Foundations of Computational Mathematics

The story of "Foundations of Computational Mathematics" (FoCM) starts with the 1995 AMS-SIAM Summer Seminar in Applied Mathematics at Park City, Utah, organised by Steve Smale, which has brought together a wide range of mathematicians working in real-number calculations. In one of the last days of the month-long event, at Narendra Karmarkar's initiative, a small group of us have assembled for a typical lunchtime event of indifferent food and inspiring discussion, with just one item on the agenda. The Summer Seminar was a truly unique event, an inspiring meeting of minds of numerical analysts, pure mathematicians and computer scientists. We have discovered that there are many themes that excite us all and that we can really benefit from joint discussion. A foundation stone for *something* has been laid. But what should this "something" be and what is the way forward?

We have decided there and then to establish *Foundations of Computational Mathematics*, at the first instance as a vehicle to organise further meetings. Since this memorable lunch, FoCM has organised the FoCM'97 conference in Rio de Janeiro in January 1997 and a special semester at Mathematical Sciences Research Institute (Berkeley, California) in Autumn 1998. Preparations are at the earnest for a major conference in Oxford this summer and a special semester in Hong Kong in Autumn 1999, we are planning monograph series and a journal. . . . So, what is FoCM and what is its "mission statement"?

It is probably easier to answer the question what FoCM is *not*. Firstly, it is not yet another "organisation", with an office, members and membership fees. Its affairs are run by an Executive Committee, assisted by a wider Council, both assembled on almost an *ad hoc* basis and with the occasional reliance on a helping hand from more established mathematical organisations (AMS, IMA, IMU, SIAM, . . .). Indeed, with the preponderance of many active and effective mathematical organisations, we are reluctant to establish additional formal structures. Secondly, FoCM is definitely not an attempt to sweep away all that has been done in numerical analysis and replace it with a more "mathematical" construct. We neither bear a new, revolutionary truth nor carry light unto the heathen: foundational work in mathematical computation has been done, often brilliantly, since the dawn of numerical analysis. Having said this, we believe that there is a need for a forum that brings together diverse professionals with an interest in mathematical computation and that both parts of this compound phrase, "mathematical" and "computation", are bound to benefit.

Seeking answers to what FoCM is (or, at any rate, should be) and to set the stage for the forthcoming FoCM'99 Oxford Conference we have approached a number of colleagues and asked them to describe their personal perspective on this issue. Their opinions are diverse and often tentative, yet they display a common denominator and, arguably, provoke a broader discussion on the future of numerical analysis and scientific computing as the new millennium knocks at the door.

A Iserles

(1) Is there a need to explore foundations of computational mathematics?

Wolfgang DAHMEN:

I would give an affirmative answer to this question from the following perspectives:

- The importance of mathematical concepts for essentially all areas of science like physics, chemistry, biology, economics, technology and engineering is growing (not always very visible though) at an extremely rapid pace. The increasing computing power even accelerates this process because of an emerging feasibility of even more ambitious goals. In order to avoid getting completely lost in myriads of concrete and detailed questions and problems arising in such contexts and not having to be content with *ad hoc* answers, a systematic exploration and foundation of mathematical concepts for computation are needed for coordination.

  It will be as important as it is difficult to find the right balance between the two extremes namely, on one hand, dealing with each concrete application separately and, on the other hand, developing completely "detached theories" that might end up being completely irrelevant. Developing such theories must be a proper reflection of true challenges from "real life problems". To sort that out in a proper way will be a dynamic process which also requires a suitable forum where ideas and concepts from *different* disciplines can be exchanged and give rise to synergetic effects.
- The above comments stress the need for a systematic long term exploitation of mathematical resources for *applications*. I think the other way around matters as well. Exploring the foundations of computational mathematics in the above sense will import new types of problems, ideas and view points into the mathematical community and may trigger stimulating developments.

