

SOME ASPECTS OF CONTINUITY AND CHANGE AMONG ANTHROPOLOGISTS IN AUSTRALIA

OR

'HE—WHO—EATS—FROM—ONE—DISH—WITH—US—WITH—ONE—SPOON'

Plenary address to the 2001 Meeting of the Australian Anthropological Society

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I understand that, given the theme of this year's meetings, the organisers felt that it might be useful to call in someone who, if he didn't know where anthropology was going to, at least had some idea of where it was coming from. I have in mind a kind of *longue durée*, although I can't tell whether we're at the end of it or have a way yet to go.

I guess I should provide a bit of a CV if I'm going to aspire to the position of elder. I began anthropology in Darryl Forde's department at University College, London, in the early 1950s, and arrived at ANU to do graduate work in 1956. I had my first teaching job in Auckland, came back to Australia during the tertiary education boom of the 1960s, experienced the Marwick meltdown at Monash and then moved on to Sydney. The senior anthropologists I had contact with during those early years were for the most part students of Malinowski, for example Phyllis Kaberry, and sometimes also of Radcliffe Brown, including Ian Hogbin and Bill Stanner. Darryll Forde, the professor at UCL, had been trained in the United States, and unlike the other departments, exposed us to North American anthropology as well as British. I also did a course in race relations at London School of Economics, with Maurice Freedman, although he did not consider it to be a real subject. However, it was British anthropology in the form of structural functionalism that I found to be the reigning paradigm in Australia, during my post-graduate years.

I'm not going to take you through an institutional history of anthropology in Australia: that job has been done quite recently by Kathy Robinson and it will be re-published in *Forum* shortly. I'll try a broader brush. The first, intentionally dry title of my talk, is in parody of the sober, positivistic titles one got in the heyday of British Structural Functionalism. No cute titles in those days, and no present participles! I might add that the section devoted to 'change' came at the end, and amounted to little more than grudging admission that things weren't what they used to be. I'll try to do better than this. My use of the title is deliberately slippery, in that I'll be saying something about the way anthropology has dealt with 'continuity and change' in the course of its own changing.

I've been careful to say 'Anthropology in Australia' rather than 'Australian Anthropology' because I don't think there has ever been An Australian school or even style. Anthropologists in Australia are a cosmopolitan bunch: the expatriates occupying Australian chairs over the years outnumber locals by 17 to 9, and of that nine, several took their doctorates overseas. Several departments are predominantly expatriate. There has been a longstanding preoccupation with the Aborigines, though this is more apparent now than it was in my early years here, when Papua-New Guinea was where the action was supposed to be. This preoccupation makes anthropology in Australia like that in Canada or some countries of Latin America, and unlike Britain or the United States – Native American topics are a notable absence from AAA meetings. However, expatriate and overseas anthropologists have made a major contribution to Australianist anthropology over the years so that I cannot regard it as distinctively Australian either. But more of that later on.

As one might expect with this pattern of recruitment, anthropology in Australia has been influenced by most of the schools and currents to be found elsewhere. Until the 1970s, the British influence predominated, partly because the senior anthropologists had got their doctorates in Britain and went back there for sabbaticals. Remember that until the early 1970s, the Australian Association of Social Anthropologists (the predecessor of AAS) was a chapter of the British body, and all membership applications were sent to the UK for approval. The Manchester 'school' was taken as the latest thing in the early 1960s, presently to be displaced

by Edmund Leach's soft Anglo-structuralism. Towards the end of the decade, some began looking towards France and Lévi-Strauss; the visits of Maurice Godelier - between field trips to Papua-New Guinea opened windows for some of us. In the late 1960s Mervyn Meggitt took up a position in the United States, and enabled a number of his colleagues - including myself - to take visiting positions and get a sense of the greater diversity of approaches in what was a very much larger profession. The experience certainly gave me alternative ways of thinking about my work, and fed into the courses I began to teach, to the dismay of some of my colleagues. Nowadays there is no AAA meeting without its small contingent from Australia, eagerly fingering the cheap books in the basement. I'm not sure how many go to the British ASA meetings.

