Frobenius sense, to a given matrix A. For another approach to solving the closest EDM problem see [1, 11].

3. Uniqueness of EDM completions

Given a EDM completion D_1 of a given partial matrix A_G, an interesting problem is the problem of determining whether or not D_1 is unique. A characterization of the uniqueness of D_1 is discussed next using the notion of Gale transform from the theory of polytopes [10, 13].

Let $p^1, p^2, \ldots, p^n$ be points in $\mathbb{R}^r$ whose centroid coincides with the origin. Assume that the points $p^1, p^2, \ldots, p^n$ are not contained in a proper hyperplane. Then the matrix

$$P := \begin{bmatrix} {p^1}^T \\ {p^2}^T \\ \vdots \\ {p^n}^T \end{bmatrix}$$

is of rank r. Let $\bar{r} = n-1-r$. For $\bar{r} \geq 1$, let Λ to be an $n \times \bar{r}$ matrix, whose columns form a basis for the null space of the $(r+1) \times n$ matrix $\begin{bmatrix} P^T \\ e^T \end{bmatrix}$; that is,

$$P^T\Lambda = 0, \;\; e^T\Lambda = 0, \;\; \Lambda \text{ is full column rank.} \qquad (4)$$

Λ is called a *Gale matrix* corresponding to the EDM matrix D generated by p^i, $i = 1, \ldots, n$; and the ith row of Λ, considered as a vector in $\mathbb{R}^{\bar{r}}$, is called a Gale transform of p^i. It is clear that Λ is not unique. In fact, for any nonsingular $\bar{r} \times \bar{r}$ matrix Q, ΛQ is also a Gale matrix. We will exploit this property to define a special Gale matrix Z which is more convenient for our purposes.

Let us write Λ in block form as

$$\Lambda = \begin{bmatrix} \Lambda_1 \\ \Lambda_2 \end{bmatrix},$$

where Λ_1 is $\bar{r} \times \bar{r}$ and Λ_2 is $(r+1) \times \bar{r}$. Without loss of generality we can assume that Λ_1 is nonsingular. Then Z is defined as

$$Z := \Lambda {\Lambda_1}^{-1} = \begin{bmatrix} I_{\bar{r}} \\ \Lambda_2 {\Lambda_1}^{-1} \end{bmatrix}. \qquad (5)$$

Let ${z^i}^T$ denote the i-th row of Z. i.e.,

$$Z := \begin{bmatrix} {z^1}^T \\ {z^2}^T \\ \vdots \\ {z^n}^T \end{bmatrix}.$$

Hence z^i, the Gale transform of p^i, for $i = 1, \ldots, \bar{r}$ is equal to the ith unit vector in $\mathbb{R}^{\bar{r}}$. Now we have the following result.

Theorem 3..1 *[2] Let A be a given partial symmetric matrix and let D_1 be an EDM completion of A. Let $X_1 = \mathcal{T}_V(D_1)$ and let $\bar{r} = n - 1 - rankX_1$. Then*

1. *If $\bar{r} = 0$ then D_1 is not unique.*
2. *If $\bar{r} = 1$, let Z be the Gale matrix corresponding to D_1 defined in (5). The following condition is necessary and sufficient for D_1 to be unique:*
   (a) *There exists a positive definite $\bar{r} \times \bar{r}$ matrix Ψ such that ${z^i}^T \Psi z^j = 0$ for all i, j such that a_{ij} is free.*

For a proof of this theorem and other results concerning the case $\bar{r} \geq 2$ see [2].

4. EDMs and the elliptope

As it turned out, there is an interesting connection between the set of EDMs and the set of correlation matrices, i.e., the set of symmetric positive semidefinite matrices whose diagonal is equal to e. Denote the set of all $n \times n$ EDM matrices by $\mathcal{D}_n$. Then it easily follows from Theorem 2..1 that $\mathcal{D}_n$ is a closed convex cone. Let $\mathcal{E}_n$ denote the set of $n \times n$ correlation matrices often referred to as the *Elliptope* [9]. Next we discuss the relationship between $\mathcal{D}_n$ and $\mathcal{E}_n$.

Given a convex set $K \in \mathcal{S}_n$ and a point $A \in K$, let $R(K, A)$ and $N(K, A)$ denote, respectively, the *radial cone* and the *normal cone* of K at A; that is

$$R(K, A) = \{B : B = \lambda(X - A), \forall X \in K, \lambda \geq 0\},$$
$$N(K, A) = \{B : \langle\, B, A - X \,\rangle \geq 0, \forall X \in K\},$$

where $\langle\ ,\ \rangle$ denotes the matrix inner product $\langle\, A, B \,\rangle = \operatorname{trace} AB$. Hence, $N(K, A) = (R(K, A))^\circ$, where Q° denotes the *polar* of cone Q; defined as

$$Q^\circ = \{B \in \mathcal{S}_n : \langle\, B, X \,\rangle \leq 0, \text{ for all } X \in Q\}.$$

Furthermore, $(N(K, A))^\circ$ = closure of $R(K, A) = T(K, A)$, where $T(K, A)$ is called the *tangent cone* of K at A. Let E denote the matrix of all ones. Then it is not difficult to show that

$$N(\mathcal{E}_n, E) = \{B : B = \operatorname{Diag} y - V\Phi V^T\},$$

where y is any vector in $\mathbb{R}^n$ and Φ is any positive semidefinite matrix in $\mathcal{S}_{n-1}$. It would easily follows then that $\mathcal{D}_n = -\,T(\mathcal{E}_n, E)$; i.e., the cone of EDMs of order n is equal to the negative of the tangent cone of the set of $n \times n$ correlation matrices at E, the matrix of all ones [9, Page 535].

An interesting question is whether the radial cone of $\mathcal{E}_n$ at E is closed. If this is the case then $R(\mathcal{E}_n, E) = T(\mathcal{E}_n, E)$ which would imply that for any EDM matrix D there exists a nonnegative scalar λ and a correlation matrix C such that $D = \lambda(E - C)$. Unfortunately this is not true. The radial cone $R(\mathcal{E}_n, E)$ is not closed as can be shown by the following example.

Let

$$D = \begin{bmatrix} 0 & 1 & 4 \\ 1 & 0 & 1 \\ 4 & 1 & 0 \end{bmatrix}.$$

Then clearly D is a EDM generated by the three points p^1, p^2, and p^3 on the x-axis of coordinates -1, 0 and 1 respectively. It is also clear that there exists no $\lambda \geq 0$ such that $D = \lambda(E - C_1)$ for any correlation matrix C_1 since the matrix $\lambda E - D$ is indefinite for all λ's.

For a characterization of those EDM matrices D which can be expressed as $D = \lambda(E - C)$ for some scalar λ and some correlation matrix C see [4]. Here again this characterization is given in terms of the Gale transform of the points $p^1, p^2, \ldots, p^n$ that generate D.

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Three Interviews

The three interviews that follow appeared first in the Bulletin of the International Center for Mathematics (Centro Internacional de Matemática, CIM): `http://www.cim.pt`

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An Interview with R. Tyrrell Rockafellar

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There are obvious reasons for concern about the current excessive scientific specialization and about the uncontrolled breadth of research publication. Do you see a need for increasing coordination of events and publications in the mathematical community (in particular in the optimization community) as a way to improve quality?

There are too many meetings nowadays, even too many in some specialized areas of optimization. This is regrettable, but perhaps self-limiting because of constraints on the time and budgets of participants. In many ways, the huge increase in the number of meetings is a direct consequence of globalization—with more possibilities for travel and communication (e.g. e-mail) than before, and this is somehow good. The real problem, I think, is how to preserve quality under these circumstances. Meetings shouldn't just be touristic opportunities, and generally they aren't, but in some cases this has indeed become the case. I see no hope, however, for a coordinating body to control the situation.