Arieh ISERLES:

In the last fifty years numerical analysts have created an impressive body of work which, when used properly, forms *the* vital basis to the exploration of science and engineering by computer. Yet, because of the sheer scale of the enterprise, we have often became *self referential*, addressing ourselves to issues neither because of their potential in applications nor for their intrinsic mathematical value but simply since everybody else deals with them. The discussion of foundations is thus, to my mind, an opportunity to break out of self-imposed walls. It is important to stress that the idea is not to make numerical mathematics more "mathematical" and less "applied": it is to make it *both* more mathematical and more applied! The idea that somehow good mathematics and good applications contradict each other, that pure maths is sterile or that applied maths is bad maths, is sheer nonsense. Losing sight of applications, striving for computer-free numerical analysis, is futile. But so is any attempt to forget the Trefethen dictum that any mathematical problem, upon discretization, becomes a more challenging mathematical problem.

"Foundations of Computational Mathematics" means deliberately going back to the drawing board, to dare asking the "big" questions and provoke a debate.

Marie-Françoise ROY:
To study the foundations of computational mathematics is clearly important. Computational mathematics in its various aspects: numerical analysis, symbolic computation, theorem-proving, are already related to many branches of classical mathematics (analysis, algebra, algebraic geometry, logic). Computational aspects of other branches of mathematics, e.g. topology, will develop in the future.

Mike SHUB:
The computer is changing everything, even the world economic structure and including the relations between numerical analysis, applied mathematics and core mathematics. There is also what we have already begun to call computational mathematics, distinguishing it from the others. Not only are all the relations between the traditional areas changed, but whole new areas for mathematical analysis have arisen. To mention just a few new areas, in no particular order, which are related to the computer industry and the societal uses of computers we have pattern recognition, data mining, voice and handwriting recognition, image recognition and transmission, secure communication, electronic business transactions and the protection of intellectual property rights in an electronic world.

Undoubtedly there is a need, even a responsibility for the mathematical sciences community to study foundational problems in computational mathematics. The only sure thing is that the most important results and applications of the exercise are unlikely to match any specific goals which we set for ourselves as we are starting out.

Steve SMALE:
It is important to give the subject of numerical analysis a greater coherence through a focus on the mathematical side. In particular, to aim to strengthen the unity of mathematics and numerical analysis, and to narrow the gap between pure and applied mathematics. That goal is appropriate since many of the heroes of pure and applied mathematics, Newton, Euler, Lagrange, and Gauss among them, established the basic real number algorithms. With the revolution of the computer and the great achievements of scientific computation, it does service to both the pure and applied communities to support the mathematical development of numerical analysis.

Endre SÜLI:
Computational mathematics is an amorphous subject whose contours are difficult to define. It evolves in permanent contact with pure mathematics and computer science, and its development is stimulated by the needs of applied sciences and engineering. This constant interaction with other branches of mathematics and its applications is the hallmark and the driving force of the subject. Given the increasing diversity of computational mathematics, there has never been a more urgent need to explore the foundations of the field.

Henryk WOŹNIAKOWSKI:
I believe, the answer is strong yes. Foundations of computational mathematics are related to models of computations and computational complexity. Let me elaborate on these points.

Computational mathematics has its roots in ancient history. Probably Heron's algorithm for computing the square-root of a number is one of the first examples of a computational algorithm. (Today we know that this is a special case of the powerful Newton algorithm for solving nonlinear equations.) The need for computational algorithms grew proportionally with the development of applied mathematics. The presence of computer in the twentieth century marked the new level of scientific computing. Thousands of computational algorithms have been proposed for solving numerous problems of applied mathematics. In most cases, we know many algorithms for solving the same problem. The natural questions were posed already in 1950's (if not earlier): which algorithm is better, which algorithm is the best and what is the intrinsic difficulty of solving the problem. This led to the new field of computational complexity. This field may be viewed as the important extension of the classical mathematical approach where the study of existence and properties of the solution is enlarged by asking what is the complexity of computing the solution.

Computational complexity requires the precise definition of a model of computation. And we need to study more than one model of computation depending on the underlying computational problem and computational resources. For examples, discrete and continuous problems need different models. Sequential, parallel, distributed and (maybe in future) quantum computers correspond to different models. Furthermore, we may be interested in various notions of cost and error of an algorithm. This leads to the computational complexity in the worst, average case, randomised, probabilistic and others settings.

This short discussion shows the need of studying many of such computational complexity questions. In short this can be summarised as the strong need to explore foundations of computational mathematics.

(2) How do you see the future of research at the interface of numerical computation, pure mathematics and theoretical computer science?

