Reading doctors’ writing

Race, politics and power in Indigenous health research, 1870-1970

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A thesis based on work performed at Menzies School of Health Research and submitted for the Doctor of Philosophy degree of the Northern Territory University in March 2001.
Declaration

I hereby declare that the work herein, now submitted as a thesis for the degree of Doctor of Philosophy of the Northern Territory University, is the result of my own investigations, and all references to ideas and work of other researchers have been specifically acknowledged. I hereby certify that the work embodied in this thesis has not already been accepted in substance for any degree, and is not being currently submitted in candidature for any other degree.

[Signature]

David Thomas
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I am very grateful to my supervisor, Komla Tsey, and my associate supervisor, Peter d’Abbs, for their commitment to this project, their insightful advice and comments and their willingness to share some of their broad intellectual experience. I am particularly pleased that Komla continued to supervise this project after he left the Menzies School firstly to work at James Cook University and later at the University of Queensland.

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Dedication

This thesis is dedicated to the memory of my father, Cadwaladr Pirs (Bill) Thomas (1923-1980).
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<td>AMA</td>
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<td>Federal Council for Aboriginal Advancement</td>
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<td>FCAATSI</td>
<td>Federal Council for the Advancement of Aborigines and Torres Strait Islanders</td>
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<td>MVA</td>
<td>Murray Valley encephalitis</td>
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<td>NH&amp;MRC</td>
<td>National Health and Medical Research Council</td>
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<td>REPP</td>
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<td>UNESCO</td>
<td>United Nations Educational, Scientific and Cultural Organisation</td>
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Abstract

In this thesis, I have described the way in which doctors represented Aboriginal and Torres Strait Islander people in Australian medical journals. I have concentrated on publications in the *Medical Journal of Australia* before 1970. Such medical representations influenced not only the way doctors reading these journals thought about Indigenous people but continue to influence how all Australians, Indigenous and non-Indigenous, think about Indigenous people and their health.

I have explored the researchers' changing ways of writing about race and the ambiguities of how they described Indigenous Australians as sometimes different from and sometimes the same as settler Australians. In contrast to many researchers' views of science as objective, it has been possible to see how this research was influenced by broader social and political forces: from the colonial drive to settle northern Australia, through the changing politics of race, to the dramatic changes in Australian society and Indigenous politics at the end of the 1960s.

I also used the medical journals to begin to describe how research was performed in this colonial setting. Many of the researchers, such as John Burton Cleland from Adelaide University, wrote that Aboriginal people passively and powerlessly cooperated with their research. It has been possible to read the accounts written by Cleland and other researchers and begin to see the constrained agency of these same Aboriginal people with their considered participation given in exchange for material goods, medical care and entertainment. The researchers attempted to influence this exchange by using their more powerful political position in the colonial encounter and occasionally deceit.

The conclusion describes the implications of this history for present-day Indigenous health research. I encourage the writers of this research to more explicitly acknowledge the historical and political context of their medical research.
Chapter 1

Introduction
Figure 1.1. Map of places mentioned in thesis

See also figures 5.2 and 7.1 for maps of expeditions by Board for Anthropological Research and Adelaide University Department of Anatomy.
In the post-Mabo world, do Aboriginal people need a reinterpretation of their own tradition? Or is it the traditions of a white invader culture that need interpreting?


This thesis is a history of the way doctors have written about Aboriginal and Torres Strait Islander people in Australian medical journals. I concentrate on the first hundred years of this story, from 1870 to 1970, and on one medical journal, the Medical Journal of Australia (MJA). I have written this history mainly for those who read or write Indigenous health research and those Indigenous people who are described in this research. But it is also for all Australians, settler and Indigenous, whose thoughts and ideas about Indigenous people and their health have been influenced by the assertions based on this research that they have heard, read or spoken. In this introduction I begin by explaining why I have chosen to write this history and something of my approach to historiography. After introducing some of the existing work that illuminates my subject, I then describe the structure and methods of the thesis that follows.

An insider's story

This thesis is not a history of the health of Aboriginal and Torres Strait Islander peoples in Australia. My examination of the mere textuality of the words and images in medical journals is not meant to distract attention from the brutal reality of Indigenous peoples’ distress and suffering caused by their illnesses and early deaths. My concern about this Indigenous suffering need not diminish my decision to examine the research articles written by non-Indigenous doctors. In The birth of the clinic, Michel Foucault contextualised his examination of the transformation of French medicine and medical ideas that occurred two hundred years ago by claiming:
But to look in order to know, to show in order to teach, is not this a tacit form of violence, all the more abusive for its silence, upon a sick body that demands to be comforted, not displayed? Can pain be a spectacle? (p. 84)2

The catalyst for my thesis was not Foucault’s iconic text of post-structuralism but many years of real and ‘imagined’ conversations with Aboriginal people. These have mainly been with my Aboriginal work mates; I am a non-Aboriginal doctor and have mainly worked in Aboriginal Medical Services. My Aboriginal colleagues have told me that they have been angry or disappointed not only with the terrible health status of their people that is repeatedly reported in medical journals but also with the way researchers have chosen to write about them as Indigenous people. ‘Imagining’ their response, I have not always had the heart to tell them what my mob has most recently written (and I have read) about their mob. A Maori researcher, Linda Tuhiwai Smith, has described this predominantly oral rather than published Indigenous critique in her book *Decolonizing methodologies.*

The word itself, ‘research’, is probably one of the dirtiest words in the indigenous world’s vocabulary. When mentioned in many indigenous contexts, it stirs up silence, it conjures up bad memories, it raises a smile that is both knowing and distrustful. … The ways in which scientific research is implicated in the worst excesses of colonialism remains a powerful remembered history for many of the world’s colonized peoples. It is a history that still offends the deepest sense of our humanity. (p.1)3

Smith has emphasised memory and the links between the remembered past and the present. Whilst I do not wish to talk for these Indigenous memories, I can talk for my own slightly different memories of past Indigenous health research. I too had somehow long known that this research was bad; that it was racist. The cause of my past certainty is not clear. In spite of having read many medical articles as part of my work before I began this thesis, I cannot remember having read any of the medical journal publications that I soon describe in this thesis. Many of my non-Indigenous colleagues share my vague certainty about past Indigenous health research. The purpose of this thesis is not to investigate the history of the development of these Indigenous and non-Indigenous certainties about past medical
research – although I would be interested to read such a history. Rather, it is to take these Indigenous (and non-Indigenous) concerns about past medical research seriously by writing a detailed history of some of this research, just as historian Henry Reynolds has described his research as a serious response to Indigenous people’s remembered stories of frontier violence. 4

Whilst this thesis is not an Indigenous person’s interpretation of this history, it is still an insider’s story with the many advantages (and some disadvantages) that this brings to its telling and interpretation. It has been my job as a doctor to read medical journals and to occasionally write for these journals. I have learnt a great deal from this reading and writing and have modified the way that I have worked using this knowledge. I believe this has improved the quality of my work with Indigenous people. However, I have become increasingly irritated and disappointed with the way my colleagues in the present as well as the past have written about Indigenous people and their health and I have responded a couple of times using the letters pages of the MJA. 5- 7

One of these exchanges became quite bitter. I wrote that a history of the Royal Flying Doctor Service (RFDS) published in the MJA omitted to mention Aboriginal or Torres Strait Islander people. Indigenous people now account for a large proportion of RFDS’s work but were specifically excluded from medical treatment by RFDS’s parent organisation. 6, 8 My point was not that RFDS’s history was only a racist one but that this story could be told alongside the more obvious story of the organisation’s achievements. A radiologist from southern Queensland replied that I had been blinded from the truth by ‘well-meaning emotionalism’ (p. 168) and concluded:

Stop bleeding, Dr Thomas; delete the word racist from your vocabulary and be positive. (p. 168) 9

He also claimed that ‘History cannot be changed.’ (p. 168) 9 Similarly, an ophthalmologist from Geelong wrote that attempts to tarnish the image of John Flynn and RFDS would fail because the celebration of their success ‘is part of our
nation's folklore.' (p. 168)\(^\text{10}\) In contrast, Australian historian Greg Dening has written that 'The past can never be changed, but the memory of it in the present is always changing.' (p. 211)\(^\text{11}\) His distinction between past events and our histories of these events is crucial and has long been accepted by historians.\(^\text{12}\) Dening has also written of a quite different myth in Australia's folklore (Simpson and his donkey at Gallipoli) that 'the interesting thing about myths is their need to be true, not their fabrication.' (p. 72)\(^\text{13}\)

My purpose in telling this anecdote is not to contrast my example of discriminatory treatment with other writers' examples of non-discriminatory treatment of Indigenous people. Rather, it is to describe the ferocity and derision of these writers' responses to the suggestion that a history of the past can include both a celebration of noble achievements and an acknowledgement of ignoble failures. It also provides a hint of wider debates about the role of history and in particular histories of medicine such as this thesis.

**Writing history**

A week before I formally commenced this project, the *Lancet* published two articles promoting the value to medical education of examining the history of medicine and medical research.\(^\text{14,15}\) The first, by its editor, Richard Horton, advocated the reading of a 'canon' (p. 872)\(^\text{14}\) of great medical texts. His plea was about strengthening medicine by promoting its 'foundations' and 'the intellectual estate of medicine' (p. 874).\(^\text{14}\) It was not really about encouraging any historical analysis of these texts or about analysing or questioning medicine's power or place in society (or the place of society in medicine).

The second article was written by Michael Biddiss - an English historian.\(^\text{15}\) Whilst not suggesting a disregard of the histories of great men (and rarely women) and the contributions of their discoveries in medicine, he celebrated the increased emphasis on the social context of medicine in some recent historical research. He claimed that these newer social histories of medicine have begun to include those marginalised
by earlier historiography because of class, ethnicity or gender. They have included patients’ stories and have discussed changing ideas about health and healing and have even addressed bigger questions like the changes in the role of the state and state power.\textsuperscript{15}

In contrast to the triumphalist linear version of history in Horton's canon, Biddiss wrote that these ‘new’ historians have sought out and explored past ‘blind-alleys’ (p. 876) in the history of medicine and medical research. He wrote that they tell us much about the complexity of past societies and provide a critical antidote to the complacency of our conceptions of present ideas. In so doing, history gives us a greater understanding of 'our context in time' (p. 875).\textsuperscript{15} This emphasis on the social and historical context of medicine has considerable value in current medical education (and health care practice) that is saturated with enormous quantities of technical ‘facts’ bleached of any contextual information and is focussed on the most recent medical advances and evidence. In this thesis, I have adopted the approach advocated by Biddiss rather than by Horton.

This questioning of the social context of scientific knowledge production by historians has been accompanied by similar questioning of historical knowledge.\textsuperscript{16} Is truth and objectivity possible in history? In a spirited and convincing response, three American historians – Joyce Appleby, Lynn Hunt, and Margaret Jacob – rejected the nihilism of excessive relativism. They defended a future for both science and history. They wrote that ‘objective truth can be produced by deeply subjective people’ (p. 185) and so they prefer to think of the ‘social framing’ (p. 185) rather than the social construction of scientific knowledge.\textsuperscript{16} They optimistically promoted a ‘democratic practice of history’ which ‘encourages skepticism about dominant views, but at the same time trusts in the reality of the past and its knowability.’ (p. 11)\textsuperscript{16}

In the writing of Australia’s history, debates about histories of frontier violence and more recently the stolen generations have been ferocious both inside and outside the academy. Richard Hall has written that the reduction of these debates to Geoffrey Blainey’s slogan ‘black armband’ history avoids confronting the complex issues of
the debate. All that remains of the debate is to be for or against such black armband historiography. The ‘black armband’ historians have been accused of distorting (not just questioning) past events with their values from the present. The Prime Minister John Howard has been prominently involved in this debate and has expressed this concern about the implications of such historiographical distortions:

The “black armband view” of our past reflects a belief that most Australian history since 1788 has been little more than a disgraceful story of imperialism, exploitation, racism, sexism and other forms of discrimination.

In response, Howard and other opponents of ‘black armband’ history have been accused of delaying the reconciliation between Indigenous and non-Indigenous Australians in the present by their refusal to acknowledge the truth of past events (and say ‘sorry’). The simultaneously most vilified and revered of the ‘black armband’ historians has been Henry Reynolds. In his recent book *Why weren’t we told?*, he reflected on his work as a historian and responded to critics of so-called ‘black armband’ history:

Much critical, revisionist history springs from a belief that Australia should do better and is capable of doing so. It is written in the hope and expectation of reform...

They [revisionist historians] believed that it was time for Australia to come face to face with its past, to cast off the evasion and avoidance, that contemporary society could cope with the truth and find it bracing and challenging rather than threatening. (p. 245, 246)

He contextualised these remarks about his historiography by writing that he did not accept that the racist elements of our past wipe out the extraordinary achievements of Australia’s history. He dismissed as exaggeration claims that Australia’s past is only racist or that Australia is the most racist country in the world. Similarly, I do not mean to belittle the magnificent achievements of medical science: from

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*Howard quoted on the back cover of Hall (1998).*
immunisation to antibiotics to surgery rendered safe and painless by asepsis and anaesthesia. I believe that Australian medicine is sufficiently mature and secure to cope with a critical and honest examination of its past – including that part which I investigate in this thesis. Like Reynolds, I write in a spirit of reform with an optimism that my colleagues can do better. Indeed it has been apparent at most of the seminars that I have given and in informal discussions that I have had that most researchers do believe they can do better than those in the past. But Dening has warned:

There is no temptation stronger than to make the past a grotesque and laughable mimicry of our civilised present. The past, that way, can be mocked and judged so easily and with such little cost. (p. 124)\textsuperscript{11}

He has argued that all history is ‘cross-cultural’ – the past is not ‘us in funny clothes.’ (p. 209)\textsuperscript{13} There are many superficial similarities between past and present medical research but there are also differences that are not immediately obvious. Many of the words, diseases, medical explanations, and clichés of medical writing in past medical research publications appear deceptively familiar but their meanings may have changed. My greater facility with the genre of (present-day) medical research and its detail and nuances that I share with my medical colleagues may be a less certain insider’s advantage that I have over outsider historians. Dening would argue that we all start as outsiders to the past. However, with a historian’s saturating reading of the past’s texts, all the while being wary for shifts of meaning and silences, together with some imagination we might begin to understand the past as insiders.\textsuperscript{13} Dening has written that imagination is essential to this understanding of the past.\textsuperscript{13}

In order to understand how these researchers wrote what they did, it is better if we abandon hindsight’s knowledge that some of their ideas were later discredited and instead imagine the humanity we share with them. This will not always be easy. If present day researchers, like myself, as well as other readers are to learn from this history, we should try to understand these past researchers even if we loathe what
they did and wrote. There is little value in merely playing spot-the-racist or find-the-racist-remark.

I became more confident of the moral and ethical integrity of this task following some reading in a quite different field – histories of the Holocaust. I was particularly struck by Jewish psychologist Robert Lifton’s reflections on his feelings during his interviews of Nazi Doctors for his troubling book about the role of doctors and medical images in the Holocaust.18 He wrote of his shame at trying to enter ‘the psychological world’ (p.11)18 of these doctors and his resisted urge to morally confront them for their heinous actions. But in order to make his research as accurate as possible and useful in understanding and combating their evil, he felt obliged to be ‘fair’ (p.11) to these doctors. His balanced approach and examination of ‘unremarkable’ as well as the more well known and evil Nazi doctors such as Josef Mengele led to a haunting remark by an Auschwitz survivor to Lifton about these doctors (and their potential for genocide). He said ‘But it is demonic that they were not demonic.’ (p. 5)18

The research project by the United States Public Health Service that observed the effects of untreated latent syphilis on four hundred poor Black men from Alabama from 1932 until 1972 has been compared to Nazi medical research. These government researchers deceived these men who thought they were part of a treatment program. The researchers withheld treatment thought to be effective (including penicillin when it became available in the late 1940s). The research was only stopped after media reports in 1972; there had been no criticism of the 13 articles published about the study in American medical journals. Susan Reverby has recently edited a fascinating and disturbing collection of documents and historical research about this syphilis research: Tuskagee’s truths: rethinking the Tuskagee syphilis study.19 The approach of the historians of this research, like that of the historians of the Holocaust, with their attention to medical research’s links with racism and state power, is very relevant to my thesis. The moral ambiguities and complexities of the issues discussed in her book are much more confronting and illuminating for present-day researchers and readers than the certainties. Reverby has written in her introduction:
Ultimately our attempts to create a Manichean story in literally white and black terms explains little, leaving us without the necessary elements to attempt a more complete effort at understanding. (p. 3)\textsuperscript{20}

Later in this same book, John Fletcher has explained how it is still possible make some moral judgements about past events that do not undermine this understanding of their complexity.\textsuperscript{21} Indeed, he has argued that such judgements require a detailed understanding of the historical context to avoid the fallacy of moral triumphalism, which imposes present-day moral judgements on the past. In spite of these risks he has written:

If we fail to judge the past, however measured our judgements, we will lose in our collective memory the harm and suffering caused by older practices. We will lose, too, in our moral evolution the ability to change those harmful practices. (p. 279)\textsuperscript{21}

My thesis is largely dedicated to describing and understanding the complexity of the historical context of past Indigenous health research practices and representations rather than judging these past events. But I acknowledge that present-day researchers of Indigenous health cannot avoid making some implicit judgement of these past practices and representations. They are required to make decisions about how to conduct and describe their own research. These decisions require them to choose whether or not to continue to use past research practices and representations. It seems only fair that I too take the risk of declaring some of my own tentative judgements of this past research. These mainly appear in the concluding section of individual chapters or in the Conclusion (chapter eight). Like present-day researchers, I have concentrated on elements of the past that persist in present-day research practices and representations. Of course, my judgements are not meant to be prescriptive but to encourage readers to reflect and make their own judgements.
A theoretical catalyst and reference point

Leela Gandhi has described the publication in 1978 of Edward Said's book *Orientalism* as both a 'catalyst' and a 'reference point' (p.64) in the emergence of postcolonial theory and postcolonial studies. She also has described the more intimate stories of its revelatory impact on its readers in India and other former European colonies. Said's book provided the words to articulate feelings they had not previously been able to express. I too can remember the liberating excitement of first reading Said's description of the links between colonial texts and colonial power; links which now seem so obvious. And I remember immediately beginning to think about the links between colonial *medical* texts and colonial power in Australia. Said's work, like the work of Henry Reynolds and Greg Dening, has been an inspiring influence on my intellectual development and my decision to write this thesis. The discussion of the ideas of these three men at the start of the thesis is meant not only as a gesture of intellectual honesty towards my readers but of intellectual acknowledgement and gratitude towards these writers.

In *Orientalism*, Said described the way the West has written about its colonies and their peoples, which he called Orientalism. His focus was on the Middle East but he expanded his attention to other colonial contexts in his later book *Culture and Imperialism*. He defined Orientalism as both the academic study of the Orient and the more general Western 'style of thought' that stressed that the Orient is very different from the Occidental 'us'. He argued that this distinction was important not only in defining the Orient as the Other but in how the West saw itself - in opposition to this Orientalist Other. Finally, he described Orientalism as a Foucauldian discourse and wrote:

Orientalism can be discussed and analysed as the corporate institution for dealing with the Orient - dealing with it by making statements about it, authorizing views of it, describing it, by teaching it, settling it, ruling over it: in short Orientalism as a Western style for dominating, restructuring, and having authority over the Orient. ... Moreover, so authoritative a position did Orientalism have that I believe no one
writing, thinking, or acting on the Orient could do so without taking account of the limitations of thought and action imposed by Orientalism. (p.3)²²

These sentences demonstrate some of the complexity and scope of the intertwining of knowledge and power in the notion of a "discourse".²⁵-²⁸ A discourse is the accepted ways of writing, talking and thinking about an institution such as medicine or colonialism. What is important is the internal consistency of the discourse rather than its correlation to what it describes – it has a life of its own. The conventions of the discourse constrain the institution itself by limiting its available texts, language and thought. Nothing in the institution is not affected by its discourse. All that we know of the institution is constrained by its discourse. Thus the texts of colonial discourse helped shape the way the colonisers dominated the colonised and how both parties imagined themselves, each other and colonialism itself. The texts became colonialism's hidden straightjacket – they determined (rather than justified) colonialism as colonialism determined them. The writings of travelers, novelists and academic specialists that Said described later in his book were no longer just books but were now instruments of colonial power – this is what Said described as the 'worldliness' of these texts.

Foucault had earlier used discourse to explain the links between knowledge and power in European institutions. In *Birth of the clinic*, he weaved his discussions of knowledge and power through his description of fifty years of French medicine and his theoretical constructs – the Clinic, the Gaze, and the Body.² In the last decade there has been increased attention by scholars to Foucault's notion of 'governmentality'.²⁹ Governmentality encompasses the close inter-relationship of his ideas on the rise and reach of social control and surveillance of individuals by the state with his work on knowledge, power and discourse. Foucault's ideas have been particularly influential in medical sociology.²⁵,³⁰-³² Said's innovation was to identify the consistency between the discourses of all colonial institutions and conflate these discourses to the single 'colonial discourse' – Orientalism.²³,²⁶

Said does have his critics. They are not confined to threatened Orientalists and have drawn attention to the homogeneity and generalising arguments of *Orientalism*, his
inattention to resistance and opposition to Orientalism, and his ambivalent use of Foucauldian methodology. Whilst Said emphasised the generalisability of his arguments in Orientalism he also wrote in its introduction that the relationship of Orientalist texts to colonial political power and cultural hegemonic power was 'uneven' rather than 'direct' (p.12). One effect of the wide acceptance of Orientalism and its generalising arguments about colonial texts and colonial power is that it has enabled a more subtle discussion of the worldliness of these texts – their inconsistencies and their particularities in different colonial contexts. The surprises of these subtle inconsistencies provides the 'human' element to historiography's attempts to understand the more general forces of history such as colonialism.

In their review of Said's work, Bill Ashcroft and Pal Ahluwalia have described his increasing ambivalence with respect to a rigidly Foucauldian approach, especially to its lack of political commitment or 'worldliness'. Said wrote that there was 'a "real" Orient' (p.5) with real people with their own cultures and lives – 'a brute reality obviously greater than anything that could be said about them in the West.' (p. 5) (my italics) In the same way, I would emphasise the greater importance of Indigenous peoples' lived experiences of diseases and early death than of the representations of Indigenous health, even though I accept that they are mutually constitutive. I have not only examined the representations in medical journals merely as a discourse. I have examined them as a record (albeit a record constrained by the discourse) of the research encounter they claim to describe.

Said described his work as part of a wider project of resistance to imperialism and Orientalism; his politics have never been hidden and he has often explained the importance of his politics to his intellectual work. Said has emphasised that all intellectual work is political and has encouraged a 'passionate engagement' with politics but not with professional politicians and their political processes, which he described as 'Gods that always fail' (p. 77). He has suggested intellectuals adopt a slightly more marginal and secular criticism of dominating power: choosing criticism over solidarity and anti-imperialism rather than nationalisms. He has also promoted a similar distancing from professionalism in academia with its increasing specialisation and its associated obfuscating jargon.
The Aboriginal academic and activist, Marcia Langton, whose work I discuss in the next section of this introduction, has said that she has adopted a similar approach to her work as a public intellectual as Said. She called an essay about representation 'anti-colonial cultural criticism' (p. 7) choosing to emphasise her political position in relation to Australian colonialism rather than using the more typical academic label – postcolonial. She has also despaired of other academics' preference for 'obscure debate' (p. 236) amongst themselves rather than engagement with the public and politics.

It is not just Said’s theoretical insights about colonial discourse, Orientalism, and the Other, but also his engagement, like Langton, with the politics of his subject and his examination of his insider as outsider position as an author that has made his work so useful for the reading and writing of this thesis. I have already hinted at my political position in this story as a non-Indigenous doctor and occasional researcher who has mainly worked for Aboriginal organisations. I share these organisations’ explicit agenda of Aboriginal self-determination and their opposition to racism and the remnants of Australian colonialism. But I have not limited my narrative and its interpretation by forcing it to replicate and conform to Said’s approach and findings or to my personal politics. Theory has the potential to constrain as well as to illuminate. I agree with Inga Clendinnen when she wrote:

Large theories may generate good questions, but they produce poor answers. The historian’s task is to discover what happened in some actual past situation - what conflicting or confused intentions produced what outcomes - not to produce large truths. The most enlightening historical generalisations tend to be those that hover sufficiently close to the ground to illuminate the contours and dynamics of intention and action in circumscribed circumstances. (p. 21-22)

Similarly, Stuart Hall has described the contribution of theory to his pioneering work in Cultural Studies as either a useful interruption or ‘wrestling with the angels’ (p.266). He described this wrestling metaphor for his theoretical work as moving beyond mere recognition and repetition of existing theoretical explanations and
approaches and their application to new problems: ‘The only theory worth having is that which you have to fight off, not that which you speak with profound fluency.’ (p. 265)\textsuperscript{37}

Often whilst writing this thesis, I have interrupted my engagement with the central narrative of the past, sometimes reluctantly and sometimes not, to struggle with the questions about my approach and arguments raised by Said, Foucault and the scholars they have influenced. However, I acknowledge that these are not the only writers who have contributed to the theoretical debates about history and colonialism. Robert Young in \textit{White mythologies} has provided a detailed and critical overview of the theoretical tensions and difficulties of history and historiography in a colonial context. In particular, he has investigated the contributions of Marxist and post-structuralist scholars as well as more recent theorists of postcolonialism.\textsuperscript{38}

\textbf{The politics of the representation of Aboriginal and Torres Strait Islander peoples}

The work of Said and Foucault provided a useful theoretical fillip to Australian scholars who had only recently begun to examine the representation of Indigenous people and the social construction of Indigenous identities.\textsuperscript{39} In 1991, Bob Hodge and Vijay Mishra described a similar phenomenon to Orientalism operating in Australian culture and literature and named it ‘Aboriginalism’.\textsuperscript{40} In the following year, the \textit{Journal of Australian Studies} published a special edition entitled ‘Power, knowledge and Aborigines’ which investigated European Australian representations of Aboriginal peoples.\textsuperscript{41} In the introduction to these essays, Bain Attwood acknowledged the debt of this research to Said and Foucault.\textsuperscript{41} Other authors have not always been so explicit in acknowledging the usefulness of the theories of colonial discursive power.

This expanding interest in the representation of Indigenous Australians in the arts, sciences and government administration has paid little attention to medical representations. But this other research still provides useful contextual information
and questions for my history of representations in medical journals. And so I have chosen to describe some of the more general findings of this research before discussing the research about representations in medical journals.

Before I consider the various constraining discourses about Indigenous people, I should mention the ultimate constraint of exclusion. Langton has written that ‘The easiest and most “natural” form of racism in representation is the act of making the other invisible’ (p.24). This invisibility of Indigenous people was particularly apparent in the discourse of Australian history in the twentieth century until the late 1960s. The anthropologist WEH Stanner called this ‘the great Australian silence’ (p. 27) in his 1968 Boyer lectures. He lamented the marginal status of Aboriginal people as a ‘melancholy footnote’ (p. 24) in Australian histories but he saw positive signs of an end to this ‘disremembering’ (p. 63). Henry Reynolds and other historians responded to Stanner’s wake-up call and now histories of Indigenous people have a prominent place in Australian histories.

Attwood has argued that the different ways that all Australians relate to these histories of Indigenous people have become central to Australians’ representations of their own present and future.

Stephen Muecke has identified three dominant non-Aboriginal discourses of Aboriginality: the Anthropological, the Romantic and the Racist. Attwood has written that the idea of Aborigines as ‘primitive’ is rarely absent from these non-Aboriginal discourses, either as noble or ignoble savages. Jeremy Beckett has argued that in these discourses a person’s Aboriginal identity and rights were determined by their links with ‘the Past’ - in particular, with Aboriginal people at the time of European invasion. The most concrete and powerful of these connections used is heredity and the metaphor of ‘blood’. Similarly, remote Aborigines and their ‘culture’ become a focus of constructions of Aboriginal identity because they ‘live in ways that are regarded as in some sense the same as those followed before the arrival of Europeans’ (p.6).

The limited number of discourses of Indigenous identity constrain our (Indigenous and non-Indigenous) thinking and experience and are a particular burden for Indigenous people. Non-Indigenous Australians have largely built their
understanding of Indigenous people using these discourses rather than by relating with actual Indigenous people. Muecke wrote that the limited number of ways of ‘being Aboriginal’ (p. 40) in these discourses rely on prescriptive notions of Aboriginal ‘culture’ that have become ‘the prison of twentieth century Aborigines’. (p. 40) These constructions of Aboriginality are often offensive to and create major problems for Aboriginal people who live in towns and cities and those of mixed descent. Until more recently, with few exceptions, anthropologists and other writers paid little attention to these Aboriginal people suggesting ‘an absence of Aboriginal cultural traits’ (p.6) and thereby undermining their Aboriginality.

Muecke and Beckett have noted that Aboriginal people have confronted and challenged these powerful non-Aboriginal representations of themselves. Aboriginal people have revitalised ‘traditional’ culture and language in towns and cities and have successfully appropriated the power of these representations of Aboriginality for their own political struggles. They have also constructed their own discourses of Aboriginality based on independence, survival, and the shared experience of colonialism as the colonised. But Beckett warned that the power to influence public constructions of Aboriginality is not equally held by the State and Aboriginal people.

Langton has eloquently argued for the need to investigate and understand the power and the politics of the process as well as the result of representation in order to begin to undermine the colonial hegemonic discourses of Aboriginality. In her essay, ‘Well, I heard it on the radio and I saw it on the television...’, she dismissed the ‘banality’ of the obsession with positive versus negative images of Aboriginal people in film. Like American critic Michele Wallace, Langton argued that such an approach avoids confronting the racist politics of most representations. Instead she proposed concentrating on the politics of the dominance of the (usually non-Aboriginal) person making the representations of Aboriginal people. One response to this politics is to leave such representing to only Aboriginal people as a group of Aboriginal historians proposed in 1981. But Langton suggested that it may not be so simple.
There is a naive belief that Aboriginal people will make “better” representations of us, simply because being Aboriginal gives “greater” understanding. This belief is based on an ancient and universal feature of racism: the assumption of the undifferentiated Other. More specifically, the assumption is that all Aborigines are alike and equally understand each other, without regard to cultural variation, history, gender, sexual preference and so on. (p.27)

Langton mirrored Said's concern which he expressed when he wrote that the 'subaltern' histories of India should not be construed as merely 'separatist', thereby running 'the risk of just being a mirror opposite of the writing whose tyranny it disputes' and being 'as exclusivist, as limited, provincial, and discriminatory in its suppressions and repressions as the master discourses of colonialism and elitism' (p. viii). Also like Said (whom she did not cite in this essay), she claimed that conventions of film can trap both Aboriginal and non-Aboriginal producers in their conventional racism. If not the promotion of ‘positive’ representations of Aboriginal people or the delegation of responsibility to an Aboriginal committee or author, what did Langton suggest?

Langton seemed most enthusiastic about films and videos that confront head-on the politics of the processes of representation and the audience’s previous models of Aboriginality. Her cited films by Tracey Moffatt and the Warlpiri Media Association make explicit the open-ended dialogue between the different imagined models of Aboriginality of the directors, actors and audiences. Both Aboriginal and non-Aboriginal people participate in this testing and adjusting of their working models of Aboriginality and their mutual comprehension – which we would now call part of the Australian political process known as ‘reconciliation’. This self-conscious dialogue necessarily confronts the histories of the colonial encounter, racism, sexism and the dominant discourses of Aboriginality.

Her challenge for this thesis, and that also made in Eric Michael's work with the Warlpiri Media Association which she acknowledged and extended, is to examine the politics and power of the text's production and circulation as well as the representations in the text itself. Michaels wrote that even though an ABC
documentary filmed at Yuendemu was 'accurate, informative, and ... respectful' (p. 78), it could replicate the politics of colonialism leading to a loss of Aboriginal control because it disguised the politics and story of its production and broadcast. Similarly, Tim Rowse wrote that this context may be more important than a text's accuracy and its 'positive' images in assessing the politics of the representation of Aboriginal health. Neither the goal nor the gold standard of this thesis is more 'positive' medical representations of Indigenous people.

The dialogue between Indigenous and non-Indigenous people about their positions and representations is rarely explicitly or self-consciously portrayed in the medical journal publications that I discuss in this thesis. However, these texts occasionally included the non-Indigenous authors' one-sided opinions about the texts' production and dissemination. Patrick Wolfe's remarks about his examination of anthropological texts are generalisable to my study of medical research texts. He wrote that to try to find a hidden Indigenous voice in the texts would be 'empirically self defeating' not just a 'moral or political scruple' (p. 4). It is not possible to resuscitate the Indigenous voice that was present in dialogues that occurred in the field in the past (with their very lop-sided balance of power) but was hidden in the texts.

However, I hope this thesis has an impact on the audibility of the voices of the Indigenous researched in future texts. This thesis aims to promote a self-conscious dialogue about medical representations between Indigenous people and non-Indigenous people and between the Indigenous researched and researchers (both Indigenous and non-Indigenous). Ideally, the researchers will increasingly confront the difficult politics of the issues that emerge in these dialogues and include them in their medical journal publications rather than hide them with the conventions of the genre.

Some brief remarks about the politics of the production/ writing and circulation/ reading of this thesis now conclude this discussion of the politics of representation. Ian Anderson, an Indigenous doctor and academic critic, has written of the need for non-Indigenous academics to begin to imagine an Indigenous audience when writing
about Indigenous people. In contrast, the medical research publications that I describe were of the tradition in which a non-Indigenous expert wrote about Indigenous people for other non-Indigenous people to read. They can be read as ‘internal conversations’ (p. 4) amongst non-Indigenous doctors. The writers’ use of the pronoun ‘us’ to describe non-Indigenous and ‘them’ for Indigenous Australians makes it clear that they did not expect Indigenous people to read these texts – and in 1970, at the end of the period I have examined, there were as yet no Indigenous Australian doctors.

Anderson wrote that the first step towards an Indigenous audience is to choose research questions which might interest them. In the quotation that begins this chapter, he proposed research about ‘the traditions of a white invader culture’ (p. 371). Muecke has similarly advised that non-Aboriginal researchers writing about Aboriginal people concentrate on their insider stories about their own institutions, as I have done in this thesis. He also convincingly suggested making an explicit space for an Indigenous audience in the telling of these stories – this is the space created by the politics of his refusal to interpret the meaning for Indigenous people of these stories.

**History, representation and Indigenous health research**

There has been only one overview history of Indigenous health research in Australia. It is Lindsey Harrison’s 1979 MA thesis – *Racial ideas and the health care of Aborigines: an analysis of articles on Aborigines in the Medical Journal of Australia 1914 – 1979*. As it is an unpublished thesis, it is not easy to find and read. I only found and read it six months after I had commenced this project when I noticed its citation in the final ‘Miscellaneous’ section of Thomson and Merrifield’s bibliography of Aboriginal health. I have never seen it cited in another publication, although Harrison summarised her findings in a one page ‘vignette’ in a book of introductory essays about Aboriginal health – but in this vignette she did not cite her thesis.
Other historical research only partly illuminates small fragments of this story, usually with Indigenous health research not as its main focus. In spite of its different focus, this research by historians and doctors has considerable insights for the history of Indigenous health research and is referred to throughout the thesis at relevant points in the narrative. Historians have included examinations of health services for Indigenous people and Indigenous health outcomes but less often Indigenous health research in their more general histories of the colonial encounter or of health services. And only a few of the increasing numbers of medical writers about Indigenous health have included historical research as a central focus of their research. Ernest Hunter's *Aboriginal health and history* and Stephen Kunitz's *Disease and social diversity* have been notable recent exceptions to this medical inattention to the value of historical research into Indigenous health.\(^{57,58}\)

Harrison's thesis provides the most useful introduction to the history of Indigenous health research in Australia and my thesis. She also chose to investigate research in the *MJA*. Her thesis concentrated on one of the major themes that emerged in *MJA* publications about Indigenous people – 'race' and racism.\(^{54}\) The history of these scientific ideas about 'race' is also a central theme of my thesis. Throughout the thesis I discuss the subtle rather than dramatic differences in our interpretations of the portrayed racial ideas. There is now an enormous body of published literature about race and racism in medical science.\(^{b}\)

Harrison identified 171 *MJA* publications about Aboriginal people before 1970 (and a further 172 in the 1970s). Unfortunately she did not appear to be aware of Moodie and Pederson's 1971 Aboriginal health bibliography,\(^{59}\) which together with her cited publications has enabled me to identify nearly twice as many (313 before 1970) *MJA* publications about Indigenous people. This and a subsequent Aboriginal health bibliography published in 1988\(^{55}\) (and more recent electronic databases) are excellent tools in identifying publications about Indigenous health. The locality and subject indexes in the bibliographies also provide an indication of the principal concerns of the research publications. Harrison's discussion of the major themes

\(^{b}\) Much of this research is American, see for example the database at Massachusetts Institute of Technology's 'History of race in science' web site - http://di-145c.mit.edu/racesci.
that emerge is not greatly skewed by her smaller selection of *MJA* publications. But my identification of a greater number of identified publications has enabled a more elaborate contextualisation of those publications discussed.

The main difference between our two theses is not our source material but the questions Harrison and I have asked of this material. Her questions and concerns twenty years ago were different to mine today. She concentrated on questions about ‘race’ and I have added more detailed questions about the representation of Indigenous people, politics and power. This is not a criticism of Harrison’s scholarship. Most of the research that has informed my approach (and has been discussed in this introduction) was published after she completed her thesis. Historian Ann McGrath has explained the reason for this need for Australian historians to look beyond race to colonial power relations:

> The paradigm of race relations provides an inadequate tool to explain the particular tensions between indigenes and colonisers. Colonialism has wider explanatory potential, for the conflict between Aboriginal and non-Aboriginal people was not premised only on “race” notions but upon the wider power relations implicit in a colonial past. In this light, the relationship of Aborigines to the nation also requires close examination.’ (p. xxviii)60

In 1994, the *MJA*’s editor and librarian wrote a one-page history of Aboriginal health research in the *MJA*. After quickly describing the examination of Aboriginal bones and artefacts in the earliest of these articles in the *MJA* as ‘not medical, but anthropological’ the authors quickly jumped to the 1970s. Linda Smith has warned that medical ‘scientific research’ is perceived by most indigenous peoples as just as random and damaging as (and so not distinguished from) amateur collecting and the skull measuring of physical anthropology. Similarly, the Indigenous staff at the Aboriginal medical service in Darwin, Danila Dilba, gently mocked me when, during a brief presentation, I emphasised the difference between the physical anthropology published in the *MJA* and that published in other non-medical journals.
The few other examinations of the representations of Indigenous health have also concentrated on research since the beginning of the 1970s. Sherry Saggers and Dennis Gray's introductory book about Aboriginal health compared their own 'political economy' approach to 'ecological', 'psychiatric' and 'community development' theories that had emerged in research published in the 1970s. They had quickly dismissed the utility of earlier explanations of Aboriginal ill health based on 'biological determinism'.

Peter Lake performed a content analysis of 404 Aboriginal health research publications from 1982 to 1991. He reported on which Aboriginal people were included in the studies and the topics of the research. He found an under-representation of research on Aboriginal people living in cities, of research into the conditions that caused most Aboriginal deaths, and of intervention studies and evaluations of health promotion programs. Mark Brough's interesting discourse analysis of a decade (1988-1998) of representations of Indigenous health focussed on three newspapers rather than scientific publications. He found representations of failure and fatalism that might tend to blame individual Indigenous people for their ill health rather than wider social forces.

The much more vigorous and widespread debates about the politics of the processes (not just the texts) of Indigenous health research have had much greater influence on health researchers' thinking and practice than these historical examinations of Indigenous health texts. These public debates began with the arguments of Indigenous people at a meeting about the 'ethics' of Aboriginal health research held in Alice Springs in 1986. The meeting recommended greater Aboriginal control of research - not only more consultation and involvement in all stages of the research process but also ownership and control over the publication of research results. These recommendations have been incorporated to various degrees into major government reports about Indigenous health and different versions of National Health and Medical Research Council guidelines for researchers. David Paul and David Atkinson somewhat simplistically recently assessed the value of the research in a special Aboriginal health edition of the Australian and New Zealand...
Whilst these debates have become known as about the ‘ethics’ of research, they are about what Michaels called the politics of text production and text circulation. I have shown that there has been little discussion of the representations in the research texts themselves and, apart from Harrison’s thesis, almost no sustained attention on research published before 1970. The focus of academic attention has been quite different to that in film and justifies a greater emphasis in my thesis on the texts themselves. However, the debates in the present about the research process have led me not to neglect reading these research articles in the past for traces of evidence about the research process.

**This thesis**

The body of this thesis is arranged in three chronological parts. I have not divided my attention equally amongst the one hundred years from 1870 to 1970; rather, my emphasis has been determined by the relative quantity of and changes in Indigenous health research published during this period. The first part includes only one chapter but covers research in the first half of the historical period. It describes research before 1914. The *MJA* was established in 1914 by the amalgamation of two other Australian medical journals: the *Australian Medical Journal* and the *Australasian Medical Gazette*. Medical journals in the past were not the same as journals today. The history of the *MJA* and other medical journals provides useful contextual information for the examination in this thesis of the Indigenous health publications in these journals. The first part of the thesis tells the story of the early general medical journals in Australia before the *MJA*. It then describes the small number of publications in these journals about Aboriginal and Torres Strait Islander people.

The remainder of the thesis concentrates on publications in the *MJA*. Like Harrison, I have chosen the *MJA* because of its ‘venerability’ (p. 1), ‘continuity’ (p. 1) and because it was the journal of the major medical professional body in Australia.
The *MJA* remains the leading generalist medical journal in Australia and the journal of the Australian Medical Association (AMA). By concentrating on the *MJA* and avoiding specialist journals that few will have read, I hope to avoid one of the complaints about similar research using discourse analysis: which is that it places too heavy an explanatory load on texts that few have read. The *MJA* was not only frequently read by doctors but was also frequently chosen by writers to publish their health research about Aboriginal and Torres Strait Islander peoples. Of all the publications (from 1914 to 1969) in Moodie and Pederson’s Aboriginal health bibliography, 17% (263/1569) were published in the *MJA*.59

Part two of this thesis (chapters three to five) examines *MJA* publications before 1950. Chapter three introduces and contextualises the more detailed stories in the two chapters that follow. It includes a brief description of some of the other themes that emerged from the publications before 1950 that will not be addressed in these chapters and the significant contemporary events in the history of the *MJA* and other Australian medical journals. A quantitative analysis of the *MJA* publications about Indigenous people (compared to a random selection of other *MJA* publications) is also included in chapter three.

Chapter four describes some tropical medical research in north Queensland. This research was intimately associated with contemporary ideas about race and with the history of the Australian national identity, economic development of northern Australia, and health care administration in Australia. Chapter five concentrates on the physical anthropological research (especially blood group serology) performed by JB Cleland and other Adelaide researchers from the Board for Anthropological Research on expeditions to Central Australia. Cleland’s published descriptions of his research practice provide a fascinating, if one-sided, account of the research encounter on the colonial frontier.

The *MJA* publications in the 1950s and 1960s are discussed in part three. There had been a dramatic increase in the number of *MJA* publications about Indigenous

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5 Throughout this thesis I only use the word ‘contemporary’ to mean ‘at the same time’. I do not use it to mean ‘present-day’.
people in these decades compared to the years before 1950. There had also been an equally dramatic change in the way researchers wrote about race. After the 1967 referendum, which successfully removed the discriminatory references to Aboriginal people from the Australian constitution, there was another similarly large increase in the number of Indigenous health research publications in the *MJA*. The late sixties and early seventies were associated with further changes in the way Indigenous health research was written as well as dramatic changes in Indigenous politics and broader changes in Australian society. The historical period examined in the thesis closes at the end of sixties.

Part three has a similar structure to part two: an introductory and contextualising chapter (chapter six) followed by two more detailed chapters. Chapter seven discusses the research performed when the Board for Anthropological Research recommenced after the Second World War its expeditions to central and later northern Australia. The comparison of the research discussed in chapter seven with the inter-war Adelaide research (chapter five) demonstrates the changed approach by scientists to race following revelations about the Holocaust. This chapter also examines the power relations between researchers, Indigenous people and government in the Northern Territory in the assimilation era.

Chapter eight describes the letters to the *MJA* editor about Indigenous health in the 1950s and 1960s – especially those by Barry Christophers of the Federal Council for the Advancement of Aborigines and Torres Strait Islanders (FCAATSI). Surveys of readers showed that the letters pages were the most frequently read section of the *MJA*. This chapter gives an indication of the ideas and responses of the doctors reading the *MJA* publications about Indigenous people. The story of FCAATSI’s political struggle for equal Aboriginal rights is also an important focus of this chapter.

The conclusion (chapter nine) brings the story from the past to the present. It begins with a brief quantitative analysis of the *MJA* publications about Aboriginal and Torres Strait Islanders from 1970 to 1999. I then discuss my thoughts on the
implications of my analysis of research publications from the past for the present, in particular for present-day health researchers.

Research methods

The methods of this thesis might be described as either a historical analysis or a discourse analysis of the research publications. As a historian, I have examined the research publications as my main primary sources supplemented by a small amount of archival and oral sources. I have immersed myself in these primary sources, repeatedly returning to them and re-reading them (my task of identifying source material has been made easier by Harrison’s thesis and the Aboriginal health bibliographies and databases). I have then contextualised my narrative, assessment and interpretation of these primary sources using extant historical research as secondary sources.

Norman Fairclough has suggested that discourse analysis is best limited to a small number of texts. I have mainly used the quantitative analyses of texts (along with preliminary qualitative analyses) to select the texts for this more detailed examination and discourse analysis in chapters 4, 5, 7 and 8. Given this purpose of the quantitative analyses, I have not included summaries of their results in the main chapters of the thesis but in appendices 2, 3, and 4. Appendix one describes the methods I employed in these quantitative analyses. The main chapters only discuss which authors and institutions were selected for more detailed study by the quantitative analysis in the appendices and the shifts in the level of attention to Indigenous health in the MJA charted by these analyses.

Simon Chapman has written that the major limitation of these quantitative (content) analyses is their ‘quantification bias’ (p. 489) – the assumption that the elements of the texts that appear more frequently are more important. In spite of their ‘scientific’ objectivity and reproducibility, he wrote (and I agree) that these analyses miss the important intertextual and contextual relationships of these textual elements.
The more detailed textual, intertextual, and contextual elements of discourse analysis is most obvious in chapters 4, 5, 7 and 8. As I have no training in linguistics, I have concentrated on the broader discursive components of the language and images in the texts rather than attempt a more stringent and microscopic linguistic analysis. The intertextual analysis relies on examining the research publications not just as texts but also as the context of other texts. Other histories (of medicine, anthropology, and more general histories of the colonial encounter in Australia) and a small number of archival and oral sources are also used to describe the historical, social and political contexts of the research publications.

Histories of tropical medicine in other colonial contexts have been particularly useful in this contextualisation – and as models for this thesis. In the last twenty years these have ceased to be confined to somewhat uncritical celebrations of the achievements of doctors and medicine on the colonial frontier. There has been an increase in the number of histories that have begun to examine the links between tropical medicine and colonial power. In these histories, tropical medicine has been increasingly viewed as a tool of colonial control and as providing a benevolent and legitimising gloss to colonialism. However, Megan Vaughan has written that histories of tropical medicine can also identify the limitations as well as the power of the colonial state (and medicine). She wrote that in colonial Africa doctors were too few and their medicine too ineffective to be simply seen as either colonial oppressors or scientific liberators.

Jonathan Sadowsky’s history of colonial psychiatric discourse and psychiatrists in Nigeria included a similar and possibly more important caveat for this thesis. He wrote that ‘Colonial officials did discuss these psychiatrists seriously, but it was financial expediency, more than psychiatric theory, that drove asylum policy.’ (p.97) Tony Austin has similarly emphasised the over-riding importance of Commonwealth Government parsimony in the colonial administration of Indigenous lives in the Northern Territory before World War Two. My thesis does not aim to answer whether the Indigenous health discourse did or did not cause colonial policy
and practice. Nor does it assume that it did. My assumption is more modest — that the discourse framed or constrained the thinking of those who made and implemented the policies (including those about finances) about the administrative control of Indigenous Australians. This assumption seems quite reasonable and is supported by the theoretical discussion earlier in this chapter. I have chosen to accept this assumption based on these theoretical arguments rather than to search for new empirical evidence.

Warwick Anderson has recently and rather harshly criticised historians of tropical medicine for not going far enough in their use of and contributions to the theoretical insights of postcolonial theories, a criticism that he might make of this thesis. But it is an approach that has been consciously made and it need not be seen as anti-theory. As I have already explained, I am content to have limited myself in this thesis to what Anderson has described as merely another 'minor' and merely 'national' history by only examining the specific nature of the colonial context of medicine in Australia.

A few words

At the end of this introduction, it is useful to explain how I have used a few words in this thesis. Whilst preferences in word use change with time and politics, I have attempted to use words that are both precise and are least likely to distract present day readers from my narrative. All direct quotations are unchanged and I have avoided interrupting these quotations with 'sic' after errors or discriminatory language from the past. Similarly I have not repeatedly surrounded with scare quotation marks words that might appear disreputable or disputable. The most obvious example of this is the word 'race', which changed in meaning and use considerably over the hundred years and has continued to change since. Following Nancy Stepan's approach in her history of race in British science, I have chosen neither to define race in this introduction nor to limit my use of race to its present-day definition. The ambiguities of the usage of the term race and its changing
usage over time are an important element of my story, and would be undermined by such a narrow definition.

The interpretations of my research, and often those of the health research that I discuss, usually do not just apply to a small local context, so I have rarely been able to refer to groups of Indigenous peoples using their more specific collective names – such as Pitjantjatjara and Wiradjuri, or even their less specific names like Anangu or Koori. It was also not always clear whether researchers were including Torres Strait Islanders in their remarks and sometimes it was clear they were not – and so in these cases I have referred to Aboriginal people. When either I have meant to include Torres Strait Islanders with Aboriginal people or have felt researchers were being similarly inclusive, I have referred to Indigenous people, Indigenous Australians, Aboriginal and Torres Strait Islander people or Aboriginal and Torres Strait Islander peoples. I have referred to indigenous peoples (rather than Indigenous peoples) when I have meant to use the term generically to describe indigenous people from all countries not just Australia.

When I have meant to distinguish between different non-Indigenous Australians, I have referred to White and European Australians as seemed appropriate. Finally, as this research discusses letters to the editor, editorials, reviews, and anonymous reports as well as original research articles, I have usually referred to the more inclusive if cumbersome term - research 'publications'.

**Summary**

This thesis is a history of non-Indigenous writing about Indigenous health not of the Indigenous experience of health and disease. It is a history largely of texts rather than events or lives. But these texts can also be read as events that generate other events – such as interpretations, debates, policies, and the writing of other texts - and influence lives. In this chapter I have also described some personal and theoretical contexts for my examination of these texts as well as introducing some of the work of other scholars that further contextualises my research.
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Part one

Before 1914
Chapter 2

Tentative beginnings
This chapter describes the history of the tentative beginnings of medical journals in Australia and the tentative beginnings of Indigenous health research in these journals. The period discussed in this chapter begins in 1870 with the first medical journal publication about Indigenous health and closes in 1914 with the first issue of the Medical Journal of Australia (MJA). As well as describing the history of the early Australian medical journals, I describe some of the ideas and representations of Indigenous people and their health that emerge in these publications, including the attention to the uncommon disease now known as donovanosis.

The first Indigenous health publication in an Australian medical journal

The first publication about Aboriginal people in an Australian medical journal was written by Andrew Ross of Molong about 300 km West of Sydney and was published in New South Wales Medical Gazette in 1870. The article described the role of Aboriginal medicine in the 'startling' (p. 48) recovery of an Aboriginal man from an abdominal spear wound. The patient's condition had declined during the first two days following Ross's surgical removal of the barbed spear. The doctor noted that when he visited his patient on the third day, his surgical dressing had been removed and replaced by a poultice of gum leaves and shoots. Ross then stated that 'the slight chance of his recovery seemed now to be completely taken out of my hands'. (p. 46) To his surprise, he documented the patient's steady improvement over the next two weeks.

The patient's recovery would have appeared much more amazing to contemporary than present-day medical readers as deaths from infected surgical wounds were then common. Joseph Lister's ideas about antisepsis and aseptic surgery had only just been published in the Lancet in 1867 and had not yet been widely accepted nor implemented. The germ theory of infection was yet to be proposed and the subsequent discoveries of vaccines and then antibiotics were yet to occur. After describing the patient's illness and recovery, Ross explained to readers the purpose of his article:

In submitting so remarkable a case to the notice of the medical profession, I do so to
stimulate research in the matter, and to attract special attention to the effects of so novel and simple a remedy, than with the view of pluming myself with any degree of credit for the recovery of the patient, a circumstance more indebted, I fully believe, to the efficacy possessed by this species of the Eucalypti than any services which I may have rendered the unfortunate sufferer. (p. 47)\(^1\)

At a time when Western medicine was far less effective than today, Ross could see the advantage of investigating effective examples of Aboriginal treatments that were as yet unknown to Western medicine. Ross was not the only doctor to describe Aboriginal medicine or surgery in these early medical journals. Before discussing how these other writers described Aboriginal medicine, it is useful to know something of the history of these early Australian medical journals.

**Early medical journals in Australia**

The early medical journals in the Australian colonies were not always certain of survival and many folded after only a few years. Their status as an appropriate place to publish medical research was also not assured. These tentative beginnings were somewhat similar to the earlier history of medical journals in England. The earliest medical journals in Europe in the seventeenth and eighteenth centuries were mainly short-lived and of relatively low status. Many European medical scientists chose to publish their research in scientific rather than medical journals.\(^4\) The nineteenth century saw the beginnings of the general medical journal. These are journals for all doctors (not just a single specialty) and they usually combine original research papers with commentary and news items.\(^4\)

In England, the *Lancet* was first published in 1823 and the *British Medical Journal*, the journal of the British Medical Association, was first published in 1857 (although it began in a different guise as the *Midland Medical and Surgical Reporter* in 1828).\(^4\) The medical historian, WF Bynum has suggested that journals published by medical associations and societies were more likely to survive than those published by
individuals. The *Lancet* was a notable exception to this rule. It was independently published for many years by its first editor and owner, Thomas Wakley, and survives today even though it is still not associated with a medical association.\(^5\)

**Medical journals and associations in Sydney**

The Australian medical historian AM McIntosh has written that the first medical journal published in Australia was probably the *Australian Medical Journal*.\(^6\) It was published in Sydney from August 1846 but only continued until September the following year (a later but unrelated Victorian journal used the same name). It combined editorials, original articles, news items and summaries of articles from other journals in monthly issues of twelve pages.\(^7\) Following the journal’s demise, doctors were again forced to disseminate their findings about new medical techniques in the lay press but this was met with considerable consternation by their medical peers.\(^6\)

A more professionally acceptable forum was available following the formation of the first Australian Medical Association in 1859.\(^b\) It held more than a hundred meetings where scientific papers were given and other professional matters were discussed over the next ten years. The association eventually ceased to function (except for its Benevolent Fund) in 1869 because of falling subscriptions and attendances at meetings. The association’s final president, Frederick Milford, blamed its downfall on the absence of a medical journal to publish the papers read at meetings.\(^6\) The necessary mutual support of a medical association for its medical journal and vice versa did not occur in Sydney until the 1880s. In spite of the valiant efforts of a small group of doctors, two more medical journals (the *New South Wales Medical Gazette* and the *Australian Practitioner*) started and then closed after only a few years of publication in the 1870s.\(^6\)

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\(^a\) McIntosh has claimed that there are two references to the existence of an un-named earlier medical journal but no further evidence that such a journal existed.\(^6\)

\(^b\) The present-day Australian Medical Association was not established until 1962 when it replaced the Federal Council and state branches of the British Medical Association (BMA) in Australia. (see chapter 6)\(^8\)
A New South Wales branch of the British Medical Association (BMA) was formed in 1880. The BMA had been formed in England in 1832 with branches outside Britain first formed in the colonies of Jamaica in 1877 and South Australia in 1879. In the following year, 1881, the *Australasian Medical Gazette* was first published. It was initially published privately but the New South Wales branch of the BMA purchased and took over its publication in 1895. Whilst it was owned by the New South Wales branch, it became the official publication of all Australian branches of the BMA (except the Victorian branch) with each branch asked to appoint a local editor. The establishment of the medical school at Sydney University in 1882 further supported these developments in the foundations of the Sydney medical profession in the 1880s.

The *Australasian Medical Gazette* was the most enduring early general medical journal published in Sydney. It also published three quarters of the Indigenous health publications from Australian general medical journals before 1914 – most of these in its last four years. In 1914, the *Gazette* amalgamated with the Melbourne-based *Australian Medical Journal* to form the *MJA*.

