Management of Applied Research — One Scientist's Perspective

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ABSTRACT

New Zealand's innovative silvicultural research was in large part the result of the unselfish and positive leadership of Harry Bunn. He constantly stressed the need to seek quantum gains and to work on tomorrow's opportunities rather than today's problems. Bunn insisted that scientists should only criticise if they had something better to offer.

Success in technology transfer has little to do with the quality of the research. Technology transfer is most likely to succeed where managers perceive that the risk of adopting a new technology is less than staying with the old.

Three orientations of applied research are recognized: Problem, service and innovative. Each has its own characteristics and needs. The gains from innovative research can be considerable but so are the risks, especially to the scientist.

Science always fascinated me. As a child I was intrigued with why things happened, and, as I became older, by the increasing interdependence and cohesion of the sciences. There are no clear boundaries between any of the sciences, and it is impossible to get far in one science without an understanding of many others.

Although in no way mechanically minded, I was fascinated by the way science was used to serve mankind through technology. When I began my research career I genuinely believed that everybody thought like me. I wanted my research to be useful. Even if potential users didn't share my interest in the research, I expected they would see the results of *my* research as I had seen them — as a means to increase efficiency, to do a better job, achieve an objective, to save costs, and so on. That rarely happened.

For most of my career I have not been able to understand why some findings and ideas were accepted while others were not. The only certainty was that acceptance had little to do with the weight of scientific evidence. I believe I now have a better understanding.

Early Days

The present era of silvicultural research at the FRI began in the mid 1960s. At that time our perception was that New Zealand was well behind the efforts of much of Australian and South African research. There were a whole range of seemingly urgent questions: should we prune in two or three lifts to 20 feet (6 metres)? How do we prune the second log? How do we production thin profitably? What is the best initial spacing? Should we blank? Each seemed a simple question. Conclusive answers, we believed, should be possible with some simple, well designed experiments and analysis.

These research opportunities, and similar topics, were referred to as "ripe fruit waiting to be picked" — the inference was "why take on something difficult (like an unripe fruit) when there are all these easier opportunities (the ripe fruit) around". However, only very rarely did the research on these simple questions provide the conclusive answers we had hoped for. Slowly we began to appreciate that everything was interdependent. We could not provide realistic answers to questions on spacing, thinning, pruning etc. unless we knew how all these, and all other factors, interrelated. Nor could we provide answers if we did not know the overall management objectives.

Our research emphasis shifted away from isolated aspects to a study of interdependence. In the early stages this research, rather than help, only seemed to demonstrate the complexity of relationships. Effective resolution, and a high level of management acceptance, did not come until the Radiata Pine Task Force developed the stand simulation model SILMOD.

I now believe that that early research effort was much more successful than it appeared to us at the time. A major factor in that success was the research guidance given by our Divisional Director, E.H. (Harry) Bunn. Harry Bunn was a stimulator and motivator par excellence.

He sometimes had an important direct input into our studies, but his major influence was by his direction and approach to research.

He emphasized a positive approach. The resulting environment was innovative. No-one felt threatened. "Revolutionary" ideas and solutions were encouraged. We really believed we were making "breakthroughs". This was all aided by a virtual absence of budget restrictions. There were very few limits on what we could do.

Harry Bunn was totally unselfish in the giving of ideas. As a junior scientist I soon became aware that I was gaining credit for work which Harry had suggested and initiated. Successful research leadership obviously has a great deal to do with the ability to ask the "right" question.

Harry Bunn has never recorded the principles that guided his research direction and I doubt if he ever will. Those principles, as I see them, are:

1. Work on tomorrow's opportunities not yesterday's problems. Harry believed we should never become too involved in today's management problems. He felt that by the time we have found a solution management would have solved the problem anyway. Far better to concentrate on the questions forest managers would be asking in five or ten years' time.
2. In trials we must concentrate on extremes. Spacing, pruning and thinning trials, for example, must include options far beyond anything management currently considers extreme.
3. If possible avoid early or pre-judgement of results and of management intentions. Even the obvious must be proved. Avoid making value judgements about what will happen. Test instead.
4. Try to concentrate on these ideas and concepts that will provide the largest gains. Concentrate on practices that will result in quantum leaps rather than incremental improvements.
5. Opportunities and solutions sometimes come from quite unexpected sources. "Way out/radical" ideas or solutions should be explored.
6. It is not enough to understand why something happens. We must go on to explore how management can make the most use of that knowledge.