Wolfgang DAHMEN:
Several newly founded centres for scientific computation in Europe and in the US indicate a strong trend for interdisciplinary research activities with mathematics in a pivotal position (quite in the spirit of the above comments). While at a first glance the importance of engineering sciences and computer sciences is evident, a closer look reveals which tremendous opportunities are opening up for mathematics. I am *not* referring to all the familiar mathematical techniques that are *used* by engineers and computer scientists but rather to developments that

require a *direct* interaction of mathematicians in such an interdisciplinary context. One among many examples is the role of numerical simulation. While many important concepts such as the finite element method could be developed to a high degree of practical functionality by practitioners like engineers, we are entering a phase where guidance by physical principles combined with basic mathematical skills do not seem to suffice any more. To tackle many important problems at the frontier of the respective area of science often also leads to (or beyond) the frontiers of current computing capabilities, see eg the so called *Grand Challenges*. Solving these computational tasks is not possible by simply increasing computing power nor by employing (problem independent) pure computer science concepts. An essential role is played in this context by the development of mathematical algorithms offering a much more significant reduction of complexity than eg parallelism, exploiting to the highest possible extent the analytical structure of the underlying mathematical model. As a result, all of a sudden concepts from pure mathematics, such as the theory of function spaces, are drawn in, ultimately allowing one to make a computational task tractable (in a practical sense). I am sure that an increasingly closer intertwinement of numerical and analytical concepts is only one example for an extremely prosperous future of the above interface.

Arieh ISERLES:

These are exciting time at the interface of mathematics, computer science and applications. On the one hand we see the evolution of industrial mathematics and of "computational science" — a new approach to modelling that places computing firmly at the centre. On the other hand, it seems as if pure mathematics has discovered the potential and excitement of computing, including the non-numerical variety: computational number theory and group theory and topology, to say nothing of symbolic computation. Some of us might view this encroachment into "our" territory as a danger, I prefer to see it as the greatest opportunity of numerical mathematics since the introduction of the computer.

Teresa KRICK:

It is always difficult to explain to outsiders what we the *mathematicians* do or try to do. One of the best examples I know, that people not only understand but also appreciate for its great importance, is at the cornerstone of these three "areas": cryptology. I think that this single subject shows perfectly how fundamental it is to mix boldly numerical computation, theoretical computer science and pure maths.

Marie-Françoise ROY:

The opposition between abstract and concrete mathematics is irrelevant. Lower bound results require abstraction. Sophisticated mathematics are a source for new methods and efficient algorithms.

Computer science not only provides mathematics with computers. Algorithms are clearly important. Intelligently created data structure are also important. In the future, new representations of mathematical objects will play an important role in computational mathematics.

The nature of mathematical truth will change in part. "Hybrid mathematics" will develop, I am thinking of results conjectured by women (or men) but only proved by computers as the computation leading to the proof is too long and too intricate to be verified by human beings.

Mathematicians will always fight for simplicity and insight, but who says that true facts always have simple proofs?

Mike SHUB:

There will unquestionably be a deepening of the relationship between core mathematics and numerical and symbolic computation. More computational power leads to more interest in and applicability of computational algorithms for the solution of problems, which in turn lead to a greater need for invention, analysis and comparison of algorithms. It is these issues that FoCM has mostly focused on in the past.

There is a less transparent effect that the computer is having on mathematics itself, as an experimental and even theorem-proving tool. In dynamical systems theory, with which I am most familiar, there is not yet a good understanding of how to evaluate the outcome of an experiment or methodologies for what constitutes a good experiment. We begin to confront some of these issues in our workshops at the Oxford FoCM meeting.

Steve SMALE:

Finding a natural meeting ground between the highly developed complexity theory of computer science — with its historical roots in logic and the discrete mathematics of the integers — and the traditional domain of real computation, the more eclectic and less foundational field of numerical analysis, with its rich history and longstanding traditions in the continuous mathematics of analysis, presents a compelling challenge.

