The Business of Science

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1. Good Science, Bad Science

In science we have good, successful laboratories and scientists and we also have wasteful, inefficient and unproductive ones. The differences are not matters of chance.

It is not an accident that just one Cambridge College, Trinity, has achieved more Nobel Prizes than all of Japan. There was something in common amongst the Nobel prize winners Todd, Adrian, Perutz, Bragg, Crick, Ryle, Hewish and Mott that I was fortunate enough to meet and, in some cases, get to know well. Of course there was the personal and intellectual calibre of these people, their drive and determination. But also, there was the laboratory culture within which they worked, a culture still benefiting from the influence of Maxwell, Thomson and Rutherford.

The Cavendish and the other laboratories were, for experimental science, dependent on increasingly complex instruments, on team effort and increasingly requiring large amounts of funding. Success came from good management of a complex organisation and success was measured in a number of ways — by growth in size, by publication of papers not only read by others but which led others to follow, by awards, by coverage in the popular media, by the training and exporting of skilled scientists to senior posts elsewhere and, perhaps finally but most importantly, by the successful generation of further income.

Some major laboratories, like some major companies, never have been able to produce the quality of papers, discoveries and outputs achieved by others. Some research laboratories, just as some commercial companies, wither and die as a result of an inability to produce results perceived by their ‘customers’ and ‘markets’ as of sufficient quality, relevance, and good value to justify the expense. Some researchers, some laboratories and some commercial companies exude an air of vitality and excitement while others seem dull, listless and declining.

There are strong parallels between good companies producing good products and good scientific laboratories producing good science. This being so, science can undoubtedly benefit from the tools of successful company management.

2. The Product Life Cycle

From the initial decision to proceed with an experiment, the scientist incurs costs ranging from salary and overheads to preparation of grant proposals, to construction of equipment, to preparation of software, to hiring of assistants and so on. Hopefully it is not too long before one begins to see a return on this investment, results which appear in print or are presented to a conference or released as a press release. A period of increasing return for one’s investment soon begins to falter as one realises the deficiencies of the observational technique, as the results are finding increasing competition or as a competitor comes up with a higher quality version of your own observations. Finally, the rate of return for one’s effort becomes absolutely zero. To persist with any further effort merely absorbs resources and the enterprise runs at a loss. Running at a loss leads to bankruptcy and ruin.

So a bright idea which was fleshed out in a few minutes has absorbed significant resources and time to reach observational status. Against all sorts of odds one might press ahead with the experiment. But the over-riding question to be asked is whether the market exists which wants the type and quality of output being produced. Will enough return be generated to not only cover costs but to make a profit?

For if one looks at the cumulative flow of investment and reward in what is a product life cycle and applies it to the case of experimental science, one sees that it is not sufficient in a scientific endeavour to achieve just some results. One must achieve enough results to cover the accumulated investment of resources. Beyond that break-even point one achieves a profit and only through profit will one achieve the concept of growth, visibility, and prestige critical to the continued viability of the enterprise.

But a single narrow area of science, just as a single manufactured product, does not make a consistently successful laboratory or company. A flow of results and products at various stages of their cycles is needed for consistent returns on investment. If, at the same time, one sets targets for growth in profit, it is clear that new topics and experiments must be developed, new markets and customers tapped, or new activities pursued.

This is not really so fanciful. One remembers Rutherford’s advice that the Cavendish should only explore a new discovery until there were several significant figures to the measurement and then move on. This was a deliberate means to maximise return on investment and continue to develop new areas and results.

Another example is the process by which Cambridge quite deliberately withdrew from pulsar observations within several years from their discovery. It was not by accident but as part of a deliberate strategy based on well understood objectives.

3. Objectives and Strategies

The cornerstone of success in manufacturing companies as in scientific laboratories is a clear sense of what one stands for, what one aspires to be, what business one is in. These are the objectives and to achieve objectives, one needs strategies built upon a realistic and careful analysis of one’s own strengths and weaknesses and a careful analysis of the threats and opportunities around one. Continual review of strategies, in conjunction with a development of the objectives is a part of successful management.

