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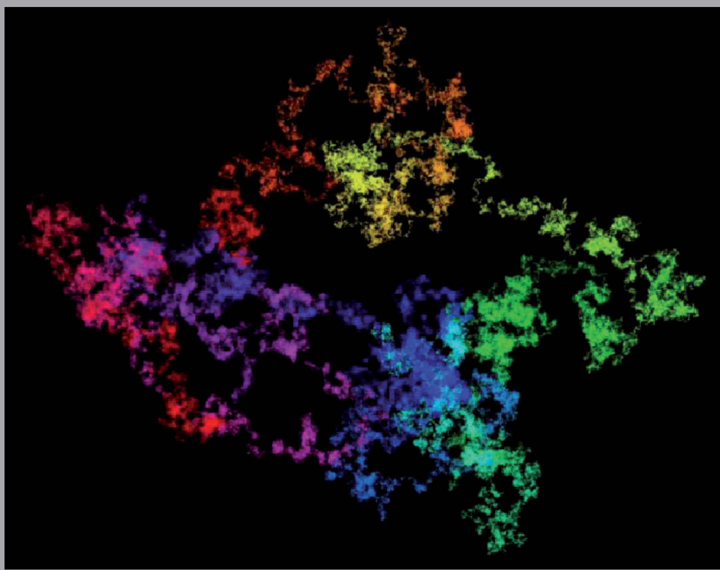

Pi Day

Research on introduced wildlife

The Austronesian Diaspora

Book reviews

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A forum for the exchange of views on science and science policy

Editor: Allen Petrey
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Contents

Cover: Random walk on pi (base 4) to 100 billion digits. From paper by Humble on page 99 of this issue.

Instructions to Authors

New Zealand Science Review provides a forum for the discussion of science policy. It also covers science education, science planning, and freedom of information. It is aimed at scientists, decision makers, and the interested public. Readability and absence of jargon are essential.

Manuscripts on the above topics are welcome, and should be emailed to the editor (editor@scientists.org.nz).

As well as full papers, short contributions, reports on new developments and conferences, and reviews of books, all in the general areas of interest detailed above, are invited. The journal may also accept reviews of a general nature and research reports.

Full manuscripts (with author's name removed) will be sent for peer review, and authors will be sent copies of the reviewer's comments and a decision on publication. Manuscripts should not normally have appeared in print elsewhere, but already published results discussed in the different, special context of the journal will be considered.

Manuscripts should be accompanied by biographies of not more than 100 words on each author's personal history and current interests. Authors are also expected to supply a suitable high-definition passport-size photograph of themselves. This will be published with the article.

Articles may be submitted in MS Office Word, rich text format, or plain text. Diagrams and photographs should be on separate files (preferably eps, tif, jpg, at 300 dpi), not embedded in the text.

All tables and illustrations should be numbered separately – Tables 1, 2, 3, 4, etc., and Figures 1, 2, 3, 4, etc. – and be referred to in the text. Footnotes should be eliminated as far as possible. Diagrams and photographs will be printed in black and white, so symbols should be readily distinguishable without colour, and hatching should be used rather than block shading. However, colour may be used if the author or the author's institute is willing to pay for the added cost.

References should preferably be cited by the author–date (Harvard) system as described in the Lincoln University Press *Write Edit Print: Style Manual for Aotearoa New Zealand* (1997), which is also used as the standard for other editorial conventions. This system entails citing each author's surname and the year of publication in the text and an alphabetical listing of all authors cited at the end. Alternative systems may be acceptable provided that they are used accurately and consistently.

In this issue

In his article, *Scientific research on introduced wildlife in New Zealand: For whom and at what cost?* Jamie Steer traces a short history of natural science research on introduced wildlife in New Zealand.

Using a critical discourse approach grounded in a social constructionist perspective, he argues that maintenance of public trust in New Zealand natural scientists will require: (1) more consistent self-reflection to identify and communicate the assumptions and predispositions of their research, and (2) more active endorsement and support for research that investigates the questions that are not considered useful to ask.

He uses a series of case histories to explore what has happened within this perspective over time in relation to introduced mallard ducks, deer, and trout.

In *How a farm boy from Wales gave the world pi* and *Pi might look random but it's full of hidden patterns*, Gareth Ffowc Roberts and Steve Humble, respectively, give us fascinating insights into one of the most important numbers in mathematics. Roberts acquaints us with the 18th century autodidactic mathematician William Jones, who gave us the Geek letter π or 'pi' for this number, and Humble points out that, despite an endless string of unpredictable digits that make up 'pi', it is not truly a random number – and that it actually contains all sorts of surprising patterns.

There a plethora of book reviews in this issue. Three are reviews from Bridget Williams recently introduced *BWB Texts* series – short books on big subjects. In the first, Nobel Laureate Peter Doherty reviews Mike Berridge's *The Edge of Life: Controversies and challenges in human health,* and Veronika Meduna's *Towards a Warmer World: What climate change will mean to New Zealand's future* and Ralph Chapman's *Time of Useful Consciousness: Acting urgently on climate change* are then reviewed by professional science editor Geoff Gregory.

In his extended review of editors Robert Geyer's and Paul Cairney's *Handbook on Complexity and Public Policy,* Paul Gandar examines their attempt *to improve the theory and practice of policymaking by drawing on the theory, concepts, tools and metaphors of complexity and to advance 'complexity thinking' as a means for understanding and explaining the policymaking world and as a basis for policy development.* These aims, pursued through 482 pages in 27 chapters, with a total of 40 authors, are critically examined and appraised by Paul. This is a must-read review from a reviewer who has an extensive front-line experience in public policy development as well as the natural sciences.

Finally in this issue, details are given of the 2016 NZAS annual conference *The Future for Scientists in New Zealand*, to be held Tuesday 26 April 2016 at Te Papa Tongarewa, Wellington (see inside of back cover).

Allen Petrey
Editor

President's column

The Association has reached in its 75th year! Consider the changes in human society, both within New Zealand and globally in that time, the changes to our environment and ecosystems, the global conflicts, the adventure into space and more besides. It has been a period like no other in history – and science has been a big part of this. Looking forward, continued change seems to be the only foreseeable constant.

This is at the heart of our 75th Anniversary Conference, being held at Te Papa right around the time of publication of this issue of *New Zealand Science Review*. With planning led by Nicola Gaston and myself, the conference runs with a dual theme of looking at what the Association has achieved to date, and then turning to face the future. Speakers will address topics ranging from the history of the Association and New Zealand science, through to challenges facing various sectors of the community now and in the future. The conference wraps with a discussion about how we can do our best for the next generation of scientists. This relies on us, the system, providing a supportive environment for the next generation of leading scientists to grow and evolve in. The challenges facing early-career researchers in all facets, be they funding, diversity, or career path, are ours to share. This strikes at the core purpose of the Association: to be about the role of 'The Scientist' in the past, present and future.

While we've been planning the conference there's been a disturbing example across the Tasman of the battle being faced by 'for the public' science everywhere. Around the start of the year, CSIRO, Australia's government science agency, announced it would effectively stop climate prediction science. Arguing that the case was closed and now it was time to get on with working out how to respond, CSIRO announced wholesale culling of internationally high-profile scientists and science. The perversity of this Australian move so soon after the Paris Climate Agreement is well-documented elsewhere[1]. From the climate perspective, this shift has substantial implications for New Zealand, as Australian climate science not only is a key player globally, but even more so in the Southern Hemisphere. The incredulous condemnation has been swift and global in scale. We wait to see if this outcry has any impact on the decision.

It is worth considering how the Australian shock-treatment throws some light on the New Zealand 'for the public' science system. In dynamics there is such a thing as 'impulse response', whereby you improve your understanding of a system by giving it a sharp perturbation. This sometimes shows up behaviour where analysis of the response to gradually varying forcing is not so clear. It is worth noting that, when scaled by population or science proportional investment, recent events in the New Zealand science scene (recent staff losses at AgResearch, Landcare and Callaghan Innovation) would be greater than the CSIRO losses presently creating international condemnation.

It seems that this was an Easter Egg (not the chocolate kind; for those of you without access to someone under 25, look it up!) left by the recently departed Abbott government looking to drive *all* science with entrepreneurial values. One of the key claims made as the cuts were announced was that the initiative was numbers-neutral – there would be the same number of employees after as before. This in itself is a clear reflection of not understanding how science works. Scientists are HR pieces to be chopped and changed, started and stopped? It simply doesn't work like this. Generating, evaluating and propagating new knowledge takes time and continued investment. One thing is certain, there is nothing about predicted climate, population and ecosystem metrics that indicates enhanced ignorance is the way forward.

So what to do about it? Come to the conference, talk to people on the NZAS Council, drop me an email (president@scientists.org.nz), and use the Association membership as a gathering of colleagues. I know it is hard to think beyond that next manuscript, report, class, field-trip, proposal(s), etc., but try and use the Association. It is also worth mentioning that we have announced our annual Awards, to be held at the Royal Society of New Zealand in Wellington in early September. Put on your thinking caps and nominate yourself or a colleague who you think is bringing, or has brought, something to New Zealand science.

And use the *New Zealand Science Review*. It provides a unique outlet for refereed analysis and discussion of science and science policy issues. Better still, consider submitting an article. This issue looks at research into introduced species in New Zealand, as well as some book reviews showing that writing about science in New Zealand is very healthy indeed. As ever, I acknowledge the continued work of everyone on the Council, and all the members who helped out with other roles. I look forward to a constructive and energising 75th year for the Association.

Craig Stevens
President

1 http://www.nature.com/news/job-cuts-in-australia-target-climate-scientists-1.19313

Article

Scientific research on introduced wildlife in New Zealand: For whom and at what cost?

Jamie Steer*
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In his keynote address to the 2015 New Zealand Association of Scientists Annual Conference, Professor Peter Gluckman offered a timely reminder to New Zealand scientists: that their legitimacy within public discourse and decision making is ultimately founded in integrity and trust (Gluckman 2015). Reflecting on the growth of new information and media sources in the last few decades, and the erosion in trust in the more established ones, Professor Gluckman urged scientists to adhere to the features that distinguish science from other epistemologies. He also asked New Zealand scientists to consistently recognise and acknowledge the limits of scientific knowledge and the unhelpful biases that can creep into its production (also see Gaston 2015).

With these thoughts firmly in mind, this article traces a short history of natural science research on introduced wildlife in New Zealand. Through this history, I argue that a maintenance of public trust in New Zealand natural scientists will require (1) more consistent self-reflection to identify and communicate the assumptions and predispositions of their research and (2) more active endorsement and support for research that investigates the questions that are not considered useful to ask. I argue that if these points are not considered and adequately addressed, New Zealand's natural scientists risk substantiating the claim that their science is compromised by undisclosed value frameworks or that their analyses are simply another tool being used to further the agendas of their employers.

To narrow what might otherwise prove a formidably broad topic, I focus my history on a subset of introduced game species – specifically mallard ducks (*Anas platyrhynchos*), deer and trout (various species). I employed a critical discourse approach (Phillips & Hardy 2002) to interviewing and documentary research, grounding this approach in a broad social constructionist perspective (Burr 2003). For a full explanation of my methodology and information sources refer to Steer (2015). I do not intend this analysis to be comprehensive. It is offered merely as a contribution to the discussion of how the New Zealand natural science community, in particular, might choose to refine their practice and thereby maintain a credible seat at the decision-making table.

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Scientists' support for introductions

While Polynesian colonists introduced species to New Zealand from as early as the 13th century (Walrond 2012), the most systematic and large-scale efforts at acclimatisation occurred from the mid-19th century with the establishment of Acclimatisation Societies around the country and the importation of dozens of new species each year (McLeod 2007). Thomson (1922, p. 2) lamented that this acclimatisation effort had been 'carried out in the most haphazard and irresponsible manner [with] districts, societies and individuals acting quite independent of, and often in direct opposition to, one another.' He characterised the history of acclimatisation in New Zealand as a series of 'bungles and blunders' undertaken 'with zeal unfettered by scientific knowledge' (*Ibid.*, pp. 3, 22). Most subsequent histories of New Zealand acclimatisation have been no more complimentary (e.g. Aramakutu 1997; Walrond 2012).

According to Davies (1996), acclimatisation was undertaken with little thought or analysis, and it is clear that there *was* little effective coordination of introductions. Failed initial introductions were often followed by multiple subsequent liberations, without regard for overall rationale (Walrond 2012). A common perception was that if the conditions suited the introduced species, they would thrive, and if not they would perish (see McDowall 1980). The lives of the animals themselves were largely immaterial. Much like contemporary ecological restoration initiatives, concern was for populations, with individuals routinely sacrificed for the common purpose.