**Medical journals and associations in Melbourne**

The early history of the medical profession and of medical journals in the Victorian colony was quite different to that in New South Wales. Unlike the more uncertain beginnings in Sydney, doctors in Melbourne had a professional association with its own journal from the 1850s. Their first professional association, the Port Phillip Medical Association, was formed in 1846, just 11 years after John Batman's 'purchase' of 60,000 acres of land (including what is now Melbourne) from the Kulin. It lasted only five years but was succeeded the following year by the Victoria Medical Association which survives today, after a few name changes and amalgamations, as the

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6 The official journals of Victorian branch of the BMA were the *Intercolonial Medical Journal of Australasia* from 1896 to 1909 and the *Australian Medical Journal* from 1910 to 1914. All BMA members in Australia (as elsewhere) also received the *British Medical Journal*. 

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Medical Society of Victoria.

As in Sydney, this early professional association in Melbourne was not entirely without its troubles - many related to internal divisions over allegations that a member was guilty of unprofessional or unethical conduct (often related to advertising). After one such attempted expulsion of a member in 1861, the viability of the society became threatened as monthly meetings were often not held, quorum was not met for the 1862 annual general meeting, and subscriptions to the society dropped to 28 (from 60 the year before). It recovered in 1863, with a new president and new members including George Halford, the first professor and dean of the new medical school at the University of Melbourne. The medical school was established in 1862, 21 years before the first medical school in Sydney. In 1907, the Medical Society of Victoria amalgamated with the Victorian Branch of the BMA, which had formed in 1879 and had initially competed with the society for members. After this amalgamation, the society continued only as the title holder of the branch's land and buildings.

The society first published the *Australian Medical Journal* in 1856. The journal was renamed the *Intercolonial Medical Journal of Australasia* in 1896 after it amalgamated with the *Intercolonial Quarterly Journal of Medicine and Surgery*. It belatedly returned to its original name - the *Australian Medical Journal* - in 1910, some years after Federation and the end of colonies in Australia. The medical historian Bryan Gandevia has written that the journal was often criticised for its elitism - being seen as dominated by the interests of a small number of doctors from Collins Street, Melbourne Hospital and the university. The journal's interests did not include Indigenous health and it did not publish any Indigenous health publications after the 1870s. Neither did any of the five short-lived rival medical journals published in Melbourne.

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4 Gandevia listed these as the *Medical Record of Australia* (1861-3), the *Melbourne Medical Record* (1875-7), the *Medical and Surgical Review (Australasian)* (1863-5, 1873-5), and the *Australian Medical Gazette* (1869-71). Ronald Winton listed another rival Melbourne medical journal, the *Melbourne Lancet*, which was only published briefly in 1858. Winton did not mention this journal in an earlier publication and other authors have also omitted to mention this journal.
Amalgamations

The Intercolonial Quarterly Journal of Medicine and Surgery had begun in 1894 after a successful resolution at the 1892 Intercolonial Medical Congress which proposed: ‘That it is desirable to found an Australian Medical Journal’. However, this journal only lasted one year before it amalgamated with the Australian Medical Journal. At the next congress, which was held in Dunedin, New Zealand, in 1896, a proposal was made to amalgamate the three journals of the various Australasian branches of the BMA: the New Zealand Medical Journal (of the New Zealand branch), the Intercolonial Medical Journal of Australasia (of the Victorian branch), and the Australasian Medical Gazette (of the New South Wales and other Australian branches). The proposal was rejected as was a proposal to form an Australian medical association in order to facilitate a single journal.

Discussions between the editors continued and it was agreed to amalgamate the New Zealand Medical Journal with the Australasian Medical Gazette commencing in January the following year, 1897. For three years the Australasian Medical Gazette was subtitled ‘The Journal of the Australasian Branches of the BMA, with which is incorporated the New Zealand Medical Journal’. Growing dissatisfaction amongst New Zealand doctors led to the dissolution of the amalgamation and the re-appearance of a discrete New Zealand Medical Journal in 1900.

There was no formal national structure to link the branches of the BMA in the different Australian colonies and then states until the Federal Committee of the BMA in Australia was formed in 1911. This national body quickly resolved to establish a national weekly medical journal. First, the Australasian Medical Gazette and the Australian Medical Journal changed from monthly to weekly publication in 1911. Then, in 1913, the committee formed the Australasian Medical Publishing Company with directors from each state branch of the BMA. The company then purchased these two remaining Australian medical journals before replacing them with the first truly national Australian medical journal – the Medical Journal of Australia. The first issue
of the Medical Journal of Australia was published on July 4, 1914.\textsuperscript{10,12}

In this chapter, as well as the publications from the early Australian medical journals, I also discuss publications from the transactions of the Intercolonial (and after 1905 the Australasian) medical congresses held in Australia and New Zealand. These transactions were later published as supplements to the MJA. Before the formation of the MJA, the congresses had a more important role in the dissemination of medical information to a national professional audience. The first of these congresses was organised by the newly formed South Australian branch of the BMA and held in Adelaide in 1887 and attracted 155 people.\textsuperscript{22} In 1902, 420 doctors attended the sixth congress in Hobart.\textsuperscript{23} I have also included the few publications from the British generalist and tropical medicine journals cited by Moodie and Pederson's Aboriginal health bibliography.\textsuperscript{24} For much of this period, Australia was still just a collection of British colonies and members of the BMA in Australia received the British Medical Journal as well as the journals produced by local branches.

Apart from the transactions of the congresses there were no significant medical journals published outside New South Wales and Victoria before 1914.\textsuperscript{6} In South Australia and Queensland, as in New South Wales and Victoria, there were local medical societies or associations before branches of the BMA were formed in 1879 and in 1894. These were either short-lived, voluntarily dissolved with the formation of a branch of the BMA, or eventually amalgamated with the local branch of the BMA.\textsuperscript{9,11} The South Australian branch notably forced a change in the BMA's rules to enable women to become members.\textsuperscript{9} A West Australian branch was formed in 1898.\textsuperscript{25} The Tasmanian branch was not formed until 1911, although, a subdivision of the Victorian branch had been established in northern Tasmania in 1897.\textsuperscript{23} Bryan Gandevia and Ann Tovell have suggested that Tasmania can at least claim the first medical library in Australia - established in 1842.\textsuperscript{26}

A medical school was established at the University of Adelaide in 1884 but the next

\textsuperscript{6} The exception was the three issues of the Journal of the Sanitary Inspectors of Western Australia published between March and May in 1908 which Howard Cumpston has mentioned in his brief history
Australian medical school was not to be opened until 1936 at the University of Queensland. However, the last decades of the nineteenth century saw the birth of tropical medicine as a specialty with the discovery of the insect vectors of filaria, yellow fever and, most importantly, malaria. These discoveries were followed by the founding of both the Liverpool School of Tropical Medicine and the London School of Tropical Medicine in 1899. Soon after, in 1910, the Australian Institute of Tropical Medicine opened in Townsville. This institute and its tropical medicine research are discussed in chapter four.

All of the early medical journals published in Sydney and Melbourne, like the later *MJA*, were general medical journals. The first specialist medical journals in Australia were not published until the 1920s and 1930s - at least a decade after the establishment of the *MJA*. This thesis will not discuss these Australian specialist medical journals in much detail. Rather, I focus on a general medical journal, the *MJA*. Specialist journals had already begun appearing in Europe in the nineteenth century and rapidly increased in number at the end of the nineteenth and beginning of the twentieth century. SP Lock has written that these specialist journals publish more original research articles and less news and commentary than general medical journals.

**Indigenous health publications in these early journals**

Against the background of the unsteady development of Australian medical journals and medical associations in the nineteenth century, I now discuss where the earliest descriptions of Indigenous health were published before the *MJA* was established in 1914. This discussion is based on an examination of those publications cited by Moodie and Pederson's Aboriginal health bibliography. Figure 2.1 shows when these cited descriptions of Indigenous health were published in Australian medical journals, the transactions of the Intercolonial and Australasian medical congresses and British medical journals.
Early Indigenous health publications

Figure 2.1
The first publication about Indigenous health in an Australian medical journal appeared in the *New South Wales Medical Gazette* in 1870.\(^1\) In the 1870s, there were two more publications in the *New South Wales Medical Gazette* and four in Victoria’s *Australian Medical Journal*.\(^{30-35}\) The bibliography cited no further publications from a Melbourne-based journal, apart from one article from the 1894-5 volume of the *Intercolonial Quarterly Journal of Medicine and Surgery*,\(^{36}\) even though there was always at least one medical journal published in Melbourne from 1856 until 1914. There were only one or two Aboriginal health publications in each of the first three decades of the *Australasian Medical Gazette* but then 16 publications in its last four years before it amalgamated with *Australian Medical Journal* to form the *MJA*.\(^{37-57}\)

The Aboriginal health bibliography cited 363 publications from 1870 to 1913 (the last year before the establishment of the *MJA*). Twenty-nine (8.0%) of these were from Australian medical journals\(^1,30-57\) \(^8\) and ten (2.8%) were papers from the transactions of the Intercolonial or Australasian medical congresses\(^{58-67}\) \(^h\). There were only seven citations from English medical journals.\(^{68-74}\)

More than half (15/28) of the Indigenous health publications in these early Australian medical journals were written anonymously. As well as news items and commentaries, these anonymous publications included extracts from newspapers, compilations of letters from named authors, and reports from papers given by named researchers at various meetings. Does this indicate that whilst the editors thought these extracts and reports about Indigenous health were of interest to their medical readers, the researchers thought there were other more appropriate places to disseminate their work? The dramatic increase in Indigenous health publications in the last few years of the *Australasian Medical Gazette* (see figure 2.1) included original publications by named authors. This possibly reflected a more certain status of Australian medicine and

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\(^1\) The bibliography cited an 1867 article in the *Australian Medical Journal* but as it was found not to mention Indigenous people and so is not discussed.

\(^8\) Including one article that did not mention Indigenous people.

\(^h\) Including one paper that did not mention Indigenous Australians.\(^62\)
medical journals in these years. There was no similar peak in citations in these years from either the transactions of the medical congresses or from the most commonly cited non-medical journals.

Anthropological journals and the transactions of meetings of general scientific associations seem to have been the most preferred alternatives to publication in an Australian medical journal. The journal with the most citations before 1914 was the *Journal of the Royal Anthropological Institute of Great Britain and Ireland* – with 27 citations. This included 7 of the 43 citations before 1870. This journal had been published in London since 1844, albeit under several different names. The other frequently cited journals included other anthropology and anatomy journals and the journals of Australian general scientific associations. I discuss the links between Indigenous health and physical anthropology in chapter five.

In their introduction, Moodie and Pederson acknowledged that the selection criteria for publications from non-medical journals to be cited in their Aboriginal health bibliography was somewhat ‘arbitrary’ (p. 1). It was likely to have been more difficult than for publications in medical journals. The bibliographers had to decide whether the non-medical publications were not only about Aboriginal people but about their health as well – and so they chose to exclude many of these publications about social anthropology. There may have been some articles from non-medical journals not cited that would have been cited if they had been published in a medical journal. However, an article in a medical journal would have had wider influence on doctors than the same article published in a non-medical journal.

I do not believe that all publications about Indigenous peoples’ health have the same influence on the representations of Indigenous people and their health in the associated discourse. I believe that the most influential texts in this discourse have been those either written primarily for a medical audience or those which proclaimed the medical expertise of their author or authors. And so I will concentrate on the *MJA* and, in this chapter, the Australian medical journals before the *MJA* was established. These texts
own non-Indigenous explanations of disease causation. Seemingly odd non-Indigenous lay and scientific explanations were more frequently discussed and tested (even if they were only quickly refuted) than Indigenous explanations. An example was the detailed discussion and animal experiments by JB Cleland to test the 'popular' belief that donovanosis was transmitted to Aboriginal women by sexual contact with their dogs.73

Unlike Ross's 1870 article, the apparent purpose of most of these descriptions of Aboriginal treatments was to provide a better understanding of Aboriginal life rather than to identify new and possible useful treatments for all people. The description of Aboriginal medicine would have been one part of contemporary and past Aboriginal culture of special interest to a medical audience. John Creed explained his choice of publication in the *Australasian Medical Gazette* in 1883 of his descriptions of subincision of the penis and other surgical procedures by Central Australian Aboriginal people - 'as it (subincision) is surgical in its nature, the Gazette appears to be the best medium for the purpose.' (p. 95)37 He seemed to imply that because he was discussing an Aboriginal surgical procedure he should be writing in a medical rather than an anthropological journal. i

Most authors emphasised the exotic and 'primitive' elements in Indigenous medical practices (such as subincision of the penis)36,37. Less than forty years after both Ross's article and Lister's introduction of aseptic surgery, Heuze Hogg's comment to the 1902 Intercolonial congress was not atypical:

With regard to the surgery of the Tasmanian aboriginals, it was, as might be expected, of a most primitive character. Bleeding was stopped by the action of clay and leaves. (p. 176)81

In spite of this seeming emphasis on practices quite different to Western medicine,

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i The report of the monthly meeting of the Victorian branch of the BMA, which followed this article, provided another more mundane reason for his choice of journal. The meeting unanimously elected Creed as editor of the *Australasian Medical Gazette*. Had he not been so elected we might wonder whether he still would have chosen to publish his article in this journal.
authors often described these practices using language of Western medicine. In the 1903 report in the *British Medical Journal* of his paper given to the Linnaean Society of NSW, John MacPherson made the training and practice of Aboriginal healers more familiar and comprehensible to his readers by using the lexicon of Western medical training: examination, admission, 'a kind of General Medical Council', "taken off the register".

These descriptions of Aboriginal medicine as a 'primitive' version of Western medicine had a role in the construction in readers' minds of representations of both 'primitive' Aboriginal culture and 'modernist' Western medicine. Marianna Torgovnick has written in her book, *Gone primitive*, of Western writers and artists' exploitative use of representations of 'primitive' Others. She has argued that their purpose was often not to understand non-Western lives but to use these 'primitive' representations as a comparative standard in their examination of Western culture. She wrote that 'The primitive has traditionally (by sleight of mind) been viewed as our beginnings, persisting into the present.' (p. 245) Similarly, these stories of 'primitive' Aboriginal medicine could have been read primarily as the stories of the origins of Western medicine with its modernist notions of beginnings and linear progress to the present. The readers' certainties about the strengths of Western medicine and its modernity would have been reinforced but at the same time representations of the 'primitivity' of Indigenous people would have been emphasised.

**Donovanosis**

In this period, the only disease to attract sustained attention was an uncommon tropical disease we now call donovanosis but was then called a variety of names but most commonly 'ulcerating granuloma of the pudenda'. It was discussed in a quarter of the publications in figure 2.1. Donovanosis is a mutilating disease of the genitals. It was first described in 1896 in the *British Guiana Medical Annual* and was usually assumed to be sexually transmitted.
The first published description of donovanosis in Aboriginal or Torres Strait Islander people was by Dr F. Goldsmith, the protector of Aborigines in the Northern Territory. On November 1, 1898, Goldsmith wrote to Patrick Manson - 'the Father of Tropical Medicine'. Manson had included a description of the disease in the first edition of his classic text of tropical medicine, *Tropical diseases: a manual of the diseases of warm climates*, which had been published in London earlier that year. Goldsmith had certainly been quick to both receive his copy of the book in Darwin and then write to the author before the end of the year about a little-known condition that was illustrated on page 440 of the manual. Manson published extracts of Goldsmith’s letter with his own introductory and concluding remarks in the January 1899 issue of the *Journal of Tropical Medicine*.

In his letter, Goldsmith described his difficulty in treating this condition in two Aboriginal women. Hospital treatment had lasted six months: four months failed systemic anti-syphilitic treatment followed by a couple of months of successful topical treatment. Manson’s book had arrived after cure and discharge. Goldsmith had probably quickly read this book looking for advice about this clinical problem which he had recently struggled to treat and understand. He then discussed two other similar cases before concluding that the disease was transmitted sexually, was different to syphilis, and that it was not confined to Indigenous people. Goldsmith described a case of the disease in a White man but he clearly implied that an Aboriginal woman was the source of that infection.

I was consulted yesterday by a white man with an ulceration on the under surface of the glans penis with corresponding ulceration on the contiguous portion of the prepuce. He informed me that he had had connection with a bibra months ago, and that the sore had remained unchanged for the last two months. (p.157)

Similarly, most of the subsequent publications about donovanosis described it as a largely Aboriginal or Torres Strait Islander disease. They implied that Indigenous people were always the source of infection with this disease even if they acknowledged

1 The Englishman, Manson, obviously did not recognise Goldsmith’s use of the word ‘lubra’ to describe an Aboriginal woman and incorrectly transcribed it as ‘bibra’
that Donovanosis could also affect non-Indigenous Australians. JB Cleland even proposed calling the infecting pathogen *Spirochaete aboriginalis* - he had mistakenly thought the responsible organism was a spirochaete as in syphilis.73

Goldsmith later included Donovanosis in a more general discussion of tropical medicine in northern Australia in a paper given to the 1899 Intercolonial Medical Congress. Donovanosis was discussed by other authors in papers at the 1902, 1908, and 1911 congresses; and in four publications in the *Australasian Medical Gazette* in 1911. At least two other congress papers and another article from the *Australasian Medical Gazette* described Donovanosis but were not cited in the bibliography. What are we to make of this emphasis on Donovanosis? Was it just chance that a mutilating disease of the genitals that most assumed was sexually transmitted was the first disease to receive such concentrated attention in Indigenous health research?

The clinicians were no doubt excited by their descriptions of the more effective treatments that they had tried for this new and difficult to treat condition. They also shared the enthusiasm of those searching for an aetiological organism. It is likely that each new publication encouraged others to publish in this search for the cause and treatment of a relatively rare but 'interesting' condition. The aetiological organism was finally identified in 1913 by two Brazilian researchers.81 After this, authors of Indigenous health publications in the *MJA* lost interest in Donovanosis although other sexually transmitted diseases continued to be mentioned. There were only twelve publications that even mentioned Donovanosis in passing in the more than 300 *MJA* publications about Indigenous health before 1970.81-92 In the last few years, there has been renewed research attention on Donovanosis among Aboriginal people in central and northern Australia associated with the availability of new shorter courses of treatment and new diagnostic advances.93

Donovanosis was probably not the most important health problem for Indigenous people at the beginning of the twentieth century. However, historian Suzanne Parry has reported that it was an important reason for attendance at a small medical clinic in
Darwin in the first years of the twentieth century. Regardless of its uncertain national contribution to Indigenous ill health, each new publication discussing donovanosis in Indigenous people would have emphasised to readers the idea that donovanosis was of special concern in Indigenous health. Readers also would have noticed that sexually transmitted diseases (STDs) were prominently discussed in overviews of Indigenous (and tropical) health. Given this prominence in the emerging Indigenous health discourse, readers may not have been surprised that James Hope, the principal medical officer of WA, chose to discuss only donovanosis in the section about diseases amongst Aboriginal people in the abstract of his annual report that was published in the *Australasian Medical Gazette* in 1911.

Hope was not the only government official to emphasise the importance of STDs over other diseases in Indigenous people. The control of the sexuality of Indigenous people, especially Indigenous women (but rarely their sexual partners if they were White men), was a central element of colonial policy and power. The representation of Indigenous people as a source of STDs in non-Indigenous Australians was a justification of the policy of police examinations of Indigenous people followed by isolation and incarceration of those found with STDs in lock hospitals on islands in Western Australia and Queensland. This policy was an example of the close links between medicine (including the representations of Aboriginal and Torres Strait Islander people in health research) and some of the cruelest excesses of colonialism.

*Photographs and sexualised power*

Not only was donovanosis the most prominent disease discussed in these publications but all of the photographs of Aboriginal or Torres Strait Islander people in these early Indigenous health publications were of people with donovanosis. There were not many other photographs of people in these early Australian journals – there were many more photos of dead people and their parts, in particular freakish pathology specimens such as foetuses with monstrous deformities.
There were six photos of donovanosis (five of Aboriginal people and one of a Sri Lankan) in the two thousand pages of the 1911 volume of the *Australasian Medical Gazette* and the transactions of the 1908 Australasian Medical Congress. Only nine other photographs of people appeared in these publications. These were of people with either rare conditions of the vulva, disfiguring skin conditions or polio. In the series of four photographs of people with polio, one of the patients was naked but the photograph had been altered so that the genitals were erased. This suggests an odd prudishness absent from other photographs. The photographed polio patient was White.

Whilst there was considerable medical interest in other STDs in Australia at this time, it is the colonial context of the representations in tropical medicine that appears to have most influenced these representations of Indigenous people with donovanosis. In the 1909 volume of the *Journal of Tropical Medicine and Hygiene*, as well as the seven photos of Aboriginal people with donovanosis there were more than 30 other photographs of people. These included a few formal photographs of the staff of the London School of Tropical Medicine. Like the photos of Aboriginal people with donovanosis, most of the remaining images were of the grotesque with an emphasis on the sexual – with photos of conditions like elephantiasis of the scrotum and bilharzia of the anus and penis.

The photos of Aboriginal people with donovanosis were part of a broader discourse: the way British (and other European and American) tropical medicine writers represented the indigenous peoples in their colonies. This discourse of tropical medicine was a specific example and component of the more general colonial discourse that Edward Said called Orientalism (see chapter one). Torgovnick emphasised the importance of sexuality and sexual voyeurism to this discourse. The photographs on the covers of both her book, *Gone primitive*, and Said’s *Orientalism* emphasise this point. Man Ray’s famous and sexually charged photograph, *Kiki*, juxtaposes his lover’s naked upper body beside an African mask on the cover of Torgovnick’s book. And Said’s book cover shows a painting of a naked performing boy wrapped in a snake in front of his audience and some Islamic frescoes.
Other writers about colonial discourse have made similar remarks about its very sexual images. The Martiniquan psychiatrist Frantz Fanon has written about the excessive focus by Whites on the myths of the sexual potency and the genitals of Blacks (especially but not only of men) in *Black skin, White masks*. He wrote that 'one is no longer aware of the Negro but only of a penis; the Negro is eclipsed. He is turned into a penis. He *is* a penis.' (p. 170)

More recently, both Ann Laura Stoler and Robert Young have described more complex evidence and theoretical arguments about the importance of sexual images in the production of colonial discourses of race (and discourses of class in Europe) in the nineteenth century. In *Colonial desire*, Young made a strong argument that nineteenth century racial theorists repeatedly described a repugnant sexuality of Blacks that they found both disgusting and fascinating. He has claimed that their racial theories were explicitly about sex:

> Nineteenth-century theories of race did not just consist of essentializing differentiations between self and other: they were also about a fascination with people having sex — interminable, adulterating, aleatory, illicit, inter-racial sex. (p. 181)

It is not implausible to see the concentration on images and diseases related to sex in both the Australian Indigenous health publications and in tropical health publications about other colonised peoples as both contributing to and influenced by this broader sexualised discourse. The Indigenous people with donovanosis were photographed either completely naked or partially disrobed with their disfigured genitals exposed. In some photographs, only the genitals were shown; in others, most of the person was shown including their face. As in other photographs in these medical journals, there was no attempt to hide or protect the patient's identity.

The photographs in a 1909 article in the *Journal of Tropical Medicine and Hygiene* by Cleland and Hickinbotham were of Aboriginal people in much more humiliating positions than the other photographs of both donovanosis and other genital diseases. One photograph was taken from behind a naked emaciated twelve year old boy bending forward with his head resting on the bed and looking back between his legs at the
camera. In another photograph, we can see a woman who we are told is called 'Polly' lying on a bed. The photo was taken from the end of the bed looking straight at her exposed mutilated perineum; because of the low camera angle we can only see her perineum and legs and a tangle of either sheets or clothes. A White man is shown standing beside her and is with some effort opening her legs in order to expose her genitals to the camera. All we see of him are his hands pushing against her thighs, his dark suit, his white cuffs and his white handkerchief in his breast pocket.

Whilst this and other photographs graphically displayed the disfigurement of the genitals caused by donovanosis, they reveal even more about power (not just sex) and colonialism. The absence of photographs of the genital lesions of the White men with donovanosis that were also discussed in these same articles is revealing. We can see the White man's power over this Aboriginal woman and her loss of dignity in the name of medical science. Herbert Basedow, the first Chief Protector of Aborigines (and Chief Medical Officer) in the Northern Territory, acknowledged this possibility but did not address its implications. He chose to embed the following anecdote and a similar story from Central Australia in a discussion of the 'strange' and seemingly contradictory Aboriginal attitudes and behaviours towards nudity and clothing.

It so happened that for scientific purposes it was necessary for me to photograph a semi-civilised lubra of the Daly river district in an attitude that under other conditions would have been considered most unbecoming. Although the woman submitted to the ordeal, she later went to my brother, who was at the time acting in the capacity of district-magistrate, and laid the following charge: "Boss, this man" – pointing to me – "been take 'em wrong picture longa me. I want you tell 'em Gee Arr." (GR referred to the Government Resident who was also the Chief of the Judiciary at Port Darwin). (p. 21)102

I have chosen not to include reproductions of these photographs. The purpose of this thesis is not to teach clinicians about the clinical signs of donovanosis. The shock of seeing these photographs might have reinforced my arguments to my readers but it would have been at the cost of replicating the abuse of power and the loss of dignity caused by their original publication. In chapter four, I return to more detailed
examination of other tropical medicine research in the early years of the twentieth century. I now move from the disease discussed most frequently in these early Indigenous health publications to introduce a similarly frequently discussed and accepted idea in these publications that has since been discredited.

**Aboriginal people as a 'doomed race'**

The authors of early Indigenous health publications frequently mentioned the idea that the objects of their research, Aboriginal people, may become extinct and that their potential contribution to science would be lost. Their concern over the possible loss to science often seemed as important to the researchers as their concern over the loss of human lives. Russell McGregor has called this the 'doomed race theory' in his history of this idea in anthropology and the Administration of Aboriginal Affairs in the Northern Territory. He has written that this idea was not necessarily related to demographic data. Gordon Briscoe has argued in his PhD thesis that the Aboriginal population was probably increasing during the early decades of the twentieth century, when this idea was still widely accepted.

This 'incorrect' idea had an enormous impact on colonial thinking and policies about Aboriginal people. McGregor has written that it led people to believe that 'the best that could be done for the Aboriginals was to protect them from overt injustice and brutality - for the short time they had left upon this earth.' But he has cautioned that the same people who confidently accepted that Aboriginal people would become extinct sometimes were also appalled and even campaigned against frontier violence. This combination also occurred in the medical writings. So, McGregor wrote:

> The doomed race theory was neither merely a sop for disturbed consciences nor an empirical demographic prediction. More than anything else, it was a manifestation of ultimate pessimism in Aboriginal abilities.' ... 'Aboriginal extinction was a corollary of their primitivity. A race so undeveloped and immature could not possibly survive in competition with the superior and progressive Europeans, any more than the dinosaur
There were two essential elements to the doomed race theory: Aboriginal people were a different race and this race was inferior to Europeans. JB Cleland, who became the most prolific author of Indigenous health research in the first half of the twentieth century (see chapter five), emphasised the distinctiveness of Aboriginal people by describing cases of common conditions just because they had occurred in an Aboriginal person. In the first Australian Indigenous health article, Ross also implied a difference between Aboriginal and non-Aboriginal bodies when he expressed doubt whether the Aboriginal medicine he described would ‘act so obediently in similar cases in the European subject’ (p. 47). In contrast, some of the other earliest publications had included Aboriginal patients alongside non-Aboriginal patients in case series without comment. Cleland wrote in the introduction of a description of autopsy specimens from two Aboriginal patients with the common disease, tuberculosis:

The Australian aboriginal is fast disappearing. In settled portions of the south of the continent pure-blooded individuals are rarely seen, and in some of the States are already almost extinct. It seems of value, therefore, to record every occurrence of disease amongst them, and still more so when the manifestations of such disease seem to differ from those usually found in Europeans. It is with this object in view that the two following cases of tuberculosis in Australian natives are recorded. (p.256)

In order to understand how Aboriginal people were thought of as a different and doomed race, it is worth knowing something about how nineteenth century ideas about race were different from those in the present. In his study of scientific racism in biology and anthropology in Britain and the USA, Elazar Barkan warned present-day readers:

At the beginning of the twentieth century, the term “race” had a far wider meaning than at present, being used to refer to any geographical, religious, class-based or color-based grouping. Although sanctioned by science, its scientific usage was multiple, ambiguous and at times self-contradictory. (p. 2)
Barkan has suggested that the lack of any exact definition of racial categories was one of the reasons for the later decline of scientific racism between the world wars. In *Colonial desire*, Robert Young has described the history of the various early uses of race. Young has explained that race and racial categories permeated all academic thought in the nineteenth century not just the science of race. He has claimed that this usage was not so much imprecise but broad and can perhaps best be summarised by his quotation of the anatomist Robert Knox, who wrote in 1850 that ‘Race is everything: literature, science, art – in a word, civilisation, depends upon it’. (p. 93)

Race was used similarly in early Australian medical publications. The author of the paper entitled ‘Race and Insanity in New South Wales, 1878-1887’ given at the 1889 Congress, after some discussion, used both race and nationality similarly to describe nationality. So, even though authors often described Indigenous people as a race and discussed possible causes of disease that were distributed according to race, they did not necessarily assert that immunity or susceptibility to disease was inherited by a racial group (as a present-day definition of race might imply). Therefore, it is not so surprising that those words that are associated with the scientific idea of an inherited racial explanation of disease (‘half-castes’, ‘full-blooded’, and ‘pure-blooded’) were only rarely used compared with the frequent use of the word race.

These words were used at the time in other contexts. In a discussion of descriptions of Indigenous people in legislation, McCorquordale has written that the colonies and then the states and the new Commonwealth Government largely defined Indigenous people by ‘blood’. Legislation first referred to ‘half-castes’ in New South Wales in 1839, South Australia in 1844, Victoria in 1864, Queensland in 1865, Western Australia in 1874, and Tasmania in 1912. ‘Blood’-based definitions and descriptions were incorporated quickly into Commonwealth legislation after Federation – with the first mention in the *Sugar Bounty Act* of 1905. These terms – which now we would acknowledge as offensive to Indigenous people - were used much more commonly in subsequent decades in the *MJA.*
These were not the only words whose absence is apparent. The authors and editors seemed discomforted by some contemporary words used for Aboriginal people. Some of the more derogatory colloquial words were not used at all. The anonymous author of a report in the *Australasian Medical Gazette* of a meeting of the Royal Geographical Society of Queensland in 1912 distanced himself or herself from the visiting speaker’s (Dr Mjoberg) language with scare quotation marks and a caveat: ‘he had already obtained some unique stationary pictures ... of “niggers”, as he called them’. The similar use of quotation marks in an earlier publication suggested some discomfort about calling some of the Aboriginal men at Yarrabah “the boys”.

In the second half of the nineteenth century and early twentieth century, physical anthropology became central to the science of race and the increasingly scientific definitions of race. It was at its most influential at the turn of the century. Whilst the early authors of Indigenous health publications and their readers may have been influenced by the ideas in physical anthropology publications in non-medical journals, there were only a few publications in the Australian medical journals before 1914 describing the physical anthropology of Indigenous people. In part, this may just have reflected the status and tentative beginnings of the Australian medical journals compared to a journal like the *Journal of the Anthropological Institute of Great Britain and Ireland*, which did publish such physical anthropology. The inexact and sometimes contradictory science of race by medical researchers and anthropologists is discussed in more detail in the next section of the thesis in chapters four and five.

Publications throughout this period promoted a view that Aboriginal people were relatively healthy prior to contact with Europeans. This included not only Aborigines before 1788 but also contemporary Aborigines in remote Australia who had had little contact with Europeans. Hogg’s comments to the Intercolonial Medical Congress in

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1 These were a letter to the editor of the *New South Wales Medical Gazette* in 1873 describing the inferiority of two Aboriginal peoples’ skulls, a short news report promoting the study of Aboriginal peoples’ skulls in the *Australian Medical Journal* in 1879, and a 1903 discussion in the *Australasian Medical Gazette* of a dentiferous cyst in the skull of an Aboriginal woman taken from a grave in Victoria. There were later reports about the physical anthropology of Aborigines in the *British Medical Journal* in 1908 and the *Lancet* in 1911.
1902 were again typical:

Before European colonisation, they seemed to have been a healthy race. The scientists of D'Entrecasteaux's expedition found but little trace of disease ... After English colonisation, however, various diseases spread amongst them, syphilis, phthisis, and pneumonia becoming frequent and fatal. (p. 176)

The causes of the destruction of the healthy idyll of pre-contact Aborigines were conflated by many early authors to the effects of 'civilisation' – which included the intended and un-intended elements of colonisation. In *Keywords*, Raymond Williams has explained how the word civilisation was used in the nineteenth century to describe both a process and its historical endpoint. But he has noted that even some early nineteenth century English writers, such as Mill and Coleridge, described both the positive and negative elements of this process and endpoint. The more neutral relativism inherent in the plural civilisations only began to appear in the late nineteenth century. The use of civilisation as an explanation of Indigenous ill health relied on the earlier meanings of the word and implied that Indigenous people were inferior and less advanced historically than Europeans. Norton Manning, the Inspector-General of the Insane in NSW, told the 1889 Intercolonial Medical Congress:

So far as can be gathered from the accounts published by explorers and early colonists, insanity was a very rare affection among the Australian aborigines whilst in their primitive and uncivilised condition. ... As time rolled on, and the aborigines were brought more in contact with Europeans, and became acquainted with the vices and the cares of civilisation, we find more frequent notices of mental disease. (my italics) (p. 857, 858)

Manning did not return to describe how 'the cares of civilisation' induced insanity but did explain more about civilisation's 'vices'. In particular, he emphasised alcohol as did John MacPherson in the *British Medical Journal* in 1903 when he wrote: ‘Under the influence of drunkenness and other evils which civilization has brought in its train,
the aboriginal tribes are fast dying out'. But far more commonly mentioned in the Australian medical publications were the diseases that civilisation had introduced. At the 1902 congress, Hogg described the effects of European contact on tuberculosis (phthisis) amongst Tasmanian Aborigines:

Lung diseases became very common amongst the Tasmanian natives, and were the chief cause of the final extinction of the race. Inflammation of the lungs was often very rapid and fatal, and phthisis was prevalent, partly because of the alteration of the habits of the race, partly, no doubt, because of the introduction of that disease by Europeans. (p. 177)

Whilst a few publications discussed the changed habits subsequent to contact (or pre-contact behaviours that made people susceptible to disease) many more discussed various theories about the origins of the infectious diseases introduced to the Aboriginal population. Whilst these representations did not necessarily emphasise Indigenous inferiority and primitivity they did imply that their bodies were inherently more vulnerable and less experienced than European bodies. An example was the debate in the Australian Medical Journal in 1877 about the spread of smallpox to the Aboriginal people of the Murray River 40 years earlier.

Whilst authors often accepted the role of English colonisation in the spread of disease, they rather more vigorously explored non-English origins of disease. Alternative theories about the source of smallpox concentrated on the French and in a later publication the Dutch East Indies. Publications about the introduction of leprosy to the Northern Territory included a Macassan origin and in Western Australia ‘a Chinaman, who was a cook on one of the stations’ was mentioned. Origins of hookworm and other tropical diseases were also discussed. Unlike tropical medicine research in other colonies (see chapter four), and with the exception of STDs, Indigenous people were rarely described as a source of infection for the non-Indigenous population.

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1 This French source of smallpox in the Aboriginal population was also discussed in Thomas Jamison’s article in the Sydney Gazette in 1804 which both Howard Cumpston and Edward Ford have
**Medicine, the colonial State, and the need for more medical research**

The claims in these early medical journals that (non-Indigenous) civilisation and its introduced diseases were the cause of Indigenous ill health were both consistent with and justified the government policy of 'protection' of Indigenous people. This was most clearly articulated in an anonymously written discussion in the *Australasian Medical Gazette* in 1913 of Baldwin Spencer's 'Preliminary Report on the Aboriginals of the Northern Territory'.55 It described Aboriginal people as having been 'degraded by a civilisation that they did not understand and from which they needed protection'.55 This proposed protection involved the appointment of protectors, the creation of large reserves, and the prohibition of supplying opium and alcohol to Aborigines.55

Two earlier articles in the *Australasian Medical Gazette* in 1911 had promoted a special role for medicine in the colonial administration of Indigenous people - a not surprising emphasis given the medical audience of the journal.43,44 These articles were written anonymously and may have been read as the opinion of the Australian branches of the BMA (who published the journal) - now a more unified political voice of an organised medical profession in Australia. The first article was a response to a pastoral letter about Aboriginal people written by the Anglican bishops.44 The article concluded with these comments:

> One of the chief problems to be solved, most grievous wrongs to be righted, is a medical one. It is earnestly to be hoped that the matter will be taken up in a true Federal spirit by the Commonwealth and that a capable and energetic medical man will deal and deal thoroughly with those aspects of their case that are our more especial province.44

In 1911, following the transfer from South Australian to Commonwealth administration of the Northern Territory, Herbert Basedow was appointed as both the Chief Medical Officer and Chief Protector of Aborigines in the Northern Territory (a combination described as the first article about a medical subject in an Australian publication.7,110
repeated by Cecil Cook from 1927 to 1939\textsuperscript{111}). With this appointment, the colonial control and medical care of Aborigines were combined in one position. This did not last as Basedow resigned only one month after taking up his position.\textsuperscript{95} Basedow’s resignation was discussed in a news item in the October 20 issue of the \textit{Australasian Medical Gazette} in 1911. The anonymous author wrote:

This is a matter for regret, especially as we learn from a statement made by Dr. Basedow that the reason for his resignation was a conflict with the Department of External Affairs. ... As head of a professional department he certainly declined to take his instructions from anybody, as he wanted to make his recommendations and suggestions personally for the approval of the political head. (p. 591, 592)\textsuperscript{43}

One of Basedow’s recommendations which was not approved by the Department was his proposal for a permanent unique identifying mark scratched onto the skin of all Aborigines in the Northern Territory. This could have become an extreme physical manifestation of the colonial control (and the complicity of medicine and no doubt eventually medical research in that control) of Aborigines’ lives. Basedow claimed that it was necessary for his ‘scheme of protection, management and medical supervision (my emphasis) of the aborigines’.\textsuperscript{112} Basedow was replaced by Baldwin Spencer. The journal’s news item finished with this sentence: ‘Mr. J. T. Beckett, chief inspector of aborigines, is to undertake part of the administrative work, and so leave Professor Spencer free to devote his time to scientific inquiry.’\textsuperscript{43} Many other authors also promoted further scientific inquiry as a strategy to address the Indigenous health problems they had discussed.

Not surprisingly, the publications discussing donovanosis promoted further research to identify its aetiology which was still unknown. However, JB McLean may have been over-stating the likely impact of such research when he wrote: ‘no doubt one of duties of our new Government Bacteriologist will include an investigation into the details {of the pathology and bacteriology of donovanosis} that are necessary for the preservation of a rapidly passing away race.’ (p. 137)\textsuperscript{48} Hope described similar research already happening in Western Australia.\textsuperscript{47} This medical scientific research was seen not just
as a part of a disease control strategy but as a valid goal in itself - the advancement of science and the accumulation of scientific knowledge. Hope concluded his description of plans for research into the aetiology of donovanosis with this remark: ‘Under any circumstances it is a well merited investigation from a scientific point of view.’ (p.52)

The acceptance by many authors that the Aboriginal race would soon be extinct made the collection of information not only more urgent but more valuable and was reason enough for further research as they believed that this information would soon be lost to science. McGregor has suggested that there was an element of self-interest in these claims of urgency as researchers may have hoped they would lead to either extra funding or importance for their work. Such claims were more frequent in the limited amount of physical anthropology published in the medical journals. An article in the *Lancet* in 1911 was typical. It praised several times the usefulness of Melbourne University’s Professor RJA Berry’s collection of data about Aboriginal people’s skulls.

**Conclusion**

This chapter has described the tentative beginnings of Australian medical journals and of Indigenous health research in these journals before 1914. Colonial power was particularly apparent in a sexualised form in the gruesome photographs in the research publications about the rare tropical sexually transmitted disease donovanosis. These representations of a repugnant yet fascinating sexuality of Indigenous people were accompanied by representations in other publications of Indigenous primitivity which could also both fascinate and imply inferiority and weakness. These notions of the inferiority and weakness of Indigenous people were most obvious in the widely-held belief that the Aboriginal race was doomed to an imminent extinction. The chapters in the next section of the thesis move from these tentative beginnings to a more detailed discussion of race in tropical medicine and physical anthropology in Australia between the wars.
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Part two

1914 - 1949
Chapter 3

A different race
This chapter introduces and provides a brief overview of publications about Indigenous health in the *Medical Journal of Australia* (*MJA*) from 1914, when it was first published, until the end of the 1940s. The authors of these publications described Indigenous people as different to themselves and their non-Indigenous readership. The writers ascribed these differences to race and attempted to define these racial differences and racial categories.

**Two stories about race: Tropical medicine and blood groups**

The research performed and published by the most prominent authors (and their institutions) of Indigenous health research is examined in more detail in the next two chapters (four and five). The prominence of these authors and institutions was identified by the quantitative analysis reported in appendix two.

The publications about Indigenous people in first decade of the *MJA* were dominated by tropical medicine – particularly publications about the Australian hookworm control programs. Chapter four will show that these publications were more concerned with hookworm in non-Indigenous than Indigenous Australians, even though the disease was more common amongst Indigenous people. Another major non-Indigenous focus of early Australian tropical medicine was research investigating whether the White race could live and work in the tropics. Whilst about the White rather than the Aboriginal race, an examination of this research provides useful insights into researchers’ thinking about race.

Most of this early tropical medicine research was performed by the Australian Institute of Tropical Medicine in Townsville or at least closely associated with this institute, as were the early hookworm campaigns in Queensland. The quantitative analysis highlighted the work of this institute, its first director Anton Breinl, and the Australian Hookworm Campaign. This tropical medicine research (into hookworm and the White race in the tropics) is discussed in chapter four. Interest in tropical medicine was rekindled in the 1940s by Edward Ford and other Army doctors in northern Australia. Tropical diseases – including hookworm, yaws, leprosy, and
malaria – were the most frequently mentioned diseases in Indigenous health *MJA* publications before 1950.

The description of the blood groups of Aboriginal people was the most common subject of Indigenous health research publications in the *MJA* in the 1920s. Physical anthropologists hoped that this research would enable the classification of different races according to their blood groups. This research was continued in the 1930s by JB Cleland, who included the results of his blood group testing in his descriptions of his expeditions to Central Australia. Cleland contributed much more Indigenous health research to the *MJA* than any other author in the 1920s and 1930s and was still prominent in the 1940s before he retired in 1948 as Professor of Pathology at the University of Adelaide.

Cleland’s publications about his expeditions are very interesting as they include more detail about the justification for this research and the processes of research than most other publications. Cleland’s published reflections enable an investigation of his representation of the relationship between the researcher and the researched.

There were four more *MJA* publications about Aboriginal people’s blood groups in the 1940s following the discovery of the Rh blood groups. Cleland’s Aboriginal health publications together with other blood group research in the *MJA* are described in more detail in chapter five. The remainder of this chapter provides some context for these more detailed stories discussed in chapters four and five.

**The early years of the MJA**

The President of the Victorian branch of the British Medical Association (BMA) hailed the publication of the first issue of the *MJA* on July 4, 1914 as the most significant event so far in the history of the medical profession in Australia. The *MJA* was published weekly, in two volumes each year. The first editor was Henry Armit. He remained editor until he died suddenly in 1930. The initial staff was only the editor, the manager and a typist. In 1925 the staff had grown to 25 and to 63 by
1939. The second editor was Mervyn Archdall. He had been assistant editor from 1922 until 1930. He was editor from 1930 until he retired in 1957.24

The First World War started only a month after the first issue appeared and put the emerging journal and company under considerable pressure. Of particular concern was the dramatic increase in the price of paper and its decreased availability. The journal survived and consolidated its financial position by gradually taking over more of the production itself. It began its own typesetting and composing in 1921 and then its own printing in 1925 in its new building on land purchased next to the University of Sydney. This expansion led to AMPCo needing to take on the printing and publication of other largely medical and scientific material.24,25

Six new specialist medical journals were established in Australia before 1950. The first of these was the Australian Journal of Experimental Biology and Medical Science published by the University of Adelaide with the Medical Sciences Club of South Australia. The MJA also published reports of some of the club's meetings. Some of the research published in this new journal is discussed in chapter five because it was performed by Cleland's Adelaide colleagues on their expeditions to Central Australia.

Archdall had an extraordinary influence over Australian medical journals during this period. As well as being the second editor of the MJA, he was the first editor of three of the new Australian specialist medical journals – the Australian and New Zealand Journal of Surgery, the Transactions of the Ophthalmological Society of Australia (BMA), and Australasia. Annals of Medicine. Archdall was also consultant editor of another journal, the Bulletin of the Post-Graduate Committee in Medicine, University of Sydney. The remaining specialist medical journal, the General Practitioner of Australia and New Zealand, had a single editor, WH Fitchett, from 1930 until his death in 1950.26,27

The MJA was the official organ of all Australian branches of the British Medical Association (BMA) (see chapter two). Following the formation in 1911 of the Federal Committee (and after 1933 the Federal Council) of the BMA in Australia,
these early years of the *MJA* were associated with a more uniform national political voice for the Australian medical profession than in previous years. The Federal Council strongly opposed the unsuccessful plans by the Labor government to nationalise medicine after World War Two. The BMA also took over the organisation of the regular Australasian medical congresses. Other specialist medical organisations were established in this period — including the Royal Australasian College of Surgeons in 1930 and the Royal Australasian College of Physicians in 1938.28

**Little attention to Indigenous people in the MJA, 1914-49**

I have identified 113 publications in the *MJA* from 1914 to 1949 about Aboriginal or Torres Strait Islander people and their health. The results of the quantitative analysis of these Indigenous health publications and their comparison with a random sample of 29 other *MJA* publications are described in more detail in appendix two.

There was still only scant attention paid to Indigenous health in these early years of the *MJA* compared to later decades. (figure 3.1) There were only seven years when the *MJA* published five or more Indigenous health publications: 1914, 1924, 1930, 1931, 1932, 1935, and 1938. a More Indigenous health publications and more pages of Indigenous health were published in the *MJA* during 1931 than during any other year before 1950. Figure 3.2 shows the initial decrease in Indigenous health publications in the first few years after the *MJA* was founded followed by a faltering rise in the number of publications until 1931. This peak was followed by a sudden and then more gradual fall in the number of publications per year until the end of the 1940s. A similar pattern is seen in figure 3.3 when the number of pages of text rather than the number of publications is analysed. This analysis using pages of text rather than number of publications, avoids an inappropriate over-emphasis on the large number of very short publications.

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1914 (with 8 publications and 7.3 pages of text), 1924 (7 publications, 23.5 pages), 1930 (6 publications, 22.3 pages), 1931 (10 publications, 36.0 pages), 1932 (6 publications, 17.9 pages), 1935 (6 publications, 24.5 pages), and 1938 (5 publications, 32.4 pages).
Figure 3.1

Number of *MJA* publications about Indigenous health, 1914 - 1969

Figure 3.2

Number of *MJA* publications about Indigenous health, 1914 - 1949
Pages of *MJA* publications about Indigenous health, 1914 - 1949

Percentage of total pages (excl supplements) of *MJA* publications about Indigenous health, 1914 - 1949
After excluding the publications from supplements it is possible to calculate the proportion of *MJA* pages devoted to Indigenous health. The only publications to appear in supplements were four publications in 1924— all from supplements which published the transactions of the Australasian Medical Congress of the BMA that had been held in Melbourne in 1923. Overall, Indigenous health publications accounted for 0.6% (316.0/54364) of the pages of the *MJA* (excluding supplements). Figure 3.4 shows that in only five years did Aboriginal health publications occupy much more than 1% of *MJA* pages: 1914, 1930, 1931, 1935, and 1938. Similar trends over the period are shown in figure 3.4 as in the earlier graphs.

Might this low level of interest just reflect the fact that Indigenous people were only a small proportion of the Australian population? By comparing these graphs with graphs of the estimated Indigenous population as a proportion of the total Australian population, it is possible to assess whether Indigenous people and their health was under- or over-represented in the *MJA* when compared with other Australians. However, this comparison does not include any acknowledgement of the poorer health status of Indigenous than non-Indigenous Australians which might have led to greater attention to Indigenous health by researchers. Figure 3.5 shows Len Smith's estimations of the total Aboriginal and Torres Strait Islander population from 1901 to 1971 (from his table 8.2.5).29 He has showed that the population fell to a nadir in the 1930s (which he has estimated as 23.5% of the pre-contact Aboriginal population in 1788) followed by a gradual recovery.3 Smith's estimates have been used to calculate the Indigenous population as a percentage of the Australian Bureau of Statistics' estimates of the total Australian population (p. 124-5)31 in figure 3.6. Indigenous people and their health were clearly under-represented in these early years of the *MJA*: being more than 1% of the population but only accounting for

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29 Before the *Torres Strait Islander Act* in 1939, Torres Strait Islanders were simply counted as Aboriginal. After this time, there were different strategies for counting Torres Strait Islanders. Smith included Torres Strait Islanders in his estimates of the total Indigenous population.29

31 I have accepted Smith’s estimates but Gordon Briscoe has asserted recently that the decline in the Aboriginal population in both WA and Queensland had stopped before 1900. He has claimed that some of Smith’s estimates were erroneous because both colonies had over-estimated the size of their Aboriginal populations not in contact with Europeans before 1900.30
Estimated minimum total Indigenous Australian population, 1901-1971

Figure 3.5

Estimated minimum total Indigenous population as a percentage of total Australian population, 1901-1971

Figure 3.6
0.6% of the pages of the *MJA*. However the under-representation had been much more marked in the decades before the *MJA*.

These population figures suggest a possible reason for the trends in the annual numbers of Indigenous health publications in these first 36 years of the *MJA*. As the estimates of the Indigenous population fell and it approached its lowest documented size, the volume of publications increased. As the population began to recover, publications fell until they stabilised at a lower level. The documented fall in the Indigenous population and the frequently mentioned notion that the Aboriginal race may soon become extinct may have led to an increasing interest in Indigenous health. This interest rapidly but only partly abated when it contemporary population estimates began to make it apparent to researchers that the population was no longer falling but increasing. This decline in interest in Indigenous health was exaggerated by attention on the pressing medical matters associated with Australia’s involvement in the second World War.

**From a different and doomed race to a New Deal for Aborigines**

The Indigenous health publications in the first decade of the *MJA* concentrated on Indigenous people living in northern Australia, particularly those living in north Queensland. In the 1930s, attention turned to Central Australian Aboriginal people and in the 1940s most attention was divided between Central Australia, the Top End (or northern part) of the Northern Territory, and north Queensland. The next most commonly written about Indigenous people in the *MJA* were from New South Wales, South Australia and then Western Australia. Aboriginal people from Victoria and Tasmania were only rarely discussed in the *MJA*.

The high proportion of publications in the *MJA* about Indigenous people from Queensland and Northern Territory was similar to Len Smith’s estimated proportions of the total number of Indigenous Australians who lived in Queensland (27 - 29%) and Northern Territory (22-26%) during this period (his table 8.2.3)\(^\text{29}\). In contrast, South Australian Aboriginal people were over-represented and West
Australians under-represented in the pages of the *MJA* for the size of their population. The large number of South Australian publications was probably due to the keen interest in Aboriginal people by Cleland and his colleagues at Adelaide University (who wrote about Aboriginal people in South Australia and the Northern Territory). The absence of a University medical school in Western Australia until 1956 and the distance from the established universities and their medical schools in Adelaide, Melbourne and Sydney may provide some of the explanation of the lack of Aboriginal health research from that state.

The nouns and adjectives used most frequently to describe Indigenous people changed from 'aboriginals' and 'aboriginal' in the early years (and in the Australian medical journals before the *MJA*) to 'native', 'natives' and 'aborigines' in the 1940s. These words did not start with capital letters except in titles (in which most words began with capitals). Except for the first few years of the *MJA*, race-based words were often used to differentiate between Aboriginal people of full and mixed descent in *MJA* publications. Labels like 'full' and 'pure blooded' were compared to 'half caste' and less commonly 'mixed blood' and 'hybrid'. Similarly, Aboriginal people from Tasmania were described as a quite different race from other Aboriginal people. In chapter five, I discuss the way researchers emphasised the racial 'purity' of their Aboriginal subjects.

In spite of the contradictory evidence provided by the blood group research which is described in chapter five, notions of biological race and racial hierarchies persisted in the *MJA* until scientists were confronted by the evils of the Holocaust. As well as the blood group research, the *MJA* published other physical anthropology which used measurements of Aboriginal people's skulls, brains and bones in order to describe their primitivity in a racial hierarchy. Unlike the blood group research none of these were original research articles. There were three copies of papers given at scientific meetings and the rest were either short anonymous reports of papers or anonymous book reviews.

Authors of this type of physical anthropology (and Aboriginal blood group research after the 1940s) chose to publish their research in other scientific journals. The *MJA*
just reported to its medical audience on a few of the papers they had given at scientific meetings. Doctors may have been more interested in the related *MJA* publications which described the examination of Aboriginal bones to decide which of the treponemal diseases, yaws or syphilis, had affected Aboriginal people in the past and the frequency of healed fractures in these bones.

Stephen Gould has described the gradual replacement in the first half of the twentieth century of craniometry (skull measuring) with IQ testing as a means to create a racial hierarchy based on ‘intelligence’. Stanley Porteus from the University of Hawaii was the first to conduct such psychological testing of Aboriginal people. His books *The psychology of a primitive people* and *Primitive intelligence and environment* were reviewed in the *MJA* in 1932 and 1938. HK Fry accompanied Cleland on eight of his expeditions to Central Australia from 1929 to 1937 and also performed such testing. Fry and Porteus (and Porteus’s anonymous reviewer) expressed some doubts about their tests in measuring intelligence. They both supplemented their test results with ethnographic observations. This uncertainty about these tests was quite different to the more determined promotion of IQ testing by researchers in Europe and America described by Gould.

Porteus justified the publication of his results in spite of their scientific methodological weaknesses because so little of this work had been done with Aboriginal people. He called on readers and reviewers not to be too critical of this research as it was important to attract researchers to this field: ‘there is so much to be done and the opportunity is passing swiftly.’ (p. 434) In spite of their methodological uncertainties, both Porteus and Fry’s *MJA* publications gave the clear impression to readers that Aboriginal people’s intelligence, cognition and even emotions were different, more primitive and slightly inferior to European or civilised Australians.

These notions of primitivity were even more obvious in two earlier papers from the 1923 Australasian Medical Congress describing case series of Aboriginal inmates of mental hospitals that were later published in the *MJA*. Differences with non-
Aboriginal psychiatric patients were described as due to their limited thought processes caused by their less developed brains with their 'primitive cerebral structure'. (p. 459)\textsuperscript{58} For example, the second paper concluded:

The aboriginal has not reached an evolutionary stage in which the neuroses and certain of the psycho-neuroses, Hysteria and the phobias can exist. (p. 464)\textsuperscript{58}

Like the authors of other Indigenous health research at this time, this author, J Bostock, did not tell readers that this case series was presented to improve their understanding and therefore the treatment of the mental health problems of Aboriginal people. Bostock used \textit{a priori} assumptions about Aboriginal people's mental primitivity (and hence assumed similarity to his or her own primitive ancestors) to justify this research as a means of improving the understanding of the genesis of non-Aboriginal mental health problems. Like earlier researchers and other researchers between the wars, Bostock further emphasised the importance of this research by referring to Aboriginal people as a 'doomed race'.

One of the few direct methods of attacking the evolutionary problems connected with psychiatry is by a study of the Australian black and as the years go by the opportunity lessens. Contact with civilization, phthisis and other diseases, mixed breeding and general racial decay are the order of the day. In a few years this line of research will be closed for ever. Whatever may be one's sentimental views on the passing of the primitive peoples, from the scientific or even the utilitarian aspect it will be more than unfortunate if our records are not completed before they vanish. (p. 459)\textsuperscript{58}

Aboriginal people's ill health was described as due to their putative racial inferiority and their contact with Europeans and their diseases as well as poor sanitation. Tropical medicine researchers were the most likely to discuss the impact of imported diseases. They also debated notions of 'racial immunity' to some diseases and the danger of the transmission of diseases from Aboriginal people to Whites in northern Australia. As in the medical journals before the \textit{MJA}, the social as well as the microbiological dangers for Aboriginal people from contact with civilisation were mentioned by authors who still often portrayed a healthy pre-contact idyll. These
repeated medical representations of the negative impacts of the colonial encounter supported the government policy and practice of the ‘segregation’ and ‘protection’ of Indigenous people.

These policies and practices were increasingly attacked between the wars by emergent Aboriginal organisations, like the Aborigines Progressive Association in NSW and the Australian Aborigines League in Victoria, and also by White humanitarians. The Aborigines Progressive Association questioned the consciences of White Australians over their national celebrations of 150 years of progress and achievements in 1938. The Aboriginal activists, Jack Patten and William Ferguson, called for a Day of Mourning, and clearly implicated scientists (and their representations of Aboriginal people) in the oppression of Aboriginal people when they wrote:

>We do not wish to be regarded with sentimental sympathy, or to be “preserved,” like the koala bears, as exhibits; but we do ask for your real sympathy and understanding. We do not wish to be “studied” as scientific or anthropological curiosities. ... We ask you to be proud of the Australian Aborigines, and not be misled any longer by the superstition that we are a naturally backward and low race. This is a scientific lie, which has helped to push our people down and down in to the mire. (p. 84, 86)

The opinion of bureaucrats, politicians and academics about these policies also began to change in the years leading up to the 1937 Native Welfare Conference. It was increasingly apparent that Aboriginal people were not ‘dying out’. Government officials called for a policy that acknowledged the increasing numbers of Aboriginal people (particularly those of mixed descent) and the impracticability of keeping the Indigenous and non-Indigenous populations separate. In 1939, the Minister for the Interior, John McEwen announced his ‘New Deal for Aborigines’ with its distant aim of citizenship for Aboriginal people.

