Grand Challenges

Grand Challenges for Computing Research

TONY HOARE[1] AND ROBIN MILNER[2]

[1]*Microsoft Research Ltd, 7 JJ Thomson Avenue, Cambridge CB3 0FB, UK*
[2]*Cambridge University Computing Laboratory, William Gates Building, JJ Thomson Avenue, Cambridge CB3 0FD, UK*
Email: thoare@microsoft.com, Robin.Milner@cl.cam.ac.uk

What are the major research challenges that face the world of computing today? Are there any of them that match the grandeur of well-known challenges in other branches of science? This article is a report on an exercise by the Computing Research Community in the UK to answer these questions, and includes a summary of the outcomes of a BCS-sponsored conference held in Newcastle-upon-Tyne from 29 to 31 March this year.

1. INTRODUCTION

1.1. The nature of a grand challenge

The tradition of grand challenges is common in many branches of science. If you want to know whether a challenge qualifies for the title 'grand', compare it with:

- prove Fermat's last theorem (accomplished)
- put a man on the moon within 10 years (accomplished)
- cure cancer within 10 years (failed in the 1970s)
- map the human genome (accomplished, 2004)
- map the human proteome (too difficult for now)
- find the Higgs boson (under investigation)
- find gravity waves (under investigation)
- unify the four forces of physics (under investigation)
- Hilbert's program for mathematical foundations (abandoned in the 1930s).

In the relatively short history of computer science, the following examples may be familiar:

- determine whether P is equal to NP (open)
- the Turing test (outstanding)
- the verifying compiler (outstanding)
- a championship chess program (completed)
- a GO program at professional standard (outstanding)
- literary translation from English to Russian (failed in the 1960s).

From these examples, particularly the human genome project, we can infer the characteristic properties that distinguish a grand challenge project in science and engineering from the many other more normal kinds of challenges that face researchers every day. First, a grand challenge can be a major scientific and engineering project that is undertaken by large international efforts spread over 10 or 15 years. Grand challenges are correspondingly rare: most scientific discoveries (and nearly all breakthroughs) are made by research of individuals and small teams pursuing brilliant ideas as soon as they are conceived—and many good scientists prefer to maintain their freedom to work in this way. But some of the more mature branches of science (particularly nuclear physics and astronomy) cannot be pursued on such a small scale. They require planning over decades and collaboration from laboratories throughout the world.

Second, a grand challenge project requires the support of the general scientific community as well as dedicated commitment from those who engage in it. Its eventual success is recognized as a major milestone in the advancement of scientific knowledge or of engineering technology or of both. It is celebrated throughout the world, not only by the scientific teams who have been engaged in the project but also by the general scientific community and (just as important) by students who are inspired to enter the research area, and even by the general public.

Finally, a grand challenge project requires understanding from the national funding agencies whose participation enhances the international reputation of those who engage in it. Furthermore, international collaboration is highly cost-effective, as the cost is shared among many nations and the benefits are distributed equally among them all.

Because of its long timescales, a grand challenge project can concentrate on advancement of basic understanding and technology and aim at revolutionary rather than evolutionary advance. It can pursue absolute ideals like accuracy of measurement in physics or purity of materials in chemistry—far beyond the current needs of the marketplace and even beyond the comprehension of currently practising engineers. At the start, there may be no known way of transferring the results into industrial practice, even in cases where the general area of research is one of great commercial or social significance. Thus, research conducted under a grand challenge is entirely complementary to shorter term and more

competitive research directed towards the development of commercial products.

2. THE GRAND CHALLENGE EXERCISE OF THE UKCRC

The UK Computing Research Committee (UKCRC) was constituted in 1999 with a mix of academic and industrial members. Its goal is to contribute to the good health and international high standing of UK research in computer science. It is recognized as a joint expert panel by professional bodies (the BCS and the IEE) and also by the CPHC, representing professors and heads of academic computing departments. In pursuance of its goal, the UKCRC embarked on an exercise to determine the long-term aspirations of the computer science community in the UK.

