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Otago 1958 - 1968 (Part 1)

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Why New Zealand?

I was poorly equipped to contribute to archaeology and anthropology in New Zealand in 1958. When Jack Golson wrote the previous July suggesting I apply for a joint museum-university post in anthropology in Dunedin, I was the museum curator at Scunthorpe, a booming Labour controlled steel town in the north Lincolnshire rural (and very Tory) hinterland. Having spent two years as a trainee at Birmingham Museum, I was reasonably well prepared for the range of responsibilities this tough but absorbing job (salary £600) entailed, and had no thought of moving abroad.

Cambridge hadn't thought my degree adequate for research, and although I then completed the Diploma in European prehistoric archaeology at London University, subsequent contact with university life was limited to extramural lecturing. I had published some excavation reports, and done one book review (for the Daily Worker), but my field archaeology was largely self-taught, my knowledge of physical anthropology limited, while all I knew of social anthropology had been acquired as an extra-mural student, supplemented by working on Birmingham Museum's ethnographic collections. Nonetheless Jack thought I should apply, which, after much thought, and discussion with my wife, Falmai, I did.

Jack and I had known each other since 1949 at Cambridge, where we had pursued similar academic and political interests, and had excavated together at Norwich in 1953, not long before he was appointed lecturer in prehistoric archaeology at Auckland University College (Gathercole 1993). My future seemed to be in a museum career in Britain. Now Jack's suggestion, bizarre but

exciting, was a sudden reminder that Scunthorpe had its drawbacks. Tory England many constraints, and the Dunedin salary (£1750) was enormous.

We left England on 25 May for a six weeks' voyage. My preliminary reading had been pitiful: Andrew Sharp's *Ancient Voyagers in the Pacific* and Roger Duff's *The Moa-hunter Period of Maori Culture*. I was aware that my field experience had been in rescue excavation dealing with periods neatly delimited and understood, and that I had never run more than a small digging team. But I was excited by the opportunity of linking museum and university practice, especially where archaeology was placed in an anthropological setting.

Of New Zealand I appreciated the achievements of its Labour governments, the prowess of its soldiers and footballers, and, to a more limited degree, its record in race relations. Discovering John Mulgan's *Report on Experience* in 1948 when in the British Army, I had admired both his understanding of the psychology of soldiering and his insistence that New Zealand was its own self. I had heard of Charles Brasch and *Landfall*, and I was delighted to consider myself a university lecturer - even only half a one.

Jack was in Wellington for a Polynesian Society meeting when we landed in early July. He told me to come to a meeting of the New Zealand Archaeological Association in two weeks' time, when a new scheme for site recording would be launched. I began work at Otago Museum two days later.

I shared a screened-off section of the gloomy moa bone store with Dr Skinner (Fig. 1), who came in several days a week. This became a bonus, because, although he talked continuously, he taught me what no-one else could have done: the history of Polynesian anthropology as he knew it. He saw one of our links as Cambridge, where he had taken a BA by research in 1918 (Freeman 1959 : 15), and I became perhaps his last student. Over the next three years, he talked about his work, the Museum's collections, the people he had known in Pacific studies, and occasionally his teaching. What began as peripatetic gallery excursions into Maori material culture became a means of transforming my limited, pragmatic approach to what I should do at Otago into a coherent programme. In return, I helped him finalize papers on Murdering Beach and Little Papanui (Skinner 1959, 1960). I realized I must build on Skinner's foundations, not in one direction alone, but equally in anthropology, archaeology and museum ethnography. At Scunthorpe, I had deliberately worked within bounds established by my predecessor, the town's first curator. The situation at Otago Museum was similar. Skinner had dug deeply and broadly since his arrival in 1919.

It soon became clear, however, that Skinner's foundations, though much admired, were very parochially perceived within the University. Anthropology had been taught as a Stage 1 subject since 1920. The terms of my appointment envisaged no enlargement. Indeed, how could one person teach more than a one year course, and also have responsibility for all the Museum's cultural collections, ranging from the Palaeolithic to local ceramics?