Since the 1960s, anthropology in Australia has seen more change than continuity: ethology and socio-biology, and structural Marxism, both of which waxed and waned rather quickly; interpretative anthropology; political economy - my own intellectual base - and cultural materialism; and then phenomenology and post-colonial studies, and what I can only call the gender revolution, which rapidly erased class from the discourse, although it need not have done.¹ I don't need to go on because I'm sure you know what's currently 'in' better than I do.

This diversity of paradigms is both an outcome and a cause of the increased diversification of subject matter. When I was a graduate student, people were very certain about what was and what wasn't anthropology, and ruled a lot of things out. I remember asking someone why the discipline was so preoccupied with kinship, to be told 'because that was what anthropologists did best'. Certainly kinship, or rather descent was where the action was, and studying other things was a lonely business. Nowadays, it seems to survive only in the Native Title field. Otherwise, kinship is off the syllabus of many departments, 'because the students find it boring'. This is partly because studies of what we used to be able to call 'primitive' societies are in the minority. Nowadays the world is our playground, though the preferred destination has varied over the years.

As for the topics, Anthropology seems to become ever more pluralistic: tourism, the environmental movement, sport, ethnicity, identity, diaspora, place, memory - and the gender aspects of all these. We don't seem to go for the commanding heights, however, or the rich and famous, though that may be because our research grants are too meagre to enable us to sustain the life style for more than a couple of days. I doubt if the multi-sited research that George Marcus proposes is within our means, these days.

I'm not sure whether the diversification of paradigms has triggered this diversity of topics or the other way about - perhaps a bit of both. However, what strikes me is that anthropologists increasingly find their paradigms outside anthropology: when it come to the big ideas I hear a lot more about - Pierce, Rorty, Said, Butler, Heidigger, Sartre, Foucault, Derrida, Kristeva, Irigaray - than other anthropologists. All this is fair enough, or it would be if the traffic went both ways, but I get the impression that Anthropology is intellectually importing a lot more than it exports. Clifford Geertz and Marylyn Strathern are among the few anthropologists who get taken up by people in adjacent disciplines. Moreover, I get the impression that anthropologists are more likely to go straight to their guru of choice, rather than engaging with the other anthropologists who have tried to work with the same source, for example reading Rabinow who has applied Foucault to his anthropology. I sometimes wonder if one can still speak of a distinctive anthropological discourse?

Leaving aside Clifford Geertz, who is still cited by anthropologists if only to critique him, I am hard put to assemble a contemporary anthropological canonical literature. It seems as though the canon is shot! Partly this comes from a loss of confidence, even legitimacy. Since the late '60s anthropology has come under fire from cultural studies, post-colonial studies, not to mention the historians who have belatedly discovered 'natives'; but more savage have been the criticisms coming from within. We seem to be afflicted with a post-colonial cringe!

Thirty years of continuous auto-critique have taken their toll, and it's hard to send students to read someone who was has been denounced as a hand-maiden of colonialism, or called the people savages, or failed to mention the women. My own inclination is to see these

texts in historical perspective, rather than reading them always against the grain. Even so, I wonder if we shouldn't give ourselves a break.

I am intoxicated with the richness of imagination that sends anthropologists into so many different places and intellectual systems, and I enjoy shopping around the different sessions in conferences like this. At the same time I do get a sense of fragmentation, which as we all know is a feature of post-modernity, as well as the Decline of the Roman Empire.

This diversity of subject matter, topic, and paradigm, spilling over into the domains of various other disciplines and genres, leaves us with the increasingly difficult question, what is it that makes us special? Where is that 'essential continuity' that links us to our anthropological ancestors? The answer has to be field work, which by my book is no mean thing. To be sure, as Clifford says, ethnographic representations are always 'partial truths', and as Lila Abu-Lughod adds, they are also 'positioned truths', but that simply means 'reader beware!' (Abu-Lughod 1991: 142)

Of course this practice too is ideologically stressed. That we have actually *talked* to the people we write about seems to rouse our rivals to shrill accusation, which I of course believe conceals their anxiety about their own text-based speculations.

