

Norman Taylor memorial lecture 2002

The time machine

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Introduction

I'll begin by thanking our President, Dr Alan Palmer, for his introduction and especially for inviting me to give this year's Norman Taylor Memorial Lecture at Victoria University of Wellington. I feel greatly honoured and humbled when I look over the names of my 25 illustrious predecessors.

My talk commemorates the achievements and character of Norman Taylor, our Society's founding 'father'. However, rather than beginning with Taylor, as usually occurs, I intend starting by going back in geological time and then travelling forward to the present by reviewing a series of research topics I've been involved with. I should get to 1952—the year our Society began—about two-thirds through the talk, and I will pay tribute to Taylor then.

I have devised a suitable vehicle for this journey through

time, namely tephra, a central focus of my research over the past ~25 years. 'Tephra' is a collective term for all the explosively ejected, unconsolidated pyroclastic products of a volcanic eruption (Lowe & Hunt, 2001). It encompasses volcanic ash, lapilli, blocks and bombs, which strictly are grain-size terms. Tephra layers can be dated directly or indirectly and therefore provide time planes (or isochrons) across the land and oceans. My interest in tephra,

sparked by my mentor Professor Emeritus John McCraw, has enabled me to branch out in various research directions including studies of environmental change, pedology, and an amalgam of these in palaeopedology (Fig. 1). I have interests in dating and volcanology as well. Although tephra makes up the 'chassis' of my time machine, the critical driving force is provided by the discipline of stratigraphy (Fig. 1). 'Stratigraphy' is the study of layered deposits and their sequence in time. It has been essential to my research because it has allowed me to move between geology and pedology in the zone described by John McCraw as 'No Man's Land' in the 1978 Taylor Lecture (McCraw, 1979).

I've chosen to talk about five topics dealing with long-standing pedological or paleoenvironmental problems involving tephrostratigraphy. I'll begin with the oldest tephra and work 'stratigraphically upwards' to the youngest. Along the way I'll mention or show a few of the

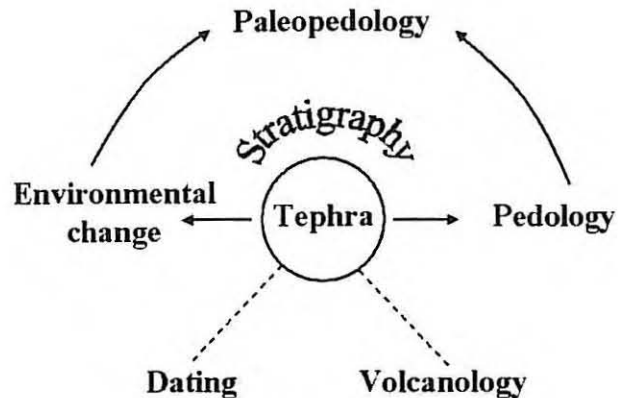


Fig. 1. Tephra and related research interests. Stratigraphy provided the means for me to undertake both geological and pedological research, and is the 'driving force' for the tephra-based 'time machine'.

people I've worked with, or who were involved historically in the research areas. I've allowed around five or six minutes for each topic (but I'd be grateful if you didn't set your stopwatches!). As promised, the second part of the talk will begin when we reach the year 1952. Among a range of personal observations relating to the birth of the society and to Norman Taylor, I will attempt to explain how I came to be giving this lecture today.

I have devised a suitable vehicle for this journey through time, namely tephra ...

Differentiating the undifferentiated (2,300,000 years ago)

Over much of the North Island there are thick sequences of strongly weathered and clayey tephra beds and paleosols. 'Paleosols' are defined as soils of

a landscape or environment of the past (Newnham et al., 1999). The sequences were usually lumped together on maps simply as 'undifferentiated brown tuffs'. In the Waikato region, the topmost deposits are known colloquially as the Hamilton beds (following Grange & Taylor, 1931; Ward, 1967), and the lowermost as the Kauroa beds (Ward, 1967; Pain, 1975; Lowe et al., 2001). The sequence of Kauroa beds stretches to twelve metres in total. I well remember Harry Gibbs and John McCraw asking, three decades ago, 'What is the origin of these beds and how old are they?'

Getting the answer has not been easy. Various people have tried (e.g. Pain, 1975; Salter, 1979; Stevens & Vucetich, 1985; Briggs et al., 1989) but always the great difficulty was that weathering had obliterated most of the primary minerals and hence the means of getting the evidence we needed. However, chance remarks (by Brad Pillans and John

Westgate) made during an Inter-INQUA conference field trip in 1994 (Briggs et al. 1994) provided the key to cracking the problem. I persuaded Jo Horrocks, a graduate fresh from Oxford, to work on the Kauroa beds for her Ph.D.

Jo did did some very clever things. Firstly, she measured paleomagnetic signals, after being trained up by Brad Pillans, and quite remarkably obtained evidence of changes in magnetic polarity through the sequence that matched well-dated global signals (Horrocks, 2000). Secondly, we obtained fission-track dates from rare but resistant grains of zircon, thanks to Peter Kamp's expertise in this technique (Lowe et al., 2001). And finally, Jo made the breakthrough we'd been looking for. Normally, the most powerful tool we can use to identify and fingerprint tephras is the electron microprobe. The probe provides major element analyses of volcanic glass, but in the Kauroa beds all traces of glass had long been removed by weathering. However, we knew they contained very rare quartz grains and so, on a hunch (following new work by Delano et al. 1994), Jo set about extracting them in a tortuous job rather like finding the proverbial needle in a haystack. When the quartz grains were cut open we were excited to find a core of pristine glass, representing the original magma, preserved and sealed within them (Horrocks et al. 1999). These glass inclusions meant that we could turn to the electron probe after all to

fingerprint the Kauroa beds, and to match them with known eruptives in other parts of the North Island. Jo did this and at last we had some answers (Horrocks, 2000).