An aspect of meetings that I believe can definitely have a bad effect on the quality of publications is the proliferation of "conference volumes" of collected papers. This isn't a new thing, but has gotten worse. In principle such volumes could be good, but we all know that it's not a good idea to submit a "real" paper to such a volume. In fact I often did that in the past, but it's clear now that such papers are essentially lost to the literature after a few years and unavailable. Of course, the organizers of a conference often feel obliged to produce such a book in order to justify getting the money to support the conference. But for the authors, the need to produce papers for that purpose is definitely a big distraction from their more serious work. Therefore it can have a bad effect on activities that are mathematically more important.

There are also too many journals. This is a difficult matter, but it may also be self-limiting. Many libraries now aren't subscribing to all the available journals. At my own university, for example, we have decided to omit many mathematical journals that we regard as costing much more than they are worth, and this even includes some older journals that are quite well known (I won't name names). And hardly a month goes by without the introduction of yet another journal. Besides the problem of paying for all the journals (isn't this often really a kind of business trick of publishers in which ambitious professors cooperate?), there is the quality problem that there aren't enough researchers to referee the papers that get submitted. Furthermore, one sees that certain fields of research that are perhaps questionable in value and content, start separate journals of their own and thereby escape their critics on the outside. The governments paying for all of it may some day become disillusioned, and that would hurt us all.

Before I ask you questions about yourself and your work, let me pose you another question about research policy. How do you see the importance and impact of research in the professor's teaching activity? Do you consider research as a necessary condition for better university teaching?

Personally, I believe that an active acquaintance with research is important to teaching mathematics on many levels. The nature of the subject being taught, and the kind of research being done, can make a big difference in this, however. Ideally, mathematics should be seen as a thought process, rather than just as a mass of facts to be learned and remembered, which is so often the common view. The thought process uses logic but also abstraction and needs to operate with a clear appreciation of goals, whether coming directly out of applications or for the sake of more complete insights into a central issue.

Even with standard subjects such as calculus, I think it's valuable to communicate the excitement of the ideas and their history, how hard they were to develop and understand properly—which so often reflects difficulties that students have themselves. I don't see how a teacher can do that well without some direct experience in how mathematics continues to grow and affect the world.

On the higher levels, no teacher who does not engage in research can even grasp the expanding knowledge and prepare the next generation to carry it forward. And, practically speaking, without direct contact with top-rate researchers, a young mathematician, no matter how brilliant, is doomed to a scientifically dull life far behind the frontiers.

You started your career in the sixties working intensively in convex analysis. Your book "Convex Analysis", Princeton University Press, 1970, became a landmark in the field. How exciting was that time and how do you see now the impact that the book had in the applied mathematical field?

C. Carathéodory, W. Fenchel, V. L. Klee, J.-J. Moreau, F. A. Valentine,... Who do you really think that set the ground for convex analysis? Werner Fenchel?

Was it A. W. Tucker himself who suggested the name "Convex Analysis"? What are your recollections of Professor Tucker and his influential activity?

Some of the history of "convex analysis" is recounted in the notes at the ends of the first two chapters of my book Variational Analysis, written with Roger Wets. Before the early 1960's, there was plenty of convexity, but almost entirely in geometric form with little that could be called "analysis". The geometry of convex sets had been studied by many excellent mathematicians, e.g. Minkowski, and had become important in functional analysis, specifically in Banach space theory and the study of norms. Convex functions other than norms began to attract much more attention once optimization started up in the early 1950's, and through the economic models that became popular in the same era, involving games, utility functions, and the like. Still, convex functions weren't handled in a way that was significantly different from that of other functions. That only came to be true later.

As a graduate student at Harvard, I got interested in convexity because I was amazed by linear programming duality and wanted to invent a "nonlinear programming duality". That was around 1961. The excitement then came from all the work going on in optimization, as represented in particular by the early volumes of collected papers being put together by Tucker and others at Princeton, and from the beginnings of what later become the sequence of Mathematical Programming Symposia. It didn't come from anything in convexity itself. At that time, I knew of no one else who was really much interested in trying to do "new" things with convexity. Indeed, nobody else at Harvard had much awareness of convexity, not to speak of optimization.

It was while I was writing up my dissertation—focused then on dual problems stated in terms of polar cones—that I came across Fenchel's conjugate convex functions, as described in Karlin's book on game theory. They turned out to be a wonderful vehicle expressing for "nonlinear programming duality", and I adopted them wholeheartedly. Around the time the thesis was nearly finished, I also found out about Moreau's efforts to apply convexity ideas, including duality, to problems in mechanics.

Moreau and I independently in those days at first, but soon in close exchanges with each other, made the crucial changes in outlook which, I believe, created "convex analysis" out of "convexity". For instance, he and I passed from the basic objects in Fenchel's work, which were pairs consisting of a convex set and a finite convex function on that set, to extended-real-valued functions implicitly having "effective domains", for which we moreover introduced set-valued subgradient mappings. Nevertheless, the idea that convex functions ought to be treated geometrically in terms of their epigraphs instead of their graphs was essentially something we had gotten from Fenchel.

Less than a year after completing my thesis, I went to Copenhagen to spend six months at the institute where Fenchel was working. He was no longer engaged then in convexity, so I had no scientific interaction with him in that respect, except that he arranged for Moreau to visit, so that we could talk.

Another year later, I went to Princeton for a whole academic year through an invitation from Tucker. I had kept contact with him as a student, even though I was at Harvard, not Princeton, and had never actually met him. (He had helped to convince my advisor that my research was promising.) He had me teach a course on convex functions, for which I wrote the lecture notes, and he then suggested that those notes be expanded to a book. And yes, it was he who suggested the title, Convex Analysis, thereby inventing the name for the new subject.

So, Tucker had a great effect on me, as he had had on others, such as his students Gale and Kuhn. He himself was not a very serious researcher, but he believed in the importance of the new theories growing out of optimization. With his personal contacts and influence, backed by Princeton's prestige, he acted as a major promoter of such developments, for example by arranging for "Convex Analysis" to be published by Princeton University Press. I wonder how the subject would have turned out if he hadn't moved me and my career in this way.

I think of Klee (a long-time colleague of mine in Seattle, who helped me get a job there), and Valentine (whom I once met but only briefly), as well as Caratheodory, as involved with "convexity" rather than "convex analysis". Their contributions can be seen as primarily geometric.

Since the mid seventies you have been working on stochastic optimization, mainly with Roger Wets. It seems that it took a long while to see stochastic optimization receiving proper attention from the optimization community. Do you agree?

I owe my involvement in stochastic programming to Roger Wets. This was his subject when we first became friends around 1965. He has always been motivated by its many applications, whereas for me the theoretical implications, in particular the ones revolving around, or making use of duality, provided the most intriguing aspects. We have been good partners from that perspective, and the partnership has lasted for a long time.

Stochastic programming has been slow to gain ground among practitioners for several reasons, despite its obvious relevance to numerous problems. For many years, the lack of adequate computing power was a handicap. An equal obstacle, however, has been the extra mental machinery required in treating problems in this area and even in formulating them properly. I have seen that over and over, not just in the optimization community but also in working with engineers and trying to teach the subject to students. A different way of thinking is often needed, and people tend to resist that, or to feel lost and retreat to ground they regard as safer. I'm confident, though, that stochastic programming will increasingly be accepted as an indispensable tool for many purposes.

Your recent book "Variational Analysis", Springer-Verlag, 1998, with Roger Wets, emerges as an overwhelming life-time project. You say in the first paragraph of the Preface: "In this book we aim to present, in a unified framework, a broad spectrum of mathematical theory that has grown in connection with the study of problems of optimization, equilibrium, control, and stability of linear and nonlinear systems. The title Variational Analysis reflects this breadth." How do you feel about the book a few years after its publication? Has the purpose of forming a "coherent branch of analysis" been well digested by the book audience?