At all events, despite the deplorable downgrading of graduate research in Australian universities, and the crass surveillance of ethics committees, Australian departments are still sending off graduate students to undergo what we like to call a rite of passage, and many of us – and I think this may be a change – keep on going back to the field into old age, or at least talking to our friends by telephone. I should admit here that it was the prospect of field work that made me want to be an anthropologist, rather than the thought of what I might contribute to knowledge.

So rather against my usual inclinations, let me try to re-invent some tradition for us. This brings me to the second title of this talk, and the more cryptic, unless you happen to know about an American ethnologist of the 1880s called Frank Hamilton Cushing.ⁱⁱ It's usual in talks of this kind to resurrect some 'illustrious corpse' – regard if you like as a kind of secondary burial.

I started thinking about Cushing when I was writing a piece for the centenary of the Cambridge Expedition to Torres Strait (Herle and Rouse 1999). Our British colleagues were celebrating Haddon and his team for the first anthropological field study. It wouldn't rate highly now, but then nor would the field work of Haddon's contemporary, Boas, still less the kind of field trips mounted by the Bureau of American Ethnology in preceding decades. Back in 1879, Cushing joined one of the BEA's expeditions, to Zuñi, in New Mexico, but when after a few weeks the party prepared to leave, Cushing announced to their astonishment that he was staying on. Even more outrageous, he burst in on the Zuñi governor of the pueblo, declaring that he was moving in: an appalling prospect to a group as closed as they were. The job of the governor was to deal with outsiders, but this was something else. Cushing stayed for four-and-a-half years, got some competence in a particularly difficult language, and eventually was initiated into several of the secret societies. He claimed that to qualify for this, he had to join a scalping expedition, though his colleagues were sceptical.

According to one of his stories, he eventually got himself invited to join a family meal, but unlike his hosts, his hands were too tender to plunge into the scalding stew pot, and he was reduced to asking for a spoon. This earned him the nick-name which I have taken as my second title: 'He who eats from one dish with us with one spoon.' Perhaps I should just have just called my paper 'Cushing's spoon'. Now Cushing created his own legend, and we can read this story as establishing his authority on things Zuñi, and as a latter-day frontiersman. But we can also read that spoon as a metaphor for the in-but-not-really-ness that is the field workers' experience: he was after all studying the Zuñi. But that wasn't all he was doing. After all, he never revealed what went on in the secret societies, so perhaps we can say his sojourn in Zuñi was not just to get material for his publications. According to the professional standards of the time, he did not need to spend four-and-a-half years with them, contracting tuberculosis in the process. There was something else going on. He did after all eat that stew, even if he needed a spoon to do it.

The Cushing that came out of Zuñi was surely not the Cushing that went in four-and-a-half years earlier. But how much of this experience should he communicate to his fellow Americans, and if so how? It seems he was given to going around in Zuñi costume, but, his rivals complained, it was not strictly authentic. Trickster that he was, he added his own touches, demonstrating his mastery of the culture by ringing the changes on it. Much of his writing, particularly for popular readership, came in the form of narratives, with himself as actor.

If this 'going native' was transgressive by 1880s standards, so was his inviting two of his Zuñi friends to stay with his parents in Massachusetts, and his account of this visit shows a remarkable sensitivity to the complexities and incommensurabilities of the encounter. Despite the flamboyance, there was a reflexivity about his project that does not surface again for several generations. Remarking on the observers' tendency to read in their rendering of things their own personalities, he asserted that the 'personal equation – I think he coined the term – is the supremely essential thing in researches such as this' (Green 1979: 16). In his case the personal equation overflowed the ethnographic project, affecting his position as an American.