Although questions may remain over the *interpretation* of science, suggestions that early acclimatisation efforts in New Zealand had *disregarded* science are probably inaccurate. As Sullivan (1990, p. 311) wrote, while:

Jamie Steer recently completed his PhD in Environmental Science at the University of Auckland. The thesis explored understandings of introduced species in New Zealand in the context of biodiversity management, arguing for a more reconciliatory approach to their history and fate in the country.

Dr Steer has a MSc in ecology from Victoria University of Wellington and has published studies on behavioural ecology and bioacoustics. He has worked in a range of different roles in the environmental services industry, including as an ecologist for an environmental design consultancy. He currently works as a Senior Advisor for the Biodiversity Department at Greater Wellington Regional Council.

> *...most historians today decry the lack of knowledge and the woeful ignorance of the early Acclimatisation Societies...they contained many men with scientific or background education in the related disciplines and who were conversant with the most up to date findings about them.*

In fact, there is little evidence that early acclimatisers acted in opposition or disregard of scientists. Rather, many Acclimatisation Societies included prominent scientists among their foremost members (Star 1997). Despite this, few scientists in New Zealand had considered the consequences of acclimatisation in the early to mid-19th century. In part, this was because there were few scientists in New Zealand at the time to consider the matter. Moreover, those that *were* present, and disposed to studying the biota, were predominately natural philosophers, schooled in the romantic descriptive tradition of Gilbert White (1977 [1789]). They were hardly 'wildlife managers' and with the bulk of arrivals after 1840, they barely had time to begin describing the local biota, anyway, let alone commenting on or analysing the ramifications of introductions.

The view of the scientists that *had* considered acclimatisation, however, was generally supportive. They were guided, in this respect, by the 'displacement' theory that had been introduced to them by Charles Lyell (1830). This theory taught that, under colonisation from 'new and more vigorous' European forms, native species were impermanent features in New Zealand (Gillies 1877, p. 306). The 'displacement' of native species with British and Continental species was inevitable: 'Just as the Māori would be replaced by 'Pakeha,' so too would native birds be replaced by stronger northern hemisphere avifauna' (Aramakutu 1997, p. 70). According to Charles Darwin's (2009 [1859]) theory of evolution, only the 'fittest' would survive. As native species declined in the face of European expansion in New Zealand it was 'obvious' that they were inferior to European species and that their extinction was 'inevitable' (Galbreath 1993). For Gillies (1877, p. 306) 'ancient races' would 'pass away' as naturally as a geriatric on their death bed. Assistance for native species was thus constructed as 'hopeless,' a conception accepted by prominent New Zealand scientists of the day such as James Hector, Julius von Haast and Frederick Hutton, in addition to 'almost every other scientist in the colony' (Star 1997, p. 114). The accepted scientific theory of the day thus only provided justification for the work of colonists. As scientists in New Zealand mostly saw it, their role was to document the 'natural' process of displacement, not to hinder it. The remaining natives could not be saved and should instead be catalogued in museums for posterity (see Martin 1885). Indeed, this was scientists' 'sacred duty' (Moncrieff 1949, p. 4). The appropriate focus at the time, however, was on the future of the biota, and this was clearly to be a European-dominated one.

The pendulum swings against introductions

From around the 1870s, however, some scientists began to entertain a revised view of the concept of 'displacement' in New Zealand. It was becoming increasingly evident, for instance, that there was no universal biological imperative guiding the replacement of natives with exotics. Many native species were, in fact, surviving and in some cases flourishing. As a result, by 1890 'extinction was now seen not as the result of immutable scientific law but as the result of more mutable human practice' (Star 1997, p. 244). Just as introduced species had prospered under a raft of protective measures, it was discovered that native species could survive too, should similar measures be directed in their favour. This theoretical revision coincided with a growing awareness of the worth of native species. It was not *just* introduced game animals that lured international tourists to New Zealand, for instance, but also the unique native-dominated scenery they encountered while pursuing them. The creation of parks and reserves in New Zealand around this time was initially dominated, not by nature conservation, but by the identification of forests and mountainscapes as tourist resources for development (Coombes 2003; Star 1997). The rejection of displacement was thus not only scientifically justifiable but also increasingly useful.

The scientific consensus in the late 19th century began to move in favour of a more cautious approach to introductions and an emphasis on the conservation of native species. The introduction of mustelids in the 1870s and 1880s, for example, was opposed by the majority of scientists in New Zealand who, by then, foresaw the effects of such taxa on native birds (Young 2004). Nevertheless, very little scientific work was undertaken to actually quantify the effect of introduced species on native species in the 19th century. Early fisheries scientists, for instance, devoted most of their efforts to documenting the success of trout liberations, rarely making observations on native freshwater fish (Crowl *et al.* 1992). In 1895 a paper in the *Transactions of the New Zealand Institute* concluded that 'there is no evidence to show that the few native freshwater fishes have suffered from the introduction of…the trout' (Kirk 1895, p. 7). This 'lack of evidence' was widely taken as an indication that there had been no effects of trout on native freshwater fish (e.g. see Hamilton 1904). It was, however, merely descriptive of the state of scientific knowledge in this area. Indeed, a scientific understanding of most freshwater species in New Zealand was severely limited at this stage (McDowall 1991). What *is* clear is that research into the effects of trout on native fish at this time was not considered useful. As the president of the New Zealand Institute, P. Marshall wrote, 'it is natural and inevitable that in this country research should tend to be centred…round those industries upon which the prosperity of the country depends' (Marshall 1926, p. 1). Aside from taxonomic work, most wildlife science in New Zealand, until well into the 20th century, was thus focused on how to grow and release game species, largely because they were one of the primary sources of revenue in the country (Bathgate 1897).

Wildlife management as a scientific discipline in New Zealand did not develop until the mid-1930s (Westerskov 1957). At that time, it was focused on 'the development of natural resources for the benefit of mankind,' rather than the advancement of scientific knowledge *per se* (*Forest & Bird* 1937, p. 15). Again, the emphasis was on the health of game populations, often at the expense of native species. This focus on 'natural resources,' moreover, necessitated the destruction of 'those parts of Nature – and they are in the majority – which are not of immediate use for economic ends' (*Ibid.*, p. 15). Nonetheless, even for game species that were *relatively* well studied, there remained significant deficiencies in knowledge. Pellett (1935), for instance, noted that attempts to apply scientific knowledge to the propagation of trout was limited by a dearth of scientific literature on the topic in New Zealand. He lamented that '…there has been almost no knowledge of what to do, and what results might be expected, from any effort expended' (*Ibid.*, p. 11). This was, in part, because of an enduring reluctance 'to accept the word of the trained research man if it conflicted with the general opinion or with general observations' (Lumley 1937, p. 4). For example, it was not until 1939 that the Department of Internal Affairs appointed a fisheries biologist to work in Rotorua, one of the most important fisheries in the country. The appointed scientist, a woman by the name of A.L.K. Welch, was not taken seriously and her superior did not think that scientific research was necessary (Galbreath 1993).

A 1941 editorial in *Forest & Bird* entitled 'Research – An Urgent Need' argued that research on wildlife in New Zealand, in general, remained in its infancy: 'In New Zealand it has always been a practice to make decisions on wildlife matters without expert research and biological knowledge' (Editor 1941, p. 1). Study continued to be directed almost exclusively to those species that were deemed most useful. Indeed, 'the truth is that nobody in New Zealand knows much about the more common species of wildlife inhabiting this country, because proper research and study by trained observers has never been considered worth payment' (*Ibid.*, p. 2). As Galbreath (1993) concurred, even in the relatively established areas of fisheries science and deer ecology, growing practical experience was backed up by little scientific investigation. Holloway (1950, p. 123), for example, reported that by 1950 'a very considerable amount of information is now on file' regarding the effects of deer, 'although it is not yet possible to make any detailed analysis of it.' Having bemoaned the lack of scientific research into wildlife, the Royal Forest and Bird Protection Society took it upon themselves to rectify this deficiency by undertaking highly unscientific 'data' collections. The Society considered the introduced magpie (*Cracticus tibicen*), for example, to be an aggressive 'butcher bird,' that was detrimental to native wildlife (Editor 1950, p. 1). In the absence of scientific evidence they set about 'proving' the magpies' 'guilt' themselves by setting up a 'dossier of crimes,' and asking members to submit any observations that supported the Society's hunches (*Ibid.*, p. 1). This was subsequently taken as the evidence needed to support widespread killing of magpies.

With some exceptions, it was not until the 1960s that any substantial quantitative research began to be undertaken on wildlife in New Zealand. Following wider international trends, natural history was no longer deemed sufficient as science in New Zealand. Rather, results from around this point would have to be substantiated with quantifiable evidence. The 1960s, for example, marked the start of large-scale ornithological research in New Zealand – finally moving beyond the taxonomic and descriptive work of Walter Buller and others. It was also the beginning of scientific work on wild mammals such as deer. As Graham Nugent (Interview, Deer Ecologist, May 2013) commented, 'reasonably scientific publications [on wild deer] started in the 1950s until it came to be more quantitative in the [19]60s and [19]70s.'

Work in developing a scientific understanding of freshwater fish, particularly the native species, finally commenced too (McDowall 1991). Lakes were particularly poorly researched: 'Before 1966 there had been scant investigation of New Zealand lakes…only a few general limnological studies had been carried out and fewer still had been published' (Burns 1991, p. 359). This dearth of study began to be redressed with the appointment of limnologists to the staff of universities, the formation of the New Zealand Limnological Society, and the establishment of a Freshwater Section in the Department of Scientific and Industrial Research. An emerging emphasis on scientific research, however, did not mean that *any* questions could be assessed; only those that were approved. A young R.M. McDowall, for example, was forced to shelve his investigation into the potential ecological effects of introduced largemouth bass (*Micropterus salmoides*) until the departure of a disapproving superior. As he later explained, 'my analysis…was completed somewhat after the senior fisheries scientist promoting the introduction left New Zealand for overseas, and so I was no longer in danger from criticising my superiors' (McDowall 1999, p. 52).

Case studies

Deer and erosion

Many of the scientists working on the ecology of deer through the early 20th century were predisposed to proving the impact of deer on New Zealand soils and vegetation. The question of *whether* they had an impact was generally not scientifically considered. Leonard Cockayne, a prominent botanist, was foremost in the scientific castigation of deer (Cockayne 1926). Noting early that deer ate the plants he had devoted his life to studying, he developed a 'passionate hatred' of them (Caughley 1983, p. 68). According to Caughley, he 'used every argument he could muster to urge their extermination,' some of which were 'less than impeccable scientifically or logically' (*Ibid.*). Importantly, Cockayne argued, with others, that deer caused 'vast areas of mountain-side [to] be turned into moving debris' (in *Ibid.*, p. 63). Rather than assessing the effects of deer on native vegetation and soils, Cockayne, like most New Zealand scientists of the time, was content to move his scientific reasoning straight to questions of extermination and how it could be undertaken. Scientists present at the Deer Menace Conference in 1930, for example, accepted unanimously that deer were an environmental problem. They thus aimed only to provide 'practical suggestions as to the best method of carrying out deer destruction' (Figgins & Holland 2012, p. 41).

For Grant Nugent (Interview, Deer Ecologist, May 2013), 'the intuitive link in the [19]30s and [19]40s was that where there was no forest there was lots of erosion and you just had to look at the Southern Alps to see that.' The scientific work that should have gone into proving that deer caused damage to vegetation, and thus brought about erosion, however, was never undertaken. In fact, such basic research was deemed unnecessary by most scientists at the time. For example, in 1934 the New Zealand Forestry League wrote to the Royal Society of New Zealand asking for support in a request to the Commissioner of State Forests to set up a Royal Commission to inquire into the effects of deer and other introduced mammals on native forests. The Native Bird Protection Society wrote on the same subject, 'but expressed the view that there was no need to incur the expense in the setting up of a Commission, as there was abundant evidence of the destruction caused by these animals' (Royal Society of New Zealand 1934, p. 375). However, there were only two papers dedicated to the issue, Walsh (1892) and Hutchinson (1930), both of which provided only anecdotal reports of 'damage.' This was deemed sufficient for the Royal Society, nonetheless, who duly commended the Department of Internal Affairs 'for the measures taken to reduce the number of deer, and urge[d] that its efforts be increased' (Royal Society of New Zealand 1939, p. 24).