The combined evidence showed the base of the Kauroa sequence to be 2.3 million years old, and the top about 0.8 million years old. Most of the beds had originated from early eruptions in the Taupo Volcanic Zone (Mangakino volcano). We also discovered that about fifteen percent of the deposits were not tephra but instead were wind-blown sediment, or loess, including the oldest documented in New Zealand at 1.7 million years (Horrocks, 2000). Our new data allowed us to tie the deposits to glacial and interglacial units in the Wanganui Basin (e.g. Naish et al., 1996). What was really interesting, though, was that we found we could also correlate the Kauroa sequence to the long climatic record of loess and paleosols in China (e.g. Kukla et al., 1988; Derbyshire et al., 1995). There, loess deposits correspond to cold, dry glacial conditions whereas paleosols correspond to warm, wet interglacial conditions during which loess accumulation largely 'switches off'. The question therefore arose as to why the Kauroa sequence, most of which is a volcanic record remember, should match up with the climate record of far-off China. Volcanic eruptions are unrelated to climate yet we seemed to have a glacial-interglacial signal in the Kauroa beds.

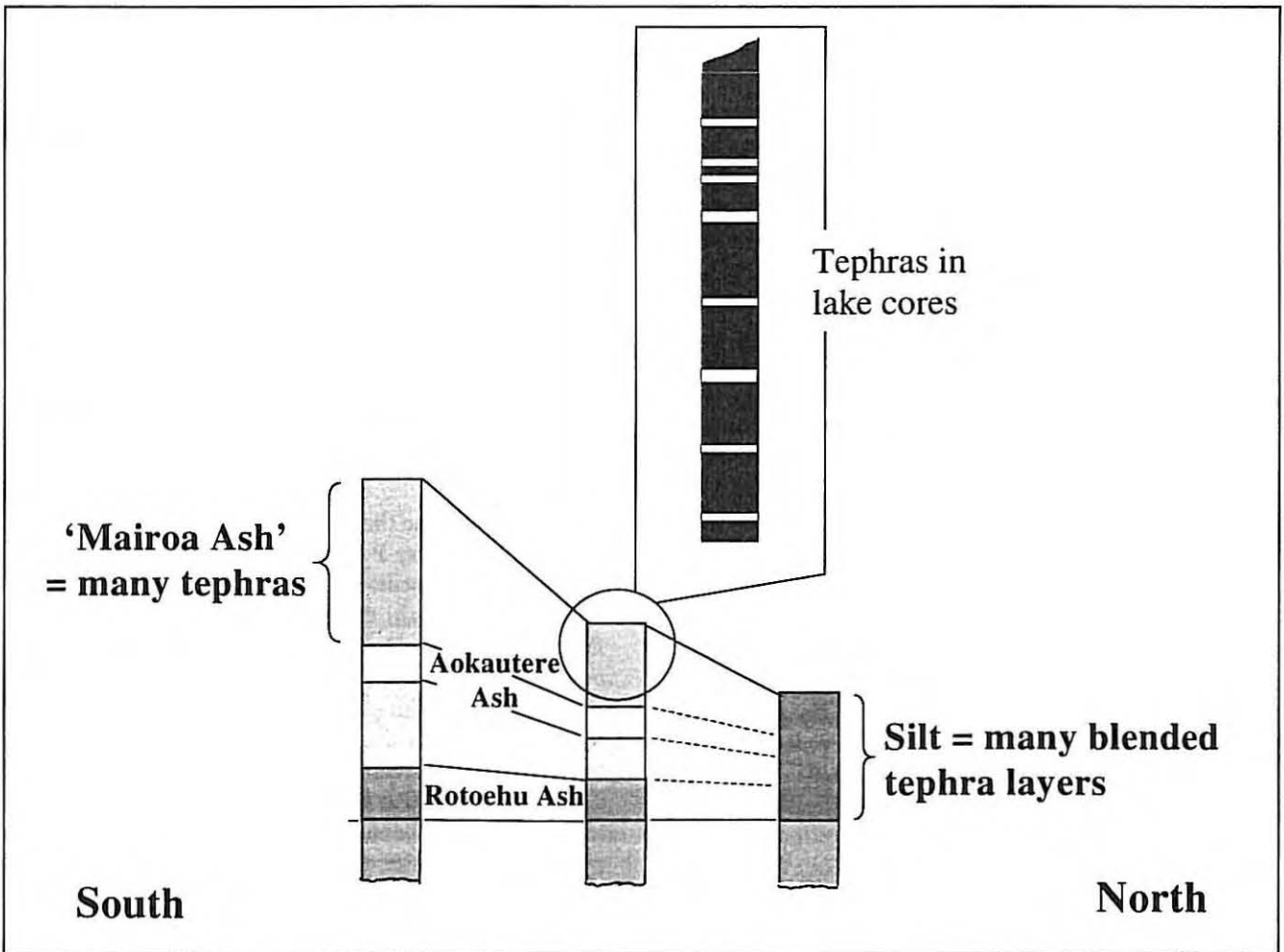


Fig. 2. Tephra-soil transect, Hamilton Basin, showing stratigraphic relationships and soil parent materials. Marker beds are distinguishable in the south but around Hamilton and further north the tephra become intermixed and marker beds are indistinguishable. Aokautere Ash is c. 26,500 yrs old and Rotoehu Ash c. 55,000 yrs old. A paleosol on Hamilton Ash underlies the profiles. Lake cores indicate likely contributions to upper soil profiles of multi-sourced tephra. After Lowe (1986, 1991) and Selby & Lowe (1992).

Our explanation is that eruptions have been so frequent that tephra accumulation has been effectively continuous, behaving like loess but without ever 'switching off'. While the Kauroa beds built steadily upwards through doses of tephra fall, the rate of soil formation increased markedly during interglacials ('developmental upbuilding': Lowe, 2000). The Kauroa paleosols therefore represent the imprint of enhanced soil formation during the warm, wet interglacial conditions, the ongoing accumulation of tephra remaining sufficiently slow (most of the time) to allow pedogenesis to proceed. During glacials, tephra kept falling but soil formation was much slower. Only at some points were conditions right for genuine loess to accumulate as well as tephra (Horrocks, 2000).