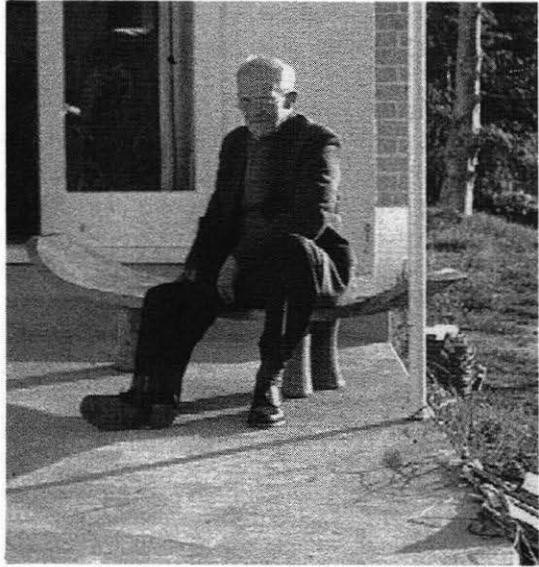


Figure 1. H.D. Skinner outside his home at Saddle Hill, Otago, 8 May 1964.

Having arrived in mid-year 1958, I had no teaching until 1959. Delightedly, I discovered that I was my own boss, directly responsible only to the Faculty of Arts. I drew up my own prescriptions, initially based on Skinner's, which, in their turn, were derived from those Haddon used at Cambridge. There were to be four lectures per week, and two hours weekly practical per student, the course embracing physical anthropology, social/cultural anthropology and archaeology. By starting with palaeoanthropology, concurrently with some basic ethnography (sometimes the students would arrange themselves in the raked lecture room into a kinship diagram), during the winter term one could move to palaeolithic and later archaeology, parallel with more detailed ethnographic studies, culminating in the spring term with Pacific archaeology and ethnography. From 1960 the University gave approval for Les Lockerbie, the Museum's Education Officer, who had been one of Skinner's students, to take some of the practicals. Initially, his payment was derisory, but at least he was drawn into the teaching.

Once the 1959 course was launched - an exhilarating moment - with some 30 students (whom I soon realized could be useful ambassadors for the subject),

I obtained university approval for the Unit to be offered also for the BSc, and later that Maori be recognized for the foreign language requirement.

An unexpected result of the re-introduction of anthropology teaching was to discover some of the subject's friends in the University, as well as those who were less enthusiastic, including a handful who saw it as the thin end of a sociological wedge. Guy Manton, Professor of Classics and future Dean of the Arts Faculty, was a strong supporter, as was Angus Ross, Reader in History, a former Skinner pupil (he had written an MA thesis in 1933 on Te Puoho's last raid) and a much decorated wartime soldier. Other supporters included Bill Adams, Professor of Anatomy and another of Skinner's former students, and Brian Marples, Professor of Zoology. A later ally was Professor Dixon, Head of Social Medicine, who enlisted me to run a seminar in social anthropology for public health specialists. But there was no talk of growth. To the University the re-establishment of Stage 1 Anthropology was sufficient justification for it continuing that way.

Nonetheless, the subject received a boost in September 1959, when Golson gave three magnificent Macmillan Brown lectures. The first was on Polynesian prehistory, of which the Maori section was about to appear in more extended form in his paper in Skinner's *Festschrift* (Golson 1959a; see below); the second had much to say about the new evidence of pottery and its associations in western Polynesia and eastern Melanesia - the Lapita culture (Golson 1959b), while the third ranged more widely over island and mainland south east Asia, and beyond, even to circumpolar zones. The lectures brought home to many among his audience, perhaps for the first time, what archaeology could mean to those seeking new insights into New Zealand and Pacific culture history. But would I be able to build on this?