To be sure, Cushing was a man of his time. But he wasn't the advance guard of the frontier; in fact he saved the Zuñi from a land grabbing US senator, and got recalled for his pains. Although evolutionism was the reigning paradigm, Cushing's Zuñi was no Vanishing Indian.ⁱⁱⁱ He gave a lecture at the Bureau on how the Zuñi might eventually be assimilated, but he also argued for the unchanging character of their culture despite centuries of contact, with Spain before the arrival of the Americans. More of that later,

For modern anthropology Cushing is more like a culture hero than an ancestor, and like the raven in Native American mythology, he comes down to us a trickster. Malinowski was a bit of a trickster too, but he is rather our ancestor, the one who routinised his own charisma, and invented his own mythical charter. Having returned from the Trobriands he settled down to being a professor and never went back to see his 'friends', but sent his students off to do what he had done somewhere else and come back to write their dissertations. The institutionalised anthropology ironed out the transgressive character of Cushing's entry into and exit from the field, and its scientific discourse defined and constrained what parts of field experience could be memorialised in ethnography. The practitioners of structural functionalism wrote themselves out of the story altogether.

When I was a graduate student, there was no reflexivity about the field work encounter, and no recognition that it was integral to one's material or one's later life. Nothing much beyond a few droll stories for the dinner table. One had had to gain sufficient rapport to get the data, but wasn't expected to like the people, nor were they expected to like you. A joke went about that anthropologists got the people they deserved, capped by Marshall Sahlins – the great stand-up anthropologist – to the effect that a particular people had got the anthropologists they deserved!

I'm thinking particularly of people working in the P-NG Highlands, which of course had been pacified relatively recently. But in Aboriginal Australia too, anthropology was conducted under the constraints of a colonial order and a racial hierarchy.

Instead of Cushing's gate crashing, one had to apply to the colonial authorities for entrée, and as a condition for this, one was not expected to 'go native', nor indeed to come back 'native'. The local whites regarded one as declassé simply for doing this kind of thing, and maintained various rules and conventions that put one in the situation of having to choose between them and the people. Nor should one get involved in politics: in fact people with the wrong views were barred or expelled. More generally, creating any kind of a scandal was spoiling the game for the anthropologists who came after.

Since that time, this distinctive practice has been subjected to painful scrutiny, both in terms of the ethics of research and its implications for the ethnography we finally write. George Marcus is quite right to challenge that old cliché, 'rapport' in the discourse of field work (Marcus 1999). It suggests one way communication from informant to researcher but not much coming back the other way. It also implies a dyad defined simply in terms of this transaction, sealed perhaps with a stick of tobacco. 'Complicity' says it better, but I don't see it quite in the negative sense that Marcus gives it. For him it is simply about the power

structure in which the 'field' is situated. I'm a bit impatient with the vulgar Foucauldianism that totalises the colonial situation, missing the ambiguities and the contradictions. If Cushing's stories are true, he was complicit with the Zuñi in their conflicts with Mexicans and Mormons and even a US Senator, although of course he never stopped being an American and indeed admitted to keeping a hand gun in the pueblo, though he never used it. Perhaps I should have called the paper Cushing's gun.

My own experience of field work in Torres Strait, under Queensland administration was full of contradictory encounters and compromises that make me uncomfortable to remember; even so it wasn't a tragi-comedy acted out in solar topees and grass skirts. There were spaces in which anti-colonial sentiments, even resistance, could be nurtured. The only other white people Islanders saw were government officials and occasionally missionaries and pearling masters, but during the Pacific war they had had relations with Australian ex-servicemen which they remembered as egalitarian. The social order as they perceived it, offered an alternative role which I could tailor to my own needs. However, this meant that I was made the recipient of endless complaints against the authorities, which could have been a problem, though in fact it wasn't. Even so, the space for negotiation had its dangers for the Islanders; there were moments where the thrill of the chase got me into situations that in retrospect I should have stayed out of. Writing up, and protecting my sources, was a tricky business. Fortunately, I don't believe I got anyone into trouble.

I am not suggesting that all colonial situations were like this. Working on a supervised Aboriginal settlement in Western NSW left me little room for manoeuvre. In the early years of work in the New Guinea Highlands, my guess is that the positions of the anthropologists were more or less fixed; the Highlanders were, as one researcher complained, a busy people, with little interest in the world beyond the valley – just as long as the shells kept coming. The only alternative as Andrew Strathern seems to have demonstrated in later years was to become a Big Man. But as the Cushing case shows us, there is always a touch and sometimes more than a touch of mimesis in anthropological field work. One has also to admit that it can take a sinister – one might say Taussigian turn. Napoleon Chagnon's field among the Yanomami, by his own account – and leaving aside Tierney's grave allegations – is worrying. We have to recognise that there are some places anthropologists should not go, or do I mean some anthropologists who should not go...? But that doesn't mean we have to love the people we study. Does Susan Harding love her Christian fundamentalists?