That book took over 10 years to write—if one includes the fact that at least twice we decided to start the job from the beginning again, totally reorganizing what we had. In that period I had the feeling of an enormous responsibility, but a joyful burden one even if involved with pain, somewhat like a woman carrying a baby within her and finally giving birth. I am very happy with the book (although it would be nice to have an opportunity to make a few little corrections), and Wets and I have heard many heart-warming comments about it. Also, it has won a prize[1].

Still, I have to confess that I have gone through a bit of "*post partum* depression" since it was finished. It's clear—and we knew it always —that such a massive amount of theory can't be digested very quickly, even by those who could benefit from it the most. Another feature of the situation, equally predictable, is that some of the colleagues who could most readily understand what we have tried to do often have their own philosophies and paradigms to sell. It's discouraging to run into circumstances where developments we were especially proud of, and which we regarded as very helpful and definitive, appear simply to be ignored.

But in all this I have a very long view. We now take for granted that "convex analysis" is a good subject with worthwhile ideas, yet it was not always that way. There was actually a lot of resistance to it in the early days, from individuals who preferred a geometric presentation to one targeting concepts of analysis. Even on the practical plane, it's fair to say that little respect was paid to convex analysis in numerical optimization until around 1990, say. Having seen how ideas that are vital, and sound, can slowly win new converts over many years, I can well dream that the same will happen with variational analysis.

Of course, in the meantime there are many projects to work on, whether directly based on variational analysis or aimed in a different direction, and such matters are keeping me thoroughly busy.

Nonlinear optimization has been also part of your research interests, in particular duality and Lagrange multiplier methods. Nonlinear optimization has been recently enriching its classical methodology with new techniques especially tailored to simulation models that are expensive, ill-posed or that require high performance computing. Would you like to elaborate your thoughts on this new trend?

The growth of numerical methodology based on duality and new ways of working with, or conceiving of, Lagrange multipliers has been thrilling. Semidefinite programming fits that description, but so too do the many decomposition schemes in large-scale optimization, including optimal control and stochastic programming. Also in this mix, at least as close cousins, are schemes for solving variational inequality problems.

I've been active myself in some of this, but on a more basic level of theory a bigger goal has been to establish a better understanding of how solutions to optimization problems, both of convex and non-convex types, depend on data parameters. That's essential not only to numerical efficacy and simula-

[1] *Frederick W. Manchester Prize (INFORMS, 1997).*

tion, but also to the stability of mathematical models. I find it to be a tough but fascinating area of research with broad connections to other things. It requires us to look at problems in different ways than in the past, and that's always valuable. Otherwise it won't be possible to bring optimization to the difficult tasks for which it is greatly needed in economics and technology.

Let me now increase my level of curiosity and ask you more personal questions. The George B. Dantzig Prize (SIAM and Mathematical Programming Society, 1982), the The John von Neumann Lecture (SIAM, 1992), and the John von Neumann Theory Prize (INFORMS, 1999) are impressive recognitions. However, it is clear that it is neither recognition nor any other oriented-career goal that keeps you moving on. What makes you so active at your age? Are you addicted to mathematics?

It's the excitement of discovering new properties and relationships—ones having the intellectual beauty that only mathematics seems able to bring—that keeps me going. I never get tired of it. This process builds its own momentum. New flashes of insight stimulate curiosity more and more.

Of course, a mathematician has to be in tune with some of the basics of a mathematical way of life, such as pleasure in spending hours in quiet contemplation, and in dedication to writing projects. But we all know that this somewhat solitary side of mathematical life also brings with it a kind of social life that few people outside of our professional world can even imagine. The frequent travel that's not just tied to a few laboratories, the network of friends and research collaborators in different cities and even different countries, the extended family of former students, and the interactions with current students—what fun, and what an opportunity to explore music, art, nature, and our many other interests. All these features keep me going too.

Recently, at the end of a live radio interview by telephone that was being broadcast nationally in Australia, I was asked whether I really liked mountain hiking and backpacking. The interviewer had seen that about me on a web site and appeared to be incredulous that someone with such outdoor activities could fit her mental picture of a mathematician. So little did she know about the lives we lead!

Have you ever felt that a result of yours was unfairly neglected? Which? Why?

Yes, I have often felt that certain results I had worked very hard to obtain, and which I regarded as deep and important, were neglected. That was the case in the early days and still goes on now. For instance, the duality theorems I developed in the 1960's, connecting duality with perturbations, were ignored for a long time while most people in optimization thought only about "Lagrangian duality". And in the last couple of years, I and several of my students have worked very hard at bringing variational analysis to bear on Hamilton-Jacobi theory, but despite strong theorems can't seem to get attention from the PDE people who work in that subject.

In most cases the trouble has come from the fact that new ideas have been involved which other people didn't have the time or energy to appreciate. That can be an unhappy state of affairs, but time can change it. I've never been seriously bothered by it and have simply operated on the principle that good ideas will come through eventually. This has in fact been my experience.

Anyway, there are always so many other exciting projects to work on that one can't be very distracted by such disappointments, which may after all only be temporary.

What would you like to prove or see proven that is still open?

Oh, this is a hard kind of question for me. I belong to the class of mathematicians who are theory-builders more than problem-solvers. I get my satisfaction from being able to put a subject into a robust new framework which yields many new insights, rather than from cracking a hard nut like Fermat's last theorem. Of course, I spend a lot of time proving a lot of things, but for me the main challenge ultimately is trying to get others to look at something in a different and better way. Of course, that can be frustrating! But, to tie it in with an earlier question, a key part is getting students to follow the desired thought patterns. That's good for them and also for the theoretical progress. Without having been so deeply engaged with teaching for many years, I don't think I could have gone as far with my research.

So, if I would state my own idea of an open challenge, it would be, for instance, on the grand scale of enhancing the appreciation and use of "variational analysis" (by which I don't just mean my book!). I do nonetheless have specific results that I would like to be able to prove in several areas, but they would take much more space to describe.

What was the most gratifying paper you ever wrote? Why?

Oh, again very hard to say. There are so many papers, and so many years have gone by. And I've worked on so many different topics, often in different directions. Anyway, for "gratification" it's hard to beat books. The two books that I'm most proud of are obviously Convex Analysis and Variational Analysis. Both have greatly gratified me both "externally" (recognition) and "internally" (personal feeling of accomplishment). So far, Convex Analysis has been the winner externally, but Variational Analysis is the winner internally.

About the interviewee: R. Tyrrell Rockafellar completed his undergraduate studies at Harvard University in 1957, and his PhD in 1963 at Harvard as well. He has been in the faculty of the Department of Mathematics of the University of Washington since 1966.

His research and teaching interests focus on convex and variational analysis, optimization, and control. He is well known in the field and his contributions can be found in several books and in more than one hundred papers.

Professor Rockafellar gave a plenary lecture in the conference Optimization 2001, held in Aveiro, Portugal, July 23-25, 2001.

Interviewer: L. N. Vicente (University of Coimbra, Portugal).

An Interview with M. J. D. Powell

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I am sure that our readers would like to know a bit about your academic education and professional career first. Why did you choose to go to the Atomic Energy Establishment (Harwell) right after college in 1959?