The case for a link between deer and erosion was further expressed through the mid-20th century. In his Presidential Address to the New Zealand Institute of Forestry, C. Biggs (1946, p. 214) argued that the 'delicate balance' of geological erosion was being upset by deer and that they should therefore be considered to be 'the most serious enemy.' Writing in the *New Zealand Science Review*, McKelvey (1959, p. 28) furthered the proposition that any animal effect on vegetation could cause accelerated erosion and flooding. This was seconded by Holloway (1959, p. 21) who considered 'acceleration of erosion as an inevitable consequence of [vegetation] depletion.' Suggestions to the contrary were swiftly rebuked. William Graf, a visiting American biologist, disputed claims that erosion was the inevitable consequence of deer browsing. His report, however, was dismissed by A.L. Poole, Assistant Director of the New Zealand Forest Service, who persisted with the claim that exposed faces of bare shingle in mountainous areas of the South Island, in particular, were 'entirely unnatural' (Poole 1958, p. 5). He suggested, somewhat improbably by this stage, that 'Dr Graf evidently did not see any forest that was not frequented by [grazing] animals' (*Ibid.*, p. 5). In an article in the *New Zealand Journal of Forestry*, McKelvey (1960, p. 325) continued to claim that there was 'much evidence' that deer browsing was an important cause of erosion. This was backed up by the New Zealand Forest Service which printed regular educational advertisements to that effect in magazines such as *Forest & Bird* and scientific journals, including the *New Zealand Science Review* (New Zealand Forest Service 1960, 1962a, 1962b).

By 1956, Thane Riney, another American biologist, considered research on introduced animals such as deer in New Zealand to still be in 'an early phase of development' (Riney 1956, p. 16). Taking up a position at the Department of Internal Affairs, he was commissioned to undertake some of the first formal research on wild deer in New Zealand (Department of Conservation 1998). There, he was highly innovative and energetic, producing around 25 published reports and papers (Caughley 1983). Nevertheless, he 'was soon in hot water with the Department because he…set about examining [the Department's] assumptions [about deer] as if they were hypotheses' (*Ibid.*). One of his papers, for example, showed that the areas prone to erosion had little overlap with the areas in which the Department were shooting deer. As Caughley noted, 'hard facts are as often an impediment to attaining a goal as they are a help. If the goal is clear and the cause is just, information is not so much right or wrong as it is convenient or inconvenient' (*Ibid.*, p. 119). As a staff scientist, Riney was expected to produce science that supported the Department's objectives. When this was not manifest, his position became untenable. He resigned and departed the country in 1958.

By the 1970s, doubts began to creep into the thesis that deer were responsible for erosion rates (Holloway 1993). Orman (1979 in Holden 1987), for instance, observed that slips apparently caused by deer might just as easily have occurred without them. Noting the presence of such conflicting evidence, Holloway (1970, p. 11) accepted that 'depending on which pair of spectacles we choose to wear and which piece of country we choose to look at, we can find evidence that can be used to support almost any argument that may be advanced.' Indeed, Holloway had noted as early as 1959 that the rate of normal geological erosion in many parts of New Zealand was 'spectacularly high before grazing animals were introduced' and therefore not necessarily a correlate of deer herbivory or trampling (Holloway 1959, p. 22). In the 1970s and 1980s, 'people began to realise that what looked like current erosion in the form of screes were often very old' (Interview, Graham Nugent, Deer Ecologist, May 2013). The earliest photographs of some of the mountainous headwaters of the Southern Alps, for example, showed that most of the screes and erosion gulleys were there in the 1860s and 1870s, when deer populations in New Zealand were in their infancy and largely restricted to the lowlands (Caughley 1983). The pre-human rate of erosion was found to be much higher than earlier suspected.

Although the Forest Service had conducted much research into the effect of deer on erosion rates, its starting assumptions were flawed. At no point, moreover, were these assumptions measured. Rather,

> *…all through the period that [both the Department of] Internal Affairs and [the New Zealand Forest Service] expended large sums of money on killing deer, no research was launched to discover how much this effort retarded erosion. The simplistic formula went: fewer deer means more vegetation, which means less erosion, which means less flooding. How much less was neither known nor investigated* (Caughley 1983, p. 73).

Caughley noted that there was a reluctance of research staff to question what a department had already promulgated, suggesting that although,

> *…it can be done, and no insurmountable barrier will be placed in the way of doing it…it leads to hassles and ill-feeling that most researchers can do without. Far easier to tackle a problem whose purity is guaranteed by its answer having been anticipated officially* (*Ibid.*, pp. 72–73).

By the early 1980s it was established that the major determinant of erosion rates in mountain country was simply rainfall. The effect of plant cover 'was so slight as to be virtually unmeasurable' (*Ibid.*, p. 76). The idea that forests absorb downpours and release them slowly over several days was applicable only for light to moderate rainfalls. The torrential downpours that cause flooding quickly saturate the thin forest floor and the vegetation is largely powerless to stop or even slow it down. The effect of deer on forests, therefore, was only very loosely related to erosion rates. The 'final death knoll' of animal control as a solution to erosion sounded in 1986 when Patrick Grant presented a talk at the annual conference of the New Zealand Geological Society (Hunter 2009, p. 267) which showed that erosion rates had little to do with introduced animals, but rather were a consequence of long-term geological-scale weather patterns. In a subsequent paper in the *New Zealand Journal of Ecology*, he concluded that 'even in the absence of humans and [other] animals, [New Zealand] vegetation would be in a dynamic state of imbalance and change' (Grant 1989, p. 143). The thesis that deer caused erosion had survived for at least five decades without being scientifically tested. Despite this, it was regularly and often forcefully endorsed during that time by scientists. Again, this shows the ways that science can be employed to answer some questions to the detriment of others, and how the ways that scientific research is funded can unhelpfully predetermine outcomes.

Mallard science, or a continuing lack thereof

Despite the rising numerical importance of mallards in the early 20th century and the broader importance of waterfowl to hunters nationally, scientific research on waterfowl was similarly underdeveloped and not officially instigated until 1947 when Ron Balham was appointed to the Wildlife Branch of the Department of Internal Affairs (Galbreath 1993). Because the Wildlife Branch's research was funded by levies on game licences it focused mostly on game birds (*Ibid.*). Between 1947 and 1961 more than 30,000 wild native grey ducks (*Anas superciliosa*) and introduced mallard ducks were leg banded as part of research on movement patterns and survivorship (*New Zealand Outdoor* 1961). Work on waterfowl habitat began in 1949 in conjunction with the Department of Scientific and Industrial Research and the Marine Department (Galbreath 1993). Despite these initiatives, few scientific results were published (Balham 1952).

As late as 1963, Jenkin lamented that, '…there [had] never [even] been an official duck census taken' (Jenkin 1963, p. 12). The principal tool used to collect information on waterfowl from the late 1960s to mid-1980s was the National Waterfowl Diary. This was a New Zealand Wildlife Service scheme instigated by Tom Caithness in which shooters recorded their daily 'bags.' Few of the resultant data were ever scientifically assessed. This lack of accurate quantification has promoted ongoing confusion as to the status of the mallard population in New Zealand (e.g. see Barker 1989; Muller 2010; Moriarty *et al.* 2011) and most estimates over the years have relied on 'educated guesswork' (Creasy 1987–88, p. 41).

However, mallard populations throughout New Zealand, were thought to have increased rapidly from the mid-1960s through the early 1980s (Buchanan 1990; Hayes 1989; Marchant & Higgins 1990), so much so that concerns began to be expressed about their effects on horticultural interests, particularly the seeding stages of grain and root crops which were sometimes partially consumed by mallards (Buchanan 1990; *New Zealand Outdoor* 1978). The response was a general loosening of regulations on mallard harvest. A 1986 Wildlife Branch, Department of Internal Affairs, internal report perceptively titled 'Mallard Management – A 'People' Problem or a 'Duck' Problem?' noted that:

> *Expanding mallard populations in the late 1970s and early 1980s led many districts toward an increasing liberalisation of conditions to allow hunters to take full advantage of high mallard numbers. The adoption of three month seasons and large or even no daily bag limits on mallards became acceptable practice* (Buchanan 1986, p. 3).

However, any sense that mallards *were* in high numbers *or* expanding was not based on any accurate quantitative assessment with 'no real attempt made to monitor the effects of changing regulations on harvest rates, nor to understand the relationship between hunting and population status' (*Ibid.*).

Data from the diary scheme seemed to show, however, that the mallard duck population was levelling off in the mid-1980s (Poynter 1986). One equilibrium-inspired theory was that the population had reached its 'carrying capacity' at this stage and was declining to a lower but stable, sustainable level. Barker (1989, p. 4), for instance, wrote that:

> *…the pattern of mallard population change has followed the classical form for an animal moving into a new environment. There are countless examples that illustrate that in a new environment, animal population levels increase rapidly, overshoot, then oscillate about a stable equilibrium, at a point somewhere below peak levels* (also see Buchanan 1990).

This ignored the fact that most waterfowl introduced to New Zealand did not follow such a 'classic' trajectory, instead declining rapidly to extinction (Thomson 1922; Williams 1962). Nor did it take into consideration the work of acclimatisers over the previous 100 years (Dyer & Williams 2010, 2011; Veltman *et al.* 1996). The mallard's supposed favourable ecological niche, for example, did not appear nearly as favourable in the 1930s when they were mostly abandoned as a future sporting proposition (Dyer & Williams 2011). Nor did it incorporate changes in hunting regulations or the substantial provision of new habitat by hunters from the 1950s onward (Galbreath 1993). The thesis posited that the reputed changes in mallard numbers were simply to be expected, ignoring the fact that they were actually quite extraordinary and, if accurate, certainly influenced by a wide range of factors, most of which were poorly understood.

Murray Williams (Interview, Waterfowl Biologist, January 2013) described the current system of monitoring waterfowl in New Zealand as an 'inexact science' at best, and 'absolutely fraught' at worst. He suggested that Fish and Game New Zealand,

> *…do not have a reliable or even a nationally applied technique for monitoring game bird numbers. That may come as a surprise but it's absolutely true […] Fish and Game don't employ any biologists as such to do that sort of work, even though they have some quite competent field staff. All the decisions are made around a council table by lay people. Often they will use their own observations or prejudices to guide hunting and the [only] thing that saves them is the fact that the number of hunters is declining year by year […] They get data, but they've got no way of checking what they get (Ibid.).*

Indeed, general scientific research on mallards in New Zealand, of any kind, remains underdeveloped. Despite being the most populous species of waterfowl in New Zealand, not a single scientific study (excepting the issue of hybridisation) has addressed their effects on the environment. In fact, it is only in the last decade that *any* questions about the effects of mallards in New Zealand have been raised in the scientific literature. Some studies suggested that mallards may be vectors for introduced plant species both from Australia and within New Zealand (de Lange *et al.* 2011; Heenan *et al.* 2004). A recent study in the *New Zealand Journal of Marine* *and Freshwater Research* suggested that mallards may act as reservoirs of faecal contamination (Moriarty *et al.* 2011). Murray Williams (Interview, Waterfowl Biologist, January 2013) noted that mallards may also physically displace other waterfowl from breeding habitat (also see O'Connor *et al.* 2007; Williams & Basse 2006). Such suggestions continue to await quantification.

The low importance placed on mallard research is reflected in the fact that most of the research, to date, has been undertaken by just one researcher. As Nathan Burkepile (Interview, Field Officer, Fish & Game (Northland), February 2013) exaggerated, 'If it wasn't done by Murray Williams it really hasn't been done.' This is partly a consequence of the legislative arrangement that vests responsibility and management of mallards solely with Fish and Game New Zealand. Their mandate is to provide game birds for hunters. There is little incentive to fund research looking into any potential negative effects of mallards. Even research on the extent of hybridisation between mallards and native grey ducks has never been adequately funded. As Rob Pitkethley (Interview, Regional Manager, Fish & Game (Eastern), January 2013) noted, Fish and Game New Zealand is 'an under researched organisation […] if you looked at our percentage research spend against total budget we would be right down the low end' compared with other 'natural resource managers.' What little research *is* undertaken, moreover, is not typically directed at questions that could potentially undermine the public perception of hunters' quarry. This again demonstrates the way that scientific information can be both used, and not used, to promote certain arguments and understandings of wildlife to the detriment of others. Equilibrium theory, though now considered deficient (see Pickett *et al.* 2007), is used as a way of suggesting that the mallard population is stable and under control, while very little scientific work has actually gone into proving this supposition. This underlines the need to explore and contextualise the motives of those in control of the science on different species of introduced wildlife to ensure that the full scope of questions are being asked.