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be explained by considering management's perceived risks. For example, new nursery technology should improve seedling quality. But although nursery management accepts that, they see risks because of possible increased costs (the only criteria on which most nurserymen believe they are judged).

Wide-spaced early pruning and thinning may reduce costs and improve profitability, but also reduce total overall volume yields. Forest management tends to be production, rather than economically orientated, so a risk is perceived if management forgoes some volume production. Other risks were seen because managers (incorrectly) believed that alternative practices reduced, rather than increased, management flexibility.

Clearly the key to technology transfer lies in changing management's perception of risk. Successful technology transfer is only achieved when the researcher develops strategies which show the risks of not adopting a new technology are greater than those of adopting it.

Differences in the orientation of applied research

The problems I experienced in technology transfer led me to think about differences in applied research. I have always been puzzled when people often asked me: "What problem are you working on?" I very rarely worked on problems — where possible I worked only on opportunities. If there was a problem, that problem was me — not my work!

But there were researchers who did work on problems. They seemed to have support from management, and to get their research findings applied.

Another research area also enjoyed a fair level of management support although they were not working on well defined problems. Nor were they working on alternative management systems. Their work was service orientated.

These considerations have led me to believe that there are three different orientations of applied research. Although the boundaries between them are not always distinct, there are fundamental and important aspects which are different for each category. My three categories and their definitions are:

> **Problem orientated research**
> Research into a relatively easily defined problem (for example, forest health, trees on a poor soil, inability to harvest trees on a steep broken slope).
>
> **Service orientated research**
> Research designed to help management do their job better (for example, yield tables, optimum spraying techniques).
>
> **Innovative research**
> Research into new systems and ways of doing things (for example, nursery and planting systems, alternative silvicultural systems).

These three research orientations have quite different characteristics, and I believe it is very important to consider them and to consider what the further implications might be. Table 1 summarizes these.

Problem orientated research is usually initiated by management; service orientated research can originate either from management or from the scientist; innovative research is almost invariably initiated by the researchers. The source of initiation has major implications for the level of management support, and for eventual management acceptance. There are also implications to scientific risk and for the risk to the scientist involved.

With problem orientated research management support will usually be high and results relatively easily accepted. The risk to the scientist, even if the work largely fails, will be low. With service orientated research, management will usually provide support and accept results. Scientific risks are generally low but could be important if the research is not successful. Management support for innovative research is at best minimal and management is likely to resist acceptance. The scientific risk is high. This is not only because the research itself almost invariably has fewer chances of success, but also because a great deal of effort may be required to win management acceptance. Advocacy carries its own risks.

TABLE 1

Summary of Research Orientation/Scientist/ Management Interactions

Research Orientaton	Most Likely Source of Initiation	Expected Level of Management Support	Ease of Management Acceptance	Relative Level of Scientific Risk
Problem	Management	Very High	Relatively easy	Low
Service	Management or Scientist	High	Few Problems	Low/ med
Innovative	Scientist	Low	Difficult	High

It is clear that although innovative research may offer lots of promise it also carries the greatest risk to the scientist. I believe it may be increasingly difficult for FRI to do innovative research. If research budgets become tighter, there could be less willingness by research management to fund "risk projects". Scientists may also become increasingly reluctant to undertake work involving risks.

The paradox is that the future success of forestry, and the forest industry in New Zealand, largely depends on innovation.

Because of the risks, promotion systems for researchers should give due regard to the degree of innovation involved. Innovative researchers, since they are concerned with change, are also the most likely to be controversial. It might well be argued that if they are not controversial perhaps they are not doing their job well.

I believe it is of paramount importance that management not be allowed to have a dominant influence in determining the research priorities of innovative research. It must never be forgotten that in the early days of at least two key areas of innovative silvicultural research (the experimentation with wide spacing and heavy early pruning and thinning to low final crop stockings and with agroforestry) this work was vigorously and actively opposed by management at all levels, including the most senior. Had we not had the research direction that encouraged innovation and resisted management influence, that research would never have reached the level of development it has. It may never have even begun.

Similar opposition in the key area of sawmilling research had been experienced more recently.