When I studied mathematics at school, nearly all of my efforts were applied to solving problems in text books, instead of reading the texts. Then my teachers marked and discussed my solutions instead of instructing me in a formal way. I enjoyed this kind of work greatly, especially when I was able to find answers to difficult questions myself. Thus I gained a good understanding of some fields of mathematics, but I became unwilling to learn about new subjects at a general introductory level, because I do not have a good memory, and to me it was without fun. I also disliked the breadth of the range of courses that one had to take at Cambridge University as an undergraduate in mathematics. Fortunately, I was able to complete that work adequately in two years, which allowed me to study for the Diploma in Numerical Analysis and Computation during my third year. It was a relief to be able to solve problems again most of the time, and the availability of the Edsac 2 computer was a bonus. I welcomed the use of analysis and the satisfaction of obtaining answers. I wished to continue this kind of work after graduating, but the possibility of remaining in Cambridge for a higher degree was not suggested to me. Contributing to academic research and publishing papers in journals were not suggested either, although I developed a successful algorithm for adaptive quadrature in a third year project. Therefore in 1959 I applied for three jobs at government research establishments, where I would assist scientists with numerical computer calculations. I liked the location of Harwell and the people who interviewed me there, so it was easy for me to accept their offer of employment.

You obtained your doctor of science only in 1979, *twenty years after your bachelors degree and three years after being hired as a professor in Cambridge. Why was that the case?*

After graduating from Cambridge in 1959 with a BA degree, I had no intention of obtaining a doctorate. All honours graduates from Cambridge are eligible for an MA degree after about 3 further years, without taking any more courses or examinations, but from my point of view that opportunity was not advantageous, partly because one had to pay a fee. When I became the Professor of Applied Numerical Analysis at Cambridge in 1976, I was granted all the privileges of an MA automatically, and my official degree became BA with MA status. Two years later, I was fortunate to be elected as a Professorial Fellow at Pembroke College, and the Master of Pembroke suggested that I should follow the procedure for becoming a Master of Arts. Rather than expressing my reservations about it, I offered to seek an ScD degree instead, which required an examination of much of my published work. Thus I became an academic doctor in 1979.

Was it hard to adapt to the academic life after so many years in Harwell?

After about five years at Harwell, most of my time was spent on research, which included the development of Fortran software for general computer calculations, the theoretical analysis of algorithms, and of course the publication of papers. The purpose of the administrative staff there was to make it easier for scientists to carry out their work. On the other hand, I found at Cambridge that one had to create one's own opportunities for research, which required some stubbornness and lack of cooperation, because of the demands of teaching, examining and admitting students, and also because administrative duties at universities can consume the time that remains, especially during terms. This change was particularly unwelcome, and is very different from the view that most of my relatives and friends have of university life. Indeed, when I was at Harwell they did not doubt that I had a full time job, but they assume that at Cambridge the vacations provide a life of leisure.

In your work in optimization we find several interesting and meaningful examples and counter-examples. Where did you get this training (assuming that not all is natural talent)? From your exposure to approximation theory? From the hand calculations of the old computing times?

The construction of examples and counter-examples is a natural part of my strong interest in problem solving, and of the development of software that I have mentioned. Specifically, numerical results during the testing of an algorithm often suggest the convergence and accuracy properties that are achieved, so conjectures arise that may be true or false. Answers to such questions are either proofs or counter-examples, and often I have tried to discover which of these alternatives applies. Perhaps my training started with my enjoyment of geometry at school, but then the solutions were available. I am pleased that you mention hand calculations, because I still find occasionally that they are very useful.

Was exemplification a relevant tool for you when you taught numerical analysis classes? Did your years as a staff member at Harwell influence your teaching?

My main aim when teaching numerical analysis to students at Cambridge was to try to convey some of the delightful theory that exists in the subject, especially in the approximation of functions. Only 36 lectures are available for numerical analysis during the three undergraduate years, however, except that there are also courses on computer projects in the second and third years, where attention is given to the use of software packages and to the numerical results that they provide. Moreover, in most years I also presented a graduate course of 24 lectures, in order to attract research students. The main contribution to my teaching from my years at Harwell was that I became familiar with much of the relevant theory there, because it was developed after I graduated in 1959, but I hardly ever mentioned numerical examples in my lectures, because of the existence of the Cambridge computer projects, and because the mathematical analysis was more important to my teaching objectives. Therefore my classes were small. Fortunately, some of the strongest mathematicians who attended them became my research students. I am delighted by their achievements.

Could you tell us how computing resources evolved at Harwell in the sixties and seventies and how that impacted on the numerical calculations of those times?

Beginning in 1958, I have always found that the speed of computers and the amount of storage are excellent, because of the huge advances that occur about every three years. On the other hand, the turnaround time for the running of computer programs did not improve steadily while I was at Harwell. Indeed, for about four years after I started to use Fortran in 1962, those programs were run on the IBM Stretch computer at Aldermaston, the punched cards being transported by car. Therefore one could run each numerical calculation only once or twice in 24 hours. Of course it was annoying to have to wait so long to be told that one had written DIMESNION instead of DIMENSION, but ever since I have been grateful for the careful attention to detail that one had to learn in that environment. Moreover, it was easier then to develop new algorithms that extend the range of calculations that can be solved. Conveying such advances to Harwell scientists was not straightforward, however, mainly because they wrote their own computer programs, using techniques that were familiar to them. The Harwell Subroutine Library, which I started, was intended to help them, and to reduce duplication in Fortran software. Often it was highly successful, but many computer users, both then and now, prefer not to learn new tricks, because they are satisfied by the huge gains they receive from increases in the power of computers.

You once wrote: "Usually I produced a Fortran program for the Harwell subroutine library whenever I proposed a new algorithm,..."[1]*. In fact, writing numerical software has always been a concern of yours. Could you have been the same numerical analyst without your numerical experience?*

[1] *A View of Nonlinear Optimization*, History of Mathematical Programming: A Collection of Personal Reminiscences (J.K. Lenstra, A.H.G. Rinnooy Kan, and A. Schrijver eds), North-Holland (Amsterdam), 119-125 (1991).

My principal duty at Harwell was to produce Fortran programs that were useful for general calculations, which justified my salary. My work on the theoretical side of numerical analysis was also encouraged greatly, and its purpose was always to advance the understanding of practical computation. Indeed, without numerical experience, I would be cut off from my main source of ideas. It is unusual for me to make progress in research by studying papers that other people have written. Instead I seek fields that may benefit from a new algorithm that I have in mind. I also try to explain and to take advantage of the information that is provided by both good and bad features of numerical results.

Roger Fletcher wrote once that "your style of programming is not what one might call structured". Some people think that a piece of software should be well structured and documented. Others that it should be primarily efficient and reliable. What are your views on this?

I never study the details of software that is written by other people, and I do not expect them to look at my computer programs. My writing of software always depends on the discipline of subroutines in Fortran, where the lines of code inside a subroutine can be treated as a black box, provided that the function of each subroutine is specified clearly. Finding bugs in programs becomes very painful, however, if there are any doubts about the correctness of the routines that are used. Therefore I believe that the reliability and accuracy of individual subroutines is of prime importance. If one fulfils this aim, then in my opinion there is no need for programs to be structured in a formal way, and conventional structures are disadvantageous if they do not suit the style of the programmer who must avoid mistakes. Those people who write reliable software usually achieve good efficiency too. Of course it is necessary for the documentation to state what the programs can do, but otherwise I do not favour the inclusion of lots of internal comments.

And by the way, how do you regard the recent advances in software packages for nonlinear optimization?

Most of my knowledge of recent advances in software packages has been gained from talks at conferences. I am a strong supporter of such activities, as they make advances in numerical analysis available for applications. My enthusiasm diminishes, however, when a speaker claims that his or her software has solved successfully about 90% of the test problems that have been tried, because I could not tolerate a failure rate of 10%. Another reservation, which applies to my programs too, is that many computer users prefer software that has not been developed by numerical analysts. I have in mind the popularity of simulated annealing and genetic algorithms for optimization calculations, although they are very extravagant in their use of function evaluations.

Many people working in numerical mathematics undervalue the paramount importance of numerical linear algebra (matrix calculations). Would you like to comment on this issue? How often was research in numerical linear algebra essential to your work in approximation and optimization?