I had another shot in the arm later in the year, a visit from Roger Green, then a Fulbright Scholar at Auckland, and his wife Kaye. Their enthusiasm and knowledge were infectious (driving them one night, deep in talk - was it to visit Skinner? - my 1928 Dodge (Fig. 2) could only stagger up the hill to Roslyn, an early warning how little this ancient contraption, much loved by our kids, could be trusted for fieldwork). To my still-English mind the Greens' different approach to the subject was refreshing. Not for nothing did Roger later send me from New York a copy of Willey and Phillips' *Method and Theory in American Archaeology*. Such encounters were very valuable, but they could also produce negative reactions. At times Otago was a lonely one-person set-up.

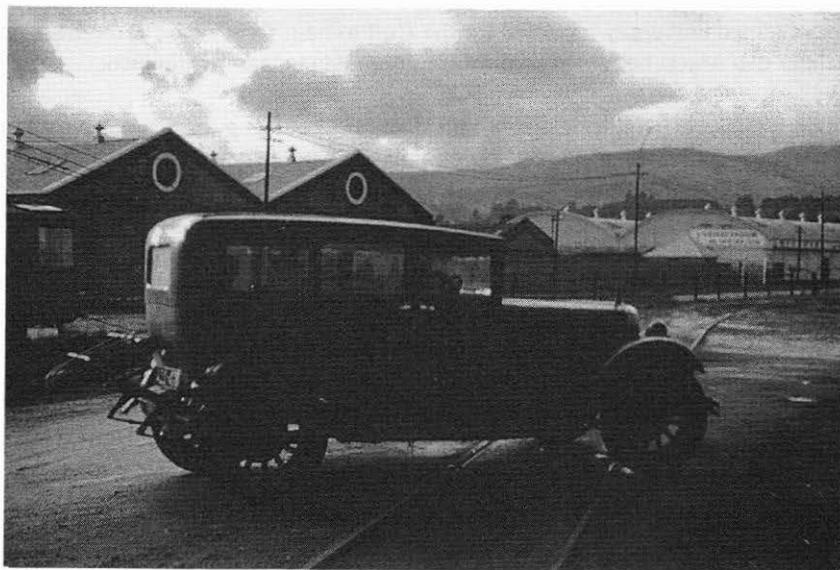


Figure 2. The 1928 Dodge, Dunedin, June 1959.

In November 1959 Lockerbie and I tried, but failed, to set up an Anthropological Society, the meeting attracting a mere handful. We succeeded the following March, when 120 people came along. I became the secretary, Ernie Munro, who had worked extensively with Lockerbie in the Catlins, the chairman. Two of the first enrolments were Dunedin schoolgirls, Helen Keedwell (later Leach) and Jean Kennedy. Both became Otago students, strongly loyal to our department, later having notable careers in Pacific archaeology.

Shortly before Christmas we celebrated the publication of Skinner's *Festschrift, Anthropology in the South Seas* (Freeman and Geddes 1959), with a party at the house of Margot and Angus Ross, he being one of the contributors. Charles Brasch, whose grandfather, Willi Fels, had been a munificent benefactor to the Museum, asked me to review the book for *Landfall*. A lovable man but stern editor, he rejected my first version as too uncritical, which I took as a rebuke for sloppy thinking (Gathercole 1960).

A perspective for development?

At a Faculty Heads of Departments meeting in March 1960, I circulated a paper setting out a development plan for the department. This was deliberately

conventional, envisaging a steady progression to a General Degree Stage 3, then the introduction of a BA Honours year, followed by a thesis oriented MA. Initially the paper was greeted in silence, then welcomed by Guy Manton when discussion became general. Although nothing specific followed, the proposal was at least on the table.