In the late 1940s, the *MJA* reported papers given at scientific meetings by the former Chief Protectors of Aborigines in Western Australia (AO Neville) and the Northern
Territory (Cecil Cook). Both men promoted the new policy of ‘assimilation’ to this medical audience. The MJA also reported HK Fry’s response to Cook’s paper. Fry said that the problem with ‘protection’ was that it gave ‘a white man the authority over the black’ and decried its ‘mere good intentions’ (p. 457). Instead he promoted greater power to Aboriginal people to form their own local councils but this call for something very similar to present-day notions of ‘self-determination’ was very atypical in the 1940s.

The implementation of the ‘assimilation’ policy was delayed by the war but the great social changes wrought by the war also added further impetus to the need for these changes. The revelations of Nazi atrocities led to a series of statements by UNESCO that reflected and hastened the acceptance of biological egalitarianism and the demise of the scientific acceptance of biological race. The impact of these changes on Indigenous health publications in the MJA becomes apparent in the next part of the thesis (chapters 6, 7, and 8) which deals with the 1950s and 60s – the ‘assimilation’ era.

Apart from the hookworm campaigns, there were very few medical interventions suggested in the Indigenous health publications in the MJA before 1950. Lindsey Harrison has argued that before the Second World War, the acceptance of pessimistic ideas about Aboriginal people’s racial inferiority and imminent extinction led to research that aimed to merely document rather than improve Indigenous health. However she has accepted that there were some exceptions. There was a series of MJA publications following a 1929 report by the Victorian branch of the BMA expressing concerns about the absence of any medical or nursing care for Aboriginal people in Central Australia. Medical services for Aboriginal people were not discussed again in the MJA until the mid 1940s. At the end of the war, these two MJA articles were by doctors who had worked with the Army, which had provided medical care for Aboriginal people in the Top End during the war.
Photographs of dead or 'extinct' Aboriginal people

Only 12 of the 113 Indigenous health publications in the MJA before 1950 included photographs; there were also ten publications with maps. A similar proportion (2/28) of the control MJA publications included photos. Unlike the images of donovanosis discussed in chapter two, there were only two tropical medicine articles that included photographs. These displayed exotic diseases (a disfiguring skin disease of unknown aetiology and an unusual endocrine abnormality) in women without any obvious clothing but were not as overtly sexual as the earlier photographs.

Few of the remaining images were of living Indigenous people – most were of pathological specimens taken from Aboriginal corpses. These included photographs of skulls and other bones and, in 1948, a post-mortem photograph of a stillborn Aboriginal infant with two heads. It is not certain from the article whether the mother of this child consented to this post-mortem or whether the dead infant’s corpse was ‘stolen’. The author, Lawrence Alderman, wrote that the mother had been examined by a nurse four hours after delivery but that she and her birth attendants ‘were by this time almost beside themselves with anxiety to be off and leave the haunted camp, and showed disinclination to discuss the confinement at all.’ (p. 532) It is easy to imagine that they would have also been ‘disinclined’ to have the infant photographed and then dissected in an autopsy.

Five years earlier, an MJA article about congenital deformities in the art of Oceania included a photo of an Aboriginal rock drawing of a ‘dicephalus monster’ (p. 419). The author, Isadore Brodsky, used this drawing and others to make inferences about indigenous understanding of embryology and included the aside that such congenital ‘monsters are not restricted to species, race or zone.’ (p. 419) In contrast, Alderman used his case report to emphasise the difference and primitivity of Aboriginal

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\(^d\) Whilst this small random sample indicates the proportion of MJA publications with photographs, it identifies too few photographs to make any reasonable comparative comments about the content of photographs in publications not about Indigenous people. So I will restrict my remarks to those photographs in the Indigenous health publications.
obstetric practices. He made dismissive remarks about 'Aranda midwifery' (p. 532) in spite of the excellent maternal outcome in this case (there was no evidence of vaginal tears or damage following this obviously difficult vaginal delivery) – 'when delay is experienced the midwives usually pummel the abdomen of the unfortunate mother unmercifully, and if any part has presented they pull on whatever portion gives a good grip.' (p. 532) The implicit comparison was with civilised Western medicine and obstetrics.

Alderman concluded his article with a paragraph about infanticide following the birth of twins. Edward Ford and AO Neville also mentioned infanticide in the MJA in the 1940s. Similarly unsubstantiated reports of infanticide (especially of infants of mixed descent) are used in present day political debates to justify past government practices (like taking children of mixed descent from their Aboriginal mothers) as a civilised response to seemingly brutal primitivity. My purpose here is not to decide whether infanticide did or did not occur but to mention the way writers used infanticide to describe Aboriginal people as different and as savage primitives.

Photographs of Tasmanian Aboriginal people and their skulls were included in two MJA articles which were published in 1934 and 1935. The central purpose of these articles was to describe a race that these authors and most Australians falsely believed was extinct. Whilst Indigenous health research concentrated on northern Australia, these few articles from Tasmania played an important role in the Aboriginal health discourse. In his book about the idea that Aboriginal people were a 'doomed race', Russell McGregor has written:

> The extinction of the Tasmanians lent some credibility to the expectation that the Aboriginal Australians would soon suffer the same fate; and the former was freely used to buttress the latter. (p. 51)

But like many of his mainland colleagues who acknowledged the possibility of the imminent demise of this race, WELH Crowther concluded the first of these articles with a plea that such an 'extinction' should not be allowed to be repeated amongst

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* For a considered discussion of infanticide see Cowlishaw (1978).
Aboriginal people on the Australian mainland, and suggested strategies to prevent this outcome. Crowther included photographs of the man and woman described as the ‘last’ of the Tasmanians and discussed the theft of the man’s head and then body after his death. He wrote that his grandfather, a Hobart Surgeon, was accused of the theft of this man’s head but that ‘(if he were responsible) his scientific zeal was to blame.’ (p. 157)33

In the second of these articles, the honorary craniologist from the National Museum in Melbourne, J Wunderly, described the skulls of Tasmanian Aboriginal people and included nine photographs of these skulls.34 One of the photographs was of the skull of the so-called last Tasmanian, Truganini (or Tru Ger Nan Ner82). The discussion of her skull and its photograph was appended with a footnote that remarked that ‘before her death, she dreaded the possibility that her body would ultimately be cut up for scientific purposes.’ (p. 459)34 Her corpse was not only ‘cut up’ but it was displayed in the Tasmanian Museum and Art Gallery from 1904 to 1947.79 Aboriginal activists were eventually able to ensure that her wishes were met and her remains were cremated and scattered in 1976.79

Both Crowther and Wunderly seemed to partly acknowledge the wrongfulness of these thefts and mutilations but did not criticise the use of ‘science’ in the justification of these events. This failure means that Tasmanian and other Indigenous peoples might understandably group these authors (and their medical audience) with the grave robbers and as partly responsible for these events which have such extraordinary metaphorical power. These events and their portrayal in the MJA highlight the close involvement of medical science in the abuses of colonial power over Aboriginal bodies and lives.

**Conclusion**

There was little attention to Indigenous health research in the MJA before 1950. The Indigenous health research that was published was dominated by a small number of researchers – most notably by JB Cleland and his colleagues from Adelaide. The
stated purpose of this research was not to improve the health of Aboriginal and Torres Strait Islander people but to add to scientific knowledge and to the understanding of non-Aboriginal health problems. Many authors still believed that the Aboriginal race would become extinct. Indigenous people were overwhelmingly represented as a very different race. Authors described this race as both more primitive and slightly inferior to European Australians. The next two chapters describe research about this different race in the tropics and the use of blood groups to describe this race.
References


Chapter 4

Race, politics and tropical medicine
There were only thirty publications about Indigenous people in the *MJA* in its first decade (1914-24). These publications were dominated by tropical medical research in north Australia, especially in north Queensland. This work was mainly performed or at least heavily influenced by the newly established Australian Institute of Tropical Medicine (AITM) in Townsville and its first director, Anton Breinl. Whilst Aboriginal and Torres Strait Islander people were mentioned in these publications, they were not the dominant focus of this research.

The institute's research into whether the White race could live and work in tropical Australia dominated Australian tropical medical research at this time. I discuss this research in this chapter even though it was not primarily about Indigenous people and often did not even explicitly mention Indigenous people. It tells us a great deal about early Australian medical researchers' attitudes to race. These ideas and this research reflected and influenced the histories of colonialism, Australian nationalism, Australian health services and in particular health services to Aborigines and Torres Strait Islanders.

Following this, I discuss the Australian Hookworm Campaign which began in 1918 (as the Queensland Hookworm Campaign) and continued until 1924. The Australian Hookworm campaign was part of a series of international campaigns against hookworm promoted by the Rockefeller Foundation. The articles about the hookworm campaign in the *MJA* allow us to see how Indigenous Australians were represented in this large and politically significant public health intervention.

**Anton Breinl and tropical medicine on the colonial frontier**

Before discussing the early Australian tropical medicine research and research publications, it is useful to provide some background information about the establishment of the AITM and Breinl's arrival in Townsville. The AITM was the

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* These 30 publications from July 1914 to June 1924 were identified from those selected for the quantitative analysis in chapter three.
first medical research institute established in Australia. The next was the Walter and Eliza Hall Institute in Melbourne in 1915. Breinl and his laboratory assistant, JW Fielding, started the AITM in 1910 in a small building that had been the wardsman’s quarters in the grounds of Townsville Base Hospital. They were later joined by other staff and a new building was built for the AITM and opened in 1913. Breinl remained director of the institute from 1910 until 1920. During this time, he published three articles in the MJA that discuss Indigenous Australians. Over the same period the AITM published 57 scientific papers of which Breinl was the author or co-author of 22. Much of the work of the institute was involved in the investigation of parasites in animals and of other veterinary diseases. A newly identified genus of filaria that infected marsupials was even named Breinlia.

Breinl was born in Vienna to parents of German origin. He was naturalised as an Australian citizen in early 1914 and during the war voluntarily took on the additional predominantly clinical duties of the superintendent of the adjacent Townsville General Hospital. He was well liked by staff and patients but still came in for criticism as an ‘ex-enemy alien’ (p. 784). After the war a few key staff members left the institute to work in southern universities and finally in 1920 Breinl also resigned. After his resignation from the institute, Breinl returned to clinical medicine in a general practice in Townsville where he worked until just before his death in 1944. He was involved in no further scientific research after he left the institute.

Breinl had been recruited for the AITM from the Liverpool School of Tropical Medicine where he had studied and worked since studying medicine at the University of Prague. Whilst at the Liverpool school he was involved in research into a variety of tropical infections and was promoted to assistant lecturer and director of the Runcorn Research Laboratory. His most significant contribution had been to the treatment of African trypanosomiasis or ‘sleeping sickness’. He and Wolfertsan Thomas were the first to test and use successfully an organic arsenical, Atoxy1, for this usually fatal disease. He was even able to treat successfully his own trypanosomiasis, which he caught from a bite from a laboratory.
rat. Robert Douglas has written that after a visit to Breinl and Thomas, Paul Ehrlich also began testing organic arsenicals – but for syphilis. This led to Ehrlich’s development of the antibiotic salvarsan which revolutionised the treatment of syphilis.\(^{33}\) Salvarsan’s impact on the treatment of yaws (another treponemal infection like syphilis) was possibly even more significant for Indigenous health.

Breinl’s work at the AITM was not as dramatically successful as his earlier research. In 1916, he and his colleague Henry Priestly published a case report in the *MJA* describing the successful treatment of an Aboriginal woman’s donovanosis.\(^{12}\) Their case report only verified the effectiveness in this Aboriginal patient of tartar emetic (antimony) that Aragao and Vianna had already demonstrated in a paper in 1913. As such it demonstrated neither the research effort nor the impact of his earlier work on Atoxyl in trypanosomiasis. But it did show that he read the international tropical medicine literature and applied its ideas to his work with Indigenous people in north Queensland. It also reflected the Australian interest in donovanosis in the preceding decade which I discussed in chapter two. This simple case report was not a sample of a wider research program investigating chemotherapeutic and other treatments for the diseases afflicting Indigenous (and non-Indigenous) residents of the Queensland tropics.

Whilst at the AITM, Breinl also participated in several disease surveys in Queensland, Northern Territory, and New Guinea (see figure 4.1).\(^{33,38}\) This type of research forms an important part of the tropical medicine colonial story, with the researcher playing a similar role to the great White explorer in uncharted savage lands. The researcher named diseases and organisms in the name of science in a manner analogous to the naming of rivers and hills in the name of Empire. I discuss this expeditionary type of research further in chapter five when I talk about JB Cleland’s expeditions to Central Australia in the 1930s.

These surveys were not without their dangers to the researchers. We can acknowledge their dedication and even bravery without descending into unreflective triumphalism. In 1905, Breinl was both shipwrecked and nearly died from yellow fever (which he had gone to investigate) in an expedition up the Amazon for the
Figure 4.1. Anton Breinl

Liverpool School of Tropical Medicine. His expeditions for the AITM were also arduous. His 1913 expedition in New Guinea involved walking 500 miles and travelling long distances by canoe.33,37

These surveys provided much of the information for his overviews of tropical medicine.39 In one of these overviews at the 1911 Australasian Medical Congress, Breinl made it clear that new ‘discoveries’ could still be made by disease surveys amongst Aboriginal people in northern Australia. He said: ‘There occur, among the aboriginals in the Northern Territory, diseases which have not yet been classified.’ (p. 530)39 Later, in January 1915, Breinl wrote to the Administrator of the Northern Territory, Dr JA Gilruth, proposing another disease survey in the Northern Territory. He emphasised the importance of identifying ‘interesting’ diseases for ‘science’ as reasons for this research – rather than the treatment of the people with these diseases. It is also possible to see parallels between his photographs and collection of Indigenous peoples’ diseases and colonial explorers’ collections of ‘native’ or ‘primitive’ artefacts (a collection that would become more valuable – to science – should the Aboriginal race die out as expected). b He wrote:

At the end of December I had an opportunity of meeting Dr M. J. Holmes c who passed through Townsville on his return to Darwin. During our conversation I learned that there are quite a few interesting diseases amongst the natives in the Northern Territory in parts which I have not had an opportunity of visiting and Dr Holmes remarked that a visit to certain parts of the Territory within easy reach would be very interesting indeed. I showed him most of my photographs concerning diseases in natives of New Guinea and Dr Holmes informed me that he had come across several of the diseases described, amongst Northern Territory Natives. Personally I think that a visit to the Northern Territory would be highly interesting. ... Dr Holmes thought it would be a distinctly interesting and valuable journey from a scientific point of view.40

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b Breinl also collected artefacts and some of his collection was on display in January 2001 at the Anton Breinl Centre of James Cook University’s School of Public Health and Tropical Medicine in Townsville.

c Holmes was the Principal Health Officer in the Northern Territory.
The colonial power of these expeditions need not just be seen through Foucauldian spectacles – as colonial researchers controlling Indigenous bodies by naming, knowing, and collecting their diseases. These expeditions and the researchers had a much more explicit colonial function. The researchers were quite frank about their role in expanding the colonial frontier and making it safer for European settlers. Breinl told the Australasian Medical Congress in 1911:

> From our experience in other tropical countries it would seem imperative to study all the diseases endemic amongst the aboriginals, as on closer settlement these diseases may spread amongst the white settlers, and may cause great havoc and mortality, which could be prevented if proper precautions were taken in time. (p.530)\(^3\)\(^9\)

And more eloquently in the same paper:

> In future, the pioneer should not be the settler, but the scientifically-trained man, who studies local conditions in a new territory, and investigates the existing diseases. (p.526)\(^3\)\(^9\)

Just as these researchers explicitly wrote about Indigenous people’s health to understand and protect European Australians’ health, we can read their articles about the health of White Australians to understand their attitudes to Indigenous people. Moodie and Pederson’s Aboriginal health bibliography\(^4\)^ cited a 1914 MJA article by Breinl, Priestly and Fielding of the AITM describing the epidemiology and the laboratory and clinical features of ‘endemic glandular fever’.\(^5\)^\(^6\) The authors did not talk about its course or treatment in any identified Indigenous patients. They only mentioned Aboriginal people twice. Indigenous health was only discussed in this article because it answered questions about the health of White settlers. They introduced the disease with a second hand report of ‘early settlers’ (p. 391) saying that it occurred amongst the Aborigines of the area prior to White contact. Later they wrote: ‘A racial immunity does not exist; Australian aborigines, natives of the

\(^{4}\) Ross Patrick has suggested this disease and other ‘coastal fevers’ were not a single disease but either scrub typhus, murine typhus or leptospirosis.\(^{31}\)
Pacific islands, and Asiatics are known to have contracted the disease.’ (p. 394)²⁸
The notion of ‘racial immunity’ to disease was a part of the long-running discourse and many publications about whether the White race could live in the tropics, which I now discuss.

**The White race in the tropics**

Breinl had introduced his plans for Australian tropical medicine and explained the three ‘objects’ of the new institute to the 1911 Australasian Medical Congress.³⁹ The first two related to the prevention of the introduction of diseases and the study of existing diseases in tropical Australia. It was the third object of the institute which became most significant and which I discuss now – ‘a thorough and impartial inquiry into the physiology of the white race living and working under different conditions in tropical Australia.’ (p. 526)³⁹

In the nineteenth century, many doctors accepted that the tropical climate was unhealthy for the White race. The tropical heat and humidity was thought to cause a myriad of complaints. The discourse was often identified as being about the ‘White man in the tropics’ and in particular the working White man (with its explicit economic concerns). The consequences were also thought to be particularly dire for White women. A persistent Australian advocate of the deleterious effect of the tropical climate, Dr Walter Nisbet of Townsville, talked at the 1911 Australasian Medical Congress of White women’s rapid deterioration in the tropics. He mentioned their ‘lack of mammary development’ leading them to be unable to breast-feed, the increased numbers of miscarriages, and the rarity of ‘handsome women’ in the tropics.³⁹

The vector of this decline may have been the tropical climate but the cause was race. The world was seen as divided not only into different climates but different ‘color-zones’ (p. 66)⁴². Each human race or racial type had evolved in and adapted to its own climate but could not easily move and adjust to a markedly different climate.
The Professor of Physiology at London Hospital Medical School, Leonard Hill, wrote in the *British Medical Journal* in 1914 that: 'Evolution has settled the dark-skinned man in the tropics, and the white in the temperate zones of the earth.' He asserted that White settlement of north Australia was 'counter to the laws of Nature.' Nisbet had talked of the problem of 'race degeneracy' (p.534) in Queensland caused by the failure of the White race to adapt to life in the tropics. His comments about the effects of the tropics on White women had concentrated on their procreative role and their ability to produce and nurture future White generations.

The message was that not only did White individuals fail to adapt to the climate and become sick but the resultant race could become weakened in only a few generations. This coincided with turn-of-the-century concerns about the decline of (White) Australian birth rates. This decline was contrasted with perceived higher birth rates of non-White races (but not Aboriginal people who were thought to be dying out) and of the physically and mentally unfit (which often just meant the poor) by advocates of the short-lived Australian eugenics movement.

The idea of race degeneracy in the tropics seems to suggest a degree of racial plasticity and change that many present-day readers might find odd. This can in part be explained by the broader use of race at the time, which included social and political definitions as well as biological definitions of race. It may also imply an acceptance of Lamarkian (rather than Darwinian) ideas that acquired characteristics could be inherited. This idea that the White race could change in only a few generations in the tropics was not unusual and was also held by one of Nisbet's fiercest opponents, Raphael Cilento. Cilento saw the tropical climate as no problem for the White settlement of the tropics. He was able to describe a typical north Queensland 'racial type' that was emerging after only a couple of generations as being tall, slight, pale with slow gait and darkening hair colour (see figure 4.2). This plasticity of racial characteristics could also be applied to contemporary ideas about Indigenous people. With this less fixed definition of race, readers would have been less fatalistic than present-day readers about the likelihood of changing so-called racial characteristics.
Figure 4.2. The north Queenslander 'racial type'

This was also the time of the birth of Australia's now notorious White Australia Policy. In the first parliament of the new Federation in 1901, politicians chose to show their nationalism by opposing non-European immigration and passing the *Immigration Restriction Act*.\textsuperscript{47} In order for all of Australia to become White, they also specifically targeted the tropics and Queensland. The sugar industry in Queensland had long relied on cheap Melanesian labour. The medical arguments about their racial adaptation to work in the tropical climate had been used to justify their economic exploitation. The early union movement and many of the new federal parliamentarians from Queensland, both Labor and non-Labor, were opposed to these Melanesian labourers.\textsuperscript{47} They ensured the *Pacific Islanders Labourers Act* was passed. No more labourers would be allowed to arrive after 1904 and those already in Australia would not be allowed to stay. With few exceptions, they would have to be deported by 1907.\textsuperscript{38,47} Now the sugar industry would have to rely on White labour.

The industry was compensated for the loss of its exploited cheap labour force. The *Sugar Bounty Act of 1901* provided a subsidy for White-grown sugar.\textsuperscript{47} In the next 10 years the proportion of the sugar crop grown using White labour rose from 15 to 93 per cent.\textsuperscript{47} Whilst the economic costs had been met, it was not clear what the medical consequences would be. It was hoped but not clear that White men would be able to work as labourers rather than as overseers of non-White labour and that White capital would now be able to exploit the economic potential of the tropics with White labour as it had done with Black labour.\textsuperscript{43} Might the previous medical arguments against the working White man in the tropics have been wrong?

Warwick Anderson has written that there had already been a change in the international tropical medicine literature at the beginning of the twentieth century. Tropical diseases rather than the tropical climate were now seen as most threatening to the White settlers.\textsuperscript{38,48,49} The increased medical acceptance of the germ theory in the late nineteenth century had been at the expense of belief in disease-causing miasmas.\textsuperscript{50} The earlier acceptance of these ideas about miasmas had assisted in the belief in a pathogenic role for climate. The new emphasis on a microbiological explanation for the distribution of disease in the tropics was supported by the late-
nineteenth century discoveries and increased capacity of tropical medicine to understand tropical diseases. This international shift was replicated in Australia.

The first significant Australian evidence of this change in the discourse was JSC Elkington's 1905 paper called 'Tropical Australia: is it suitable for a working white race?'. It was considered of such national significance that the paper was tabled in the Senate and ordered to be published by the Government Printer only nine days after it was delivered at a meeting of the Royal Society of Tasmania. Elkington said that the tropical climate was not a significant obstacle to colonisation by the White race and that diseases and ill health in the tropics could largely be averted by improved personal hygiene and sanitation. Historian AT Yarwood has claimed that Elkington, following his move to Queensland, had a large influence on Raphael Cilento – a later and significant contributor to this discourse. However, before Cilento there was Breinl. At the 1911 Australasian Medical Congress, Breinl clearly stated that he agreed with the new emphasis of his international tropical medicine colleagues:

Manson's remark, that "the more we learn about these diseases the less important in its bearing on geographical distribution and as a direct pathogenic agency becomes the role of temperature, and the more important the influence of tropical fauna," sums up our experience. (p. 524)39

Whilst Breinl seemed convinced of the primary importance of microorganisms and infectious disease, later in his paper to the congress he accepted that there was conflicting evidence about the several theories about why the White race might not be able to thrive in the tropics: tropical climate, tropical diseases, and the indigenous population. The indigenous population was described as a threat: both as a reservoir of tropical diseases and as a corrupting influence on White behaviour. This was not, but might have been, used to justify a different research agenda to that chosen, with a greater emphasis on Indigenous health research – not just disease surveys but the investigation of potential treatments like his trypanosomiasis research whilst at the Liverpool School. This may have had some value for the Indigenous population as well as for its primary beneficiary - the White population in Queensland.
Australia, Breinl chose to divert his research effort from tropical parasites and diseases to the question of the impact of the tropical climate on the White man. He told the congress:

Tropical Australia should be an ideal locality to decide this question definitively, as it is a country where comparatively few diseases are prevalent, and where aboriginal races are practically dying out. (p.531)39

The importance of the perceived absence of Aboriginal people to this field of research was clearly stated to the readers.54 Apart from mentioning the imminent demise or near absence of Aborigines, they were rarely discussed explicitly in this research until the 1930s. The research publications emphasised the differences between Whites and either Melanesians or non-White Australians or indigenous populations from other countries. Lindsey Harrison has noticed the discussion of Melanesian labour in the sugar industry in contrast to the lack of discussion of Aboriginal labour in the pastoral industry.55 She has asserted that this was due to medical ignorance of the important role of Aboriginal men and women in the pastoral industry. It was probably also because of the prominent place of the arguments about the deportation of Melanesian sugar industry labourers in the contemporary political debates about the White Australia Policy.

In other settings, the imminent demise of the Aboriginal race was used to add urgency to Aboriginal health research. Here it meant that an annoying confounding variable could conveniently be ignored. This dispassionate use of the tragedy of Aboriginal de-population in north Queensland (that we now know was in part due to White frontier murders and massacres56) seems heartless and offensive. Breinl’s acceptance of the reductionism of science meant that he could see this as an opportunity to get closer to a definitive and cleaner answer to a messy and complex problem. It was a problem that was important to the emerging Australian national identity and to the sustainability of colonialism in the tropics in Australia and elsewhere.
It appears that Breinl made a politically astute decision. He was supported by two unanimous resolutions at the congress. One was for the congress to lobby the Federal government for more funds and staff for the AITM to ‘enable an organised inquiry to be undertaken without delay into the various matters likely to affect the permanent establishment of a working white race in Tropical Australia.’ (p.532) The other was that ‘the permanent settlement of a working white race in tropical Australia be adopted as the principal subject’ at the next congress which was to be held in Brisbane in 1917. (p. 532) The extra funds and researchers duly arrived and also came with a new building for AITM which was opened in 1913. The report in the *Australasian Medical Gazette* of this opening praised the national significance of the institute’s research into the White race in the tropics but did not mention any other tropical medicine research to be undertaken by the institute.

‘The supreme national importance of this matter’

The Brisbane congress was delayed by the war until 1920 but tropical Australia was discussed as resolved. A sub-committee had also been formed to address the question and reported to the congress. It emphasised ‘the supreme national importance of this matter’. (p. 40) The subcommittee used the results of a survey of doctors advertised in the *MJA* and comparisons of the results for north Queensland with other regions for military health examinations, medical inspections of schools and actuarial investigations by a life insurance company. They found no evidence of ‘inherent or insuperable obstacles in the way of the permanent occupation of tropical Australia by a healthy indigenous white race.’ (p. 45) I do not agree with Anderson who has suggested that the congress prescribed a White Australia as ‘doctor’s orders’ (p. 36). Rather the congress, or at least the majority of those attending, did not proscribe it as unhealthy. However, like other Australians, many probably supported the policy and its aims.

Breinl was the second speaker to address the session on tropical Australia and his work and results dominated the session and were repeatedly referred to by other speakers. He described the results of physiological experiments on Townsville waterside workers and on school children. He declared that ‘there has not been a
single difference to differentiate between physiology in a temperate climate when compared with a tropical climate.’ (p.51)\textsuperscript{58} Similarly, he reported favourable results from an examination of hospital admission rates, case mortality rates, death rates, birth rates, and infant mortality.

This reflected considerable research effort and had already been reported by Breinl and the biochemist WJ Young in more detail during the previous year in a series of articles in the \textit{MJA} which were then reprinted as a long article in the \textit{Annals of Tropical Medicine and Parasitology} in 1920.\textsuperscript{60-63} Breinl and Young concluded these articles by calling the White settlement of tropical Australia ‘the “Great Experiment of White Australia”’ (p. 403)\textsuperscript{62} These articles also collated and summarised some of the contemporary international literature on the subject. This included a variety of bizarre physiological experiments. My favourite is Phalen’s 1910 paper describing the effects on 500 soldiers in the Philippines of wearing orange-red underwear for a year compared with a similar group who wore white underwear.\textsuperscript{63} Whilst bizarre, these were serious experiments measuring many physiological outcome variables to test the hypothesis that the ideal clothing would be white clothes to reflect sunlight combined with the coloured underwear which should exclude the sun’s rays.\textsuperscript{6} The soldiers with coloured undies did less well.

Nisbet’s ideas about the effects of the tropical climate on women and children were also put to the congress. These were admitted to be based on 31 years ‘experience’ in general practice in Townsville not on ‘statistics’ or experiments. His comments were overwhelmed by Breinl’s scientific evidence. Direct criticism of Nisbet’s ideas was muted by politeness: following his recent death, his paper was given by his son, also a doctor. Raphael Cilento was not so constrained five years later when he wrote the monograph \textit{The White man in the tropics} for the Commonwealth Department of Health as the new director of the AITM. In his summary of the discussions at the congress, he wrote:

\textsuperscript{6} The photographs in Lambert’s memoir of the hookworm campaigns in the Pacific in the 1920s and 1930s featured the White medical staff attired all in white (see figure 4.3), a ‘uniform’ that now seems synonymous with colonialism.\textsuperscript{64}
It was not apparent, however, that Dr. Nisbet’s conclusions were based on anything but general impressions, since there is no record of his performing even a single series of investigations on any scientific basis. (p. 55) (my italics)46

The easiest criticism in the medical literature of an opposing view was to call it unscientific. Even more damning was to call it political. Medical authors were quick to resort to these labels. In 1915, the MJA published a large series of letters to the editor debating the White Australia policy. In these letters expressing a range of positions, most authors claimed that their arguments were based on ‘science’ and questioned the similar claims of other writers that their opposing arguments were ‘scientific’. And so one anonymous writer, using the pseudonym ‘M.B.’, pilloried the prejudice of politicians that had resulted in the White Australia Policy in contrast with his or her medical scientific evidence that White men were not adapted to work in the tropical sun.10 M.B.’s argument was that the White man lacked the Black man’s pigment which protected his brain from the sun’s rays that passed through the skin and skull and were then transformed into heat. The notion of boiling brains unprotected by black pigment as science seems ridiculous today but it was a way of saying that these claims were ‘objective’ and ‘true’ and easily dismissing competing claims.

Whilst the discussions at the congress session were dominated by Breinl’s results, the outcomes were dominated by another man, Howard Cumpston. Cumpston was the Federal director of Quarantine and a member of the Advisory Council of the AITM. He was the first speaker in the session and moved the resolutions which were eventually carried. These resolutions were largely those of the committee which had reported to congress. They had concluded that:

the whole question of successful development and settlement of tropical Australia by white races is fundamentally a question of applied public health in the modern sense, such has been demonstrated and practised with success amongst civil populations, under far more difficult conditions, by the American authorities in the Philippines prior to the Great War, and throughout the military forces of every allied power during that War. (p. 45)58
What did the committee include in this modern applied public health? As well as a wider dissemination of the AITM’s results to the public, they proposed continued and increased statistical surveillance of the White tropical population’s health. They also recommended increased legislative controls related to alcohol consumption, town planning, land tenure, working hours, transport, assisted immigration of Whites to tropical Australia and finally the ‘avoidance of hybridism and the perpetuation of pure races’ (p. 47). They were talking of markedly increased social control by a stronger government bureaucracy in order to improve the population’s health. The experiences and achievements of military planning and control during the war years were an explicit inspiration. Anderson has described this as ‘the ending of medical geography and the beginning of medical government’ (p. 458).38

The congress resolutions also called for more quarantine services including diagnostic laboratories and for the formation of a Federal Ministry of Health.58 A Commonwealth Department of Health was established the following year (1921) with its national foundations based on the existing quarantine service. Cumpston was its bureaucratic head until his retirement in 1945 and was a dominant figure in Australian public health in these years.52,66

Cumpston has also been implicated in Breinl’s forced resignation two months after their shared triumph at the Brisbane congress.33,67 It is not clear whether it was post-war intolerance of his Austrian background or the way Cumpston and the Advisory Council of the AITM treated him that led to Breinl’s resignation.35,68 The main work of the institute had been completed with the acceptance of its research at the 1920 congress and it was possibly only an outbreak in Townsville of bubonic plague in 1921-1922 that postponed its closure until 1930.67,69 Raphael Cilento has

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1. In 1905, Elkington had described the hypothesised causes of the ‘degeneration of the progeny of white races in the tropics’ as ‘climate, disease, and mixture and association with coloured races.’ Like those at the 1920 congress he concentrated on the first two but did state that ‘the evidence is against the half-caste, and experience of other countries goes to show that it is advisable to keep the white stock pure’ (p. 6).51 Robert Young has investigated the history of ideas about racial hybridity in the nineteenth century in Colonial desires.65

8. Unlike Anderson, I have not discussed the geographical literature where, not surprisingly, the ideas of the insalubrity of tropical climates for the White race concentrated on the climatic variables rather than the health outcomes and lasted longer than in the medical literature.
blamed the closure of the AITM in Townsville and relocation to Sydney University on Cumpston and the struggle between the States and the Commonwealth for the control of the Australian medical system.69

Meanwhile, in contrast to the Australian research, the international tropical medical literature about the White race in the tropics did not ignore tropical diseases and the indigenous populations. This meant that the reduced role for the tropical climate compared with tropical diseases did not necessarily immediately change the perception that the tropics were dangerous to Whites. These writers could still use the same evolutionary logic to explain that the White race had no inherited racial immunity to these diseases as they may have said in the past about climate. The theories of racial immunity like those about climate were largely reliant on anecdote rather than research.38,48,70

With increasing microbiological, epidemiological and clinical evidence of disease in the indigenous peoples of the tropics, these notions of absolute racial immunity faltered to be replaced by more partial notions of acquired immunity. The indigenous populations were no longer seen as racially immune and disease free but as living reservoirs of disease that could infect the less immune White colonists. The medical solution was again, as with the White population in tropical Australia, increased social control and surveillance. But now it was directed at the indigenous populations of the tropics. The laboratory and tropical medicine became heavily involved in the coloniser's control of the colonised.48,70

*The White man's 'triumph in the tropics'*

Raphael Cilento was the director of the AITM for most of the time after Breinl's resignation in 1920 until the institute moved to Sydney in 1930. Fedora Fisher has written a very favorable yet still fascinating biography of Cilento.71 She has described his economic hardships as a medical student in Adelaide (including his

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b Perhaps reflecting his identification with the international tropical medicine literature rather than the Australian medical literature and lay opinion, Breinl emphasised in his work that Aborigines did not have *racial* immunity.8
involvement with the physical anthropologists discussed in the next chapter) and his contributions first to tropical medicine and later to public health (or social medicine), health administration and medical education. His tropical medical work was influenced by his political belief in the need for strong control of indigenous populations by colonial authorities and his distaste for more compassionate regimes.\textsuperscript{71}

During the Second World War, Cilento was harassed for his Italian migrant background as Breinl had been for his Austrian birth in the previous war. He was further attacked by the BMA for his association with the (unsuccesful) plans for a national health service in the 1940s and feeling victimised left Australia in 1945 to work for the United Nations Relief and Rehabilitation Administration. When he returned to Australia in 1951, he did not return to public health or health administration but to general practice where he worked with his wife and son.\textsuperscript{71}

In 1925, five years after the Brisbane congress and five years after Breinl's resignation, the new Commonwealth Department of Health published Cilento's 168 page monograph entitled \textit{The White Man in the tropics}.\textsuperscript{46} The subtitle was 'with especial reference to Australia and its dependencies'. At the time Cilento was both the director of AITM and temporarily the director of Public Health in the Mandated Territory of New Guinea. Like others, he emphasised that the situation in tropical Australia was quite different to that in New Guinea.

\begin{quote}
The tropical areas of Australia are unique in that they have no teeming native population, riddled with disease, but are occupied by many thousands of pure-blooded European settlers (p.9).\textsuperscript{46}
\end{quote}

In accordance with the resolution by the 1920 congress, he explained that the purpose of the monograph was 'placing at the ready disposal of Australians a comprehensive survey of the facts and figures affecting the residence of white persons in the tropics.' (p.5)\textsuperscript{46} Whilst he included the research conducted during Breinl's time at the AITM, he was much more dogmatic in his refutation of the role of the tropical climate and included much more anecdotal evidence than Breinl.\textsuperscript{63}
Whilst he used the authority of science to support his argument, he did not use its tentative voice in his conclusions. He adopted the boosterist style of politics whilst claiming a victory of science over mere politics.

Cilento included results of the recommended ‘sociological investigations’ of more than 700 households in north Queensland (concentrating on the living conditions of women), and the physical examination of 2000 school children and 100 working women. He described evidence of economic hardship but concluded that ‘The dire presages of evil which medical opinion indicated would befall the female of the race are not borne out by the figures’ (p.91)\(^46\). Similarly he mentioned approvingly the increasing numbers of the ‘second generation of tropically-born children’ and asserted that these children’s heights and weights were comparable to those elsewhere.

In the second half of the monograph, Cilento described the standard public health measures to protect health in the tropics: water supply, housing, waste disposal, personal prophylaxis, insects and pests. As a White resident of tropical Darwin for nearly a decade, I find Cilento’s ideas about clothing, exercise, hobbies and the importance of the ‘club’ quaint and amusing. More revealing is the final section of this part of the monograph, entitled ‘Control of Natives’. He suggested that the ‘correct attitude’ towards ‘natives’ involved strict adherence to notions of White propriety and White power by advocating maxims such as ‘Be insistent as to obedience in all details’ and ‘Be as considerate as possible, but never weak.’ (p. 168)\(^46\) These remarks, whilst probably about New Guinea and not primarily about Aborigines, reflected Cilento’s own politics but also emphasised the close relationship between colonial power and control and tropical medicine and the ‘White man in the tropics’ discourse.

As the research about the White race in the tropics relied on the perceived absence of Aboriginal people, the Aboriginal health bibliography cited little of this research that had been published in the \textit{MJA}. It did not cite detailed papers by Breinl in 1915\(^54\) and 1919\(^60-62\) nor Cilento’s long paper entitled ‘Conquest of climate’ in 1933.\(^72\) It only cited three such \textit{MJA} publications, all from the 1930s. - Grenfell
Price's paper 'The White man in the tropics' that he read at a meeting of the SA branch of the BMA in 1934, JB Cleland's response to this paper, and in 1938 a paper by AH Baldwin who had moved with the AITM from Townsville to Sydney.\textsuperscript{73-75} Baldwin reported further results from the statistical surveillance of the health of the White population in Queensland.

These three publications described the sometimes unstated but implicit role of Aboriginal people in this research, especially the importance of ‘the absence of any considerable numbers of natives’ (p. 734)\textsuperscript{75}. Baldwin wrote that this was important not just for the health of the White man but also for Aboriginal people's health. In this and in an earlier paper with Cilento about malaria\textsuperscript{76}, he claimed that it was the small size of the Aboriginal population and of the individual ‘tribes’ and their ‘nomadic habits’ rather than any racial immunity which had protected their health. But he warned that they could become sick if they lived with and like Whites. The cause of ill health was behaviour and environment not race: nurture not nature.

Grenfell Price and Cleland also discussed Aboriginal people together with the White race. Grenfell Price said that the ‘Outstanding questions of North Australia are the aboriginal and half-caste problems.’ (p. 109)\textsuperscript{13} He proposed ‘segregation’ where possible and where not he suggested ‘absorption’. He thus linked this ‘White man in the tropics’ discourse with discussions of the government policy towards Aboriginal people in the 1930s.

In response to the American involvement in the war in the Pacific, an anonymous alarmist \textit{MJA} editorial expressed concern in 1945 that Australian research into the health of the White man in the Tropics had ‘fallen behind’ (p. 85).\textsuperscript{55,77} There was no dramatic response but Queensland research in this field did continue into the 1950s.\textsuperscript{31} In 1951, the \textit{MJA} published results of a further survey of the weights and heights of ‘second and third generation’ Queensland school children.\textsuperscript{78} In the following year, researchers studied the effects of climate on the results of psychological tests in children living in Toowoomba and tropical Townsville. In both studies the results were similar in tropical and non-tropical Queensland.\textsuperscript{31} Ross Patrick, the author of the 1951 survey, later wrote that by the 1960s, it was accepted that the White man's body could adapt to the tropics 'but the body's covering was
not so fortunate’ (p. 365) – higher rates of skin cancer had been found in north Queensland.\textsuperscript{31} It is in the discussion of skin cancer and its prevention that we see the last vestiges of this discourse preserved into the twenty-first century.\textsuperscript{49}

The significance of this research was summarised by Cilento in 1959 when he wrote (with Clem Lack) a history of Queensland for its centenary of White settlement.\textsuperscript{69} He commenced by stating: ‘The story of the self-governing State of Queensland is essentially the record of the white man’s triumph over climate and his taming of the tropics.’ (p.1)\textsuperscript{69} His final chapter carried the same title as his book, \textit{Triumph in the tropics}, and was largely concerned with the ‘White race in the tropics’ research and debates. It was the story of the triumph of science helping to create a White Australian nation (reliant on the assumption of an absent or soon to be absent Aboriginal population). Whilst the notions of race used were different to those used today, the inherent racism of the discourse is unfortunately very familiar. As AT Yarwood has written:

\begin{quote}
For Cilento, the consequence of his attachment to the peopling of north Queensland with sturdy white settlers was an unrelenting racism that showed itself in the defence of the migrant and quarantine frontiers of white Australia, and in the rigid, paternalistic control of the Aborigines. (p.63)\textsuperscript{53}
\end{quote}

The physiological impact of the tropical climate investigated in this research was entangled with the political context of European and American colonialism and colonisation of the tropics in the nineteenth and early twentieth century. Related physiological research in the 1960s involving Aboriginal people from the Northern Territory reflected the new context of post-war internationalism (buoyed by decolonisation), scientific and technological optimism, and huge social and demographic change. In the late 1950s, following the success of research associated with the International Geophysical Year, the International Council of Scientific Unions began plans for the International Biological Programme (IBP).\textsuperscript{79}
The ‘Human adaptability’ section of the IBP investigated the adaptability of Aboriginal people in the Northern Territory not only to climate but to ‘assimilation’ and the social changes associated with increasing White contact. Reflecting the new political context the portrayed purposes of this research were not about promoting a racialised White Australian nation but increasing the internationalist understanding of the adaptability of all humankind. Both the IBP and Cilento’s earlier research programs used the optimism and power of modernist science to promote their quite different political purposes.

The Australian Hookworm Campaign:

a shining example of what can be accomplished by intelligent, well-organized cooperation in making tropical countries healthy places to live.

Whilst those promoting AITM’s research into the viability of the White race in the tropics emphasised the scarcity of tropical diseases, north Queensland was known to be not entirely free of these diseases. The most important example was the intestinal parasitic infection caused by hookworm. Soon after the beginning of the twentieth century, following Egyptian research which was quickly supported by observations in Queensland, doctors in Queensland began to discard earlier notions of faecal-oral hookworm transmission and replace them with present-day notions of transmission. It is now widely accepted by doctors that people become infected with hookworm when larvae in the soil penetrate the skin. This is usually caused by walking barefoot on ground that has been contaminated with faeces containing hookworm eggs, which leads to two main preventive strategies: wearing shoes and

\[1\text{ This investigation of climate and physiology had antecedents not only in the White race in the tropics research but in physiological research on the cold tolerance of Aboriginal people performed during the inter-war expeditions to Central Australia by the Board for Anthropological Research (see chapter five).}\]

\[2\text{ There are two different hookworms: the ‘Old World’ hookworm, Ankylostoma duodenale (hence the disease is frequently called ankylostomiasis), and the ‘New World’ hookworm, Necator americanus. This second hookworm was first identified by the American zoologist Charles Wardell Stiles in 1902. He originally called it Uncinaria americana but quickly changed it to its more evocative name which means American killer. Stiles became the medical director of the hookworm campaign in the American South.}\]
using toilets. Heavy hookworm infections can lead to quite severe anaemia and subsequent debilitation. These sequelae led to hookworm being called the 'bloodsucker' in Queensland and the 'germ of laziness' in southern USA at the beginning of the twentieth century.

In the conclusion of his contribution to the discussions about the White race in tropical Australia at the 1920 congress, Breinl singled out hookworm as the exception to the apparent scarcity of tropical diseases in Australia. Hookworm had been written about in Queensland since the late 1880s. In 1911, at the previous congress, Breinl mentioned that hookworm infection was common in north Queensland and said the 'study and eradication of this disease would be of the utmost importance.' (p. 530)

This part of chapter four investigates the Queensland and then Australian Hookworm Campaigns from 1918 until 1924. Hookworm and the associated intestinal parasites investigated by the hookworm campaigns were the first diseases to receive sustained attention in MJA articles that discuss Indigenous peoples' health. However, neither the Hookworm Campaign nor any of these articles was predominantly about Indigenous people's health. Indigenous people were usually only mentioned by authors in passing – albeit to mention that they had the highest prevalence of infection with hookworm and other intestinal parasites.

Moodie and Pederson's Aboriginal health bibliography identified five MJA articles about these campaigns. The bibliography seems to have missed another MJA article about the Australian Hookworm Campaign that mentioned Indigenous people's health which I will discuss with the other articles. These would have been very visible articles to the MJA readership. Three of these six articles (and two articles about the hookworm campaign which did not mention Indigenous people) were placed as the first article of an MJA issue. All six included tables, two included large maps and the first included a series of photographs (not of Indigenous people but of non-Indigenous children and of toilets in Chinatown in Cairns). Two had also been read at the 1920 Australasian Medical Congress.
one at the 1924 congress\textsuperscript{29} and one at a 1921 meeting of the Queensland branch of the BMA\textsuperscript{87}. The first of these hookworm articles in the \textit{MJA} began:

Acting upon the recommendation of the Australian Institute of Tropical Medicine relative to the prevalence and serious economic importance of hookworm disease (anchylostomiasis) in tropical Australia, the Commonwealth Government in 1916 invited the International Health Board to extend its worldwide campaign against hookworm disease and soil pollution to Australian territory. (p. 505)\textsuperscript{14}

This sentence overstated the role of the AITM and underplayed the influence of the Rockefeller Foundation’s International Health Board (IHB) in the decision to begin this large public health research and intervention project. The IHB always required the local government to ‘invite’ the IHB to fund a hookworm campaign.\textsuperscript{90} The author, state director JH Waite, was probably just following the IHB’s rather rigid procedures. James Gillespie has stated that ‘The IHB was insistent that the American presence should be discreet as possible.’ (p.68)\textsuperscript{91} He also has cited the diary of the IHB’s Far Eastern director during his preliminary visit to Australia in 1916 which mentioned public and official suspicion of large corporations and Americans.\textsuperscript{91}

\textit{The political and public health goals of the hookworm campaigns}

The IHB and the Australian Hookworm Campaign had its origins in the Rockefeller Sanitary Commission for the Eradication of Hookworm Disease in southern USA. The Rockefellers had been funding school and then agricultural projects in the South since 1902. The success of the agricultural projects was thought to be limited by the poor health of the Southern workers. The Sanitary Commission was established in 1909 because it was argued that the disease having greatest impact on Southern workers’ productivity was hookworm. In five years, the Sanitary Commission examined nearly 1.3 million people and treated nearly 700,000 people for hookworm.\textsuperscript{86} The Rockefellers established the Rockefeller Foundation in 1913 (with its International Health Commission which was re-named the International Health Board (IHB) in 1916\textsuperscript{92}) and took their hookworm and public health
campaigns to the world. Within five years they had 'invitations' to commence projects from 42 governments. Hookworm remained a major focus of their work. Several historians have written about various aspects of the Sanitary Commission's hookworm work in Southern USA and the IHB's work in Latin America, the Pacific and Asia. James Gillespie has written a history of the hookworm campaign in Australia. He has emphasised that the rigid application of American models for IHB hookworm campaigns was inappropriate for the different epidemiology of the disease in Australia. He has asserted that whilst Australians like Cumpston, Elkington and Cilento were privately skeptical of the importance of hookworm in Australia and the need for the campaigns, they gave their public support in order to pursue other public health aims. The most important of these was the strengthening of the Commonwealth's role in public health and the creation of a Commonwealth Department of Health.

The Australian hookworm campaigns also have been discussed briefly in histories of health services and Aboriginal affairs in Queensland, a Rockefeller doctor's memoir of the Pacific hookworm campaigns, and in Lindsey Harrison's history of Aboriginal health publications in the MJA. Harrison devotes a page to a brief description of the representation of Aboriginal people as a threat to the health of the White community. This description is based on only the first of these MJA articles about the campaigns.

After briefly summarising the contextualising stories of the Australian hookworm campaigns, I focus on the representations of Indigenous people in the articles in the MJA published about these campaigns. The representations of Indigenous people in these articles assumes greater significance given the size and importance of the campaigns. These were enormous international and Australian public health activities and had a significant impact on the development of health services in Australia.
Figure 4.3. Colonial power and imagery from the Rockefeller hookworm campaign in the Pacific

The first Australian hookworm campaign funded by the IHB commenced in the dependency of Papua in 1917. The Queensland Hookworm Campaign began the following year. After a year, the Queensland Hookworm Campaign was expanded to become the Australian Hookworm Campaign and extended for a further five years. Papua and New Guinea, the Northern Territory, and all the States were involved. The contributions of the Commonwealth Government and State Governments increased and those of the IHB decreased over the course of the campaign. As planned, the IHB withdrew entirely after five years.

This requirement of the local government to provide some funding (usually increasing over the course of longer campaigns) was characteristic of IHB campaigns elsewhere. Also usual was the incorporation of the campaign into the local health department. Often this involved using the hookworm campaign to initiate a public health focus and organisational structure within the government bureaucracy: first in the South and then outside the USA. This government commitment to public health and models for the bureaucratic organisation of public health usually remained long after IHB's departure and might be considered the most important and the most lasting legacy of the campaign. The campaigns were also crucial to the creation of the academic discipline of public health. In order to train staff for its hookworm campaigns, the Rockefeller Foundation donated funds to Johns Hopkins and Harvard Universities to establish schools of public health. Later, funds were donated to many universities outside the USA.

In his preliminary report of the Queensland Campaign, its director JH Waite emphasised the financial contribution of the Queensland Health Department and that the campaign 'became an organic function of the Queensland Department of Public Health' (p. 505). Whilst public health was strengthened in the Queensland Health Department, the campaign led to far greater changes in the Federal bureaucracy. The IHB and hookworm campaign have been credited with playing a crucial role in the creation of the new Commonwealth Department of Health in 1921. Gillespie has cited a letter from the Far Eastern Director of IHB to Waite in 1918 which
expressed some doubts about the public health importance of hookworm in Australia but stated:

The ultimate goal is the hope that a five years program would result in the creation and functioning of a central health department. (p. 72)\(^9\)

Gillespie has made much of the skepticism about the magnitude of the public health importance of hookworm infection in Australia and of the impact of the campaign in material written by Cumpston, Elkington, Cilento and even IHB employees in the Rockefeller and Australian archives.\(^9\) Breinl was also said to have doubted the severity of the hookworm problem. These men mainly saw the campaign and IHB as an opportunity to promote other local political objectives such as strengthening public health and the creation of the Commonwealth Department of Health. Was this cynical political opportunism and careerism wasting limited resources for public health or were these men heroes of public health? They probably saw the IHB offer of funds as 'a window of opportunity' to establish secure foundations for public health in the Australian government bureaucracy – a greater and more durable public health outcome than any single disease campaign, no matter how prevalent the disease.

For the IHB, an Australian campaign was intended to facilitate campaigns in English colonies in Asia.\(^9\) Other IHB hookworm campaigns also have been shown to have been motivated more by politics than disease prevalence. Anne-Emanuuelle Birn has questioned the IHB's concentration on hookworm in Mexico in the 1920s and 1930s.\(^9,9^2\) The IHB had earlier been involved in yellow fever eradication in Mexico but soon moved to hookworm work. During this period, hookworm was not an important cause of death in Mexico and IHB surveys showed that hookworm was not even very common, especially in the areas first chosen for the campaign's activities. The campaign concentrated on areas in and around the state of Veracruz which was politically unstable but economically very important to the USA due to its agriculture and large oil exporting port. Other Mexican states had much more hookworm infection.\(^9^0\) Birn and Armando Solórzano have written that:
The political ends of the campaign – to quell unrest by demonstrating the value of government services in as many places as possible – overshadowed bona fide scientific concerns. (p. 1203)

The public and published comments, such as the papers in the *MJA* and at the Australasian Medical Congresses, were quite different to the private and unpublished uncertainty of the importance of hookworm in Australia. The medical audience at the congresses and readers of the *MJA* would have been left with a clear impression that Australians should be particularly worried about hookworm. The most florid and frightening warnings in the *MJA* were made by campaign’s first director, JH Waite. The first article published in the first issue of the *MJA* in 1919 was a detailed study by Waite and school nurse Irene Neilson comparing the intelligence test results of 116 children not infected, 65 lightly infected and 159 heavily infected with hookworm. They showed results that suggested arrested mental development and photographs describing delay in the growth and sexual development of those children with heavy hookworm infections (figure 4.4). They dramatically concluded:

> The direct outcome of the results of such a blighting disease, which is preying upon 40% of the total school population from Cooktown to Townsville, and is stamping serious mental, physical and sexual degeneracy upon 25% of the total school population, can be nothing other than the weakening of the social fabric and the unfitting of the coming generation for the struggle for existence. Indeed the tendency of the disease, if not controlled, is toward the obliteration of the race through the unsexing of its victims and reducing individual resistance toward acute infections, such as pneumonia and tuberculosis. (p.4)

Their dire warning about race degeneracy and possible extinction from hookworm infection was very similar to contemporary publications about the effects of the tropical climate on the White race. Reflecting the impact of (and citing) this article and the greater prevalence of infection in school age children than other age groups, other *MJA* publications emphasised the focus and impact of the campaign on

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\(^{k}\) This article did not mention Indigenous people and was not included in the analysis in chapter three.
Figure 4.4. Waite and Nelson’s photographs of ‘physical and sexual degeneracy’ caused by hookworm disease

children’s health and education. In Australian medical journals, there was almost no discussion of hookworm and workers’ health. A large investigation of miners and adult agricultural workers found little evidence of occupational infection in Australia but was only published in the *American Journal of Tropical Medicine.*\(^{101}\) In contrast, Richard Brown has emphasised the importance of improving labour productivity to the southern American and later hookworm campaigns:

> In virtually every annual report, every memorandum, and every discussion the extent of hookworm infection was described and the loss in labor productivity estimated. (p. 900)\(^{86}\)

Brown has argued that more than just improving productivity, the programs in the South aimed to improve the Southern economy and its incorporation into the national economy dominated by Northern capitalists like the Rockefellers.\(^{86}\) In other tropical countries, the campaigns performed a similar facilitating role for US imperialism and capital.\(^{86}\) Some historians have questioned and others have just omitted Brown’s emphasis on political and economic goals and outcomes over the campaigns’ undoubted humanitarian outcomes and intent.\(^{82,94,95,97}\)

In the final publication from the campaign in the *MJA*, the campaign’s director, WC Sweet, was still promoting hookworm as ‘the most important of the tropical diseases in Australia’ (p.322)\(^{29}\) His article gave some idea of the scope of the campaign. He claimed that in the campaign’s initial survey, 248,721 people were examined for hookworm, 48,256 were found to be infected and 33,249 were treated with oil of chenopodium (a complex, unpleasant and not very effective treatment with occasional side effects that also required the patient to take a purgative).\(^{29,64}\) The bulk of the campaign’s *MJA* articles was devoted to reporting the frequency of the identification of hookworm and other intestinal parasites in the thousands of faecal specimens collected by these surveys.\(^{14,16,28,29,102-104}\) They also described the educational elements of the campaign. These were on a similarly grand scale to the surveys and treatments. They included shop window exhibits, public lectures, lectures in schools, microscopic demonstrations of hookworm eggs and larvae, media campaigns, pamphlets, and the inspection, repair and building of toilets.\(^{14,64}\)
So how were Indigenous people represented in publications from this massive public health research project and campaign that was pivotal in the development of the international and Australian public health profession and bureaucracy? In the first article about the Queensland Hookworm Campaign, Waite’s first mention of Aboriginal people sounds very similar to the contemporary ‘White race in the tropics’ discourse. He described Queensland in this way:

While the population is essentially of British stock, there are small numbers of Italians, Slavs, Chinese, and Australian aborigines. The residue of the rapidly disappearing aborigines now numbers 16,000 souls, of whom about 5,000 are serving the white settlers on land and in towns. (p. 505)

It is probable that the IHB chose to concentrate most of its Australian operations on Queensland not just in response to the agitation and the accessibility of the AITM and the already acknowledged presence of hookworm disease but because the Queensland population was mainly White. Later in this article and in subsequent MJA publications by the campaign, it was made clear that Aboriginal people had much higher rates of infection than Whites. In spite of this, the major concern of the articles and the campaign was the health of the White Australian population. Similarly, Sylvester Lambert, who replaced Waite in 1918, did not mention any work with Aboriginal people in the description of the Australian hookworm campaign in his memoir.

