Talk at Cardiff

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These notes are addressed in a grandfatherly kind of way to the junior researchers in this project and cover four things that might well be relevant.

Objectives

The real objective of Marie Curie Fellowships is not a scientific one, but a political one - the creation of good Europeans. Cosmin doesn't have to report back on this because there is a hard constraint on partners is that the fellows they employ should be from another country.

As a result, you will have learnt that people in some other country are not ogres, demons waiting to gobble you up, but just people. Some might be nasty and others nice, but there is that variation in people in every country.

There has recently been a tendency in the UK and in some other countries for politicians to seek votes by fostering people's anxieties about foreigners. If you have benefitted from this project you have some responsibility to counter this, confronting such scaremongering by making your own factual experience of foreigners known.

Feynman

Richard Feynman was a physicist, who developed the theory of quantum electrodynamics. He did a lot else and there are a couple of easy-reading quasi-autobiographical books [1] [2] which I strongly recommend as holiday reading instead of a novel. He is concerned about science and in [1], pp 211–291 and pp 338–346, he writes about what makes science work. I paraphrase his message as *"Don't believe any authority (tradition, senior people, big names, your supervisor, any textbook, even Feynman)."*.

It is the ability to doubt which allows science to progress. Galileo could say that Ptolemy might be wrong. Kepler could say that maybe planetary orbits were ellipses instead of circles. Einstein could say that Newton might be wrong. Once the doubt has entered, experiment or observations can settle the matter.

Just scepticism is, of course not the whole answer. The real message is

"Don't **just** *believe any authority:*
Listen to them and make sure you understand what they say.
Then compare it with your own experience
and if necessary maybe even do some experiments.
Then make up your own mind."

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[1] *Surely You're Joking, Mr Feynman!* ISBN 0-04-530023-2
[2] *What do* **you** *care what other people think* ISBN 004 440 5286

Bondi

Hermann Bondi was also a theoretical physicist working at the other end of scales. He worked with Fred Hoyle on the steady state theory of the universe, which ultimately got disproved by detection of the background microwave radiation which could be better explained by a phase change in the early universe. If there was a phase change the universe can't be steady.

He gave an after-dinner talk that I want to share. Again I paraphrase:

"The amount you can contribute to a field is proportional to the amount you know. It is also proportional to the amount you don't know. With any reasonable model of learning the amount you know will increase with time and the amount you don't know will decrease. Your rate of contribution will therefore rise to a maximum and then decrease. To maximise your lifetime contribution you should therefore move to a new topic every five years - or maybe seven when you are older."

My reaction to this was that five years seems very short: maybe it is right for him because he was very smart, but I am not that clever. It must be longer for me. How can I tell what my time-constant is ?

So last Monday I did some calculations. I just assumed that the amount not known would decay exponentially with time and the amount known rises correspondingly*.

$$\bar{k} = e^{-\alpha t}$$
$$k = 1 - \bar{k}$$

Rate of contribution $c(t) = \beta k(t) * \bar{k}(t)$ does indeed rise to a maximum and then also decay.

Lifetime contribution can be maximised by taking a variable period τ and evaluating the average contribution over that period.

$$C(\tau) = \int_0^\tau c(t)/\tau$$

This new function of τ also grows and then shrinks. At its maximum your average rate of contribution is maximised. Differentiating it tells you where it starts to fall off.

Now all of this has a time-scale defined by α, and if we evaluate the value of $k(t)$ at the maximum of C, we get the answer, independently of α and β, *"switch topics when you know 72% of the possible knowledge in your topic"*.

That is not a comfortable thought, because at the end of a PhD you already probably know more than 72%. Should you drop your topic straight away ?

No, because you can slide back down the curve by merely broadening your horizon a bit. It is certainly not usually a good idea to try to make a life-career out of a narrow PhD.

* Please try this yourselves with different assumptions for the learning process.

Incidentally, Bondi's Wikipedia page says that he spent his first 30 or so adult years in theoretical physics and then moved into a sequence of jobs where his scientific quality (despite the fact that his theory had been disproved) proved valuable. He directed the European Space Agency, he became Chief Scientist in two UK government departments one after another and after a couple of other jobs became the Master of a Cambridge College. His own job-switching was therefore 30 years in his first field and then about four years each in the next half a dozen, when he was older, so maybe he was falling into the trap which easily ensnares older people and recommending *"Don't do as I do, do as I tell you."*.

What Next

This project is nearing completion. Most of you will be submitting your dissertations soon and then looking for a job. You would probably have first in mind an academic career, leading via a few postdoc positions to a tenured position in a university yourself.

This is not realistic for all of you. During a professor's lifetime he or she will supervise many PhD students, and if the total number of academic posts is constant only one replacement will be needed for that position. If each prof supervises 10 students (a low estimate on average), then only 1/10 of those students will get a similar post. Even in countries where the academic sector is expanding this figure will not increase by a factor of 10.

Your experience in this project may give you an edge. It will look good on your CV, but even if it gives you a factor of two advantage over your rivals that only means that two or three of you will actually become long term academics. Most of you will land up with other employers, and it is worth considering the two most obvious ones as well as academia

The options are

(i) Academia, where you will land up teaching, applying for grant money and supervising other people's research rather than doing your own.

(ii) Industry, working for a company, probably in an R and D department.

(iii) Government, not as a politician, but in the Civil Service, making decisions about what research gets government funding, or else doing research yourself in a government lab.

In all three the salary rates for similar skills are about the same. In all three there are good and bad individual employers, though the variance in government is probably least.

In all three there is scope for continuing in a research activity, (though there are rather few government research establishments left in the UK) but in a company you can get (if you are lucky) the good feeling which comes from solving a customer's problem. Development isn't research, but it may not feel very different from the experiment-building which you have done in the course of your research.

I wish all of you in your future careers the good luck, amenable colleagues and diverse challenges which have made my own career (in academia, industry **and** government) such fun.