An optimization algorithm is no good if its matrix calculations do not provide enough accuracy, but, whenever I try to invent a new method, I assume initially that the computer arithmetic is exact. This point of view is reasonable for the minimization of general smooth functions, because techniques that prevent serious damage from nonlinear and nonquadratic terms in exact arithmetic can usually cope with the effects of computer rounding errors, as in both cases one has to restrict the effects of perturbations. Therefore I expect my algorithms to include stability properties that allow the details of the matrix operations to be addressed after the principal features of the algorithm have been chosen. Further, I prefer to find ways of performing the matrix calculations myself, instead of studying relevant research by other people.

I read in one of your articles that "a referee suggested rejection because he did not like the bracket notation". What is your view about the importance of refereeing? How do you classify yourself as a referee?...

The story about the bracket notation is remarkable, because the paper that was nearly rejected is the one by Roger Fletcher and myself on the Davidon–Fletcher–Powell (DFP) algorithm. As a referee, I ask whether submitted work makes a substantial contribution to its subject, whether it is correct, and whether the amount of detail is about right. I believe strongly that we can rely on the accuracy of published papers only if someone, different from the author(s), checks every line that is written, and in my opinion that task is the responsibility of referees. When it is done carefully, then refereeing becomes highly important. I try to act in this way myself, but, because my general knowledge of achievements in my fields is not comprehensive, I often consider submissions in isolation, although I should relate them to published work.

Actually, in my previous question I had in mind the difficulty that others might face to meet your high standards. This brings me to your activity as a Ph.D adviser. What difficulties and what rewards do you encounter when advising Ph.D. students?

Of course I take the view that my requirements for the quality of the work of my PhD students are reasonable. I require their mathematics to be correct, I require relevance to numerical computation, and I require some careful investigations of new ideas, instead of a review of a subject with some superficial originality. Further, I prefer my students to work on topics that are not receiving much attention from other researchers, in order that they can become leading experts in their fields. Some of them have succeeded in this way, which is a great reward, but two of them switched to less demanding supervisors, and another one switched to a well paid job instead of completing his studies. I also had a student that I never saw after his first four terms. Eventually he submitted a miserable thesis, that was revised after his first oral examination, and then the new version was passed by the examiners, but the outcome would have been different if university regulations had allowed me to influence the result. On the other hand, all my other students have done excellent work and have thoroughly deserved their PhDs. One difficulty has occurred in several cases, namely that, because each student has to gain experience and to make advances independently, one may have to allow his or her rate of progress to be much slower than one could achieve oneself. Another difficulty is that my knowledge of pure mathematics has been inadequate for easy communication between myself and most of my students who have studied approximation theory. Usually they were very tolerant about my ignorance of distributions and properties of Fourier transforms, for example, but my heart sinks when I am asked to referee papers that depend on these subjects.

Most of your publications are single-authored. Why do you prefer to work on your own?

I believe I have explained already why I enjoy working on my own. Therefore, when I begin some new research, I do not seek a co-author. Moreover, as indicated in the last paragraph, I prefer my students to make their own discoveries, so usually I am not a co-author of their papers.

I have been trying to avoid technical questions but there is one I would like to ask. What is your view on interior-point methods (a topic where you made only a couple — but as always relevant and significant — contributions)?

My view of interior point methods for optimization calculations with linear constraints is that it seems silly to introduce nonlinearities and iterative procedures for following central paths, because these complications are not present in the original problem. On the other hand, when the number of constraints is huge, then algorithms that treat constraints individually are also unattractive, especially if the attention to detail causes the number of iterations to be about the number of constraints. It is possible, however, to retain linear constraints explicitly, and to take advantage of the situation where the boundary of the feasible region has so many linear facets that it seems to be smooth. This is done by the TOLMIN software that I developed in 1989, for example, but the number of variables is restricted to a few hundred, because quadratic models with full second derivative matrices are employed. Therefore eventually I expect interior point methods to be best only if the number of variables is large. Another reservation about this field is that it seems to be taking far more than its share of research activity.

You published a book in approximation theory. Have you ever thought about writing a book in nonlinear optimization?

My book on Approximation Theory and Methods was published in 1981. Two years later, my son died in an accident, and then I wished to write a book on Nonlinear Optimization that I would dedicate to him. I have not given up this idea, but other duties, especially the preparation of work for conferences and their proceedings, have caused me to postpone the plan. Of course, because of the circumstances, I would try particularly hard to produce a book of high quality.

Let me end this interview with the very same questions I asked T.R. Rockafellar (who, by the way, shared with you the first Dantzig Prize in 1982). Have you ever felt that a result of yours was unfairly neglected? Which? Why? What would you like to prove or see proven that is still open (both in approximation theory and in nonlinear optimization)? What was the most gratifying paper you ever wrote? Why?

I was taught the FFT (Fast Fourier Transform) method by J.C.P. Miller in 1959, and then it made Cooley and Tukey famous a few years later. Moreover, my 1963 paper with Roger Fletcher on the DFP method is mainly a description of work by Davidon in 1959, and it has helped my career greatly. Therefore, by comparison, none of my results has been unfairly neglected. My main theoretical interest at present is trying to establish the orders of convergence that occur at edges, when values of a smooth function are interpolated by the radial basis function method on a regular grid, which is frustrating, because the orders are shown clearly by numerical experiments. In nonlinear optimization, however, most of my attention is being given to the development of algorithms. Since you ask me to mention a gratifying paper, let me pick "A method for nonlinear constraints in minimization problems", because it is regarded as one of the sources of the "augmented Lagrangian method", which is now of fundamental importance in mathematical programming. I have been very fortunate to have played a part in discoveries of this kind.

About the interviewee: M. J. D. Powell completed his undergraduate studies at the University of Cambridge in 1959. From 1959 to 1976 he worked at the Atomic Energy Establishment, Harwell, where he was Head of the Numerical Analysis Group from 1970. He has been John Humphrey Plummer Professor of Applied Numerical Analysis, University of Cambridge since 1976 and a Fellow of Pembroke College, Cambridge since 1978.

He made many seminal contributions in approximation theory, nonlinear optimization, and other topics in numerical analysis. He has written a book in approximation theory and more than one hundred and fifty papers.

Interviewer: L. N. Vicente (University of Coimbra, Portugal).

An Interview with W. R. Pulleyblank

Published originally in Bulletin of the Internacional Center for Mathematics, n. 15, December 2003.
http://www.cim.pt/cimE/boletim.html

Many of us have no idea as to how is the research environment in a private laboratory like the IBM T.J. Watson Research Center. Could you start by telling us about this research environment, in particular the one in the Department of Mathematical Sciences?

There is probably as much difference between different industrial research laboratories as there is between different universities. IBM Research has consistently had a mission that combined carrying out a top level scientific research agenda with the desire to make the results relevant to the corporation. In some ways, the Mathematical Sciences Department operates like a university department. We write and referee papers, edit journals and present papers at conferences. Some department members teach courses and supervise graduate students at nearby universities, for example, Columbia, NYU, Yale and MIT. However, there are significant differences. Many of the problems we work on come from IBM customers and other units within IBM. Often we are able to apply our work directly to real world problems. In addition, we always have the possibility of seeing the results that our research realized in the form of products. We do get pretty excited when this happens.

I recently heard a biographer of T. J. Watson (the father) emphasizing the importance of Research in the early days of the IBM company. Do you also think that research has played a vital role in the long success of the company?

Absolutely. When Lou Gerstner, our previous Chairman, formulated the principles that he wanted to guide the company, the first was “at our core, we are a technology company”. He was a strong supporter of Research, as is our present chairman, Sam Palmisano. There is a feeling here that the things we do really have a chance to have an impact on the company and on our customers. It is very energizing.

In particular, how do you envision the Department of Mathematical Sciences thirty years from now? Will research staff there continue to do basic research and prove theorems?

