Short communication

Achieving ‘step changes’ in science and innovation: Towards ‘Pasteur’s paddock’?

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The Request for Proposals for New Zealand’s National Science Challenges (NSCs) emphasises that successful proposals should ‘represent a significant step change in undertaking research and delivering impact’ (MBIE 2014). How can a ‘step change’ be achieved within NSCs where new funding is small compared to realigned funding? In a video released on 4 February 2014, the Minister for Science and Innovation suggests an expectation that ‘additionality’ will play a key role, with ‘collaborative’ and ‘multidisciplinary’ endeavour as important components of these ‘mission-led’ Challenges (Joyce 2014). This brief communication reviews and describes a timely synthesis of two important components of the science and innovation literature relevant to the ‘step change’ and ‘additionality’ expectations in NSCs.

Figure 1A shows the *Diffusion of Innovations* model (Rogers 2003). The model originated from data on the adoption of hybrid corn in Iowa in the 1930s and 40s, and Roger’s book evolved through five editions, beginning in 1962, to define the process and importance of communication in successful innovation. In an obituary inserted in a chapter published posthumously, *Diffusion of Innovations* was described as the second most cited book in the social sciences (Rogers *et al.* 2009). Google Scholar currently attributes 55,246 citations.

The diffusion of innovations model in Figure 1A describes an S-curve of cumulative success – a step change commonly measured in the level of adoption. The contributing rate of change more clearly depicts the push or impulse of adoption (Figure 1B), which represents a sequence from more cosmopolitan early adopters to less-connected late adopters. Mathematically, various forms describing an S-curve have been used, but a cumulative normal distribution (Figure 1A), and its derivative (Figure 1B), describe how a push of activity leads to change, in a manner familiar to most scientists and identical to the physics of diffusion (Crank 1975).

Rogers (2003) describes many nuances that can be related to the model in Figure 1. For instance, up to a dozen events can be identified as contributing to successful innovation, and Figure 1B shows how a series of pushes may contribute to overall change. In addition, adoption is the critical step often measured as a signal of successful innovation. However, a focus on adoption may fail to identify successful reinvention by users during implementation that greatly expands the magnitude of the innovation’s eventual impact. And while users may play an essential role in reinvention, science and technology normally have an important role in the generation of innovations.

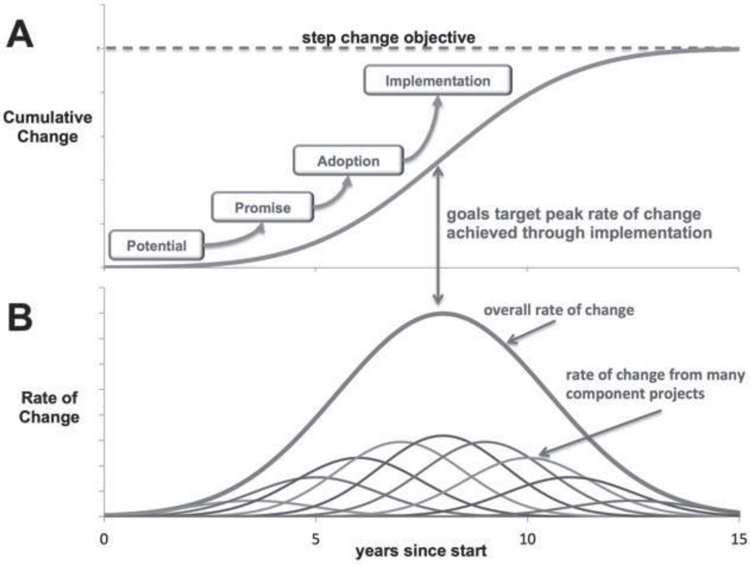

Figure 1. Adaptation of the *Diffusion of Innovations* model from Rogers (2003) to achieve a ‘step change’ in mission-led research. Cumulative change to a step change objective (a) can be related to the rate of change (b), which itself may be an overall push (impulse) composed of many smaller pushes.

When contributing events from scientific discovery and integration to user-driven implementation align well, collaboration and multidisciplinary linkages will successfully yield ‘additionality’, a multiplier effect on overall success that would not have occurred otherwise. The mission-led nature of NSCs implies that investment should be targeted toward efforts that maximise the likely magnitude of the individual and combined

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pushes in Figure 1B. The sequence in Figure 1A shows that, at initial stages, this involves incentivising many collaborative efforts involving discovery and integration based on potential – using probability-driven frameworks. As potential transforms into promise, work can be more precisely planned and targeted to optimise the timing, sequence and magnitude of adoption and implementation, resulting in an overall impact or step change.

