7 steps to “You do the walk, the walk of life ...”*

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Co-Director, Oxford Cancer Imaging Centre

Deputy Chairman : 1994 – now

Founder and Chairman, 1992-now

Founder Director, 1999-2003; 2008-now

Founder Director, 2008-now

Director

Chairman

Chairman

Chairman

Chairman

Chairman
1. Identify a major goal

• Formulate a 5-10 year plan to attack a major problem

• Don’t be a perennial puzzle solver, generating a string of incremental advances, published at minor conferences, and which are instantly forgotten

• Ask yourself “10 years from now what will be the foo* effect, the foo process, ...

• Be bold and think long term, then start putting the pieces in place

Theory-only advances are admired for a while; Many practical advances lack theoretical foundations**
Aim to solve an important practical problem based on a sound theoretical base

*YOU are foo, if you see what I mean...

**i.e. They are hacks
2. Be patient – a case study

- 1989-93: a bold idea that challenged the orthodoxy (a physics model of mammographic image formation)
  - Avoid puzzle-solving incremental step research. Identify an important problem and use it to make your name
The fundamental problem of Mammography: need for quantitative analysis

Two of the UK’s most experienced breast radiologists each examined the two mammograms shown, to estimate the percentage of dense tissue – a key risk factor for breast cancer. A (left) estimated R = 25%; B (right) estimated R = 40%.

In fact, the two mammograms are of the same breast; but the pair shown left was exposed for twice as long as the right. The two numbers 25, 40 should be the same!

Image intensity relates to anatomy in a very complex way, making quantitative image analysis a hard problem.

During the 1990s, with Ralph Highnam, I invented a solution: we developed an algorithm based on modelling the physics of image acquisition to give a quantitative representation of the image – assigning to each pixel $x$ the amount of non-fat tissue $SMF(x)$ at that pixel location.
2. Be patient – a case study

- **1989-93: a bold idea that challenged the orthodoxy (a physics model of mammographic image formation)**
  - Avoid puzzle-solving incremental step research. Identify an important problem and use it to make your name

- **1993-96: It was too much for peer review**
  - New kid trampling on their territory; I took a chance on another grant ...
  - My experience is that peer review generally favours incremental advances, despite the encouragement of the research councils. Make your proposal a judicious mix of solid technical stuff and the bold stuff that you really want to do

- **(1985 – present) research council support and encouragement**
  - They are friendly, supportive and incredibly helpful; don’t be afraid to ask.
  - Being awarded a Senior Fellowship was career changing for me

- **It took 8 years for our stuff to be accepted by the academic community (2000), and 12-15 (2005) by industry (book in 1999)**
  - This is normal on both counts. Sadly, Academics are intrinsically conservative; happily, so is industry – it has to deliver systems that work, all the time, every time

- **Graduate students are the engines of research**
  - Great qualities ... my proudest achievement is 115 DPhil students
3. Build partnerships that last

A flow of people, ideas, & intellectual property

Research

Clinical collaborators

Custodians of the problem, evaluation – positive & negative

industry

Companies sell systems, not universities

Quality systems & FDA

Need ideas, people, customers

Face challenges that limit effectiveness of products

Engineers have a key role to play in this process: We understand applications and science-based applied work, and project work makes us ready collaborators
4. Building a group

• Often, you don’t need a lot to get started: \textit{believe yourself & just do it}
• It’s much easier if you start “within” an already established group
  – Infrastructure is in place, and it is infrastructure/set-up that often costs the big money
  – The group does not have to be in your department, or even your university: the internet is a powerful tool
  – However, reporting to a supervisor who does not allow initiative is a bad place to be
  – Students cost a tiny fraction of a postdoc; but do different things
• The research councils are supportive, particularly of starting researchers
  – There’s a (bewildering) set of funding sources: be mentored
  – On the other hand, most universities are broke
  – It is easier to get industrial funding once you have track record
  – The best way to get to industry is by the recommendation of a colleague who is already working with that company
• Maximise your efficiency
  – Spread your funding, and if you are spending too much time writing proposals, hire someone to do it for you
5. Re-invent yourself every 7-8 yrs

Becoming interested; framing the problem; learning what has been done, and what has not. What can I contribute?

Beginning to contribute; conference papers; lots of rejections; the first journal article (in a reasonable journal)

La Dolce Vita!! Period of maximum contribution to the subject. Work submitted to RAE/REF/Noble Prize Committe, grants, journal articles, promotion, prizes, television interviews, keynote lectures, and a head of department who smiles.

Either, restless mind takes you elsewhere or genuinely original thoughts are fewer. Slowing down. Time to hand problems to graduate students and...

The next phase of life!
My latest reinventions

  - Breast cancer: mammography, MRI; Image “fusion”
  - Functional MRI of the brain

- **Re-invention 6 (2003-2010) Molecular Imaging of cancer**
  - Cellular pathways models of cancer growth and relationship to images; translational medicine; Oxford Cancer Imaging Centre

- **Re-invention 7 (2010-2012) “Retired”**

- **Re-invention 8 (2012-present) Professor of Oncological Imaging**

Left: Radiation Oncology & Biology; and IBME
Right: Cancer centre – 100m away
6. Graduate students

- 115 graduated to date...

- Why else would I work at Oxford?

- Sir Humphry Davey was asked “what has been your greatest scientific discovery”

Graduate students need to be developed to be better than you; they are not cheap labour
7. Formulate your own personal driver

Publishing papers and books is satisfying; but... our aim has been that the results of our research are used daily by thousands of people

Science that addresses fundamental problems of a well-defined practical problem:
- our systems are used by non-experts
- have to work 24/7, 365, 99.9%

Universities don’t (and should not) build systems within quality processes, sell, or maintain systems

License technology

Everyone at MICCAI hopes their work will contribute “eventually” to medical practice/science

Reality
Clinicians don’t download freeware software systems and use them for routine clinical use
Companies very rarely pick up a published paper, implement it, & sell it

Start new companies
Why start companies?

1. Frustration of dealing with large companies, particularly in medical image analysis, *and particularly in the UK*
   - 99% of Mirada’s sales are in the USA, as are Matakina’s
2. I can’t help it (Guidance, Mirada Solutions, Mirada Medical, Matakina, …)
3. Secure the kids’ futures yet live with academic poverty
4. The dream of a swimming pool in Provence …
What am I doing now?

Because all cells metabolise glucose, a variant of glucose with OH replaced by H was developed in the early 1950s as a drug to block accelerated rates of glycolysis in cancer, hence tumour growth. However, it also blocked glycolysis in the brain, so it could not be used as a drug ... but it has become a VERY useful imaging agent!!

Most clinical PET imaging uses FDG; but the fact that ALL cells metabolise glucose means that in some cases it has poor specificity!