Eric Wolf once complained that anthropology tended to turn 'a methodological unit of enquiry' into 'a theoretical construct by assertion, a priori' (1981: 14). My own work in Torres Strait took place mainly on that classic anthropological site, the tropical island, complete with palm trees, in which – no doubt – the trade winds whispered. I have to admit that such a location tempted me to privilege what we experienced face-to-face, day-to-day, and disregard the forces which though invisible affected our lives. In the Strait, white authority figures appeared rarely, if only because they were able to control what they wanted from a distance. Thus, it was easy not just for me but for the Islanders to act out a *communitas*, à la Victor Turner in which the structures of domination were suspended in favour of an affective friendship. But this 'anthropological irony', to use Geertz's term, reflected a tension in the community between those who settled for the limited local autonomy that the system allowed them, and those who saw their future as participating as citizens in the structures of power controlled by 'white people', over the horizon of the island or indeed the Strait.

At the same time, the experience of intense face-to-face relations prevented me from dissolving the lives of the Islanders in the impersonal movements of markets and political forces. If one took time to listen to their stories, moreover, one became aware of the changes that they had experienced and which were sedimented in their lives.

You will note that in working out how and what to write I took Islanders into account only to the extent of shielding my sources from the possibility of official reprisal. I was like many of my contemporaries in supposing that they would not read what I had written, and so did not need either to consider their sensitivities, or write a text that they could read. Islanders

were in fact literate, up to a point, some had read Ion Idriess's novels, just about the only writing on Torres Strait available at the time, but I could not imagine them reading an academic work all through. Probably also I thought that the dryer and more academic my ethnography, the less likely were the authorities to object. I suspect a lot of anthropologists took refuge in the arcane for similar reasons.

The 1970s saw an end to colonial anthropology in Australia and what one might call a loss of innocence. At home, the indigenous population came out from under the regimes of the protectors and director of native welfare, and with the arrival of a Labor government, plans were set for Papua-New Guinea to be decolonised. In Southeast Asia, where some Australian anthropologists had begun to work, the situation was a good deal more complicated. The Whitlam government withdrew Australian troops from Viet Nam, but Cold War politics had permeated the entire region, exacerbating divisions within the host societies.

Out of the campus-based protest against the Viet Nam war came, first, accusations that certain North American social scientists had been engaging in intelligence-gathering activities under the guise of counter-insurgency, and then that similar activities were being conducted under neo-colonial Australian auspices in Thailand. A lot of us read Dell Hymes *Rethinking Anthropology* (1969), and Talal Asad's *Anthropology and the Colonial Encounter* (1973), and prospective graduate students were rethinking their position. But anthropologists went on doing field work, even in Southeast Asia.

Australian research in Southeast Asia had been a relatively recent departure – which may explain a certain political naivety of researchers whose early experience had been in British or Australian colonies. The absence of colonial officials was a relief, but while relations with peasants might be easy, dealing with landlords, politicians and bureaucrats made it difficult to talk glibly of A culture or A community. As I found when trying to work in the southern Philippines, one had also to deal with cultural attachés from foreign embassies, strange missionaries whose presence in the area was hard to explain, and finally the local scholars who envied one's research grant. I for one began to think much harder about responsibility, and to ask myself who needed my research?

It was also during the 1970s that anthropologists began to look beyond the usual research destinations. In fact, since the 1940s, the Sydney Department had sponsored studies in Anglo-Australia, and there was another burst of projects in the late 1960s, also including immigrant communities.^{iv} Sociology was just developing in Australian universities, and some – particularly at Sydney and Macquarie saw it as a single discipline, continuous with anthropology – a notion that the incoming sociologists quickly rejected. This disciplinary re-orientation could be seen in part as an attempt to escape the reproach of neo-colonialism; in part the desire to shed the association with the primitive and the arcane, engaging in stead with modernity in some form.