Importantly, the relationship between the cumulative function (Figure 1A) and the push function (Figure 1B) suggests that it will be more useful for stated programmatic goals to target the maximum rate of change in Figure 1B, rather than the eventual level of cumulative step change in Figure 1A. In addition to being a more direct measure of change, goals associated with maximum rates of change will occur during the timeframe of the programme, which in the case of NSCs is 10 years. In contrast, cumulative impacts should extend beyond this planning timeframe, but will include a potentially asymmetric segment of the S-curve, which will be difficult for research organisations or government to control.

Thus, the model in Figure 1A and B provides a useful and tested framework for describing and managing a ‘step change’. But the body of work on the diffusion of innovations offers only broad generalisations; it does not describe a ‘silver bullet’ directing how innovation can be enhanced and ‘additionality’ achieved. To examine this point, it is useful to first clarify that additionality, as used here, represents a multiplier effect, as opposed to an additive effect capable of being separated from a baseline. In support of this type of additionality goal, Hendy & Callaghan (2013) suggest that larger and more successful centres of innovation often have such a multiplier. Identifying ways to increase the multiplier in selected areas of the New Zealand science system is therefore a sensible goal for science and innovation policy.

A strong yet simple prospect for enhancing additionality through NSCs emerges from efforts to dissect the lessons of science and technology policy in the USA 50 years after Vannevar Bush, the wartime head of the US Office of Scientific Research and Development, launched a 1945 blueprint for what would become the National Science Foundation (NSF) in 1950. Stokes (1997) argues that NSF’s proud insistence on funding ‘basic research’ originates from a wartime experience that led Bush to declare, ‘applied research invariably drives out the pure.’ This experience was articulated by J Robert Oppenheimer in the statement:

> *…the things we learned [during World War II] are not very important. The real things were learned in 1890 and 1905 and 1920, in every year leading up to the war, and we took this tree with a lot of ripe fruit on it and shook it hard and out came radar and atomic bombs…. The whole spirit was one of frantic and rather ruthless exploitation of the known; it was not that of the sober, modest attempt to penetrate the unknown.*

Such a view held power and immediacy in the post-war period, with the nascent cold war with the Soviet Union looming. However, Stokes’ (1997) analysis of recent decades clarifies that a sole focus on funding ‘basic research’ is a poor option in a globalised world of tight government budgets. Strong evidence emerged from the Japanese ability to commercially apply and exploit fundamental understanding of science and technology discovered in the USA. In the USA itself, the mission-led funding model at the National Institutes of Health has demonstrated greater promise than the NSF in growing both its allocation of government funding, and the health sector in the US economy. Stokes therefore argues, based on contemporary experience, that any democratic society will demand accountability for investment in science and technology, and the best policy choice for funding should target the dual payoff of both application and fundamental understanding.

In doing so, Stokes (1997) compels us to understand that the widely used differentiation between basic and applied research is a false choice rooted historically in Vannevar Bush’s design of the NSF. In contrast to the Frascati Manual’s (OECD 2002) differentiation of ‘basic’ and ‘applied’ research, and the related fallacy that technology and innovation always emanate sequentially from basic to applied research and then development, Stokes argues that there are two separate classifications: (1) is there a drive toward fundamental understanding; and (2) is there a consideration of use? Mapping these classifications on two axes to define four quadrants, we can see that Niels Bohr and Thomas Edison occupy quite separate regions, the former focused solely on fundamental understanding and the latter on immediate application (Figure 2). But other great scientists have succeeded by combining the quest for fundamental knowledge