This emphasis on the health of the White population is most obvious in the final MJA article about the campaign in its description of who was examined by the campaign’s first survey. More than 80% (167,290) of those examined in Australia

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1 Waite’s successor, Lambert, repeatedly lamented the declining indigenous populations of the Pacific and their possible extinction in his memoir but did not mention Australian Aboriginal people. He blamed colonialism and especially introduced diseases for the population decline and ill health in the Pacific – definitely not the beliefs and behaviours of the indigenous peoples (which he did blame for Aboriginal people’s hookworm in the MJA).
(excluding Papua and New Guinea) were from Queensland. In contrast, in the similarly tropical Northern Territory only 886 were examined whilst in the three least tropical states of Tasmania, Victoria and South Australia 7,987 in total were examined. The reason may have been the author’s comment that ‘The hookworm problem in the Northern Territory is concerned mostly with aborigines’ (p.320)29. Similar numbers were examined in the partly tropical Western Australia as in each of the three southern states. The survey found the highest prevalences of hookworm infection in the Northern Territory (16.7%), Western Australia (10.8%) and only then Queensland (9.2%) where most of the campaign’s activities took place. Only two cases of hookworm infection were identified from the three southern states. Before quickly turning his attention away from Western Australia, the author made a similar comment to that made regarding hookworm in the Northern Territory:

The hookworm problem in Western Australia is chiefly confined to the coastal aborigines in and around Beagle Bay, eighty-four miles north of Broome, and to such whites as are in intimate contact with them. (p.320)29

Here, as in the international literature about the White race in the tropics, once Indigenous peoples’ ill health and diseases could not be ignored, they assumed importance mainly because they might affect the health of the colonists. Improving the health of Indigenous people was not considered particularly important in itself. This representation of Indigenous people’s diseases as only important as a reservoir of ill health for Whites was central to the campaign and remarkably resilient. Earlier, in a 1918 *MJA* article that used a hookworm epidemic at Yarrabah mission and four Aboriginal deaths ascribed to hookworm in the Torres Strait to justify the hookworm campaign, Waite made a contradictory claim two pages later about the minimal importance of hookworm to Aboriginal people and their health. He affirmed that the main importance of hookworm in Aborigines was that it might impact on White health:

Like negro races in other parts of the world, the Australian aborigines seem to possess a relative immunity toward hookworm infection. They are, however, none
the less dangerous from a public health viewpoint as distributors of the disease among white communities. (p. 508)14

Ross Patrick has written that by the time of the final report of the campaign in 1924, it had become clear that there was little foundation for such notions of Aboriginal racial (either acquired or inherited) immunity to the effects of disease.15 Sequelae were rare in Whites but severe anaemia was common in Aborigines.31 Apart from Waite’s one quoted remark, the MJA articles did not attribute to inherited racial characteristics the differences in hookworm and other parasitic infections between Aborigines and non-Aborigines. Rather, the higher infection rates were said to be due to Aborigines’ bare feet and ‘sanitary habits’. Or as Lambert wrote in the MJA in 1921:

The reason for the heavier infection among the aboriginals with most of the parasites studied may be found in the total lack of sanitary conscience. (p. 335)16

Waite did not restrict this judgmental and moralising tone or the absence of ‘sanitary conscience’ (p. 509) to Aboriginal people.14 Religious metaphors abounded in descriptions of the original hookworm campaigns in the American South82,94,96,97 but less so in those of later campaigns in other tropical countries98. Hookworm campaigns were characterised as spreading the message of and converting the population to ‘scientific medicine’.92 In the South, they talked of the ‘gospel of sanitary privy’.82 It is possible that such language and campaign methods had their roots in the Sanitary Commission’s connections with nineteenth century evangelism.97

Lambert stated in his memoir that ‘The North Queensland campaign had offered the combined excitement of a blitzkrieg and a Methodist revival.’ (p.11)64 He had spent one and a half years in the Australian campaign en route from work with the United Fruit Company in Costa Rica to nearly two decades with the IHB in the Pacific beginning in Papua in 1920. He continued, ‘I was preaching a crusade, and I was
heeded.’ (p.12) In the same book, Lambert admitted to being a not very religious man and having at best ambivalent and often quite negative opinions about the missionaries in the Pacific, so he may have been using this religious metaphor with deliberate irony. But such religious and military metaphors capture the zeal, certainty and righteousness that some public health workers can still display today.

The ‘blitzkrieg’ metaphor is interesting as the memoir was published in 1941. Lambert and contemporary readers would have been very aware of the very recent etymological origin of the use of ‘blitzkrieg’ (‘lightening war’ in German) in the English language following the Nazi invasion of Poland and the use of the associated ‘blitz’ following the bombing of London. The idea of a well-planned attack through and behind enemy lines may have reflected the idea of attacking tropical reservoirs of hookworm disease before they affected (or invaded) the population of temperate USA. As LW Hackett wrote of his former boss, the Rockefeller Foundation’s Wickliffe Rose:

Rose’s design was to wage a coordinated mass attack on some of these dangerous but preventable diseases on their home grounds, wherever they were found to be endemic and uncontrolled. (p. 165)

Whilst ‘blitzkreig’ was a very topical military metaphor, it still appears to have been slightly odd and maybe ill-chosen suggesting an American insensitivity to the horrors of the war in Europe. But it does remind me that other historians have noted the strong ideological similarities between the American and Australian Progressives associated with the IHB and its campaigns on the one hand, and Fascism on the other. In Australia, Cilento was the most extreme in this group with his open support of Italian Fascism and his role as a spokesman for several extreme Right wing political organisations after his return to Australia in 1951. Cilento’s biographer, Fisher, has accepted that Cilento was also strongly anti-Zionist (especially after the assassination of his United Nations’ colleague Count Bernadotte

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88 As late as 1980, a Florida doctor writing a history of hookworm control in that state claimed that African Americans had some racial immunity to hookworm infection probably caused by ‘biological selection’ in Africa.105
by members of the Stern Gang in 1948) but unconvincingly has dismissed charges that he was also anti-Semitic.71

Historians of the hookworm campaigns have suggested that their emphasis on the individual and his or her ignorance and sanitary sins de-emphasised political and economic causes and interventions.90,96,99 Similarly, in these MJA publications, it is Indigenous people themselves who are to blame for their hookworm infections. Whilst this representation of Indigenous individual responsibility for their ill health was clear in the MJA, Rosalind Kidd has found a more critical hookworm campaign by looking in Queensland government archives. She has applauded the hookworm campaign's doctors' exposure of the poor sanitation services and poor health of Aboriginal people on Queensland reserves.100 She has written that unfortunately these doctors were 'toothless' and they made little impact on the power and parsimony of the Chief Protector of Aborigines and his department.

The later MJA articles about the campaign suggested a particular focus on the treatment of Aboriginal people - albeit to protect the health of Whites - in the continuing work by local governments after the completion of the IHB campaign.29,87 Wilbur Sawyer, who had replaced Lambert as director, suggested different survey, control and health education strategies depending on the hookworm prevalence found - except for Aboriginal people.87 For them, interventions were not just determined by infection prevalence but by whether they were living on missions, settlements or plantations 'under the influence and control of competent white persons' (p. 143)87. The proposed interventions were also different and more authoritarian with a greater use of regular mass treatments for hookworm.

Gillespie has described the debate about the most appropriate intervention strategy to use amongst Northern Territory Aboriginal people with their high rates of infection.91 Before deciding on mass treatments, other options were canvassed. This debate neatly showed how the colonial relationship between White settlers and Indigenous peoples was reflected in decisions about public health strategies. Few Aboriginal people had been treated after the initial survey and so no improvement had been shown by a follow-up survey three years later. Complete segregation of
Aboriginal people was rejected by the employers of Aboriginal labour. Compulsory treatment was opposed because it might portray ‘weakness of “the whiteman”’ (p. 86)\(^1\). Incentive payments for treatment were rejected by Treasury.

Similarly, a later request to evaluate the chosen mass treatment option was rejected by the Commonwealth because hookworm was not ‘a definite menace to white people in the Northern Territory’ (p. 86)\(^1\). Instead the Commonwealth Government decided to concentrate funds on Queensland and New South Wales — where implicitly hookworm was more important because Whites were at risk. In Queensland, Cilento blamed ‘wandering’ Aborigines for re-infecting treated areas and supported stronger attempts to confine Aboriginal people on settlements and missions.\(^1\) His disregard for the impact of hookworm on Aboriginal people’s well-being in order to protect White health was apparent — he was suggesting virtual incarceration of Aboriginal people in those places with the highest hookworm infection rates.

**Conclusion**

These publications about tropical medicine in north Queensland in the first decade of the *MJA* were closely entwined with the history of the Australian nation — as a White Australia — and the history of the development of health and public health services in Australia. Indeed, the representation of Indigenous people in the publications had more to do with this history and contemporary politics than with the data collected by the researchers - a claim that would upset the researchers who argued that their ‘science’ was above mere politics. Indigenous people were only a marginal presence but their marginality was a crucial part of these texts. Researchers asserted that in tropical Queensland, Aboriginal people and their diseases were not important because they had nearly ‘died out’.

The research into the White race in the tropics treated race and racial characteristics as much more changeable than biological notions of inherited race. The racial determinism in many of these articles was not as pessimistic as it first seems today.
Even more amenable to change were the sanitary behaviours that were described as the cause of Aboriginal people's higher rates of infection with hookworm. In these articles that used either behavioural or racial explanations, Aboriginal people were blamed for their illnesses.

Indigenous people with their high rates of infection with tropical diseases like hookworm were described and treated as reservoirs of disease for White Australia. Indigenous people's health was not considered particularly important in itself. Their usefulness as labour for White capital was not used as justification for health services as it had been in other English tropical colonies like Malaya. Anton Breinl and others had shown that north Queensland like the rest of Australia could be populated and exploited by the working White race.

The White Australia policy is now justly reviled by most in our multicultural Australia. The associated medical research into the White race in the tropics now appears, at best, poorly conceived and, at worst, morally tainted by its inherent racism. In contrast, public health and the public health bureaucracy has survived and eventually thrived. Many in the public health movement would welcome more campaigns today like the Australian Hookworm Campaign that combine surveys, research, treatment, and health education and promotion (but perhaps not the limited health outcomes of the hookworm campaigns). However, the hookworm campaigns now might also appear morally questionable in their focus on the health of White Australia and their relative lack of concern for the well-being of Indigenous people.
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Chapter 5

Blood and race
The investigation of the blood groups of Aboriginal people was the most commonly mentioned research topic in publications about Indigenous people in the *MJA* before 1950. Blood groups were discussed in 16 of the 113 publications in this period. This research began in 1922 with the work of AH Tebbutt and Sarah McConnel of Royal Prince Alfred Hospital in Sydney. Research into the blood groups of Aboriginal people continued into the 1970s. Just before he died in 1975, Roy Simmons, who had contributed to this research for more than three decades, said that more than 10,000 Aboriginal people had been tested for their ABO blood groups as part of this research. This was part of a huge international research endeavour which had tested 15 million people for these blood groups.

This chapter investigates this research into Aboriginal people’s blood groups. Whilst many present-day readers might be aware of physical anthropology’s interest in measuring the skulls of Aboriginal people, few would be aware of its concern with their blood groups. Skull measuring is now pilloried and symbolises the worst of Indigenous health research and race science. Whilst few mention Aboriginal blood group serology, it was much more prominent than skull measuring in the *MJA*.

The index of Moodie and Pederson’s 1971 Aboriginal health bibliography has listed 69 publications under the subject heading ‘blood groups’. Nearly a quarter of these (16/69) were publications in the *MJA* and most of these *MJA* publications were from before 1950. After the *MJA*, the next most important journals for this blood group research were the *Journal of Tropical Medicine and Hygiene* (with 8 publications listed under the subject heading), *Oceania* (5), and the *American Journal of Physical Anthropology* (5). There were three different subject headings dealing with skull measuring but altogether they only identified two *MJA* articles and a letter to the editor of one of the early Australian general medical journals before the *MJA*.

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*a* 36% (13/36) of the blood group publications before 1950 were from the *MJA* and only 9% (3/33) of those from 1950 to 1970 were from the *MJA*.

*b* The three subject headings were physical anthropology – craniometry (43 publications), physical anthropology – craniology (30), and anatomy – skull (21).
This chapter begins with the European research at the beginning of the twentieth century that first identified the ABO blood groups and then linked blood groups with race. Then the first decade of blood group serological research in Australia is discussed. In particular, I investigate the finding that Aboriginal people’s blood groups were ‘ultra-European’. The chapter then addresses John Burton Cleland’s work. He wrote almost all of the blood group research about Aboriginal people in the 1930s and was the most prominent medical researcher writing about Indigenous health in the first half of the twentieth century. The discussion of his blood group research with the Board for Anthropological Research’s ‘vacation expeditions’ to Central Australia helps to contextualise the blood grouping research within the broader field of physical anthropology.

The discovery of blood groups and a biochemical definition of race

Blood group research began with Karl Landsteiner’s work in Vienna in 1900. He named three different blood groups (A, B, and C (which we now call blood group O)) which explained the reactions which occurred when the blood from 22 of his colleagues was mixed – or more precisely, when one person’s blood serum was added to another person’s red blood cells. Some combinations always led to clumping (or agglutination) and other combinations did not. Subsequent blood group research has largely been based on repetition of this simple experiment - mixing one person’s blood serum with another’s red blood cells and observing the mixture for evidence of agglutination.

Landsteiner’s research followed other researchers’ work which had described the reactions in animals caused by mixing their red blood cells with sera from a different

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type of animal. It was also influenced by work in the new field of immunology on the agglutination of bacteria in the blood in response to infection. His work had an enormous impact not only on blood transfusions but also on forensic medicine and, as we shall see, physical anthropology. Landsteiner continued to play a significant role in further developments in this field and was awarded the Nobel prize in 1930.

This pioneering work was quickly followed by research involving more subjects by two of Landsteiner's students (Decastello and Sturli) who identified a rarer fourth blood group which we now call AB. They also confirmed that the blood groups were unrelated to disease. Matters became somewhat confused by 1910 because two other classifications (by Jansky from Czechoslovakia and Moss from USA) of the four blood groups were also being used. To cause further confusion, both the Jansky and Moss classifications used the first four roman numerals for the four different blood groups but assigned them differently. In 1928, the Permanent Commission on Biological Standardization of the League of Nations responded to this confusion and recommended the adoption of the ABO classification that is still used today. Unfortunately, remnants of other classifications continued to be used until the 1950s leading to unnecessary obfuscation and complication of the literature – and potential additional risks to blood transfusion.

Emil von Dungern and Ludwik Hirschfeld (or Hirszfeld) demonstrated the Mendelian inheritance of blood groups in 1910. These two researchers were also the first to call the fourth blood group by its present name AB. Having shown that blood group was an inherited characteristic, they realised that it might be used by physical anthropology to define racial groups. In his history of the discovery of blood groups and their applications, William Schneider has suggested that it was largely chance that enabled Hirszfeld to test this possibility and so initiate the field of research that I discuss in this chapter.

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¹ Red blood cells from blood group AB are clumped by sera from all of the three other blood groups. But there are no iso-agglutinins in AB sera so it does not clump the red blood cells from any of the three other groups.
The Polish Hirschfeld was separated from his German colleague von Dungern by World War I. Whilst with the Serbian Army, he and his wife, Hanka, spent two years besieged with the Allied forces around the Macedonian port city of Salonika. Together, husband and wife wrote:

It seemed, therefore, that it would be of interest to make use of the properties of blood, as defined by Landsteiner's rule, to form an anthropological criterion for the discovery of hitherto unknown and anatomically invisible relationships between different races. Through the accident of war we happened to come to a part of the globe where more than elsewhere various races and peoples are brought together, so that the problems we are discussing, which otherwise would have necessitated long years of travel, could be brought in a relatively short time nearer to solution. (p. 677)34

They made the most of these opportunities and reported the blood group results of nearly 8000 subjects in an article published in the Lancet in 1919. They included 500 subjects of each of eleven nationalities as well as 1000 Russians, 1000 Indians and 400 Malagasies. They also included Landsteiner's Austrian results and added 'from memory' Ludwik Hirschfeld and von Dungern's earlier results with German subjects. As in contemporary Australian Indigenous health research, there was some slippage between categories of race and nationality in the Hirschfelds' article. Their tables were more precise and mainly used nationality and race separately. The text frequently discussed nationality as race. It is not clear whether this conflation of race and nationality was due to the army translators, Major Stefanovitch and Captain Murphy, or the authors.

Nevertheless, their results were extraordinary (see figure 5.1). Group A was most common in European subjects and least common in Asians and Africans. The reverse was true of Group B. There was even a gradual change from Western to Eastern Europe. They proposed the use of the ratio of the proportion of a population with blood group A to that with B as 'the biochemical race-index' so that they could

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English, French, Italians, Serbs, Greeks, Bulgarians, Arabs, Turks, Jews, Negroes (Senegal), and Annamese. (Table 11)34
Figure 5.1. The Hirschfelds’ blood group results

designate these relationships by a number’ (p. 678). This number neatly separated European (with an index from 2.5 to 4.5) from Asian or African peoples (index less than 1) with an intermediate group of Arabs, Turks, Russians and Jews from the eastern Mediterranean.

They made the bold statement that ‘The prevalence of the group A is characteristic of the European peoples.’ (p. 678) This hinted at the later use of blood grouping in racial classification and the racial definition of individuals but was certainly not the focus of their interpretation of their results. Indeed even this statement was more precise than much of the subsequent research and only referred to blood group as a characteristic of ‘European peoples’ not of a European person.

They showed that their results could be better explained by the geographical distribution of their subjects’ homelands than by accepted racial classifications. Their results contradicted the accepted relationships in contemporary anthropology between different racial groups. They gave two examples. Indians, who had been thought to be racially close to Europeans, had the most different proportions of blood groups from Europeans. Russians and Jews had similar blood group prevalences but differed in ‘anatomical characteristics, mode of life, occupation, and temperament’ (p. 678). This is how the Hirschfelds explained their results:

The figures arrived at by us are most easily explained on the assumption that A and B had different points of origin and that there are two different biochemical races which arose in different places. (p. 679)

They suggested that blood group B had originated in India and A in northern or central Europe. They believed that their results made it difficult to accept the prevailing anthropological theory of a single origin of humankind. This had been an area of vigorous debate in the nineteenth century and has been well described by the historian George Stocking. That century began with the wide acceptance of ethnologists’ biblical belief in a single origin of humans (or monogenism). This was challenged by physical anthropologists’ ‘polygenic’ theories in the middle of the century (with multiple different ‘races’ and origins) but returned with Charles
Darwin's monogenism at the end of the century. Later, it was hoped that blood group serology would provide answers about equally active academic debates about the origins of Aboriginal Australians.

Schneider has written that the Hirschfelds' paper had been rejected by the editors of the *British Medical Journal* because 'the subject was not likely to be of interest to doctors' (p. 557)18. Schneider has defended the editors by claiming that the results were too 'startling'—hinting that they might not have been believed. The editorial comment published with the article in the *Lancet* also expressed concerns.36 These were largely related to the techniques associated with determining blood groups—that they were only based on a qualitative rather than a quantitative assessment of agglutination. The editorial claimed:

> it is only by the use of thorough quantitative methods by researchers trained to a truly scientific outlook that rapid and sure progress in the future can be secured. Only thus will it be possible for the infant science to escape from the jungle of imperfect or erroneous observations which make it so hard to see the lamp of Truth. (p. 696)36

Whilst the qualitative methods of determining agglutination have remained, subsequent blood group research publications were shrouded increasingly in 'scientific' methodological discussions, quantification and statistics. The Hirschfelds were aware that they were opening up a whole new field of research and their tentative hypotheses needed to be tested by further research. They stated that 'close cooperation' would be necessary between anthropologists and the blood groupers and proposed that 'the researches should be conducted on an international basis.' (p. 679)34

Their first suggestion was heeded. As for the second, whilst research was international in that it rapidly began to appear in many countries it did not proceed with an international spirit. Rather, Scheider has claimed that the research questions and populations chosen reflected national politics and local racist prejudices. He has written that 'those countries most interested in racial differentiation were in the
forefront of applying blood group discoveries to physical anthropology." (p. 559)\textsuperscript{18}

This included blood group research in Germany during the 1920s and 1930s and Australian and North American research of the blood groups of their indigenous populations. \textsuperscript{f} The final sentence in the Hirschfelds' paper provided a slightly different reason for blood group research on Aboriginal Australians:

A series of important special researches, such as the examination of various stocks, primitive races, and anthropoid apes, should be begun without delay. (p. 679)\textsuperscript{34}

It is easy to see the implicit Darwinism of their juxtaposition of 'primitive races' (as Australian Aboriginal people were thought to exemplify) to 'anthropoid apes'. Tom Griffiths has eloquently summarised the implications for Aboriginal people of Darwinism and the debates about human origins.\textsuperscript{37} Darwinism seemed to emphasise human unity by describing a common single origin of all humans. But by describing some races as less evolved and ape-like, Darwinism could be used like earlier physical anthropology to reinforce the differences between races. So, whilst research into the blood groups of 'primitive races' might be used to answer questions about the origins of all humans, it might also reinforce an implicit racial hierarchy – not internationalism - with Aboriginal people near the bottom.

Pauline Mazumdar has written a fascinating history of blood group research in Nazi Germany.\textsuperscript{38} She has described the rapid growth of this research and has stated that whilst it was fuelled by and supported racist Nazi ideology, it was rarely used by the Nazi state. This may have been in part due to the complex and sometimes contradictory results of blood group research. It produced dramatic maps showing marked racial differences between Germany and its eastern neighbours. These were originally based on the Hirschfelds' biochemical index but their neat interpretation was soon confused by new incongruous data that led to increasingly complex indices based on gene frequencies. Similarly, Jews were associated with the 'non-European' blood group B although some studies showed that they were no more likely than their non-Jewish neighbours to have this blood group.

\textsuperscript{f} A search of the Native Health History Database at http://hsc.unm.edu/nhhhd/ on 28 January 2000 using the phrase 'blood group' identified 105 citations of such North American research.
Blood groups were not used by Nazi ‘race examiners’.\textsuperscript{38} Nazi propagandists and ‘race examiners’ preferred the racial criteria based on appearance, especially of the face and skull, of physical anthropology to the invisible new race markers. These traditional visible racial markers were more evocative and more flexible in their measurement and interpretation. Ann Laura Stoler has emphasised the dependence of racial discourses on ‘visual markers of distinction’ (p.133-34)\textsuperscript{39} that could stand for moral and psychological essences. The invisibility of blood groups reduced their utility to Nazi propaganda.

The irony of this German blood group research is that it was driven by racist Nazi ideology but had been facilitated by pioneering research by Jewish scientists such as Landsteiner and Hirschfeld. Landsteiner had moved to the Rockefeller Institute for Medical Research in New York in 1922\textsuperscript{32} but Ludwik Hirschfeld was not so fortunate. After the invasion and occupation of Poland, he was required to live in the Warsaw ghetto. In the ghetto, he organised a bacteriological laboratory, taught in the clandestine medical school and was involved in typhus control. Hirschfeld escaped from the ghetto in 1942 and survived the war. After the war, he continued his research career in Poland.\textsuperscript{18,40}

Whilst the role of science in the Holocaust forced the world to re-think its understanding of medical science’s approach to race, there was an increase not a decrease in blood group serological research after the war. Landsteiner’s very simple technique of assessing a person’s blood group whilst very effective in identifying ABO blood groups masked and delayed the identification of other blood group systems.\textsuperscript{30} The first were the M,N and P blood group systems identified by Landsteiner and Levine in 1927 and the second and most important were the Rh blood groups identified in 1939.\textsuperscript{30} The latter were named after Landsteiner and Wiener’s research using serum that had been sensitised by the blood of Rhesus monkeys.\textsuperscript{30} The increasing complexity of the antigens and antibodies that had been identified as part of the Rh system and the two different original nomenclature systems make this literature a little complicated and difficult to understand, like the early research of ABO blood groups.\textsuperscript{30}
An explosion of blood group research followed with the identification of many other blood group systems such as the Lutheran, Kell, Duffy and Diego systems. Louis Diamond’s brief history of this new blood group research has highlighted the significant contribution of two of Landsteiner’s pupils, Philip Levine and Alexander Wiener, and another husband and wife research team RR Race and Ruth Sanger. Diamond, who was both a contributor and a historian of blood group research (or immunoserology as he has also called it), has written of the identification of many new blood group systems:

It was immediately obvious that the greater number of heritable red cell markers would be important in identifying individuals and ethnic groups. (p.698)

Whilst these blood groups have been well used in forensic medicine to calculate how likely or unlikely blood at a crime is to be from a suspected individual, the blood groups of an individual can rarely be used to identify their ethnic group or race. The research only provided estimates of the probability that a person from a particular population would have a particular blood group. It could not definitively identify a person as from a particular race. However, such inexact language and logic led other researchers to suggest blood groups alone could define a person’s race. No doubt many of their general medical readers would not notice the error and would make the same mistake – they would already have had much more difficulty than an expert like Diamond in understanding the baffling blood group nomenclature.

Aboriginal people as ‘ultra-European’

The first article in the MJA to describe Aboriginal blood and compare it with the blood of European Australians was published in 1917 - two years before the Hirschfelds’ Lancet article. It used the Arneth index not the Hirschfelds’ ‘biochemical race-index’. In 1904, Arneth had shown that this index, which described the fragmentation of the nuclei of a person’s neutrophils (one of the types of white blood cells), was associated with infection.
Following American research in the Philippines and research at the Australian Institute of Tropical Medicine (AITM) in Townsville, EM Sweet investigated the possibility that tropical climate might increase the Arnett index. She found that school children in north and south Queensland and Melbourne all had similar results. The researchers in the Philippines had shown Filipinos had a higher Arnett index than American soldiers and this ‘may be a visible indication of a lowered resistance to infections on the part of native races.’ (p. 243) In contrast, Sweet found that eight ‘white’ and twenty ‘aboriginal’ Thursday Island school children had similar results. She dismissed any influence of either race or climate on Arnett index, concluding it was only affected by disease and fever.

Blood group research was published in the *MJA* by several researchers in the 1920s – Tebbutt and McConnel of Royal Prince Alfred Hospital in Sydney in 1922, 1923 and 1924; Douglas Lee of the AITM in Townsville in 1926; Gilbert Phillips of the University of Sydney in 1928; and John Burton Cleland and HH Woollard of the University of Adelaide in 1929. These seven publications in the *MJA* accounted for all of the Aboriginal blood group research in the 1920s cited by the Aboriginal health bibliography except for three other publications by Cleland - each in different journals in 1926, 1927, and 1929.

All of the research into Aboriginal blood group serology in the 1930s, with the exception of a single article in the *American Anthropologist*, was by Cleland and his Adelaide colleagues but they only made passing references to their blood group results in their publications in the *MJA*. Their more detailed descriptions of Aboriginal blood group serology were in journals other than the *MJA*. I discuss the research of Cleland and his colleagues in more detail later in this chapter after beginning now with a description of their early work and the work of the other three research groups in the 1920s.

The first research into the blood groups of Australian Aboriginal people was published as the first article of the February 25 issue of the *MJA* in 1922 by AH
Tebbutt and Sarah McConnel. This paper had been read at a meeting of the NSW branch of the British Medical Association in the previous year. Tebbutt and McConnel's MJA article began with a far from clear attempt to explain this new field of international research to the MJA's medical readers. Indeed all of these early blood grouping papers in the MJA were packed with complex technical detail and are difficult to read. Part of this difficulty is due to previously mentioned confusion over the nomenclature of the blood groups. Tebbutt, McConnel and Lee all used Jansky's classification and Phillips used the other now discarded classification (Moss's) based on roman numerals but they all intermittently referred to the ABO classification as well. In his series of articles about blood-grouping in different journals, Cleland used all three different classificatory systems at different times. Although these articles could be technically confusing, they were clearly about Aboriginal people — unlike the research discussed in the previous chapter. This blood group research was about providing a very 'scientific' description of Aboriginal people.

Tebbutt and McConnel were quite tentative about their results because of their concern about their small sample size — they examined 405 White Australians and 141 Aboriginal people. Whilst they provided the formula for calculating the Hirschfelds' 'biochemical index' and the relevant data, they did not provide numerical values for the two populations but did include the Hirschfelds' values for their 16 different races. The Australian indices are easy for readers to calculate. I have calculated that the biochemical index for their White Australian group was 3.2 and the index for their Aboriginal subjects was 9.2. The next MJA paper on the subject (which was a report of a paper Tebbutt had given at the Pan Pacific Congress in 1923) included these results combined with further results for both Aboriginal and non-Aboriginal Australians. This time, Tebbutt did calculate the biochemical index for Aborigines (5.7) and non-Aborigines (3.8). The tentative conclusions about

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Sarah McConnel was possibly not the first female author of an Indigenous health research article in the MJA but she was the first female author to make her gender apparent by including her first name. She did not appear again. EM Sweet who wrote the 1919 article about the Arnhem index was almost certainly Elizabeth Sweet, who had worked as a doctor for the Queensland School Medical Branch.

These authors like many others referred to the index simply as the 'biochemical index' rather than by its original name — the 'biochemical race-index'. I shall now use the shorter name.
Aboriginal people's relationship to other populations in the first paper had become more certain:

This further work, although the figures were admittedly small, enabled him to predict with some confidence that the Australian aborigines would be found to be of the European type and probably a more western type than even the English. (p. 347)

Aborigines as more European than the English or White Australians? This was surely startling news. Instead of discussing how this could be explained, the article moved immediately to the lower biochemical indices reported from China, the Philippines and New Guinea. However, the MJA had not published Tebbutt's full paper to the Congress - only an anonymously written report of his paper and the discussion that followed. In the full paper published in the congress' proceedings, Tebbutt included 'the tentative inference that the aboriginals come from the same primitive stock as the English' (p.244).

It is not clear why the anonymous reporter or the MJA editors did not include this inference. Surely it would have been of interest to the readers. An American, Rheuben Ottenberg, took this idea further in the Journal of the American Medical Association in 1925. He collated the early results from blood group research in different populations including Tebbutt and McConnell's 1922 MJA paper. He suggested, also rather tentatively, that Aborigines and 'North American Indians' with their high biochemical indices, high prevalence of blood group O and low prevalence of A and B might be classified as a 'primitive' racial type. He hypothesised that these races had separated from other humans before the origin of blood groups A and B. The presence of some of these people with blood groups A and B was explained as merely confirming that they were 'no longer a pure race' and due to the introduction of the blood of their 'invaders'. (p. 1394) Ottenberg's hypothesis was soon undermined by Landsteiner's finding of blood groups A and B in anthropoid apes and by the presence of anti-A and anti-B antibodies in the sera of Ottenberg's 'primitive' human populations.
Cleland was far less tentative than Tebbutt in his first article about Aboriginal people's blood groups. This article was published in the *Australian Journal of Experimental Biology and Medical Science* in 1926 but described research that had begun in 1922 — the same year as the publication of Tebbutt and McConnel’s first article. In that year, 16 Aboriginal people were examined at Point McLeay at the mouth of the Murray. These results together with those of two Aboriginal patients in Adelaide hospital, 28 Aboriginal people who were examined near the NE corner of South Australia in 1924, and 55 Aboriginal people examined in 1925 near the railway siding at Wilgena in the desert NE of Port Augusta were discussed in this 1926 article.

This article stands out from other early blood group research for its straight-forward prose and seemingly unguarded opinions. Cleland's frank opinions and apparent openness are often seen in his large number of publications about Aboriginal health which makes these articles particularly interesting to the historian. He has left a trail of discarded ideas uncluttered by the justifications of hindsight. In this article, Cleland remarked that his colleagues from the Rockefeller Foundation on the 1925 trip to Wilgena 'were under the impression that they could frequently foretell to what particular group an individual native would be found to belong.' (p. 34)

Clearly these eminent scientists were deluded but this does tell us of their hopes for what blood group serology would offer physical anthropology at this time.

Cleland’s results were even more distinctive than Tebbutt’s as he found only blood groups A and O in his group of 101 Aboriginal people. He emphasised the long isolation of Aboriginal people and wondered whether they had become separated from other races before there had been any intermixture with people with blood group B — remembering the Hirschfelds’ theory of separate geographical origins of blood group A and B. Ottenberg’s less subtle hypothesis of a ‘primitiveness’ and separation before the origin of A and B, had ignored the prevalence of blood group A in Aboriginal people. Cleland even wondered whether the presence of blood groups B and AB in Tebbutt’s sample was ‘explained by a recent and slight

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1 Ottenberg’s table clearly showed Australian Aboriginal people with a prevalence of blood group A just above the median for all examined populations.
intermixture with a foreign race coming from the north.' (p. 35)43 He concluded much more boldly than Tebbutt:

In any case, our results show that the Australian native differs from all other races in having a biochemical index (iso-agglutinin index), as already pointed out by Dr. Tebbutt, higher than any other, and still higher than the English, which otherwise tops the scale. According to the blood-grouping the Australian native is ultra-European. (p.35)43

Russell McGregor has written a history of this idea that Aborigines were ‘primitive progenitors of Europeans’ who had not progressed, and discussed the idea’s political implications in the early twentieth century.48 He has claimed that blood grouping results merely provided new scientific evidence for the racial affinity of Aborigines and Europeans that had been widely accepted in different forms since the 1890s. In 1905, when he was the Government Pathologist in Perth, Cleland had examined some hair samples collected from Aboriginal people by Daisy Bates.49 He explained to her that because the hair was elliptical ‘Aborigines, like the Europeans, were Caucasian and not negroid in origin.’ (p. 131)49

McGregor has claimed that the new scientific evidence from blood group research never drove either political arguments or policy and was usually used as a ‘rhetorical flourish’ to embellish existing ideas and anecdotal evidence. It was used by a variety of groups: Aboriginal activists, White humanitarians, and those who supported the biological absorption of the Aboriginal race (including Cleland). The promoters of the biological absorption of Aboriginal people into a white Australia – ‘breeding out the colour’ – claimed that the racial affinity of Aborigines and Europeans ensured a successful whitening with no ‘throw-backs’ to Aboriginal type. Whilst blood group evidence was cited, much more commonly used was anecdotal evidence about the fading of Aboriginal features. McGregor has argued that these different groups (like the MJA) did not delve very deeply into the implications of the relatedness of Aborigines and Europeans.

1 Cleland continued to act as a mentor for Bates. However, it was he who undermined her persistent claims that Aboriginal mothers often ate their children. He analysed for her the bones that she had claimed were of a cannibalised child and found them to be of a feral cat.49
'Blood-grouping fails to provide an infallible test of race'

The report of Tebbutt’s Pan Pacific Congress paper in the *MJA* finished by discussing the implications of his research for physical anthropologists. He emphasised the value of further serological research in other races because blood groups were known to be subject to Mendelian laws of inheritance. In his audience, Dr AN Burkitt from the Department of Anatomy at the University of Sydney was reported to be very enthusiastic and optimistic.

He said that Dr. Tebbutt’s paper was almost the first attempt at what might be called “Mendelian dissection” in the races of mankind. He believed that cranial, dental and hair characters would ultimately allow of similar treatment. Almost the only other normal trait shown to be Mendelian in character was colour of the iris. (p.347)

Burkitt had given the paper before Tebbutt’s in the Anthropology and Ethnology Section of the congress. He had described the features of Aboriginal people’s skulls and brains. Similarly to his later response to Tebbutt’s work he had said that ‘the ideal would probably be attained when the factors concerned in shaping the skull were dissected out as Mendelian characters.’ (p.346) In his earlier paper in the *MJA*, Tebbutt had been uncertain but hoped that the biochemical index calculated from blood groups would be as useful to anthropologists as the cephalic index estimated by the measurement of skulls—or craniometry.

Stephen Jay Gould has made several conclusions about craniometry in his 1981 book, *The mismeasure of man*, that may be relevant to the related field of blood group serology. He has emphasised the scientific credibility gained by the craniologists’ attachment to quantification with their meticulous and repeated measurements, complex statistics and graphs. Not only did the biochemical index perform a similar function to the craniologists’ cephalic index but I have already described blood groupers’ detailed attention to describing measurements and techniques. There were also numerous tables reporting cross agglutination
Gould has found that the craniologists' prior prejudices moved via their numbers back to the same prejudices. He has argued that this more often occurred not by 'fudging' and fraud but by more subtle means. Probably unconsciously, craniologists were mainly selecting those measurements that best fit their *a priori* racial hierarchies. The blood groupers were also selective about which data or subjects to include. The most common method of excluding subjects was to label them as not 'full-blooded' or 'pure-blooded'. This exclusion of Aboriginal people of mixed descent can be easily explained by the researchers' research questions about blood groups as a clearly inherited racial characteristic. It probably also reflected and possibly contributed to growing political awareness and concerns by the State and the White Australian population (including these scientists) about the rising numbers of these Aboriginal people – the 'half-caste problem' – at the same time as concern about the dying out of the ('full-blooded') 'Aboriginal race'. The use of these inclusion and exclusion criteria demonstrate a slight shift to clearly racial research in Australia in contrast to the Hirschfelds' conflation of and slippage between race, nationality, ethnicity and geographical location.

Tebbutt narrowed his definition of his subject population from his first paper with McConnel in which they included 'only full bloods and crosses with Europeans' (p. 203)\(^1\) In his second *MJA* paper, he only included ‘full-blooded aborigines’.\(^2\) All the subsequent *MJA* blood group research in the 1920s excluded all but ‘pure-blooded’ Aboriginal people.\(^4-7\) Lee and Phillips described their different methods of assessing their subjects’ 'pure-bloodedness' and their exclusion criteria in some detail. Unlike some craniometry, there is no evidence that any 'atypical' blood group data was excluded from the final results with the subject then labeled as being of mixed descent. It is not possible to say whether borderline reactions would have been more likely to have been described as blood group O or A than B or AB in keeping with prevailing theories than if the researchers had been blinded to the subjects' race.

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1. Lee and Phillips described their different methods of assessing their subjects' 'pure-bloodedness' and their exclusion criteria in some detail. Unlike some craniometry, there is no evidence that any 'atypical' blood group data was excluded from the final results with the subject then labeled as being of mixed descent. It is not possible to say whether borderline reactions would have been more likely to have been described as blood group O or A than B or AB in keeping with prevailing theories than if the researchers had been blinded to the subjects' race.
In response to Gould’s work, Richard Glover has looked at Australian physical anthropology in the period before blood group serology. He has agreed with Gould and emphasised the resilience of racial hierarchies and the ‘ingenuity’ of the anthropologists when faced with results that contradicted accepted ideas. They responded by questioning their data, their selection of subjects or variables to measure – not the usefulness of their theories of race. The response by researchers to inconsistencies in their blood group results was similar to that of earlier physical anthropologists. Cleland cast doubt on the validity of Tebbutt’s finding of blood groups B and AB, which threatened Cleland’s hypothesis about the absence of blood group B in Australia, by describing Tebbutt’s research subjects as only ‘presumably full-blood Australian natives’ (p. 35) (my emphasis).

The blood group researchers who followed Tebbutt struggled valiantly for some years to look in directions other than the limitations of race as a biological entity. In so doing, they help us to see how important the reification of race was to these scientists. Lee found a lower biochemical index amongst his Aboriginal subjects from Palm Island in his research than Tebbutt had found. At first, he suggested this may have been due to his less meticulous serological technique caused by the remoteness of Palm Island. Then, he suggested that the difference may be explained by the more northern location of his subjects. But he avoided making the obvious step of saying that the differences between (rather than within) populations could also be explained by location without any need for discussion of race. Lee’s decision not to undermine the theoretical construct called race also may have been assisted by scientific conservatism (or at least modesty) that would lead him and other researchers to prefer to modify a well accepted idea rather than to demolish it.

Two years later, Phillips dealt with the same problem of a large variation in blood groups within the ‘Aboriginal race’ in the combined results of his and earlier researchers. He wrote that this variation could be easily explained by the social organisation of Aboriginal people in ‘tribes’. With the sentence that follows, he seems to be discarding the biological utility of conceiving of Aboriginal people as a race but in spite of his own conclusion he still continued to use the concept in the rest of his paper.
This being the case, an estimation of the percentage distribution of the blood groups among the whole aboriginal population of Australia can be of very limited value only, for the reason that although the aboriginal race had been isolated from other peoples for many centuries before the advent of the white man to Australia, nevertheless the race consisted of tribes each of which possessed a characteristic blood group distribution which differed very greatly in widely separated parts of Australia. (p. 299)6

Phillips also noted that the testing of Aboriginal blood against White sera of known ABO blood group would only determine the prevalence of the four known blood groups. If Aboriginal people had quite different blood groups they would not be detected by these standard procedures. He recorded the agglutination reactions of the sera and red blood cells of all combinations of 12 Aboriginal people. There were unexpected reactions between the serum of one person and the cells of two others and he concluded:

The blood of certain full-blood Australian aborigines contains agglutinin-agglutinogen systems which may not be recognized by testing the reactions for their corpuscles against Group II and Group III sera obtained from white people. (p. 434)5

Earlier in this chapter, I described the discovery of new blood group systems starting with the MN system at about the same time as Phillips published this research. What is interesting is that Phillips chose not this more simple and general explanation but his more complex explanation that emphasised the importance of race and racial differences and confined his discovered limitations of the ABO classification system to the Aboriginal population. It may have been more than just epidemiological reticence about making inappropriate generalisations from a small sample, although this might have initially appeared a reasonable cautiousness - neither Phillips later in 19286 nor Cleland and Woollard in 19297 found any such atypical cross-reactions in two other Aboriginal populations.
In later research (that was published in journals other than the MJA), Cleland, like Phillips, found anomalous results of cross-reactions for some combinations of Aboriginal people’s blood cells and sera. He described these as due to either aging of the red blood cells, weakness of the agglutinins, blood sub groups or blood group systems other than the ABO system. But having devoted most of their 1929 MJA article to a description of their search for possible atypical cross-reactions following Phillips’ paper, Cleland and Woollard chose to conclude very strongly:

> By extensive cross-testing between aboriginal red cells and aboriginal sera no evidence was obtained that aboriginal blood contains factors not found in the blood of Europeans. (p. 7)

Later in the same year in Man, the journal of the Royal Anthropological Institute of Great Britain and Ireland, the same two authors confronted the implications of the contradictions and inconsistencies in the racial explanations of blood group serology. Like Matthew Young’s articles in the same journal a year before, they noted that very different populations or races had been found to have similar blood group prevalences and very similar populations had very different blood groups. These three articles in Man emphasised the inconsistencies of blood group results with accepted racial groupings and types. This did not lead to any questioning of the accepted racial groupings (although Young came close) or of the notion of biological race but it did dramatically change the interpretation and implications of blood group research. Woollard and Cleland firmly stated that ‘blood-grouping fails to provide an infallible test of race.’ (p. 184)

> Thus, because the rather pathetic sub-man of Australia has a high frequency of the A factor, we need not suppose that it implies a close affinity with the nordic superman who also possesses a high frequency of A. (p. 186)
They did not propose discarding entirely blood group as a racial characteristic but they proposed that it be treated as one among many such as skin colour, hair or skull shape. They proposed ‘inverting’ current research questions about race and blood groups. Rather than investigating blood groups ‘to stamp each variety of man’ (p. 186) they suggested studying different populations to understand how they came to have their current blood group prevalences.

Of most interest to Woollard and Cleland was the question of why Australian Aboriginal people they tested had only blood groups O and A. They hypothesised that this was due to the long isolation and racial ‘purity’ of a population descended from a very small number of immigrants with only blood group O and A. The presence of blood group B in the north of Australia was explained as due to the mixture with other populations and blood group B in other parts of the country was dismissed as from ‘persons who were known to be of impure blood’ (p. 187). They proposed that blood group research continue with blood groups still very useful, albeit for different questions. Most importantly their notions of race remained unthreatened.

They and their colleagues in Adelaide were the only Australian researchers to continue this work in the 1930s. By 1939, they had tested the blood groups of 842 ‘full-blood’ Aboriginal people in Central and South Australia. All of these people had only blood groups O and A, the results of one man with blood group B were dismissed as he was found after ‘discreet inquiry’ to be a ‘half-caste’ (p. 105).

**Vacation expeditions to Central Australia**

John Burton Cleland was either the only author or the first author (rarely a second or lesser author) of almost all of the Adelaide blood group research involving Aboriginal people before 1940. He published extensively about many aspects of this research. I now briefly discuss this work and the ‘expeditions’, as he and others called them, to Central Australia where this work was done.
Cleland’s career was not confined to Aboriginal health or indeed to medicine. He was also a distinguished naturalist with several plants, animals and a national park near Adelaide bearing his name. In particular, he was a known authority on Australian fungi having amassed a large collection and written a definitive taxonomic text. He was born in 1878 and grew up in Adelaide and commenced his medical studies there and completed them in Sydney, London and Glasgow. He worked as a government microbiologist and pathologist in Perth and then Sydney before returning to Adelaide in 1920 to become the first Marks Professor of Pathology (and Microbiology). In Sydney, he contributed to ground-breaking research on dengue fever and Murray Valley encephalitis (then known only as ‘Australian X disease’). In Adelaide he also became a consulting pathologist at three hospitals where he performed more than 7,000 autopsies – which he apparently performed with great enthusiasm and diagnostic skill. He remained at the University of Adelaide until he retired in 1948 when aged 70 but was still fit and professionally active until his late eighties when he went blind. He died at the age of 93 in 1971.63,64

He wrote and published prodigiously throughout his career and across all of his scholarly activities from mushroom collecting to post-mortems. Whilst living in Sydney at the start of his career he edited the Australasian Medical Gazette in 1910.64 The Aboriginal health bibliography has included 78 citations for which he was either the only or first author with the first in 1909 and the last 60 years later in 1969 when he was 91 years old.25 His dominance of Aboriginal health research was most marked in the 1920s and 1930s.

His father, William Lennox Cleland, was the resident medical superintendent of the Parkside Lunatic Asylum in Adelaide and wrote occasional articles about Aboriginal people.64 The Clelands lived at the Asylum which had some Aboriginal inmates. His colleague Thomas Draper Campbell has said that this early involvement with this very particular group of Aboriginal people combined with his interest in ‘natural history’ and the ‘natural history of man’ contributed to his later involvement in Aboriginal health research.63 Indeed, in 1925, when Cleland coordinated the whirlwind Adelaide visit of two representatives of the Rockefeller Foundation to...
demonstrate the opportunities for anthropological research at the University of Adelaide, he arranged for them to visit three Aboriginal inmates of the Asylum on their second day. The next morning they set off by rail to Wilgena siding where they observed, photographed and determined the blood groups of local Aboriginal people. Cleland described the blood group results in his first article about Aboriginal people’s blood group serology.43

Philip Jones, who has written the most detailed histories of Adelaide-based anthropology65-67, has claimed that the visit had been arranged to convince the Rockefeller Foundation to choose the University of Adelaide over Sydney as the site of the first Australian professorial chair in anthropology for which the Foundation would provide most of the funding.65 But it seems that the decision to select Sydney as the site of the chair had probably already been made before the visitors had arrived in Adelaide.50,65 Before this time most anthropological research had been self-financed by amateurs but from 1926 until the Second World War the Rockefeller Foundation provided (through the Australian National Research Council (ANRC)) almost all the funds for anthropological research in Australia and New Guinea.68

The University of Sydney was to have the only Australian chair of Anthropology until after the war - filled first by AR Radcliffe-Brown and then AP Elkin. The functionalist social anthropologists at Sydney also soon controlled and dominated the ANRC and its Rockefeller funds for research leaving the Adelaide physical anthropologists increasingly feeling that they were marginalised and with limited funding.65 Jones has suggested that the emphasis of physical over social anthropology in Adelaide reflected a long history of a predominance of ‘medical men’ (especially anatomists and pathologists) in Adelaide anthropology.65 He also has written that the very large collection of skeletal material at the South Australian Museum following the Swanport excavations in 1911 encouraged and attracted those interested in physical anthropology.66,67

\[k\] Campbell wrote his doctoral thesis describing skulls from this collection.66
The Adelaide researchers were so encouraged by the success of the trip to Wilgena and the hope of further support from the Rockefeller Foundation that they had already begun plans for further research and a coordinating Board for Anthropological Research before they heard of the decision in favour of Sydney University. Undaunted, the Board organised research expeditions to various parts of Central Australia every year (except in 1938) after the Wilgena trip until 1939. The routes of these 14 expeditions are shown in figure 5.2. Cleland was a member of 11 of these expeditions.

In spite of the Board members’ feeling of being overlooked by the Sydney dominated ANRC and the small proportion of ANRC funds that went to Adelaide, Nicolas Peterson has pointed out that the ANRC funds still covered more than 75% (£2615 of £3403) of the costs of the expeditions between 1927 and 1935. This was sufficient only because Cleland supplemented this shoestring funding of the expeditions with rail concessions and donations of fuel and supplies from private industry. Cleland prominently acknowledged ANRC funding in his MJA articles based on these expeditions. He also explained to the MJA readers his reasons for publishing these articles in a medical journal for a medical audience:

Most aspects of anthropology have a very distinct medical bearing. Especially is this the case with physical anthropology. The recent expedition undertaken by the Board of Anthropological Research of the University of Adelaide in conjunction with the South Australian Museum, in which much data on physical anthropology and physiological observations were obtained, is therefore of definite interest to medical practitioners. (p. 793)

Throughout the first four decades of the MJA, Cleland’s were not the only MJA Aboriginal health publications about anthropology. Most of these other publications appeared as either short reports or versions of papers given at scientific or medical meetings or as book reviews and many, especially the book reviews, emphasised the relevance of anthropology to the medical readership of the MJA. The first of these and the first Aboriginal health publications in the first volume of the MJA were the reports of papers from the Anthropology Section of the 1914
Figure 5.2. Map of the inter-war expeditions by the Board for Anthropological Research

British Association for the Advancement of Science meeting in Melbourne. Coincidentally, this meeting marked the peak of interest in Australia and Australian Aboriginal people by English anthropologists and anthropological theory with all the key players in this crucial stage of the development of English anthropology travelling to Australia for the meeting.¹

Not only did Aboriginal people, or at least anthropologists’ ideas about them, play an important role in anthropological theoretical debates. The *MJA*’s readers were often told that anthropology should have a central role in the administration of Aboriginal lives. The report of Frederic Wood Jones’s address to the Australasian Association for the Advancement of Science meeting in Perth in 1926 was typical. Wood Jones was Professor of Anatomy at Adelaide University from 1919 to 1926, and was a long-lasting influence on the approach to anthropological research at Adelaide (and much later on Barry Christophers who is discussed in chapter eight).⁶⁵ At the Perth meeting he was reported to have said that:

> the establishment of a chair of anthropology at the University of Sydney meant the dawn of a new era in which enlightened methods would prevail in preserving a backward race, insuring a continuance of their culture and traditions. (p. 667)⁸⁰

The point of discussing anthropology in this chapter is more than just the well-accepted history of its entanglement with colonialism. It is because a large part of the early Aboriginal health research, and Cleland’s work in particular, had a distinctively anthropological flavour. Whilst I believe she slightly overstated her case, Lindsey Harrison has made the following assertion that means that my detailed investigation of Cleland’s ‘anthropological’ publications in the *MJA* (and their companion pieces in specialist medical journals) is warranted in order to understand Aboriginal health research at this time.

¹ Those attending from England included Elliot Smith, Haddon, Malinowski, Marett, Rivers and Radcliffe-Brown.
During this period, Aborigines were largely seen as objects of anthropological interest by the medical profession and, in the MJA, the field was left to those doctors with an interest in the subject. (p. 40-41)  

The Board for Anthropological Research's expeditions were usually short, busy affairs of two to three weeks during the mid-year university break involving an average of eight researchers. (See figure 5.3) Just as the Board members had their own keys and treated the South Australian Museum like a gentlemen’s club, these were scientific expeditions for men only. The anthropologist Olive Pink had been working nearby and assisted in the recruitment of Aboriginal people for the 1936 expedition to the Granites. Cleland had acted as her professional mentor and patron in anthropological circles but her request to join a later expedition was refused. Julie Marcus has noted that Cleland’s wife told Pink that ‘men on the expedition found women a nuisance and that there was absolutely no question of her going with them.’ (p. 191)  

A typical example of these expeditions was the one to Cockatoo Creek from the 6th to the 28th August in 1931. All Aboriginal research subjects were allocated a number which was painted on their shoulder or buttock. A cardboard number was also hung around their neck or attached to some clothing. Then came the cattle race of researchers. Norman Tindale first collected some basic genealogical and personal information. Then the dentist Thomas Draper Campbell and H Gray (a medical student) made detailed series of anthropometric measurements including measurements or descriptions of skin tint, eyes, teeth, hair, nose, spine, feet and palms. Any abnormalities or pathological lesions were noted in passing. Then Professor Cleland and Professor Harvey Johnston did blood grouping tests. Dr Robert Pulleine and Dr HK Fry performed a number of neurological examinations of strength, pain, visual acuity, colour blindness, hearing, and taste. Professor Colin Stanton Hicks and Dr RF Matters conducted a series of involved physiological testing of some people including tests to calculate basal metabolic rate, surface area

—Frank Fenner, one of Australia's most distinguished medical scientists, also accompanied two of the Board's expeditions as a medical student and later wrote his MD thesis about Aboriginal people's skulls. Raphael Cilento was also involved with Museum during the earlier Wood Jones era.
Figure 5.3. Members of 1932 expedition en route

(requiring the application of gum paper to the entire body surface), and the absorption of sodium chloride by the subcutaneous tissues.

Stanton Hicks’s physiological experiments provide the most evocative images of the expeditions — with his large and cumbersome boxes of measuring devices attached by wires and tubes to naked ‘primitive’ Aboriginal men (see figures 5.4 and 5.5). The photograph of physiological experiments to determine the cold tolerance and basal metabolic rate of Aboriginal people in figure 5.5 was reproduced in a few different articles. Stanton Hicks and his colleagues chose to publish this physiological research in journals other than the MJA, usually preferring the Adelaide-based Australian Journal of Experimental Biology and Medical Science. During the 1937 expedition, Stanton Hicks compared the physiological responses to the cold temperatures of the desert of four White researchers to eight Aboriginal men. Or as he later wrote: ‘We would lie alongside our paleolithic friends and simultaneously study the skin temperature and vascular responses of both white and black subjects.’ (p. 48) In this extraordinary experiment the naked men lay in tents rather than in the open and the published article included no photographs.

At Cockatoo Creek plaster casts were taken of some faces, chests, feet, hands and some pathological lesions by Tindale and the director of the South Australian Museum, Herbert Hale. Not only were photographs taken of the research subjects and their pathological lesions but they were also taken of scenes of Aboriginal social life. As well as these photographs, some 35 and 16 mm film was taken by Professor Wilkinson and EO Stocker. Tindale also made general observations about social structure, linguistics, place names and Cleland recorded information about plant and animal names and usages. Cleland concluded his description of this research trip in the MJA with the possibly justifiable remark:

Probably on no previous occasion has so much work on physical anthropology been carried out on the Australian aborigine in so short a time. (p. 796)

These expeditions were a world away from the emerging Malinowskian model of anthropology favoured by the Sydney social anthropologists with its prolonged
Figure 5.4. Naked amongst the boxes and wires of physiological experiments

Figure 5.5. Measuring cold tolerance and basal metabolic rate

Figure 5.6. More measuring

fieldwork by a single researcher or a very small research team. But there are some obvious similarities to some contemporary and present-day health research and health screening activities (like the hookworm campaigns discussed in chapter four). In particular, I remember participating in 1990 in a week-long health screening and research project in a small Central Australian Aboriginal community looking at diabetes and other 'lifestyle' diseases. This research involved a similar cattle race of several health professionals making a series of anthropometric and clinical measurements and then taking blood samples to make several biochemical and haematological measurements. This intense activity involved most of the adult population of this small Aboriginal community and lasted only a few days. The Board had been there on one of its expeditions in the 1930s.

'The trouble with all this collecting business'

The MJA published reports of the Board's 1931, 1932 and 1933 expeditions. These were presented to the reader not as typical research articles. Although some results and interpretations were included, each article was primarily a story of the research expedition itself. It was as if the collecting of the data and how that was done was more important than what they had found. Jones has even used the term 'data-oriented anthropology' to describe the Board's distinct approach. The impetus for this collection of information was clearly the perceived imminent demise of Aboriginal people as a result of contact with 'civilisation'. Cleland began his first article about Aboriginal people's blood groups with the sentence: 'The

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8 Whilst our activities appear very similar to the Board's, the power relationship between researchers and Aboriginal people were quite different. Our research was part of an evaluation of a health promotion program run by the local Aboriginal community controlled health service and all researchers were either currently employed by the health service or had been in the past.

9 In The road to Botany Bay, Paul Carter has described how some of the names of places given by 'explorers' such as James Cook were more about the history of the explorer and their traveling than a description of the place being named and so began to provide a 'spatial' history of Australia. Carter has favorably compared Cook's particular and historical approach with Joseph Banks' generalising taxonomic approach. Cleland's published descriptions of his expeditions provide information about the histories of his journeying in Indigenous health research not just information about Indigenous people's health, although this chapter also demonstrates the pervasiveness of his very taxonomic approach to his observations.
Australian aboriginal is rapidly dying out.' (p. 33) He then asserted that 'it was advisable to make a blood-grouping of the aboriginals, to place on record, before they disappeared, the relative proportions of the different groups met with in as large a number of individuals as possible.' (p. 33) In 1938, Cleland's close colleague at the Board, the dentist TD Campbell even devised a mathematical formula that predicted the rate of the disappearance of the Aboriginal race following European contact.