I hope so. The model of combining serious mathematics with doing things that have the potential to positively affect the company has been remarkably successful, and robust. Examples range from devices like the Trackpoint, to software systems like OSL, to inventing new algorithms for digital half-toning. At the same time, there has been a remarkable collection of papers and books written by department members. The legacy of current and former department members like Shmuel Winograd, Mike Shub, Roy Adler, Alan Hoffman, Ellis Johnson, Ralph Gomory and Herman Goldstine sets quite an example!

Now, let us focus on your career. In 1990 you moved from the University of Waterloo in Canada to the Watson Research Center. Would you like to comment on those times? What was the driving force that made you move?

In 1987, I was awarded an NSERC Industrial Research Chair in Optimization and Computer Applications, in part funded by CP Rail. This gave me a chance to expand the applied part of my research program, and also started me thinking about what I would do for the next 25 years of my career. In the spring of 1989, Ellis Johnson called and supposed that I would not move, but wanted suggestions for a possible successor to himself as Manager of the Optimization Center. It got me thinking about alternatives and I came here for a visit. I soon realized that IBM Research would be an excellent place to work on applied problems. Also, earlier in my career I had worked for IBM as a systems engineer. I had always had a high regard for the company, and the Watson Research Center had always seemed to me to be an exciting place.

So, after a lot of discussion with my wife, Diane, we decided to give it a shot and here we are.

How did your research program change as result of moving to the industry?

It evolved. I have always been an interactive mathematician, enjoying working with co-authors. Here I had the chance to work with people like John Tomlin, John Forrest and Ellis Johnson. I became very interested in computational questions. I was granted my first patent ever, jointly with John Tomlin and Alan Hoffman — an application of the Koenig edge coloring theorem to make certain matrix computations much more efficient.

A few years later you were chairing the Department of Mathematical Sciences in the Watson Research Center. How do you describe the leadership skills required for this job in comparison to those needed for a similar academic position?

The scope of a department Director's job is in some ways similar in scale to that of a dean. Tom Brzustowski when he was Provost at the University of Waterloo, described the faculty at the university as "800 small businessmen sharing a library and a parking lot". Today, he would probably add a computer network to the list. IBM is a much more hierarchical organization. When someone becomes a manager, it is not assumed that it is a temporary, three to five year, assignment. A department Director has a responsibility to generate funding for the department as well as to make sure that the careers of department members are progressing satisfactorily. In addition, it is important to understand and be able to present all the work done in a department. This has really encouraged me to broaden my outlook. For example, I am sure that I know much more Computational Biology now than I ever would have learned in a university mathematics department.

Do you think that an academic training can position one better for the industry than the other way around? I mean, do you think that someone with a career in the industry would have had a more difficult time chairing an academic department?

I believe that some of the skills needed for success in an industrial research position can be learned on the job. However, I think that the only way to understand what it takes to carry out a serious research agenda is to do it oneself. I think it is feasible for a person who has worked in an industrial research lab like IBM to be quite successful in a university environment — there are several examples I can think of who have made this switch. However, I have not seen many people who have not got a research background being very successful running a research department in industry or at a university.

How did you find time to write a book during those years? Has it payed off?

Ron Graham once, when asked a similar question, said that his secret is that every day contains 24 hours! You can do a lot of things if you decide to do so. In the case of our best seller Combinatorial Optimization (number 158,831 on the Amazon best seller list!), the big thing was the co-authors. Bill Cook and I launched it one night at Oberwolfach. We began by getting a group of luminaries to contribute comments for the back of the dustcover. Our plan was to write the index next, because then, we thought, it would be simple to write the book — just see what page we were on, check in the index, and see what had to go there. Later Bill Cunningham and Lex Schrijver joined the project. It was really interesting working through this material, that we all really loved, trying to combine our different pedagogic approaches.

I enjoyed doing it — I learned a lot and am very satisfied with the end result. However, I still earn more from my day job than from my book royalties.

Could you also tell us a bit about the Deep Computing project which you are currently coordinating? Has it been a rewarding experience for you? How will it impact IBM's future development policies?

The Deep Computing Institute at IBM Research was formed following our second chess match with Gary Kasparov in 1997 (which IBM's Deep Blue won 3.5 to 2.5!). The challenge was to see what we could do to take the ideas and apply them to a much broader set of problems. The idea of combing large amounts of computation and data to solve business and scientific decision problems is very broad, and the challenge has been to make it concrete. The breadth of topics — from simulation to optimization to data mining to advanced computation has been extremely interesting and has, I believe, led to some interesting research. For example, one of the projects I am currently leading is to construct BlueGene — the largest supercomputer in the world (by a large margin).

Let us now backtrack to the old days in Waterloo. It always impressed me in Waterloo the existence of a School of Mathematics, consisting of different departments. Did you see it as positive too?

The University of Waterloo has always been a very successful and innovative institution. In the sixties, the university decided to focus on mathematics, engineering, and an emerging discipline — computer science. It pioneered coop education in Canada. The idea of creating a Faculty of Mathematics, including pure and applied mathematics, statistics, computer science and "Combinatorics and Optimization" had very positive consequences. There were stresses and conflicts that had to be resolved, but it seemed easier because of the common background of so many of the faculty members. And, it was really fun being bigger than Engineering!

How exciting was to do combinatorics in Waterloo? Who had a greater impact on you? Do you miss those times?

It was wonderful. I spent two and a half years at Waterloo as a PhD student and nine years there as a faculty member. I had the great fortune to be part of an extraordinary group of researchers in C&O. Jack Edmonds was a huge influence and we were all inspired by being able to work around Bill Tutte. I really enjoyed the time I spent as Managing Editor of Journal of Combinatorial Theory–B with Bill, Adrian Bondy and U.S.R. Murty. One of the exciting things was the set of visitors continually passing through. I also really enjoyed working with some of the young pups — we had quite a few Bruces at Waterloo — ranging from PhD students to Full Professors. They were an amazing bunch of colleagues. Somehow the group of students, postdocs and faculty members formed an amazingly homogeneous group of researchers. The thing that mattered most was the mathematics — everything else existed to support that.

We talked about mathematics in Canada and I don't want to miss this opportunity to ask you to compare the pre-college mathematical education in Canada to the one in the United States.

Clearly there is a huge variety within both countries. I do think that the Canadian system has been more strongly influenced by the British, or European model, and we expect students to take significant responsibility for their own programs and activities. The American system has huge diversity — ranging from top tier research schools to nurturing educational environments. Top schools in both countries are very competitive with each other.

Also, do you think graduate programs in US are stronger than in Canada, especially when it comes to applied mathematics and connection to the industry?

I like the practice of including external examiners on PhD committees in Canada. I believe that it raises the standard of the doctoral program and ensures a high quality of result. I think that the NSERC funding programs have been remarkably effective in supporting a broad base of graduate research. However the much larger size of the United States educational enterprise does result in a huge variety of opportunities.

Both systems work — top graduates from both systems carry out excellent research programs and have great careers.

It is time to end this interview with your future projects. What do you have in hands for the next years?

The big thing right now is building BlueGene — a single computer with about as much power as the total of the world's 500 largest machines today. This includes hardware, software and finding ways to construct applications that can exploit this machine. We should be able to solve some pretty big optimization problems very quickly!

How and when would you like to end your career...?

I don't think of my career ending as much as changing focus. There are still many things that I have not had time to do yet — understand quantum mechanics, the proof of Fermat's Last Theorem, and how the human cell translates DNA into proteins. I'd also like to finish some of the novels that I have started. And, I've still got a long way to go before Eric Clapton will consider me a rival on blues guitar.

About the interviewee: W. R. Pulleyblank chaired the Department of Mathematical Sciences of the IBM T. J. Watson Research Center from 1994 to 2000. He is currently directing the Deep Computing Institute of this Center.

He held faculty positions in the University of Calgary (1974-1981) and in the University of Waterloo (1982-1991), before moving to IBM Corporation in 1991.