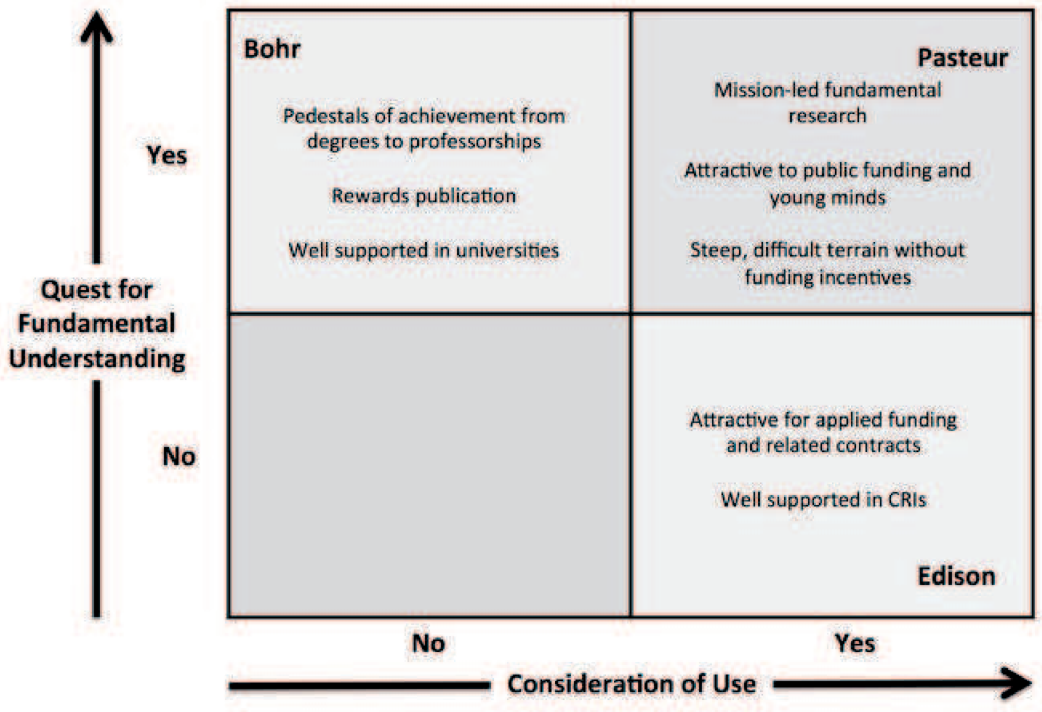

Figure 2. The two-dimensional classification of research motivation (Stokes 1997), superimposed on the New Zealand research landscape, where we might refer to the upper right dual-purpose region as ‘Pasteur’s paddock’.

with questions of practical uses. Stokes chooses Louis Pasteur as most exemplary for greatly advancing microbiology and biochemistry driven by practical applications in health and food chemistry. Stokes also cites Irving Langmuir's surface chemistry and John Maynard Keynes' macroeconomic theories as classic advances driven by needs, and New Zealanders may equally wish to include Paul Callaghan's application of physics to materials.

If we view the two-dimensional research classification derived from Stokes (1997) superimposed on the New Zealand research landscape, we can imagine two familiar paddocks that are well occupied (Figure 2). The universities and Marsden Fund strive to occupy the paddock devoted to fundamental understanding, while the Crown research institutes (CRIs) dominate the zone of application and use. The most logical path between or extending from the well-occupied paddocks enters 'Pasteur's paddock' – the joint zone of fundamental understanding and consideration of use. Mission-led research in Pasteur's paddock therefore seems a very logical investment to maximise both short- and long-run gain. Yet in the present landscape, Pasteur's paddock remains a rougher, more challenging and more diverse terrain occupied mainly by those with sufficient excellence or motivation to extend into it from the adjacent zones.

Several factors in the New Zealand research landscape appear to either limit or enhance activity in Pasteur's paddock. Foremost limiting factors are the institutional tendency of universities to focus on degrees and publications, and that of CRIs to work to a funding system favouring immediate application and user relationships generating further contracts. In a system where funding and competition are tight, the extra work to cater to multiple purposes will tend to falter unless incentivised to create valuable, but often unpredictable, spinoff and spillover benefits*. Perhaps we owe a better system to many of our best scientists, who chose their careers to pursue a joint focus on practical applications and fundamental understanding, and still aim to do so. Indeed, use-inspired fundamental research also serves as motivation for our best young minds to choose careers in science, technology and engineering that will encourage them to stay in New Zealand and improve our society, economy and environment (Leibfarth 2013).

Within our present funding system, small individual research fellowships linked with large mission-led ventures appear to hold considerable promise for clearly and simply making the most of research investment by advancing both fundamental understanding and practical applications. In this respect, the university-led Centres of Research Excellence (CoREs) already have a strong record of achievement, and are often notable for linking excellence in CRIs and universities. The NSCs have their foundation in the CRI Taskforce report's (Jordan *et al*. 2010) promotion of 'inter-institutional collaboration' and 'multi-disciplinary areas of research' such as CoREs through Recommendation 9, which stated:

> *The CRI Taskforce recommends that Government include, as part of its open access investment programme, funding to support inter-institutional, collaborative research. This should be managed by nominated research directors from within research organisations across the RS&T system, including universities. This funding can be awarded through negotiation or contest.*

Achieving Recommendation 9 within the NSC context will benefit from combining the models presented in Figures 1 and 2 to direct the structures and incentives within new and aligned research. Seeking excellence in both fundamental understanding and considerations of use will tend to generate additionality in the form of a multiplier effect, and therefore enable multiple coordinated pushes that generate a step change. The cross-fertilisation of ideas that comes with the discovery and integration steps of a mission-led quest for fundamental understanding has substantial potential. It is likely to be the most effective means to access and translate global advances in basic understanding to our nation's practical needs. Our national strengths in the management of applied science can be brought to bear as the research moves along the path from potential to promise, and on to adoption and implementation. In doing so, we may hope to move beyond bickering about applied versus basic allocations. Indeed, we may hope to rebuild a virtuous and inspiring compact between science, society and government within New Zealand, that lives up to the naming of the National Science Challenges, even if there are some missteps along the way.

Acknowledgments

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* A spillover benefit is when someone benefits from something without directly paying for that benefit. [Editor]