Tom Griffiths has investigated the collecting of Aboriginal people's skulls and stone tools by amateur historians in Victoria (and the relationships between them and both academic historians and Aboriginal people) in his book *Hunters and collectors.* He has mentioned Campbell a couple of times in these discussions of this Victorian group of amateur collectors. The linking of the Board's expeditions to these amateur collectors seems particularly appropriate. I am not using the label 'amateur' in its pejorative sense implying sloppiness or lack of skill - the meticulous recording of their many observations is not questioned. Most were amateurs in that they conducted their work during the university vacation and away from their main work in the medical faculty unlike the full-time anthropologists from Sydney. And their work was mainly collection - albeit the collection mainly of data although many specimens of material culture were also collected for the South Australian Museum.

The usual emphasis of collecting is on rarity, quantity, and taxonomy. The seemingly inevitable doom of the Aboriginal race added not only rarity but urgency to the Board's collecting of measurements of these Aboriginal people. The size of an amassed 'collection' reflects not only the range of different objects collected but the number of examples of each type of specimen. Continued repetition of the same experiment with the same results might eventually seem of little utility to a scientist but to a collector repetition is often the point of a collection. Ten examples of a stamp, model car or football card do not preclude a collector planning to collect another ten, twenty or a hundred. These later examples might be used to trade for items absent from the collection just as the South Australian Museum traded Aboriginal artefacts with other museums for preferred natural history items in the nineteenth century.
Cleland kept testing the ABO blood groups on all expeditions even though he always only found people with blood group A or O. He did not move onto a new research question. I am unconvinced that he was simply adopting a classic Popperian approach of testing his hypothesis by attempting to falsify it by looking for people with blood group B. He was quickly dismissive of results which showed Aboriginal people with this blood group.\textsuperscript{43}\textsuperscript{62}

Norman Tindale worked with Cleland on the Board’s expeditions from 1928 to 1935 and his work on linguistics, genealogies and social life provided some counterbalance to other researchers’ physical anthropology. Jones has typified Tindale’s approach as ‘salvage ethnography’\textsuperscript{67}; the approach of the entire Board could be similarly described. This credo together with Tindale’s role as a museum rather than a university anthropologist led to a rationale for his ‘collecting’ strategy. Jones has written that theory could wait:

This attention towards a wide range of data, including objects, literary texts, visual records and vocabularies, underpinned a great deal of salvage ethnography undertaken by Tindale and members of the Board for Anthropological Research. It accounts for the eclecticism of their field activities and the sheer bulk of data and objects which they collected. Further, it reveals an attitude founded on the primacy of data before theory. (p. 369)\textsuperscript{67}

Campbell wrote in 1950 that ‘The trouble with all this collecting business is to settle down with the results.’ \textsuperscript{p} But that was not the point of ‘this collecting business’. The huge amount of information and specimens in a ‘collection’ are rarely analysed to answer specific questions, although in order for the collection to be admired for its rarity and quantity, it must first be classified and collated. Griffiths has noted some odd juxtapositions in the collation and presentation of Victorian ‘local history’ collections. This need not imply inconsistency in the taxonomy and collation of the collection - a record collection collated alphabetically might place Schubert and the

\textsuperscript{p} Cited by Griffiths (1996) (p. 74).\textsuperscript{37} Campbell to Mitchell, 19 February 1950, SR Mitchell papers, AIATSIS.
Sex Pistols next to each other. Cleland usually only reported the number of each type of measurement made during each expedition, some very basic results were presented then or later, and perhaps these results were then added to those collected on previous expeditions. This was the way most of the blood group research in the 1930s was presented. The emphasis was on recording and cataloguing information ‘before it was too late’. In 1923, Cleland wrote to the editor of the *MJA* requesting information about diseases in Aboriginal people:

Sir: The Australian aboriginal is rapidly dying out. Very few records of the course of disease and of pathological changes in this race have hitherto been recorded. The reactions that occur are of considerable interest on account of the long isolation of these people from the rest of mankind. …

For some years I have been collecting records of the occurrence of disease among them. … I am especially keen to obtain any records of individual cases of disease. … Any data supplied will be suitably acknowledged.¹⁸

He collected lists of apparent disease and pathological lesions encountered on the expeditions. He included a brief description of all of these lesions found on the 1929 expedition to Hermannsburg in the first part of an *MJA* article about that expedition.¹ This list served only as an addition to his collection of diseases occurring in Aboriginal people. It was not an indication of what diseases were important in Hermannsburg. Indeed, also included was a photograph of an achondroplastic dwarf taken by an Alice Springs man in Newcastle Waters, which is more than 700 km north of Hermannsburg. The photo added nothing to the description of Hermannsburg but it did increase the size of his collection. An even more bizarre inclusion was the last paragraph in this section describing ‘pathological lesions’. The paragraph’s subheading is ‘Test to see if an enemy is dead’ and the paragraph briefly describes testing anal tone with a spear. The most plausible explanation for Cleland’s decision to include this in a series of disease case reports was his overwhelming desire to tell this tale – relevant or not.

Descriptions of Aboriginal people also appeared in other disease collections that Cleland collated and published – the descriptions of each series of 1,000 post-
mortems that he performed$^{25,64}$ and his catalogues of diseases caused by animals$^{99-102}$ and insects$^{103,104}$. In the catalogues of diseases caused by animals, reports of injuries caused by Aboriginal people were included but not those by non-Aboriginal people. In the previous century, ethnology had been included in the zoology section of British Association for the Advancement of Science conferences.$^{35}$ The South Australian Museum, which was closely involved with the Board and its expeditions and had had a crucial role in the development of anthropology in South Australia, had three main collections: zoology, mineralogy and anthropology. Of this juxtaposition the museum’s Peter Sutton wrote in 1986:

> If anthropology is a universal comparative study, why are its museum collections restricted to colonised indigenous peoples? How do Aboriginal people react, for example, to being located among native fauna and minerals? The last is the easiest to answer: they find it offensive. (p. 48)$^{105}$

This discomfort was either ignored or did not seem so obvious to those associated with the museum and the Board for Anthropological Research 60 years earlier. Sutton then went further than just the museum’s display of its collection of Aboriginal objects and has suggested that the museum and Board’s researchers adopted a ‘natural history approach’ to their anthropological research with Aboriginal people.$^{105}$ The links and similarities between natural history and anthropology at the museum and Board for Anthropological Research were investigated in much more detail by another museum employee, Philip Jones, in his 1996 PhD thesis *A box of native things*.$^{67}$ This thesis has described a history of the acquisition and display of the museum’s ethnographic collection and its relationship to the colonial encounter in Australia.

Jones has claimed that natural history not ethnography was the central concern of the museum and its directors in the nineteenth century and that this earlier natural history focus influenced the methods of acquisition, classification and exhibition of the ethnographic collection.$^{67}$ It was not just the museum that was concerned with natural history first and ethnography second but many of the anthropologists associated with the museum (and the Board) were identified primarily or originally
as naturalists. Tindale began his career as an entomologist and many of the members of the Board were not only trained in the medical sciences but were also naturalists, continuing the centuries long links between medicine and botany. Indeed, RV Southcott wrote in the *Australian Dictionary of Biography* that ‘Cleland’s botanical studies were probably more important’ than his work with Aboriginal people.

This brief description of the ‘collecting’ by Cleland and the Board for Anthropological Research in Adelaide as ‘data-oriented anthropology’, a ‘natural history approach’ and ‘salvage ethnography’ makes their representation of Aboriginal people as passive specimens in the *MJA* more understandable. This is not meant to discount the discomfort and anger felt by Indigenous people about such representations. Neither am I suggesting that Cleland deliberately planned to offend Indigenous readers. He probably would never have imagined an Indigenous readership.

**Submitting to White power?**

Whilst Cleland did not discuss his representations of Aboriginal people directly, he did discuss frequently in the *MJA* and other publications his relationship with Aboriginal people as research subjects and his perceptions of their responses to his research. These descriptions can be read to add to our fragmentary understanding of the encounter on the colonial frontier and the encounter between medical science and Indigenous Australians.

In their 1929 article in the *MJA*, Cleland and Woollard described their perceptions of the response to their blood grouping by their Aboriginal research subjects. What is clear from this description is that this must have been one of the most bizarre encounters on the colonial frontier. A small group of White men mixing the blood of two of their own group with that of the Aboriginal people they met – and both

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6 Cleland usually used his own blood to provide the reference serum and red blood cells for blood group A.
sides expecting and finding that their blood (with all its social meaning) was very similar. They wrote:

It is of interest to note that not the slightest difficulty was experienced in inducing the natives to submit to the blood test. It was explained to them that we were anxious to see whether their blood resembled more that of the white man than did the blood of a Chinaman or negro. It is perhaps amusing that they should have agreed with us in thinking this would be the case. The mixing of a little of their blood with some of our own was watched with considerable interest by them. (p.3)

A simple reading of Cleland's many other accounts of his research encounters with Aboriginal people is to see the more powerful White researchers requiring their Aboriginal research subjects to perform and submit to a variety of often unpleasant tasks that were possibly de-humanising or at least did not acknowledge their individual identities. The White researchers then totally controlled the representations of this process and portrayed these Aboriginal people as mere rare 'specimens' doomed to extinction. It could be argued that this is the way medical science treats all human subjects but the similarity of these power relationships and representations to other encounters between Indigenous and non-Indigenous people might suggest a greater significance in this colonial context. Even Cleland wrote that his relationship with Aboriginal people as research subjects was quite different than with other medical research subjects:

There is probably no other race in the world which would show such a singular spirit of co-operation with investigators, carrying out complex tests with bulky apparatus. Why they are so ready to assist it is hard to ascertain. (p. 49)

In publication after publication he described Aboriginal people 'submitting' to unpleasant testing, with no suspicion, no opposition and good humour. His repeated use of the verb 'to submit' was indicative of the Aboriginal passivity and the power of the researchers over the researched that he portrayed. He acknowledged that the promotion of the research and researchers by government and mission authorities may have influenced Aboriginal people's decision to cooperate. But again this portrayed Aboriginal people as passive. This representation of Aboriginal
participation in his research as caused by the power of some non-Aboriginal people over Aboriginal lives was expanded to include the power of all White people over Aborigines. Cleland wrote in the MJA in 1931:

It is possible that white people in general, or ourselves in particular, were looked upon as being a very superior race. Consequently, if we said this or that was to be done, they probably believed that it was no good arguing or protesting in any way; the thing had to be done and they submitted accordingly. (p. 794)¹⁰

Whilst the representation of Aboriginal people would have been unambiguous to the contemporary medical readers of the MJA and other journals the actual relationship of Aboriginal people to the visiting researchers is less clear. There is some evidence of Aboriginal choice and resistance. There is also evidence that Cleland was sensitive to this.⁶⁵ The representations in the texts rather than the events they describe are the focus of this thesis but even the texts alone allow us to see the possibility of an alternative representation. Some caution is needed as there is no written account from the Aboriginal side of this encounter.

Jones has written about the mutual pragmatism, accommodation and invention (albeit against a backdrop of unequal power) rather than just European exploitation in the acquisition of Aboriginal artefacts by the South Australian museum. Might there have been a similar willingness of Aboriginal people to ‘trade’ their participation in medical/anthropological research? Cleland mentioned the standard items of trade given for participation in his research: food, sweets, tobacco, pipes and red handkerchiefs.¹¹,¹²,⁴³,⁹₀,⁹₃ He did not seem to believe that these items, the medical and dental care provided by team members or the undoubted ‘novelty’ of the research activities could possibly account for Aboriginal participation.⁹₃

In his discussion of gift exchange on the colonial frontier in the Pacific, Nicholas Thomas has warned that indigenous peoples were not irresistibly attracted to Western commodities. He has emphasised the ambiguous and contingent meanings of traded items on both sides of the exchange.¹⁰⁹ It is not germane to my discussion of Cleland’s representations of the exchanges associated with his research to attempt
a similarly nuanced analysis. However, it is probably reasonable to suggest that the traded items were perceived by Aboriginal people as adequate exchange but only for cooperation for a short time. Cleland often stated that Aboriginal participation could only be expected for a short time after which they began to leave. Cleland wrote that this supported the Board's short trips and described this as the racial characteristic of going 'walkabout' rather than as choice or active resistance to his research. As if to prove this more active role and considered assessment by Aboriginal people, the 1933 expedition at times had problems with participation because the researchers said in their report to MJA readers that a "dogger", who was competing for their services, rendered our endeavours futile' (p. 853). 12.

Christine Cheater has claimed in her PhD thesis Argonauts of the primitive world that 'an examination of the Board’s data indicates that only men and older women were tested.' (p. 137) She has asserted that the board’s expeditions resembled an Aboriginal men's hunting trip and that 'Aborigines apparently restricted the Board's access to mature members of the community'. (p. 137) This is contradicted by the presence of the results of women in their teens and twenties and of a few young children in the articles about the blood group research on the 1930s expeditions published in the Journal of Tropical Medicine and Hygiene. But Cleland did admit to halting the blood testing of children during an earlier expedition after they became frightened. 44

Cleland also described another instance of Aboriginal people refusing to participate in spite of the researchers' 'utmost endeavours' (p. 412). The parents of a child who had died in the scurvy outbreak during the expedition to Hermannsburg refused permission for an autopsy. This shows both the refusal of Aboriginal people (in contrast with their decisions to participate in other parts of the research) and that the researchers' power was not total – they could not insist and nor did they disregard the parents' wishes. 4 The researchers were also sensitive and adapted to other

1 Readers of the MJA, and initially I, may have thought they did disregard or not ask for the parents' consent for an autopsy when an infant died on an expedition four years later and they reported that the body 'which had been roughly buried by its mother, was secured and preserved for subsequent study.' (p. 324) Had they resorted to grave-robbing to avoid denied consent? Probably not. In an article about the same expedition in Science, Cleland described unearthing the body with 'the
requirements or requests of Aboriginal people. In summary, not only were Aboriginal people portrayed differently from other research subjects but there is some evidence that this portrayal as powerless passive specimens was not the only way these encounters could be represented.

**Pessimism and protection**

Cleland was not only the most prominent Aboriginal health researcher in the first half of the twentieth century but also contributed to government policy and the administration of Aboriginal people’s lives. He was the deputy chair of the South Australian Aborigines Protection Board for many years. However, his policy influence was considerably less than that of either AP Elkin, the Professor of Anthropology at Sydney University from 1933 to 1956, or Cecil Cook, the Chief Medical Officer and Protector of Aborigines in the Northern Territory from 1927 to 1939. Cook with his large influence on the administration of Aboriginal lives in the Northern Territory combined with his interest in public health and leprosy might have been expected to have featured even more than the Adelaide pathologist Cleland in Aboriginal health articles in the *MJA* in this period. The Aboriginal health bibliography has only cited two *MJA* articles by Cook – one in 1949 and the other in 1966.

Cleland supported the protection and segregation of Aboriginal people who had had limited contact with Europeans and the ‘biological’ assimilation or absorption of those already in contact with White Australia (particularly Aboriginal people of mixed descent). Whilst his blood group results were used to support biological absorption, he referred to the policies of protection and segregation much more frequently in his research publications. This probably reflected the emphasis of his research efforts towards Aboriginal people with limited contact with Europeans. A typical summary of his policy recommendations to *MJA* readers appeared in 1931:

concurrence of the natives and their assistance’ (p. 261). An earlier Australian blood grouper, Gilbert Phillips, also reported in the *MJA* acceding to refusals of NSW Aboriginal people to participate in his research.
Our experience has shown us that it is very necessary, in the interests of all concerned, that those natives who have not yet come into contact with civilization, should be prevented from doing so. They should be allocated extensive hunting grounds in their own country, and Europeans should be prohibited from trespassing on those grounds under any pretext. This concession we surely owe to those people from whom, by force and without any compensation whatsoever, we have taken away the land they possessed, and deprived them, in too many instances, of the means of obtaining food in their own way and even water. We are afraid that those natives who had come under European influence, whatever care is taken of them, are doomed to extinction. (p. 793)¹⁰

Whilst he may have claimed that these reserves should not be entered by Europeans on ‘any pretext’, there seems to have been one exception to this exclusion. Jones has described Cleland’s recommendation to the University of Adelaide Council in the 1930s that they purchase a lease adjacent to a reserve to protect its inviolate status.⁶⁵ This was not only for Aboriginal people’s ‘protection’ but to ensure continued opportunities to study them in such an ‘ethnographical laboratory’ (p. 79).⁶⁵ The university rejected his proposal. The link between museum-based collecting research like Cleland’s and the official government policy of protection was highlighted by Aboriginal activists associated with the Day of Mourning in 1938. They dismissed ‘protection’ as an attempt to ‘preserve’ Aboriginal people as museum exhibits. Instead they called for equal citizenship rights.¹¹³ Even Olive Pink, an anthropologist close to Cleland, expressed concern about his over-riding interest in Aboriginal people as objects of scientific study at the expense of any humanitarian concern for their rights and welfare.⁶⁹

Cleland seemed very cautious about criticising government practices and policies. Late in life in a 1960 MJA article, he denounced the critics of government saying the state and federal governments were ‘doing all that could be expected’ for Indigenous people.¹¹⁴ Similarly, in 1930, he chose to avoid criticising the government or mission authorities in the MJA article that he wrote about finding a scurvy epidemic at Hermannsburg when he happened to arrive on his 1929 expedition. His refusal to blame the inadequate mission diet for the outbreak of
scurvy led him to claim that the Aboriginal people ‘were mostly in excellent general condition and were able to carry out their normal activities unless actually ill.’ (p. 411) (my italics) Instead of criticising the government or the mission in the *MJA*, he chose to congratulate the mission staff for donating their anti-scorbutic food (oranges, cabbages, and turnip leaves) to the Aboriginal residents. His reports in the *MJA* record his valuable diagnosis of the epidemic and his involvement in its control but not that he also helped organise the collection of donations in Adelaide of citrus and dried fruit as well as money both for Hermannsburg’s water supply and to relieve the mission’s debt.

This political timidity in his medical journal articles was combined with surprisingly strong general comments about European Australians’ responsibility for the negative impacts of colonisation on Aboriginal people. These comments (such as those in the long 1931 quotation on the previous page) can be read as consistent with his representations of Aboriginal passivity and submission and European superiority and not as critical of contemporary authorities. The language about European responsibility and colonialism seems strong but was not atypical at the time. His Adelaide University colleague Frederic Wood Jones also talked of Europeans having ‘invaded’ and having ‘filched this large continent’ from Aboriginal people and of the subsequent ‘moral responsibility’.

Cleland’s support of protection and absorption policies seems to have been informed by a deeply held pessimism about Aboriginal people’s abilities and his conviction that this was due to their inherited, biological racial characteristics. These ideas about Aboriginal people’s abilities were not the direct consequence of his research results but did greatly influence his representations of Aboriginal people as being passive specimens without agency in his research publications. His pessimistic assessment of Aboriginal people’s abilities (and survival) reflected the prevailing ideas in ‘post-Darwinian evolutionary anthropology’ (p. 269) at the turn of the century. He held these long after most of his professional colleagues had discarded them for a more relativist and less biologically determinist approach to anthropological questions.
These beliefs could comfortably co-exist with Cleland's repeated fond and positive remarks about Aboriginal people and their ways in his numerous publications. This co-existence can best be seen in his paternalistic recommendation to the MJA readers that Aboriginal people should be recognised and treated as 'grown-up children' (p. 793). In his description of the 1933 expedition in the MJA, Cleland emphasised Aboriginal people's high moral code, politeness, and humour and that he 'found these natives again a very delightful set of people' (p.849). However such fondness was limited like his research to 'pure' and 'uncontaminated' Aboriginal people. Linda Tuhiwai Smith has similarly remarked of earlier scientific researchers in New Zealand: 'They had a deep sympathy towards Maori people as an ideal while being hostile towards those Maori who fell short of this construct.' (p. 83)

Sunset

Cleland's influence and that of his ideas had passed long before he died in 1971. In 1960, aged 81, he wrote an unguarded 'Point of View' article for the MJA entitled 'The future of the Australian Aboriginal'. His previous stature did not protect from criticism either his now outdated biological determinist assessment of poor Aboriginal ability, in particular their 'mental endowment', or his promotion of 'dilution' of these poor 'native genes' by biological assimilation. His article led to a largely negative response by five MJA readers – JB Hogg, Charles Duguid, Doug Everingham, Barry Christophers, and even Andrew Abbie, the Professor of Anatomy at Adelaide and Cleland's colleague on the Board for Anthropological Research. See chapter eight for a description of these letters.

The Board had suspended its expeditions in 1939 because of the war. Abbie recommenced the Board's expeditions in 1951. These post-war expeditions are discussed in chapter seven. Cleland joined some of these expeditions. A short article he wrote for the South Australian Naturalist based on the 1957 expedition to Haast's Bluff indicated that his need to collect, collate and classify phenomena for science was not confined to his medical research about Aboriginal people.
described the colours of the Central Australian sunset using watercolours – not as a series of paintings but as a series of numbers. He asked another expedition member to record the changing colours of a sunset using watercolours which he later compared to Ridgway’s 1912 *Color standards and color nomenclature*. He informed readers that the sunset had changed from colour standard III to XIV to XXXIX to XXVII to XLII to XX. In the same issue, he used the same colour standards to describe the colour of desert sand.\(^{124}\) His use of these standards in these two articles and three years earlier to describe the colour of human fat found in a series of post mortems, renders more understandable his use of the same standards to describe the colour of the skin in various parts of the body of a still-born Aboriginal infant at a meeting of the Royal Society of South Australia in 1927.\(^{125,126}\)

In the 1940s, following the cessation of expeditions by the Board, researchers other than Cleland continued blood group research. Some of this research was published in the *MJA*, first by RK Gay of Sydney University and then by Roy Simmons, JJ Graydon and others from the Commonwealth Serum Laboratories (CSL) in Melbourne.\(^{13-16}\) This research by CSL was initially driven by the recent discovery of the Rhesus blood group system.

Simmons collaborated with the Board’s researchers in the 1950s and he dominated this research over the next three decades and documented the distribution of many of the newly identified blood group systems amongst various Aboriginal populations. Unlike the research before 1950, almost none of the later research appeared in the *MJA* or other medical journals and so would not have been seen by many medical readers (so I haven’t discussed it in much detail in this thesis).\(^{25}\) Whilst this research continued to collect more blood group data for both the ABO and other blood group systems it increasingly described the genetic heterogeneity of Aboriginal people using blood group gene frequencies. It also compared results in Australian Aboriginal people with those in other populations to try to answer questions about the origins of Aboriginal people before they arrived in Australia.

At a conference at the Australian Institute of Aboriginal Studies in Canberra in 1974, Simmons summarised his 35 years of research into the blood groups of
Aboriginal people and other peoples of the Pacific and nearly one hundred scientific papers. He died the following year. He had hoped that his descriptions of the distribution of the various blood group systems and his calculations of the frequencies of the associated genes would have provided answers about the origins of Aboriginal people. His concluding assessment of this work seems quite sad:

Our studies have not been a complete loss, however, in that we have assisted in providing a blood group genetic map of the Pacific peoples, and for them at least, we have made blood transfusion a safer procedure in return for their blood samples.

Finally, it must be said that our 35 years of blood group genetic research have unfortunately failed to provide us with any clues, at least obvious to us, as to "The Biological Origin of the Australians". (p. 319) 17

**Conclusion**

This chapter began with the failure of blood groups and the Hirschfelds’ ‘biochemical race-index’ to define race. Contradictory results were attributed to the researchers’ over-reliance on blood groups in defining race, not the inherent contradictions and weaknesses of the reification of biological race. Rather than dismissing such ‘failed’ research as the work of inferior scientists and less worthy of historical research, English historian Michael Biddiss wrote:

The truly critical historian, on the other hand, will see value in exploring emphatically those eventual blind-alleys which looked to many like the highroads of future progress, and which therefore often tell us most about the complexity of the past. (p. 876) 127

This chapter has discussed at length research by John Burton Cleland. He wrote one in every six publications and one in every six pages about Indigenous health in the *MJA* before 1950. Unlike the research discussed in chapter four, representations of Aboriginal people were central (not marginal) to these publications. Cleland’s work played a significant role in raising the profile of Indigenous people in the *MJA,*
although his antiquarian ‘collecting’ drew attention to the salvage of data for science rather than to the provision of medical care.

Such ‘data-oriented’ collecting is not dead in Indigenous health research. It continues in the repeated collection of the prevalence of various diseases, disease risk factors, and other physical or social characteristics from different Aboriginal and Torres Strait Islander populations. However few of these present-day collectors quite have Cleland’s zeal and drive to collect, name, and classify all that he encountered – including Indigenous Australians - and then to publish descriptions of these collections.
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Part three

1950 - 1969
Chapter 6

Just the same
This chapter introduces, summarises and briefly contextualises the Indigenous health publications in the *MJA* in the 1950s and 1960s. Unlike earlier Indigenous health research in the *MJA*, researchers avoided mentioning Indigenous people as a different race and some campaigned against racially discriminatory treatment of Indigenous people. Given the influence of the assimilation policy and the predominance of Indigenous health *MJA* publications about the Northern Territory, this chapter also includes a brief description of this policy and its implementation in the Northern Territory.

**More 'expeditions' and many more letters to the editor**

Researchers from the University of Adelaide continued to dominate Indigenous health research in the *MJA*. Their revival of their expeditionary research in 1951 enables comparisons with the inter-war expeditions led by JB Cleland discussed in chapter five. Cleland was a member of some of these post-war expeditions led by the new Professor of Anatomy, Andrew Abbie. Abbie’s Anatomy department organised ten expeditions to Central and northern Australia from 1951 to 1964. Chapter seven investigates the research performed on these expeditions – both by researchers from Adelaide University and from other institutions. Abbie’s explanations of his methods and aims, which were based on collecting data ‘while there is still time’ (p. 98), were not greatly different to those of the earlier expeditions. But the publications were no longer describing a disappearing ‘race’ but a disappearing ‘way of life’.

Chapter seven shows how the representations of Aboriginal people in these later publications were markedly different from the representations in the inter-war publications. In the earlier publications, researchers used complicated explanations to dismiss evidence that undermined their notions that Aboriginal people were a different and inferior race. After the war, the publications show the Adelaide researchers going to similar lengths to avoid explanations that mentioned biological race. Instead they investigated environmental explanations of disease. The social and political context of the research had changed dramatically and so had the
research. Following revelations about the Holocaust, UNESCO issued statements dismissing the biological foundations and predictive value of race. Meanwhile, in Australia, the policy of the cultural (rather than biological) 'assimilation' of Aboriginal people was being promoted by the Commonwealth Government.

Whilst the official assimilation policy dominated this period, a strong alternative voice emerged that included many Aboriginal and Torres Strait Islander people. In 1958, the Federal Council for Aboriginal Advancement (FCAA) (later the Federal Council for the Advancement of Aboriginal and Torres Strait Islanders or FCAATSI) was formed. A member of FCAA and FCAATSI, Barry Christophers, was the author of the most Indigenous health publications in the *MJA* during the 1950s and 1960s. The quantitative analysis in appendix three shows that he was the author of 19 Indigenous health publications, whilst no other author had more than five Indigenous health publications in the *MJA*, in the fifties and sixties. In his publications (all letters to the editor), Christophers campaigned against racial discrimination and for equal rights for Aboriginal people. Christophers wrote that Aboriginal people's health problems were caused not by racial inferiority but by racism, colonialism, capitalist exploitation, and poverty. His letters along with other letters to the editor are investigated in chapter eight.

There had been an even more dramatic increase in letters to the *MJA* editor about Indigenous health than about other matters - an indication of an increased interest in Aboriginal health by *MJA* readers. These letters demonstrated the range of medical thinking about Aboriginal people and their health. The letters frequently questioned the representations of Aboriginal people in other *MJA* publications as well as their explanations for Aboriginal people's poor health and how this might be overcome. These are the central concerns of this thesis, so it is very appropriate to discuss the contemporary investigation of these concerns in the letters pages of the *MJA*. Researchers may not value letters to the editor as highly as original research articles and editorials but readership surveys in the 1950s and 1960s (and more recent

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a A further investigation without the sampling frame (of publications cited by either the Aboriginal health bibliography3 or Harrison's thesis4 ) identified another six of his letters to the MJA about Aboriginal health.5-10
surveys) demonstrated that the letters pages were the most frequently read section of the *MJA*.11-13

Two milestones in the *MJA*

In May, 1962, the *MJA* published a special issue to commemorate the inaugural meeting in Adelaide of the Australian Medical Association (AMA) - featuring articles about the history of medical associations in Australia, USA, Britain, and various other countries which had once been British colonies.14 Whilst the British Medical Association (BMA) had granted the Australian branches autonomy to deal with local matters in 1923, Australia was slower than other countries in forming a national medical association following the demise of the British Empire after the Second World War.15 The AMA commenced operations at the beginning of 1962, with the state branches and the Federal Council of the BMA in Australia becoming the branches and Federal Council of the AMA. The AMA remained affiliated with BMA as had medical associations in other Commonwealth nations such as Canada and India. Membership no longer included subscription to the BMA’s journal, the *British Medical Journal*, but members would continue to receive the *MJA* as the official journal of the new association, the AMA.15 This would lead to even greater local prominence of the *MJA*, which a survey of Australian doctors in 1955 had already shown to be more widely read and of more interest than the *British Medical Journal*.11,12

The *MJA* celebrated its fiftieth anniversary in 1964 with an issue covered in gold rather than the standard blue. The issue included a two-page history of ‘Our first fifty years’ followed by messages of congratulations from editors of seven medical journals from other countries interspersed with photographs of the three editors of the *MJA* – Armit, Archdall, and Winton.16,17 Ronald Winton had been Archdall’s assistant editor since 1947 before taking over as editor in 1957. He held this position until 1977 and as I write this at the beginning of the next century he is still associated with the journal as its ‘Editor Emeritus’. The durability and influence of these first three editors who reigned over the first 63 years of the *MJA* is even more
remarkable as there were nine different editors in the 20 years after Winton’s resignation.

The anonymous author of this brief history told MJA readers of the strong foundations laid by the first two editors and the contemporary ‘boom’ being enjoyed by the journal.\(^{16}\) This boom was part of Australia’s post-war ‘golden age’ of prosperity – which was also a time when the sciences had great confidence and authority.\(^{18}\) This boom was associated with the arrival of several new medical journals in Australia and the beginning of a world-wide exponential rise in the number of medical journal publications each year.\(^{19-21}\) The article’s author wrote that the MJA was publishing more pages each year, being more selective, the number and quality of papers being submitted was increasing, and there were more book reviews and a dramatic increase in the numbers of letters to the editor published. Of the latter, the author wrote:

> This increase is rather staggering, but it is also encouraging, in so far as the correspondence columns are generally regarded as a fair barometer of the interest of readers in any publication. (p.28)\(^ {16}\)

The author of this celebratory 1964 article emphasised the increased ‘pride’ and ‘responsibility’ of the MJA following the formation of the AMA and then wrote of the context of increasing medical knowledge and of medicine becoming more scientific. But with all these changes, the author left a special place for a general medical journal, such as the MJA, in ‘keeping medicine human.’ (p. 28)\(^ {16}\) This part of the thesis, describing the 1950s and 1960s, will demonstrate that this increased confidence in Australian medicine and the MJA was associated not only with an increased attention to Indigenous health research but also to the humanity of Aboriginal and Torres Strait Islander people.
Greater attention to Indigenous people in the MJA, 1950-69

The quantitative analysis of the 200 identified Indigenous health publications in the MJA from 1950 to 1969 (and their comparison with a random sample of other MJA publications) is described in more detail in appendix three. There was much more attention to Indigenous health in the 1950s and 60s than in previous decades. (figure 6.1) Before 1950 there had been only six years with more than five Indigenous health publications in the MJA and none with more than ten publications. From 1950 to 1969, there were seven years with more than ten Indigenous health publications in the MJA, with the most published in 1969 (25 publications).\(^b\) Figure 6.2 shows the increase in the annual number of Indigenous health MJA publications over the 1950s followed by a slight decline in the early sixties before another increase at the end of this decade. The further increase in the annual number of Indigenous health MJA publications in the 1970s is discussed briefly in chapter nine. The annual number of pages of Indigenous health publications in the MJA fluctuated more erratically from year to year than the number of publications but similar trends are still discernible in figure 6.3.

Figure 6.4 shows the increasing number of pages published by the MJA as mentioned in the fiftieth anniversary issue in 1964\(^16\) as well as an even greater increase in the last years of the sixties. Because of this increase, the description of Indigenous health publications in the MJA as a proportion of all MJA publications, in figure 6.5 emphasises the earlier publications and accentuates the peak in the late 1950s and conceals the rise at the end of the 1960s. With the possible exception of this peak of interest in the late 1950s, the increasing amount of Indigenous health publications in the MJA over the 1950s and 1960s was associated with the increasing amount of all publications in the MJA. And the increase in the number of pages published by the MJA was accompanied by an increase in the number of pages about Aboriginal and Torres Strait Islander people.

\(^b\) These seven years were 1956 (with 11 publications and 14.7 pages of text), 1957 (18 publications, 37.7 pages), 1959 (14 publications, 22.3 pages), 1960 (11 publications, 14.3 pages), 1964 (13 publications, 28.2 pages), 1968 (14 publications, 11.2 page), and 1969 (25 publications, 40.3 pages).
Number of MJA publications about Indigenous health, 1914 - 1969

Figure 6.1

Number of MJA publications about Indigenous health, 1950-69

Figure 6.2
Pages of *MJA* publications about Indigenous health, 1950-69

Total pages of *MJA* publications by year (excl supplements), 1950 - 1969
Percentage of total pages (excl supplements) of *MJA* publications about Indigenous health, 1950-69

Figure 6.5
Len Smith's estimates of the Aboriginal and Torres Strait Islander population in 1954, 1961, and 1966 (in his table 8.2.5)\textsuperscript{22} are each 1.1\% of the Australian Bureau of Statistics' estimates of the total Australian population in those years (p. 125)\textsuperscript{23}. Indigenous health publications accounted for 1.0\% of the pages of the *MJA* (excluding supplements) in the 1950s and 0.8\% of the pages of the *MJA* in the 1960s. Unlike earlier decades, the proportion of *MJA* publications about Indigenous Australians was beginning to approach their proportion of the total Australian population, even though still not yet acknowledging their greater disease burden.

There were at least twice as many *MJA* publications about Indigenous people in the Northern Territory in the 1950s and 1960s than about Indigenous people from any of the states. This attention on the Northern Territory was more marked than in earlier decades. In the 1950s these Northern Territory publications were more often about Central Australia and in the 1960s about the Top End. The numbers of publications about Western Australia was similar to the number about Queensland in the 1950s but there was twice as many about Western Australia in the 1960s. Compared to the size of their Indigenous population there was an over-representation of publications about the Northern Territory and an under-representation of publications about New South Wales. There were more Indigenous people living in Queensland than the Northern Territory; with similar numbers living in the Northern Territory, Western Australia and New South Wales.\textsuperscript{22}

During these two decades various names were used by authors to describe the government health services in the Northern Territory. These names reflected different levels of local autonomy of the services and different levels of identification with the Northern Territory by the authors: from the Commonwealth Department of Health and its Northern Territory Medical Service to the Department of Health Northern Territory. The Northern Territory Medical Service had been made part of the Commonwealth Department of Health in 1939 when a new Native Affairs Branch was separated from the medical service.\textsuperscript{24,25} Authors only began to mention the Department of Health Northern Territory in 1967.\textsuperscript{26}
When these three differently named organisations were combined, they were acknowledged by the authors of a similar number of publications as had acknowledged Adelaide University - 21 publications (49.9 pages). Almost all of these government publications from the Northern Territory (17/21) were in the 1960s and nearly half (9/21) were in the last two years of the sixties (three in 1968 and six in 1969). There had been no Indigenous health publications in the MJA by authors associated with the Commonwealth Department of Health about the Northern Territory before 1952; nor any that acknowledged either the Northern Territory Medical Service or the Department of Health Northern Territory.

Assimilation and more research ‘on the purely medical side’ in the Northern Territory

The 1950s and 1960s were the heydays of the assimilation policy, and I now briefly discuss this policy and its administration in the Northern Territory. This policy was more influential in the Northern Territory than in the states reflecting the Commonwealth Government’s special influence in the Northern Territory.27 The administration of the Northern Territory was the responsibility of the Commonwealth Government’s Department of Territories, and the Northern Territory Legislative Council had a majority of Commonwealth Government appointed members and only a minority of elected members.27 The Australian Constitution specifically excluded the Commonwealth Government from making ‘special laws’ for ‘the aboriginal race’ in the states (but not in the Northern Territory which was and is still not a state) until the 1967 Referendum.28 The prolonged involvement in the administration of Aboriginal people’s lives in the Northern Territory of Paul Hasluck (Minister of Territories, 1951-63) and Harry Giese (Northern Territory Director of Native Welfare – later renamed as Social Welfare, 1954-72) also contributed to the assimilation policy’s greater influence in the Northern Territory.27

Colin Tatz’s 1964 PhD thesis, *Aboriginal administration in the Northern Territory*, still provides a detailed and useful (and contemporary) examination and description of the assimilation policy in practice in the Northern Territory during the 1950s and
After the announcement of McEwen’s New Deal in 1939, there had been a hiatus associated with the ‘var until the early 1950s in Commonwealth attention to Aboriginal Affairs. Tatz has written that, in the 1950s, policies based on segregation and protection were abandoned as most Aboriginal people were then thought to have significant contact with White society. The assimilation policy promoted Aboriginal ‘advancement’ and was based on a belief in equality and ‘a fair go’ and so any special measures for Aboriginal people were to be temporary and definitely not based on race. In his recent book, *White flour, White power*, Tim Rowse has contextualised these goals in Hasluck’s broader vision of an Australian nation of individuals not constrained by race or class. Rowse then eloquently summarised the flaw in these seemingly laudable goals of assimilation:

> Intended as a magnificent departure from certain kinds of colonial racism, this emphasis on “equality” proved to be racist in a different way - its failure to acknowledge the right to be different. (p. 222)

This fundamental insensitivity and ethnocentrism of the aims of the assimilation policy and the ‘cultural violence’ (p. 143) of its implementation were also remarked upon by Aboriginal activists at the time. Kath Walker described it in 1962 as ‘the most inhuman way of bringing my people forward’ (p. 188) and Herbert Groves called it ‘almost genocide’ (p. 211) in a submission to a New South Wales parliamentary committee in 1966. The Human Rights and Equal Opportunity Commission’s 1997 report, *Bringing them home*, has confirmed that the government practice of child removal during the assimilation era (and before) met the legal definition of ‘genocide’ that Australia ratified in 1949.

Tatz’s thesis is not really a critique of the assimilation policy but an assessment of the Northern Territory Administration’s adherence to the policy and of whether the policy’s aims of ‘advancement’ and ‘equality’ were being achieved. But he did write that legislation introduced as part of the assimilation policy, such as the *Welfare Ordinance 1953*, was still about the control of Aboriginal people’s lives rather than their ‘advancement’. Even by its own standards, in almost all areas, including health, he concluded that the policy had failed and had not always been
followed in the Northern Territory. Tatz wrote that the health of Aboriginal people in the Northern Territory had not improved with the policy and that their poor health had become accepted as normal. One of the many problems he identified was the lack of appropriate health research:

The list of research publications for Territory Aborigines, cited in the Bibliography, appears top-heavy on the physical anthropology and haematology side and light on the purely medical side. The medical research to date is scant. (p. 140)²⁹

However, one of the most marked differences between the Indigenous health research published in the MJA during 1950s and 1960s and that published before 1950 was that it began to be increasingly about the major health problems of Indigenous people – especially those living in the Northern Territory. But, to be fair to Tatz, this increase was more marked in the last years of the sixties after he had written his thesis. Tatz’s bibliography of medical literature was very short and based on the references cited in Annual Reports by the Director General of Health. His bibliography only cited one article ⁴ based on research performed on expeditions to the Northern Territory by researchers from the University of Adelaide and associated organisations (see chapters five and seven). This expeditionary research was dominated by physical anthropology and haematology. But readers of the MJA would have noted that this was not the only research published about Northern Territory Aboriginal people in the 1950s and 1960s – there was indeed much more research ‘on the purely medical side’ than in earlier years.

The MJA published cross-sectional surveys of diseases that were known to be common amongst Aboriginal people in the 1950s – such as tuberculosis³⁴-³⁷ and leprosy³⁸. But there was still a large number of case series or case reports in both the 1950s and 1960s of unusual diseases (most commonly the rare tropical disease melioidosis) in Indigenous people.³⁹-⁵⁵ In the 1960s, epidemics of measles⁵⁶-⁵⁸ and other diseases⁵⁹-⁶¹ amongst Aboriginal people were described along with descriptions of the medical interventions for their control. An example was the
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¹ Tatz was referring to the bibliography in his own thesis.
² This article was published in the American Journal of Physical Anthropology.
description of the vaccination of nearly a thousand Northern Territory Aboriginal children with the new live attenuated measles vaccine, just before it was routinely available in Australia, to control an epidemic that had started in late 1965. Nutritional surveys in the Northern Territory in the 1950s were followed by more detailed research on child growth and childhood malnutrition in the late 1960s.

This nutrition research at the end of the 1960s hypothesised and investigated a wide range of explanations for Aboriginal childhood malnutrition - repeated and chronic infections, poor hygiene and sanitation, lactose malabsorption, growth hormone levels, breast milk quality and the inadequacy of breast milk alone after six months of age. In 1966, in the first of these articles, Ellen Kettle, a survey nurse, demonstrated the lack of improvement in the weight curves of Aboriginal infants following the introduction of communal feeding by the Northern Territory Administration in 1958. Hers was the only one of these publications to note that Aboriginal mothers had never fully cooperated with communal feeding. She proposed the need for more information 'about the attitudes of the individual Aboriginal mother to interference with the feeding and management of her babies' (p. 977) as well as more information about the links between infections and malnutrition. The failure of communal feeding and rationing for Aboriginal people in the Northern Territory has been recently examined by Tim Rowse and Lindsey Harrison.

Colin Tatz’s thesis in 1964 and subsequent research by Peter Moodie of the University of Sydney published in the MJA demonstrated alarmingly high infant mortality rates amongst Northern Territory Aboriginal children, especially in Central Australia. Immediately following Moodie’s 1969 article, the MJA published an article by a general practitioner from western New South Wales, Archie Kalokerinos, who suggested that vitamin C deficiency was the link between childhood malnutrition and infant mortality. He calculated the infant mortality rate in Walgett area and, as in the Northern Territory, found that it was very high:
It may surprise some people to read that in settled and climatically tolerable areas, the Aboriginal infant mortality rate is also very high. (p. 186)

**Culture not race and culture as race**

Kalokerinos’s paper was one of only a few *MJA* publications in the 1950s and 1960s in which people could read anything about Aboriginal people in New South Wales and their health – even though there were nearly as many Indigenous people living in New South Wales as in the Northern Territory. Nearly half of these publications about New South Wales were excerpts from historical accounts written in the first three decades of White settlement in Sydney which described Aboriginal people and their medicine. In a 1959 letter to the *MJA* editor, a general practitioner from Armidale in northern New South Wales, Ellen Kent Hughes, provided a possible answer for this lack of published attention on Aboriginal people in New South Wales. She wrote that:

> the aborigines and the mixed bloods, which are practically the only coloured people in New South Wales, present quite different problems. When the problem of the mixed bloods is being considered, it is the problem of the poor white more than that of the primitive aboriginal native. (p. 311)

It is reasonably clear from the rest of Kent Hughes’s letter and her other letters to the *MJA* editor that she was not proposing a class-based analysis of the health problems of Aboriginal people in New South Wales. She was just suggesting that the health problems of these Aboriginal people were no different than those of White Australians. At the same time she was claiming that these ‘mixed bloods’ were different from ‘the aborigines’ and the ‘primitive aboriginal native’. With her choice of words she conflated primitivity and blood – or culture and race.

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* He claimed that his use of injections of vitamin C had led to the absence of any infant death in Collarenebri in the previous 12 months ‘without any obvious social change’ (p. 187). He later claimed that altered immunity caused by immunisation was also a cause of high infant mortality. These claims aroused considerable controversy and have little professional support today.
Gillian Cowlishaw has described the continuation in anthropology of supposedly discarded racial definitions of Aboriginal people in definitions based on culture that were similarly fixed.81,82 ‘Pure-blooded’ and ‘full-blood’ Aboriginal people became ‘traditional Aborigines’ whilst ‘mixed-bloods’ and ‘half-castes’ became ‘non-traditional’. She has claimed that the anthropological study of the second group was considered just as irrelevant as when they were defined with race-based terms.

It was not very different in medical research in the 1950s and 1960s. In spite of their avoidance of the language and explanations of race, the work of the researchers from the University of Adelaide that is discussed in chapters five and seven focussed on ‘the primitive aboriginal native’ and excluded those they called ‘mixed blood’. The lack of any sustained attention by medical researchers on the health of Aboriginal people who were not labeled ‘primitive’ (and who might be labeled ‘mixed blood’) would soon change. In the 1970s, Max Kamien wrote 14 MJA publications and a book about the Aboriginal people of Bourke in western New South Wales.83

In this part of the thesis, I have favorably described the discarding of racial explanations of Indigenous health in the 1950s and 1960s. I am aware however of the limitations of such a binary interpretation and history of cultural versus racial explanations. Following Cowlishaw’s advice, the two chapters that follow also investigate the ambiguities associated with the abandonment of racial explanations of Indigenous health and the power relations implicit in these changing representations of Indigenous people.31,84

The late sixties and the birth of ‘Aboriginal health’

The late sixties were a time of great political change for Aboriginal and Torres Strait Islander peoples as for all Australians. The ‘Freedom Ride’ led by Charles Perkins through western New South Wales in 1965 raised attention towards racial discrimination against Aboriginal people. A successful campaign by FCAATSI led
to the 1967 national referendum and the removal of discriminatory references to Aboriginal people in the Australian constitution. The ‘walk off’ Wave Hill station by the Gurindji in 1966 (along with the earlier ‘bark petition’ from Yolngu to the Commonwealth Parliament) focussed national attention on ‘land rights’ for Indigenous Australians.32

These dramatic changes would continue into the first years of the seventies which brought the erection of the Aboriginal Tent Embassy outside Parliament House in 1972, the establishment of the Aboriginal Legal Service and Aboriginal Medical Service in Redfern in 1971 and 1972, and the reforms introduced by the Whitlam government following its election in 1972.32 Many of the Aboriginal activists involved in these political changes had been involved in FCAATSI – which is discussed in chapter eight in order to contextualise the many letters to the MJA editor by FCAATSI executive member Barry Christophers in the 1950s and early 1960s.

The late sixties mark a definite turning point and the end of the historical period studied in this thesis. And so the two more detailed chapters that follow concentrate on research published in the 1950s and only the early 1960s. The research published about Indigenous Australians in the MJA began to change dramatically in the late sixties. Even the words used in these publications to name Indigenous people changed – with the noun ‘Aborigines’ and the adjective ‘Aboriginal’ becoming by far the most commonly used words. These words began to be used with a capital ‘A’ in 1964. The end of the thesis and this shift in Indigenous health research is epitomised not only by the increased attention of researchers on childhood malnutrition and other medical problems of Aboriginal people in the Northern Territory but by conferences held in 1968 and 1969. Summaries of the papers presented and the discussions at these meetings were published in the MJA. The title of the Paediatrics and Child Health Section of the Australian Medical Congress in Sydney in 1968 was ‘The health of Aboriginal children’. The MJA published anonymously written summaries of six papers and the discussion that followed.66,67,71,85-88 These included reports of Northern Territory research into
Aboriginal childhood malnutrition and Peter Moodie's mortality statistics that I have already mentioned in this chapter as well as a description by Kettle of health services for Aboriginal people in the Northern Territory. She was reported as encouraging a shift in the way these services were provided, promoting greater Aboriginal involvement. Whilst she still left space for a great deal of non-Aboriginal control and direction, the report of her proposal made it clear to readers that Aboriginal people should have a central role in their own health care in the future.

To date the health service had been orientated primarily to care, but in future the goal must be to promote self-help for the Aborigines. This involved learning the language and culture, so that, adequate communication having been achieved, potential leaders among the Aborigines themselves could be motivated and trained to conduct their own health programme and teach their own people. (p. 105) 86

The conference in Adelaide in the following year organised by the Post-graduate Committee in Medicine of the University of Adelaide highlighted even greater changes. The report of the conference in the MJA was titled 'Aboriginal health'. 1 This was the first time that the two words 'Aboriginal health' were used together in the MJA. The notion of 'Aboriginal health' had been born. 'Aboriginal health' quickly became accepted in both medical and popular discourse. In 1975, the MJA published its first 'Special supplement on Aboriginal health'. Readers were told in an editorial that this supplement to the journal, and the 12 supplements that followed in the next six years, was 'born of the need to find space for the steadily increasing flow of papers on various aspects of Aboriginal health.' (p. 24) 89 The same editorial continued:

Aboriginal health is indeed a legitimate field of interest, but also one with general relevance for current health and medical problems as a whole. (p. 23) 89

1 The Post-graduate Committee in Medicine has not operated for many years and there are no records relating to this conference in the University of Adelaide archives or medical library. (Percival, K., pers. comm., 13 July and 24 July 2000) My discussion of this conference relies entirely on the anonymously written report in the MJA. A more detailed investigation of this conference would necessarily rely on oral history research beyond the scope of my project.
The *MJA* reported that the conference had been attended by about 150 people and summarised eight papers given and the discussion that followed.\(^{69,90-97}\) Again the focus was on Aboriginal people in the Northern Territory, with three papers by doctors from the Northern Territory health department and five papers by academics from the University of Adelaide about their research in the Northern Territory. Childhood malnutrition and infections, infant mortality, and health services were discussed as at the conference the previous year. Only RB Elliott of the University of Adelaide was reported as presenting papers at both conferences. Andrew Abbie and Murray Bennett reported physical anthropological and dental findings from University expeditions to the Northern Territory (see chapter seven). There was also a paper about psychiatric services and disorders in the Northern Territory and EM Symonds’s paper entitled ‘Problems of contraception’.

Symonds’s paper and the reported discussion that followed reflect the changes that had occurred since the early years of the century (and the early research cited in this thesis). No longer were Aboriginal people described as just a race doomed to extinction while researchers’ publications concentrated on collecting data (before it would be too late). Rather the Aboriginal population was now described as increasing, which was leading to the consideration of new problems in health service delivery. Symonds emphasised the need to provide family planning services and then GM Maxwell when summarising the proceedings said:

> Family planning measures were a most important need, to contain the increase in the population as a whole, and to limit the sudden increase in numbers of children in particular. ... any increase in population compounded all existing problems. (p. 1028)\(^{95}\)

Maxwell told his audience that the problems of Aboriginal health needed a ‘multidisciplinary approach’ (p. 1027)\(^{95}\). Whilst I have argued elsewhere in this thesis that earlier researchers expressed a distaste for mere politics, Maxwell emphasised the political over the medical. However he still stressed that doctors had special responsibilities.
The cure of the problems was social and political. Physicians could only point the way to society, which must then take the necessary steps. (p. 1027)

The MJA report also listed the other disorders mentioned in the discussion at the conference: 'tuberculosis, alcoholism and venereal diseases' (p. 1027). A month later a letter to the MJA editor by Kevin Anderson from the Institute of Medical and Veterinary Science in Adelaide was published. He suggested that ‘venereal diseases’, which he knew to be common in Aboriginal people because of the number of specimens he had examined, had been purposefully avoided at the conference in favour of ‘less distasteful matters’ (p. 1233). With no archival material available related to the organisation of the conference nor a responding letter from the conference organisers it is not possible to answer Anderson’s charge.

Anderson’s claim highlights a definite change in the representation of Indigenous people in Australian medical journals. In chapter two, I explained that the first disease to have any sustained attention in medical articles about Indigenous Australians was a horrible mutilating sexually transmitted disease now called donovanosis. These articles were accompanied by photographs that can be described as unpleasant or ‘distasteful’.

Conclusion

In this chapter, I have introduced the significant changes in the representation of Aboriginal and Torres Strait islander people in medical research in the MJA in the 1950s and 1960s. Scientific ideas about race and its utility changed by the early 1950s. I have also described some of the changes in Indigenous health research in the Northern Territory and the impact of the contemporary policy of assimilation on this research. This research began to investigate the major health problems that were affecting Aboriginal people in the Northern Territory, rather than just using Aboriginal people to answer questions about settler health. These changes will be explored in more detail in the next two chapters.
References


Chapter 7

Avoiding race
This chapter investigates the post-war Aboriginal health research done by the University of Adelaide. In the 1950s and 1960s, as in the previous four decades, more Indigenous health publications in the *MJA* were written by authors from the University of Adelaide than from any other institution.\(^1\)\(^{2-4}\) Whilst there were continuities with the university’s pre-war research, some marked differences began to emerge. The most obvious of these changes were the researchers’ avoidance of racial explanations of difference and the shifting nature of the power relationships associated with the research encounter.

The Board for Anthropological Research’s expeditions to Central Australia recommenced with an expedition to Yuendumu in 1951 after they had been suspended by the war. Researchers from the University of Adelaide on these expeditions collaborated with researchers from other institutions and were at times accompanied by researchers from other institutions. This chapter also looks at some of this related research. It begins with polio and Murray Valley encephalitis (MVE) research by the Adelaide-based Institute of Medical and Veterinary Science (IMVS) and ends with the University of New South Wales’ John Cawte and his ‘ethnopsychiatric’ research.

**Polio and MVE – a distaste for race**

From 1953 to 1957, IMVS researchers published six *MJA* articles (and one short report of a paper given at the Medical Sciences Club of South Australia) describing the serology of Northern Territory Aboriginal people in order to understand the transmission of the viruses that cause poliomyelitis and Murray Valley encephalitis.\(^2\)^\(^{25-30}\) In these articles we see researchers re-visiting questions (that were discussed in chapter four) about the role of racial immunity and geography in disease. The IMVS researchers reached quite different conclusions to those investigating the White race in the tropics. Although this research was based on the serology of Aboriginal people’s blood like the blood groupers in chapter five, Aboriginal people were represented very differently in this IMVS research.
The IMVS was established in 1938 mainly due to the efforts of Trent Champion de Crespigny who was Dean of the Adelaide University Medical School and the Medical Superintendent of the Royal Adelaide Hospital. It grew out of the hospital’s laboratories and has continued to combine laboratory services with teaching and research. This combination has been facilitated by its shared location with the university and hospital. Phillip Jones has emphasised the importance of the proximity of the South Australian Museum to the medical school in the particular style of anthropology (discussed in chapter five) that developed in Adelaide at the Board for Anthropological Research. The proximity of institutions in central Adelaide was also important in the polio and MVE research by the IMVS in the 1950s.

**Poliomyelitis**

In 1951 there was an epidemic of polio in Darwin with ten cases reported. This was followed by a similar outbreak in Alice Springs. All the reported cases were in White residents. John Miles, a medical research officer at IMVS, applied to the National Health and Medical Research Council (NH&MRC) for funding to conduct a serological study of Aboriginal people in the Northern Territory to find out why paralytic polio was rare in this population. The NH&MRC provided £500 for this IMVS survey which was conducted in September and October 1952 with the results being published in the *MJA* in 1953 (describing antibodies to type II polio virus) and 1955 (describing antibodies to types I and III polio virus).

The IMVS research began with an assumption that polio was different in Aboriginal people. This assumption was mentioned both in the discussions at the NH&MRC before the research began and in the first sentence of Miles’s first *MJA* article reporting his results. The article began:

It is widely held by those who have an intimate knowledge of the Australian aborigines that paralytic poliomyelitis is very rare among them, and it has been particularly noted that when an epidemic has occurred among the white men in areas where the aborigines are numerous, the aborigines are spared. (p. 773)
Whilst a statement of difference was made, the readers could also see the author carefully choosing his words to distance himself from this statement. This was not just because he already knew the results of his research. After the discussion of the research proposal at the Poliomyelitis Committee of the NH&MRC but before the research had begun, the Director-General of Health wrote of 'the apparently low incidence of poliomyelitis in aborigines' and of 'this low incidence, if real'. (my italics)35 This was probably not just scientific caution in response to limited data. In the 1950s and 1960s, researchers tended to downplay any biological difference between Aboriginal and non-Aboriginal people. This reflected a significant change from the research published in the first half of the twentieth century and is an early focus of this chapter.