Endre SÜLI:

My own research is in the area of numerical computations and concerns the solution of partial differential equations. The history of this subject and its interactions with pure mathematics and computer science reveal a two-way flow of ideas, a phenomenon that is typical of the whole of computational mathematics. It is certain that this trend of strong reciprocal influence and cross-linking between mathematical disciplines will continue. Let me give two examples: domain decomposition methods for elliptic partial differential equations have their origin in a piece of pure mathematics due to Schwarz at the end of the nineteenth century, but the recognition of the practical significance of this idea and its development into a full-fledged numerical algorithm have occurred only during the last decade, stimulated by the advancement and proliferation of parallel computers. Conversely, the finite element method, one of the most general and powerful techniques for the solution of partial differential equations, has its historical origins in civil engineering applications at the middle of this century, while its mathematical theory, based on deep results from the theory of partial differential equations, functional analysis and function space theory, began only relatively recently, in the late nineteen-sixties.

I think that these two examples typify the general trend of interaction and cohesion of disciplines.

Mike TODD:

Computational mathematics in optimization embraces a wide variety of approaches, from the purely empirical to the theoretical. I don't believe FoCM has been *necessary* to inject a dose of rigour into the analysis of numerical methods in this field, but it has provided a highly valuable umbrella organisation to encourage the mathematical analysis of important practical algorithms and also to facilitate the investigation of less immediately applicable topics. For example, the study of interior-point methods has been the hottest topic in optimization for the last ten years, initiated by Karmarkar's method in 1984. People such as N Karmarkar, J Renegar, A Nemirovskii, Yu Nesterov, and O Güler have been involved with FoCM since its initiation. Karmarkar's method used novel ideas like projective transformations, steepest descent for a nonlinear logarithmic potential function, and ellipsoidal approximations of a polytope to study linear programming, which had been viewed almost entirely from a combinatorial geometry perspective. Renegar's related path-following method made connections with Newton's method and Kantorovich theory. Nesterov and Nemirovskii studied the foundations of polynomial interior-point methods, and hence opened them up to a wide variety of convex nonlinear problems, including most significantly semidefinite programming. And Güler found connections between Nesterov and Nemirovskii's universal barrier function and classical areas of mathematics, including symmetric cones, Siegel domains, and hyperbolic polynomials. Another very important aspect of FoCM, at least to me, is that it brings together leading researchers into the mathematical analysis of numerical methods in a variety of areas, including linear algebra, approximation, differential equations, etc. I find this very stimulating. The recent semester at MSRI was very successful in this regard, since people were around for four weeks and more. My student found the key to a considerable simplification in his thesis work (in the foundations of interior-point methods) by learning about work in differential equations by Nørsett and Iserles.

I answered (2) to some extent above. More connections with pure mathematics are one clear trend. One interesting connection with theoretical computer science is the use of semidefinite programming, a distinctly continuous part of optimization (in contrast to linear programming, which has a combinatorial flavour), to provide excellent bounds, and sometimes excellent solutions, to hard combinatorial problems. This connection between the discrete and the continuous is of course old — eigenvalues of graphs have been studied for some time, and Lovasz used semidefinite programming in coding theory twenty years ago. But the recent ability to solve reasonably large problems has led to a great flurry of activity.

Henryk WOŹNIAKOWSKI:

First of all, I believe that all divisions of mathematical research are a little artificial and, in particular, depend on time. Nevertheless, I think that numerical and scientific computing will be mostly mutually dependent on applied (not so much pure) mathematics. I hope that the interplay between which problems are needed to compute and theoretical results what can be computed will be a healthy sign of future progress.

Theoretical computer science seems today (and probably in the future) to be mostly concerned with computational problems of discrete mathematics. I would be delighted to see more use of continuous mathematics in theoretical computer science. Probably it may happen only in the study of problems which have both discrete and continuous components. A good example is optimization, especially linear programming. On the other hand, sometimes it is hard to be sure that the problem has only one component. For example, multivariate integration looks like a problem with only the continuous component. However, multivariate integration for some classes of functions is very much related to discrepancy which is a popular subject in computational number theory with a definite discrete component.

(3) What are the emerging themes, numerical and non-numerical, in mathematical computation?