The increasingly contested role of science in wildlife management

Although further effort was directed towards ensuring the veracity of scientific research on wildlife in New Zealand in the late 20th century, there remained significant gaps in basic knowledge. Holloway (1993, p. 287) noted that despite the millions of dollars spent on the management of introduced species over the preceding one hundred years, much expenditure 'had little long-term effect because of persistent failure to understand the biology of the target animals.' Writing on trout stocking policy in the Rotorua Lakes in 1984, Principal Wildlife Officer N.B. Ewing, for instance, noted that methods of imposing regulations on anglers were based less on scientific knowledge and more on 'knowledge at the time, commonsense and gut feelings' (Ewing 1984, p. 3). He felt that scientific knowledge in fundamental areas such as fish population, 'crop' available and trends in angling were 'very weak' (*Ibid.*). Moreover, although it was considered 'likely' that introduced species in freshwater environments were having adverse impacts on native species, few studies had quantifiably documented them (Collier 1993, p. 341). In lakes, there had been 'few studies…of nutrient cycling, trophic interactions, and production that include vertebrates' (Burns 1991, p. 371). Burns ascribed this omission partly to the 'institutional separation of governmental scientists engaged in research on plankton, fish and wildlife' (*Ibid.*). She also noted, however, that research on freshwater fauna had been largely 'management-oriented,' as it had since the late 19th century (*Ibid.*, see above). In other words, it had tended to be used for the purposes of promoting certain favoured species and little else.

From around the 1970s both the employment of scientific research, and its supposed impartiality, came under renewed scrutiny in New Zealand. An editorial in the *New Zealand Science Review* observed that:

> *The name of the game is business. Its creed is profit (which is the only alternative to loss), its Bible is the balance sheet, its emblem is the dollar sign, and cost is its watchword…The day is gone when one could invoke "science for the sake of knowledge," nominate a project, and research the life out of it for the next twenty years. Science is now an investment, a business venture as vulnerable to an unfavourable annual report as any manufacturer. Clearly the message is getting through, for the [National Research Advisory Council] Annual Report notes, no doubt with some amusement, that 'no organisation admits to doing pure research.'* (Editor 1970, p. 88).

As this passage suggested, scientists' research interests were seen to be tied inextricably to the interests of their financial backers, whether government or the private sector, necessarily limiting the direction of their results should they wish to expect future employment or funding. Rather than question the repercussions of this departure from objectivity, scientists were invited to see their work as a business transaction with results tailored to suit the objectives of their 'clients.' Any diversions from this formula would be swiftly punished through marginalisation and the withdrawal of funding.

Partly as a consequence of this solidifying approach to science, certain 'omissions' in knowledge frequently seemed to correlate with information that was not useful to the parties funding research. The lack of studies on the effects of trout on native freshwater biota, for example, was typical of this selective use of scientific research. Freshwater science was devoted to understanding how to grow more trout, bigger, and faster because this was what the authorities tasked with managing freshwater 'resources' were asked to achieve (McDowall 1991; McIntosh *et al.* 2010). Questions that might disrupt the flow of research aimed in this direction were not only inconvenient, but potentially damaging.

Noticing the increasingly vested interests of scientists, criticisms of the use of government science to advocate for the control or removal of deer in New Zealand became commonplace in many New Zealand hunting periodicals. An offering by McArthur (1985/86, pp. 16-17) in *New Zealand Wildlife* is typical:

> *Now one of the things which makes the environmental movement so credible is that its recommendations seem to be well founded scientifically. After all we live in a scientific age and people often take for gospel the pronouncements of scientists just as they used to believe*

> *what the church told them in previous generations…Well a scientific degree may be one thing – but a scientific attitude is another. A university degree by no means guarantees the graduate will attempt to be impartial.*

McArthur noted that, just as religion has come to suffer ignominy through distortions of truth and other injustices, science too may be heading for a similar place of disrepute. He encouraged readers, therefore, to question the word of scientists just as they had justifiably questioned the word of religious leaders. Understandings of science as simple and unproblematic were, like understandings of religion, becoming complex and often ambiguous. In an article in *Fish & Game New Zealand*, Speedy (1996, p. 75) engaged with a now common summation of the use of science in wildlife management in New Zealand, noting that it is 'as much about value judgements as it is about good science' – values that are not necessarily those of the scientists themselves.

Investigations over the 1980s and 1990s, furthermore, showed that there was good reason to be sceptical of some of the earlier faith placed in scientific understandings in isolation. Even introduced species that seemed to have been proven to be 'bad' by scientists, for example, were shown to require further consideration. Despite widespread castigation of 'introduced predators,' apparently well-founded in research, King (1985, p. 130), New Zealand's foremost mammologist, argued that 'even after considerable research effort, there is still no firm information on the effect that any common predator, such as the stoat (*Mustela erminea*), has on bird populations in contemporary times.' She offered, as perspective, the realisation that of the 153 distinct populations of birds known to have disappeared from the islands of the New Zealand group since 1000 AD, stoats [as one example] could have come into contact with only five that are now extinct and 11 that are still threatened' (*Ibid.*). To King, the level of invective routinely directed at them, and other introduced mammals, was therefore misdirected and certainly not well substantiated by the scientific evidence of the time. The introduced possum (*Trichosurus vulpecula*), was also widely seen as demonstrably 'bad' for native wildlife. A study in 1999, however, showed that the long-term effects of possums on floral biodiversity, virtually unconsidered at that time, deserved further study (Bellingham *et al.* 1999). They found 'no substantial changes in species composition' in conifer/broadleaf forests inhabited by possums over periods of 14–25 years (*Ibid.*, p. 5). Indeed, many species palatable to possums 'remained relatively unchanged' (*Ibid.*), casting doubt over some earlier cataclysmic predictions of forest collapse (e.g. Editor 1969; Kean 1953). This finding is supported by further recent evidence (e.g, see Department of Conservation 2012, pp. 108–109).

Reflecting on scientific assessments on the effects of deer in the early 20th century in New Zealand, in particular, Graham Nugent (Interview, Deer Ecologist, May 2013) contextualised the work that was undertaken. Conceding apparent inadequacies of science at that time from a contemporary perspective, he suggested that,

> *…while it was not quantified, it was reasonably good natural history of that sort of post-Darwinian [kind] […] We can cast aspersions about it now because it wasn't quantitative, but that's what they had access to. That was the way they were trained. It was the most systematic observations they were able to make* (*Ibid.*).

Whilst this is undoubtedly true, it bypasses an important realisation: that what is considered to be 'good science' changes. The methods used to indict introduced species in the 19th and early 20th centuries frequently no longer stand as 'reasonable' evidence. Typically they are now negatively characterised as 'anecdotal' or 'circumstantial' (e.g. see McDowall 1991, on the effects of trout on native fish in New Zealand). Furthermore, as the standards of good science change, there is no reason to suspect that many modern appraisals may suffer similar falls from credibility, if not respectability, in future. Again, I highlight this, not in an attempt to discredit the use of science to assess issues in relation to wildlife in New Zealand, but to maintain that a healthy discussion on how scientific research is being employed and interpreted is not only justified, but demonstrably sound.

Seek and ye shall find?

Regardless of the above, much scientific research on introduced wildlife in New Zealand remains in its infancy. According to Tony Beauchamp (Interview, Technical Advisor Threats, Department of Conservation (Northland), February 2013), ongoing insinuations of 'guilt' attributed to many introduced species (e.g. see Camp 1997; *Forest & Bird* 1951, 1956) often remain based in 'folklore and ignorance more than anything else.' Only recently have studies even begun to quantify the effects of many widely castigated introduced birds in New Zealand (e.g. see *New Zealand Hunting & Wildlife* 2003). In addition, much research on introduced wildlife remains based on short-term studies whose conclusions may not apply long term. As Graham Nugent (Interview, Deer Ecologist, May 2013) noted on research into deer in New Zealand:

> *There's a lot of detail gaps that are missing […] In terms of vegetation lifetimes, it's all pretty short-term stuff. It's decades or less and yet most of the trees we're working with have millennial or semi-millennial turnover times.*

Indeed, until Forsyth *et al.* (2011) there had been no long-term studies of ungulate population dynamics in New Zealand. According to Dave Rowe (Interview, Freshwater Ecologist, January 2013), there similarly remain many unknowns about the long-term dynamics of freshwater ecosystems that contain trout in New Zealand. For example, although Fish and Game New Zealand have long historical records dating back to the mid-1960s in Rotorua (Interview, Rob Pitkethley, Regional Manager, Fish & Game (Eastern), January 2013), much of it remains unanalysed or otherwise tied into the overarching management of the lakes (Interview, David Hamilton, Chair of Lakes Management and Restoration, Bay of Plenty Regional Council, February 2013).

It is important to note, nevertheless, that the current state of knowledge is not necessarily opposed, particularly by game advocates. This is because the available information – that determined from a fisheries science perspective – tends to uncritically support the persistence of trout in New Zealand. Further ecological-oriented science on trout may not provide the same answers and, from the perspective of anglers, may not be desirable at all. This may be one reason why science on the effects of introduced trout on native ecosystems in New Zealand (and elsewhere in the Southern Hemisphere) only began to be seriously addressed in the 21st century (Garcia de Leaniz *et al.* 2010). This is mirrored in science on mallards which has almost invariably been conducted 'from the perspective of the fishing and hunting fraternity' which has little interest in 'actually looking ecologically at where [mallards] fit in the processes' (Interview, Tony Beauchamp, Technical Advisor Threats, Department of Conservation (Northland), February 2013).

Government authorities dedicated to conserving native wildlife, such as the Department of Conservation and its precursor the New Zealand Wildlife Service, are effectively discouraged from scientifically questioning the status quo. They are placated by the protection and enhancement of wetlands by Fish and Game New Zealand (McLeod 2007), just as the New Zealand Wildlife Service was by the Acclimatisation Societies. As Ian Hogarth (Interview, ex-Department of Conservation (Northland), April 2013) reasoned,

> *...you've gotta understand that [Fish & Game New Zealand] are very very strong supporters of wetland preservation and wetland management. And the Department of Conservation is fully behind that. And that's one of the major reasons why we're in that supporting role.*

Close social links between the two organisations also carry important weight. Staff at the Department of Conservation, for instance, are often keen hunters and anglers. Ian Hogarth (Interview, ex-Department of Conservation (Northland), April 2013) recalled his experiences working for the New Zealand Wildlife Service in Northland:

> *One of the big parts of the job was actually hunting with the local acclimatisation fraternity. So we were going out hunting with them and participating in some of their programs […] The [New Zealand] Wildlife Service, in particular, had very close connections with the acclimatisation societies. We were very close.*

Promoting science that might devalue favoured quarry and sour relations with the Acclimatisation Societies was not a high priority. Investigating any possible impacts of introduced mallards was therefore a question that was not politically suitable to ask.

In contrast, much of the science on deer has been conducted from the perspective of conservationists that are opposed to them. Rather than finding ways to enhance deer populations, science on deer tends to be focused on discovering potential negative attributes and quantifying perceived ecological harm. This was highlighted by Clyde Graf (Interview, Hunter/Anti-1080 activist, February 2013). He suggested, for instance, that the Department of Conservation,

> *...have got [a Departmental scientist] doing a project at the moment trying to prove that deer are a pain in the arse. But, once again, that sort of research is not research. It's just advocacy science – predetermined outcomes* (*Ibid.*).

He wondered if opinions on deer might change if the research was directed toward answering different questions:

> *Who's doing the research on what good deer are actually doing? You know. All the research on deer in this country is 'OK, go and prove that they're bad.' Let's do some research to see if they're actually doing something good* (*Ibid.*).

Reflecting on the science on introduced mammalian 'predators,' Tony Beauchamp (Interview, Technical Advisor Threats, Department of Conservation (Northland), February 2013) made a similar reflection. Although much research is directed to assessing their potential negative effects, 'there's not a lot of work that's actually being done to prove benefit. I'm not saying that there isn't some benefit, but [rather] it's not actually an area of enquiry' (*Ibid.*). This may explain why there is so much scientific evidence for the negative attributes of many non-game introduced species and so little for any positive contributions. The latter question is simply not asked.

A consequence of this imbalance is that the perceived impartiality of science on wildlife in New Zealand now suffers from a legacy of advocacy and agenda setting. As Ian Hogarth (Interview, ex-Department of Conservation (Northland), April 2013) commented, although scientists may sometimes enter their research with 'pure' intentions, 'the objectivity disappears as they get into the subject.' Most ecologists in New Zealand, moreover, enter their fields already well-schooled on the value of native species and the disvalue of many introduced species, meaning that any sense of impartiality is typically disavowed from the outset (Steer 2015). Others have become disillusioned with the pace of research or with changes and reversals in policy. Pete Shaw (Interview, ex-Department of Conservation (Northern Te Urewera), March 2013), for example, offered a jaded view of the value of science. He advocated a pragmatic approach: 'Do the best with what you've got now and never mind the theoretical arguments' (*Ibid.*). As a result, contributions to New Zealand hunting and fishing magazines continually point to a now-enduring mistrust in scientific authority. A letter in *New Zealand Hunting and Wildlife* is typical – Hanson (2004, p. 12), furthering the now 'traditional' lamentation of deer as 'pests' in government legislation, asked detractors to avoid using science altogether: 'Please don't quote recent "science" as evidence against this. Science has been so tainted by the privatisation agenda and bidding for contracts, that much of it lacks integrity today' (also see Watson 2006). This overarching scepticism of scientists is a poor outcome as it undermines their credibility, making it difficult for future studies to receive the resonance they may well deserve.