Miles claimed to be testing three hypotheses to explain this apparently low incidence of polio: a lack of contact with the virus, very early contact with the virus (paralysis is only rarely associated with polio infection but becomes more frequent with increasing age of the patient), and racial resistance.27 Miles and his colleagues showed little enthusiasm or interest in the last hypothesis, seemingly only including it for completeness. They only returned to it in the last sentence of their last MJA article about the research in which they said that their findings were 'quite inadequate to support an alternative theory of inborn racial resistance.' (p.438)29

Whilst uninterested in race as an explanation of the different incidence of polio in Aboriginal people, the authors were quite interested in investigating the impact of Aboriginal people’s ‘way of life’. The authors did this by ranking groups of Aboriginal people according to an estimate of their ‘varying amounts of contact with white men’ (p. 774)27. The researchers had taken blood from 436 people in 12 different locations throughout the Northern Territory. When they presented their results by these locations in tables, they were listed according to this ranking. And in another table they compared the results from the five places with the estimated least contact which they labeled ‘Little White Contact’ with the rest which they called ‘Heavy White Contact’.27
The results of testing the antibodies to type II polio virus were similar in all centres with no difference between the 'little' and 'heavy' White contact groups. Miles concluded that the high rates of immunity from a young age suggested early infection as the reason for the low incidence of paralytic polio as in other published studies of different populations. The results which were reported two years later for the other two serotypes were not so neat. A couple of locations in the Top End showed no immunity in the younger age groups. From this, the authors concluded that these serotypes of the virus had not spread to these rather remote places for some years. This now meant that the apparent low incidence of paralytic polio could no longer be explained by near universal infection with the virus in infancy when paralytic symptoms are rare.

At this point the authors could have resorted to a racial explanation of their findings. Unlike the blood groupers in the 1920s, who went to extraordinary lengths to preserve a racial explanation of their findings, these viral serologists went to similar lengths to avoid such an explanation. They chose to question their original assumption of a low incidence of disease. Because the Aboriginal population was small, they claimed that even if there was a similar incidence of disease as in White Australians, the expected number of Aboriginal cases would be very small. They described examining in 1954 a couple of Aboriginal patients who had residual symptoms and histories of illnesses suggestive of earlier paralytic polio. The authors claimed to have not seen any Aboriginal people with polio on their 1952 trip. Ellen Kettle has since described Miles examining an Aboriginal girl from Yuendumu with polio-like symptoms on that trip. But she wrote that he dismissed the possibility that the girl had polio.

Ellen Kettle has described how the notion that Aboriginal people did not get paralytic polio persisted amongst some health officials in the Northern Territory - with cruel consequences. The Salk vaccine to protect children from polio first became available in the Northern Territory in July 1956. Initially, only White children and Aboriginal children of mixed descent were vaccinated; other Aboriginal children were not immunised. The error of this policy became apparent when many Aboriginal people across the Northern Territory were affected by the
polio epidemic of 1959-60. Whilst some suffered residual paralysis, fortunately, none died, unlike some patients in earlier polio epidemics in the Northern Territory. The idea of Aboriginal immunity to polio was finally debunked and the immunisation of Aboriginal people was belatedly begun in the early sixties.

The IMVS researchers had also used their results to make some comments about polio that were generalisable to all populations. They described the loss of a person's immunity over time and the lack of cross immunity between the different polio virus serotypes. This use of a large research project involving Aboriginal subjects to answer questions about the biology of a disease process that was applicable to all people was even more noticeable in the Murray Valley encephalitis research performed by IMVS.

Murray Valley encephalitis

The blood that had been collected from Aboriginal people in 1952 for polio research by the IMVS was also examined for antibodies to the virus causing Murray Valley encephalitis (MVE). IMVS researchers similarly examined blood collected on the Adelaide University expeditions to Yuendumu in 1951 and to Haast's Bluff in 1956. Not only did they also examine these blood samples for MVE antibodies but for evidence of psittacosis, Q fever, yaws and hydatid disease. As well as these opportunistic investigations of blood samples from other research expeditions, IMVS researchers collected blood samples for MVE antibodies during an NH&MRC-funded expedition from October to December 1954 when they re-visited three locations from their 1952 expedition. This MVE research by the IMVS using Aboriginal blood was reported in four articles in the MJA from 1953 to 1957.

These IMVS researchers also published papers describing veterinary research into MVE in the specialist journal of Australian medical science, the Australian Journal of Experimental Biology and Medical Science, in 1952 and 1954. The prevalence of MVE antibodies in the blood of Aboriginal residents of the Gerard Mission on the Murray river were also included in papers by IMVS researchers in the MJA.
1957\textsuperscript{37} and the *Australian Journal of Experimental Biology and Medical Science* in 1958\textsuperscript{38}.

There was considerable medical interest in MVE in the early 1950s. Twenty three articles about this disease were published in the *MJA* in the 1950s. The 1952 *MJA* included seven MVE articles (including all five original articles in the January 26 issue) and two reports from the 1952 Australasian Medical Congress of the BMA in Melbourne.\textsuperscript{a} This MVE research published in the *MJA* was dominated by researchers from the Walter and Eliza Hall Institute in Melbourne in response to the outbreak of MVE along the Murray river in 1951. Forty cases of MVE were identified from 5 January to 12 April 1951 and 17 of these died.\textsuperscript{39} IMVS researchers wrote many fewer publications (there were only two cases from the South Australian part of the Murray\textsuperscript{40}) but their contribution in isolating the virus\textsuperscript{41} and providing an explanation for the timing of the epidemics\textsuperscript{25} was very significant.

Murray Valley encephalitis was not identified as an Aboriginal health problem — it was not described as an important cause of disease or ill health in the Aboriginal population nor were Aboriginal cases discussed. But Aboriginal blood was tested for past infection with the virus. Neither the papers describing the blood taken by IMVS researchers at the Gerard Mission, a 1952 *MJA* article by Hall Institute researchers which analysed blood taken from Aboriginal people on Mornington Island in Queensland\textsuperscript{42}, nor a 1957 *MJA* article by Brisbane researchers which included the investigation of blood taken from Aboriginal and non-Aboriginal people in various parts of Queensland\textsuperscript{43} were included in Moodie and Pederson’s Aboriginal health bibliography or Harrison’s list of *MJA* publications about Aboriginal health (nor have I included them in the quantitative analyses in this chapter and chapter six).\textsuperscript{44,45}

Earlier epidemics had been recorded in 1917-18, 1922 and 1925. No Aboriginal cases were recorded in these epidemics either. Cases had occurred along the Murray

\textsuperscript{a} The 23 *MJA* articles were published in 1951 (2 articles), 1952 (7), 1953 (5), 1954 (2), 1955 (1), 1956 (1), 1957 (4), and 1958 (1). There were also reports published of papers from the 1952 Australasian Medical Congress and from the 1954 ANZAAS conference in Canberra. There were no *MJA* publications about MVE in either 1950 or 1959.
Darling river system with some additional cases in Queensland. The first epidemics had been investigated by JB Cleland (see chapter five) when he was the principal microbiologist at the Department of Public Health of New South Wales and Anton Breinl from the Australian Institute of Tropical Medicine in Townsville (see chapter four) along with several other researchers. Breinl and Cleland described the clinical and post-mortem features of the cases, of whom nearly two-thirds died, and animal experiments which showed that the disease was caused by an infectious agent that could be transmitted to animals. Little was known then of the disease and so it became known as Australian “X” disease. After their early work investigating the 1951 epidemic, Gray Anderson and Macfarlane Burnet of the Hall Institute renamed the disease Murray Valley encephalitis. After the next epidemic which spread from the Murray Valley to the rest of mainland Australia in 1974, the name of the virus was changed again to Australian encephalitis. Since 1974, all except one of the cases had been from north Australia until an outbreak in Central Australia in early 2000; and the disease has been shown to be caused not only by MVE virus but also by the related Kunjin virus.

In 1952, the Hall Institute researchers described a high prevalence of MVE antibodies in blood sent to them by other researchers taken from Aboriginal people in north Queensland and from indigenous people in Papua New Guinea (as well as in the blood they had collected from near the Murray). Similarly, the IMVS researchers noted the high prevalence of MVE antibodies in the blood that had been collected by the Adelaide University expedition to Yuendumu in 1951. Miles and Howes from IMVS then suggested a hypothesis linking these endemic foci of MVE in central and northern Australia to the epidemics along the Murray and Darling rivers. They claimed that the epidemics had predictably followed heavy rains in the Northern Territory and Queensland which were associated with the movement of wild water-birds from these areas to the Murray-Darling. The disease was then thought to be transmitted to humans along the Murray from these birds by mosquitoes. This hypothesis remained undisputed for some time but has been increasingly questioned since the 1974 epidemic.
The results of the testing of Aboriginal blood were crucial to this hypothesis. The NH&MRC granted IMVS further funds to re-collect and further investigate the blood of Aboriginal people in three of the locations the researchers had visited for their polio research in 1952. The locations were chosen because they had particularly high prevalences of MVE antibodies in 1952 (although the location with the highest prevalence in 1952, Port Keats, was not chosen perhaps because it was much more difficult to visit from Adelaide than the locations chosen). IMVS researchers returned to Beswick, Murray Downs and Elkedra at the end of 1954. But was Aboriginal blood preferred to non-Aboriginal blood?

The blood of a small number of non-Aboriginal people was also examined during this research in 1954. Similarly, 14 Aboriginal residents of the Gerard Mission were included with more than 200 other residents of Upper Murray in South Australia when a seroprevalence study was repeated in this area in 1955. It seems that the researchers had no particular preference for Aboriginal or non-Aboriginal blood. Their use of Aboriginal blood seems largely to have been determined by opportunism – there was available serum that could be examined that had been collected by Adelaide university expeditions and as part of other IMVS research. The blood just happened to be from Aboriginal people.

Whilst there may have been no preference for Aboriginal blood, readers of the *MJA* articles might have noticed that blood was collected differently from Aboriginal people. The White authorities controlling Aboriginal people’s lives had to be approached and later thanked at the end of the articles. The White power over Aboriginal lives was clearly acknowledged and little Aboriginal agency was mentioned. In his 1957 *MJA* article on the Upper Murray study, Peter Warner thanked the ‘willing volunteers’ of the various towns but only thanked Pastor Samuels for ‘permission to visit’ the mission and did not thank the Aboriginal volunteers. IMVS researchers returned to the mission in 1957 to re-confirm their results. It is perhaps a pity that few of the medical readers of the *MJA* would have read the report of this research in the *Australian Journal of Experimental Biology and Medical Science*. Warner and his co-authors, very unusually for their time, not
only thanked the Pastor again but wrote that they were also ‘indebted to ... the staff and volunteers at the Gerard Mission’ (p. 63). 38

The much higher prevalence of MVE antibodies found in Aboriginal people than in non-Aboriginal people in the upper Murray required an explanation by the researchers. Like their colleagues investigating polio, they avoided a racial explanation but they did accept that such an explanation could not be excluded. Instead they claimed that the higher seroprevalence was due to the Mission’s proximity to the river and Aboriginal people’s greater exposure to wildlife and biting insects. 37 Similarly, in the MJA in 1961, West Australian researchers did not even include the possibility of an inherited racial explanation for the higher seroprevalence of MVE antibodies in Aboriginal than in White residents of the Kimberley. They wrote:

> It is not known what factors are responsible for this difference, although several may be involved – for example, mobility, socio-economic considerations (including vector control), location with respect to rivers and so on. (p. 783) 55

The IMVS MVE research in the Northern Territory explained the high seroprevalence in Aboriginal people as due to their residence near a hypothesised endemic focus of MVE. They made nothing of their subjects’ Aboriginality. This may have been because these scientists were primarily concerned with identifying generalisable features of the biology of this important new virus which had caused so many deaths in the early 1950s. But as well as this, there was an obvious distaste for racial explanations of difference in the IMVS research of both polio and MVE.

Elazar Barkan has described the changes in the science of race in British and American biology and anthropology. 56 Whilst he has described the gradual rise of anti-racism in science between the wars, he has also written that the massive rejection of race-based theories and rise of cultural determinism in the 1950s were a response to the evils of Nazism. The UNESCO statements dismissing the biological foundations and predictive value of race were the most dramatic symbols of this shift. Such changes were not unnoticed in Australia. Before I discuss Barry
Christopher’s letters to the *MJA* editor in chapter eight, that refer at times to the UNESCO statements and the recent spectre of Nazism, I now investigate how the Board for Anthropological Research operated in such a changed intellectual and social climate.

**The post-war expeditions - ‘no types of data should be excluded’**

The Board for Anthropological Research in Adelaide recommenced its expeditions after the war in 1951. Transport had changed dramatically. Camels were no longer required and transport to the field sites was by air, rail and along much improved roads. Additional funds were now also available from the Wenner Gren Foundation of New York as well as from Adelaide University itself. After this first expedition, the Anatomy Department led expeditions each year (except 1960) from 1955 until 1964 (see figure 7.1). More than twelve hundred Aboriginal people were examined during these expeditions which included repeated visits to Yuendemu and Haast’s Bluff in Central Australia and single visits to Maningrida and Beswick in the Top End, Kalumburu in northern Western Australia, and Yalata in South Australia. At the same time, the university’s Dentistry Department made repeated visits to Yuendemu investigating changes over time in oral health and diet.

Andrew Abbie was appointed Elder Professor of Anatomy at the University of Adelaide in 1945. He had made significant contributions to neuroanatomy before he arrived in Adelaide. In Adelaide, his attention turned to physical anthropology. After his first expedition in 1951, Abbie took on a leading organisational role in the Board’s expeditions although Cleland remained involved both as chairman of the Board and as an expedition member for many years after his retirement at age 70 from the University in 1948. Abbie was not only the member of many academic committees but similarly to Cleland was also the Chairman of the South Australian Board of Aboriginal Affairs.
Figure 7.1. Map of post-war expeditions by the Anatomy Department of Adelaide University

After the war, the *MJA* did not publish any reports of the Board’s expeditions as it had done in the 1930s. However careful readers would have noticed that any *MJA* research articles arising from data collected on these post-war expeditions all briefly acknowledged that the data had been collected on these expeditions. Andrew Abbie described the physical anthropology done on these expeditions to the 1961 Conference on Aboriginal Studies at the Australian National University in Canberra.

Direct observations include some forty on non-metrical characters and as many more metrical with the calculation of about twenty indices. Blood pressures are taken. Samples of hair are collected from all subjects and, where feasible, sputum and samples of blood grouping and for haemoglobin, haptoglobin and chemical assessment — to this we must now add transferrin and other serological studies. Samples of urine have been collected for sodium and other metabolite and steroid estimates: the results so far have proved unsatisfactory but this must be persevered with. An X-ray machine is always taken into the field for determination of epiphyseal appearance and union, and times of dental eruption, which are both important in the growth pattern which we are paying particular attention. And since 1957 we have added to our armamentarium Harpenden skinfold calipers to measure localized adiposity and a reflectance spectrophotometer for more accurate recording of skin pigment. In addition we take many photographs. Every subject, naked, is photographed in front and side views orthogonally at thirty feet, with a telephoto lens against a ten-centimetre squared screen. This provides a valuable visual record, a useful corrective for any errors in transcription and an essential contribution to somatotypology. We also take close-up portraits, full-face and profile, photographs of the ears and any other feature that might be unusual of current interest — for instance, dental peculiarities. (p. 96-97)66

The number of measurements required meant that four or five researchers were able to examine about twenty subjects on an average day. The quantity of measurements to be collected led to a continuation of the Board’s distinctive, busy style of expedition. The volume of data collected also meant that analysis of the data was slow and publication delayed. Abbie confirmed that, as before the war, the emphasis was still on collecting data ‘while there is still time’ (p.98).66
questioned at the conference about the possibility of being more strategic about what data was collected in order to make analysis more manageable and timely, Abbie was adamant in his commitment to the Board’s long-held ‘data-oriented’ approach. He said:

for the moment no types of data should be excluded, even though they may not be relevant to the recorder’s immediate purposes: namely that they are the only record of these things which other people will have to work from in the future, and we do not yet know enough to decide what to leave out. (p. 117)\textsuperscript{67}

Whilst the aims and methods of the Board’s post war expeditions were similar to those before the war, the representation of Aboriginal people in the articles published in the \textit{MJA} from the expeditions was quite different. Abbie’s work concentrated on the description of the physical characteristics of Aboriginal people – in particular their skin colour, their hair, and their head and body shape. There is almost no mention of this work in the \textit{MJA}. It was mainly published in anthropological journals like \textit{Oceania} with some briefer articles in general scientific journals like \textit{Nature} and the \textit{Australian Journal of Science}.$^{\text{44}}$

Medical (and non-professional) readers would have been most likely to have encountered this work in the second chapter of Abbie’s overview and introductory book about Aboriginal people called \textit{The original Australians}.\textsuperscript{58} This book was published in 1969, a year before he retired and six years before he died\textsuperscript{60}, and concentrated more on describing the social than the physical characteristics of Aboriginal people. Throughout the book, especially in those parts about social life, Abbie described similarities between Aboriginal people and Europeans in an attempt, that is not always convincing, to balance the differences he had just described. His clear \textit{attempt} to emphasise similarity in 1969 is in marked contrast to the pre-war emphasis on descriptions of Aboriginal people as different.
Aboriginal blood collected to understand atherosclerosis in European Australians

Blood was still collected on some of the Board's expeditions in the 1950s to describe Aboriginal people's blood groups (as discussed in chapter five).\(^{68,69}\) None of this research was published in the *MJA*. However, in the *MJA* in 1958, OE Budtz-Olsen from the University of Queensland, described the haptoglobins and haemoglobins in blood collected when he had accompanied the Board's expedition to Haast's Bluff in 1957.\(^{70}\) He seemed to hope that the distribution of the types of haptoglobins and haemoglobins would provide answers to the questions as yet unsuccessfully resolved by blood group research.\(^{70}\) Like blood groups, Budtz-Olsen claimed that the types of haemoglobins and haptoglobins were determined by single genes with low mutation rates. He suggested they would therefore provide further evidence about the origins of Aboriginal people and whether they appeared homogenous (which would imply a single origin). However in conclusion, unlike the early blood group research which had been published in the *MJA* in the 1920s with similar optimism, he refused to be drawn into any firm interpretations of the differences that he found beyond suggesting further research.

The majority of the research using the blood collected on these expeditions was used to answer quite different questions. This research was conducted by John Casley-Smith, Colin Schwartz and others at the University of Adelaide with most articles published in the *Australian Journal of Experimental Biology and Medical Science*\(^ {71-77}\) but a couple were published in the *MJA*\(^ {4,5}\) and so were likely to have been more widely read. The main focus of this research was the as yet unresolved association between diet, serum cholesterol and atherosclerosis. This research involved not only Aboriginal but non-Aboriginal research subjects and also animal experiments. Aboriginal people were included in this research because of their known lower fat diet which would enable evaluation of the effect of diet on the other variables. There had been such research involving indigenous populations in other countries in the 1950s but not in Australia. The Board's 1956 and 1957 expeditions provided the Aboriginal blood needed by the researchers.
In the first of these papers in the *Australian Journal of Experimental Biology and Medical Science*, using blood collected from the 1956 expedition to Haast’s Bluff, the researchers found lower serum cholesterol levels in their Aboriginal subjects than in non-Aboriginal controls. They concluded that this was due to the low fat Aboriginal diet. They did not give an explanation for the different changes in cholesterol with age in the two groups. Unlike the earlier research in other countries that sparked this research, they avoided suggesting that any of the differences were due to inherited or inherent differences between the two groups. Indeed, even when describing the conclusions of the earlier research they did not report it as finding ‘racial’ differences but ‘a constitutional difference on an ethnic basis’ (p. 450). In their next paper, which described serum mucoprotein levels and atherosclerosis, they did not just avoid such explanations but denied them. But again, they carefully used words other than race, referring instead to ‘an ethnic constitutional factor’ (p. 120).

In their 1958 *MJA* article, Schwartz and Casley-Smith once refer to other research ‘on different racial groups’ (p. 86) but in the rest of the article choose to use the word ‘ethnic’. This paper was based on blood collected on the 1957 Board expedition from ‘24 male nomadic tribal full-blooded Pintubis’ from Haast's Bluff and ‘26 full-blooded subjects, living in the vicinity of Alice Springs, who had been “urbanized” for five years or more, having partly acquired the living and dietary habits of white Australians.’ (p. 84) These two groups were compared with a group of European controls. The intermediate cholesterol levels in the Aboriginal people from Alice Springs which were associated with an intermediate fat intake confirmed their earlier conclusion that differences between Aboriginal and non-Aboriginal subjects were due to diet.

I believe that it is possible that the authors’ description of both Aboriginal groups as ‘full-blooded’ suggests that they had not entirely abandoned racial understandings of Aboriginal health. It is possible that they were controlling for the effects of race, either consciously or unconsciously, by excluding Aboriginal people of mixed descent. However, the consistent avoidance by the same authors in other papers of such racial explanations undermines this argument. Even in Casley-Smith’s series
of four papers describing a variety of measurements of red and white blood cells\textsuperscript{71-74} and in the \textit{MJA} paper describing serum proteins\textsuperscript{3}, environmental (especially dietary) not racial or genetic explanations are given for the differences between Aboriginal and non-Aboriginal subjects.

Scientists were not the only group with a distaste for ‘race’ after the war. Paul Hasluck, the Federal Minister for Territories from 1951 to 1963, attempted to avoid and remove all mention of race (and specific mention of Aboriginal people) in the 1953 \textit{Welfare Ordinance} which was designed to continue the legislated control of Aboriginal lives in the Northern Territory.\textsuperscript{78} Instead of referring to Aboriginal people, it only referred to ‘wards’. In order to fulfill its purpose, without using the language of race, more than 15,000 Aboriginal people had to be rapidly classified as ‘wards’ in the \textit{Register of Wards} (better known as the ‘Stud Book’).\textsuperscript{78,79} Hasluck saw Aboriginal people as having social problems amenable to change and improvement rather than being a fixed inferior racial group.\textsuperscript{78} This local political context as well as the international scientific context of the UNESCO statements may have influenced the researchers’ avoidance of racial explanations.

These were the heydays of the assimilation era, and most medical research articles in the \textit{MJA} implicitly accepted the goal of the assimilation of the Aboriginal population. Abbie was a rare exception to this rather unquestioning acceptance of the goal of assimilation. Whilst he accepted that such biological and cultural assimilation was possible and was occurring already, he told \textit{MJA} readers in 1960 of his reservations.

However – and this may come as a shock to some Europeans – that is not what the average aboriginal wants. He does not accept the implication of inferiority. (p. 146)\textsuperscript{8}

In \textit{The original Australians} he also questioned the politics of ‘\textit{Our plans for their future}’ (p. 256)(his italics) and supported the recent establishment of Aboriginal councils on some reserves.\textsuperscript{58} In spite of these comments, Abbie was politically conservative like his predecessor Cleland. He favoured gradual change with
continued government paternalism (in seeming contradiction to his support for what might now be read as Aboriginal self-determination). He strongly opposed criticism of this paternalism as ‘colonialism’ and opposed criticism of the government’s efforts in Aboriginal affairs. In a recent interview, Ted Milliken, the Northern Territory Assistant Director of Welfare in the 1950s and 1960s, also expressed dismay that researchers always felt the need to be critical of the government’s work. A little surprisingly, he made special mention of Abbie in this general rebuke of researchers. Abbie had colourfully described such critics of the government as ‘whites of the perpetually frustrated type who are in a state of continual protest and find the Aboriginal cause a convenient outlet.’ (p. 255)

Although Abbie did not name him, Barry Christophers was the most prominent medical critic of government policy and administration of Aboriginal lives in the 1950s and 1960s and will be discussed in the next chapter.

Schwartz and Casley-Smith’s research was not really about Aboriginal people’s health. The report in the MJA of Casley-Smith’s paper at the 1958 ANZAAS meeting in Adelaide, which was titled ‘Blood Pressure, Serum Cholesterol and Atherosclerosis in Australian Aborigines’, did not begin with a discussion of Aboriginal people’s health. Instead it began with the statement that ‘two of the chief causes of death among Europeans were high blood pressure and atherosclerosis’ (my italics).

Indeed, an important part of the researchers’ argument was that atherosclerosis was rare in Aboriginal people. The readers of this research would have read the differences in serum cholesterol caused by diet as generalisable to other populations. Schwartz and Casley-Smith made this clear by combining in one MJA article the description of the blood collected on the Board’s 1957 expedition with the comparison of serum cholesterol levels in European subjects with and without myocardial ischaemia.

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b This assertion was based on the finding of ‘little or no aortic atherosclerosis’ in an unpublished series of 44 autopsies. In recent years there has been increased attention on the investigation of cardiovascular disease in Aboriginal people but now this is because it is more common than in non-Aboriginal people.
Like the use of Aboriginal blood by MVE researchers at IMVS, we can see the Adelaide University researchers opportunistically using Aboriginal blood collected on the Board’s expeditions to answer questions about the biology of a disease that mainly affected the non-Aboriginal Australian population. In this case it was the role of diet in atherosclerosis. The opportunistic use of this Aboriginal blood collected by the Board in Central Australia suggested to readers its easy availability to researchers and their unequal power relations with Aboriginal people as mentioned by Cleland in the 1930s (and discussed in chapter five).

The representation of the changing research encounter

Unlike Cleland’s frequent discussion of his perceptions of Aboriginal people’s response and participation in the Board’s pre-war expeditions, there are few such comments in the publications from the post-war expeditions. These few comments together with archival material demonstrate how much the research encounter had changed since Cleland’s pre-war descriptions. In 1969 in the preface to *The original Australians*, Abbie paid ‘tribute’ to the 1200 Aboriginal people who participated in his research for their ‘tolerance’ (p. 14)\(^\text{58}\). The portrayal of Aboriginal participation as bemused (and amused) tolerance was also evident when he later described a new ceremony on a return visit to Barrow Creek:

In this ceremony one participant went up to another, looked him over carefully and went through the motions of writing something down; then both burst into laughter. Gradually it dawned on the anthropologists that this “paper corroboree” was a parody of their own behaviour on the earlier visit. (p. 143)\(^\text{58}\)

Abbie described this ‘paper corroboree’ to demonstrate Aboriginal secular ceremonies and Aboriginal humour. Gillian Cowlishaw and others have suggested a more serious intent in Aboriginal people’s use of writing, books and forms as metaphors for colonial power and institutions.\(^\text{81}\) Writing and paper were viewed as the tools of colonialism and hegemonic power. In contrast, Abbie’s representation of the ceremony as merely enjoyable parody was consistent with Cleland’s earlier
descriptions of rather passive Aboriginal submission to the research process. However, later in his book, there was a stronger suggestion of Aboriginal agency and a need for the researchers to acknowledge this. Abbie admitted an obligation to negotiate with the old men who he felt held power over other Aboriginal people.

I made no progress with my work until I had the elders on my side; that achieved, the whole tribe – men, women and children – co-operated smoothly. (p. 190)  

Other remarks supported this description of Aboriginal agency but I do not believe Abbie was justified in claiming that this always resulted in smooth co-operation. The research encounter had definitely changed since the war. Whilst he did not discuss changed Aboriginal responses to blood collection, Abbie did describe in *The original Australians* their changed attitudes to clothing and nudity – and the requests for them to be photographed naked. He wrote that it was now ‘much harder to persuade an Aborigine of either sex to undress for an anthropological examination than it is to persuade a western actress to undress for a publicity photograph’ (p. 65)  

How much the encounter had changed is even clearer in the correspondence between the Northern Territory Administration and the researchers which is described in the next section of this chapter. The scientific publications only showed glimpses of difficulties for researchers – and Aboriginal agency - in collecting their samples. The only direct reference in the *MJA* to the collection of blood samples in this post-war research came not from a Board expedition but from the IMVS serological survey for polio in 1952.

Usually the natives were not reluctant to give samples of blood and we failed to collect our minimum number at only one centre. However, we did have difficulty in persuading mothers to bring in the children of the nought to four years age group, and our numbers in this important group were regrettably small. (p. 774)  

This description of Aboriginal people as ‘not reluctant’ was subtly but definitely different from either Cleland’s pre-war representations or the contemporary comments in a 1957 *MJA* article about a haematological survey in the Warburton ranges in Western Australia. The Western Australian researchers wrote: ‘The
aborigines were most cooperative in submitting to vein puncture'. (p. 605)82 The Adelaide researchers admitted to difficulties in collecting blood from very small children and also told MJA readers of difficulties in collecting urine samples. These difficulties were largely portrayed as a misunderstanding by Aboriginal people of the researchers' requests.70 The researchers wrote nothing more for their MJA readers about difficulties in collecting blood samples.

Slightly more information is available in John Casley-Smith's four part series called 'The haematology of the Central Australian Aborigine' in the Australian Journal of Experimental Biology and Medical Science.71-74 This series was based on the examination of blood collected on Board expeditions to Haast's Bluff in 1956 and 1957; in 1957 they also collected blood from Aboriginal people near Alice Springs. The series of articles described blood collected from only 20 to 30 people in 1957 at Haast's Bluff compared with about 100 people in 1956 (the numbers vary between articles which might be innocently explained as caused by insufficient blood collected from some people for all tests). There were similar numbers of men and women in 1956 but only men in 1957. The researchers appear to have responded to some gender-related preferences by their Aboriginal subjects in 1956 as Casley-Smith acknowledged in the first three papers that 'Sister P. Kriewaldt drew the blood from the aboriginal women.' (p. 35)71 The researchers made no comment in the MJA about the lower total number and the absence of women subjects in 1957. Their only comment appeared in the final paper in the four part series in the Australian Journal of Experimental Biology and Medical Science, where Casley-Smith stated:

Unfortunately, owing to religious difficulties, it was impossible to obtain blood from the women. (p. 38)74

The research encounter could no longer be simply characterised as either passive Aboriginal subjects submitting to powerful researchers or even Aboriginal people
willingly exchanging their participation for desired items of trade. This may explain in part the post-war researchers’ relative silence on this issue compared with Cleland’s pre-war prolixity. The research encounter was now difficult, messy and complicated—and perhaps best not discussed. The problems encountered at Haast’s Bluff in 1957 would lead to considerable correspondence, meetings, and tactful negotiation between the researchers, the Northern Territory Administration and the Federal Minister Paul Hasluck. The description of this correspondence that follows tells us a great deal about the different views of the research process.

Research as a nuisance and of very little real value

In the report of the 1957 Haast’s Bluff expedition sent to the Northern Territroy Director of Welfare, Abbie asserted that ‘Taken overall, the expedition must be accounted one of the most successful we have conducted.’ (p. 3)Meanwhile, Jeremy Long, the acting superintendent of the Haast’s Bluff Settlement, was complaining to the Welfare Branch in Alice Springs about the ‘nuisance value’ of the researchers’ 1957 expedition and the ‘slight importance’ of their research to the Northern Territory Administration and to ‘assisting the adjustment of the aborigines and their welfare.’ Long complained about the poor communication with the researchers and their poor dissemination of the practical implications of their findings. Some of the research work was exempted from this criticism as ‘harmless’ (such as Tindale’s social anthropology and Cleland’s work on bush foods) and some incidental services provided by the researchers were praised (such as medical services like a post-mortem by Cleland and the use of the researchers’ X-ray equipment).

Conditions at the Settlement in 1957 were rudimentary and difficult and child mortality was very high. It is not surprising that Long complained about what he perceived was a ‘stream of visitors’. In twelve months there had been two Board

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There is no published reference to post-war expeditions trading items for participation in research. Although Abbie did say at a meeting with the Northern Territory Administration and other researchers that this practice had continued after the war.
expeditions led by Abbie, a medical survey by the Northern Territory Department of Health led by John Hargrave, as well as many visits by other groups. Long then described mounting Aboriginal opposition to compliance with the requests from these groups, including the poor response to Abbie's 1957 expedition in comparison to the 1956 expedition:

Attempts were made to examine and blood test the local adults but these were reluctant and it was only through the good offices of Nosepeg who organised the less sophisticated Pintubi that the party had any success at all.

Abbie's description and interpretation of these events in his report to Long's superiors in Darwin – which did not appear in his articles for medical readers – was somewhat different. Abbie claimed that the Pintubi participated with 'no objection at all' unlike Long who implied they were more easily convinced or duped because they were 'less sophisticated' than local Aboriginal people. Abbie reported:

Blood collecting was more difficult than last year. Then, most of the local adults gave their blood freely but this year they refused. The desert Pintubis made no objection at all but their numbers were insufficient for the purposes of the haematologists. Consequently, after some 40 blood samples had been collected Dr. Budtz-Olsen and Mr. Casley-Smith returned to Alice Springs where, with the cooperation of Dr. Ireland, they secured an adequate number of samples. (p. 2)

Long was even more concerned that some of this Aboriginal resistance to White authority had spilled over to his own work. This led him to report that he had once been forced to explain that he 'was not connected with the 'Government' (a rather odd remark from a Department of Welfare officer) and that we would not ask anyone to submit to examination.' The Director of Health in the Northern Territory, Dr AH Humphry, largely agreed with Long's criticisms and added a concern that the refusal to give blood to researchers may be extended to a refusal to having blood taken by doctors in cases of individual medical need. Whilst Long

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4 In *The original Australians*, Abbie also explained the role of this prominent Pintubi man in facilitating the cooperation of the Pintubi with the researchers. 58
had been cautious in judging the scientific value of the research, Humphry was less circumspect. Although he acknowledged Abbie and Cleland were very 'distinguished men', he felt they had gone too far in their research. He wrote:

> there is no doubt that aborigines, and in particular aborigines in certain areas, suffer from a surfeit of examinations and investigations, many of which appear to have very little real value. ... one wonders what real value there is in the extensive photograph, X-ray of wrists and ankles, examination of the pigment of the skin and hair, and the measurement of skin thicknesses with calipers.\(^8^7\)

Humphry also complained about the lack of communication by the researchers with his department. He suggested that future researchers in the Northern Territory be required to seek permission from both the Director of Health and the Director of Welfare and provide a full report after their research was completed.\(^8^7\) Long had asked that Haast's Bluff be spared by researchers for a while or at least some limits be placed on the number of future researchers.\(^8^5\) Professors Abbie, Cleland, Campbell and Stanton Hicks from the Board for Anthropological Research were told of the complaints and the proposal that they be required to send plans for future research to be assessed by the Northern Territory Administration and that Haast's Bluff and Yuendemu be spared any visits until the end of 1958 at least. Reports of completed research would also now be expected.

The researchers expressed their surprise and their disbelief of the criticisms, with both Abbie and Campbell protesting that their departments could not be to blame. They then explained to the Minister the importance and utility of their research (and their willingness to provide reports of their research). Both Abbie and Campbell emphasised the disappearing opportunities for their research. They were no longer talking about the Aboriginal race becoming extinct but about Central Australian Aboriginal people's way of life changing dramatically. In a threat that was reminiscent of earlier pleas for more research before the expected demise of the Aboriginal race, Abbie wrote of his longitudinal child growth research:
If this work is stopped now, the last chance of securing such information about the aborigines will be lost and the fault will not lie at the door of the scientists.  

He then claimed that such work was necessary as a baseline to evaluate any claims of improvement resulting from the Administration’s work. Cleland in his letter as Chairman of the Board for Anthropological Research also emphasised the utility of the Board’s research to the NT Administration.

We infer that all additions to a full knowledge of the physical and mental make-up of our natives will be welcomed as of value to the Administration. ... My personal opinion at the end of our various stays was that we had been of very definite help to the Administration in the task of assimilation, which is so difficult to solve.

Clearly the Administration did not believe that ‘all additions’ to knowledge about Aboriginal people were useful. Cleland’s response reflected a perception that the Administration favoured applied research over pure research. He attempted to dress up the Board’s research accordingly. Abbie, in his much longer response, did acknowledge that most of his work was not applied but pure research. He then defended the usefulness of such research:

This certainly has less immediate value to the Northern Territory Administration than findings of an applied nature but in the long-term view it will ultimately have far greater value to medicine generally.

It is interesting that Cleland acknowledged that one of the measures of applied research was whether it had assisted the process of assimilation. Long had also emphasised that the value of research should be measured against its contribution to the aims of the assimilation policy. It appears that applied research was being evaluated by its usefulness to the Administration and its policies rather than by its direct usefulness to Aboriginal people or their health. Perhaps this was because it was the Administration making the complaints not Aboriginal people and because it was accepted that the Administration’s work and policy were beneficial to Aboriginal people.
The politics and debates about the ethics of the balance between pure and applied science in Aboriginal health continue today. As in the 1950s, these debates focus on the usefulness of the outcomes of these different types of research. But both pure and applied research can either result in a dead end or an area of future and continuing importance. I believe that rather than being about outcomes, the debates then and now are most importantly about the ethics of power and of gambling future benefits against immediate gains. Who makes these choices between the future and the present is crucial. It does not seem ethical to me for researchers to choose to conduct research that may or may not have benefit to other or future populations on a group with less power and enormous immediate needs.

In 1958, the Northern Territory Administration took some months in responding to the researchers’ letters. The Administrator was concerned that it was thought that the Northern Territory Administration had ‘too narrow’ a view of the value of scientific research. He then tried to demonstrate the Administration’s support of research by listing the large number of recent research expeditions to the Northern Territory, especially to Yuendemu and Haast’s Bluff. Whilst he still maintained that this research did interfere with the Administration’s work, he hoped that some compromise and a ‘working arrangement’ could be reached with the researchers and that the ‘misunderstandings’ could be best dealt with at a meeting between the Administration and researchers. All agreed to the meeting, although Abbie still seemed irritated and claimed that the criticisms against him were ‘unjust’ as his department had ‘offended least’. He had also been forced by the delay to change the plans for his 1958 expedition and move it from Central Australia to Yalata in South Australia.

The meeting occurred in May 1958 in Adelaide and seemed to lead to a much improved working relationship between the researchers and the Administration. Whilst it was prepared to acknowledge the value of the ‘pure science’ research proposed, the Administration was still most interested in applied research in particular the longitudinal research by Campbell and Abbie into dental health and child growth at Yuendemu. These longitudinal studies were hoped to provide an
opportunity to describe a baseline and to evaluate the impact of social change and the assimilation program.

The researchers submitted five-year plans to the Administration, with Abbie agreeing to move his attention from Central Australia to the Top End – with the exception of his longitudinal survey of child growth at Yuendumu. He quickly turned this move to his advantage by claiming that his physical anthropological surveys would now achieve a 'cross-section of the Continent from south to north' – an achievement he would mention many times in future publications. These future publications would also carefully acknowledge the support of Hasluck and the Northern Territory Administration. Whilst readers may not have made much of these acknowledgements, Abbie wanted to ensure that the Administration was aware of them and told Hasluck that 'references in our publications to the assistance afforded to us by Commonwealth Departments are exciting favourable comment overseas.'

Apart from the fulsome acknowledgements, no references were made in the publications arising from Abbie's expeditions after the Haast's Bluff 1957 expedition to his recent problems associated with the research encounter. However, Abbie’s report to Minister Hasluck of the first of these expeditions does discuss the research encounter in this new location (Maningrida) and new political context. Whilst it was no longer mentioned in the Board’s publications, Abbie’s report also mentioned the use of the pre-war practice of trading 'suitable' gifts for reluctant participation in research.

The natives were a little shy at first but this was overcome with suitable gifts and ultimately a very satisfactory number came forward. ... Partly because of the absence of Professor Cleland, but also because we did not wish to try the people too hard on our first encounter, we did not collect any blood samples.

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* Abbie affirmed that this practice had never lapsed two years later.
Cleland had been forced to withdraw at the last minute following an accident – he was 81 years old at the time. The remark about blood collection suggests that Abbie was aware that this was still a very sensitive element in the research encounter. This may reflect in part an acknowledgement of Aboriginal beliefs about the meaning of blood – both its sensitive religious meaning and its central role in an individual’s health. Abbie’s reference in a lecture published in the *MJA* to the ceremonial role of blood and the previously mentioned reference to ‘religious difficulties’ as the reason for being unable to collect blood from women at Haast’s Bluff in 1957 suggest that researchers were aware of some of these sensitivities (at least in desert Aboriginal people). The researchers and bureaucrats would have also been aware of the meanings of blood and blood taking in their own European cultures. Nearly forty years later, a Central Australian Aboriginal organisation would invoke a very European image, the vampire, in their complaint about collection of samples from indigenous peoples by the Human Genome Diversity Project.

A convenient bulk reservoir

The Administration’s concerns about the collection of blood from Aboriginal people would prevent Abbie collecting blood on his next Northern Territory expedition – to Beswick in 1961. This time it followed the refusal of the Northern Territory Administration to give permission to RL Kirk from the University of Western Australia to collect blood samples from ‘400 full-blood Aborigines’ for serum protein and blood group research. This blood was to be used not only by Kirk but also by other researchers who would investigate viral antibodies and vitamin B₁₂ levels. This opportunistic use by several researchers of Aboriginal blood was similar to that which had earlier occurred in Adelaide. It was the perception of opportunism combined with the perception of little scientific or practical importance that seemed to upset the bureaucrats. This was most clearly articulated by the Director-General of Health, who wrote:

I would adhere firmly to the policy that approval should not be given to any project of blood sampling which desired to use the native population of the Northern
Territory, simply because it was a convenient bulk reservoir from which numerous samples could be drawn.99

Unfortunately for the researchers this was not an unreasonable reading of the representations of the research encounter in the publications of the Adelaide blood-based researchers discussed in this chapter. Their careful use of all of the blood collected in many different investigations by many different researchers to answer questions unrelated to Aboriginal people’s health fostered this view. This view, held by the most senior health official in the country, was probably shared by many other readers of the medical literature. When Hasluck became involved, he telegraphed the Administrator in Darwin for information but made his position plain:

TENTATIVELY I MYSELF AM NOT DISPOSED TO DIRECT THAT WARDS CAN BE SAMPLED LIKE A HERD OF CATTLE PERSONAL WILLINGNESS OF NATIVE PEOPLE TO ASSIST IS ESSENTIAL.100

The Northern Territory Director of Welfare, Harry Giese, was concerned that his permission for the researchers to visit the Settlements or Missions to carry out their research would put undue pressure on Aboriginal people to consent to blood collection.101 He and others in the Administration were very concerned that this consent be given freely. It was also thought that it would be difficult for the researchers to explain the purpose of taking blood – particularly from healthy people. And again he worried about the implications for later medical work.101

At the meeting that was eventually held between researchers and the Northern Territory Administration following their repeated emotive representations to the government expressing concern about the ‘ban’ on blood-based research, the researchers stressed that blood was only given voluntarily and research subjects were duly rewarded.83 They believed there was no compulsion. Giese then said that he would like to see evidence of individual consent by Aboriginal research subjects either in writing or by a thumb print. He also offered to make his welfare officers available to explain this consent procedure to Aboriginal people. The blood grouper

1The slightly confusing absence of punctuation in the quotation is as in the original telegram.
Kirk disagreed. He said that consent was best obtained from a group of subjects after a demonstration of the procedure and was a simple process that could be left to the researcher. Kirk and other researchers emphasised that similar research was also occurring in White Australians and other populations. Kirk complained that:

the Director of Welfare tended to see the problem purely from the point of view of the aborigines. ... He (Kirk) believed that because of his concern for the people Mr. Giese tended to over emphasise the human rights angle.

Whilst Giese did also mention the disruption to his Department's work and officers, his main concerns were expressed as related to his role as the legal guardian of his wards – as Aboriginal people were referred to in the discussion and Northern Territory legislation. He was clearly proposing a shift in power in the research encounter.

In the Northern Territory in the 1950s, some power had moved from the researchers. It had moved not to the Aboriginal research subjects but to their legal guardian, the Director of Welfare. Giese's proposal for individual consent, which would have involved a further shift in power, this time to Aboriginal people, was not accepted at the meeting. The meeting merely proposed that future research proposals would not only be reviewed by the Northern Territory Administration but also by the NH&MRC which would assess the scientific importance of the research. Whilst the representations of Aboriginal people and their health may have been influenced by this change in the researchers' power, there was as yet not a great deal of obvious evidence in the scientific publications beyond the researchers' effusive acknowledgements.

Forty years later, I discussed these disagreements between the Northern Territory Administration and researchers with Ted Milliken. He had been the Assistant

8 Ted Milliken has recalled that the purpose of requiring researchers to obtain individual Aboriginal consent was to treat Aboriginal people the same as Whites not just to give Aboriginal people more power. His emphasis on the same rather than special treatment for Aboriginal people was very much the goal of the assimilation policy.
Director of Welfare in the 1950s and 1960s and was particularly responsible for research matters. He was interviewed in his consulting rooms in Darwin where he now practices as a private psychologist. He explained that the Welfare Branch supported the need for research involving Aboriginal people. When opposition to a researcher occurred, it appears to me to have most frequently followed criticism of the Administration by the researcher. Milliken said that Giese took this criticism of the Welfare Branch very personally, as he also took his responsibility for the Welfare Branch’s enormous task. So Milliken said that whilst researchers produced some interesting and useful information and needed to be supported in their work by Welfare Branch staff, they also needed to be controlled to minimise criticism of the Branch.

There is archival evidence of not only opposition but also considerable support given to researchers by staff of the Northern Territory Department of Health and Welfare Branch throughout the 1950s and 1960s. Local staff facilitated researchers’ entry to settlements and missions, assisted with their accommodation, and collected supplementary specimens and medical information from hospital patients for researchers. In 1953, the NT Department of Health collected more than five hundred faeces specimens from Aboriginal hospital patients and then sent them to Sydney for a polio researcher at Royal Prince Henry Hospital. The transport of these specimens required considerable local logistic support including importing dry ice from Singapore (with special customs clearance) for research which produced few interpretable or publishable results.

Blood pressure and Europeanisation

Whilst Abbie’s expeditions to Maningrida and Beswick in 1959 and 1961 were not able to collect blood samples, they did take Aboriginal people’s blood pressures and then published their results in the MJA. In a 1959 MJA article, Casley-Smith had already analysed the blood pressures of Aboriginal people at Haast’s Bluff measured in 1956 and 1957 and in earlier published and unpublished surveys. This work was

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8 The Director of Welfare, Harry Giese, died in February 2000 and was not interviewed.
a continuation of his previously discussed research with Colin Schwartz and others on the association between diet, serum cholesterol and arteriosclerosis.

One of the published surveys, which Casley-Smith included, was a 1937 report in the *MJA* by Jarvis Nye from a visit to Lockhart River Mission on Cape York. Nye had examined older Aboriginal people at the Mission to determine whether an increase in blood pressure with age was 'normal'. Nye largely explained the absence of high blood pressures in the older Aboriginal people he examined as due to their 'simple' and 'primitive' lifestyle. This was contrasted with the high blood pressure and arteriosclerosis of 'civilized' communities. His hypotheses about the important protective elements of the Aboriginal lifestyle were very tentative – intermittent fasting, living in a 'state of communism' without 'selfishness', and the absence of clothing and environmental lead. He had rejected his contemporaries' theories about the causative roles in high blood pressure of tobacco, sepsis, and a high protein diet.

The three post-war *MJA* articles by Adelaide researchers about Aboriginal people's blood pressures concentrated on the possible effect of different diets. These authors categorised different Aboriginal groups, their lifestyles and diets according to a social and environmental gradient, even though they were really only talking about the effect of diet. Some were described as 'primitive', 'pristine' or 'original'; others as 'semi-civilized', 'semi-urbanized', 'partly Europeanized'; and some diets were described as 'European'. These were not fixed racial categories but implicitly accepted the notions of social change and amelioration of the assimilation policy. The categories also contained strong elements of Herbert Spencer's nineteenth century ideas of social evolutionism with its implicit hierarchies (see figure 7.2).

The researcher's blood pressure results could not be neatly correlated with their very crude dietary and social gradient. In the paper from their Maningrida visit, Abbie and Judith Schroder found low blood pressures in spite of a high fat intake. They

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1 Stocking has made a convincing case that these ideas should be attributed to Spencer not Darwin and so I have avoided the more commonly used term 'social Darwinism'.

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Figure 7.2. Progress through a social not racial hierarchy of housing

Source: Abbie AA. The original Australians. Wellington, Auckland and Sydney: AH & AW Reed, 1969, p. 64. Reproduced with permission.
hypothesised that this may have been due to a slightly more complex interaction between dietary fat and blood pressure than their simple gradient. In Maningrida, much of the fat was from marine animals which had recently been shown to have less impact on serum cholesterol. The very different results from Queensland could still not be explained. The Beswick results were also contradictory. In spite of stressing the homogeneity of Aboriginal people based on his own physical anthropological research, Abbie and his co-authors finally suggested a genetic explanation for the variation in blood pressures in different Aboriginal populations not adequately explained by his crude measurements of diet (and environment).

It therefore seems conceivable that genetic factors, besides having a possible direct effect on predisposition towards a certain pressure level, could, through their expression in physical characters, have an indirect action on the blood pressures of ethnic groups. (p. 289)

This concluding sentence easily could be read as a return to a racial explanation of disease causation. However its context makes it clear that it was based on a tentative hypothesis about genetic variation within the Aboriginal population. It also avoided mentioning racial groups. Nevertheless, it sits a little oddly with the other research at this time that emphasised the similarities and generalisability of the findings in Aboriginal people to non-Aboriginal Australians. Rather than finish this chapter with these hints of a return to the pre-war representations of Aboriginal people, I now briefly discuss John Cawte - a researcher associated with the Adelaide University expeditions with strong links with both pre-war research and present day Aboriginal health research. His research publications in the 1960s testify to further important shifts in the research encounter.

**Continuity and change**

John Cawte published two *MJA* articles in 1964 and another in 1965 based on research performed when he accompanied expeditions by the Board for Anthropologist Research to Kalumburu and Yuendumu. He had studied
medicine at the University of Adelaide and was clearly influenced by the Professor of Anatomy, Andrew Abbie. In Cawte’s book *Medicine is the Law* which included this early research, he fondly referred to Abbie’s influence and encouragement and highly praised his book *The original Australians*. Abbie also nominated Abbie as one of his referees in a research grant application in 1965. Abbie mentioned this relationship by referring to Cawte as ‘a former student of mine’ (p. 150) in *The original Australians*.

Cawte, a psychiatrist, had been the superintendent of Enfield Hospital in Adelaide and has written that he felt particularly frustrated in his care of Aboriginal psychiatric patients referred there from the Northern Territory and northern South Australia. Later, after he moved to the University of New South Wales in Sydney, with Abbie’s encouragement, he began to visit Aboriginal communities in central and northern Australia in the university vacation. At first he accompanied the Board’s expeditions, later he would organise his own expeditions. He would continue these trips until 1990 and in 1996 published *Healers in Arnhem Land* based on his observations on annual summer university vacation visits to Galiwin’ku on Elcho Island from 1970 to 1990.

The first of Cawte’s *MJA* articles concentrated on a description of mental health problems at Kalumburu using categories from Western medicine. The next two articles described Aboriginal doctors and medical beliefs at Yuendumu and Kalumburu. His work on these and other early expeditions and his particular approach to this research are more fully explained in his books *Medicine is the Law* and *Cruel, Poor and Brutal Nations*. Cawte’s research processes seem very similar and greatly influenced by the approach adopted by the Board. His approach was most clearly articulated in his book describing his three week university vacation expedition to Mornington Island Mission in 1966.

The researchers first approached and established a rapport with the local Aboriginal doctors/ healers. With the help of these men and other informants (including non-Aboriginal residents), they then identified all the people with mental illnesses, and described the socio-cultural context and the capacity of the community to address
these problems. Whilst the Mornington Island study contained a great deal of statistical information and correlational analyses based on psychological questionnaires, much more typical was Cawte's liberal illustration of his ideas using 'case histories' of individuals with mental health problems.\textsuperscript{111}

This rapid collection of data sounds very familiar. The pre-war Board expeditions had also included some psychological (including intelligence) testing as well as the examination of Aboriginal people's motor and sensory perception. Late in his life, HK Fry summarised some of the findings from this research in a paper at the 1952 annual meeting of Australasian Association of Psychiatrists that was later published in the \textit{MJA}.\textsuperscript{21} He expressed considerable doubt about validity and reliability of their results, especially those about intelligence. He suggested that the results often only reflected the level of interest of his subjects in the test. He felt that anthropology had more to offer in the understanding of Aboriginal people's 'primitive \textsuperscript{1} mental characteristics' (p. 545) which could then be used to test psychological theories.\textsuperscript{21}

Like Fry, Cawte turned to anthropology to help explain his findings, with his starting point being Aboriginal medicine.

Cawte's descriptions of Aboriginal doctors and their medicine were part of a long history of non-Aboriginal doctors writing about their Aboriginal counterparts and their medicine for a medical audience that had begun a century before with the first article about an Aboriginal person's health in an Australian medical journal.\textsuperscript{112} Like these earlier authors, Cawte emphasised the possible lessons to be learnt from Aboriginal medicine by non-Aboriginal doctors. And like earlier allusions to the imminent extinction of the Aboriginal race, Cawte emphasised the importance of his work with Aboriginal 'doctors' by stressing the possibly imminent eclipse of these Aboriginal 'doctors' and Aboriginal medicine.\textsuperscript{107}

In spite of all these similarities and continuities with earlier research, the research encounter and Aboriginal people were portrayed quite differently in Cawte's

\textsuperscript{1} Cawte would also describe his Aboriginal health research as the 'study of primitives' (p. xiii) but expressed some 'hesitation because of some misleading connotations' (p. xiii)\textsuperscript{108} about use of the word 'primitive'.

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publications. Aboriginal agency was emphasised rather than hidden or ignored. In 1964, he became the first author in the *MJA* to formally acknowledge the cooperation and assistance of his Aboriginal research subjects. He also named and acknowledged in the *MJA* all the Aboriginal doctors he interviewed and the Aboriginal interpreters he used at Yuendumu. In a related article in the *British Journal of Medical Psychology* his co-researcher, an Aboriginal assistant teacher Nari Djagamara, was also acknowledged as a co-author. At the same time as accepting the legitimacy of this role for Aboriginal people, he began to reflect on his own role. He was aware of the potential limitations of his non-Aboriginal interpretations of Aboriginal health beliefs. Before providing a transcription of an Aboriginal explanation of *malgri* on Mornington Island he wrote of his own work:

These are the reflections of a Westerner – of one who, contemplating the ideas of a non-Western people, sometimes feels enslaved by his own analytic style of reflection. (p. 119)

Cawte’s 1964 *MJA* article about Walpiri doctors at Yuendumu included three photographs. There had been 20 photographs in three *MJA* articles from the 1950s describing Aboriginal medical practices. In these earlier articles and photographs the Indigenous practitioners were un-named, exotic (to the Western eye) elements and instruments were photographed and emphasised, and the limited hygiene of these events was contrasted with Western surgery and obstetrics. The authors of the associated articles described these Aboriginal medical practices as of interest to their medical audience as they were a ‘primitive’ version of Western medicine.

In marked contrast, Cawte (and his co-author MA Kidson) used two photographs of named Walpiri doctors wearing Western clothes standing or sitting in the bush (figure 7.3), and one photograph of a drawing by one of these men which portrayed

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\[k\] There were few other photographs of Aboriginal people in *MJA* articles in the 1950s and 1960s – these appeared in two articles about early European settlement of the Northern Territory (two photographs and one reproduced painting), two articles about childhood malnutrition (one photo each), and a case report of a child with albinism (one photo of the child and her mother).
Figure 7.3. *Warlpiri doctors*

one of the central concepts of their healing. Following a suggestion from Langsford, Cawte had even withdrawn two photographs of sacred objects to avoid offence to Warlpiri people should publication in the *MJA* inadvertently lead to more widespread public display.\textsuperscript{121,122} Even more than the more respectful and less patronising tone, what is most different about this article is that its authors claimed that an understanding of and an engagement with Aboriginal medicine would be of practical use to Western doctors working with Aboriginal people. Although, as Harrison has very critically written, its practical usefulness was closely linked to the contemporary policy of 'assimilation' which she derided for its ethnocentrism and cultural arrogance.\textsuperscript{45}

This explicit statement of this practical purpose of Cawte’s research seems markedly different from earlier research. He appears very concerned that his anthropological and clinical insights be used to improve the care of Aboriginal people with mental illnesses and to prevent these illnesses. This seems to support his remarks that his interest in this research grew out of his frustrations about the poor care available to those Aboriginal patients who had been referred to Enfield hospital. Cawte provided psychiatric care when he could to those Aboriginal people he examined on his expeditions and emphasised the need for local management of these patients and called for a field psychiatric service for northern Australia.\textsuperscript{105,111,123} Other researchers on Board expeditions had also provided medical care to Aboriginal people but this was only incidental to their primary obligations to add to scientific knowledge. In Cawte’s publications, it appears that this balance had changed and probably been reversed, although he did add after describing the practical value of his work that it may also 'throw some light on the basic assumptions of psychiatry.' (p. 468)\textsuperscript{105}

His criticisms and suggested improvements of psychiatric services in the Northern Territory were not unnoticed by local bureaucrats. The Director of Health, William ‘Spike’ Langsford, wrote in 1965 that he had no problems with Cawte’s ‘pure research into ethno-psychiatry’ but was very concerned about the negative publicity from his criticisms of the department’s work.\textsuperscript{124} He felt Cawte was attempting to establish himself as the psychiatric expert on the Northern Territory, which would
have ‘unfortunate consequences’ for the department. After an article appeared in *People* magazine about Cawte’s research, Giese angrily wrote to Langsford that Cawte was ‘given a permit to undertake medical research and one would expect therefore that he would restrict his writings to scientific journals.’ It seems that their tight control of knowledge about Aboriginal people was more important to these government officials than their previously stated preference for applied over pure research.