That's the end of my first topic, and I'll now move forward in time to the second.

The 'hole' in the ashes (55,000 years ago)

John McCraw came up with another problem, this time the puzzling 'hole in the ashes' in the northern Hamilton Basin. In the early 1930s the name 'Mairoa Shower', later called 'Mairoa Ash', had been applied by Grange and Taylor to friable, allophanic soils mapped to the south of Hamilton (Grange, 1931; Taylor, 1930, 1933; Grange & Taylor, 1932). However, the Mairoa Ash evidently petered out at Hamilton, giving way instead to a silt deposit dominated by the sticky clay, halloysite (Grange et al., 1939; McCraw, 1967; Bruce, 1979).

Because the Mairoa Ash occurred on uplands surrounding the Hamilton Basin, there seemed to be some sort of 'Bermuda Triangle' over the northern part where it was missing. John McCraw went a step further and suggested that the silt might be loess blown from the surface of the active braids and bars of the old Waikato River during the Last Glacial Maximum (McCraw, 1967).

So, my job was to work out the origin of the silt and to explain the 'missing' Mairoa Ash. At this point you must realise that ~25 years ago the model for the weathering of tephra was based around time: allophane formed first and then converted to halloysite 10,000–15,000 years or so later (e.g. Kirkman, 1975; see also review by Lowe, 1986). Consequently, the allophanic soils formed on Mairoa Ash were regarded as being much younger than the halloysitic soils formed on the silt.

Well, was there a 'hole' and was the silt loess? In a word, no. The most compelling evidence was the fantastic record we got from coring lakes in the region. The cores revealed dozens of thin tephra layers preserved in the organic lake sediments (Lowe, 1988). It was clear that John McCraw's silt was a highly condensed column of tephra layers, all blended together by soil processes. As well, it was evident that the 'Mairoa Ash' was made up of a similar blending of multiple tephra. Other work showed that older tephra were also present in both sequences, with Rotoehu Ash, erupted about 55,000 years ago, forming the base (Fig. 2).

So, having established the stratigraphy and age of the parent materials across the Basin, we were faced with another question: why were the soils so different? Putting it another way, why was one allophanic and the other halloysitic? Bill Ward had earlier latched on to the right idea (Ward, 1967), but getting a definitive answer involved piecing

together new bits of evidence. The key pieces were knowing how allophane could be quantified in soils, and understanding its structure (Parfitt et al. 1980; Parfitt & Wilson, 1985; Parfitt & Childs, 1988; Parfitt, 1990a; Lowe & Percival, 1993). These topics themselves would be worthy of an entire lecture, not to mention the comparable work on ferrihydrite, but to me a benchmark paper was that published in 1983. Roger Parfitt and Gary Orbell, together with M. Russell, showed that there is a rainfall-based leaching gradient from south to north across the Waikato region (Parfitt et al. 1983). By using a careful stratigraphic approach, they proposed that where silicon leaching was high in the south, allophane had formed, but where it was low in the north, halloysite had formed (Fig. 3).

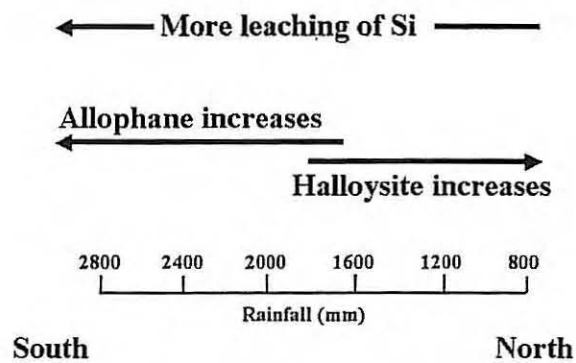


Fig. 3. Simplified rainfall-based Si-leaching model for the formation of Al-rich allophane and halloysite from weathering of tephra in the Waikato region (after Parfitt et al., 1983). A more comprehensive model is given by Lowe (1986) and Lowe & Percival (1993).

Their model was a big advance because as well as explaining the differences between the soils, it explained why some occurrences of allophane or halloysite had seemed 'out-of-step' with the time-based model. These ideas were supported by other work (e.g. Parfitt et al., 1984; Lowe, 1986, 1995; Parfitt, 1990b).

The story didn't end there. In 1989, Peter Singleton, Malcolm McLeod, and Harry Percival tested the leaching model by actually measuring silicon in soil solution in the well-known Horotiu-Te Kowahi drainage sequence (Singleton et al., 1989). They confirmed the model by showing that in the poorly drained Te Kowahi soil, silicon was high and so there was no allophane, but in the well-drained Horotiu soil, silicon was low and therefore allophane content was high. Furthermore, the threshold value of about 10 parts per million silicon in soil solution beautifully matched thermodynamic stability diagrams because below 10 ppm, allophane is more stable than halloysite, whereas above 10 ppm the reverse is true and halloysite is more stable (Percival, 1985; Singleton et al., 1989; Lowe & Percival, 1993; Lowe, 1995).

Just before I move on, I wish to go back to Gary Orbell, pictured a few moments ago. Gary employed me in the Hamilton office of Soil Bureau way back in the hot summer of 1976–77, mainly to work on the Matamata County survey. Gary was a terrific boss, and I have always appreciated the help he gave me in starting out on my career.