Bill Pulleyblank is one of the authors of the book Combinatorial Optimization, John Wiley and Sons, 1998, and the author of more than seventy research papers in this field. He has served on an extensive number of external and editorial boards.

Interviewer: L. N. Vicente (University of Coimbra, Portugal).

Bulletin

1. Workshop Announcements

V Brazilian Workshop on Continuous Optimization: In honor of the 60th birthday of Clóvis Caesar Gonzaga
March 22–25, 2004, UFSC – Florianópolis, Brazil
`http://jurere.mtm.ufsc.br/~workshop`

The V Brazilian Workshop on Continuous Optimization will take place at the Jurere Beach Village, in Florianpolis, Brazil, between March 22 and 25, 2004. Subjects to be discussed encompass theoretical, computational and implementation issues, in both linear and non-linear programming, including variational inequalities, complementarity problems, nonsmooth optimization, vector optimization, generalized equations, etc.. The workshop will consist of invited talks and sessions with contributed talks.

Large Scale Nonlinear and Semidefinite Programming: Workshop in memory of Jos Sturm
May 12–15, 2004, University of Waterloo, Canada
`http://orion.math.uwaterloo.ca/~hwolkowi/w04workshop.d/readme.html`

The breathtaking progress in algorithmic nonlinear optimization, but also in computer hardware has thrown new light on the solving large scale nonlinear programs. The aim of the workshop is therefore to bring together researchers from several communities, such as: algorithmic nonlinear optimization; combinatorial optimization, dealing with computational methods for NP-hard problems; computer scientists interested in scientific parallel computing, who share a common interest to do computations on (large-scale) hard combinatorial optimization problems.

Topics to be covered include: general nonlinear programming problems, semidefinite programming and reformulation schemes, quadratic and multi-dimensional assignment problems, boolean quadratic programming, massive graph problems, massively parallel distributed processing, VLSI design.

Invited plenary speakers: S. Boyd, N. I. M. Gould, D. Henrion, M. Kocvara, J. Lasserre, Y. Nesterov, J. Nocedal, P. Parrilo, J. Sturm (was scheduled to be a plenary speaker but passed away on Saturday, Dec. 6, 2003), R. Vanderbei, Y. Ye.

On the first day of the conference, on Wednesday May 12, 2004, there will be two short courses: (1) Theory and Applications of SDP (9:00-12:00) (including motivation as to how large scale SDPs arise both in theory and applications); (2) Algorithms for SDP (14:00-17:00).

IPCO X

June 9–11, 2004, University of Columbia,
New York City, USA
IPCO Summer School, June 7–8, 2004
`http://www.corc.ieor.columbia.edu/meetings/ipcox/ipcox.html`

This meeting, the tenth in the series of IPCO conferences, is a forum for researchers and practitioners working on various aspects of integer programming and combinatorial optimization. The aim is to present recent developments in theory, computation, and applications of integer programming and combinatorial optimization.

Topics include, but are not limited to: integer programming, polyhedral combinatorics, cutting planes, branch-and-cut, lift-and-project, semidefinite relaxations, geometry of numbers, computational complexity, network flows, matroids and submodular functions, 0,1 matrices, approximation algorithms, scheduling theory and algorithms.

In all these areas, the organizers welcome structural and algorithmic results, revealing computational studies, and novel applications of these techniques to practical problems. The algorithms studied may be sequential or parallel, deterministic or randomized.

The IPCO Summer School will take place June 7 and 8, 2004, and will present the following speakers: J. Feigenbaum, T. Roughgarden, R. Vohra. The summer school will focus on the interactions between operations research, computer science, and economics.

8th International Workshop on High Performance Optimization Techniques: Optimization and Polynomials (HPOPT 2004)

June 23–25, 2004, CWI, Amsterdam,
The Netherlands
`http://www.cwi.nl/~monique/hpopt20041`

This workshop focuses on the recent exciting developments on the interplay between optimization and polynomials, in particular, the field of real algebraic geometry dealing with representations of positive polynomials as sums of squares. The linking element relies on the following facts:

- tight approximations for optimization problems can be constructed via sums of squares of polynomials (and the dual theory of moments);
- sums of squares of polynomials can be modelled using semidefinite programming and thus can be computed efficiently with interior-point algorithms.

The theoretical justification for the convergence of the relaxed bounds is provided by representation results for positive polynomials. The aim of this workshop is to bring together researchers with interests in optimization and polynomial representations. The objective is to provide a forum for the exchange of ideas and knowledge about algorithmic developments and new theoretical achievements.

The workshop will begin with a one-day tutorial on the theme *Sums of Squares in Optimization*, whose aim is to present results about polynomial representations and optimization to non-specialists. The tutorial lectures will be delivered by three experts in the area.

International School of Mathematics “G. Stampacchia”, 40th Workshop, Large Scale Nonlinear Optimization

June 22 – July 1, 2004, Erice, Italy
`http://www.dis.uniroma1.it/~erice2004`

The workshop aims to review and discuss recent advances in the development of methods and algorithms for Nonlinear Optimization and its Applications, with a main interest in the large scale dimensional case, the current forefront of the research effort. It is the fourth in a series of Workshops on

Nonlinear Optimization: the preceeding ones have been held in 1995, 1998, and 2001.

Topics include, but are not limited to: constrained and unconstrained optimization, large scale optimization, global optimization, derivative-free methods, interior point techniques for nonlinear programming, linear and nonlinear complementarity problems, variational inequalities, nonsmooth optimization, neural networks and optimization, innovative applications of nonlinear optimization.

The workshop will consist of invited lectures and contributed lectures. Invited lecturers who have confirmed the participation are: D. P. Bertsekas, F. Bonnans, O. Burdakov, Y. Evtushenko, F. Facchinei, J. Gondzio, N. I. M. Gould, A. Griewank, L. Grippo, S. Leyffer, L. Luksan, J. Moré, J. Nocedal, J.-S. Pang, P. Pardalos, M. J. D. Powell, L. Qi, E. W. Sachs, S. Scholtes, J. P. Vial, H. Wolkowicz, M. H. Wright, J. Zhang.

Workshop on Linear Matrix Inequalities in Control

July 1–2, 2004, LAAS-CNRS, Toulouse, France
`http://www.laas.fr/~henrion/lmi04`

The workshop aims at reporting latest achievements in the area of linear matrix inequality (LMI) methods in systems control. Confirmed invited speakers are: V. R. Balakrishnan, B. Clément, J. C. Geromel, F. Leibfritz, E. Prempain, C. W. Scherer, L. Vandenberghe.

Optimization 2004

July 25–28, 2004, University of Lisbon, Portugal
`http://www.opti2004.fc.ul.pt`

Optimization 2004 is the fifth international conference on optimization to be held in Portugal since 1991. The meeting will be held at the Faculty of Science, University of Lisbon.

The invited plenary sessions are: W. J. Cook (*Solving Traveling Salesman Problems*), M. Dorigo (*The Ant Colony Optimization Metaheuristic*), C. A. Floudas (*Deterministic Global Optimization: Advances and Challenges*), P. E. Gill (*Recent Advances in Large-Scale Nonlinearly Constrained Optimization*), T. L. Magnanti (*All Roads Lead to Lisbon: Some Modeling Approaches for Designing Optimal Networks*), F. Rendl, (*Does Semidefinite Programming help to Approximate Combinatorial Optimization Problems?*).

A special stream on "Network Design" will also be organized. A collection of selected papers of this stream will be published in a special issue of Networks, to be edited by L. Gouveia and S. Voss.

4th Annual McMaster Optimization Conference: Theory and Applications (MOPTA 04)

July 28 – July 30, 2004, McMaster University, Canada
`http://www.cas.mcmaster.ca/~mopta`

The 4th annual McMaster Optimization Conference (MOPTA 04) will be held at the campus of McMaster University. It will be hosted by the Advanced Optimization Lab at the Department of Computing and Software and it is co-sponsored by the Fields Institute, MITACS, and IBM Canada.