In these studies, the Other – although that term did not catch on until the '80s – was the subject of study, first and foremost, and only secondly different. After all, studying anyone, whoever it be, creates an Othering effect. Of course, there was difference, but it might be of education or class or lifestyle, rather than some radical alterity. For some radical alterity remained (and remains) a fascination, but this was no longer the only game in town. Still, researchers continued to use some version of the old anthropological field work method, and stayed a long time. They too came back changed, though this might not be apparent from the clothes they wore.

Given the diversity of topics and field situations, the researcher is likely to have to improvise, much as Cushing did. For similar reasons, I suspect, projects tend to be more open ended. There has always been some tendency in anthropology for the field to shape the project, rather than the other way about. We all know the story about the researcher who went off to a remote part of the Andes to study a rare form of prescriptive marriage; but on finding after all that the people didn't practice it, he turned round and went home again. Most of us, confronted with a similar situation, stay and make the best of it, in the knowledge that one is not going to get another plane ticket in a hurry. In such a situation, what may happen is that the people in effect design the study according to their own preoccupations. I

must admit that both my Torres Strait research, and that with Aboriginal people in western NSW worked out like this. Perhaps this is why I regard serendipity as one of anthropology's strengths.

The anthropology of approximately the first two thirds of the 20th century had a basic ethnological agenda: an inventory of the world's cultures, usually achieved through a division of labour among national schools of anthropology and particular departments or specialist teachers. Oxford 'did' the Sudan, Cambridge Anglophone West Africa; France and Belgium Francophone Africa, the Dutch did Indonesia, and the US Latin America. Students could be sent virtually to see what was there, albeit with a fair expectation of what they were likely to find. There were various rationales for this kind of research: the natural historian's impulse to record whatever there was to be seen; the evolutionist's attempt to construct prior states of culture, and to preserve for science what seemed to be disappearing; and – as Marcus and Fischer reminded us some years ago, to serve as cultural critique for ourselves (1986). The shift away from the old stamping grounds of anthropology, to explore the effects of colonisation, decolonisation, urbanisation and globalisation, could be made broadly consistent with these agendas. Anthropology's other 'promise', 'to serve as a form of cultural critique for ourselves', acquired its own kind of urgency, as the West underwent rapid change, and its polarisation with the Rest became starker.

Even so, with this great smorgasbord of cultural delights, there seems sometimes no pressing reason for selecting one location or topic rather than another. A case of the 'personal' in search of something to 'equate' with. A discipline that works this way is at risk of losing its sense of necessity.

In Australia, the study of the Aborigines, has been an exception, driven over the years by a variety of necessities, on the one hand the various international debates about hunter-gatherers, kinship, and the elementary forms of the religious life; but on the other – and nowadays more pressing – the political and ideological necessities of human rights, racial equality, the national project and, at a practical level, of government policy. On both accounts, the core problem has been the nature of indigenous difference, in human history and in the context of a settler society.

Aboriginal studies have a long genealogy, beginning with the amateur anthropologists of the 19th century, through the natural scientists who were the first to undertake field research, notably Baldwin Spencer – currently playing at the museum of Victoria – and then on to the professional anthropologists at Sydney between the wars. Spencer was called upon to advise the Northern Territory administration, and one of the justifications for the establishment of the first anthropology department in the country was to have 'experts' who would advise government and train personnel – though Papua and New Guinea were probably of greater concern than the supposedly vanishing Aborigine. As the story goes, Radcliffe Brown, the first professor, was uninterested in such matters and reneged on the deal, leaving Elkin to save the day.

Government intervened again in the 1960s, founding the Australian Institute of Aboriginal Studies which, during its first decade, might have been called –to borrow from Rosaldo – the Institute of Imperial Nostalgia. Its task was to record the last snatches of a dying culture - in Spencer's words, 'before it was too late' – while government agencies set about assimilating its heirs into the Australian way of life. Policy matters were off limits as far as the Institute's research was concerned, and since there was a tendency to regard anything involving Aboriginal relations with the majority society as 'policy', the research program favoured archaeology, rock art, linguistics, and films of the 'last' performance of this or that ceremony, rather than social/cultural anthropology.