Climatic change and challenging kinetics (17,600 years ago)

For this topic I wish to introduce two of our Society's most colourful figures, Colin Vucetich and the late Alan Pullar. Vucetich and Pullar, a famous 'duo' rather like Grange and Taylor before them, set the scene for tephra studies in New Zealand with their work from the 1950s to the 1970s (e.g. Vucetich & Pullar, 1964, 1969, 1973). Their mapping was often undertaken on what they called 'secret correlation missions' because of Soil Bureau's ban on such activities at the time (Vucetich, 1977a, 1977b; Lowe, 1990). Pullar described himself in 1973 as a 'fringe man' in that he made contributions to various disciplines through soil surveying (Pullar, 1973). In other words, when he dug a pit or examined a cutting, the profile told him more than one story. An example of this was the recognition by Vucetich and Pullar early in the 1960s of a climate signal in the deposits they were examining in central North Island (Vucetich & Pullar 1963, 1969; Vucetich, 1968). They inferred from various features, including the occurrence of loess and the amount of paleosol development, that climate had changed markedly during the past 20,000 years. The transition was invariably marked by the Rerewhakaaitu Tephra, one of the suite of tephra layers Vucetich and Pullar had been mapping.

More than 30 years later, the precise timing of this transition, from glacial to interglacial conditions, is again being debated, chiefly to find out if it is globally synchronous or not. We know that high latitudes in the Northern Hemisphere received increasing heat from the Sun because of changes in the Earth's orbit but we don't know if this heat was transmitted to the Southern Hemisphere by ocean currents or through the atmosphere. The only way to find out is to link paleoclimate data from the land and the oceans to an instant in time common to both records, and of course certain tephra layers can provide this critical link (Newnham et al., 1999).

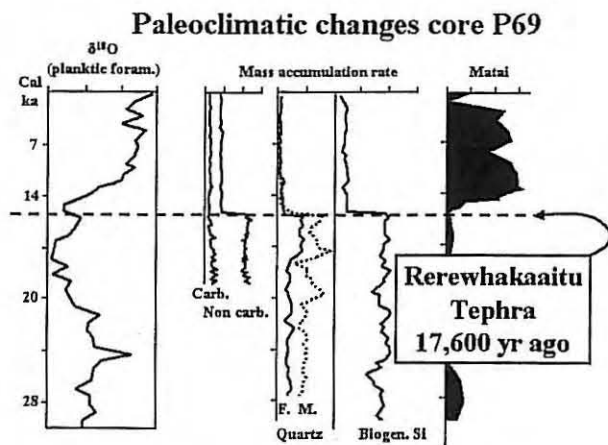


Fig. 4. Marine-based records of climatic change from core P69 showing abrupt change at the time of eruption of Rerewhakaaitu Tephra. Oxygen isotope curve is for foraminifera *Globigerina bulloides*; mass accumulation rates apply to carbonates and non carbonates, fine (5–2 mm) and medium (63–20 mm) sized quartz, and biogenic silica; and the pollen curve is for *Prumnopitys taxifolia* (after Newnham et al., 2003).

Figure 4 shows various indices used to infer paleoclimatic change from marine core P69, taken east of North Island. Bob Stewart and Vince Neall published some of these indices in 1984 (Stewart & Neall, 1984) but others have since carried out further work (Carter et al. 2000; Nelson et al., 2000; McGlone, 2001). The main point of the figure is to show that the obvious abrupt change occurred right at the time Rerewhakaaitu Tephra was erupted 17,600 years ago (Newnham et al., 2003).

Back on land, the same abrupt change is found in pollen records from lakes containing this same tephra (Fig. 5; see also Birrell & Pullar, 1973; McGlone et al. 1984; Kennedy, 1988). This figure shows a temperature reconstruction based on the pollen record since 20,000 years ago. It was drawn by my U.K.-based colleague, Rewi Newnham. Alongside the pollen-based curve is one we derived quite independently using a kinetic weathering model developed from studies of paleosols on tephra near Rotorua (Hodder et al., 1990). The novel work that led to this 'kinetic thermometer' was undertaken by one of my early MSc students, Brent Green. Brent was co-supervised by Peter Hodder and both thrived on the challenge of understanding the kinetics of glass weathering and clay formation. I thrived on the challenge of trying to understand all the equations!

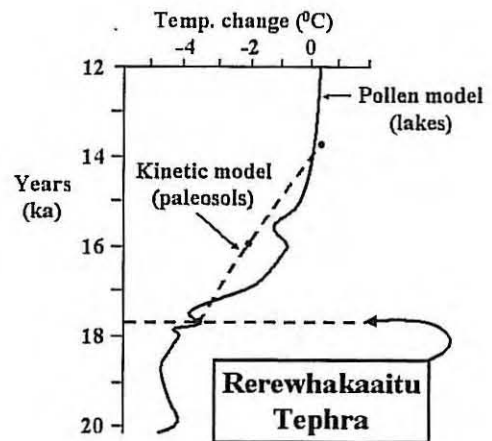


Fig. 5. Land-based records of temperature change reconstructed from pollen records for Lake Rotomanuka (near Hamilton) and from a kinetic weathering model for tephra-derived paleosols near Rotorua (after Newnham et al., 2003).

So, putting together all the land and ocean data, have we an answer to the original question on the timing and thus mechanism of climate change? Our evidence shows the abrupt change 17,600 years ago in the New Zealand region is, in fact, synchronous with similar change in the Northern Hemisphere. This means that the high-latitude warming in the Northern Hemisphere was transmitted quickly through the atmosphere rather than by slow ocean currents, driving simultaneous ice retreat and melting in both hemispheres (Newnham et al., 2003).

We have advanced since Vucetich and Pullar first put up their hypothesis, but it's taken a lot of detailed work across a wide range of disciplines—and the link between these has been tephrochronology as I've tried to show.

Big bang and big myth? (late summer–early autumn, ca. 232 AD)

The famous Taupo eruption around 232 AD (Sparks et al., 1995; Lowe & de Lange, 2000) forms the basis of my next topic. Volcanologists tell us that it was the world's most powerful eruption for the past 5000 years (Walker, 1980; Wilson & Walker, 1985; Wilson, 1993; Smith & Houghton, 1995). It evolved over several phases generating widespread tephra fall and then reached a climax with the extremely violent emplacement of the Taupo Ignimbrite when the very high ('ultraplinian') eruption column collapsed (Wilson, 1985). Ignimbrite, literally 'fiery storm-clouds', is the name given to the products of pyroclastic flows. These flows (or density currents) consist of ground-hugging clouds of scorching hot gas laden with shattered pumice and other particles. The ignimbrite raced away from Lake Taupo at around 700 kilometres per hour or more, reaching distances up to 80 km away in barely seven minutes. The flow flattened forests and incinerated everything in its path, and carbonised logs are common. It also flattened the Pureora-Bennydale forest which was then nicely preserved by peat formed as a result of changes to drainage (Clarkson et al., 1988; Palmer et al., 1988).