The conference aims to bring together a diverse group of people from both discrete and continuous optimization, working on both theoretical and applied aspects. We aim to bring together researchers from both the theoretical and applied communities who do not usually get the chance to interact in the framework of a medium-scale event.

Invited speakers include: A. Ben Tal, L. Biegler, J. Lee, J.-S. Pang, C. Shoemaker, G. Vanderplaats.

Inaugural International Conference on Continuous Optimization (ICCOPT I)

August 2–4, 2004, Rensselaer Polytechnic Institute, Troy, New York, USA
`http://www.math.rpi.edu/iccopt`

The conference is sponsored in part by the Mathematical Programming Society. It is organized in cooperation with the Society for Industrial and Applied Mathematics (SIAM) and the SIAM Activity Group on Optimization.

The scientific program of ICCOPT will cover all major aspects of continuous optimization: theory, algorithms, applications, and related problems. A partial list of topics includes linear, nonlinear, and convex programming; equilibrium programming; semidefinite and conic programming; stochastic programming; complementarity and variational inequalities; nonsmooth and variational analysis; nonconvex and global optimization; optimization of partial differential systems; applications in engineering, economics, finance, statistics, game; theory, and bioinformatics; energy modeling and electric power market modeling; optimization over computing grids; modeling languages and web-based optimization systems.

The conference will consist of a mixture of plenary, semiplenary, invited, and contributed talks. It is anticipated that at most four sessions will be scheduled in parallel. Selected papers will appear in a special issue of Mathematical Programming Series B.

A dedicated session will be devoted to papers by young colleagues, to be chosen by a panel of reviewers. See the separate Call for Papers by Young Researchers for details, including guidelines and submission information.

The conference will be preceded by a summer school for graduate students, junior faculty, and other interested participants, which will describe some of the recent exciting developments in continuous optimization.

Eighth SIAM Conference on Optimization, 2005 SIAG-OPT VIII

Sunday to Wednesday, May 15-18,
Stockholm, Sweden

The conference is sponsored by the SIAM Activity Group on Optimization and the invited speakers include:

- D. Bienstock, Columbia University,
  Discrete Optimization and Network Design;
- M. C. Ferris, University of Wisconsin,
  Complementarity Problems and Applications;
- O. Ghattas, Carnegie Mellon University,
  Large Scale Earthquake Inversion Problems;
- J. Gondzio, The University of Edinburgh,
  Practical Aspects for Large Scale Interior-Point Methods;
- M. Kojima, Tokyo Institute of Technology,
  Parallel Computing for Semidefinite Programming and Polynomial Optimization;
- M. Laurent, CWI,
  Semidefinite Programming and Approximation Algorithms in Combinatorial Optimization;
- A. Shapiro, Georgia Institute of Technology,
  Stochastic Programming;
- A. Sofer, George Mason University,
  Optimization in Medicine.

More details (including information about short courses) will appear in upcoming issues of SIAG/OPT Views-and-News.

2. Special Issue

Call for papers Mathematical Programming Series B, Special Issue on "Large Scale Nonlinear and Semidefinite Programming"

`http://orion.math.uwaterloo.ca/~hwolkowi/w04workshop.d/mpbspecialissue.d/callforpapers.html`

We invite research articles for a forthcoming issue of Mathematical Programming, Series B, on "Large-Scale Nonlinear and Semidefinite Programming". This issue is in memory of and dedicated to Jos Sturm.

The issue is associated with the May/04 Workshop at the University of Waterloo on this topic. Interest in algorithmic nonlinear and semidefinite optimization has increased in recent years for many reasons, of which we mention three. First, the great success of interior-point methods (IPMs) in linear programming, at both a practical and theoretical level, and the development of highly appealing interior-point theory and methods for cone programming problems, has motivated researchers to seek practical interior-point methods for wide classes of large-scale nonlinear problems. Second, the door to practical solution of hard combinatorial optimization problems has been opened by nonlinear and semidefinite relaxations of these problems, along with astonishing advances in computational hardware. Third, many new applications of nonlinear and semidefinite programming have been identified in recent years, but solution of practical instances awaits the development of algorithms for large-scale problems.

The focus of this special issue is on large-scale nonlinear programming, including cone programming problems such as semidefinite and second-order cone programming. We invite papers that address the following topics, individually or in combination:

- algorithmic nonlinear optimization;
- computational methods and NP-hard problems involving large-scale nonlinear and/or semidefinite programming;
- scientific parallel computing for (large-scale) nonlinear and semidefinite programming and its application to hard combinatorial optimization problems.

Deadline for submission of full papers: Nov. 1, 2004. We aim at completing a first review of all papers by May 1, 2005.

Electronic submissions to the guest editors in the form of pdf files are encouraged. All submissions will be refereed according to the usual standards of Mathematical Programming. Information about Mathematical Programming, Series B, including author guidelines and other special issues in progress, is available at URL `http://www.cs.wisc.edu/~swright/mpb`. Additional information about the special issue can be obtained from the guest editors:
Erling Andersen (**e.d.andersenmosek.com**),
Etienne de Klerk (**edeklerkuwaterloo.ca**),
Levent Tuncel (**ltuncelmath.uwaterloo.ca**),
Henry Wolkowicz (**hwolkowiczuwaterloo.ca**),
Shuzhong Zhang (**zhangse.cuhk.edu.hk**).

Chairman's Column

It is time to welcome the new committee members for our SIAG-Opt.

First, I would like to thank Luís for stepping in to replace Jos as editor of our newsletter. As mentioned in Luís' comments below, Jos did an exceptional job. We will all miss his pleasant nature and his contributions to our field.

Kurt Anstreicher is our *new leader*, Chair. He will be assisted by: Bob Vanderbei as Vice Chair, Sven Leyffer as Program Director, and Kees Roos as Secretary/Treasurer. We (the previous committee) are leaving the activity group in exciting and capable hands.

As part of the current committee, I am fortunate to be involved in the organization of the upcoming SIAM Conference on Optimization to be held May 15-18, in Stockholm, Sweden. I hope that you will all participate. We already have chosen eight outstanding plenary speakers for our Eighth Conference. Consider this a request for both contributed papers as well as for minisymposium organizers. The conference will be extended to a fourth day if we get enough preregistered participants. So, once the preregistration web site is up, please tell your friends AND preregister.

It seems that my term as Chair has just started rather than already ended. I have particularly enjoyed working with the other members of my committee. Thank you Philippe, Anders, Natalia and, thank you Jos.

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Comments from the Editor

As the new editor of the SIAG/Optimization Views-and-News, I would first like to thank my predecessors, Larry Nazareth, Juan C. Meza, and Jos F. Sturm, for their excellent work. They have given our newsletter a reputation of the highest quality.

It is a great honor for me to continue the work started by Jos. I feel that my responsibility is increased since this was a job that he was unfortunately unable to continue. I first met Jos at a workshop at the IMA in Minneapolis in the Winter of 2003. We did not have much time to get to know each other, but I appreciated talking to him and I enjoyed the too brief moments we spent together. I will do my best to maintain the high standards that he has established for this newsletter.

This issue contains two expository articles by J. C. Nash (on a statistical tool called R) and by A. Y. Alfakih and H. Wolkowicz (on Euclidean distance matrices) which were in the queue when I assumed my editorship. In addition, the issue features interviews with three of our greatest optimizers: Terry Rockafellar, Mike Powell, and Bill Pulleyblank. I hope you enjoy reading their opinions and thoughts as much as I did.

I take this opportunity to ask the community for contributions to the newsletter, e.g.: expository articles on interesting topics (ranging from applications or case studies to theory or software), announcements of events, book and software releases, etc..

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