Cawte continued to publish articles about Indigenous health in the *MJA* in the 1960s, 1970s, 1980s and 1990s. But his influence on Indigenous health research extends beyond his own work. He also founded in 1977 and edited until 1989 the *Aboriginal Health Worker* – a journal for Aboriginal health workers to read with many contributions written by Indigenous people including health workers. He was also a significant influence on other researchers of Indigenous people’s health. The purpose of this brief discussion has not been to fully investigate Cawte’s ideas and influence in Indigenous health research. It has been merely to show elements within his research in the 1960s of both early Adelaide-based anthropology expeditionary research and present-day Indigenous health research.

**Conclusion**

This chapter has described the work of a wide variety of research associated with the post-war expeditions of the Board for Anthropological Research in Adelaide – virology, physical anthropology, psychiatry, and both clinical and laboratory research into the antecedents of cardiovascular disease. These researchers expressed a universal distaste for notions of biological race and racial explanations in their publications which was probably influenced by the international response of scientists to the Holocaust and by the local politics and lexicon of assimilation.

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1 He was absent overseas for one year beginning in late 1985 when Don Eastwell acted as editor. The journal changed to its present name, the *Aboriginal and Islander Health Worker Journal*, in 1990 after Cawte retired.
They instead investigated ‘environmental’ explanations of disease. These explanations included Aboriginal people’s proximity to water-birds and mosquitoes, their contact with non-Aboriginal society, their level of ‘civilisation’, and most commonly their diet. In these explanations, there was an implicit social hierarchy not dissimilar to earlier racial hierarchies except that positions were less fixed. The notion of the potential for improvement of Aboriginal people in these explanations was consistent with (but not necessarily caused by) the goals of the assimilation policy.

However, most of this research was neither about ‘improving Aboriginal people’ nor improving their health (although Cawte’s research was a notable exception). It was concerned with diseases that were a problem among non-Aboriginal Australians but thought to be rare in Aboriginal people – polio, Murray Valley encephalitis, and cardiovascular disease. This use of a less powerful group as research subjects to answer questions that were of no immediate benefit to them may have been merely opportunism but it certainly also appeared exploitative. The Northern Territory Administration intervened and placed some limits on the researchers, limits that were greatly resented by the researchers. Whilst the researchers’ power over the research process may have been curtailed, this had not yet led to increased Aboriginal power but only extended the Administration’s control over another aspect of Aboriginal lives.
References


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35. AJ Metcalfe, Director -General of Health to Secretary, Department of Territories, 7 May 1952, AA(ACT): CRS A452, 1952/179.


80. Interview with Ted Milliken, 6 November, 2000. .


83. Summary of meeting at Department of Territories, 21 September 1961, AA (ACT): CRS A452, 1961/1698..


85. JPM Long to District Welfare Officer Alice Springs, 8 October 1957, AA (ACT): CRS A452, 1957/3522..


87. AH Humphry to Director of Welfare, 29 October 1957, AA (ACT): CRS A452, 1957/3522..


89. JB Cleland to Hasluck, 17 December 1957, AA (ACT): CRS A452, 1957/3522.


92. JC Archer to Secretary Department of Territories, 17 December 1957, AA (ACT): CRS A452, 1957/3522.


95. AA Abbie to Hasluck, 14 September 1959, AA (ACT): CRS A452, 1957/3522.


121. WA Langsford to JE Cawte, 7 September 1964, AA (NT): E51/0, 1964/594.


Chapter 8

Equality, politics and dissenting voices
The letters to the editor of the *MJA* tell a different but complementary part of the story of Indigenous health in the 1950s and 1960s to that told in the previous chapter. In the 1950s and 1960s, nearly a quarter (24% or 48/200) of the publications in the *MJA* about Indigenous Australians were letters to the editor. In all the years before 1950 there had only been 13 letters to the *MJA* editor about Indigenous Australians. Three quarters of these letters in the fifties and sixties were written in response to earlier *MJA* publications. These letters provide a picture of the contemporary response of some *MJA* readers to the representations in these publications. This contemporary critique cannot be dismissed with the label 'black armband' history and the charge of making inappropriate judgements of the past using hindsight and today's values. The letters also included responses by doctors to contemporary political debates about the administrative control of Aboriginal and Torres Strait Islander lives – in particular they referred to the campaign for equal Aboriginal rights by the Federal Council for Aboriginal Advancement (FCAA). This chapter discusses these letters and in particular the 25 letters to the *MJA* editor about Indigenous people written by Barry Christophers.

**'A fair barometer of the interest of readers'**

In the preceding chapters, I have concentrated on the prominent researchers and prominent research projects in a prominent medical journal to avoid exaggerating the importance of ideas in publications that few doctors would have read. Whilst the writers of letters to the editor may not have been as academically distinguished as the researchers discussed in these earlier chapters, their letters were widely read. Three readership surveys in the 1950s and 1960s demonstrated that the letters pages were the most read section of the *MJA*.29-31

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8 A later investigation of the letters of the three most prolific letter writers about Indigenous health in these two decades – Christophers, Duguid, and Kent Hughes – revealed a further seven letters about Indigenous health not identified by Moodie and Pederson1 or by Harrison2 or included in my quantitative analyses, tables and graphs in chapter six or this chapter.3-9 If included they would take the total number of letters from 48 to 55.
These letter writers were a voice of contemporary readers of the *MJA*. They demonstrated that the academics writing research articles about Indigenous health did not necessarily reflect the views of all Australian doctors. Whilst some letter writers were also academics, this genre of medical writing was potentially open to all readers of the *MJA*. But even the letter writers were only a partial and often partisan reflection of doctors’ attitudes, as they were usually only the most passionate, those most strongly opposed or in favour of an idea. The letters suggest the range of readers’ responses but may not reflect the relative frequencies of the different responses.

The editorials marking both the fiftieth and sixtieth anniversaries of the *MJA* in 1964 and 1974 described the number of letters to the editor as a barometer of the interest of readers in a journal. The editorials stated that the *MJA* was encouraged by the gradually increasing numbers of letters sent to the editor in the 1950s and 1960s. The 1974 editorial also revealed that most of these letters were published.

Figure 8.1 shows the gradual increase in the number of letters to the *MJA* editor about Indigenous health over the 1950s, suggesting an increased interest in Indigenous health by readers. This increase occurred just before a dramatic increase in the total annual number of letters published in the *MJA*. In only a few years beginning in the late 1950s, the total number of letters increased from 150 to 400 each year (figure 8.2). It is still possible to see an increase in the number of letters about Indigenous people as a proportion of all letters – from a small fraction of a per cent at the start of the 1950s to about two per cent at the end of the decade. (figure 8.3).

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*b* Including six letters not cited by Moodie and Pederson or by Harrison nor included in my quantitative analyses in chapter six.

*c* These graphs only identify letters cited by Moodie and Pederson or by Harrison. They do not include the other seven letters written either by Christophers, Duguid or Kent Hughes.

*d* These numbers were calculated from the *MJA* volume indexes which separately listed all letters under a single heading – ‘Correspondence’. This separate listing ceased in 1966 which explains why these graphs do not continue until the end of the decade.
Number of Indigenous health letters to the MJA editor, 1914 - 1969

Figure 8.1

Number of letters to MJA editor, 1940-1966

Figure 8.2
Percentage of total letters to *MJA* editor about Indigenous health, 1940-1966

Figure 8.3

Number of Indigenous health letters to *MJA* editor, 1940 - 1969

Figure 8.4
Four opinion articles led to the greatest number of letters to the *MJA* editor: anonymous editorials in 1952 and 1956 and articles by Bruce Barrack in 1958 and JB Cleland in 1960 both entitled ‘The future of the Australian Aboriginal’. These four articles led to a combined total of 16 letters to the editor. Neither the named nor un-named authors of these articles replied to the letter writers’ criticisms. The first of these articles, the 1952 editorial entitled ‘The Australian Aboriginal and ourselves’, dealt with the changed ideas about biological race in the 1950s. Four letters were published in response to this editorial. No previous *MJA* publication about Indigenous Australians had led to more than a single letter to the editor. Similar opinion articles about Aboriginal people and their health and future in the 1940s (like those by former Chief Protectors of Aborigines in Western Australia and the Northern Territory, AO Neville and Cecil Cook) did not elicit even a single letter in response.

The editorial in 1952 and the letters in response by readers marked the starting point of this new and public relationship between readers and writers of Indigenous health publications in the *MJA*. This editorial, ‘The Australian Aboriginal and ourselves’, was written following the announcement of a scholarship for an Aboriginal student to attend the University of Sydney. The editorial writer claimed that this announcement ‘marks an interesting stage in the development of our attitude to the aboriginal peoples of Australia. The past history of this attitude is too well known to need elaboration, but it is not, in general, to our credit.’ (p. 633) The writer then acknowledged a past of ‘deliberate brutality’ and an acceptance of the inevitability of the decline of the Aboriginal population following contact with ‘the white invader’ (p. 633). Rather than lament these past attitudes and practices, the editorial proposed making amends and adopting the new approach, ‘assimilation’, promoted by the Professor of Anthropology at Sydney University, AP Elkin.

An obstacle to this new approach was stated to be ‘the monstrous fiction of racial superiority and inferiority’, which the editorial strongly denounced as ‘illogical’, a ‘fallacy’, and a ‘vicious lie’. The editorial claimed that doctors (and therefore the medical journal) had a special role in educating the public about the fallacy of racial inferiority as they were often expected to have an opinion on a wide
range of scientific, biological and anthropological matters. The author’s distaste for notions of biological race was much more explicit than that expressed in the research articles at the same time (and discussed in chapter seven). The strongly emotive language was emphasised by juxtaposing ‘Hitler’s anti-semitism’ with the need to combat racism in Australia – which the writer called ‘the colour bar’.

It seems that the writer of an editorial had much greater freedom of language and freedom to express opinions than writers of scientific articles – even in a scientific journal like the MJA. Letter writers had even more freedom. This freedom and their necessary brevity meant that letter writers’ opinions were even more obvious to readers. Indeed, unlike research articles, the letters discussed in this chapter show that the author’s opinions, not ‘objective’ research data, formed the foundation of most letters at this time.

The first letter writer to respond to the 1952 editorial was JB Hogg from Perth. He replied forcefully to the editorial writer’s strong remarks about the inappropriateness of notions of biological race and Aboriginal racial inferiority. He claimed that racial inferiority could be discussed ‘scientifically between men of good will’ (p. 793) but without the emotive language of the editorial writer. Hogg countered the distinction of the Aboriginal men named in the editorial – Harold Blair, Captain Saunders, Albert Namitjira, and Reverend James Noble – with these remarks:

Before the arrival of white people in Australia they had done so little to influence their environment that it would be fairer to regard them as fauna than as inhabitants. They had evolved a social system regarded as the lowest in the world. (p. 793)

Hogg negatively compared the contributions and assimilation of Aboriginal people with that of convicts. For all his bluster and low assessment of Aboriginal people, Hogg’s arguments neither addressed nor refuted the original author’s remarks about

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6 Letters to the editor are being used increasingly now by academics to publish preliminary results or results of very small studies that may not be accepted as a full research article.

JB Hogg’s gender is not obvious but EP Dark’s letter refers to Hogg using the masculine pronoun.
biological race and racial inferiority; rather, they were all about a social hierarchy. This was probably more than sloppy writing. In a letter to the editor eight years later, Hogg stressed the greater importance of 'nurture' over 'nature' but maintained the same negative predictions and judgments of Aboriginal people. The refutation of biological race in the editorial remained unchallenged.

The editorial had admitted a social hierarchy when it approved of Elkin's remark that 'their stage of civilisation is in some respects lower than ours' (p. 634). Hogg was just more harsh in his appraisal. The implications of Hogg's dismal representation of Aboriginal people were made clear in his letter. Aboriginal people, with a few exceptions, could not be accepted as citizens in Australia. This was another feature of the letters to the editor: the practical implications of their remarks were much more often articulated than in the more cautious research articles.

Whilst I have emphasised the changed thinking about race in the 1950s both in the preceding paragraphs and chapter, Lindsey Harrison has used the same Aboriginal health MJA publications to stress the continuities in racial thinking. In my view, she has insufficiently acknowledged the significance of the abandonment of biological notions of race. But I can accept there is a danger in over-emphasising this shift in thinking. Was it really significantly different for Aboriginal people to be placed at the bottom of a social rather than a biological hierarchy? The most important difference was that at least a social hierarchy was less fixed – this was crucial to notions of Aboriginal 'advancement' in assimilation policy and practice. It also led writers and readers of Aboriginal health MJA publications to consider strategies to improve Aboriginal people's health. But medical representations of Aboriginal social inferiority could still fuel non-medical racist stereotypes of Aboriginal people as easily as notions of inherited biological inferiority.

The other three letters in response to The Australian Aboriginal and ourselves editorial expressed opinions strongly opposed to those in the first letter by Hogg. These letters by Lahz, Dark, and Duguid supported most of the claims of the editorial but refuted claims of Aboriginal social inferiority as well as biological
inferiority. Lahz called on his medical colleagues to begin 'regarding aborigines as our equals not as our biological, social or evolutionary inferiors.' The letter writers claimed that Aboriginal people's problems were due to racism by governments and non-Aboriginal Australians and not due to racial inferiority. The solution was to fight against racism. Lahz also replied to Hogg's opposition to citizenship for Aboriginal people and wrote:

'the granting of full civic rights to all aborigines is an utter necessity if we are sincere in our protestations of adherence to democratic ideals. At least each aboriginal ought to be asked if he wishes to be free or enslaved'.

Charles Duguid wrote the third of these letters in response to the editorial as well as three other letters to the MJA editor about Aboriginal health. Apart from his claims for authority based on his long and personal association with Aboriginal people, Duguid's letters do not appear very different to those of many other letter writers. Like other writers, he told readers that 'the aborigines are like ourselves'. In particular, he emphasised their similar mental capacity. He claimed that Aboriginal people suffered because of racism, the lack of opportunities and especially the lack of appropriate educational opportunities and that these obstacles needed to be addressed.

Duguid was a Scottish surgeon who had moved to Adelaide in 1914 and became greatly concerned about Aboriginal people's health and lives in the 1930s. He remained a strong advocate for Aboriginal people for the rest of his long life. Like Cleland and Abbie, he spent time as a member of the South Australian Aborigines Protection Board in the 1940s. Unlike these two other Adelaide doctors, he did not refrain from being publicly critical of governments and missions and their treatment of Aboriginal people. He resigned from the Board as part of his public (but unsuccessful) campaign to prevent rocket testing by the British government across the 'uninhabited' Central Australian desert. He also chose to take his criticisms overseas (especially using the Anti-Slavery Society in London) knowing that governments were sensitive to such international attention. In Australia, he served as President of the Aborigines' Protection League in South Australia and in
1958 as inaugural President of the new national coalition of organisations campaigning for Aboriginal rights, the Federal Council for Aboriginal Advancement (FCAA).48,50

Duguid’s life of advocacy about Aboriginal issues rarely spread to the pages of the MJA. He did not write any articles about Aboriginal health and his first letter in 1953 was published when he was 68 years old. It is not clear why Duguid did not write more about Aboriginal health in the MJA. But there had been no more than the occasional single letter about Aboriginal health in MJA until the 1952 editorial ‘The Australian Aborigines and ourselves’ and the three other letters in response which were published just before Duguid’s first letter to the editor. After this series of letters, the MJA became an occasional forum for debate about Aboriginal health in which Duguid participated like many others. The most prominent participant in these debates was Barry Christophers who attended the 1958 FCAA meeting in Adelaide which appointed Duguid as President.

**Barry Christophers, FCAA, and FCAATSI**

Christophers wrote half the letters to the MJA editor published about Indigenous people from the mid-1950s until the mid-1960s. Although his letters account for much of the increase in letters published about Indigenous health, figure 8.4 shows that even after excluding his letters there was still an increase in other letters about Indigenous health over this period. His work was part of wider changes in the writing about Indigenous people in the MJA, in Indigenous health and Indigenous lives, and in Australian society. He published 25 letters to the editor about Indigenous health (from 1956 to 1969) but no articles about Indigenous health. He also published 25 letters and 2 articles in the MJA from 1958 to 1969 (and 6 letters and one article in the 1970s) that were not about Indigenous people. His letters display a quite different medical understanding of Indigenous people’s health from that described in the research articles discussed in earlier chapters. In order to discuss his letters, I now briefly examine the story of FCAA and its struggle for the rights of Indigenous Australians.
Figure 8.5. Barry Christophers in Melbourne, August 2000

Source: David Thomas
FCAA was formed in 1958 at a meeting in Adelaide of representatives from nine different organisations. The original goals for FCAA arising from this meeting emphasised equal civil rights for Aboriginal people (equal pay and the removal of discriminatory legislation) but also improved housing, education and rations and the retention of existing reserves (with Aboriginal ownership). In the 1960s, FCAA began to assert the need for different Indigenous rights (like land rights) not just equal civil rights. Torres Strait Islanders were acknowledged when the organisation changed its name to the Federal Council for the Advancement of Aborigines and Torres Strait Islanders (or FCAATSI) in 1964.

Christophers attended the first FCAA meeting as the President of the Victorian organisation, the Council for Aboriginal Rights (CAR), which he had joined the previous year. He had just returned from Adelaide to Melbourne where he had studied medicine at Melbourne University and had briefly worked in laboratory research. He had found himself unsuited to research work and had moved to Adelaide to work in a general practice. Soon after returning to Melbourne he began working as a general practitioner in the inner-city suburb of Richmond, where he was still working more than 40 years later when I interviewed him in August 2000. (see figure 8.5)

He had joined the Council for Aboriginal Rights after meeting one of its founding members, Shirley Andrews, through a mutual friend soon after his return to Melbourne. Christophers and Andrews, who was a scientist, have been fondly remembered by other FCAATSI members for their energetic contribution to FCAATSI’s equal wages campaign. Faith Bandler included these comments by the NSW State Secretary of FCAATSI, Ken Brindle, in her memoir of FCAATSI Turning the Tide.

Shirley Andrews and Barry Christophers were on the FCAATSI executive. They were both professional people, and although they didn’t talk our language, I knew what they were on about. I considered people like that a necessary part of our organisation, ... They were genuine people and they worked bloody hard for the
organisation. Later, when I began to speak at meetings like Apex or Rotary, I'd quote their figures and put forward some of their ideas. People like Christophers and Andrews I considered the brains of the organisation. (p. 134)52

All of the original FCAA executive were non-Indigenous except for Bert Groves from the Aboriginal - Australian Fellowship in New South Wales.51 However, for most of the FCAA and FCAATSI's twenty year history Joe McGinness, a Kungarakan man originally from the Top End of the Northern Territory, was its President. McGinness has explained in his autobiography, *Son of Alyandabu: My fight for Aboriginal rights*, that after his White father died, McGinness was taken away from his Kungarakan mother and incarcerated in Darwin's Kahlin compound with other members of the Stolen Generations.57 He has written that his long involvement in Indigenous politics and FCAA/ FCAATSI was fueled by the experiences and insights he gained from his friendship with the novelist Xavier Herbert and through his trade unionism as waterside worker in Thursday Island and later Cairns, where he settled in the early 1950s.

When I interviewed Christophers, he remembered proudly that he had nominated McGinness at the 1961 annual FCAA conference when he was elected President.56 He said that all previous Presidents – Duguid, Don Dunstan, and Gordon Bryant – had been non-Indigenous and he suspected that a non-Indigenous person had probably expected to become President in 1961. Christophers and McGinness worked together on several FCAA/ FCAATSI campaigns for equal Aboriginal rights and are still good friends enjoying holidays together and a regular correspondence.58

Historian Peter Read has asserted that there were two dominant groups in FCAA in its early years: the unions and the churches.53 These groups included Aboriginal pastors like Doug Nicholls and Aboriginal unionists like Groves and McGinness but these men had no special status as Indigenous people. FCAA/ FCAATSI was a 'multi-racial' (or anti-racial) organisation not an Indigenous organisation – all people involved were treated 'equally' – although, in practice, it was largely run by non-Indigenous people.53 The emphasis on the equality of Indigenous and non-
Indigenous FCAATS1 members reflected the organisation’s campaign goals for equal rights for Indigenous Australians. The limitations of a non-Indigenous controlled organisation like FCAATS1 and of equal rather than special Indigenous rights became evident in the late 1960s and are discussed later in this chapter. 53

Christophers recently described himself as one of the group of activists from the political left (although not from a union) in FCAA. 59 Historian Stuart MacIntyre has emphasised the role of communists in the Aboriginal rights campaigns in the two decades after the war and even described the formation of the FCAA as ‘a left initiative’ (p. 221). 60 Christophers was a member of the Communist Party long before he was involved in Aboriginal rights. 61 In an interview in 1997, he was dismissive of the role of the Communist Party in the Aboriginal rights movement. He said that ‘the leadership wasn’t much interested in the welfare of Aborigines. Rather it was that rank and file members took it upon themselves to get involved.’ (p. 3) 59 Christophers had written critiques in the Communist newspaper Tribune in 1964 of the Communist Party’s draft policy on ‘the Aborigines of Australia’ in which he also disputed the Party’s claim for a prominent early role in the Aboriginal struggle. 62

There had been a very powerful and often successful clandestine and public campaign against the Communist Party in Australia in the 1950s. 60 The leader of the Communist Party was gaoled and agents of the newly formed Australian Security and Intelligence Organisation (ASIO) reported on the activities of party members and many people from the Left who were not communists. All were tarnished and threatened by the same ‘red’ label including those associated with the Aboriginal equal rights movement. ASIO agents reported on Christophers’s movements and some of his letters to the editor and articles in newspapers (but not his many letters in the MJA) from 1952 to 1962. 8 Christophers’s ASIO file included a letter from Colonel Spry, the Director General of ASIO, to the Secretary of the Department of Territories thanking him for earlier correspondence and then claiming:

8 Only half of the more than 200 pages of Christophers’s ASIO file are available to be viewed at the Australian Archives in Canberra. 63, 64
The Council for Aboriginal Rights (Victoria) is a communist front organization, and I should appreciate being kept informed of such of its activities as come to your notice.65

The public campaign against the ‘red menace’ was led by Prime Minister Menzies.60 He introduced the Communist Party Dissolution Bill in 1950 and then after the bill was declared constitutionally invalid he persisted with a referendum in 1951 to change the constitution. The referendum was only narrowly defeated. Anti-communist sentiment within the Labor Party eventually led to its bitter ‘Split’ and the formation of the anti-communist Democratic Labor Party. The bitterness caused by the Split was particularly nasty in the Labor heartland of Richmond where Christophers had his medical surgery.66 In this environment, it should not be too surprising that FCAA members chose to avoid acknowledging the influence of the publicly vilified Communist Party – even if they were party members.67 What seems excessively cautious or paranoid now that the Cold War is distant, would have seemed the only politically sensible reaction to the scare campaign about the threat of communism at the time.

The influence of Christophers’s political ideology on his understanding of the importance of the economic conditions of Indigenous Australians is most apparent in the chapter he wrote about government policies and practices in the Northern Territory in the book The Struggle for Dignity published by the Council for Aboriginal Rights.68 He began the chapter by stating that ‘the key to the Aboriginal question in the Northern Territory’ was that Aboriginal people were vital to the pastoral industry as a cheap exploited labour force. He believed that the purpose of discriminatory legislation and government practices was to enable the continuation of this exploitation of their labour. He later chided the draft Communist Party policy for its inadequately Marxist analysis. He complained of the policy’s undue emphasis on the assimilation, integration and identity of Aboriginal people rather than on the top priority – fighting ‘economic exploitation’.69
Equal wages for Indigenous Australians were not the main focus of Christophers’s letters to the MJA but there is evidence in his letters of both his political thinking and his antipathy to the pastoral industry’s treatment of Aboriginal people. For example, in a 1961 letter to the MJA about Aboriginal health statistics in the latest annual report for the Northern Territory he pointed to the low wages paid to Aboriginal pastoral workers before concluding:

Ill health is one of the consequences of this poverty, and the basic evil to be attacked is not so much the consequences of this poverty, but just the poverty itself. (p. 412)"}

Frederic Wood Jones was Christophers’s earliest and one of his most persistent inspirations for his involvement in the Aboriginal rights movement. Wood Jones was one of the founding fathers of the Adelaide school of physical anthropology (see chapters five and seven). Christophers told me that he first became interested in Aboriginal rights after reading Wood Jones’s writings about Aboriginal people whilst living in Adelaide in the early 1950s. He said that Wood Jones’s book Australia’s vanishing race and his 1926 Presidential Address to the Australasian Association for the Advancement of Science ‘The claims of the Australian Aborigine’ had a dramatic impact on his thinking. Christophers said that the impact of reading Wood Jones’s discussion of ‘filching the continent’ from Aborigines was increased because he already knew and respected Wood Jones’s anatomical work. Christophers wrote many, often short, letters to the MJA editor from 1960 to 1979, as well as a longer MJA article in 1972, about Wood Jones’s career. The article began with the rather extravagant claim:

Wood Jones was perhaps the greatest man of science to grace our shores, and I am very conscious of the fact that Charles Darwin visited Australia (p. 325)"

After a hiatus in the 1980s, Christophers published nine articles in the 1990s in the Australian and New Zealand Journal of Surgery about Wood Jones. He had also

\(^h\) Christophers was very dismissive in his assessment of the poor quality of Cleland and Abbie’s scientific work in comparison with that of Wood Jones.\(^{56}\)
used and quoted Wood Jones’s ideas in his earlier *MJA* letters about Indigenous health. These letters mainly referred to Wood Jones’s notion that Aborigines were *not* biologically inferior to Whites just because they had not ‘progressed’ beyond nomadism. Wood Jones (and Christophers) argued that Aboriginal people were nomads by necessity rather than intellectual inferiority because there were no animals or plants suitable for domestication or cultivation in Australia before White invasion. He reinforced this lack of evidence for Aboriginal inferiority by adding that Whites, like Aboriginal people, had not domesticated any indigenous Australian flora or fauna. Christophers combined quotations from Marx with these arguments of Wood Jones in a 1961 article in the *Communist Review*: Aboriginal people were not biologically inferior just in an earlier economic epoch because they lacked the necessary material conditions to progress from nomadism to ‘higher economic formations’ (p. 164).

Apart from his letters about Wood Jones, many of Christophers’s letters to the *MJA* editor (and one of his three articles) that did not mention Indigenous people were about worker’s conditions — in particular, about sick pay and medical certificates. His later letters contained some reminders of his letters about Indigenous health when he discussed his concerns about racism, the representation of women, and drug addicts, and the abuse of confidential medical information by the police.

Many of Christophers’s letters to the editor of the *MJA* that are discussed in the remainder of this chapter were also sent to and appeared in newspapers throughout Australia in identical or very similar form ensuring a wider non-medical audience for his ideas. Clippings of these newspaper versions appear in Christophers’s papers at the National Library of Australia and in his ASIO file. In contrast, there are no copies of Christophers’s many letters to the *MJA* in his ASIO file. Without more detailed research, we cannot know whether this was a bumbling oversight by the spies or a conscious decision and assessment of the political influence of publications in the *MJA*. More than 40 years later, Christophers could not remember exactly why he began writing letters to the *MJA* but said that he was probably ‘just
'proselytising' and that doctors were 'important folk in the community' who influenced people’s views and attitudes.  

**Starvation in the Warburton ranges?**

Christophers drew attention to several FCAA/ FCAATSI campaigns in the letters pages of the *MJA*. These more overtly political letters contextualise his letters about the representations of Indigenous people in other authors’ *MJA* publications and so are discussed first. The first series of letters about such a campaign addressed claims of malnutrition in the Warburton ranges in the arid inland of Western Australia. Christophers wrote the first and last letter in a series of nine publications in the *MJA* (four letters, four research articles, and one anonymous editorial) between May and December in 1957. The attention to the Warburton ranges had arisen due to the considerable publicity following the tabling of the report of a Select Committee of the Western Australian Legislative Assembly in December 1956.

The Chairman of the Select Committee was the Independent Liberal Member for South Perth, William Grayden, who had previously traveled through the reserve in 1953 and 1955. In 1957, he published his reflections on the findings and immediate aftermath of the Select Committee in his book, *Adam and atoms*. The book combined Grayden’s reflections with the Select Committee’s report, the reports of later investigators, and various press reports. The Select Committee was formed in response to matters raised in Parliament by Grayden. He had expressed concerns about the impact of atomic testing at Maralinga in South Australia, the planned excision by the Commonwealth of 285,000 acres for a meteorological station (at Giles) and other purposes, and the proposal by the Department of Native Welfare to remove all school-age children from the Warburton mission to the distant Cosmo Newbery mission.

The Select Committee report highlighted the hardships of Aboriginal people on the reserve and the need for the government to accept responsibility for these people’s
welfare and provide better access to food, water and medical care. It also opposed 'unequivocally and unanimously' (p. 31) plans to remove the children. The urgency and importance of the claims of their report were initially strengthened by their story of the arrival in the mission, just before the Select Committee’s party, of nineteen ‘starving’ Aboriginal people from the country to the north – one of whom died (Grayden later heard of two other deaths in this group). The report led to considerable alarm in newspapers throughout Australia, particularly about the claims of starvation, and three further investigations of conditions.103

The first of these was a short flying visit at the beginning of February 1957 by Rupert Murdoch of News Limited, publisher of The News in Adelaide and other newspapers. He quickly reported in his own papers that the Select Committee’s findings had been exaggerated and that there was no starvation and living conditions were good. Grayden later wrote that there had been rain just before Murdoch’s arrival filling temporary waterholes. Later that month, the Western Australian Minister for Native Welfare led another group investigating the Select Committee’s findings. His party included senior Government doctors and medical scientists and a journalist from The West Australian newspaper.103

Grayden was concerned about possible misrepresentation by this Government party and decided to accompany them. He was joined by Stan Lapham (another parliamentarian from the original Select Committee) and the Victorian Aboriginal pastor, football star, and activist, Doug Nicholls (who became Governor of South Australia in 1976).103104 Grayden later wrote that his fears were realised with press reports from the Government visit suggesting that conditions were quite favourable for Aboriginal people on the reserve and that there was little need for public concern.

In response, Nicholls, Grayden and Lapham organised a meeting on their return to Perth to inform the public of their very different findings on the same trip. The film they had taken on their trip was later shown at many meetings around Australia causing considerable controversy and highlighting the appalling plight of these Aboriginal people.105 This film and the conditions on the Warburton reserve
became a rallying point for Aboriginal rights activists and organisations and the first FCAA conference was held in Adelaide so that it would be easier for people to attend from Western Australia. At that meeting, Grayden was elected as one of the organisation’s original vice-presidents.

In March 1957, the press publicised the results of both the reports of the medical survey that had accompanied the Ministerial visit and a routine University of Western Australia anthropological survey led by Ronald and Catherine Berndt to the Eastern Goldfields region that had also visited the reserve. The Berndts did not confine their comments to conditions on the Warburton Reserve but discussed living conditions in the whole region and the implications of the current policy of assimilation. They emphasised their anthropological expertise and contrasted their findings with the overly ‘spectacular’ claims by the non-expert Select Committee and Murdoch’s exaggerated claims in the opposite direction. Similarly, the Deputy Commissioner of Public Health WS Davidson’s report of the Government’s medical survey rejected the non-medical Select Committee’s finding of ‘starvation’ but acknowledged ‘dietary deficiencies’ (p. 146). Davidson’s failure to verify the most evocative and extreme finding of ‘starvation’ was used to allay the public concerns raised by the Select Committee.

In spite of these ‘expert’ assurances, concern and controversy continued to be fuelled by the activists, organisations and the screening of the film at meetings around the country. Grayden wrote that there had been a cover-up and a conspiracy between the Western Australian Government and the press and ‘an adamant refusal by official after official to accept the aborigines as human beings and measure their plight by human standards.’ (p. vii) One activist who responded to the film and Grayden’s plaint was Barry Christophers, who raised the issue in the MJA, in a letter published in May 1957 under the heading ‘The medical profession and the plight of the Aborigines’.
The medical profession and the plight of the Aborigines

Christopher's letter included a long quotation from the Select Committee's report, which accounted for more than a third of the letter's length. His letters were often no more than a large quotation with his brief introductory or concluding remarks. He wrote that the report's findings were verified by the film which he had seen but did not mention the Government's medical survey or other investigations. This omission was soon addressed by the editors of the *MJA*. Any concern about these Aboriginal people's malnutrition and trachoma was given greater significance by Christopher's claims that it demonstrated the unequal treatment of Aboriginal people in Australia:

The medical attention received by the white population of Australia ranks with the best in the world; yet a section of our people receive perhaps the poorest medical attention in the world. (p. 659)

Christopher's told readers that he believed Aboriginal people's 'salvation' depended on constitutional change. Attempts to change the Constitution led by White humanitarian organisations (such as the Association for the Protection of Native Races) and early Aboriginal organisations (the Australian Aborigines' League and Aborigines Progressive Association) had failed in the first half of the century. In the month before Christopher's letter, a new campaign and petition for constitutional change was launched by the feminist activist Jessie Street and others. The official launch at Sydney Town Hall included a showing of Grayden's film of the Warburton ranges. The group proposed deleting the two references to Aboriginal people in the Constitution. Section 51 stated that the Commonwealth Government could make special laws for the 'people of any race, other than the aboriginal race in any State' (my italics). Section 127 stated that in 'reckoning the numbers of people of the Commonwealth of a State or other part of the Commonwealth, aboriginal natives shall not be counted.' (my italics) (p. 2)

Christopher's letter described the discriminatory element of Section 51 and the need for Federal coordination of Indigenous affairs through a 'Commonwealth
Department of Native Affairs' but concluded by re-emphasising medical concerns. He explained that it was the medical nature of the Select Committee's report's findings that warranted his raising the matter with the medical audience of the *MJA*. He called for an organised response by the medical profession (the Federal Council of the BMA) similar to the likely clinical response by 'any doctor in Australia' should they be confronted with such suffering.\textsuperscript{11}

There were two letters and an anonymous editorial published in the following month in response to Christophers's letter.\textsuperscript{96-98} The letter by a Melbourne doctor was generally sympathetic to Christophers's arguments but he, like the other two responses, emphasised the good work being done 'without discrimination' (p. 810)\textsuperscript{97} by doctors currently working with Aboriginal people.\textsuperscript{97} He stressed that Christophers was only talking of Aboriginal people who had no contact with these services.

The other letter writer, JR Nimmo, derided both Christophers's letter and the press campaign by those seeking to 'achieve notoriety for themselves' (he had written letters to the *MJA* editor with a similar tone and political message in 1938 and 1941\textsuperscript{107,108}).\textsuperscript{96} Nimmo was concerned about the use of such publicity by international critics of Australia in the United Nations. He called on readers to support the good work of governments and missions (singling out the work of Bill McKenzie of Aurukun mission whose repeated acts of cruelty have been most recently described by Rosalind Kidd\textsuperscript{109}). Nimmo preferred to blame Aboriginal people themselves – citing their violence and inferior diet - and the unintentional impact of colonialism through introduced disease. He hoped to protect the good name of early White settlers from false charges of mass killings but in his letter's description of Aboriginal violence he included a story of his father firing shots into a group of Aboriginal people in North Queensland. His description of such criticisms of the frontier as just a 'fashionable belief' (p. 774) was very similar to present-day politically conservative dismissals of critiques as 'political correctness' and 'black armband history'.
The editors of the *MJA* entered the debate and immediately tried to end the controversy. The June 15 issue of the journal began with an anonymous editorial which commenced in this way:

Much publicity has been given recently to a report by a Select Parliamentary Committee on the health and nutrition of the natives in the Warburton range area of Western Australia, which the committee visited. This committee was not specially medically qualified to make observations on disease or nutrition, and it was desirable that further information on the subject would be forthcoming from those trained to assess these matters. For that reason it is reassuring to learn that the findings of a subsequent medical expedition to the Warburton area were not so gloomy as those of the Select Parliamentary Committee. The full report of the medical party is not available; but in view of the widespread interest in the matter and particularly of the misconceptions that have been aroused, some reference should be made at this stage to a preliminary report of the medical party which has been prepared at the request of the Minister for Native Welfare in Western Australia. (my italics) (p. 837)98

Christophers had emphasised that the health of these Aboriginal people, being a medical matter, would be of interest to a medical audience and that the medical profession should play a role in addressing their problems. The editorial writer did not dispute this but emphatically added that only a medically trained person had authority to speak on such medical matters. Mere politicians like Grayden and Aboriginal activists like Nicholls were clearly denied an authoritative voice. A closed circle was proposed: doctors talking to doctors and doctors deciding what to do (and what not to do, as in this case). This did leave a small place for doctor activists like Christophers – and it is through his letters that the views of FCAA and FCAATSI (but only indirectly Indigenous people) appear in the pages of the *MJA* in the 1950s and 1960s. In the 1960s, there were no Indigenous doctors in Australia, so the *MJA* was still part of a tradition which Ian Anderson, one of the small group of present-day Indigenous doctors, has written was for 'a white-fellah to write about Aboriginal mob for another white-fellah' (p. 370)110.

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The editorial rather pointedly did not mention Christophers directly, only referring to the non-medical Select Committee and the general subsequent publicity. It then used the government medical report to systematically refute all of Christophers’s claims. There was no ‘starvation’ (only malnutrition of a ‘mild nature’ (p. 837))98. There were ‘no real bush natives’ (p. 838)98 beyond contact with ‘white civilization’ (p. 838)98 and medical care. The country was not desert but full of game and bush fruits and seeds. The problems were minimised and then blame shifted from the government to Aboriginal people themselves – their choice of diet and their lack of hygiene, lack of sanitation and lack of a sense of responsibility for their own health. The editorial writer dodged the issue of the Constitution but indirectly dismissed the need for more Federal rather than State government involvement – praising the present work by both State and Federal governments and relegating administrative deficiencies to some time ‘in the past’ (p. 838).98

The editorial contrasted the superior diet based on bush foods with the poor diet based on sugar and flour on missions and stations – with Aborigines described as ‘irresistibly attracted’ (p. 838)98 to these places. Tim Rowse has written about the history of the idea that these rations ‘pauperised’ Aboriginal people in his history of rationing in Central Australia.111 He has described the resilience of the idea (at least until the mid-1960s) that rationing led to the moral and cultural decay of Aboriginal people; the implicit corollary of this was the belief that Aboriginal agency was both ‘flawed’ and ‘fragile’ (p. 25)111. He has asserted that since then there has been more acknowledgement of Aboriginal people’s agency in their migration (rather than just a passive ‘drift’) to these places and a construction of more purposive Aboriginal participation in rationing as part of a reciprocal transaction - albeit participation constrained by unequal power.

The *MJA* followed this editorial four months later with the publication of a series of four articles from the doctors and medical scientists associated with the government medical survey.99-102 These articles provided a more detailed account of those findings already reported in the June editorial. The more detailed documentation of their results seem to make their claim that the malnutrition was only ‘mild’ rather difficult to believe - especially as the only photograph was of an infant with
kwashiorkor (a severe and often fatal form of malnutrition) (figure 8.6). The solution to this 'mild' malnutrition was the addition of dietary supplements (fortified flour and skim milk powder) to children's rations – a long way from Christophers's proposals for constitutional change and an organised political response by the medical profession.

The greater length of these articles enables the reader to find some features similar to the work of contemporary Adelaide researchers discussed in chapter seven. Two of the articles flirted with the possibility of a racial explanation of their findings but neither seemed very convinced. Davidson wondered whether the resistance of the children to more 'flamboyant' forms of yaws was due to a 'racial characteristic' (p. 604) or the dry climate. The biochemist DH Cumow claimed that different serum protein levels of Warburton Aborigines compared with those of White controls could be explained by dietary differences but 'racial factors should not be overlooked' (p. 609). The researchers, like those accompanying the expeditions by the Board for Anthropological Research, used the medical survey to address research questions other than malnutrition in the area. Blood was collected and the results of a variety of haematological tests reported - John Casley-Smith had collected blood for a similar purpose at Haasts Bluff in 1956 and earlier in 1957 but not yet published his findings.

Similarly, Ida Mann used the medical survey as an opportunity to repeat a 1954 ophthalmological survey (mainly looking for trachoma) that she had conducted in the same area. Mann included an astonishing remark about her assessment of Aboriginal people's participation in her research. In her description of visual acuity testing, she equated participation with her research as intelligence. She either ignored or was unable to imagine the possibility of Aboriginal people's agency or decision-making and their different priorities when she wrote:

The 6/5 line should be read by the normal subject at six metres or a little over. In cases in which this was accomplished and the patients were intelligent enough to make the effort of concentration, they were moved successively back to 9, 12, 15 and 17 metres from the test type. (my italics) (p. 612)
Figure 8.6. An Aboriginal infant from the Warburton ranges with kwashiorkor

In late 1961, Christophers wrote to the MJA briefly describing stories told to him by two informants about the withholding of blood transfusions from Aboriginal patients in Northern Territory hospitals until their haemoglobin levels were extremely low. He called for immediate investigation and action (if the investigation proved the claims correct). The immediate investigation and response came from the Commonwealth Director-General of Health, William Refshauge, and was published in the same issue as Christophers’s letter. He denied Christophers’s claims of discrimination, citing similar numbers of transfusions at Darwin Hospital for Aboriginal and European patients in spite of many more European admissions. He emphatically wrote:

The treatment of anaemia in aborigines in the Territory follows precisely the same principles as would apply to the treatment of Europeans in a similar condition. (p. 25)

But then he hedged – Aboriginal people’s anaemia was different (but of course Aboriginal people were not different). ‘Experience had shown’ (p. 25) that many Aboriginal patients had long-standing anaemia due to hookworm and for these patients transfusion would provide only a temporary response compared to a more lasting response from hookworm eradication, iron supplements and an appropriate diet. The policy was equal treatment but ‘experience’ meant that in practice different treatment was still thought of as appropriate for Aboriginal patients.

Christophers reply was published three weeks later. He acknowledged that ‘No one would suggest that anaemia due to ancylostomiasis (plus poverty) be treated by regular transfusions.’ (italics in original) (p. 143) His medical argument was very reasonable – unlike Refshauge, he wrote that transfusions together with other therapies should be used for severe long-standing anaemia. He colourfully

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1 Health care in the Northern Territory was coordinated by the Commonwealth Department of Health at this time.
2 Hookworm disease
3 This combination of transfusion together with hookworm eradication and iron therapy equates with accepted present-day medical practice. However, there are some increased risks (of fluid overload) with transfusion in this condition and it would be undertaken with a little more caution than in other
demonstrated his claim of discrimination by suggesting that the Prime Minister would not be denied transfusion if he had long standing iron deficiency anaemia; and the public debate in the MJA ended.

Christophers's complaint should be read as a criticism of a discriminatory medical policy of withholding transfusions from Aboriginal people not as about the discriminatory treatment by individual doctors of individual patients. Christophers was careful to make this distinction - his concern was with widespread, unthinking and officially sanctioned racism more than with just isolated bigots. As well as making his complaint more politically significant, this emphasis also portrayed the medical profession (and his MJA readers) as his allies rather than the targets of his criticisms. He had also chosen to portray the medical members of the Western Australian Government survey of the Warburton ranges as misled rather than misleading. And a few months before his letter about transfusions, in a MJA letter about the recent release of appalling health statistics for Aboriginal people in the Northern Territory, he exonerated Northern Territory doctors from blame:

I have no doubt that the medical officers of this department are doing their best to combat and beat disease in the Territory irrespective of the skin colour of the patient. (p. 412)

As an aside, it is interesting to contrast Christophers's description of the cause of long-standing anaemia in Northern Territory Aboriginal patients as due to a tropical disease (hookworm) 'plus poverty' with Refshauge's reference only to the tropical disease. Christophers's insertion of an economic cause of Aboriginal ill health was an atypical addition to the more usual biomedical/ microbiological proximate explanation. A copy of a slightly different version of Christophers's first letter about blood transfusions is in his papers. The following sentence was not included in the version published in the MJA:

Iron therapy and hookworm treatment alone would lead to an eventual but much slower recovery and so prolong the exposure to the risks of the cardiovascular complications of severe anaemia.
The not too distant chaining and flogging of Aborigines, the medieval drink laws, the appalling wages and many other indignities would make the withholding of blood just another addition to an already long list.117

This handwritten version of the letter was only addressed to 'the Editor' – it did not state of which journal or newspaper. It is possible that this version with this strong sentence was not sent to the *MJA* and then toned down before publication; it may have been sent and published in a newspaper. Christophers told me that the *MJA* editor, Ronald Winton, gave him a very fair deal and only ever suggested minor changes and only rejected one letter which Christophers admitted was 'a bit ripe'.56

**Campaigns against discriminatory legislation, 1961-65**

The first of the listed goals of FCAA that were adopted by the original conference was to 'Repeal all legislation, federal and state, which discriminates against Aborigines.' (p. 13)52 Bain Attwood and Andrew Markus have claimed that most of the successes of these campaigns for legislative change occurred in the first half of the 1960s.106 As part of these FCAA campaigns, Christophers wrote eleven letters to the *MJA* editor from 1961 to 1965 calling for the change of either Federal 1 or Western Australian legislation: mainly, but not only, health legislation.

The first of these letters was about the *Native Welfare Act* in Western Australia and was published in 1961, twelve months before his letters about blood transfusions in Northern Territory.17 He wrote that Section 10 of the Act stated that 'in order to prevent the spread of leprosy within the State', Aboriginal people were prohibited from moving from the North to the South of the state across the twentieth parallel of south latitude (the "leper line") except when 'as the employee of or in company with any person droving stock' (p. 147)17. Christophers's interpretation was emphatic and clear:

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1 The NT was subject to only Commonwealth legislation prior to self-government in 1978.
This is unadulterated colour-prejudice legislation. (p. 147)\textsuperscript{17}

If not, it would refer to all people, not just Aboriginal people, and would appear in the Health Act. Christophers omitted to mention that there were reasons for exemptions from the restriction other than droving such as travel for health, education or welfare purposes - an omission that was quickly pointed out in the government’s response. This emphasis may have reflected Christophers’s assessment (mentioned earlier in this chapter) that pastoralists manipulated the administrative control of Aboriginal people to ensure their continued access to exploited Aboriginal labour. He later explained to MJA readers that, as it was the employer who held the exempting permit for this movement of his or her droving Aboriginal employees, the Act treated Aboriginal people as ‘the squatter’s chattel’ (p. 345)\textsuperscript{18}. He linked this image with earlier British colonialism in India by calling it a ‘classical example of pukka sahibism’: a metaphor that may have had more sting then than today, as it was only just over a decade since India had ceased to be a British colony. He gave a further example of this use of medical arguments and legislation to facilitate the colonial control of Aboriginal people in Western Australia.

Some natives from the Port Hedland area crossed the 20th parallel to assist some of their not so fortunate friends. These liberators were subjected to interference by police under the leper-line legislation. (p. 345)\textsuperscript{18}

This portrayal of Aboriginal people as their own liberators and as liberators of other Aboriginal people was very unusual. The representation would have linked these Aboriginal people, in MJA readers’ minds, with other independence movements at a time of rapid de-colonisation throughout the world. The acknowledgement of the implicit agency and involvement of Aboriginal people in addressing the fundamental causes of their problems was in marked contrast to most medical representations. These usually mentioned only the work of White humanitarians and experts – leaving Aboriginal people as only passive recipients, victims or ‘the problem’.
Christophers had visited these Aboriginal ‘liberators’ and their non-Aboriginal associate and advocate, Don McLeod, near Port Hedland in Western Australia the previous year. He was clearly impressed by the impact of their pastoral strike in the Pilbara in 1946 and had already written in his *MJA* letters of their Aboriginal cooperative as a successful strategy and a worthy role model. He has told me that the nights on that trip spent reading aloud to a group of Aboriginal people around the campfire from Don Stuart’s book *Yandy* about their struggle was one of the highlights of his career. He also greatly admired the cantankerous but determined activist Don McLeod. McLeod’s book *How the West was Lost* told the dramatic story of his involvement in the 1946 strike and Aboriginal people’s struggles in the Pilbara and elsewhere in Western Australia against the conspiracies and malevolence of the Western Australian Government, bureaucrats, missions and pastoralists.

The Western Australian Government’s reply was by the Acting Commissioner of Public Health, WS Davidson – who had led the government medical survey of the Warburton ranges in 1957. His letter was published in the same issue of the *MJA* and immediately below Christophers’s letter. Davidson wrote that the legislation was simply a public health matter of protecting the health of Aboriginal people by using quarantine to control the spread of an infectious disease from an endemic area (the state’s North) to an area that was still disease-free. He explained that the legislation was confined to Aboriginal people as the disease was also largely confined to Aboriginal people – so there was little to be gained for public health in imposing the restrictions on non-Aboriginal people as well. Unlike Christophers, Davidson chose to ignore the restrictive colonial context and parallels of these public health measures. It was this context that Christophers emphasised with his metaphors of colonial India when he had the last word in the *MJA* in his letter published a month later.

After Christophers’s letters about the Warburton ranges in 1957, the Western Australian leper line in 1961, and transfusions in the Northern Territory in 1962, governments no longer responded in the *MJA* letters pages to his criticisms of discriminatory legislation or practice. So his letters about NT Ordinances affecting
Aboriginal people appeared in the *MJA* in 1962, 1963 and 1964 — without a published reply.\(^{23,24,26}\) Perhaps this was because a government reply just prolonged their embarrassment in front of medical readers until Christophers had the last word. He told me that when they replied, 'they just gave me more ammunition'.\(^{56}\) Christophers complained that the medicine chests required in the *Wards Employment Ordinance* to be kept by employers of Aboriginal labour were vastly inferior to those kept by outposts of the Royal Flying Doctor Service (RFDS). He suggested that changing the Ordinance to require these employers to be RFDS outposts and so possess superior medicine chests 'would not be too revolutionary.' (p. 420)\(^{26}\) But this bait led to no response, and neither did his two letters about the estates of Aboriginal people dying intestate in the Northern Territory.\(^{23,24}\) Echoing Christophers's concerns about the representations of Aboriginal people in the *MJA* (that I discuss in the next part of this chapter), these two letters were more concerned with the effects of the representations in the legislation rather than the financial implications for Aboriginal people. He wrote that the legislation about the estates of Aboriginal people had been taken directly from the *Mental Defectives Act* and was worried about the possible health implications of such legislative representations.

The psychological trauma inflicted upon aborigines by regarding them and treating them as mental defectives and bankrupts must be immeasurable. (p. 537)\(^{23}\)

There was also no response in the letters pages to Christophers's four letters from August 1963 to May 1964 campaigning against the discriminatory exclusion of Aboriginal people from allowances paid to other people with tuberculosis (TB).\(^{4,6,27}\) However, as Sue Taffe's fascinating recent description of this successful campaign has demonstrated, this does not mean there was no government response at all.\(^{55}\) The generous TB allowance was to encourage convalescence and treatment by compensating for lost wages and so avoid early return to work, non-compliance with treatment and therefore the spread of the disease.

The campaign began after the FCAA President, Joe McGinness, when visiting a non-Aboriginal mate in Cairns Hospital with TB, became aware that the Aboriginal
patient in the next bed was not receiving the allowance. McGinness wrote to Christophers who found the discriminatory elements in the *Tuberculosis Act* and the Department of Social Services manual for administering the Act – although it was only in Queensland that Indigenous people were denied the allowance. Using case histories collected by McGinness, Kath Walker (Oodgeroo Noonuccal) and the sympathetic Queensland Director of the Tuberculosis Advisory Council, EW Abrahams, Christophers began a tenacious letter writing campaign to State and Federal bureaucrats and ministers, the Tuberculosis Advisory Council, and AMA branches. The organisations affiliated with FCAA, like trade unions, were also used to disseminate a one page pamphlet about the discrimination and write more letters.

The TB allowance campaign highlighted the low wages being paid to Aboriginal people – and the work of Christophers and others on FCAA’s Equal Wages Committee. The TB allowance for a married man was £12. 2s. 6d. a week and was only just below the level of the basic wage but was much higher than the wages paid to Aboriginal people in Northern Territory, Western Australia, and Queensland. The weekly wage for an Aboriginal pastoral worker in the Northern Territory was only £2 plus rations. Christophers explained to *MJA* readers in his first letter on the topic that it was this embarrassing discrepancy between the allowance and wages in the pastoral industry that made the non-payment of the allowance ‘understandable’ (p. 296). However, Christophers’s letters kept readers’ attention primarily focussed on the ‘racial discrimination’ (p. 638) in the legislation.

The more meticulously one dissects legislation such as this, which denies the biological equality of man, the more does its incorrectness become apparent. (p. 428)

Whilst there had been no published official response to his letters in the *MJA*, his unpublished letters written directly to health bureaucrats and medical organisations

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*In spite of, or contradicting, this use of gendered language in 1963, Christophers wrote a letter complaining about gender stereotypes in an anonymously written *MJA* letter published two years later as an unfortunate reflection of the unequal status of women.*

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led to considerable activity, which Christophers was happy to acknowledge to MJA readers. Christophers thanked many of these medical bodies who had supported his campaign in a letter in 1965 announcing the campaign's success and the deletion of the discriminatory legislation. This letter in 1965 marked the end of the campaign in the letters pages but Christophers continued to help individual Indigenous TB patients in their attempts to get the allowance that was now due to them following the change in the legislation. He wrote this celebratory letter on behalf of FCAATSI, and it was his only letter published with his position as an executive member of FCAATSI. All of his other letters, except two in 1962 which he signed as President of CAR, were just signed with his name and his Richmond street address. In this letter on behalf of FCAATSI, he concluded by contextualising the TB allowance campaign with other FCAATSI campaigns:

> There are many other forms of discrimination practised against Aborigines, and our organization will continue to pressurize governments until full equality for Aborigines prevails. (p. 629)

These future FCAATSI campaigns – including the final years of its campaign for the 1967 referendum – did not involve Christophers writing letters to the MJA. He was very involved in the referendum campaign but when interviewed could not recall why he did not write any letters to the MJA as part of the campaign. However, his papers include a letter written to him in 1962 by the President of the AMA, HC Colville. Christophers had written to Colville along with other prominent Australians asking them to support the FCAA petition and campaign for a referendum to remove the discriminatory elements from the Constitution. Colville replied by asking Christophers to refrain from having Colville’s name - as President of the AMA - associated with the campaign, as the AMA was ‘a strictly non-political body’.

Christophers's celebration of the success of the TB allowance campaign in 1965 was his penultimate letter to the MJA about Indigenous health. He wrote no more MJA letters about Indigenous health except for a short letter about a Queensland court case in 1969.
Critiques of the representation of Indigenous people in the MJA

Having described the contextualising story of FCAATSI and its campaigns, I now discuss other letters written by Barry Christophers and other writers about the representations of Indigenous Australians and their health in the MJA. I began this chapter with a discussion of the letters to the editor in response to the 1952 MJA editorial entitled ‘The Australian Aboriginal and ourselves’. I now turn to the equally vigorous letter-writing reaction to an editorial in 1956 and two overview articles about Aboriginal people and their health in 1958 and 1960.

‘The upsurge of the savage’

The September 1 issue of the MJA in 1956 began with an odd editorial called ‘The upsurge of the savage’. It was not primarily about Aboriginal people or their health but about the overwhelming influence of ‘the upsurge of savagery innate in the growing boy and girl of all races’ (p. 347) leading to teenage vandalism and the ‘hysterical frenzy’ (p. 347) associated with rock and roll. This innate savagery was also blamed for the public’s preference for the ‘irrational’ ‘hocus-pocus’ (p. 348) of herbalism to medical science. The editorial began with an example of these ancestral savage urges: the story of Aboriginal girls growing up in a White foster families and then apparently reverting to their inherent savagery. The author quickly stated that this was not an example of a characteristic particular to Aboriginal girls but a more generalisable ‘urge’ which was the main focus of the article. The editorial did not link its case for biological determinism with either biological or racially inherited difference.

The letters by Charles Duguid and Douglas Everingham both denied such an ‘urge’ was unavoidable and gave examples of fostered Aboriginal children who had shown no evidence of such a reversion. They suggested that the seemingly negative outcome of the example may be caused by the foster family providing neither adequate parental love and respect nor appropriate training. After denying the
inevitability of the Aboriginal story in the editorial, they used their letters to promote their own beliefs about Aboriginal health. Everingham used his letter to attack biological determinism (including the idea ‘that genes for black skins go with genes for barbarity’ (p. 546)\(^{121}\)), choosing to emphasise the role of ‘nurture’ over ‘nature’. Similarly, Duguid wrote that Aboriginal people had an equal ‘mental potential’ (p. 546) to that of his implicitly non-Aboriginal audience and that Aboriginal success was only constrained by White ‘condescension’ (p. 546).\(^{46}\)

Barry Christophers provided a more precise critique of the representation of Aboriginal people in the editorial.\(^{10}\) He wrote that the implicit argument of the editorial was that vandalism and the savagery of delinquency was the ‘normal way of life’ (p. 587)\(^{10}\) of so-called primitive Aboriginal people with this urge usually held under control by more civilised Whites. He disputed this representation of Aboriginal life with two long quotations from Wood Jones about pre-contact Aboriginal life and compared this ordered Aboriginal society with the decadence of his ‘own “civilized” society’ (p. 588)\(^{10}\). He blamed White teenage delinquency on this decadence of his civilisation rather than Aboriginal-like primitive urges. Like Duguid, he emphasised the intellectual equality of Aborigines and Whites. His explanation of the Aboriginal girl’s story was significantly different from that of the other two letter writers:

\[
\text{The aboriginal girl rejoins her tribe, not because of her savage inherited instincts,}
\]
\[
\text{but because she becomes conscious of the white man’s colour bar and social}
\]
\[
\text{equality and happiness. (p. 588)}^{10}\]

Unlike the other representations of primitive urges or failed White education, Christophers emphasised the Aboriginal girl’s agency in her choice of Aboriginal acceptance over White racism. Christophers was not merely arguing for a shift from representations of Aboriginal failures to successes (measured against White values or even the White-made Aboriginal primitive stereotype, the ‘noble savage’\(^{126}\)) but a shift from the passivity of victimhood to purposeful considered actions (albeit with constrained power).
Marcia Langton has expressed irritation with the banality of the obsession with positive over negative representations of Aboriginal people. In its place, she has called for explicitly ‘anti-colonialist cultural criticism’ (p. 7) that concentrates on issues of power and dominance. Christophers’s critique does show anti-colonialist elements but there is no evidence of the Aboriginal involvement in the development of his critique that Langton has suggested (and I have discussed in the introduction). His letter is still an example of one White man writing about Aboriginal women for other White men and women.