Later that month I gave a Faculty open lecture on Gordon Childe, the beginning of a preoccupation with his life and career which has lasted ever since (see, for example, Gathercole 1994). Much of the rest of the year was concerned with the activities of the Anthropological Society, on which I pinned much hope. Here I made many friends, especially Lin Phelan, Hardwicke Knight and Ken Wildman, of whose commitment to the Society and involvement in fieldwork I shall have more to say in the second part of this article.

In August the family took a break, travelling to Auckland in the Bedford van (which by then had replaced the horrid Dodge), where we were warmly welcomed by Jack's departmental colleagues, especially Ralph Piddington, who immediately endorsed my development plans. Auckland, after Dunedin, was *warm*. I lectured again on Childe, prompting searching questions from an erudite, and to me unknown, member of the audience, who turned out to be Ham Parker, another former Skinner student. I bore him in mind as a possible Otago appointment.

The development of fieldwork

My introduction to New Zealand archaeology was at the 1958 Archaeological Association meeting in Wellington mentioned earlier, which set up the Site Record Scheme. There I met, among others, Alastair Buist, Roger Duff, Wal Ambrose, Tony Batley and Ron Scarlett, and learnt something of the advances made in North Island archaeology since the Association's first Conference, held in Auckland in 1956, and where the Auckland Society was such a driving force. Obviously there was a need for something similar in Dunedin (I had set up a comparable society at Scunthorpe), potentially of equal significance, I thought, to any developments in teaching and museum work.

At the Wellington meeting I was asked to be site record filekeeper for Otago and Southland. This I was reluctant to do, my inclination being that Lockerbie, well known for his field work in South Otago (see, for example, Lockerbie 1959), who was at the meeting, would be more suitable. But he declined, it also becoming evident during our discussions that, in terms of division of labour, I should look to the north of Dunedin for at least my initial field experience.

In late 1958 the Museum had given me a small grant to visit Otago sites, so I had made contact with Michael Trotter, then working on the family farm at Katiki, North Otago, seeing some of the sites he was investigating (Trotter 1959). Always open and helpful, he welcomed the idea that I should work between Dunedin and the Waitaki River. In addition, I was guided round some of the Catlins' sites by a local member; excavated an umu at Clarendon, near Milton, to see what it comprised, and at the invitation of Brenda Bell, stayed at her family's Shag Valley homestead, in what turned out to be a fruitless search for a moa bone site. Etched in memory of that time is of a visit to Invercargill with Geoffrey Blake-Palmer, then Superintendent of Seacliff Hospital, to see something of Southland's topography, and to visit David Teviotdale, long a co-worker of Skinner's, then dying in a local nursing home.

By the time the 1959 teaching year began, therefore, I felt I had acquired some knowledge of local archaeology. But I was making one big mistake. That knowledge was heavily biased towards Moa-hunter sites, with little attention to later evidence, apart from Murdering Beach. Over the next few years I began to appreciate how unfortunate this preoccupation could become.

I might have learned something of the significance of this partiality from attending the Association's annual meeting at Rotorua in May 1959, where I saw how different North Island archaeology was from that of the South. At the training excavation at Pakotore I met Rosemary and Les Groube (wondering idly if either might ever work at Otago). But for me the excavation was a disaster. I was put in charge of a team to dig an area later shown to be much disturbed. I could make no sense of those ubiquitous ash deposits and fills. Worse, my handling of the team was so unfeeling that one member told me bluntly that if I went on like that I could be thrown out of the Association. I had a lot to learn.

During Jack's visit to give the 1959 Macmillan Brown lectures we explored the Pleasant River Mouth, where there seemed to be an undisturbed Archaic site. Once teaching was over for the year, I organized some small weekend exploratory digs (Otago Anthropological Society 1960:15-16). The presence of occupation layers beneath the sand led to more extensive work in the next two years (Fig. 3).