Wolfgang DAHMEN:

I am only able to judge a small portion of the full scope of relevant themes. So the following comments (primarily influenced by my own interests) address only some among probably many more examples. The first example that comes to mind is handling *multiscale phenomena*. This is encountered already when dealing with simple partial differential equations of model type, in large scale optimization, in cascade models in theoretical physics and in prominent areas such as turbulence modelling or long time integration of dynamical systems. It draws upon discrete and analytical concepts. The second example concerns *geometry processing*, ranging from automatic geometry modelling and recognition and intelligent mesh generation concepts to computing on manifolds. An area with a particularly strong interdisciplinary potential is *scientific visualisation*, which will become more and more important for evaluating complex results. Of course, these areas are by no means disjoint. Techniques from the first one may well penetrate into areas like visualisation or pattern recognition or mesh generation, etc. *Complexity theory* is certainly at the heart of the matter. Being able to estimate the computational cost of all these tasks will be the more important, the more constructive the analysis is and the more the underlying models correspond to relevant problems in the spirit of (1) above.

Nick HIGHAM:

The twin aims of most research in numerical linear algebra are to solve large, possibly sparse problems efficiently, and to obtain high (or at least, predictable) accuracy in the solution. Some of the most novel work is motivated by parallel computation, but a number of algorithms developed in this context have proved to be of general interest.

Developing parallel methods for the nonsymmetric eigenproblem remains a major challenge: as yet, no parallel method has been found that is as reliable as the standard QR algorithm, which is difficult to parallelise.

Applications such as signal processing lead to recursive least squares problems in which new data progressively replaces old. The consequent need to efficiently and accurately update and downdate matrix factorizations is a continuing topic of research in which major advances have been made in the last five years.

Iterative methods for large, sparse eigenproblems are the subject of much current research and high-quality software is starting to appear, thanks to better theoretical understanding of Arnoldi and Lanczos methods and their variants.

In the development of numerical methods for the eigenproblem new perturbation theory and error analysis is often required, and much current research is devoted to extending and unifying efforts in this area over the last decade.

Iterative methods for linear systems have enjoyed a resurgence of interest in recent years, motivated by applications. A number of new Krylov subspace methods have been developed (GMRES, CGS, Bi-CGSTAB, QMR) and good preconditioners have been found for many applications. However, there is still much to be understood about preconditioning iterative methods for nonsymmetric linear systems.

Complexity issues are always present, particularly in connection with parallel methods, and it is an unfortunate fact that the asymptotically fastest methods are often not the most stable. The challenge is then to bound the degree of instability and to find cheap ways of detecting instability in practice.

Another area where complexity issues play an important role is direct factorization methods for sparse matrices. In the last ten years, elimination tree-based methods have resulted in much greater storage efficiency and have led to parallel factorization algorithms. Multifrontal methods have produced much more efficient sparse least-squares software. Important remaining challenges include the use of direct methods to provide more effective preconditioners for iterative methods, the development of better methods for symmetric indefinite factorizations, and the development of better ways to take advantage of distributed architectures.

Arieh ISERLES:

Sticking my neck out, I believe that emerging themes in computation will be consistent with the interaction of pure and applied. The goal will increasingly be neither to solve numerically nor by some other means, but to *understand* mathematical structures by harnessing a wide range of mathematical techniques with knowledge originating in application areas. An important example is presented by multiresolution and multiscale techniques: numerous natural phenomena (and, truth to be told, many numerical algorithms) exhibit a mixture of widely-differing (and often spatially distributed) rates of change. Only lately, aided by techniques from harmonic analysis (wavelets!), we have started to understand how to take advantage of this behaviour to understand and to improve numerical algorithms. Another example is geometric integration: the recovery of invariants, integrals and symmetries of differential equations under discretization. This attempt, to maintain correct qualitative behaviour of a numerical approximant, requires techniques from differential topology and the theory of Lie groups. In general, geometry is bound to play an increasingly central role in computation, thus computer-aided geometric design, multivariate approximation and visualisation. Another theme which, in my opinion, will become of an increasing importance is adaptivity, although the search for its proper mathematical framework is still on.

Among emerging non-numerical themes, I believe that numerical analysts will be ignoring symbolic algebra and analysis at their peril. The new world of ideas that has been opened up by symbolic techniques is both important and immensely valuable. We should be exploring common ground and attempt to employ numerical and symbolic techniques in a synergistic manner.

Finally, the day might be near when real-number complexity theory is forged into a powerful and (yes!) practical tool, telling us what is the potential and what are the limits of numerical computation. Discrete complexity theory has a proven track record in computer science. The real-number variety is considerably more challenging. It calls for a great deal of new, hard mathematics but recent developments seem to imply that this intellectual investment is just about to start paying dividends.