Concluding remarks

These findings do not discount the importance of science, but rather reinforce the understanding that scientific information on introduced wildlife needs to be assessed in the context of its production. Understandings of science in New Zealand are beginning to move from the somewhat naïve accounts of the past that presented scientists working in a political, social, and economic vacuum, to more nuanced understandings that incorporate the many factors that underlie the production of scientific knowledge. In the case of introduced wildlife, these understandings demonstrate that scientific assessments of ecological effects need to be more cautious and explicit in communicating the assumptions of that research and the predispositions of its funding sources. While scientists are often confident of their own objectivity and the vetting process of scientific peer review, others are not quite so convinced and need to be given as much information as possible to ensure that the conclusions of scientists can be fairly considered alongside other literatures.

I have also demonstrated how the standards for 'good science' change. Prior to the 1930s there was no formal wildlife science in New Zealand and assessments were largely based on what would now be considered expert opinion. Only from around the 1960s did assessments of wildlife begin to fully quantify those opinions. However, for most introduced species in New Zealand, a full consideration of their effects (both positive and negative) on the environment remains in its infancy.

Importantly, I have emphasised the extent to which science on introduced wildlife has also regularly been employed as an advocacy tool to 'prove' certain predetermined positions. Thus advocates for introduced game species fund and endorse research showing how to improve the survivorship and fecundity of favoured game species, but fail to fund any research on the effects of game species on native species. In contrast, work funded by conservationists commonly investigates the impacts that introduced species have on natives, but fails to ask whether they might be providing benefits. In both cases, scientific research is paraded as an impartial arbiter of truth to an, at times, justifiably sceptical public. A consequence of this ongoing science as advocacy is an erosion in the credibility of science itself.

Ultimately, I suggest that New Zealand's natural scientists need to be given the opportunity to, at least occasionally, ask the questions that no one else will support or pay for because otherwise their science risks being reduced to a blunt tool for those that fund and therefore direct it. I would argue that there may be some truth in the view that asking a conservation biologist under the employ of the Department of Conservation to determine if deer have positive effects on biodiversity, or a fisheries or waterfowl biologist under the employ of Fish and Game New Zealand to determine if trout or mallards have negative effects on biodiversity, is a little like asking a scientist working for Imperial Tobacco to investigate the ways that smoking damages people's health. Such questions, although possible, are generally not politically sensible to ask within these organisations. If no one else can afford to fund different questions, is it surprising that the same questions keep meeting with the same answers? At its worst, I am concerned that the death of 'science for the sake of knowledge' in New Zealand may, without conscious intervention, also prove the death of the very objectivity that natural science relies on for credibility. I hope this article will help, in some small way, to pique this concern in other New Zealand natural scientists.

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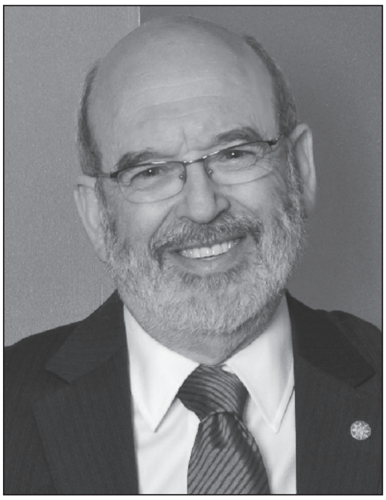
News

Peter Gluckman receives 2015 AAAS Award for Science Diplomacy



Prof Sir Peter Gluckman, who convened the 2014 international conference for governmental science advisers and now chairs the International Network for Science Advice to Governments that emerged from the conference, has been chosen by the American Association for the Advancement of Science (AAAS) to receive the 2015 Award for Science Diplomacy.

He was honoured by AAAS for transforming the theory and practice of science advice – an essential component of science diplomacy – by bringing its practitioners together into a global network.

‘In an increasingly interconnected world characterised by global threats, the demand for good scientific advice has arguably never been greater. While many governments have structures and systems in place to provide this at a national level, the cross-border nature of these threats means that the need for effective communication and coordination between scientific advisers is becoming increasingly essential,’ the nominating committee wrote.

Gluckman was nominated to receive the 2015 AAAS Award by an advisory committee including Cathleen Campbell, president and CEO of CRDF Global; William Colglazier, past science and technology adviser to the US Secretary of State and former executive officer of the National Academy of Sciences and the National Research Council; and Romain Murenzi, executive director of The World Academy of Sciences. The committee cited Gluckman’s leadership in using science diplomacy to benefit New Zealand. Under his tenure as the Prime Minister’s Chief Science Advisor, New Zealand established the Global Research Alliance on Agricultural Greenhouse Gases. The Alliance, which now counts 46 countries as members, focuses on the research and development of methods to grow food using climate-resilient systems without increasing emissions – and demonstrates the significant role that small nations can play on the global stage through science diplomacy.

Gluckman is also the founding head of the secretariat of the Small Advanced Economies Initiative and served as co-chair of the World Health Organization Commission on Ending Childhood Obesity. He also proposed that the Asia Pacific Economic Council should have a grouping of chief science advisers; the group, the first formalised regional network of its kind anywhere, was approved in 2012 with Gluckman as its standing co-chair.

Source: http://www.aaas.org/news/sir-peter-gluckman-receives-2015-aaas-award-science-diplomacy-bringing-together-science

Pi Day Cymru – 3/14

The following two articles are republished from The Conversation, dated March 14, 2016.
The articles have not been edited, and we have attributed the authors and their institutes, and given the internet citations.

THE CONVERSATION

How a farm boy from Wales gave the world pi*

Gareth Ffowc Roberts
Emeritus Professor of Education, Bangor University

One of the most important numbers in maths might today be named after the Greek letter π or "pi", but the convention of representing it this way actually doesn't come from Greece at all. It comes from the pen of an 18th century farmer's son and largely self-taught mathematician from the small island of Anglesey in Wales. The Welsh Government has even renamed Pi Day (on March 14 or 3/14, which matches the first three digits of pi, 3.14) as Pi Day 'Cymru'.

The importance of the number we now call pi has been known about since ancient Egyptian times. It allows you to calculate the circumference and area of a circle from its diameter (and vice versa). But it's also a number that crops up across all scientific disciplines from cosmology to thermodynamics. Yet even after mathematicians worked out how to calculate pi accurately to over 100 decimal places at the start of the 18th century, we didn't have an agreed symbol for the number.

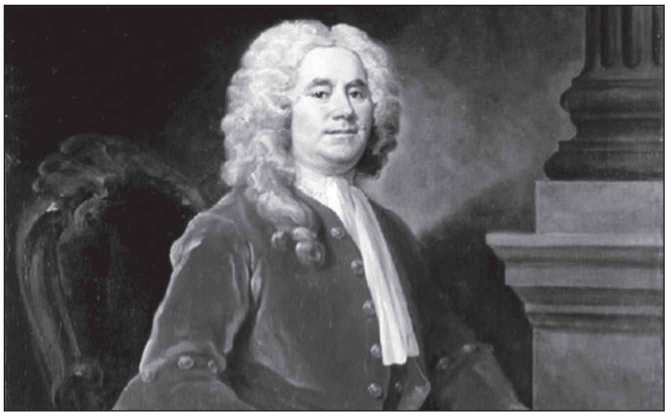
From accountant to maths pioneer

This all changed thanks to William Jones, who was born in 1674 in the parish of Llanfihangel Tre'r Beirdd. After attending a charity school, Jones landed a job as a merchant's accountant and then as a maths teacher on a warship, before publishing A New Compendium of the Whole Art of Navigation, his first book in 1702 on the mathematics of navigation. On his return to Britain he began to teach maths in London, possibly starting by holding classes in coffee shops for a small fee.

Shortly afterwards he published *Synopsis palmariorum matheseos*, a summary of the current state-of-the-art developments in mathematics which reflected his own particular interests. In it is the first recorded use of the symbol π as the number that gives the ratio of a circle's circumference to its diameter.

We typically think of this number as being about 3.14, but Jones rightly suspected that the digits after its decimal point were infinite and non-repeating. This meant it could never be 'expressed in numbers', as he put it. That was why he recognised the number needed its own symbol. It is commonly thought that he chose pi either because it is the first letter of the word for periphery (περιφέρεια) or because it is the first letter of the word for perimeter (περίμετρος), or both.

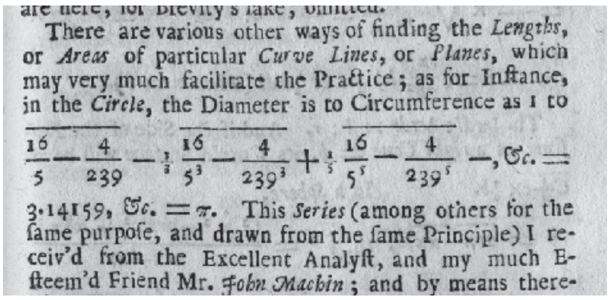
In the pages of his *Synopsis*, Jones also showed his familiarity with the notion of an infinite series and how it could help calculate pi far more accurately than was possible just by drawing



Maths pioneer, William Jones.
William Hogarth/National Portrait Gallery

and measuring circles. An infinite series is the total of all the numbers in a sequence that goes on forever, for example ½ + ¼ + ⅛ + and so on. Adding an infinite sequence of ever-smaller fractions like this can bring you closer and closer to a number with an infinite number of digits after the decimal point – just like pi. So by defining the right sequence, mathematicians were able to calculate pi to an increasing number of decimal places.

Infinite series also assist our understanding of rational numbers, more commonly referred to as fractions. Irrational numbers are the ones, like pi, that can't be written as a fraction, which is why Jones decided it needed its own symbol. What he wasn't able to do was prove with maths that the digits of pi definitely were infinite and non-repeating and so that the number was truly irrational. This would eventually be achieved in 1768 by the French mathematician Johann Heinrich Lambert. Jones dipped his toes into the subject and showed an intuitive grasp of the complexity of pi but lacked the analytical tools to enable him to develop his ideas further.



are here, for Brevity's sake, omitted.

There are various other ways of finding the *Lengths*, or *Areas* of particular *Curve Lines*, or *Planes*, which may very much facilitate the Practice; as for Inftance, in the *Circle*, the Diameter is to Circumference as 1 to

$$\overline{\frac{16}{5}-\frac{4}{239}}-\frac{1}{3}\overline{\frac{16}{5^3}-\frac{4}{239^3}}+\frac{1}{5}\overline{\frac{16}{5^5}-\frac{4}{239^5}}-, \&c. =$$

3.14159, &c. = π. This *Series* (among others for the fame purpofe, and drawn from the fame Principle) I receiv'd from the Excellent Analyft, and my much Efteem'd Friend Mr. *John Machin*; and by means there-

Finding pi. *Synopsis palmariorum matheseos*

The Conversation, March 14, 2016. https://theconversation.com/how-a-farm-boy-from-wales-gave-the-world-pi-55917

Scientific success

Despite this - and his obscure background - Jones's book was a success and led him to become an important and influential member of the scientific establishment. He was noticed and befriended by two of Britain's foremost mathematicians - Edmund Halley and Sir Isaac Newton - and was elected a fellow of the Royal Society in 1711. He later became the editor and publisher of many of Newton's manuscripts and built up an extraordinary library that was one of the greatest collections of books on science and mathematics ever known, and only recently fully dispersed.

Despite this success, the use of the symbol π spread slowly at first. It was popularised in 1737 by the Swiss mathematician Leonhard Euler (1707–83), one of the most eminent mathematicians of the 18th century, who likely came across Jones' work while studying Newton at the University of Basel. His endorsement of the symbol in his own work ensured that it received wide publicity, yet even then the symbol wasn't adopted universally until as late as 1934. Today π is instantly recognised worldwide but few know that its history can be traced back to a small village in the heart of Anglesey.