Throughout the world forestry is not noted for innovation. The reasons are many. They include the relative abundance of wood and the very long rotation needed for most of the world's timber-producing trees. Forest research investment has been minimal. In most countries it is so poorly funded that there is not enough money for the important problem and service orientated research. In most countries research is totally, or very largely, government funded. Research is not given much freedom. It is so dominated by forest management that innovative research is virtually impossible. The FRI is probably unique in its degree of independence from management.

The problems of the innovator are not new. In the early 16th century Machiavelli wrote:

> "There is nothing more difficult to take in hand, more perilous to conduct, or more uncertain in its success, than to take the lead in the introduc-

tion of a new order of things, because the innovator has for enemies all those who have done well under the old conditions, and lukewarm defenders in those who may do well under the new."

In more recent times the Bell Telephone Company is universally recognized as a leader in innovative 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's success is largely the result of a strategy implemented about 1910 by the then company chief, Theodore Vail. Vail recognized 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 the current telephone management. Because of the contrived 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. [Drucker, 1966]

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

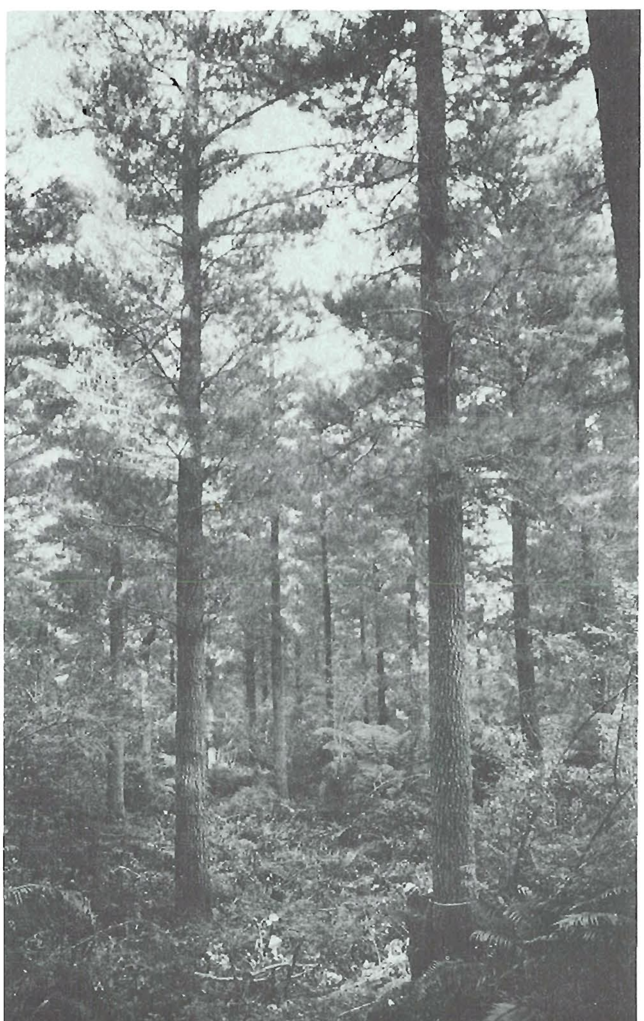
Drucker claimed that "even today few businessmen understand that research, to be productive, has to be the 'disorganiser', the creator of a different future and the enemy of today. In most laboratories defensive research aimed at perpetuating today predominates".

I believe the analogy we have used about "picking the ripe fruit" is too simplistic. In silvicultural research we did eventually harvest, and I hope will continue to harvest, rich rewards. However, in the end that harvest was only possible because we carefully nurtured and monitored our "fruit trees" to maturity. Unless you steal someone else's fruit the "ripe fruit" analogy may prove to be an illusion.

I would like to believe that had we had a better understanding of these philosophical aspects of applied research and technology transfer, we could have been even more successful, and less frustrated, than we were.

I offer these comments in the hope that you may learn a little from my 20 enjoyable and exciting years in research.

It is vital that innovative research is encouraged and extended. Defensive research must be avoided. The guiding philosophy of Harry Bunn must continue to be built upon. Innovation is the key to FRI's future and that of NZ forestry.



Pinus radiata **aged 26 years with a final crop stocking of 110 stems per hectare and pruned to 6m. Mean dbh is 65cm. An extreme accidental treatment in Mohaka Forest. (Photo: D.J. Mead)**

Acknowledgements

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