Teresa KRICK:

Perhaps one theme of great importance in the field (although not really "emerging") is the classification of the complexity of different problems (saying nothing of the difficult task of exploring lower bounds). This is strongly related to the conjecture $P \neq NP$ and even to the subject of cryptography that I have mentioned for the previous question.

Another emerging topic seems to be the systematic consideration of mixed techniques (numerical plus symbolic) in order to combine their advantages when solving problems arising in the real world.

Marie-Françoise ROY:

Here are a few ideas, with illustrative examples pointing to simple problems in algorithms of real algebraic geometry, that I consider as important for future progress.

1. Take advantage of sparsity in algorithms. It is known (since Descartes) that a polynomial with few monomials has few real roots but existing algorithms counting real roots are unable to exploit this fact.
2. Take advantage of special structure in algorithms. How can we quickly evaluate the sign of a number given by a straight line program, count real roots (or decide the existence of a real root) of a polynomial given by a straight line program?
3. Integrate symbolic/numerical methods. Use numerical methods, say structured matrices, in order to design algorithms counting the number of real roots quickly in easy cases and reaching worst case complexity only in bad cases.
4. Extend the portions of algebra that are algorithmic. For example, in real algebra, the algorithmic manipulation of sums of squares, cones of positivity needs to be investigated much more.
5. Encourage the study of practical complexity. In algorithms manipulating systems of algebraic equalities and inequalities, the tricks used to improve the theoretical complexity create algorithms whose implementation is very inefficient. Only experiments and the development of rigourous comparisons between variants of the algorithms can improve the efficiency of software.

Mike SHUB:

Along with the increase of the size and numerical intensity of problems that are being solved, more and more sophisticated techniques have been developed from numerical linear algebra to optimization theory and the discretization of the solution of differential equations, to mention just three which are thrilling. Part of the successes here are achieved with what I might call geometrical analysis, which is very much to my taste. Personally, I like complexity theory, the $P = NP$ problem as a particularly grand example of the theory and some special but still intriguing themes as finding good starting points for Newton's method.

Endre SÜLI:

Concerning emerging themes, the development and the mathematical analysis of robust adaptive algorithms driven by sharp a posteriori error bounds, and the study of domain/data partitioning techniques can be identified as major topics in the field of computational partial differential equations. In tandem with these, one observes the advancement of fast iterative methods and the development of efficient preconditioning techniques. I believe that these research themes will stay with us in the foreseeable future.

Mike TODD:

Two trends I could mention are fundamental problems that have applications across a diverse range (for example semidefinite programming, with applications to combinatorial optimization, to optimal control, and to structural optimization; also the efficient solution of so-called KKT or equilibrium systems of linear equations in linear algebra, which come up in economics, in optimization, and in electrical circuits), and new paradigms in problems arising from our increased computational abilities (one I especially like is the idea of robust optimization, where the data lies in a set and a solution is sought which is feasible and near-optimal whatever data point is chosen; also Renegar's condition number studies for problems beyond just linear systems).

Henryk WOŹNIAKOWSKI:

I believe that for both numerical (continuous) and non-numerical (discrete) mathematical computations the already existing famous conjecture $P \neq NP$ over different rings will still play a significant role. Steve Smale is right that this conjecture is one of the very few most important problems in mathematics (not only computational mathematics) for the next century.

As far as emerging themes in continuous computational mathematics are concerned, I restrict myself to one theme which is close to my work. This is the theme of solving multivariate problems defined on functions of d scalar variables with large d. There are plenty of applications of such problems. In particular, recent study of finance mathematics supplies such problems with huge d. Path integration which occurs in many fields corresponds formally to $d = \infty$. There was a common belief that such problems suffer the curse of dimensionality, that is, that computational complexity depends exponentially on d. This is indeed the case for many classical classes of functions. I believe that the curse of dimensionality may be broken even in the worst-case setting for some "weighted" classes of functions. It is a challenging problem to find such classes and to show that the solutions of applied multivariate problems belong to these classes.

(*Compiled by* **T Krick**)

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Arieh ISERLES is a Reader in Numerical Analysis of Differential Equations at University of Cambridge, chair of the Executive Committee of FoCM, Managing Editor of *Acta Numerica*, editor of several other journals and of Cambridge Monographs in Computational and Applied Mathematics. His current mathematical interest is mainly in geometric integration, ie discretization methods that retain known geometric features of differential equations.