In January 1960, with members of the North Otago Scientific and Historical Society, I worked under Trotter's guidance at Tai Rua (Trotter 1979). A novice, trowelling alone in the evenings, I experienced an extraordinary sensation of



Figure 3. Pleasant River Mouth, Otago, 19 February 1961. Area A, to S.E.: general view of excavation with main occupation layers shown in section.

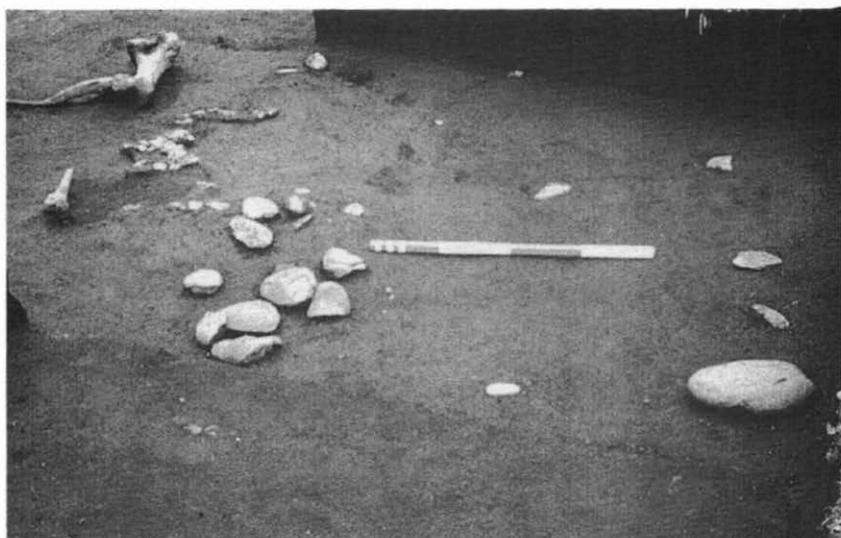


Figure 4. Tai Rua, Otago, 14 January 1960. Moa bones, moa egg shell fragments and stone debris in situ at swamp margin.

timelessness when exposing moa bones, moa egg shell fragments and stone debris lying towards the base of the occupation layer (Fig. 4). Work continued with members of OAS during the Easter break, revealing posthole evidence, a quest we pursued in further short seasons in 1961 and 1962 (OAS 1960:14-15; Gathercole 1961; Trotter 1966:49-52, 1967:138-139). Beginning to work up the Pleasant River artefacts, I spoke on the stone material in the Association's Wellington meeting in May 1960 (when I also became secretary), but this first attempt got little reaction from colleagues. Was it so bad, I wondered?

Later that year, partly to extend the field experience of members of the Anthropological Society, there was rescue work on the Otago Peninsula, at Hooper's Inlet and nearby Kohuka (OAS 1960:16-17). The latter consisted of the chance exposure, and so rapid removal, of human skeletal material. This rushed job co-incided with Jon fracturing a leg when Falmai was in the maternity wing of the Dunedin Public Hospital awaiting the birth of our third child. Looking after the boys meant that Jon had to be carried to the site, Nick burdened with some of the kit. Julia was welcomed home a few days later (Fig. 5).



Figure 5. PG with Julia, Jon, Nick and kitten, Dunedin, 3 November 1960.

Fieldwork at that time also included a largely inconclusive search in January 1961, prompted by an earlier visit with Yosihiko Sinoto, for undisturbed deposits at Waitaki River Mouth (Knight and Gathercole 1961), and an exploratory visit to Tiwai Point, at the Bluff, to view the proposed Comalco smelter site (Park 1969). At the same time, the need to maintain a steady flow of Museum activity, particularly at that time the installation of a ceramics and furniture gallery, and to be a competent secretary of the Archaeological Association and the Anthropological Society emphasised how inadequate was the existence of only one post to cope with this spread of responsibilities. Of course, my critics could argue that I had deliberately taken on new ones in order to demonstrate the impossibility of fulfilling them properly, and so make a case for the provision of more staff. The difficulty was that once this strategy was adopted, it became almost impossible to change it without renegeing on obligations already entered into.