A Programme Committee was appointed to organize and conduct the exercise, beginning with a Grand Challenges Workshop. An open call was issued for ideas conceived on a scale appropriate to the criteria outlined in the previous section. The workshop was held in Edinburgh in November 2002 and discussed 109 submissions from the UK computing research community. It identified a set of possible topics for grand challenges, and chose one or two champions for each of them, to carry its development forward. A drafting phase followed the workshop, leading to seven draft proposals. They were mounted on the website for public discussion via email, moderated by the champions. The discussion is still open. The titles of the seven themes are:

GC1 In Vivo–In Silico: the Virtual Worm, Weed and Bug
GC2 Science for Global Ubiquitous Computing
GC3 Memories for Life: Managing Information over a Human Lifetime
GC4 Scalable Ubiquitous Computing Systems
GC5 The Architecture of Brain and Mind
GC6 Dependable Systems Evolution
GC7 Journeys in Non-classical Computation

A principle of the grand challenge exercise is that no submission from the community is, or will ever be, rejected by the UKCRC: it has no desire to usurp the selective functions of the fund-granting agencies, nor does it seek to promote its own views on research directions. Indeed the seven topics arose—via panel discussions—from the original submissions to the workshop, and all these submissions (except those withdrawn by authors) are still accessible on the website. Further submissions may be made at any time.

A second principle is that in formulating the grand challenge proposals no questions are raised about sources of funding. In this way the independence of purely scientific judgement is maintained. A third principle is that the exercise does not attribute higher importance to research aimed at a grand challenge project; it gives equal weight to basic exploratory research and to research aimed at more immediate industrial goals. Its primary role is rather to

promote interchange and collaboration in suitable research directions, where they can be seen to contribute to related long-term aspirations.

The next coordinated step in the UKCRC exercise was a conference on 'Grand Challenges for Computing Research', held in Newcastle from 29 to 31 March 2004 [1]. Its stated aims were:

(i) to encourage UK researchers in computing to articulate their views about long-term prospects and progress in their academic discipline;

(ii) to discuss the possibility of speeding progress by broader collaboration both nationally and with the international community;

(iii) to facilitate the pursuit of more ambitious scientific and engineering goals;

(iv) to work towards the definition of a grand challenge project, where this is an appropriate means to achieve the goals.

Again an open call was issued for submissions, either related to the existing seven challenges or proposing new topics for challenges. Some 50 submissions were received (remarkably, almost all linked to an existing challenge) and these were again the subject of panel discussions at Newcastle. The conference attracted over 150 attendees.

3. THE GRAND CHALLENGE PROPOSALS

Because of the potentially lengthy timescales, any plans for a grand challenge project must start with a prediction of the state of the world in 15–20 years time. Judging by the fantastic changes to the computing scene over the last 20 years, simple realism requires the prediction to be rather fantastic too. By 2020,

- there will be hundred times more computers in the world than there are today;
- many of them will be hundred times more powerful than now;
- and have hundred times the storage capacity;
- they will be everywhere, in toys, in books, in clothes, in furniture, in pills, . . .;
- they will communicate with all their neighbours, mostly without connecting wires;
- the connections will be hundred times faster than now;
- they will all be controlled by software;
- the software will mostly still be written by people.

The last line distils the essence of the challenges that face computer science today.

To understand the opportunities—and the dangers—of these developments, it is best to regard all the computers in the world as just one big computer, the global ubiquitous computer (GUC). This is the direct topic of two related grand challenges. The more theoretical one 'Science for Global Ubiquitous Computing' plans to explore the underlying scientific framework for the GUC and to develop a coherent informatic science whose concepts, calculi, theories and

automated tools allow descriptive and predictive analysis of the GUC at many levels of abstraction. The goal is that every system and software construction—including programming languages—shall be fully explained in terms of a theory with a limited range of simple concepts; and that all design decisions can be justified by these theories and checked by tools based on the theory. The related, more practical challenge 'Scalable Ubiquitous Computing Systems' takes a more experimental, engineering approach to the design and exploitation of the GUC. It will explore the compromises and the technological refinements that are essential in all practical engineering projects. Its goal is to formulate general design principles, perhaps maturing from ad hoc rules of thumb, pertaining to all aspects of ubiquitous systems, and to justify these principles both by instantiation in successful real systems and by theoretical analysis.