A detailed example is the reasoning behind the later Cambridge pulsar observations alluded to above. The major external threat was undoubtedly Jodrell Bank since their instrument had higher sensitivity over a range of radio frequencies and was a steerable rather than transit instrument. This meant that they could observe each source for much longer periods at frequencies where the pulsar phenomena could be measured with more accuracy. Consequently, for all those pulsar observations possible at Cambridge, a relative analysis was made. Cambridge’s performance was calibrated in units of JB, i.e., the performance level possible at Jodrell Bank. It was obvious that there was no point in investing in the development of new experiments unless one had a competitive advantage of at least 2 to 3 JBs or unless one was guaranteed to be able to...
Publish before the opposition. This was no more than part of the strategic management which had made that laboratory so successful. It also reflected the reality that scientific research is not the pure search for truth but is an operation in a cutthroat and competitive marketplace.

4. Product Selection
As has been said already, a new scientific experiment will require significant investment of resources before there is any return on the investment. The internal management of the process of selection, development and marketing of new areas of experimentation makes the difference between success or failure. In modern, successful manufacturing companies the management process is based on inputs from two parts of the external environment. One significant source of ideas is the scientific and technical knowledge available at that time. To base decisions on just this influence leads to science-led or technology-led instruments and results which often fail miserably to have the return on investment perceived for them. There are many radiotelescopes and other scientific instruments developed solely because of their technical or scientific innovation which have failed miserably to give the scientific returns needed to cover the investment.

Of extreme importance is to consider the other main source of ideas from the external environment. This is through market information: ‘What observations and studies will be wanted and appreciated by the marketplace?’ ‘What observations will achieve notoriety or publicity or will lead others to follow?’ ‘What are the key scientific questions?’

The lesson here can be summarised by requiring one not to ask,

This is what I observed, what do you think?

Instead one asks,

What are the unanswered questions that are important to you?
We will find the answers.

So within the organisation, one sees a progression from concept to dissemination of the results with at all stages various reviews and checks based upon both market as well as scientific and technical criteria.

Within a healthy research environment one has a continual list of possible observations being assessed against corporate strategy, resources, strengths and weaknesses, external environment, growth and profit targets, and existing research commitments.

In all cases, however, timing is of critical importance. Too late to market and your competitor shoots you in the foot. Too early to market and you can shoot yourself in the foot.

An important example of being too early to market was the publication of the Cambridge 2C sky survey of radio sources flawed by many instrumental (grating) ambiguities. The survey received much negative publicity and it was this painful lesson that meant that Ryle never again let a flawed product enter the scientific marketplace. It was one reason why the discovery of pulsars was kept secret for 3 or 4 months. Detailed observation had to be done to build up confidence that pulsars were, in fact, real.

5. Laboratory Cultures
There is risk associated with all undertakings, no less so than in scientific observation. As in finance, so in science, it is a fact that to achieve a higher rate of return or profit one has to accept a higher degree of risk.

A cautious investment has a high probability of returning some profit. But investment in a more risky project has a somewhat higher expected profit but has a finite chance of making a loss. It also has a finite chance of making a significantly higher profit.

Judicious management is therefore the skill of offsetting risk through a balanced portfolio of investment.

By far and away the most exciting and famous laboratories are those taking the scientific offensive and which have been able to manage consistently, over a period of time, to stay in the region of short term, newer areas of science with experiments based on new technology. They achieve scientific and market leadership ahead of their competitors. This culture is invariably risky and requires the highest level of scientific expertise. The sheer novelty and excitement of the new science from such groups can often market itself and can achieve enormous returns flowing back to the scientists and laboratory. The members of this group are the Berkeleys, the Stanfords and Cambridges of this world.

