For a postgraduate student embarking on a research career in computer and information sciences the world must appear a very daunting place.

The orderliness that existed when in 1976 Peter Wegner proposed that Computer Science (we were still able to find a single name to describe what we did then) was dominated by empirical research paradigms in the 1950s, by mathematical research paradigms in the 1960s and by engineering oriented paradigms in the 1970s, seems very distant. Incidentally, Wegner's assertion expressed at that time that "information as the central idea of computer science is both scientifically and sociologically suggestive" has certainly been born out.

Is it possible, let alone easy, to identify the dominant research paradigms of the subsequent three decades? Indeed have there been dominant research paradigms? Do the paradigms we might have identified in computer science still apply in an environment where computer science is but one aspect of information and communication technology? What are the things that characterise the nature of research in our discipline(s) today?

To help answer these questions it is useful to look at what are seen as the big research questions.

In 2002 the Computing Research Association (CRA)convened a group of researchers to discuss the urgent research challenges related to building information systems of the future. The concept of the grand research challenge is not new. Putting man on the moon and mapping the human genome are examples of grand challenges.

The five grand research challenges in information systems that emerged are:

1. **Create a Ubiquitous Safety.Net**

   Providing a ubiquitous Safety.Net will save lives and minimize damage from disasters through timely prediction, prevention, mitigation, and response.

2. **Build a Team of Your Own**

   Building cognitive partnerships of human beings with software agents and robots will enhance individual productivity and effectiveness.

3. **Provide a Teacher for Every Learner**

   Tutoring each individual in a tailored, learner-centred format will enable people to more fully realize their potential.

4. **Build Systems You Can Count On**

   Assuring reliable and secure systems - from the regional electric grid to an individual's
heart monitor - will allow us to rely on information technology with confidence.

5. Conquer System Complexity

Building predictable and robust systems with billions of parts will enable broader and more powerful applications of information technology.

One could devote at least an article to discussing each of these separately. But the interesting question in this context is "what are the characteristics of these challenges from the perspective of an ICT researcher". What are the common features they share that might give a clue to the sort of tool-kit graduate students might need to equip themselves for research in ICT today?

Firstly, they are strongly applications-based. The applications are extremely complex and integrated. They rely on substantial computational infrastructure. Secondly, each has a societal perspective. Concerns about human and organizational issues are at least as important as concerns about machine and computational efficiencies.

Finally there is a holistic element to solving the problems. Participants in the CRA workshop decided that there are advantages to a framework for systems research that emphasizes full-system solutions to daunting problems.

This perspective is reinforced by recent experiences with a major multinational industrial research partner of my institute. Coming from a strong technical base in telecommunications, a base which saw this company develop and exploit some of the major technical research advances over recent decades, it now saw the key opportunities in the value chain moving towards providing total solutions. This brought them into a space they were unprepared for. Where concepts like 'quality of service' once dominated and sufficed (concepts that were readily identifiable through parameters that were indicative and measurable), now the market emphasis moved to needing to research and measure 'quality of experience' - more diffuse, systemic concepts with a strong orientation to the usage chain. And the measurement of quality of experience needs to draw the researcher beyond the comfort of quantitative research methodologies focused around conventional scientific method into methodologies drawn from other disciplines, such as the social sciences - grounded methodologies, methodologies requiring contextual integrity.

So the well-equipped ICT researcher needs to not only understand but also to respect methodologies drawn from a wider world than the empirical paradigms Wegner referred to. The real challenge confronting the researcher though is how to integrate all these approaches which individually address parts of the research problem into something which protects both the external and internal validity of the research.

Finally an observation. It is curious that as disciplines as diverse as commerce and biology require ever increasing sophistication in mathematics to model and underpin research, the information and computing sciences are steadily but consistently de-emphasising the importance of mathematics, and decreasing the mathematical component of undergraduate curricula. While this can be readily seen in the superficial way many statistical analyses are done in published ICT research, it is perhaps more invidious in the inability to develop sophisticated models. Description is certainly a key aspect of research, interpretation and explanation more important, but it is the predictive value arising from research that is perhaps of most value to the external community.

So it is an interesting challenge for a student embarking on a research career, with a breadth and depth of knowledge required beyond that of the past. But it is important not to forget what a re possibly the prime drivers of successful research - passion, curiosity and an ability to take advantage of serendipity - and of course, a capacity to continually learn.

Sir Alexander Fleming summed it up this way in his Nobel Prize acceptance speech in 1945:

"... team work may inhibit the primary initiation of something quite new, but once a clue
has been obtained team work may be absolutely necessary to bring the discovery to full advantage” and “...destiny may play a large part in discovery”

References