The pressure waves formed by the collapsing eruption column were so powerful we think they generated an atmospheric (more strictly, a volcano-meteorological) tsunami that travelled right around the world—amazing but probably true because a similar type of tsunami was formed during the Krakatau eruption of 1883 (Lowe & de Lange, 2000; Fig. 6).

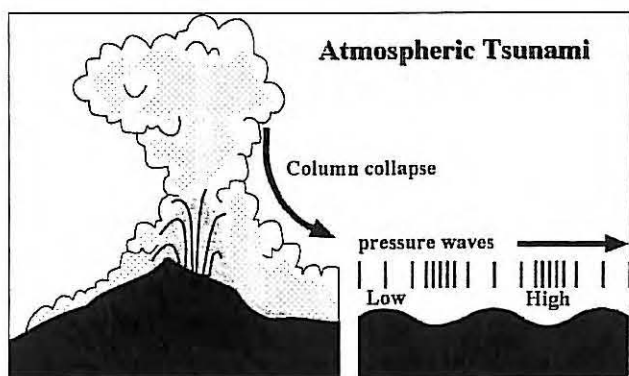


Fig. 6. Model for generation of a volcanically-induced meteorological tsunami by atmospheric phase coupling during collapse of the Taupo eruption column in ca. 232 AD. The eruption column displaces a large volume of air laterally, generating pressure waves in the atmosphere; high and low pressures depress and raise the water surface, respectively (after Lowe & de Lange, 2000).

From a soil-forming viewpoint, the eruption generated a new parent material for about 20 percent of the North Island (Lowe et al., 2000a). The dietary problems for sheep and cattle arising from inherent cobalt and other nutrient deficiencies in the Taupo-derived soils are well known. However, an enduring myth about these soils has developed alongside these defects. Last July, I visited the Putaruru Timber Museum with graduates en-route to the Mamaku Plateau. We saw an old 1950s film that emphasised the barren and infertile nature of the soils in the central volcanic region. It was claimed that the soils were so inadequate

they were unable to grow native forest after the devastation wrought by the Taupo eruption. This idea that the forest was unable to recover is still around today—it's even in the 'The Living Mantle' (Molloy, 1998).

Stands of mature native forest, growing quite happily on Pumice or Podzol Soils (e.g. Tihoi soils) developed on Taupo Ignimbrite, suggest that this idea is flawed. It probably arose because much of the Taupo landscape that greeted European explorers was scrubby and treeless. William Colenso, said to be a 'forceful and often obdurate man', described the area in 1841 as 'an interminable succession of dry and barren hills of broken lava, pumice and ashes...where the stunted vegetation was all but burnt up with the exceeding heat of the Sun's rays'. There was evidence in the soils themselves of a forest imprint. Taylor's discovery of podzols on Taupo eruptives (Vucetich, 1983) led to the construction of the Taupo vegetation—rainfall leaching suite (Daly & Rijkse, 1974; Rijkse & Daly, 1981). Conclusive evidence of forest recovery was not published until 1996, however. Pollen specialists Janet Wilmshurst and Matt McGlone examined peat bogs in central North Island and found that at most sites the native forest had fully recovered within 100 years of the eruption (Wilmshurst & McGlone, 1996; Horrocks & Ogden, 1998). It seems that even after an event as violent as the Taupo eruption, the forest was able to recover, as it had obviously done after many previous eruptions (Clarkson et al., 1995). So, the question arises as to why there were large tracts of scrub and fern in the Taupo region when the Europeans arrived. If neither the Taupo eruption nor the 'exceedingly hot Sun' were to blame, what was? The answer is the subject of my final topic, which I've called 'Landnám'.

Landnám (winter, ca. 1314 AD)

From the time of first human settlement, much of the New Zealand landscape has been drastically modified, the rate of change possibly the fastest in the world (Newnham et al., 1999). Prior to humans, about 85% of New Zealand was forested but by the time of European settlement forest cover had been reduced to about 55% through Polynesian burning (McGlone, 1989; Page & Trustrum, 1997; Wilmshurst, 1997; Ogden et al., 1998). Over the past 200 years it has been further reduced to 25% today.

When was the initial Polynesian settlement of New Zealand? Did forest burning start at the same time? Tephra have helped to answer these difficult questions, as we'll see next. Three Maori ovens have been found on the slopes of Mt Taranaki. The tephra sandwiching the cooking stones date the ovens to about 1450 AD, so we know people were in Taranaki by then (Alloway et al., 1990; Lowe et al., 2000b). We know that the eruption of Rangitoto, around 1400 AD, was definitely witnessed because human footprints were preserved in the ash-fall on nearby Motutapu Island (Lowe et al., 2000b).

But it is the widespread Kaharoa Tephra, erupted from Mt Tarawera, that provides the critical 'settlement layer'. In Iceland, the tephra marking settlement there, around 870 AD, is called the *landnám* tephra, meaning the layer that coincides with human impacts relating to permanent settlement. The Kaharoa fulfils this function as the *landnám* tephra of New Zealand in two ways (Newnham et al., 1998;

Lowe et al., 2000b, 2002). Firstly, no artefacts have ever been found beneath it. This means that features such as shell middens at Papamoa (illustrated in the talk) must be younger than Kaharoa Tephra. Secondly, pollen profiles from peat provide indirect evidence of human impact in the form of a rise in bracken that coincides with tree decline (Fig. 7).

