Good research is more than problem solving

Sir

The editorial in the last issue of this journal included the statement that "forestry research seems to have lost its sparkle" and that the March meeting of the Future Forests Research radiata pine theme was a disappointment. The editorial then asked "where was the infectious excitement of previous years?"

My reasoning is that this is a result of how research is now funded where most is directed towards problem solving or service orientated research. Almost no funds are directed to the much more exciting innovation research. When I was in research I was sometimes asked: "what problem are you currently working on". My reply was that we rarely work on problems as most of our effort was directed at opportunities. If there was a problem it was us!

When I left the Forest Research Institute in 1985 I gave a presentation that included my assessment of research (Sutton 1986). Three categories of basic research were identified: problem solving, service orientated or innovation. As both problem and service orientated (e. g. management aids, yield tables, etc) research were often initiated by forest management, funding is less of a problem than for innovation research. In contrast (and even though it may finally result in a quantum improvement for industry), innovation research (which is very often initiated by the scientist and very rarely by management) does not generally attract private funding. Innovation research carries a far greater risk (both to the scientist and the research organization) of not being successful.

As few, even those in both research and research administration, understand the importance of innovation research I included in that 1986 paper a summary of what I regard was the most successful research programme of the twentieth century - viz that emerging from the Bell Telephone Laboratory. The following is the relevant quote from my paper:

"In more recent times the Bell Telephone Company is universally recognized as a leader in innovation research. Surprisingly, very little is written as to why that company - a public utility at that - has been responsible for so much innovation. The best account is given by Peter Drucker in his book "The Effective Executive". The Bell Telephone Company success is largely the result of a strategy implemented about 1910 by the then company chief, Theodore Vail. Vail recognised that a public utility would be unlikely to be innovative. To overcome the difficulty he set up a research organization which had the stated objective of making "…obsolete the present, no matter how profitable and efficient". He created an independent research organization which had to be in constant conflict with current telephone management. Because of constant conflict, financial support for research could not come from telephone management. Vail solved that problem by ensuring a continued high level of funding from an independent source (Ducker 1966).

Vail's strategy resulted in a whole string of successful developments, including the transistor, telecommunications, computing, etc.

Drucker claimed that "even today few businessmen understand that research, to be productive, has to be the 'disorganiser', the creator of a different future and the enemy of today. In most laboratories defensive research aimed at perpetuating today predominates"

A modern day example might be an enzyme scientist asking for sponsorship from the dairy industry. If a case was made to support the funding of research that might greatly reduce (or eliminate) methane emissions from cows there is a high possibility of industry support - an example of problem research. If that same scientist wanted support to research enzymes that could convert grass into milk without the need to pass through a cow it is very very unlikely that the dairy industry would give the research proposal any support - an example of innovative research which might result in a whole new and profitable industry but it would make completely obsolete most of the dairy industry as it is now.

My 20 years in forest research were exciting. Under the direction of Harry Bunn we were encouraged to be bold, to be controversial, to test extremes, etc. We should not be looking for gains of 5 or even 10%: we should be looking for gains of 100 or even 200%! Our talk was about breakthroughs, quantum advances, etc. Yes, it was exciting and we did feel we were changing the world but I doubt if industry would ever have given us support at the beginning of our research. There were occasions when I was phoned by Harry Bunn or Bob Fenton late in the night or at the weekend with a new thought, solution or inspiration they had just had!

Do today's research directors encourage their scientists to be as adventurous as we had been in the 1960s and 1970s? Do they defend their scientists as Harry Bunn did in the past?

It is perhaps not surprising that forestry research has become boring and so relatively unproductive?

References

Ducker, P. 1966. *The Effective Executive.* Pan Business Management.

Sutton, W. R. J. 1986. Management of Applied Research - One scientist's Perspective. *New Zealand Forestry* 31(2):15-18

Wink Sutton