In slight contrast are the laboratories on the defensive, not the initiators of an area of science but not wanting to be left behind. They decrease risk and are ready to pounce into the market calling on better resources, better processes and longer time scales to achieve their scientific returns. Many laboratories fall into this category. One good example is the story of pulsars at Jodrell Bank already mentioned. Another could well be the development of the Westerbork radiotelescope in the Netherlands. Originally, this instrument was designed as a large reflector cross-shaped system called the Benelux Cross based very closely on the work of Mills in Australia. That plan was abandoned after the announcement of the 1-Mile synthesis radio telescope at Cambridge. Westerbork became a direct copy of the 1-Mile in size, frequency and baseline. The only difference was that the Dutch invested in 12 dishes rather than only three so speed and sensitivity was better than at Cambridge.

Even worse for Cambridge, the original Dutch plan to use it for spectral line work was abandoned so that the Dutch competed in exactly the same scientific market place with the same observations as the 1-Mile, to the clear annoyance of Ryle in Cambridge. No wonder the 5-km telescope had to be designed and built at Cambridge.

Then there are the imitative laboratories entering late in the scientific product cycle with no aspirations to leap-frog or keep-up. They try to grab some market share with the same sorts of experiments, developing the same sorts of processes, and producing the same-looking results. No wonder such laboratories scrape a meagre marginal income and are very vulnerable to the losses which can lead to closure.

Fourthly, one finds the dependent laboratories without the ability to develop new processes and without the scientific perceptions to see new opportunities. Such laboratories rely on access to the ideas and processes of other, larger and more successful laboratories. Quite often this type of laboratory is found in smaller universities or poorer countries. But even where resources are not a problem one can still find such laboratories, struggling along with no chance of developing that degree of profit needed to grow and develop. The future for such laboratories is very bleak indeed.

The most bleak outlook is reserved for the traditional laboratories. With very long term experiments and with a limited range of scientific interests, these traditional laboratories run all the risks of losing touch with their scientific customers and the external environment. Especially in times of changing external environment such places are almost invariably doomed if they are unable to respond to the change sweeping over them. In Australia the fate of the State Government Observatories...
is well documented. These observatories, persevering in a positional cataloguing task whose initial raison d'être has been overtaken by handheld satellite receivers able to give one's position to within 10 metres, offered little to the State Governments supporting them. They have almost all been closed. All too often the original reasons for the work have been lost in time and only by a dramatic change of market can they survive. For example, the Sydney Observatory has been able to survive and flourish by becoming a part of the scientific museum infrastructure of New South Wales.

Then, finally, there are the opportunists laboratories, fleet of foot and well versed in both scientific and technology skills and well in touch with the scientific market. Ready to pounce on opportunities as they arise, such laboratories sweep up quick returns. Some universities are exceedingly successful at this strategy and in Australia the University of Tasmania has been able to repeatedly gather profit from under the noses of other less alert, less agile, more protected laboratories.

6. The Market Imperative

The concept of a market for science may be a hard concept to grasp. Yet there is undoubtedly a wide range of groups and organisations and individuals who, for their own and distinct reasons, have an interest in the results of experimental science. Identifying those with an interest, and achieving an understanding of the reasons for that interest, are the first steps in marketing. Producing scientific results and outputs in the areas and in the form which match these reasons is what is meant by a marketing strategy. Recognising that reasons for such interest in scientific outputs change with time, and responding to such change, is what makes successful management.

One interesting piece of evidence of changing customer interest in scientific results can be found in statistics of those papers on pulsars which were published in the journal Nature in the months after the discovery paper. An initially explosive growth in papers reached an average of over three papers being published every week. Such a rate could not be sustained and it is now rare to find a paper on pulsars in Nature.

The initial rise in published paper numbers was exponential with the editor initially being prepared to publish papers with an average length of four pages. It did not take long before the novelty of the discovery decreased to the point where the editor could only justify a paper length of less than two pages.

Another aspect of the competition for space between scientific topics was the changing delay between submission and publication of these pulsar papers. The initial novelty of the discovery meant that publication occurred only days after submission (even though the first paper took about 15 days). With time the delay increased to weeks and months — more for papers giving repeat or improved observations of known phenomena such as pulsars and even longer for theoretical papers.