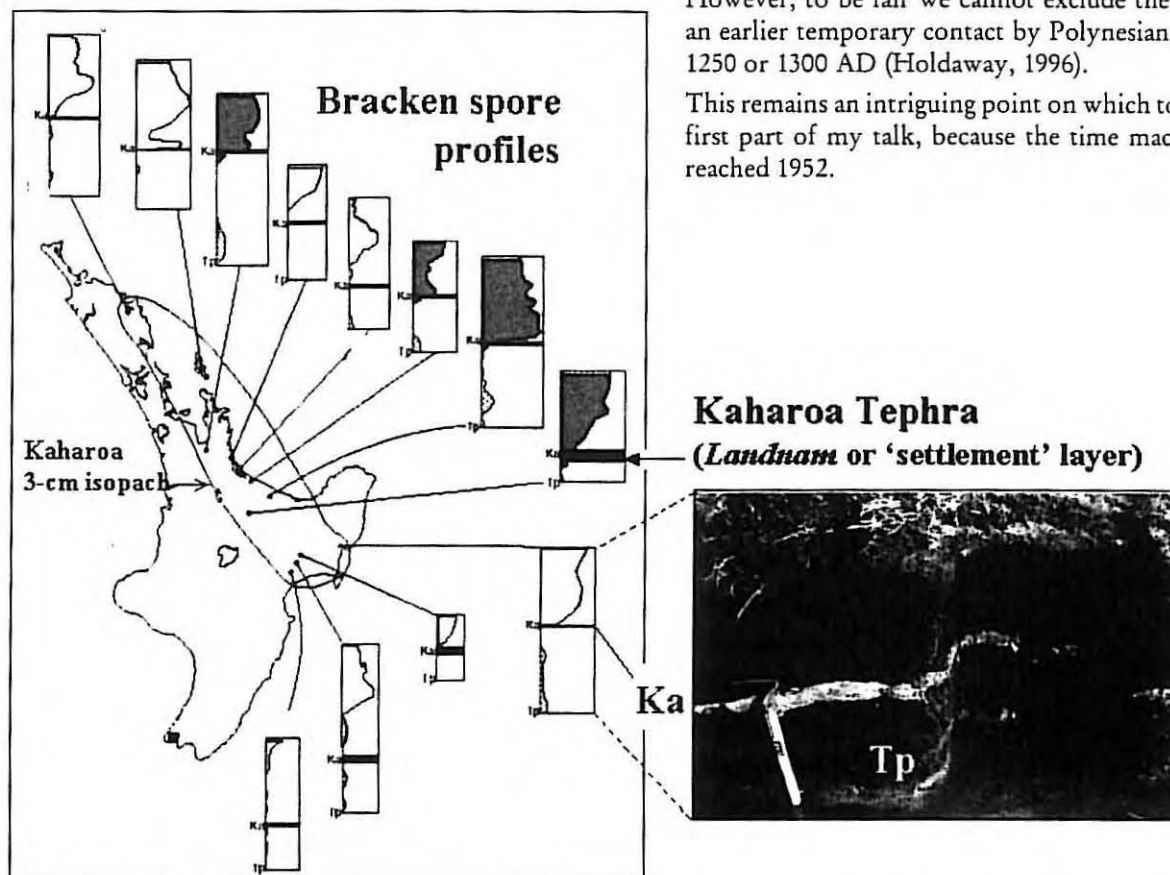


Fig. 7. *Pteridium* (bracken) spore profiles from North Island peat sites containing the ca. 1314 AD Kaharoa Tephra datum. The earliest inferred Polynesian deforestation signal occurs just before Kaharoa in four profiles (dark shading), probably in the late 13th Century. Taupo Tephra (Tp) provides a pre-impact datum in most profiles. After Newnham et al. (1998) and Lowe et al. (2002, in press).

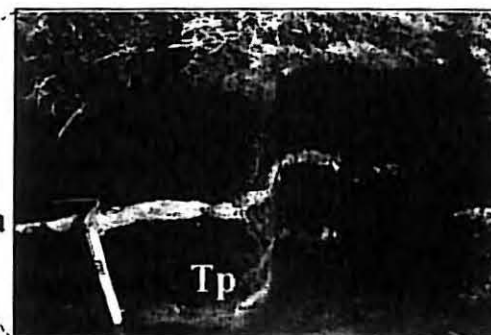
Bracken is used widely as an indicator for the disturbance of forest (McGlone, 1989; Newnham et al., 1998; McGlone & Wilshurst, 1999). A large, sustained rise, invariably with charcoal as well, is likely the result of repeated burning by people. In Figure 7, most of the profiles show that bracken starts to rise dramatically at around the time Kaharoa Tephra fell. If you look closely, you can see that at four sites the increase in bracken starts just below the Kaharoa Tephra layer, which means that burning began a short time before the eruption. Our conclusion therefore is that forest burning by early Polynesians began a few decades before the Kaharoa eruption (Lowe et al., 2000b, in press).

Clearly we now need an accurate date to complete the picture, but it's been hard to pin down. Until recently, the best we could get using radiocarbon was somewhere between 1290 and 1400 AD (Lowe et al., 1998). But, in the last twelve months we have applied a new tree-ring-based technique called 'wiggles-matching' to the problem. This method showed that the eruption took place during winter of the year 1314 AD, plus or minus 12 years (Hogg et al.,

2003). This date puts the earliest deforestation signal, and by implication, earliest settlement, in the second half of the 13th century. As it turns out, the oldest archaeological sites in New Zealand are reliably dated to this time also—that is, between 1250 and 1300 AD (Higham & Hogg, 1997; Higham et al., 1999)—and so we conclude that settlement and deforestation in New Zealand were more-or-less coincident (Hogg et al., 2003; Lowe et al., in press). However, to be fair we cannot exclude the possibility of an earlier temporary contact by Polynesian sailors before 1250 or 1300 AD (Holdaway, 1996).

This remains an intriguing point on which to conclude the first part of my talk, because the time machine has now reached 1952.

