overall impression is that compared with US, we are not doing enough in this country and it is not clear what we should be doing. The US places more emphasis on mission-driven research and the term R&D is a very broad category. In the UK we draw more heavily on basic science in conjunction with universities, and this raises the question of how much defence science can the nation afford?

The expenditure on R&D takes several different forms from applied science with rapid returns to basic science which takes longer to generate an impact. Hence measures of expenditure do not provide a clear view of the overall benefit likely to be obtained. The presentation exhibited our strengths essentially in physics and engineering. However the whole point here is to get an advantage in conflict and to respond to the adversary doing different things. If Iraq teaches us anything it is that we have done very badly in anticipating the outcome. Who should be having the responsibility for a more sophisticated heart of research in defence and asking what is the nature of tomorrow's conflict? Have we got to relearn the lessons of Malaysia that we have forgotten? Whose responsibility is that? How is the world changing? Do you accomplish your goals by retooling a bomber? Or might you be better off by not dropping bombs?

The return of increased capability for expenditure in the UK is value for money when compared with the US, which shows lower rate of return overall for a much greater expenditure. It is not easy to understand the future, it may be possible to understand the risks. You do need to have somebody responsible for a no-holds-barred approach. However the approach should assume that you will not necessarily be able to foresee the outcomes and therefore need to build flexible architecture into the platforms that can adapt to the circumstances as they evolve. People in MoD are speaking that sort of language. Another strategy would be to invest in people skilled in social sciences.

China in 2020 and the UK appear well positioned on the capability chart with an optimum return for the investment made compared with all other entries. Fundamental research investment in the UK Research Councils also bypasses the MoD. The model adopted in the UK depends on the relationship between the science base academics and the take up and build supply chain that makes things happen, as exemplified by QinetiQ. That is what matters and work at the University of Warwick is a good example of this arrangement.

The US spends approximately $600 billion on defence and a further $100 billion on homeland security and intelligence, much greater than anyone else, which puts them in a different league. There is no sign of any slowdown in this expenditure. In the UK we use our skills to take technology such as Global System for Mobile technology (GSM) for example back to the UK. Our defence science base also facilitates our interaction with the US in an effective way. This enables the UK to sit at the top table and access US development technology directly, especially because they know we have the knowledge to do it ourselves should we need to do so.

TOWARDS 2020 SCIENCE AND THE EUROPEAN SCIENCE INITIATIVE

MEETING OF THE PARLIAMENTARY AND SCIENTIFIC COMMITTEE ON TUESDAY 15TH JULY

Towards 2020 Science

Professor Stephen Emmott

Head of Science, Microsoft

Science will be absolutely central – indeed critical – to understanding and addressing the most important challenges we face this century; indeed perhaps any century, not least because the scale of the challenges is so great this may be the last century we have to address them. Chief amongst them are:

1. possible rapid and highly non-linear climate change and loss of Earth's life support system,
2. pressures on the planet with a population of over 9 Billion people,
3. intractability of the prevention and eradication of a range diseases that prematurely kill millions of people every year,

4. the real risk of a sudden global pandemic that could kill hundreds of millions, possibly over a billion people.

These challenges potentially [probably] have profound implications for the continued availability and security of food and water, the displacement and movement of hundreds of millions of people, the means by which we will need to produce our energy requirements, and for our very future and the future of thousands of species with whom we share our planet.

Major advances in science have been made over the past fifty years in particular, in areas spanning biology (especially molecular biology and genetics), medicine, physics, astronomy and climatology. And yet, despite what are undeniably remarkable scientific achievements, our understanding of our climate system is far from adequate and – alarmingly – we are still not able to predict *accurately* the scale, nature and consequences of either climate change or resource (especially 'ecosystem services') loss over this century. We still cannot fully explain even the working of a eukaryotic cell, let alone how even 'simple' organisms such as the 1mm long *C.elegans* nematode worm works. And we are nowhere near an understanding of, for example, the brain, or the one thing standing between you and me and the cemetery – our immune system. Thus, predictive biology is almost unheard of, predictive medicine remains largely an aspiration, and millions of people continue to die prematurely of disease because our understanding of the functional aetiology of disease, and effective treatments, remains inadequate.

The primary reasons for this state of affairs are twofold: first is because they are complex natural systems – complexity being the problem. The second is because complex living things continue to resist revealing much by the reductionist approaches characterising current scientific methods used to study them. Third, the kinds of conceptual and technological tools currently available to scientists are therefore not sufficient. In short, significant barriers exist to fundamental scientific progress in precisely the areas in which advances are urgently required to address the enormous societal challenges we face.

I believe that breaking through these barriers requires – and urgently requires – a radical re-think in science. A re-think that would certainly represent a transformation of how science is done, and a transformation that would perhaps form the foundations of nothing less than a new scientific revolution, and 'new kinds' of science.

This is a non-trivial statement. Transformations in science are rare. Scientific revolutions are rarer still. Arguably there's only been one – '*The* Scientific Revolution' of the 17th century. What brought this one about, and what will bring about the one we now need, are three important things. First is the development of new 'conceptual' tools (eg, Copernicus' use of algebra enabling a precise, formal, testable theory of the heliocentric universe; Newton's calculus which underpinned formulation of the laws of physics, thermodynamics and the universe). Second are new 'technological' tools (eg, Kepler's mechanical model of the universe, Galileo's telescope). The third are new kinds of scientists: highly quantitative, computationally literate natural scientists who also have a *different way of thinking* about problems (call it creative imagination in scientific discovery) whose hands these tools are created by and/or get into. When the combination of these events occurred in the 17th century it created a *'new kind'* of natural philosophy: *Science.*

I believe that in order for science to fulfil its important role in understanding and addressing the challenges we face, we once again need radically new kinds of conceptual and technological tools, and new kinds of scientists who can create them and use them.