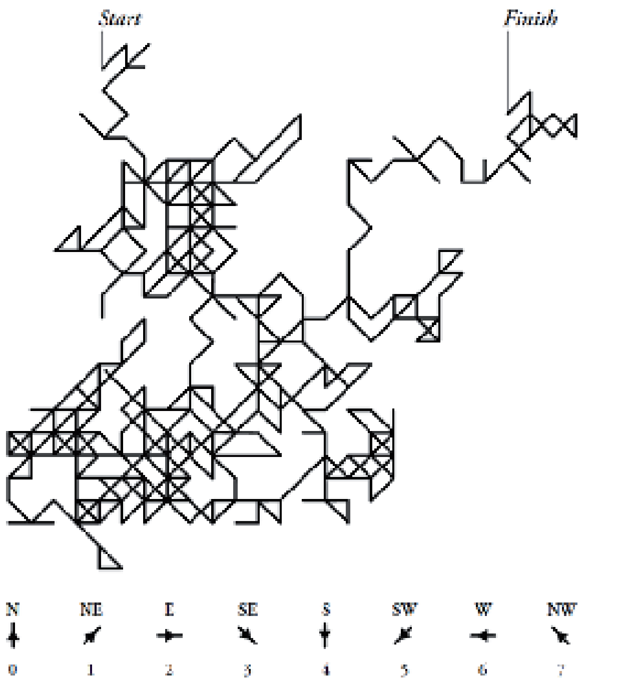
Pi might look random but it's full of hidden patterns*

Steve Humble

Mathematics Education Primary and Secondary PGCE, Newcastle University

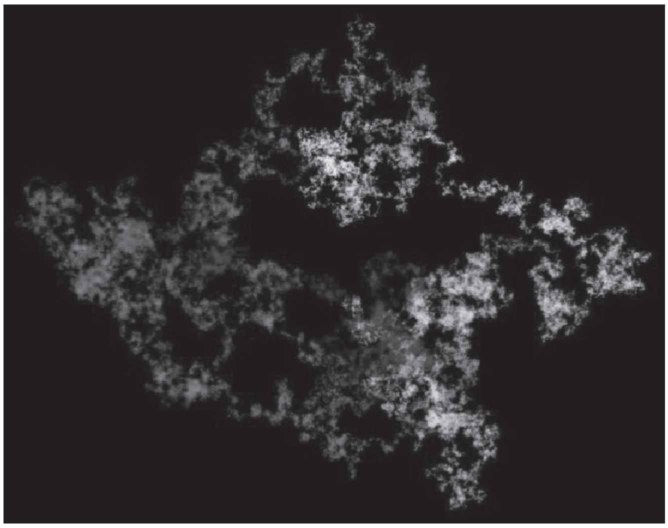
After thousands of years of trying, mathematicians are still working out the number known as pi or "π". We typically think of pi as approximately 3.14 but the most successful attempt to calculate it more precisely worked out its value to over 13 trillion digits after the decimal point. We have known since the 18th century that we will never be able to calculate all the digits of pi because it is an irrational number, one that continues forever without any repeating pattern.

In 1888, the logician John Venn, who also invented the Venn diagram, attempted to visually show that the digits of pi were random by drawing a graph showing the first 707 decimal places. He assigned a compass point to the digits 0 to 7 and then drew lines to show the path indicated by each digit.



Pi Walk by John #Venn – *The Logic of Chance*, 1888 #PiDay /via@ alexbellos

Venn did this work using pen and paper but this is still used today with modern technology to create even more detailed and beautiful patterns.



Random walk on pi (base 4) to 100 billion digits. [See front cover for colour representation.]

But, despite the endless string of unpredictable digits that make up pi, it's not what we call a truly random number. And it actually contains all sorts of surprising patterns.

Normal not random

The reason we can't call pi random is because the digits it comprises are precisely determined and fixed. For example, the second decimal place in pi is always 4. So you can't ask what the probability would be of a different number taking this position. It isn't randomly positioned.

**The Conversation*, March 14, 2016. https://theconversation.com/pi-might-look-random-but-its-full-of-hidden-patterns-55994

But we can ask the related question: 'Is pi a normal number?' A decimal number is said to be normal when every sequence of possible digits is equally likely to appear in it, making the numbers look random even if they technically aren't. By looking at the digits of pi and applying statistical tests you can try to determine if it is normal. From the tests performed so far, it is still an open question whether pi is normal or not.

For example in 2003, Yasumasa Kanada published the distribution of the number of times different digits appear in the first trillion digits of pi:

Digit	Occurrences
0	99,999,485,134
1	99,999,945,664
2	100,000,480,057
3	99,999,787,805
4	100,000,357,857
5	99,999,671,008
6	99,999,807,503
7	99,999,818,723
8	100,000,791,469
9	99,999,854,780
Total	1,000,000,000,000

His results imply that these digits seem to be fairly evenly distributed, but it is not enough to prove that all of pi would be normal.

Every sequence

We need to remember the surprising fact that if pi was normal then any finite sequence of digits you could name could be found in it. For example, at position 768 in the pi digits there are six 9s in succession. The chance of this happening if pi is normal and every sequence of n digits is equally likely to occur, is 0.08%.

This block of nines is famously called the 'Feynman Point' after the Nobel Prize-winner Richard Feynman. He once jokingly claimed that if he had to recite pi digits he would name them up to this point and then say 'and so on'.

Other interesting sequences of digits have also been found. At position 17,387,594,880 you find the sequence 0123456789, and surprisingly earlier at position 60 you find these ten digits in a scrambled order.

Pi-hunters search for dates of birth and other significant personal numbers in pi asking the question: 'Where do I occur in the pi digits?' If you want to test to see where your own special numbers are in pi, then you can do so by using the free online software called 'Pi birthdays'.

Abstract

The Austronesian Diaspora: A Synthetic Total Evidence Model

Geoffrey K. Chambers[1],* and Hisham A. Edinur[2]
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[2]Human Identification/DNA Unit, School of Health Sciences, Universiti Sains Malaysia, Kelantan, Malaysia

This is an evidence-based account of a remarkable, but perhaps somewhat underestimated, series of human population movements lasting continuously for around 5000 years. Information has been collected from a wide variety of studies across a range of disciplines and subjected to critical examination. The emergent picture is presented as a *Synthetic Total Evidence Model* which traces the Austronesian Diaspora from Taiwan via a genes, language and culture trail to Island Southeast Asia. From there two distinct branches are shown, one to lead across the Pacific and another through Malaysia and Indonesia then on to Madagascar. Along the way there are many confounding episodes of admixture, language shifts, and cultural assimilation. The Pacific branch is shown to contain two distinct groups known as Polynesians and Melanesians with similar, but still individually characteristic, gene pools. Despite all these complexities, the evidence does build to a single unified multi-dimensional picture.

Reference

Chambers, G.K.; Hisham, A.E. 2015. The Austronesian Diaspora: A Synthetic Total Evidence Model. *Global Journal of Anthropology Research 2*: 53–65.

This open access article is available at: http://www.cosmosscholars.com/phms/index.php/gjar/article/view/515

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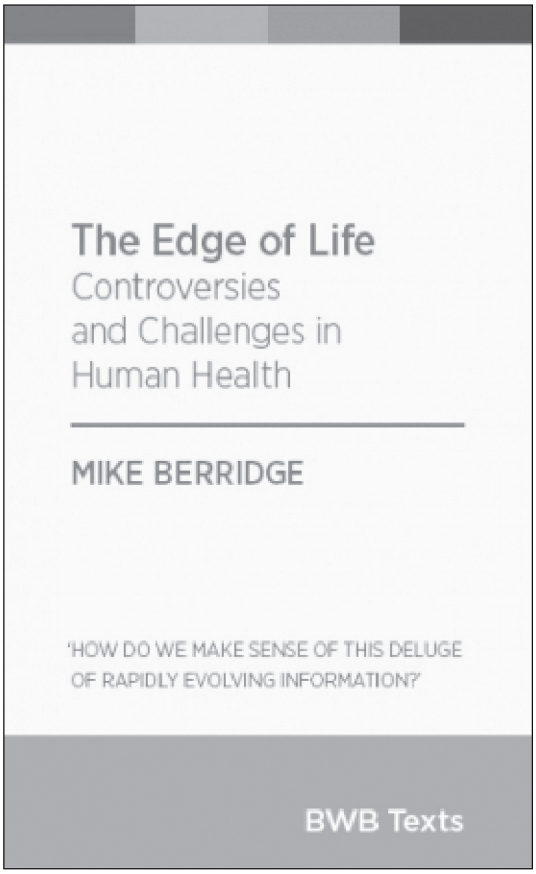
In another study yet to be published, Dr Chambers explores how these ancient migration events caused the gene pools of Māori and Pasifika people to diverge markedly from Europeans, and explains why this has significant medical implications for present-day New Zealand.

Mike Berridge

The Edge of Life: Controversies and challenges in human health

Reviewed by Peter Doherty*

Though the past decades have seen a revolution in medical understanding and practice that has greatly improved health outcomes and increased human life spans, most of us have a very incomplete understanding of what has actually occurred. That need no longer be the case if we take the trouble to access this elegant little book by Malaghan Institute (Wellington) cancer researcher Mike Berridge.



The case for evidence-based medicine is put clearly, using language that everyone can understand. Complex ideas backed by years of detailed research are explained succinctly and in plain English. There is a complete absence of jargon and, at no stage, does the author 'talk down' to a non-scientist reader. The topics covered range from the 150-year-old plus history of infection and immunity, through the case for fluoridation of water supplies, to the recent focus on the importance of the gut microbiome, to the view that being 'too clean' can be bad for us, to the evolving area of regenerative medicine, and new insights from areas like epigenetics that are illuminating our understanding of cancer and, in fact, of who we are in the hereditary sense. The text is topical, lively and a pleasure to read.

Though shorter and, of course, much more up-to-date, the book reminds me of the widely read Lewis Thomas classic, *Lives of a Cell*, that provided such a clear exposition of medical advances in the 1960s and 1970s for intelligent, but non-specialist readers of that time. And locals will enjoy the fact that it is written from a New Zealand perspective.

Beyond the science, Mike Berridge also discusses the profound ethical issues that have arisen as a consequence of our newly accessed capacity to manipulate the human genome. The era of cell and gene therapy is already here from the viewpoint of the researcher and, increasingly, the doctor and patient. These technologies can potentially impact each and every one of us. Democracy only functions well if voters are informed. Reading *The Edge of Life* provides an accessible and non-threatening 'in' to these complex and massively important issues.

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*Peter C Doherty is Laureate Professor, Department of Microbiology and Immunology, University of Melbourne at the Doherty Institute and the Michael F. Tamer Chair of Biomedical Research at St Jude Children's Research Hospital, Memphis, Tennessee.

Book review

Robert Geyer and Paul Cairney (Editors)

Handbook on Complexity and Public Policy

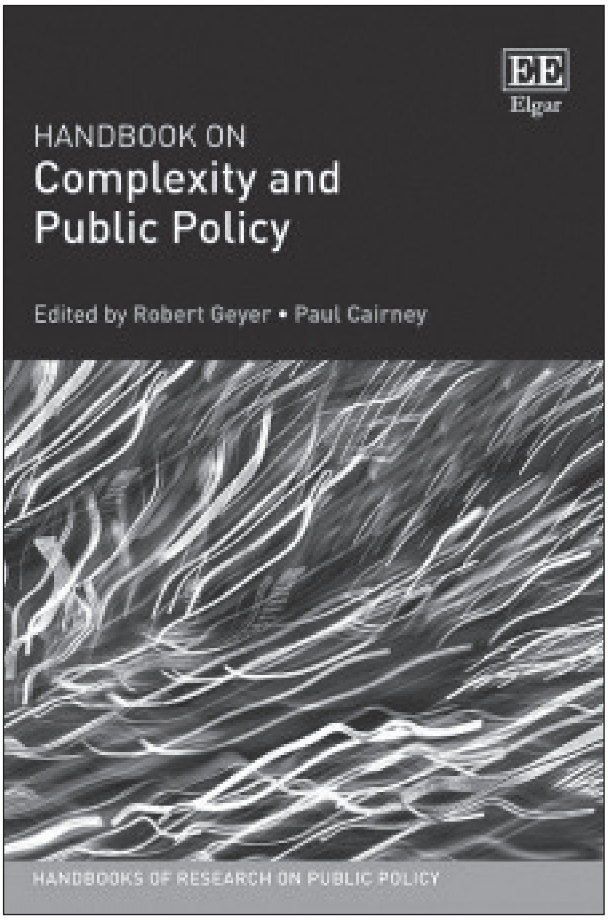
Reviewed by Paul Gandar*

The aims of *Handbook on Complexity and Public Policy* (HCPP) are set out in an introductory chapter by the editors, Robert Geyer, Professor of Public Policy at Lancaster University, UK, and Paul Cairney, Professor of Politics and Public Policy at the University of Stirling, UK. They are to 'improve the theory and practice of policymaking by drawing on the theory, concepts, tools and metaphors of complexity' and to advance 'complexity thinking' as a means for understanding and explaining the policymaking world and as a basis for policy development.