Kaharoa Tephra (*Landnam* or 'settlement' layer)



27 August, 1952: Birth of our society and personal reflections

Our Society was born in Wellington on this date, with Norman Taylor elected Foundation President. I'd like to stay in 1952 for a little while, and so I've compiled a short list of various events that took place around the world and in New Zealand in that year. I'll start with the world and then move on to New Zealand (Table 1).

The opening of the veterinary hospital in Waipukuarau might seem rather obscure to you, but to me it's not because the veterinarian was my late father, John Lowe. I mention my father so that I can lead into 1953 and two further births in August of that year. The first issue of *Soil News* appeared in August, 1953, and by good fortune so did I (8 August). I've discovered that I'm exactly the same age as our newsletter, to the month. I wonder which of us is in better shape 49 years on?

In reading that first issue, I was struck first by the clarity of writing. The editorial by Bruce Miller, one of our special guests here today, emphasised this point by writing (Miller, 1953):

Table 1. The world and New Zealand in 1952¹.

World	New Zealand
Allies bomb Korean city of Suan	Sid Holland PM (since 1949)
Decisive French attack on Viet Minh (near Saigon)	Sir Willoughby Norrie appointed Governor General
George VI dies ² , QE II accedes to throne	Yvette Williams wins gold at Helsinki Olympics
Eisenhower elected US President, Nixon VP	Athelete John Walker born
Japan, W. Germany become independent	Rimutaka Tunnel collapses (1 death)
1 st British atomic bomb test in Australia	ANZUS Pact signed
Czech Emil Zatopek wins 3 golds at Helsinki Olympics	N.Z. population reaches 2 million in Dec (NI 1.4M, SI 0.6M)
Sony make 1 st pocket transistor radio	'Civilian Saucer Investigations' group forms ³
Cheese ration cut to 1 oz/week in U.K.	Veterinary hospital opens in Waipukurau
Oxford wins Boat Race in blizzard (apparently)	

1. Main source: Ross (2000)
2. Before George VI's death, King Farouk of Egypt abdicated. At the time (March) he stated: 'There will soon be only five kings left—the Kings of England, Diamonds, Hearts, Spades, and Clubs.'
3. This UFO-spotting group is still in existence and apparently has about 30 members (most in Tauranga).

'As we do not want a large and pretentious publication all contributors are asked to write clearly and concisely. Our aim is to give members material that is topical and interesting and at the same time useful in their work and reference.'

Perfectly expressed, wouldn't you say? The second point was that the Society's subscription rate was set at nine shillings. To our younger members, 90 cents might not sound much but 9s in 1953 was a fair whack. On looking closer I found that the sub was for membership of both our Society, at 1s 9d, and of the International Society of Soil Science (ISSS) which took the lion's share at 7s 3d, equivalent then to one U.S. dollar (Miller, 1976). This dual membership was clearly a legacy of Taylor's membership on Council of the ISSS, and quite smart because as the New Zealand membership grew so did our International representation, and standing: in 1956, New Zealand had 200 International members, second only to the U.S.A in number (Miller, 1976).

There were two other events in 1953 relevant to soil science that I'll mention. One was publication of the first radiocarbon date in New Zealand (NZ-1) on charcoal from the Taupo eruption, and still a perfectly adequate date (1820 ± 150 ¹⁴C years B.P.; Fergusson & Rafter, 1953; Froggatt & Lowe, 1990). The charcoal sample was collected by Ian Baumgart, who went on to write a pathfinding paper on the history of Taupo volcano using his new dates (Baumgart, 1954; Lowe, 1990). He also mapped the eruptives using isopachs, or lines of equal thickness, a feature that prompted Alan Pullar to later describe Baumgart as the 'father' of tephrochronology in New Zealand (Pullar, 1973). Baumgart also gave the first Taylor Lecture in 1976 (Baumgart, 1976).

The second event was that caesium-137 fallout first reached New Zealand in 1953 (Basher & Ross, 2002). Although atmospheric tests were later scrapped, a silver lining to this

particular cloud has been the use of caesium-137 in soil erosion studies, as carried out by Les Basher, Craig Ross and others in recent years (e.g. Basher & Mathews, 1993; Basher et al., 1997; Basher & Ross, 2002).

Returning to my father, he completed a zoology degree at Victoria College of the University of New Zealand. In his final year he took Geology-1, which was taught by Sir Charles Cotton. My father told me how Cotton ran optional field trips for his class every second weekend or so during the term—somehow I can't imagine many first-year students today going on optional trips at weekends—but my father's lifelong interest in New Zealand landscapes was imbued in me, a bit reluctantly at times, during family outings in the car. (I'm sure that many of us here today inflict the same on our own children!)

After two years of service in the RNZAF (including time as a meteorologist based at Ohakea), my father went on to study veterinary medicine at Sydney University during the war on a special government scholarship aimed primarily at helping to increase food production through improved animal husbandry. Whilst in Australia he soon put his foot in it when he casually asked a friend, 'What are those hills over there?', to which the reply was, 'Those are the Australian Alps!'

Fast forwarding to the 1960s, I got my first paid job working for an orchardist and flower grower in Tauranga, the late Frank Sydenham. My brother and I used to pick oranges in the August holidays for Frank, who was a Massey soils graduate. Today Frank is commemorated by a Massey scholarship open to soil science graduates from coastal Bay of Plenty. Chris McLay was the first Waikato student to pick up this scholarship, thanks to all my hard work every August!

In another link with soil science, my father worked briefly with Alan Pullar in the late 1960s on animal health problems*

*Hewitt, S.R., Lowe, J.S., Anderson, D.C. 1978. Dairy cattle ill-thrift investigations on Rangitaiki Plains. Pp. 87-88 in: Pullar, W.A., Hewitt, S.R., Heine, J.C. Soils and land use of Whakatane Borough and environs, Bay of Plenty, New Zealand. *New Zealand Soil Bureau Bulletin* 38. 100pp.

in the Bay of Plenty. Pullar was working on the Kaituna flood protection scheme near Te Puke at the time, and he passed on to me, through my father, a set of early maps of tephra distribution in the region. I had no idea of course who Pullar was and don't recall understanding much about them, but I still have the maps.