The climate changed decisively in the 1970s, with the official redefinition of indigeneity as enduring into modernity rather than vanishing before it. Over the next twenty-five years we have seen the legislation of native title, the institution of self management and consultative processes, the incorporation of Aboriginal art and dance into the national image, and the emergence of an indigenous intelligentsia, with a stake in the articulation of a national Aboriginal identity in Australian life.

Over the last 25 years, anthropologists have been involving themselves in this situation, particularly in native title process, but also as advisers on art, the environment and medicine. Work of this kind has provided employment for considerable numbers of graduate students, and also for senior anthropologists, some of them based in the academies. Given the parlous state of the humanities in the universities we should of course be thankful for this employment it offers our profession, as well as for the opportunity of supporting Aboriginal aspirations. Having done a little of this work myself, however, I have to admit to some misgivings. In particular, I have doubts about a discipline that is focused on native title research.

We are in a general sense complicit in what I would call a post-colonial situation. As we all know, and as Noel Pearson has forcefully reminded us, indigenous Australians are almost wholly dependent on government funding – to a much greater extent than they were a quarter-of-a-century ago. It is this source that funds the elite in the form of salaries and grants, and that supports families in the forms that we loosely call welfare. What I see here is the characteristic irony of post-colonialism: the tropes of national identity and the forms of self government, subjected to the iron law of economic dependence. Many of us feel uneasy about the way things are going and so we duck and weave to dodge the worst of the complicities to give a little help to our friends.

In the course of meeting this challenge, Aboriginalist anthropology has grown away from the rest of the discipline, much as Native American studies have diverged from the anthropological mainstream in the US. In part this is because of the particular nature of the problems that confront it – particularly the need to convert tentative ethnography into legal certainty. But it is also because of the particular constraints at work – the complicities that go with the territory.

Mainstream anthropology, like the other social and cultural disciplines, is as I said earlier about critique. But critique does not sit easily with advocacy and political engagement. How to play off the trope of 'time immemorial' against the critiques of 'tradition' and the cold society? The idea that Aboriginal culture is essentially unchanging and thus 'the oldest in the world' appeals to many Australians, and presumably strengthens the substratum of support for native title, as well as the Aboriginal art market. How then to write about change in Aboriginal society and culture?

Francesca Merlan, has remarked on the way 'traditionalism as ethnographic practice has developed as continuous with wider social practices of compartmentalisation of the changing and the unchanging'. In *Caging the Rainbow* (1998), she shows how Aboriginal cultural forms do not change only to disintegrate, but respond to novel circumstances. This destabilises the popular notion that the Aboriginal relation to place through totemism and myth is timeless, and it is probably the case that a historicised Aboriginality is harder to negotiate through the land claim process.

There are similar problems with accounts of 'classical' Aboriginal practices and institutions which are supposed to be 'in large measure in continuity' with the pre-colonial past, notwithstanding the post-colonial conditions in which they live their lives. As Diane Austin Broos has argued, 'a politics of difference involves not only a ritual, landed culture identified with a pre-contact past, but also the histories conferred on and realised by indigenous people who have had to negotiate their own and European-derived institutions in forging new modes of being themselves'.

Perhaps the most profound divergence arises with those who derive a totalised difference from a primordial past. As Gerald Sider and Gavin Smith have written, 'we are not so much dealing with any abstract, historically static quality – distance, otherness, 'alterity' – but rather with processes of differentiation. These processes simultaneously create and destroy various kinds of differences both between and within societies, and they turn out to be based as much on closeness and intimacy as one any imagined (or more, desired) difference'.

If, in the current climate of events, anthropologists want to challenge primordial and totalising constructions of Muslims or 'people of Middle Eastern appearance', they cannot at the same time uphold this kind of essentialism in the case of Aboriginal people.

