e-Brain - A Large Scale Brain Modelling Experiment

Mark Lee, 9th September 2002

Grand Challenge

The challenge is to build a massively connected (possibly distributed) simulation model that can simulate some of the key attributes of the human brain. The aim is to explore brain-like computing as a potential new computational technology, not to make advances in brain science or psychology.

Data and insights from psychology, computing and neuroscience will be recruited in the design and implementation, but high fidelity of the model is not as important as exploring a wide range of highly connective cognitive structures that may offer useful new computational technologies.

This challenge has resonances with the Foresight Cognitive Systems programme which predicts major new developments in the physical and life sciences in the next decade and states that "artificial cognitive systems equalling or surpassing the capacity of the human brain are a real possibility in the future."

Background and Motivation

The workings of the human brain are one of the last great remaining mysteries in science. Recently much new knowledge has been gleaned, especially about the structure and gross activity levels of brain regions. However, we still know very little: in terms of our huge advances in other areas of science and technology the complexity of the brain still defies proper scientific understanding.

The brain consists of a huge number of massively parallel computing elements: some 10^{11} neurons, with over 10^{14} internal connections. There exists considerable knowledge of individual neuron structure and behaviour but this seems to help little in explaining how systems of neuronal elements function. Current thinking suggests that most brain activity *essentially* involves large numbers of neurons in groups, $>10^4$, and that the study or simulation of small assemblies will not therefore lead to useful insights. The Nobel Laureate Gerald Edelman has pioneered this approach, called synthetic neural modelling, and shown how early brain-like functions can be developed in simulation.

Aims and Objectives

The goal is to investigate, design, construct and study a brain-like computational engine (and variations) that is inspired by, but not dominated by, known principles of neural organisation. There are a number of issues to address:

Tools As analysis of massively parallel simultaneous events is currently impossible, so understanding may be gained through the modelling and synthesis of brain-based devices. However, this poses a methodological problem as the brain-like system will need to be analysed and evaluated. All our traditional analysis tools (mathematics, statistics, and even diagrams) are ultimately based on serial structures (languages) that are appropriate for human serial comprehension. We will need to invent significant new tools, notations and methods for the monitoring, study and evaluation of new brain-like computers.

Modelling Authenticity between model and brain at all levels is not as important as implementing and exploring a range of structural ideas and operational data on the brain at the behavioural and functional levels. The model will thus not focus on a narrow approach or specific technique but will explore a range of general heterogeneous structures and architectures. For example, current artificial neural networks will be unsuitable as they contain minimal structure, are quite primitive, and behave very unrealistically.

System behaviour Much knowledge is available from cognitive science where human behaviour is treated in terms of: adaptation, perception, memory, response co-ordination, proto-communication, etc. This expertise will be needed in assessing a brain-like computer. Conventional computational measures and performance concepts will not be appropriate and new ways of viewing tasks, programs, results and operations will be needed.

The Vision of the Challenge

Ultimate success will deliver a new form of computational system that has a range of novel features and implications. The system will be brain-like without being an authentic copy at the neural level. It will be a system of very high complexity that can not be used by conventional methods but can be approached through new ways of human-machine interaction. It will offer a complementary role to that of existing high performance computing. Some of the features displayed will include:

- autonomic - self-managing
- self-organising - highly adaptive
- reliability - flexible and resilient
- multilevel structures - hierarchical, multimodal and non-linear
- internal dynamics - significant interaction of modules/cells
- emergent behaviour - internally generated

Response to Challenge Criteria

1. *It arises from scientific curiosity about a major unsolved problem area.*
As one of the major enduring scientific problem areas, there are many disciplines and technologies that are keenly interested in this topic - and which would also benefit from the results.

2. *It gives scope for engineering ambition to build something that has never been built before.*
Such a large-scale model has never been constructed before and just its existence will give experience of studying, operating and managing such a complex system - quite apart from the main aims and objectives of the project.

3. *It has enthusiastic support from (almost) the entire research community, even those who do not participate and who do not benefit from it.*
This problem is widely recognised across science and many would support it. While many will have views on the best way of proceeding, only certain philosophers would argue against the concept or the value of such a model.

4. *It has international scope: participation would increase the research profile of a nation*.
The topic is global and international. Many research labs would follow the project and many would wish to participate.

5. *It is generally comprehensible, and captures the imagination of the general public, as well as the esteem of scientists in other disciplines.*
This challenge concept is already well within the public's grasp, often being discussed, in various forms, in the popular science press. Human Cognition stands along with Black Holes, Cosmology, Evolution, Prehistory, Cyberspace, the Origins of Life, and Quantum Phenomena that fascinate and capture the public imagination. Notice that "Consciousness" is also a popular science topic but this is an area of contention that we do not include as a serious part of the challenge.

6. *It was formulated long ago and still stands.*
The workings of the brain are an age old problem that has seen little progress until the last century. Recent findings are accelerating and knowledge has grown rapidly in the recent decade but many key issues are still relatively untouched.

7. *It goes beyond what is initially possible, and requires development of techniques and tools unknown at the start of the project*.
As mentioned above, we do not have suitable tools, notation, or mathematics to handle systems of enormous size. Designing and building these is a significant part of the challenge.

8. *It calls for planned co-operation between identified research teams and communities*.
No one discipline (never mind research team) has enough knowledge to carry out the challenge alone. Expertise, data and co-operation from many different labs, teams, organisations and individuals will be involved. It seems likely that the quality of the final result will be proportional to the range of co-operation that can be achieved.

9. *It encourages and benefits from competition among identified individuals and teams, with clear criteria who is winning, or who has won.*
Co-operation (see above) is more important than competition. Some competition may emerge but this would be a side effect.

10. *It decomposes into identified intermediate research goals, whose achievement brings scientific or economic benefit, even if the project as a whole fails*.
There are component parts to the challenge, and each of these, e.g. tool-building, offer benefits in their own right. However, the complexity of the system to be built must be beyond the current threshold of full human comprehension. The essence of the challenge is to explore how to use and understand systems of such complexity and so, by definition, they are not scalable or decomposable into mini-problems.

11. *It will be obvious how far and when the challenge has been met, or not.*
A set of metrics and performance levels can be investigated to measure progress and to classify system behaviour. The tools developed for analysis will be important in this respect. It is also expected that general perceptions of system behaviour will be manifest in some form that will be readily appreciated by lay observers.

12. *It will lead to a radical paradigm shift, breaking free from the dead hand of legacy*.
Regardless of the value of the results of the challenge model, this approach has not been tried before on this scale and the endeavour itself will mark a new attack on the problem, thus stimulating new approaches and further investigation.

13. *It is not likely to be met simply from commercially motivated evolutionary advance*.
As the challenge addresses a relatively pure scientific question, there has been little commercial investment in this area. Also its size is revolutionary rather than evolutionary and so requires a leap of commitment to initiate. Ironically, the commercially driven evolution of computing power is what makes this challenge now possible. However, supercomputers provide the substrate for the challenge, not the solution.