As a footnote to this story, it is not well known that rice was grown around Te Puke in the late 1960s as a trial. I well remember my father bringing home a large sack of 'Te Puke Rice' which took us several years to wade through. I recall also my father, a Department of Agriculture veterinarian by this time, becoming involved with the intractable facial eczema problem, now known to be a liver disease in grazing stock caused by fungus growing on dead grass under warm, humid conditions and producing large numbers of toxic spores (*Sporidesmium bakeri*) (Thornton, 2002). John Lowe was the first to propose the use of a common worm drench, Thiabendazole (TBZ), which contained a fungicide, as a possible pastoral treatment. Trials eventually showed TBZ to be extremely efficacious (Campbell & Scott, 1989).

It was around this time that my parents took me to see the new university in Hamilton, in those days a fair hike over the Kaimais from Tauranga. I don't know what I expected but I do remember feeling a bit let down with the handful of buildings scattered over wide open spaces. Probably I was expecting Cambridge or Oxford but on a smaller scale! (Thirty years later there was a sequel to this early disappointment when I visited Cambridge University during my U.K.-based leave in 1998. My son Sebastian, aged ten, had visited the much-expanded Waikato University many times of course, so when we were walking through the corridors of the Earth Sciences Department at Cambridge, displays of famous sons and daughters all around us, he remarked to me, 'This doesn't look like a *real* university, dad.')

When I arrived at Waikato in 1972, the Big Three were John McCraw, Michael Selby and Harry Gibbs. These three established the Department of Earth Sciences in 1970 and chartered its direction. Their great legacy has been to inspire countless students, myself included, into geoscience careers.

N H Taylor

I'll return now to Norman Taylor (1900–1975), the main reason we're all here today. Taylor's life as a teacher, scientist and administrator has been well described, and Vince Neall wrote an excellent biography a few years ago (Neall, 2000) (see also Tonkin, 1990). Many articles appeared in a special memorial issue of *Soil News* in 1976 (Childs & Leamy, 1976) and all described Taylor as someone very special. He was also seen as warm and kindly with a genial and outgoing personality, and with a delightful, boyish sense of humour. Charles Wright likened life with Taylor to an endless voyage of the 'Beagle'. He wrote that despite the appearance of normal domestic routines at home—including Taylor bottling home-brewed beer from the family bathtub—one always had the sense that underneath it all Taylor never stopped thinking about soils and trying to discover some new truth about them (Wright, 1976).

Taylor's international status was also evident in the tributes, and when I travelled to Japan in 2000 I was only a little surprised that senior academics I met still remembered him and his contributions.

Conclusions

I will now conclude my lecture with three comments or thoughts about being a scientist today and in the future.

(1) The first stems from words written by Alan Metson, who said the one thing he remembered about Taylor, above all else, was Taylor's observation that 'the most difficult thing about doing research is to choose the right problem' (Metson, 1976). This truth was taken a little further by the final person I wish to acknowledge in my talk, Phil Tonkin.

Phil, recently retired as we know, wrote a thoughtful article for the Society's Silver Jubilee in 1977 (Tonkin, 1977). He concluded that the challenge facing the third generation of soil scientists in New Zealand—those who began practising in the late 1960s—was 'defining new research objectives to answer tomorrow's questions.' I believe that this challenge is being met by today's generations, the fourth and fifth by my count. Despite great difficulties imposed on the soil science community through funding cuts and other managerial constraints, soil scientists today continue to undertake excellent research, as did their predecessors, for the ultimate betterment of New Zealand society. I am constantly impressed by the stream of good ideas and new findings about our soils and their use.

(2) My second comment relates to the need to continue recognising the primacy of science tradition. Ken Lee expressed this well in 1976 by writing (Lee, 1976):

'There is a continuous fellowship in science that goes back to its origins. It derives largely from the passing on of philosophical attitudes and absolute standards of truth from great scientists to those who learn from them.'

As well as highlighting the importance of tradition, Ken was acknowledging the role of Norman Taylor as both a great scientist and as a gentle man, and I would propose a similar accolade for Phil Tonkin. To me, Phil has been an inspiring figure in New Zealand soil science. He is an extremely generous man who willingly gives of his knowledge and thoughts. Knowing this, I asked him to speak at the Inter-INQUA conference in 1994 where he described the concept and role of collegiality in science. He used the term 'mateship' in our New Zealand context.

(3) This leads me to my final comment: the importance of 'mateship' or friendship in our science. Friendship is an essential ingredient because it adds humanity to our endeavour and makes us value and appreciate others for their character as much as their work. Thirty years ago, Pullar wrote of the need to humanise science more so that 'the frustrations, the hours of concentrated effort, the excitement, and the satisfaction of gaining a foothold after a long struggle' became better known (Pullar, 1973). We all know and empathise with these feelings yet we rarely acknowledge them except perhaps to our loved ones (or, in the case of students, to their supervisors if they're feeling especially brave!).

Our Society today is still the binding force for our soil science community that Taylor and others envisaged 50 years ago. Our conferences, the newsletter, and other Society activities must continue because they provide opportunities to advance friendships as well as soil science.

It is appropriate to add here Jock Churchman's view that the Society is necessary also because it provides a 'virtual monastery' carrying the discipline of soil science through the current 'dark ages' of science (Churchman, 2002).

4:30 pm, 25 November, 2002

Well, the time machine has told me it's time to stop. In closing, I especially thank Dave Palmer for his help in preparing illustrations for this talk, and several others (Brent Alloway, Alan Palmer, Reg Nichol, Warren Gumbley, Richard Smith and Willem de Lange) who provided photos or figures. I also acknowledge and thank my wife Maria and family for their loving support. Thank you all for listening.

Note: In this slightly modified version of the Lecture, only a few illustrations have been included and references have been added for completeness. All ages are in calendar (calibrated) years.

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