Ultimately, however, the joint University/Museum Committee responsible for my appointment agreed, apparently on the urging of Angus Ross, that the joint post should be abolished, to be replaced by two, one in the Museum, the other in the University. Given the choice and opting for the University position, I learned that I would run the department as a senior lecturer from February 1962, when Dave Simmons would take up appointment as Keeper of Anthropology in the Museum. I would then bid farewell to Skinner, moving from the moa bone store to one containing human skeletons and thousands of Murihiku artefacts.

These forthcoming changes were joyfully reported to Jack during his visit in May 1961 (Fig. 6), when we started planning an article for *Antiquity* surveying developments in New Zealand archaeology since Duff's papers in the same journal (Duff 1949, 1950). This appeared the following year (Golson and Gathercole 1962). Another welcome visitor in 1961 was Karl Erik Larsson, of the Ethnographical Museum, Göteborg, who introduced me, conceptually speaking, to Pacific ethnohistory, and also insisted that I encourage Skinner to complete some articles long contemplated on Pacific ethnological themes. This plan was carried forward by Simmons, resulting in two papers in the Otago Museum's *Records* (Skinner 1964, 1966) and renewed work on others. Simmons certainly helped reduce my academic isolation. He also enhanced the standing of the Museum by publications of his own (e.g. Simmons 1967).

1962 - a crucial year

We had a fine Anthropological Society team at Tai Rua that January. The

digging was good, though constrained by the depth of sand that made retention of some baulks more necessary than I would have wished. By then I had become preoccupied with sand-stained structural evidence, but unfortunately I found much of it so difficult to understand that I could not interpret it. Michael Trotter and those digging with me were, and continue to be, let down by the fact that my work there remains unpublished, along with other of my Otago field projects.



Figure 6. Falmi, Julia and Jack Golson (having lunch) in the Bedford van, Taieri Airport, May 1961.

These doubts over fieldwork competence fuelled growing uncertainty concerning my administrative efficiency. Luckily I was due to resign the secretaryships of both the Archaeological Association and the Anthropological Society in the spring, and had anticipated the Association having an even run to the annual meeting, to be held in Christchurch in September 1962. Its administration had been localized when Lockerbie became President the previous year. But the Treasurer, also local, assumed I would do his routine work, so when I messed up the final accounts (from memory, to the tune of £40) I earned a rebuke in the auditor's written report, a rather sad note on which to end my tenure.

At Christchurch I met Derek Freeman, who, before going to ANU at Canberra, had taught for one year at Otago after Skinner's retirement. Entranced by his anthropological table-talk, I reported lyrically on plans to introduce a Stage 2 in 1963. I had received permission to appoint another staff member, a social anthropologist, with whom I would split the teaching. Accommodation, however, would remain a problem. A year or so earlier, just by asking for it, I had acquired a redundant Botany Department laboratory, fine for practical classes. But office space still had to be sought in Museum store rooms.

At the Association's meeting Roger Green and Wilfred Shawcross had argued impressively, and, I thought, conclusively, for a more sophisticated interpretation than offered hitherto of the prehistoric sequence in the Auckland Province (Green and Shawcross 1962; Green 1963). This raised interesting questions about its implications for our work in Murihiku. Pondering this, I flew to Sydney for an ANZAAS meeting. I had been asked by Jack, by then at ANU, to talk on New Zealand experiences in building an archaeological organization. There was just time on arrival to familiarise myself with the extent Australian colleagues were doing likewise.

After four years in a modest environment, it was exhilarating to be in a big city once more, as was the opportunity to see John Mulvaney again (we had first met as students at Cambridge), and to meet Bill Geddes, Rhys Jones and others in Australian anthropology and archaeology. The break gave a short breathing space to contemplate what departmental growth might mean.

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Part 2 will be published in the December issue.