These aims are pursued through 482 pages in 27 chapters, with a total of 40 authors. There are seven chapters on 'theory and tools', six on 'methods and modelling for policy research and action', twelve chapters on applying complexity to local, national and international policy, and a concluding chapter by the editors.

There are two ways to approach a review of a book of this size and scope, in detail and in overview. Both approaches deserve attention.

In detail, there are points of interest in most chapters to an extent that makes it impossible to do justice to all in a brief review – indeed the editors themselves find it necessary to use eight pages in their introductory chapter simply to outline the contents and conclusions of the chapters that follow. So, I shall touch briefly and selectively only on a few highlights.



An introductory chapter by the editors sets the scene for the book. They provide a definition for complex systems based on a list of the properties that these systems are said to share – being greater than the sum of their parts, operating with various positive and negative feedbacks, exhibiting sensitivity to initial conditions, behaving in ways that depend more on local interactions than on central organisation ('emergence'), and giving rise to various patterns of behaviour (e.g. 'attractors', 'punctuated equilibrium'). They note that this set of properties positions complexity theory as a scientific paradigm focussed on wholes rather than parts and that this makes it an alternative to the 'fatally flawed' paradigm of reductionism.

These ideas are picked up in the group of seven chapters on 'theory and tools' that follow. These unite in the view that complexity theory is necessary because real-world systems cannot be studied or understood adequately using frameworks such as realism, positivism and objectivism[1] based on, or related to, reductionism. This view is not, however, one-sided. Together, the seven chapters provide a useful critique of ways in which ideas about complexity relate to, sometimes complement, and sometimes fall short of other explanatory frameworks and philosophies of science, and they also highlight various quite significant gaps in current views of complexity theory, a point to which I shall return.

There are then six chapters on 'methods and modelling for policy research and action'. These focus on methods such as data mining, social network analysis, case studies and document analysis for describing what is there and on approaches such as scenarios and agent-based and dynamical-system simulations to gain insights into ways in which system behaviours change over time. All of the chapters discuss 'modelling' in one way or another. Here, there is some of the usual conflation of this term with computer-based simulation modelling but there are also several useful discussions of the wider view of models as tools[2] used for thinking and therefore as spanning from mental models, to verbal models (including metaphors), to diagrams,

* Dr Paul Gandar was formerly a policy advisor in the New Zealand public sector

[1] Variously, in Little (Ch.3) on applications to politics; Webb (Ch.4) on applications to law; Givel (Ch.5) and Morçöl (Ch.6) on applications to public policy and public administration.

[2] Most notably in discussions by Occelli & Sembolini (Ch.12) and by Edmonds & Gershenson (Ch.13) on the roles and nature of models.

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and on to simulations and mathematical models. A chapter by Kasey Treadwell Shine (Ch.11) stood out for me in this grouping because of a model for child poverty that makes use of a metaphor in which social complexity is described as a force warping a social fabric and channelling behaviours into basins of attraction, some positive, some negative. This is a nice example of the way in which complexity theory opens up new and quite liberating ways for thinking about social phenomena and, from there, creates new avenues for carrying out research and looking for solutions[3].

The remaining twelve chapters of HCPP are focussed on 'applying complexity', and span from the small and local[4] to global issues such as climate change and the 2007/08 international financial crisis. The need to find the right loci for policy formation and decisionmaking is a common theme in many of these chapters. The argument is that much public policy is still based on forms of centralised, top-down policy formation and implementation and that this approach is often ineffective because it fails to take into account the realities of diversified, distributed knowledge and of bottom-up and adaptive responses to settings. Methods for balancing top-down and bottom-up approaches, for working with uncertainty and incomplete knowledge, and for adjusting policies and targets as learning occurs are discussed in most chapters, but there is little consistency about the packages of approaches discussed. Overall, there is a sense that the main impacts from attempts to use complexity theory in applications come through the 'conceptual inspiration'[5] provided, the freeing up of thinking and greater willingness to work with a plurality of approaches to problems.

The book ends with a further chapter by the editors highlighting the tension between the belief by 'converts' that a complexity view of the world is necessary, and the belief elsewhere – notably in much of science and in political and policy discourse – that reality is sufficiently described using reductionist frameworks. Two responses to this tension are suggested: challenging reductionism within academia by establishing better evidence for the explanatory efficacy of complexity theory; bridging gaps between complexity thinking and reductionist thinking in policymaking by finding pragmatic ways to work with, and educate policy makers, politicians and the public on gains that can come through use of concepts and tools from complexity theory.

What to make of all of this? The book certainly provides a summary and update on a particular way of thinking about complexity (see below) and it contains many ideas of interest and much that is thought-provoking. As such, HCPP should find a place as a useful reference book in university libraries and on the bookshelves of academics in places such as Victoria University's School of Government. But for less specialised readers, including most policy advisors and policy makers, its value is less certain. These readers will rapidly discover that HCPP is not the handbook its title suggests – it does not supply a concise framework to explain complexity, nor, despite a range of interesting applications and useful discussions, does it provide clear guidance on how to use complexity thinking in policymaking. Furthermore, and particularly in the New Zealand context, it is hard to imagine policy makers having the time, the mandate, the motivation or the energy to work through, and sift out the useful ideas from the large amount of quite dense material covered in HCPP.

So how well does HCPP measure up against its overall aim of improving the theory and practice of policymaking through use of ideas about complexity? My reaction here is mixed. The book helps to expand horizons and contains many useful insights but, at the same time, I have been left with the sense that it misses its mark in at least two critical areas. The first is that it is tied too narrowly to a particular view of complexity and therefore that it fails to get to grips adequately with the ramifications of 'complexity theory'. The second is that some of the material in HCPP on improving the practice of policymaking seems to be inspired more by advocacy for the superiority of complexity-based thinking than by the actualities faced by policymakers.

My first point of criticism deserves an immediate qualification: it is unfair to charge a book whose forty authors almost certainly hold forty different understandings of 'complexity theory' – a diversity noted ruefully in the editors' concluding chapter – with failure to get to grips with the ramifications of the concept; across all authors, HCPP touches on many ramifications. However, there is still an issue of framing because of the way in which the overall approach in the book has been anchored on a narrow view of the nature of complexity.

This view comes from studies of complexity in the natural sciences[6] and is summarised in the editors' opening chapter: systems are complex when their behaviours as wholes are greater than the sums of the behaviours of their parts[7] and this complexity is characterised through the properties listed in my fifth paragraph. To a greater or lesser extent, this framing is also used by most of the other authors of HCPP so that the book's overall approach to complexity is tied into conceptual origins in the natural sciences[8].

This linkage has had both positive and negative effects. The positives have come from the infusion and use of new concepts, metaphors, simulations and mathematical models in the social sciences. Thus, much is made in HCPP of the way in which complexity thinking has freed these sciences from the limitations of reductionist worldviews based on assumptions about simple causal mechanisms, clear rules, certainty of information and the feasibility of rational decisionmaking. Indeed, many chapters

[3] For example, Shine (Ch.11) discusses various factors that help to create behavioural attractors and ways to deepen positive attractors and drain away the negative ones.

[4] A New Zealand touch is provided through Ch.23 by Wellington general practitioner, Dr Ben Gray, on the ways in which New Zealand public-health policies and plans worked in response to various epidemics and on the tensions between these plans and policies and the realities faced by frontline healthcare workers.

[5] Tenbensel (Ch.22) discussing application to health policy

[6] The 'natural sciences' referred to centre on physics but with admixtures from chemistry, biology and ecology. The understanding of complexity derived from studies in these disciplines came to prominence in the 1990s (a point noted by the editors) most famously through popularisations by writers such as Kauffman, Holland, Arthur and others with links to the Santa Fé Institute.

[7] This idea appears in several places in HCPP but is nowhere adequately explained. 'Sum' should be viewed as a metaphor because the mathematics involved is far removed from simple notions of addition. An introductory explanation is provided by Robert Rosen (1991), *Life Itself*, Columbia University Press.

[8] As several authors in HCPP note, the idea that social systems are, in some way, 'complex' long predates the transfer of concepts, metaphors and models from natural to social sciences that started mainly in the 1990s (footnote 6). The transfer appears to have been a starting point mainly because it popularised and, to a large degree, legitimised the idea of complexity in the social sciences.

illustrate the richer ways of thinking that unfold when these assumptions are muted or abandoned. Metaphors like 'attractors' to explain dynamics and 'small worlds' to characterise networks provide prominent examples of these ways of thinking. As noted above, these conjure up new mental models for the whats, hows and whys of social phenomena and they also open up new avenues for the design of research, for gathering and analysis of data, and for simulation modelling.

Unfortunately, these gains are offset by general looseness surrounding concepts related to complexity. Thus, HCPP is replete with terms like 'nonlinearity', 'self-organisation', 'path-dependency' and 'emergence', but these are rarely defined with precision or tied down to the specific features of particular systems under discussion. Instead, there are mainly multitudes of words, and these leave a prevailing sense that much of what passes for complexity thinking is vague, abstract, incoherent, high-level and in a state of flux. Looseness of this sort creates a poor foundation for the advancement of any science, and this is, indeed, one of the impressions left with me by HCPP.

Besides the multitudes of words, there are also gaps in the framework adopted from the natural sciences. These are noted in a good number of the chapters in the book. Thus the framework has little or no place for ideas like power, motivation, purpose, choice, values, beliefs, interests, culture, knowledge, anticipation, memory, learning, observers, levels of organisation, viewpoints or even, it would appear, for the characterisation of 'policies'. In other words, much of what matters in systems involving humans is left out.

These lacunae arise because of use of a natural-sciences view of complexity as a starting point. In essence, the sets of the entities and relationships required for the analysis of complexity in the natural sciences, their ontological stances ('what exists and needs to be considered'), are simply much smaller and much more restricted in type than those that are required in the social sciences. As a result, complexity as conceived in the former is too simple and too narrow for the latter[9].

Much the same criticism about over-simplification and narrowness can be made when it comes to the epistemological stances ('how to think about, or model, the behaviour of the things that exist') adopted in HCPP. These too are based predominantly on approaches used in the analysis of complexity in the natural sciences – network theory, agent-based modelling and, in particular, dynamical systems theory. The importance of the latter is shown by the fact that at least three of the five defining characteristics of complexity in the editors' introduction[10] arise out of models based on systems of differential equations. The drawback with these models as starting points is that they place the analysis of complexity in the social sciences close to the state-variable-based approaches of standard physics, a framing that is far too restricted to deal with the essentially relational concerns of the social sciences.

Approaches to complexity that focus on relationships rather than states are widespread in disciplines such as computer science, theoretical biology, quantum physics, and even business modelling, and these build upon the rich and deep mathematics of category theory[11]. Unfortunately, there is almost no hint in HCPP that these approaches exist. To this outsider, it seems as if a form of path-dependency is in operation: there has been an infusion into the social sciences of ideas about a particular type of complexity found in comparatively simple physical and biological systems and this has been deemed to be sufficient and has now acquired a self-contained and self-perpetuating life of its own. From this perspective, the insufficiency of the starting point is evident in shortfalls, illustrated in HCPP and noted above, in the form of loose and vague terminology and of ontological and epistemological gaps. It also seems clear from the shortfalls that many more infusions from outside are going to be required before there is something like an adequate version of complexity theory for use in applications within the social sciences.

My second area of reservation about HCPP is that, when it comes to applications to the processes of policymaking and policy implementation, there is an over-emphasis on advocacy for adoption of complexity-based thinking because of its supposed superiority over other approaches and an under-emphasis on the fact that policy processes are already complex and that the needs of policy makers and implementers therefore centre more on being able to 'navigate' across a spectrum of approaches than on adopting any single approach.

This reservation requires some unravelling. First, there is a central point that both the systems that policy makers deal with and the processes of policymaking and policy implementation are complex in the colloquial sense of the word – i.e. intricate and consisting of, or involving, many different and interwoven parts[12]. Because this interpretation differs from that generally used in HCPP it is worth distinguishing the two at this point by writing 'complex_{CQ}' when the colloquial sense is in use and 'complex_{NS}' when the sense carried over from studies of natural systems is in use[13].