If the native title agenda imposes one kind of constraint on anthropological work, so too do the various Aboriginal decision making bodies. Aboriginal people have been under

media gaze for thirty years and more, and they quite reasonably complain - as the Navajo used to do – that they are the most studied people in the world. Small wonder then that they want to have a say in what research is done, and also to ask what benefit the research might have for them. Some research contracts, drawn up by land councils, require that investigation be confined to a particular matter, such as a land claim, rather than building up a sense of context, as anthropologists normally do. This seems to me a formula for bad anthropology, even when the objective is a land claim.

Fortunately, not all Aboriginal people take such a restrictive view. The Yölngu have accepted researchers over many years, to the point where the 'typical' Yölngu family includes mother, father and anthropologist, much as the Hopi family used to do according to the old joke. It is still sometimes possible for anthropologists with longstanding relations with Aboriginal or Islander people to do anthropology in the old way, and not impossible to set up new relationships with families or individuals, once they have had time to look the researcher over.

Publishing is not as easy as it was, however. To put a new spin on Geertz's famous image, 'the native' is now reading over our shoulder'. Our subjects are now aware that what gets put in print acquires an authority, not just in the academy but in their own affairs. What they are likely to want is not arcane anthropological analysis, no matter how politically correct it is, but facts that can be utilised for a land dispute or a claim on government. My book on Torres Strait (1987) had a brief value for Islanders who were proposing secession, but now that they are preoccupied with native title, the old fashioned anthropology of the Cambridge Reports is much more useful. I notice that some anthropologists are producing texts specifically for the people they study, and some for their colleagues. Gillian Cowlshaw has recently published *Love Against the Law* (2000), the life of two of her long standing friends in the North. I tried a back-to-back text in my editing of Myles Lalor's autobiography (2000).

However, the subjects of anthropological writing may not be the only ones reading over our shoulder nowadays. There is a nationally oriented indigenous intelligentsia that feels entitled to police our research if not actually insisting only indigenous people have the right to speak about indigenous people. There are others in post-colonial studies who echo their views. In her recent book, *Indigenous Movements and their Critics* (1998), Kay Warren, describes the attempt of mainly North American anthropologists to work out a position vis-a-vis the Pan-Maya nationalists who emerged with the cessation of the civil war in Guatemala. Although the Maya situation is in many respects different from that of Aborigines, anthropologists in Australia would do well to read her book, and begin working out a future for themselves beyond working for native title. In particular they need to engage with Aboriginal scholars and writers, rather than the unsatisfactory combination of deference and evasion that seems to be all we can manage at the moment.

I must conclude. But before I put old Cushing back in his box, let me briefly mention my favourite among his writings, on the subject of continuity and change. The Zuñi had been first colonised by Spain, or rather by its missionaries, who had built a church, which however their converts quickly re-coded in terms of their own religious beliefs. Eventually the mission and the church were deserted, the building fell into decay, and presently tourists souvenired the surviving religious statues. Cushing asked whether the elders would not like to have the church of their forefathers restored. But no, they wanted to leave it the way it was. 'the church could not be rebuilt. It had been dead so long that, rehabilitated, it would be no longer be familiar to the [fore]fathers who in spirit had witnessed its decay. Nor could it be taken suddenly away. It had stood so long that, missing it, the [gods and spirits] would be sad, or might even abandon it.' (Green 1979: 180-1)

Perhaps Cushing is sending a message from beyond the grave to this meeting of Australian anthropologists in 2001. Perhaps we had better leave anthropology to crumble away, haunted by the shades of Elkin and Hogbin, Stanner and the Berndts, and presently anthropologists like me who are too old to change.

On the other hand, I should tell you that long after Cushing's time, the Zuñi did restore their church, and I have seen it.

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ⁱ See some interesting comments by Shelly Ortner (1991) on this point.

ⁱⁱ My main source on Cushing is Green (1979). I have not yet seen Eliza McFeely's (2001) book, though I have read McMurty's review (2001)

ⁱⁱⁱ This was the title of Edward Curtis's series of photographic series, popular around the turn of the century.

^{iv} Jean Martin, the founder of sociology at Latrobe University, was originally a student of Elkin, and conducted Master's research among cow cookies in Taree.