In the future, the power of computers as repositories for information will probably exceed their significance as devices for calculation and communication. By far the greatest part of the stored information will be personal—photographs, video clips, correspondence, diaries, tax forms, marriage licences, email sent and received, web pages visited, telephone calls etc. To exploit this power for the benefit of the individual is the subject of the third challenge 'Memories for Life'. This project has to tackle serious questions of the permanence, privacy, provenance and ownership of the data, as well as the central challenge of rapid and accurate recall of information by loose association. Will the computer be able to answer such simple requests as 'find the picture of me playing trains with my nephew Peter' or 'What did we have for dinner when my school friend from Scarborough came to visit last summer? (and what was his wife's name?)'. The power of this kind of recall could be of increasing value in an aging population.

The rapidity of developments in computing hardware is now fully matched by experimental techniques in all branches of biology. Twenty years ago it was possible for a skilled biologist to sequence about 70 base pairs of a genome on a good day. Now a well-equipped laboratory can sequence 2 million base pairs per day. The amount of raw static data being accumulated is far beyond the range of current theoretical understanding of classical biology. Computer science is familiar with similar problems of scale and complexity and perhaps can contribute (or more likely learn) some relevant theoretical ideas in biological modelling. In particular, computer simulation of theories of biology will have to make predictions that can be tested against experimental reality. This is the topic of another grand challenge proposal, 'In Vivo–In Silico'. To make progress, it will probably concentrate on simple organisms that are the subject of widest biological experiments, e.g. the thale weed *Arabidopsis thaliana*, which is currently under intensive study at the John Innes Centre in Norwich.

An ultimate joint challenge for the biological and computational sciences is the understanding of the mechanisms of the human brain, and its relationship with the human mind. A single human brain has about a million million nerve cells,

and its accurate simulation will be far beyond the power even of the whole of the GUC, even in 20 years' time. A computer program that throws light on the mind/brain problem will have to incorporate the deepest insights of biologists, nerve scientists, psychologists, physiologists, linguists, social scientists, and even philosophers. This challenge, 'The Architecture of Brain and Mind', is one that has inspired computer science since its very origin, when Alan Turing himself first proposed the Turing test as a challenge for artificial intelligence, still unmet.

The classical theory of computation is based on another of Turing's insights. The Turing machine is an imaginary computer that is the archetype of all conventional machines of today. Like them, it is structured as a single localized computer sequentially executing a deterministic program to completion. But computations carried out in nature, e.g. in the brain and body of a living organism, are nothing like that. They are widely distributed over space and over time; they essentially involve massively parallel operation; they involve continuous interaction with their environment; and they are highly non-deterministic. The GUC is much more like a living organism than the Turing machine. So the next challenge 'Journeys in Non-classical Computation' is to find a theory of computation more firmly based on a non-classical model, more realistic than the Turing machine, perhaps even taking into account the discoveries of biology and the promise of the quantum computer.

The final challenge addresses an all-too-familiar issue of the dependability of software in daily widespread use. It aims to answer questions on the safety and soundness and security of programs running in homes, offices, cars, planes, rockets, ... The project is to develop technology and tools that will get the computer itself to guarantee the integrity, the safety and even the correctness of its own programs; and the guarantee must remain in place as those programs are gradually changed and evolve to deliver improved service or to meet new needs. The project 'Dependable Systems Evolution' will throw light on the logical foundations of computer science and its application to software engineering; also, the basic knowledge and understanding gained in the project is highly relevant to the problem of inadequate program testing, which was estimated in 2002 to cost the US economy something between 20 and 60 billion dollars per year.

In summary, these are seven projects that have excited the enthusiasm and curiosity of computer science researchers in the UK. Many of the themes are echoed in similar conferences held by the Computing Research Association in the United States, and they have attracted interest from scientists in many countries of the world.

4. CONCLUSION

The approach taken to these grand challenges by university researchers is driven primarily by scientific curiosity and idealism; the aim is a basic understanding of computational phenomena, even if it takes many decades to acquire, and involves widespread international collaboration.

The discoveries of the pure research of academic computer science in this country and throughout the world are as valuable in themselves as the discoveries of any other branch of science; and it is easy to predict that some of the discoveries of research directed towards grand challenges will be the basis of revolutionary improvements in the way that we exploit the fantastic power of our future computing devices for the enduring benefit of mankind.

REFERENCES

[1] Grand Challenges for Computing Research, proposals and discussion. www.nesc.ac.uk/esi/events/Grand_Challenges