New Kinds of Conceptual tools

The conceptual tools of the 17th century enabled what I might term the 'Codification of Heaven' (a precise explanation of the solar system). The new kinds of conceptual tools we need now are those that enable the codification of *Nature.* That is, the precise, formal representation and accurate prediction (predictive models) of *dynamic processes* of complex natural systems – from biochemistry and cells, to *C.elegans*, sea urchins and the brain, to forest dynamics and the Biosphere.

By *codification* I mean literally turning knowledge into a coded representation, in terms of data or programs, that is mechanically executable and analysable. The overall task typically involves building mathematical models of natural phenomena – from biochemistry to biotic-abiotic coupling and feedback of the climate system. But it goes beyond that, turning models into coded representations that are useful to the broad scientific community. Codification is just beginning in the major fields of scientific knowledge. Codification has at least one basic scientific property: once obtained, it can be right or wrong, or 'not even wrong', but it is at least exactly reproducible and independently analysable. The general, hardest, problem in this area is going to be how to store, search, compare and analyse biological *processes*. A process, here, is intended as a dynamic interaction of multiple discrete components, eg the process of cell division.

This last example brings into focus the full meaning of codification: it is not, in general, just to represent scientific facts as *data*, but to represent scientific phenomena as dynamic *processes.* Martin H Fischer's aphorism emphasises the point: "Facts are not science, as the dictionary is not literature".

A considerable part of this effort will be underpinned by concepts adapted from computer science. Calculus, and its more modern derivatives, is the main way in which mathematics deals with dynamics, but it does so in a continuous fashion. In contrast, computer science deals predominantly with the interactively discrete *(reactive)*. In most kinds of complex systems, biology being the primary example, the discrete is both central and is also much harder to deal with; they not only behave but also affect, prescribe, cause, program and blueprint other behaviour. In short, the characteristics of computer science are also central to the dynamics of biological systems: concurrency, time dependence, cause-effect phenomenon and distributed control.

New kinds of technological tools

Hand in hand with new kinds of conceptual tools is the need for new kinds of technological tools – computational tools – for doing new science. Here I want to distinguish between *computation* and computers. Computers have played an important role in science for almost 50 years, and will continue to do so. However, I am emphasising something very different. It is a fundamentally important shift from *computers* supporting scientists to 'do' traditional science to computational methods transforming the *kind* of science possible. Such tools will include those to implement the new conceptual methods (eg, programming languages for modelling biology will form the foundation of tomorrow's 'systems biology'); computational tools integrating data, models and theory; computational tools for the development of complex dynamic models of complex natural systems and which will enable scientists to perform realistic experiments on a computer. These kinds of technological tools, combined with the new kinds of conceptual tools I briefly outlined, will transform how science is done and the kind of science that is possible, enabling new kinds of science.

New Kinds of Scientists

Critical to the realisation of the new kinds of science required will be new kinds of scientists. By 'new kinds' of scientists, I mean a generation of scientists who will not just work in highly inter-disciplinary, highly computational science, but who are themselves inter-disciplinary and highly computationally literate. But even over and above this, we need to be producing scientists who have a different way of thinking about currently intractable problems. There is an urgent need to re-emphasise the importance, and encourage the development of *creative imagination* to scientific discovery. Its importance is best emphasised by Einstein and Infeld (1937): "The formulation of a problem is often more essential than its solution, which may be merely a matter of mathematical or experimental skill. To raise new questions, new possibilities, to regard old problems from a new angle, requires creative imagination and marks real advance in science". We have barely begun to produce such scientists, but it is at least starting to happen. In the UK, Oxford is leading the way on this front, primarily through its Life Sciences Interface Doctoral Training Centre. Elsewhere, the Weizmann Institute of Science in Israel also stands out. We need to do far, far more in creating the kinds of scientists we urgently need.

New kinds of Research Institutions

This brings me last, but not least, to the need for new kinds of research laboratories. We need more labs that attract, produce, develop, bring together and enable to flourish these new kinds of scientists, and that pioneer these new kinds of science. Such research labs are rare. Janelia Farm (Howard Hughes Medical Institute) is one of them. In uncharacteristically immodest fashion, I believe I can rightly claim that my own laboratory in Microsoft Research in Cambridge is another leading this transformation. I mention this not to boast, but to indicate how few of such labs exist and how much needs to be done – and done urgently – to lead a transformation of science that will break through barriers in important areas of science; an undertaking of profound importance if we are to tackle the profound challenges we face this century, as well as the unprecedented social, technological and economic benefits that achieving this would bring.

During discussion the following points were raised

No mention had been made of quantum computing as the decision had been taken to focus on natural complex systems in order to be able to learn how to build complex systems for future use. The development of quantum computers, if even possible, is seen as 40 years away into the future.

The question was raised: how can computing help to control a pandemic? By understanding the problem, modelling early stages, differentiate between pathogens, identify general principles and apply the outcome to 6.3 billion people. A computer model exists that accounts for every plane flight from every country that can be used to model how a disease could spread. This can be applied to both existing and potential pathogens and take account of rapid mutation in malaria for example.

One of the implications of the use of predictive modelling in molecular biology is that school education is heading in the wrong direction and has been doing so for 15 years. Much greater interaction is required between arts and sciences, in preference to interaction within these areas.