However, when a new pulsar topic emerged, such as short period pulsars (October 1968) or optical flashes from a pulsar (January 1969) or a jump in the period of a pulsar (April 1969) the market interest increased to the point where these papers were published only several days after submission — then again to be followed by an increase in delay.

A similar flurry of interest followed by disinterest occurred in the popular press. The public, just as scientists, thrives on discovery, novelty and controversy. There have been a number of quite famous public astronomical controversies such as the big-bang theories of Ryle versus the continuous creation or steady-state theories of the universe of Bondi, Hoyle and Gold.

Such visibility in the scientific and popular media helps significantly in prestige and funding.

Besides the scientists and public, other 'customers' of science are government and industry. Since government is the significant source of funds for scientific research it would be unwise of any laboratory to ignore their expectations of return on investment. Also, since scientific observation relies increasingly on advanced technology, there is potential application to economic exploitation in industry. This is one market which, in the current climate, can provide returns and potential profits in ways not possible with some of the other scientific markets.

It is the wider dissemination of the results of science on which the future growth of a laboratory depends. There is no justification for a scientist to consume resources to achieve a result which never reaches the light of day.

7. The Importance

The importance of all this is the reality that science is now being done within a competitive market in which all expenditure needs to be justified and priorities need to be set. Australia, like many countries, is finally waking up to the fact that it does not have the wealth to give unquestionable right to scientists and others to pursue their own fancies, free from financial and other responsibility.

One can readily show that astronomy, as one example in science, is an expensive science and that a lot of responsibility has been placed on astronomers to do justice to the generous levels of funding they have received.

This has been a pre-occupation of the author for a long time, ever since undertaking regular one-week observing stints on the Culgoora radioheliograph making detailed maps of solar radio emission at one-second intervals.

Especially during periods of low solar activity at solar minima, a week of observing might produce less than 30 seconds of edited solar activity. On reflection, it could be seen that this instrument had an observing efficiency of less than 1 part in 10,000. That is, adding up the duration of all the solar observations from the instrument which actually formed the basis of a publication of its results came to no more than about 1 hour per year.

This was despite the fact that the instrument would have been useful for study of other areas of science such as interplanetary scintillation or would have been a much greater contributor to galactic and extra-galactic radioastronomy with some extra investment.

Of concern was that the results were published almost always remote from the mainstream scientific journals and that the scientific return on the investment was not being maximised.

Currently, Australia is in the final stages of commissioning the even more expensive radiotelescope system, the Australia Telescope. The importance of doing justice to the investment being made in this instrument cannot be overstated. If astronomy is to have any long-term future in Australia, then a profit must be made.

The Australia Telescope cost about $50 million. An alternative decision could well have been to invest these funds at 15 or more percent interest to give a return of $7.5 million per year. This is what the investment costs and when one adds the annual running cost which must be at least $2.5 million, one sees that astronomers need to justify an investment of over $10 million per year or over $30,000 per day.

An instrument such as the Australia Telescope can achieve about 50% of the time on actual observations. This is based on experience with other such synthesis radiotelescopes where the remaining time is spent on calibration, development,
maintenance or at rest. Also, to make a map with the instrument requires a number of days. An average might be 4 twelve hour periods of observation for one map and so, at 50% observing efficiency, one concludes that it is costing at least $120,000 for the collection of map data. Also, one sees that less than about 100 maps might be made per year from the instrument.

To actually make the map and analyse it will require a lot of time and computing. If one then adds the salary and overhead costs of the astronomer and shares this (perhaps generously) over two maps per year per astronomer, one cannot avoid the conclusion that the total investment by the community in each map is at least $150,000 and the radioastronomer is subsidised by the community by at least $300,000 per year.

Unlike other forms of investment, the capital value of the instrument will not maintain its value, let alone grow. The scientific lifetime of the instrument is limited unless significant further funds are invested in upgrades. This can only increase the investment made and therefore the level of return needed before one reaches break-even and achieves the profit needed for a viable future.

The Australia Telescope and all the other major instruments in experimental science are real and a fact. One can only hope that by understanding the lessons available from commerce, they can represent but one stage in a long and thriving growth in experimental science.