The fact that the systems policy advisers and implementers deal with and the processes involved in policymaking and policy implementation are complex_{CQ} is fairly obvious – those involved

[9] This point is made in places within HCPP, notably by Room (Ch.2), Little (Ch.3) and Morçöl (Ch.6). Wider-ranging commentaries on the same issue in other disciplines can be found, amongst others, in Rosen (2000), *Essays on Life Itself,* Columbia University Press; Baianu & Poli (2011), *From simple to highly complex systems: a paradigm shift towards non-Abelian emergent system dynamics and meta-levels,* http://www.academia.edu/3600494/From_Simple_to_Highly_Complex_Systems_Baianu_Poli_

[10] The five characteristics are summarised in the fifth paragraph of this review. The three alluded to here are feedback effects, sensitivity to initial conditions, and behaviours such as convergence on phase-space attractors and punctuated equilibria.

[11] Category theory itself underpins much of mathematics and is wide-ranging, often highly abstract and generally hard for non-mathematicians to get to grips with. Unfortunately, the latter is unavoidable: complexity in the real world demands matching complexity in the tools used for thinking about it. *Category Theory for Scientists* by David Spivak (2013, http://ocw.mit.edu/courses/mathematics/18-s996-category-theory-for-scientists-spring-2013/textbook/) provides a relatively accessible introduction. Applications of category theory and the wider approaches to complexity alluded to are illustrated in: Goguen (1991), A categorical manifesto, *Mathematical Structures in Computer Science 1*: 49–67; Ehresmann & Vanbremeersch (2007), *Memory Evolutive Systems*, Elsevier; Louie (2009), *More Than Life Itself,* Ontos Verlag; Coeke (2008), *Introducing categories to the practicing physicist,* http://arxiv.org/pdf/0808.1032.pdf; Macfarlane (2011), *Category Theory and Business Modelling,* http://www.slideserve.com/loren/category-theory-and-business-modelling.

[12] 'Complex' is sometimes also used in policy circles to mean a problem is intractable with few avenues for progress in sight.

[13] As in the fifth paragraph in this review.

cannot avoid being aware of the multitudes of stakeholders, viewpoints, interests, uncertainties and incomplete understandings that surround issues and even the simplest of policies and policy implementations. But their job is to find a way through these thickets and, at least in the New Zealand context, they often do so on an ad-hoc basis with little reference to particular theories about system behaviour or policymaking and, even more rarely, with any reference to complexity$_{NS}$ theory. This leads to circles of miscommunication in dialogues between policy advisors and implementers and complexity theorists. Both groups agree that the world is 'complex' but there is a confusing mixture of overlap and gap between advisors' and implementers' somewhat inchoate conceptions of this term as complex$_{CQ}$ and complexity theorists' conceptions of it as complex$_{NS}$. The overlap comes when theorists offer complexity-based lists of principles for use by advisors and implementers[14] – many of the items in the lists seem already to be part of practices under complexity$_{CQ}$ and hence, barely new. The gap appears when theorists advocate use of concepts derived from models for complexity in natural systems – although sometimes liberating, as noted above in the case of Shine's use of attractors, many of them seem to be abstract and poorly connected to the practicalities of policymaking and hence, of little direct use.

This brings me to the issue of advocacy. In parts of HCPP there is a somewhat evangelical tone about the desirability of use of complexity theory driven by the belief that it provides superior explanatory power to alternatives and, most notably, to reductionism. I do not doubt the overall truth of this proposition, particularly if more comprehensive understandings of complexity are developed through better interactions with frontiers in other disciplines, as suggested above. But in the meantime, I can imagine the reaction of, say, a Minister of Finance to the suggestion that a 'holistic approach' involving a 'complexity lens' be used develop the annual budget or some similar area of policy. The point here is that reductionism, simple models and simple, direct, top-down policies are useful in some circumstances[15] despite the fact that underlying systems and processes are almost invariably complex in any of the senses of the word touched on in this review. Under these circumstances, direct advocacy for use of greater use of complexity ideas is not persuasive.

How then to connect complexity ideas more effectively to policymaking and implementation? The editors' answer, noted above, is in essence to chip away at the problem by demonstrating value through specific applications of the sort illustrated in many of the chapters of HCPP. This seems to me to be only part of the answer. The larger need is to develop better methods for 'navigating' through the thickets of policymaking and policy implementation alluded to above. Here, I have in mind elements such as (i) the use of more careful and complete descriptions of systems of interest, including their levels of organisation, so that there are explicit and enduring foundations for policies, (ii) the tracking of assumptions so that there is understanding of the ways in which possible models for behaviours and policies, ranging from simple to complex, fit together and (iii) the use of ideas such as subsidiarity and requisite variety so that there is some basis for balancing decisionmaking between top-down and bottom-up. Elements of this sort are central to any theory of complexity but are often missing in policy development and application[16]. Efforts to make them central components in these processes seems likely to be an effective way to increase the impacts of complexity theory on public policy.

[14] For example, by Price et al. (Ch.7) in HCPP and, locally, by Eppel, Matheson & Walton (2011), *Policy Quarterly 7(1)*: 48–55.

[15] The fact that reductionist simplifications are used, and are useful in policy making and implementation alongside other approaches is recognised in several chapters in HCPP. See, for example, Bovaid & Kenny (Ch.16) in local government, de Roo (Ch. 21) in urban planning, and Tenbensel (Ch.22) and Gray (Ch.23) in health systems. These and other chapters also outline reasons for the preference for reductionist-based policies ranging from the human preference for cognitive short-cuts to pressures surrounding communication and public accountability that politicians operate under.

[16] Readers of this journal have probably been puzzled by the ever-changing and amorphous nature of science policies in New Zealand over past decades. The continuing state of flux owes much to the lack of a complete and enduring framework for describing the science system, to disregard by ministers and managers of the need to understand assumptions and learn about their validity from past experience, and to the absence of consistent principles for determining who should have responsibility for research and allocation decisions at various levels of system organisation.

Veronika Meduna

Towards a Warmer World:
What climate change will mean for New Zealand's future

Ralph Chapman

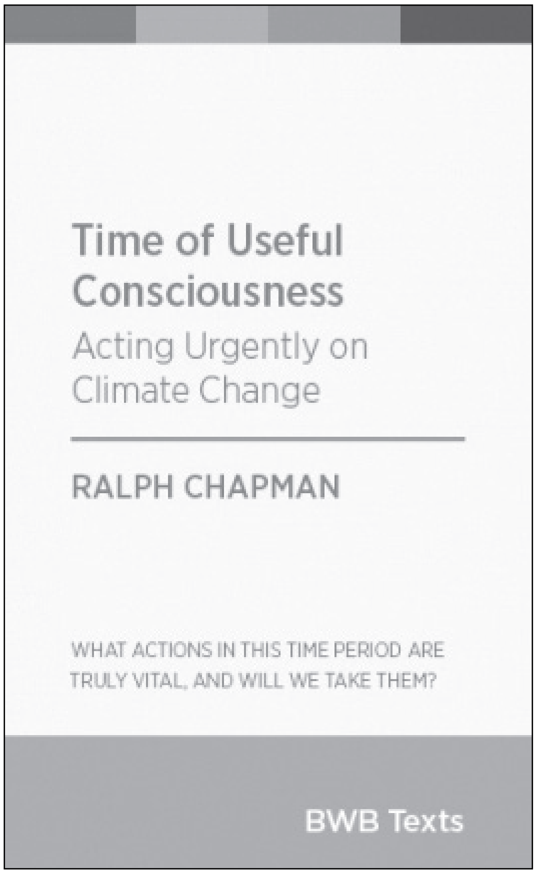
Time of Useful Consciousness:
Acting urgently on climate change

Reviewed by Geoff Gregory*

These two books are in the series from Bridget Williams Books entitled BWB Texts, described as 'short books on big subjects from great New Zealand writers'. They complement each other in describing what climate change will mean for our future and what actions are needed now to reduce the causes and mitigate the effects of climate change.

Veronica Meduna, a prize-winning science writer, describes the research results of experts in many disciplines which together show clearly that the huge burden of carbon dioxide released by human activities is resulting in heating of the oceans and atmosphere as well as the land. Besides melting ice shelves and glaciers and raising sea levels, this is changing large-scale weather patterns (causing storms and droughts) and ocean currents, and this in turn is affecting the survival of our agricultural industries and native species and threatening communities and development in coastal and low-lying regions and on islands.

One of many pertinent facts reported in this book is that the last time global warming of more than 2°C above that experienced in recent decades was in the last interglacial, 125 thousand years ago, when humans were hunter/gatherers and naturally occurring global warming raised sea levels by 6–10 metres. This is what we can expect in the not too distant future as human-generated warming approaches the same benchmark. Veronika Meduna relates this to the future faced by New Zealand and its neighbouring small island states, and suggests that flexible adaptive management strategies will be needed at all levels of political planning.



Professor Ralph Chapman, a negotiator for the New Zealand government on the Kyoto Protocol from which the recent UN Climate Change Conference in Paris arose, considers that a combination of 'tipping points', or points of no return, such as melting of the Greenland, West Antarctic, and East Antarctic ice sheets, disrupted climate systems, and accumulating costs of

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departing from the current global economic trajectory and its associated social patterns of production and consumption, will be unlikely to prevent a 3°C rise in global warming by 2100. He expresses the urgent need for New Zealand to have a clear, coherent and integrated strategy for assisting communities to introduce low-carbon initiatives. The strategy will also have to handle 'obdurate resistance' from fossil fuel industries, and roading and housing developers, and 'more diffuse resistance from those more generally lacking awareness of the dizzying rate of change of the science and projected impacts of climate change'.

Note that both books were published before the recent Paris Agreement, in which countries committed to 'holding the increase in the global average temperature to well below 2°C above pre-industrial levels' ['pre-industrial levels' being approximately 0.9°C below present temperatures, I believe]. I suspect that, while commending the not-insignificant achievement of 196 nations reaching an agreement, Professor Chapman might be disappointed by the muted pledges by some of the developed nations, including New Zealand. However, he warns against allowing pessimistic thinking to set in. He assesses the situation as being far from helpless, and points to available or emerging technological and policy solutions that can be applied. He asks: 'Is it fair to future generations to sleepwalk into climate instability, essentially for a very temporary extension of this generation's current prosperity?' and urges us to 'act smartly, in both senses of the word', while there is still time.

Both books are well referenced with authoritative research papers and reports, and Veronika Meduna's book has interviews with local experts. They are inexpensive and can be bought as ebooks. Both should be compulsory reading for policymakers, and could lend direction to public awareness programmes and political action.

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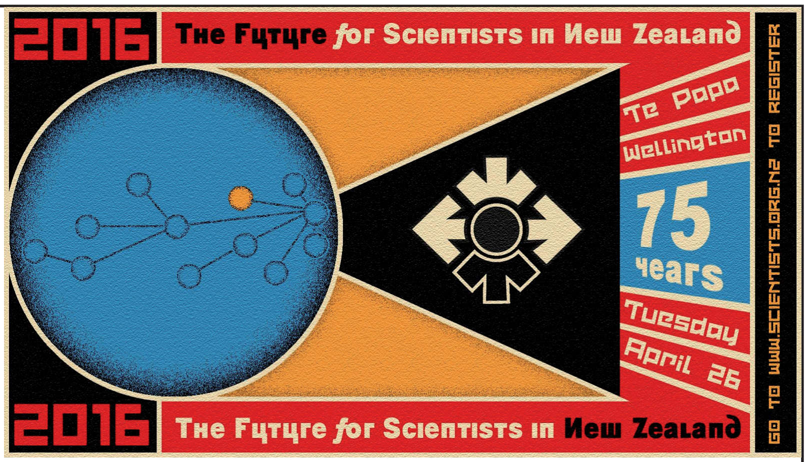

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Professor Shaun Hendy, Professor of Physics at the University of Auckland, author of *Get off the Grass* (2013) and *Silencing Scientists* (2016), and Past President of the New Zealand Association of Scientists

Dr George Slim, Research consultant, Rhadegund Life Sciences

Bernard Beckett, teacher at Hutt Valley High School and author of books for young adults, including *Genesis* (2007) and the non-fiction *Falling for Science* (2007)

Kate Hannah, Executive Manager of Te Pūnaha Matatini, a New Zealand Centre of Research Excellence focussed on the study of complex systems and networks

Professor Wendy Nelson, NIWA's Coasts & Oceans National Centre, the University of Auckland, and Chair of the Royal Society Expert Advice Panel on Taxonomic Collections in New Zealand

Professor Steve Pointing, AUT and the Sci21 project

Dr John Perrott, Senior Lecturer at AUT, Mātauranga Māori Advisor to the Department of Conservation Specialist Advisory Group

Dr Hamish Campbell, Senior Scientist at GNS Science, Science Communicator at Te Papa Tongarewa, and Past President of the New Zealand Association of Scientists

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