Teresa KRICK is Assistant Professor at the University of Buenos Aires. She works in Computational Algebra, focusing on problems coming from Algebraic Geometry and Diophantine Approximation, mainly on the computational aspects of solving systems of equations. She is a member of FoCM Executive Committee.

Marie-Françoise ROY is Professor of Mathematics at Université de Rennes (France). She is a specialist in real algebraic geometry and is currently working on algorithms for real algebraic geometry. She was the first president of the association "femmes et mathématiques", a president of the CNRS commission for mathematics, chaired the MEGA 98 conference in Saint-Malo and is a member of FoCM Executive Committee.

Michael SHUB is a Research Staff Member at IBM Research, where he has been since 1985. His research is focused on two areas. The chronologically older of the two is chaos theory. Here he is currently interested in understanding the degree of generality of statistical robustness of dynamical systems. The other area is the computational complexity of real number algorithms. Particular interests here are the P versus NP problem of theoretical computer science in this new context and Newton type methods for solving systems of equations. He also enjoys assisting his wife, Beate Echols, as she travels around the world in search of the work of folk and other untrained artists. Mike was the first chair of FoCM Executive Committee.

After a long career at University of California at Berkeley, Steve SMALE is presently Distinguished Professor of Mathematics at City University of Hong Kong. His current interests are complexity and computation. Recipient of the 1966 Fields Medal, 1965 Veblen Prize, 1988 Chauvenet Prize and 1989 SIAM von Neumann Award, he was a driving force behind the formation of FoCM and is a member of its Executive Committee.

Endre SÜLI is Reader in Numerical Analysis at the University of Oxford. He is editor of the Oxford University Press Monograph Series in Numerical Mathematics and Scientific Computation, the Springer-Verlag Undergraduate Mathematics Series, the International Journal of Numerical Methods for Partial Differential Equations and is an associate editor of the IMA Journal of Numerical Analysis and chairs the Local Organising Committee of the Oxford FoCM Conference. His research is at the interface of Numerical Analysis and PDE theory and concerns the construction and analysis of finite element methods for partial differential equations.

Mike TODD is Leon C Welch Professor of Engineering at the School of Operational Research and Industrial Engineering, Cornell University. An authority in optimization algorithms, he currently specialises in interior-point methods and is the recipient of the 1988 George B Dantzig Prize.

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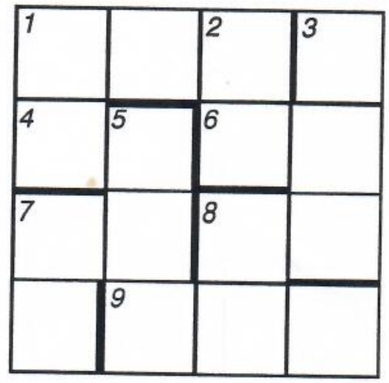
PUZZLE

Enigmaths 61 — Some sums 4 or Composition
by ZAG

As regular solvers will know, "Some sums" puzzles place a restriction on the sum of the digits of all the answers. In this case the restriction is that every digit sum is composite. The conventions for the puzzle are that 4 is the lowest composite number, multiples of a number exclude the number itself and no answer starts with a zero. This puzzle is quite demanding and you must make use of every hint presented by the clues to achieve the par solving time of 45 minutes. The solution is given on the inside back cover.

Across
1. see 5dn
4. a square
6. see 3dn
7. less than 6ac
8. a prime
9. reverse of 1ac

Down
1. a prime
2. see 7dn
3. a multiple of 6ac
5. a multiple of 1ac
7. a multiple of 2dn
8. a square — 8ac



Solution — Enigmaths 59 — 98/99 Christmas Prize Puzzle

A more challenging puzzle than usual this year with the theme being the fact that 98 is $2*7^2$ and 99 is $11*3^2$; both of the form pq^2 where p and q are different primes. Are there an infinite number of such non trivial consecutive pairs I wonder? Perhaps if anyone knows or could shed some light on the subject please write in. In the meantime the winner of the book token prize is Claire Gough. Many congratulations to all who took part and I hope the puzzle added to the entertainment of the festive season.