However, Langton has worried about a recent trend for non-Aboriginal people to hide behind this requirement for Aboriginal involvement and so dodge their responsibilities to avoid the colonialist representations of the past. The greater certainty of the power of the medical expert and profession in the 1950s led Christophers and others to emphasise and not avoid that responsibility. He concluded his letter by calling on doctors to fight racism and racist ideas because with their scientific training they knew that Aboriginal people were their ‘biological equals in every way.’ (p. 588) Christophers joined and at times led this fight in the pages of the MJA over the next decade.

‘The future of the Australian Aboriginal’

In December 1958, the MJA published an article by Bruce Barrack of Brisbane called ‘The future of the Australian Aboriginal’ which led to four letters to the editor; and ten months after the last of these letters an article with the same title by JB Cleland was published which was followed by five more letters to the editor. Whilst Cleland had dominated Indigenous health research in the inter-war years (see chapter five), Barrack had only published one other article about Aboriginal health – about yaws and syphilis in an American dermatology journal. Neither Cleland nor Barrack published a reply to their critics.

Barrack gave no indication of why he chose to write his article. He linked Australia’s future to the future of its Aboriginal people and supported continued paternalism, the assimilation policy and delayed Aboriginal citizenship and rights.
He concentrated on Aboriginal people in northern Australia and acknowledged the 'cruel and unfair' past treatment of Aboriginal people by Whites and the negative impacts of colonisation in Australia and also in the Americas, South Africa, the Pacific and Siberia. The letter writers did not disagree with this history or his use of race-based descriptions of Aboriginal people. They also mostly seemed to accept that the implicit goal for Aboriginal people was assimilation in Australian society with debate largely about how quickly this could occur and when 'citizenship' should be granted. Whilst Christophers and Lillian Cameron used the term 'integration', they did not differentiate it from assimilation. At this time Aboriginal activists in FCAA were promoting 'integration' over 'assimilation' - characterising the former's goal as a pluralist Australia and the survival of Aboriginal cultures and the latter as leading towards a mono-cultural nation and Aboriginal culturecide.

Barrack had suggested that the unrestricted access to alcohol (access that many people, albeit incorrectly in a precise legal sense, still associate with the granting of citizenship rights) would inevitably lead to Aboriginal degradation and 'their eventual extermination' and so citizenship should be opposed. Christophers vehemently disagreed, writing that Aboriginal alcohol abuse was merely a symptom of the disease of 'lack of citizenship, low wages and colour prejudice.' (p. 30) He argued that since Aboriginal people and Whites were biologically equal, once this 'disease' was addressed the alcohol problem in Aboriginal people would be no different to the problem in Whites. In her letter, Cameron also suggested the primacy of addressing citizenship rights and racist legislation over the secondary problem of alcohol abuse and other Aboriginal health and social problems. She did not link her arguments with those of biological equality as did Christophers.

Christophers's letter made a detailed eight point case to MJA readers for the importance of Aboriginal citizenship. His arguments referred to the principles of the Christian 'brotherhood of man' (p. 29) and the UNESCO declaration on race with its 'scientific evidence' (p. 29) for the unity and biological and intellectual equality of different groups. He claimed that the centrality of the family as a 'bulwark' (p. 29) of Australian society would mean that the inclusion of Aboriginal people that would
follow citizenship should stop ‘the nefarious practice’ (p. 29) of stealing Aboriginal children from their parents.\textsuperscript{13} He also suggested Aboriginal people would benefit from subsequent voting rights, property rights, and the rights to receive social security allowances rather than rely on charity.

Christophers concluded that in Victoria, where Aboriginal people already had citizenship rights: ‘This move has increased the dignity of both black and white alike.’ (p. 29)\textsuperscript{13} This was not the only time that Christophers asserted and appealed to his implicitly White \textit{MJA} audience that racism diminished them as well as Aboriginal people.\textsuperscript{22,23} Recent cultural critics have much more explicitly elaborated the role of the representations of Aboriginal people in the construction of an Australian (non-Aboriginal) national identity.\textsuperscript{129}

Christophers also supported his claims for immediate citizenship rather than indefinite delays by citing the successes of the Aboriginal cooperatives near Port Hedland. In a second letter in response to Barrack’s article, Christophers wrote that ‘tribal natives’ had become ‘integrated, industrious members’ (p. 275) of the cooperative within a few weeks of arrival from the desert.\textsuperscript{14} These remarks may have portrayed to readers an unproblematic hierarchy and (rapid) progress from desert primitive to industrious workers. However the rest of the letter, which was dominated by a large excerpt from Don McLeod’s critique of Barrack’s article, emphasised the independence of the Aboriginal people at the co-operative. McLeod contrasted this to most non-Aboriginal people’s ideas of Aboriginal people as ‘subservient’ and Barrack’s ‘patronizing’ (p. 275) suggestions of kindly and firmly keeping Aboriginal people in an inferior position in society.\textsuperscript{14} McLeod then demonstrated the power imbalance by inverting it and asking:

\begin{quote}
How would the good Doctor react if his future was discussed in terms similar to that he has used in connection with the blackfellows? (p. 275)\textsuperscript{14}
\end{quote}

It is not possible to know whether many \textit{MJA} readers recalled McLeod’s rhetorical question about power and representation when they read the final letter in response to Barrack’s article which was published in the following week’s issue. It was
written by Ellen Kent Hughes from Armidale in northern NSW. Like Christophers, this country doctor was a frequent letter writer to the *MJA*, writing 19 letters to editor in the 1950s and 60s including three that mentioned Aboriginal people. She did not question the power or paternalism of a doctor deciding about the future for Aboriginal people. She agreed with Barrack’s dismissal of the need for citizenship.

Unlike Christophers, Kent Hughes tended to blame Aboriginal people themselves for their poor health rather than poverty and racism. But like Christophers, she saw a special role for the medical profession in addressing the terrible health status of Aboriginal people. In an earlier letter, she lamented the failure of the profession to live up to its high ideals and railed against the segregation of hospitals and schools and the profession’s inactivity in response to Aboriginal poverty and poor Aboriginal housing, education, employment, and health. She balanced these remarks by acknowledging the work done by many individual doctors. Her representation of her proposed role for the profession and of Aboriginal people was dramatically different to that in Christophers’s letters. This remark in her 1955 letter to the editor was typical:

*These people will not cooperate voluntarily; they are as children and must be treated as such.* (p. 144)

Cleland’s article, which followed some months after Kent Hughes’s response to Barrack and led to another flurry of letters, similarly represented Aboriginal people as child-like – to be protected, nurtured, educated and spoken for – with some possibility of growing up. He and Kent Hughes also clearly loathed those who would do Aboriginal people harm; they did not wish Aborigines harm nor did they despise Aborigines. Theirs was a racism that was mainly pessimistic of Aboriginal people’s abilities, with Cleland describing nature or ‘inheritance’ (p. 28) as the principal cause of Aboriginal inferior ‘mental endowment’ (p. 28) and limited capacity.
By this time, Cleland’s views were clearly out of step with his younger colleagues; none of the five letters in response supported his crude arguments against the prevailing orthodoxy of the biological and intellectual equality of all peoples and the importance of nurture over nature.\textsuperscript{15,45,124,125} One writer linked Cleland’s support of a race-based clause in the South Australian \textit{Police Act} to the oppression of apartheid and Nazism.\textsuperscript{125} And Christophers’s dismissive response merely repeated the arguments and examples from his letters in reply to Barrack. It was an ignominious finale in Cleland’s distinguished career but reflected how out of favour his race-based ideas had become.

Cleland’s colleague from Adelaide University and the Board for Anthropological Research, Andrew Abbie, also wrote a letter in response to Cleland’s article.\textsuperscript{124} He agreed with Cleland’s support for governments and their work with Aboriginal people but dismissed Cleland’s central argument that ‘the brain of the aboriginal works differently from that of the European.’ (p. 146)\textsuperscript{124} Whilst his initial focus was on Cleland’s arguments about biological assimilation, Abbie’s remarks also referred to social assimilation. Abbie’s questioning of assimilation was extremely unusual in the \textit{MJA} at that time. He accepted that assimilation could occur but that this was not what Aboriginal people wanted.

He does not accept the implication of inferiority. He is as proud of his colour and his people as is the European, and does not regard the title “blackfellow” as opprobrious. What he wants is to retain all the ethnic characteristics of which he is proud with the right to enjoy all the privileges of the “whitefellow”. (p. 146)\textsuperscript{124}

This appears to be a very early call in the \textit{MJA} for Aboriginal self-determination from an unlikely source. It included what Rowse has described as self-determination’s implicit critique of assimilation.\textsuperscript{111} Abbie continued this public questioning of assimilation as ‘\textit{Our plans for their future}’ (p. 256)\textsuperscript{132} in his book published at the end of the 1960s.\textsuperscript{132} This was an early critique of the authority of non-Aboriginal experts to know better what Aboriginal people think and need than Aboriginal people themselves. It is not entirely clear to what extent Abbie included himself in this criticism.
Names and other words

Both Cleland and Barrack discussed the importance of the way Aboriginal people were represented. Cleland proposed that ‘Those of our natives who have white ancestors should be called “part whites” and not “part natives”, so as to bring them more closely into the white fraternity into which they must ultimately merge.’ (p. 28) There was no enthusiasm for this new language just as there was little for its explicit goal – biological absorption. Duguid’s response reflected his and FCAA’s quite different concern - equal rights. He wrote that ‘these people are Australians and need no specific title, and few need special treatment.’ (p. 146) Barrack concluded his article with this discussion of the representation of Aboriginal people:

Finally, it is high time that our weekly magazines and periodicals gave up lampooning our aborigines. As long as I can remember we have been regaled with this type of so-called amusing caricature. The same themes have been done over and over again and still they continue. They are in poor taste and often very stupid, appealing only to the lowest mentality, and they have a very definite psychological effect on the natives. (p. 767)

Barrack went no further than calling for replacing either an Aboriginal absence or negative Aboriginal stereotypes with positive stereotypes. Similarly, his attention on the popular press avoided any self-reflexivity and concern with medical representations in a journal like the MJA. Whilst not explicitly highlighting Barrack’s evasion, Christophers was particularly critical of Barrack’s use of the words ‘savage’ and ‘primitive’ to describe Aboriginal people but acknowledged that the usage of these words was extremely common. He proposed that ‘the noun savage should be discarded from our diction’ (p. 30). In this letter, his reason for this word’s expulsion was not the impact of such a representation but its inaccuracy. He supported this claim by referring to Wood Jones’s arguments for environmental over racial explanations of Aboriginal people’s nomadism (rather than agriculture or pastoralism).
In other letters it is clear that Christophers's concern with legislative and other representations of Aboriginal people extended to the impact of the words used to name or describe Aboriginal people. In one letter, his sarcasm showed his distaste for the assimilationist re-naming of Aboriginal people as 'wards' which he called 'a polite name for aborigines' (p. 412)\textsuperscript{19} But it was the older rather than the new names given to Aboriginal people that were his more frequent targets. In his second letter to the \textit{MJA} about TB allowances for Aboriginal people he complained about a Commonwealth Department of Health booklet's failure to begin Aboriginal with an upper-case 'A' and its use of the term 'people of mixed blood'.\textsuperscript{5} He argued for the use of a capital letter which he wrote was then accepted terminology and was also treating Aboriginal people the same as other 'races and nationalities' (p. 638)\textsuperscript{5}. He belittled the term 'mixed blood' as scientifically inaccurate by writing that all people who had had transfusions could be so labeled, preferring what he called the more 'dignified' and 'accepted' label – 'people of Aboriginal descent' (p. 638).

In the following year, Christophers gave a brief explanation of his avoidance of two other words that were often earlier used to name Aboriginal people – 'natives' and 'blacks'. This letter to the \textit{MJA} editor was replying to criticisms of his earlier letter about the representation of a punitive massacre of Aboriginal people in a historical article,\textsuperscript{7,8,133,134} He acknowledged that the use of these terms was due to 'habit' rather than any purposeful attempt by the writers to cause Aboriginal people any 'psychological trauma' (p. 610)\textsuperscript{8}. The letter then described the more frequently negative metaphorical associations of the word 'black' than 'white' and the colonial association of 'native' with 'uncivilised' as reasons for this unintended trauma. Perhaps more interesting in the context of FCAA's campaigns against race-based legislation and practice, was the other reason Christophers gave in the letter for avoiding the word 'black'. His comments also indicated his (and FCAA's) lack of preparedness for the rise in the next few years of Black consciousness and Black Power amongst younger Aboriginal activists.

Using blacks synonymously with Aborigines is pandering to the view that skin colour is important when describing a member of \textit{Homo sapiens}. (original italics) (p. 610)\textsuperscript{8}
Christopher's discussion of the words used to describe Indigenous people was not an end in itself. It was part of a more nuanced discussion of the representation of Indigenous people and his broader campaign for Indigenous rights. In this same series of letters, Christophers not only discussed the author's use of words, he deftly turned three seemingly unproblematic sentences in a four-page surgical history article into a discussion of the dispossession of Aboriginal land, cross-cultural miscommunication, and the cruelty that occurred in the guise of frontier justice and Christianity. A feature of this series of letters was Christophers's portrayal of an alternative version - from an imagined Aboriginal perspective - of the historical events described. This rhetorical device was designed to encourage readers to think about Aboriginal resistance and suffering.

**From equality to Indigenous control**

FCAATSI's greatest public acclaim came in 1967 with the success of its ten year campaign to remove the two discriminatory references to Aboriginal people in the Australian Constitution. This campaign was barely mentioned in the *MJA*. There were no references to the referendum in the 1967 indexes of the *MJA*. Authors made a few passing mentions both supporting and opposing citizenship and the referendum in earlier overview articles and in letters in response to these articles that have been discussed in this chapter. None of these were published after 1960.

Bain Attwood and Andrew Markus have recently described FCAATSI's referendum campaign in their book *The 1967 Referendum, or when Aborigines didn't get the vote*. An astounding 90.77% of voters approved the changes proposed in the referendum. In spite of the campaign rhetoric there were few noticeable immediate changes for Indigenous Australians after the referendum leading to considerable Indigenous frustration. The initial disappointment with the impact of the referendum eventually dissipated and since the late 1980s Indigenous and non-
Indigenous Australians have almost universally celebrated the referendum campaign and result.106

Dissatisfaction in the late 1960s coincided with and fed increasing internal divisions in FCAATSI focussing on non-Indigenous control of the organisation. Charles Perkins, Kath Walker (Oodgeroo Noonuccal) and increasing numbers of young urban Aboriginal people influenced by the ideas of Black Power began publicly voicing concerns about FCAATSI in 1967. Annual conferences became increasingly acrimonious until the 1970 conference which failed to pass constitutional amendments which would have ensured Indigenous control of FCAATSI. This led to considerable bitterness and the formation of the rival National Tribal Council and in 1977 a much weakened FCAATSI eventually folded.53 Barry Christophers was one of the few White people who remained in the organisation until its demise.56

Peter Read has suggested that the collapse of FCAATSI was due not only to the self-interest of its non-Indigenous leadership but to their ‘ignorance’ (p. 80)53 about the inappropriateness of their involvement in the control of the organisation.53 The long history of White control of Aboriginal lives made the non-Indigenous dominance of FCAATSI unsustainable. Read is less convincing in his argument that the 1970 conference marked the time ‘when the demand for civil rights passed to the demand for indigenous rights.’ (p. 81)53 FCAATSI had been prominently involved since the early 1960s in supporting Yolngu land rights in NE Arnhem Land after they were threatened by bauxite mining.52 Read has added that whilst Indigenous people now much more clearly demand special rights that acknowledge their indigenous status (like land rights and native title), many governments and non-Indigenous people continue to be uncertain. Instead they frequently call for ‘merely equal’ (p. 81) rather than ‘special’ (p. 81) rights: using terms like ‘mainstreaming’ of services.53 This dispute between Indigenous rights and equal treatment also occurs in health services and health research.

Perhaps too hastily, Read justified his explanation for the demise of the FCAATSI with the rise and success in the following decades of hundreds of Indigenous
organisations (including Aboriginal community controlled health services) managed and led by Indigenous people.\textsuperscript{53} Since he wrote his remarks about the 1970 FCAATSI split, a new official and grass-roots coalition promoting ‘reconciliation’ between Indigenous and non-Indigenous Australians has emerged with prominent Indigenous \textit{and} non-Indigenous leaders and members. Whilst clearly lobbying in a different time and for different causes, this present-day coalition or social movement evokes memories of FCAATSI – and the final Chair of the Council for Aboriginal Reconciliation, Evelyn Scott, had been on the executive of FCAATSI.\textsuperscript{52} This reconciliation process and its associated organisations have also proved to be extraordinarily popular amongst both non-Indigenous and Indigenous Australians and very effective in spite of the considerable early and continued scepticism of many Indigenous people.\textsuperscript{135}

\textbf{Conclusion}

There was a dramatic explosion in the number of letters to the editor published about Indigenous health in the \textit{MJA} during the 1950s and 1960s, which reflected increased interest by readers in Indigenous health. Many of these letters, especially those by Barry Christophers, discussed the central concerns of this thesis. His letters emphasised the negative impact of racist medical representations on Indigenous people. He investigated the portrayal of power in these medical representations. Unlike the contemporary research publications that were discussed in chapter seven which failed to detect (or hid) Indigenous agency, Christophers emphasised Indigenous agency and questioned notions of Indigenous passivity. But he accepted that this Indigenous agency was constrained by the greater non-Indigenous power in the colonial context, in particular the economic exploitation of Indigenous labour by pastoralists. The story of FCAA/FCAATSI shows how he joined in this organisation’s anti-colonialist political fight.

\textsuperscript{5} However, it must be added that many Indigenous people of diverse politics now involved in Indigenous organisations were also involved in FCAATSI.
Whilst Christophers questioned the portrayal of Indigenous power in these medical representations, he rarely discussed the constraints on Indigenous power in the creation of these representations. He did not question the fact that these representations of Indigenous people were created in their absence by non-Indigenous doctors. Enormous social changes since the 1950s in Australia make this omission seem obvious today. No longer can doctors expect to be unchallenged as the only authoritative voice on health matters. No longer can non-Indigenous people remain untroubled about the colonial context of their position when they speak about Indigenous people's lives — or if they try to speak for Indigenous people. Since the 1980s, debates about Indigenous health research have increasingly addressed not only the published outcomes of research but Indigenous power in the whole research process.\textsuperscript{136,137}
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Conclusion
In this concluding chapter, I discuss the relevance and implications of my history of research before 1970 for present-day and future Indigenous health research. It is mainly directed at those who will perform this research and write future research publications. It also has implications for all those affected by this future research: Indigenous people who will be its research subjects, readers of the research publications, and all Australians who will be indirectly influenced by the ideas and representations in these publications.

There was a dramatic increase in the annual number of Indigenous health publications in the *MJA* between 1970 and the end of the century (see appendix four for a more detailed quantitative analysis). Figure 9.1 shows that there were more than twice as many (709 versus 313) Indigenous health *MJA* publications in the 30 years after 1970 than in the journal's first 56 years. Figure 9.2 shows that the rise in the annual number of Indigenous health publications reached a peak in the mid-1990s. This was accompanied by an increase in the number and proportion of *MJA* pages devoted to these Indigenous health publications; these publications accounted for more than 4% of *MJA* pages in the 1990s (see figures 9.3 and 9.4). There were ninety publications in special supplements about Indigenous health from 1974 to 1986 (see figure 9.5). This increased attention to Indigenous health in the *MJA* was accompanied by an increase in reader interest as measured by an increase in the number of letters to the *MJA* editor about Indigenous health (see figures 9.6 and 9.7). The Indigenous health publications after 1970 were largely written by authors not discussed in this thesis, most of whom were from different institutions to those discussed in detail in this thesis.

The relative inattention to Aboriginal and Torres Strait Islander people by health researchers before 1970 together with the enormous changes since 1970 - in Indigenous health research, Indigenous health care services, and the politics of the relationship between Indigenous and non-Indigenous Australians - has led to a dismissal of the importance of this early research. This has been exacerbated by the prevalent notion of medical progress which discards refuted research as irrelevant. I do not dispute that many of the hypotheses and explanations of the research in this thesis have now been refuted but that does not make them irrelevant.
Number of Indigenous health publications in the *MJA*, 1914-1999

![Bar chart showing number of publications by decade from 1914 to 1999.](image)

Figure 9.1

Number of Indigenous health publications in the *MJA*, 1960-1999

![Bar chart showing number of publications by decade from 1960 to 2000.](image)

Figure 9.2
Pages of Indigenous health publications in the MJA, 1914-1999

Figure 9.3

Indigenous health publications as percentage of total MJA pages (excl supplements), 1914-1999

Figure 9.4
Figure 9.5

Number of Indigenous health publications in *MJA* supplements, 1914-1999

Figure 9.6

Number of Indigenous health letters to the *MJA* editor, 1914-1999
Letters to the editor as a percentage of all Indigenous health publications in the MJA editor, 1914-1999

Figure 9.7
Exposing and hiding histories of the past

History has an important role in helping us to not forget the past. This is important when the past events were inspiring, when they were loathsome and also when they do not evoke such strong moral responses in the present. It is also true for past medical research that has saved lives, harmed lives, and had little obvious impact. James Jones, a historian of the research undertaken by the United States Public Health Service, which observed the effects of deceiving and withholding syphilis treatment from four hundred Black men near Tuskegee, has written: ‘What worries me the most is when the Tuskegee Study is forgotten.’ (p. xii)¹

I have found little evidence of the maltreatment of Indigenous research subjects by researchers and nothing to compare with the Tuskegee study, nor have I found much research that greatly helped Indigenous people. The harm caused by past health research and its representations of Aboriginal and Torres Strait Islander people was of a much more indirect nature; but it is still worth remembering. In reading and writing this thesis, we remember those Indigenous people whose lives were diminished because they were labeled as from an inferior race. We remember those Indigenous people for whom health services were of less concern because others believed their demise was inevitable. We remember those Indigenous people who were not able to have a say in their future because they were always portrayed as passive, powerless victims. This history is also about remembering the researchers: remembering how they could write research so entangled with the politics of colonialism.

In the Introduction, I explained that there had been little published evidence of detailed engagement by researchers with the history of Indigenous health research, although some publications do include rather superficial acknowledgements and explanations of the historical causes of Indigenous ill health. An example of the usefulness of such an engagement occurred in the first year of my research project. From an adjacent building on the hospital campus where I worked, a group of
paediatricians (Susan Skull, Alan Ruben and Alan Walker) published a research article in the *MJA* linking the small head circumference (and therefore impaired intellect) of a group of hospitalised Aboriginal children with their malnutrition. They justified their choice of research question by arguing that the association between childhood malnutrition and skull size (and intellect), which had long ago been demonstrated overseas, had only been addressed in one study in an Australian Aboriginal population.

In their original article and when later questioned, they did not acknowledge the possible links between their research and the hurt caused by the skull measuring of earlier researchers. Unlike these three non-Indigenous doctors, many Indigenous people immediately recognised the links between the past and present in this research, and were very angry. Aboriginal people have often and loudly expressed their continuing pain or anger caused by earlier skull-measuring research to determine an evolutionist and intellectual hierarchy of the races – with Aboriginal Australians at the bottom. This craniometry has an iconic status for many Indigenous people as symbolic of the dubious nature of health research and has led to an associated suspicion of present-day researchers.

An improved understanding of the history of Indigenous health research may have helped. The researchers would have benefited from taking seriously this Indigenous suspicion and its basis in histories of the past. If the paediatricians had recognised and then acknowledged this historical legacy, their arguments about the purpose of their research question may have become more convincing. By not acknowledging the continuing relevance of this history, they appeared to be dismissing the importance of this legacy for Indigenous people: a dismissal that led to the Indigenous response to their research. In a recent essay, which lamented the lack of interest in the history of the first encounters between Europeans and indigenous peoples in the Pacific, Greg Dening re-worked a well-known saying about the utility of history:

> I think a knowledge of history helps. ... There has been a noticeable lack of interest in Pacific history in our education system. It is a dangerous vacuum. It is a cliche,
but a true one ... : those who do not know their history are destined to repeat it, or at least never to see themselves as others see them. (p. 194)

Anthropologist Deborah Bird Rose has reflected upon many non-Indigenous Australians’ practice of keeping their present separate from the colonial past of frontier violence. She has argued that this has been achieved by creating a sense of distance from this history: it occurred a long time ago in neatly packaged (and now closed) chronological periods and in some distant part of Australia. She found a quite different approach to history amongst Aboriginal people in the Victoria River District in the NW of the Northern Territory. She shared these Aboriginal people’s concerns about many White Australians’ relationships with their history:

White people, they say, don’t know what to remember and what to forget, what to let go of and what to preserve. They don’t know how to link the past with the present; they fail to recognise their own stories. (p.160)

Rose has argued that both many White historians and the Aboriginal people of the Victoria River have a much more open approach to history. She has related the approach of these Aboriginal people to their concepts of space and time and to the oral nature of the Aboriginal history or histories that she was told. These histories and their telling are an important part of these Aboriginal people’s present.

Such histories are powerful because they are local, and oriented toward the living, and because they sustain a relationship between past and present in which neither consumes the other. Stories are open to retelling, rethinking, reworking. (italics in the original) (p.15)

In Rednecks, eggheads and blackfellas, a history of racial discourses and Rembarrnga people’s encounter with pastoralists, anthropologists and bureaucrats in southern Arnhem Land, Gillian Cowlishaw has also described the continuing relevance of Indigenous histories of the past. She has proposed that the continued presence and salience of these histories in spite of being overlaid with new cultural forms can be understood as a ‘palimpsest’. She has used this metaphor to describe
the writing of new stories over the top of earlier stories. These layers of earlier stories are still present and only concealed rather than erased.

What is hidden or undisclosed can remain powerful, although what is smothered may be dying unseen. At the same time, the palimpsest suggests that an active concealing and revealing of indigenous forms is used in the struggle for legitimacy and against shame, and can point to the way new shades of meaning can be attached to old indigenous and racial practices. Thus the palimpsest refers both to the hidden and to the process of hiding and exposing, as reflections and resonances from the indigenous and colonial past come to mean different things in the present. (p. 294-295)

Other definitions of 'palimpsest' suggest that subsequent writing does erase not just apparently erase earlier writing but I agree with Cowlishaw that these histories are not erased but only concealed and so are still very relevant and important to understand and study. This is true not only of the more symbolic elements of the history of Indigenous health research such as the measuring of Aboriginal people's skulls. It is also true of the more complex parts of this history. One of these more subtle elements of this history is how researchers have described Indigenous people as 'different' and in particular researchers' understanding of 'race'. But even here some researchers have preferred to distance themselves from this history rather than highlight its continuing relevance in the present.

An example of this distancing tendency was a recent article by Tarun Weeramanthri, a doctor involved in Indigenous health research. After briefly describing the history of 'race', he seemed to distance himself from this history and celebrated the almost total disappearance of the term 'race' from the Australian public health literature. Like many other health researchers, including myself, he claimed that it was preferable to use the term 'ethnicity' as it is a more accurate term. He suggested that as the biological basis of race had long been discredited it was better to move to a term that has no biological connotations and instead has an explicitly social, cultural and political definition. He concluded that 'abandoning the terminology of race leaves racism without any logical basis, and may contribute to a process of social change'. After this rather bold prediction he added the caveat that this change
'cannot be expected to eliminate the phenomenon of racism.' (p.1)\textsuperscript{10} A less subtle abandonment of the terminology of race than Weeramanthri's might easily turn into what American activist academic bell hooks has strongly dismissed as:

the notion of racial erasure – that is, the sentimental idea (often voiced by religious folks) that racism would cease to exist if everyone would just forget about race and just see each other as human beings who are the same. (p. 151)\textsuperscript{11}

She has argued that a non-racist community will only exist when we acknowledge and engage with difference rather than forget it.\textsuperscript{11} In England, Stuart Hall has argued for an understanding (and a political struggle) that works with and through difference, not against difference, that acknowledges different identities, interests and ethnicities that are not fixed.\textsuperscript{12} Both Hall and hooks have emphasised the need to move from studying race alone to studies that confront the different workings of race and racism as they interact with class, gender and sexuality. \textsuperscript{a} Similarly, the collection of essays entitled Race matters edited by Cowlishaw and Barry Morris examined the ambiguities rather than the binarisms of race in order to understand racism in Australia as it has affected Aboriginal people.\textsuperscript{13}

This thesis is a contribution to the understanding of the ambiguities of race in the encounter between health researchers and Aboriginal and Torres Strait Islander people in Australia. The continuing relevance of this history of notions of race cannot be simply evaded by avoiding the term race. I would suggest researchers more explicitly engage with the historical and political meanings of the different categories that they use and the differences they describe. This engagement is more important than whether these categories are labelled 'racial' or 'ethnic'. After such an engagement, the American public health researcher David Williams has chosen to promote the continued use of 'racial' categories.\textsuperscript{14} Like Weeramanthri, Williams has written that he 'rejects the use of race as a biological concept and agrees that the current racial categories capture ethnic status'. (p. 325)\textsuperscript{14} But Williams has argued

\textsuperscript{a} Whilst I have concentrated on the various and changing representations of race, when relevant, I have also investigated the intersection of race with gender (e.g. chapter four) and with class (e.g. chapter eight).
that racial rather than ethnic categories better acknowledge the history and politics of race and racism. Whilst Weeramanthri has welcomed the distance between past and present research, Williams has promoted categories in present research that highlight and embed the relevance of past research in the present.

Lindsey Harrison’s history of the same Indigenous health research that I have described in this thesis does not tell the story of the hidden but persisting ambiguities of medical understandings of race. She did not have the benefit of the insights available in the literature about race that has been published in the twenty years since she wrote her thesis. It is a history of medical progress towards ‘racial erasure’. She concluded with an optimistic assessment of the possibilities of Indigenous health in 1979. This optimism was largely based on the notion that medicine had finally begun to discard the discredited racial ideas of its past.

Like Harrison, I have presented my history using a largely chronological structure. This is not because I accept an entirely linear view of history and medical progress towards the scientific truths and political values of the present, although I do not dispute that there have been enormous increases in the understanding of Indigenous ill health. This dominant linear notion of medical progress, either through discoveries or the elaboration and refutation of hypotheses, leads to a sense of superiority and smugness of those in the present in comparison to this history of the distant and now discredited past.

The chronological structure of my thesis is designed to emphasise the different assumptions and traditions of researchers and the different historical and political context of research performed at different times: difference but not always progress. It is this different historical context that made the past research seem plausible when it was published.

The nationalist politics of Federation and the desire to populate and then economically develop tropical northern Australia rendered plausible not laughable the decision by researchers to examine whether White people could live in the tropics. The wide acceptance of racial categories and the encouraging early results
in separating racial groups by the new and simple (but metaphorically powerful) technology of blood grouping makes the enthusiastic adoption and long persistence with this research not surprising. Similarly, the avoidance of racial categorisation in research (and legislation) is understandable after the revelations about the millions murdered during the Holocaust in the name of racial purification based on a racial hierarchy. It was the changed political context, not just new scientific evidence (and medical progress), that led to these changes in the scientific approach to race. Whilst these contexts made researchers' decisions plausible I do not mean to imply that they made them inevitable. The researchers had some choice. I have described some of these alternatives and inconsistencies in the published research which belie an absolute determinism that might lead to nihilism or pessimism about the present and future.

The politics and values of medical research

Researchers today are not comfortable talking about the influence of politics and values on their research. This is part of a tradition that sees science as about 'truth' and 'objectivity', and above the distortions, compromises and values of politics and the social realm. Sarah Cunningham-Burley and Anne Kerr have discussed these rhetorical boundaries that medical scientists (working in the field of genetics) construct between their science and the social realm.¹⁶ They have written that these rhetorical boundaries protect scientists' cognitive authority, which relies on the supposed value neutrality of science. These rhetorical boundaries limit the possibility of reflection about the social framing of science. They have found that these scientists limited their discussions of the social realm to the social implications (not framing) of their genetics, concentrating on its beneficial applications. When the scientists did discuss the negative implications of genetics, these discussions were found to be very narrowly focussed.

Even within these narrow confines of the separate scientific realm there are further scientific traditions which exclude the social realm. Martha Solomon (Watson) has claimed that the dehumanising language and conventions of scientific writing tend
to obscure or diminish the social, political and ethical elements of the medical research being described. She has described the typical depiction of medical researchers as impartial observers on a scientific quest, a disease as the other elusive active agent, and the research subjects' patients dehumanised and reduced to being the scenery against which this drama unfolds. But she has stressed it is the genre of scientific writing that manipulates the researchers not vice versa. This rhetorical dehumanisation of the research subjects both assists and emphasises the objectivity of the medical research but it diverts attention from the ethics, politics and values of the research and Solomon has written that:

If allegiance to objectivity and detachment blinds us to other values, it produces neither humane behaviour nor sound science. (p. 264)

Whilst these scientific traditions obstruct discussions about the influence of politics and values in present medical research, I have found that medical researchers do not find it so difficult to talk about these aspects of past research. These researchers, like most people, find it easier to see the politics, values and traditions of other people (in this case past researchers) than their own. The authority of present day science is also less threatened by the examination of the social framing of now refuted research. With the many now disproved conclusions of this early research of little interest, the researchers' attention can be drawn to the research processes: the research aims, the research questions, and the writing and reading of the final research publications. In contrast, they usually prefer to talk about the more objective elements of their own research: their data and results, which are largely objective except in the rare cases of scientific fraud.

This focus of attention on the processes of past research enables greater discussion of how politics and values affected this research. This might provide a less threatening and more successful route to the engagement of researchers with the social framing of present day medical research. I believe that future Indigenous health research would be strengthened by confronting rather than evading this social and political context just as I have earlier argued for an engagement with historical context.
Research that explicitly engages with its history and politics

I hope this thesis will be used by present-day researchers of Indigenous health (particularly but not just non-Indigenous researchers) as a starting point for reflections about their own work. How will they choose their next research question? How will they choose to represent Aboriginal and Torres Strait Islander people and their illnesses in their next paper? What are the links between their own research and both the history of colonialism and the relations between Indigenous and non-Indigenous Australians today? I hope researchers will include some of this questioning of history, politics and values in their published research.

Researchers may find such questioning difficult and confronting at times; so will their readers. It may lead researchers to confront not only the stereotypes of Indigenous people in health research but also the stereotypes of health research itself. This is more difficult than attempting to hide the past under the mountain of new research papers. It is more difficult than camouflaging offensive concepts from the past in more acceptable language from the present. It will probably lead researchers to highlight rather than hide any colonial and racist remnants in their work.

Medical science can explore productively many questions about the health and sicknesses of Aboriginal and Torres Strait Islander people. However, the contribution of medical research to Indigenous well-being is diminished by its evasion of the historical, social, and political elements of this research. The explicit examination of these elements may undermine some of the power and moral authority of medical science and medical researchers and so some reluctance by researchers is to be expected. But it will also lead towards a better relationship between health researchers and Aboriginal and Torres Strait Islander people and their organisations, and ultimately to better more cooperative Indigenous health research.
References


Appendixes
Appendix 1

Methods of quantitative analysis

The quantitative analysis was restricted to publications in the Medical Journal of Australia (MJA) about Aboriginal and Torres Strait Islander people and a random selection of other MJA publications. The MJA was first published in 1914. Reflecting the historical period chosen for study in this thesis, there was a more thorough analysis of publications before 1970 than of more recent publications. I did not examine research published after 1999.

Selection of MJA publications about Indigenous people

MJA publications about Indigenous people were identified using three different sources—two Aboriginal health bibliographies, three electronic bibliographic databases, and Lindsey Harrison’s MA thesis Racial ideas and the health care of Aborigines: an analysis of articles of Aborigines in the Medical Journal of Australia 1914-79.1-3 The two bibliographies of Aboriginal health research cover the two consecutive time periods: before 1970 and 1970-85.1,2 These two bibliographies have cited a total of 3358 references related to Indigenous health. The bibliographies were searched manually to identify publications from the MJA. Additional publications were identified in the bibliography of Harrison’s thesis.

The electronic databases were most useful in identifying more recent MJA publications about Indigenous people. Three databases were used: MEDLINE and two AustHealth databases, Australian Medical Index (AMI) and ATSIhealth. These databases were

MEDLINE was first searched for ‘MED-J-AUST’ in source (neither ‘MJA’, ‘MED J AUST’ or ‘MEDICAL JOURNAL OF AUSTRALIA’ in source identified any publications). This search was combined with searches for the subject heading, ‘AUSTRALOID RACE’, and the key word searches: ABORIGIN* (which includes the subject heading ABORIGINES and the key words: ABORIGINE, ABORIGINES, ABORIGINAL and ABORIGINALS), TORRES STRAIT, and INDIGENOUS.

The AMI database was similarly first searched for ‘MED J AUST’ in source (neither ‘MED-J-AUST’, ‘MJA’ or ‘MEDICAL JOURNAL OF AUSTRALIA’ in source identified any publications). This search identified publications in journals other than the MJA - Aust J Med Sci, Aust J Med Lab Sci, and Aust NZ Med - which were ignored. As with MEDLINE, this search was combined with searches for AUSTRALOID RACE, ABORIGIN*, TORRES STRAIT and INDIGENOUS.

As it is a database of Indigenous health research, the ATSIHealth database was just searched for publications with ‘MEDICAL JOURNAL OF AUSTRALIA’ in source. Neither ‘MED-J-AUST’, ‘MED J AUST’ or ‘MJA’ in source identified any publications.

All publications that were identified were photocopied and then collated according to their year of publication. Not all of the publications identified were only or primarily about the Indigenous health, but all of the included publications discussed Indigenous people. Publications that were found not to mention specifically either Aboriginal of
Torres Strait Islander people were excluded. Minor typographic corrections to previous articles were also not included. Publications about Indigenous people that were not identified using the sampling frame of the three sources were not included in the quantitative analysis – but were discussed elsewhere in the thesis if appropriate. The exception to this rule was a small number of recent publications that were not identified by the electronic databases but were included in the analysis in appendix four. The electronic databases did not always specifically cite all letters in an exchange (e.g. omitting to cite the authors’ response printed below a letter to the editor). These were all included if about Indigenous people.

**Selection of control MJA publications not about Indigenous people**

The *MJA* publications before 1970 about Indigenous people were compared to a random selection of *MJA* publications not about Indigenous people. This random selection was stratified by the decade of publication and by whether they appeared in supplements or not (only because of the different page numbering of supplements). One of these control publications were selected for every four publications about Indigenous people. Random numbers generated by Epi Info Version 6 were used to select a point on a page of the *MJA* – by randomly selecting the year, then the volume, the page number, and then how many centimetres down the page. The first publication to commence after this point was selected. When this process selected a publication about Indigenous people, the next publication was selected. Advertisements for professional association activities were similarly not selected, although reports of meetings were included (as these had been included in selected publications about Indigenous people). All selected publications were then photocopied and collated.

**The questionnaire and data entry**

A questionnaire was completed for each *MJA* publication. The data from the questionnaire was entered directly into an Epi Info Version 6 computer file. The
questionnaire collected information very similar but not quite identical to that collected by the Research and Evaluation Policy Project (REPP) at ANU. That project performed detailed bibliometric analyses of all Australian research published in Institute for Scientific Information (ISI) journals from 1981 to 1995. The questionnaire in this thesis described each consultation using the following variables.

- A unique Epi Info identifying number for each publication.

- The year of publication.

- The journal volume number. From 1914 until 1983, there were two volumes per year numbered 1 and 2; since 1984, there have been two volumes per year numbered consecutively starting with number 140.

- The page number of the first page of the publication. Page numbers start at 1 with each new volume and so the first page of a new issue is that after the last page number of the previous issue. The page numbering of supplements is quite separate but also starts with 1 for each new volume.

- The page number of the last page of the publication.

- The length of the publication in number of pages. Column length of the publication (including all figures and photographs) was measured to the nearest millimetre. This was compared with column length of the page in order estimate page length to one decimal point.

- The author(s). Their last names were written first then the initials of their other names. Ten authors for each publication could be included and if there were more than ten authors, the first nine were entered with the tenth entered as ‘et al’. Individual authors’ names were used rather than corporate authors such as ‘working parties’. If no author’s name was given it was entered as ‘anon’. In the early

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years of the *MJA*, there were many anonymously written reports of named authors’ research – especially of conference presentations. These were recorded with the authors’ names followed by ‘(anon)’. In contrast, anonymously written reviews of and author’s work which included a significant amount of the anonymous author’s assessment were just recorded as ‘anon’.

- The institution(s). Ten institutions could be included and if there were more than ten institutions the first nine were entered with the tenth entered as ‘et al’. Institutional addresses were favoured over street or ‘private’ addresses and ‘former’ addresses were used rather than ‘current’ addresses or addresses for reprints.

Following the example of the Research Evaluation and Policy Project, university departments based in hospitals were coded as the university rather than the hospital - the exceptions being if the address was written as the university department and the hospital (in which case both were entered) and if the name of the university was not given.

A single name was used for each state health department. In the Northern Territory, name changes of the government health services were more complex and reflected whether these services were Northern Territory or Commonwealth Government services. These services were coded separately although I have also included the results of combining all these government services in the Northern Territory. If the author included their position (e.g. District Medical Officer, Derby, WA) but not the name of the state health department, their institution was coded as the state health department.

A small remaining grey area in the coding of institutions was how to deal with the state health laboratories (mainly in Queensland and WA). These were either coded under their own names (similarly to hospitals), the hospital were they were located (if this was listed last in the address) or the state health department (if this was listed last). As this involved a relatively small number of publications it did not
dramatically alter the results.

If no address was given, it was entered as ‘z’. Private addresses were coded and entered as ‘x’. The inclusion of only the name of a country town was accepted as a private address but if only a capital city was included this was coded as no address.

- The title of the publication. Thirty words from the title could be included. The title of the section of the journal was used as the title only if no other heading was given. All words were entered in upper case – many of the titles were printed entirely with upper case letters so it was not possible to determine which words began with or without an upper case letter.

- The publication type. The distinction between letters to the editor and all other types of publication was straightforward. I also coded original articles and editorials separately from the many usually anonymously written news reports, reports of meetings, or reports of research. The coding of these two categories was not always so clear, and so I have not emphasised its analysis. Harrison has also mentioned the difficulties caused by changing and variable types of publications in the MJA over the same period. 3

Data entry in Epi Info Version 6 was accompanied by Epi Info’s CHECK program to disallow illegal values. Data entry of every publication was also checked manually. Neither the reliability nor validity of data entry or coding was tested.

Data analysis

A simple descriptive analysis of the data was performed using Epi Info version 6. The results were manually amended for those very few publications with more than 10 authors, 10 institutions, or 30 words in their title. Non-parametric statistical tests of
comparison (Mann Whitney) were calculated using Epi Info and parametric tests (t test) of comparison were calculated manually using the methods described by Bland. These tests were used to test the statistical significance of comparisons of publications about Indigenous people to either other *MJA* publications or publications from an earlier period about Indigenous people. There were also some tentative descriptions of the proportion of publications by the different types of research institution as in the REPP research. The results of the analysis of these publications appear in appendixes 2, 3, and 4.
References


Appendix 2

Quantitative analysis, 1914 –1949

I have included in this analysis 113 MJA publications about Indigenous people from 1914 to 1949 (see chronological list of these publications at the end of this appendix). Ninety-three publications were found using Moodie and Pederson’s bibliography and a further 20 using Harrison’s thesis (a further 3 were identified by Harrison but not by the bibliography but not included as they did not explicitly mention Aboriginal or Torres Strait Islander people). These publications were compared to random sample of 29 control MJA publications stratified by decade of publication. As supplements had separate page numbering to the remainder of the MJA, the control sample was also stratified by whether the publications were in supplements or not.

The publications about Indigenous people accounted for 329.1 pages of MJA text. The median length of these publications was 1.3 pages (interquartile range 0.4 to 4.9) and the mean length 2.9 pages. The difference between the mean and median being due to the effect of a small number of very long articles; with the median a better indication of central tendency. There were also a large number of very short publications (22% of publications were less than a quarter of a page long) — especially letters to the editor and reports of papers given at medical meetings. A simple method of not over- emphasising the influence of these very short

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a Hogg’s paper at the 1923 Australasian Medical Congress was published in two parts in supplements to two separate issues of the MJA in 1924 but was included in the analysis as a single publication.

b There was one control publication from a 1921 supplement and four Aboriginal health publications in supplements (all from 1924).
publications in the subsequent quantitative analysis was to analyse by the pages of text as well as by the number of articles.

The control *MJA* publications were slightly shorter but this difference was not statistically significant (*p* > 0.05, Mann Whitney test). The median length of the control *MJA* publications was 1.5 pages (interquartile range 0.9 to 2.8) and the mean length 2.1 pages. Only 10% of these publications were less than a quarter of a page long.

Table A2.1 shows the increasing numbers of publications about Indigenous people until the 1930s followed by a decline in the 1940s – probably associated with the war. Table A2.2 demonstrates that the most number of publications about Indigenous people in any one year was in 1931 with 10 publications and 36.0 pages of text. Other years with larger numbers of these publications and pages of text were 1914 (8 publications, but only 7.3 pages as 7 of these publications were brief anonymous reports of papers given at the 1914 British Association for the Advancement of Science meeting), 1924 (7 publications, 23.5 pages), 1930 (6 publications, 22.3 pages), 1932 (6 publications, 17.9 pages), 1935 (6 publications, 24.5 pages), and 1938 (5 publications, 32.4 pages).

### Table A2.1. *MJA* publications about Indigenous people by decade, 1914-1949

<table>
<thead>
<tr>
<th>Decade</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>1914 –1919</td>
<td>14</td>
<td>21.8</td>
</tr>
<tr>
<td>1920s</td>
<td>31</td>
<td>74.5</td>
</tr>
<tr>
<td>1930s</td>
<td>41</td>
<td>168.1</td>
</tr>
<tr>
<td>1940s</td>
<td>27</td>
<td>64.7</td>
</tr>
</tbody>
</table>
Table A2.2. *MJA* publications about Indigenous people by year, 1914-1969

<table>
<thead>
<tr>
<th>Year</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>1914</td>
<td>8</td>
<td>7.3</td>
</tr>
<tr>
<td>1915</td>
<td>2</td>
<td>6.3</td>
</tr>
<tr>
<td>1916</td>
<td>2</td>
<td>1.6</td>
</tr>
<tr>
<td>1917</td>
<td>1</td>
<td>1.5</td>
</tr>
<tr>
<td>1918</td>
<td>1</td>
<td>5.1</td>
</tr>
<tr>
<td>1919</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>1920</td>
<td>0</td>
<td>0</td>
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<tr>
<td>1921</td>
<td>4</td>
<td>6.3</td>
</tr>
<tr>
<td>1922</td>
<td>3</td>
<td>8.6</td>
</tr>
<tr>
<td>1923</td>
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<td>2.2</td>
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<td>7</td>
<td>23.5</td>
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<td>0.2</td>
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<td>1926</td>
<td>4</td>
<td>10.1</td>
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<td>1927</td>
<td>1</td>
<td>0.2</td>
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<tr>
<td>1928</td>
<td>3</td>
<td>16.8</td>
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<tr>
<td>1929</td>
<td>4</td>
<td>6.6</td>
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<td>1930</td>
<td>6</td>
<td>22.3</td>
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<td>1931</td>
<td>10</td>
<td>36.0</td>
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<td>1932</td>
<td>6</td>
<td>17.9</td>
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<tr>
<td>1933</td>
<td>1</td>
<td>2.8</td>
</tr>
<tr>
<td>1934</td>
<td>2</td>
<td>17.5</td>
</tr>
<tr>
<td>1935</td>
<td>6</td>
<td>24.5</td>
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<tr>
<td>1936</td>
<td>1</td>
<td>11.4</td>
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<tr>
<td>1937</td>
<td>3</td>
<td>2.8</td>
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<tr>
<td>1938</td>
<td>5</td>
<td>32.4</td>
</tr>
<tr>
<td>1939</td>
<td>1</td>
<td>0.5</td>
</tr>
<tr>
<td>1940</td>
<td>3</td>
<td>10.1</td>
</tr>
<tr>
<td>1941</td>
<td>4</td>
<td>1.4</td>
</tr>
<tr>
<td>1942</td>
<td>3</td>
<td>11.9</td>
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<tr>
<td>1943</td>
<td>1</td>
<td>3.3</td>
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<td>1944</td>
<td>3</td>
<td>13.0</td>
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<td>4.9</td>
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<tr>
<td>1946</td>
<td>3</td>
<td>6.3</td>
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<tr>
<td>1947</td>
<td>2</td>
<td>1.5</td>
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<tr>
<td>1948</td>
<td>4</td>
<td>7.9</td>
</tr>
<tr>
<td>1949</td>
<td>3</td>
<td>4.4</td>
</tr>
</tbody>
</table>

Of the 113 Indigenous health publications in the *MJA*, 63 (56%) were articles, 13 (12%) were letters to the editor, and 37 (33%) were reports (usually anonymously written) of meetings or reviews. The only publications to appear in supplements were 4 publications in 1924. The 63 articles accounted for 300.4 (91%) of the total 327.6 pages (letters: 4.4 pages (1%) and reports: 24.3 pages (7%)). There were no
letters to the editor in the control publications, which was significantly different to the Indigenous health publications (p<0.05, t test). There were similar proportions of articles (62% of total publications) and reports (38%) in the control sample as in the Indigenous health sample (p>0.05, t test).

Authors

Table A2.3 shows the most prolific authors of Indigenous health publications in this period and table A2.4 ranks only those authors listed as the first author of publications. The most prominent author in the period was John Burton Cleland from Adelaide University. He published 13 publications (53.5 pages of text) in this period \(^c\) – all as the first author – from 1923 until 1944.\(^{22,31,43,48,49,51,61-63,69,71,96,99}\) This was the most publications and pages of publication of any author or as a first author. There were also a further 5 anonymously written reports (2.3 pages of text) that reported papers he had given at meetings.\(^{19,21,34,46,73}\) He had more Indigenous health publications and more pages of text published than any other author or first author in the 1920s and 1930s; and the most pages of text published as a first author in the 1940s.

Table A2.3. Most prolific authors of MJA publications about Indigenous people, 1914-1949

<table>
<thead>
<tr>
<th>Author</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>John Burton Cleland</td>
<td>13</td>
<td>53.5</td>
</tr>
<tr>
<td>HK Fry</td>
<td>4</td>
<td>15.9</td>
</tr>
<tr>
<td>JJ Graydon</td>
<td>3</td>
<td>15.5</td>
</tr>
<tr>
<td>Roy Simmons</td>
<td>3</td>
<td>15.5</td>
</tr>
<tr>
<td>Anton Breinl</td>
<td>3</td>
<td>11.8</td>
</tr>
<tr>
<td>Edward Ford</td>
<td>3</td>
<td>5.5</td>
</tr>
</tbody>
</table>

\(^c\) Cleland also published articles after 1949 in the MJA and before 1914 in the Australasian Medical Gazette.\(^{117-119}\)
Table A2.4. Most prolific first authors of MJA publications about Indigenous people, 1914-1949

<table>
<thead>
<tr>
<th>Author</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>John Burton Cleland</td>
<td>13</td>
<td>53.5</td>
</tr>
<tr>
<td>Anton Breinl</td>
<td>3</td>
<td>11.8</td>
</tr>
<tr>
<td>Edward Ford</td>
<td>3</td>
<td>5.5</td>
</tr>
</tbody>
</table>

Anton Breinl from the Australian Institute of Tropical Medicine was the first author of publications in 1914, 1915 and 1916.8,9,12 Breinl had the most publications and pages of publications as an author or first author from 1914 to 1919. Edward Ford, both as part of the Army and Sydney University’s School of Public Health and Tropical Medicine, wrote articles about the Northern Territory in 1940, 1941, and 1942 — all as a first author.90,91,95 This was more publications than any other first author in the 1940s. But Cleland’s 10.2 pages as a first author in the 1940s was the most of any first author in that decade.

Roy Simmons and JJ Graydon from the Commonwealth Serum Laboratories (CSL) in Melbourne together wrote two articles in 1944 and another in 1948 about blood groups in Aboriginal people.100,101,110 This was more than any other author (but equal to Ford’s contribution) in the 1940s; Simmons and Graydon’s publications accounted for more pages of text than any other author in the 1940s. Simmons was the first author of two of these articles and another CSL colleague, Harold Wilson, was the first author of the other article.

No other authors except HK Fry (one of Cleland’s colleagues), with 4 publications (1930-35)49,51,69,75, published more than 2 publications in this period. Other authors with more than 10 pages of text published over the period include: AH Baldwin (1930, 1938)47,83, WEL Crowther (1934)70, JHL Cumpston (1931)55,56, CJ Hackett (1936)78, CV Mackay (1938)85. Crowther, Hackett, and Mackay are each included only on the basis of one very long article. These articles by Crowther (12.5 pages long), Hackett (11.4 pages), and Mackay (18.1 pages) were the three longest of the period but not necessarily the three most influential on the discourse. Whilst very short publications were not likely to be very influential, very long articles were not
necessarily overly important in attracting the attention of readers or influencing the discourse. There were no named authors with more than one publication in the control sample.

Of the 113 Indigenous health publications, 23% (26 publications, 12.5 pages of text) were anonymously written short publications that simply reported a paper or work by a named author.\textsuperscript{1-7,17-19,21,23-25,34,35,38,45,46,58,73,74,79,105,107,111} At times the complete paper was published in a later issue of the \textit{MJA}. This sort of anonymously written paper was more common in the early years of the \textit{MJA} - 7 in the 1910s, 12 in the 1920s, 4 in the 1930s, and 3 in the 1940s. These publications seem to reflect the editorial ‘news’ style of the journal at that time. It is not clear who wrote these reports but a reasonable assumption would be that they were written by either \textit{MJA} staff or the representative of the \textit{MJA} assigned to each state.

There was a similar proportion of such publications in the control sample (8/29 or 28%) (p>0.05, t test). There were 10 (9% of the total publications) (4.8 pages of text) other anonymously written Indigenous health \textit{MJA} publications, including one letter signed "M.B.".\textsuperscript{10} Half of these (5/10) were published in the in the 1930s. There was a not statistically significantly larger proportion (5/29 or 17%) of such anonymous publications in the control sample (p>0.05, t test). Unlike the anonymous reports discussed above, these publications all included significant comment or analysis by the anonymous author. So their authors’ identities might be more interesting than those of the first type of anonymous publications; as would be the reasons for the authors’ decisions to write anonymously. Unfortunately, the identity of any of both groups of anonymous authors is likely to remain elusive. The \textit{MJA}’s librarian, Joanne Elliott, informed me on 17 February 1998 that the journal kept archival material for only seven years before destroying it.

\textbf{Institutions}

The University of Adelaide was the most commonly acknowledged institution by authors of \textit{MJA} publications about Indigenous people in the period - in 15
publications (Cleland wrote 13 of these). It was the most frequently acknowledged institution in the 1930s – 9 publications – and had the most pages of text in the 1920s (12.3 pages) and 1930s (39.3 pages).

The only other institutions acknowledged in five or more publications were: the University of Sydney (9 publications), the Australian Institute of Tropical Medicine (AITM) (7), the Australian Hookworm Campaign (5), and the Commonwealth Department of Health (5) (see table A2.5). The AITM was most prominent in the 1910s, the Australian Hookworm Campaign and the University of Adelaide in the 1920s, the University of Adelaide in the 1930s, and the University of Sydney and CSL in the 1940s. The prominence of institutions may merely represent the affiliations of the most prominent authors in the previous section.

Table A2.5. Institutions most often acknowledged in MJA publications about Indigenous people, 1914-1949

<table>
<thead>
<tr>
<th>Institution</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>University of Adelaide</td>
<td>15</td>
<td>63.7</td>
</tr>
<tr>
<td>University of Sydney</td>
<td>9</td>
<td>28.5</td>
</tr>
<tr>
<td>Australian Institute of Tropical Medicine</td>
<td>7</td>
<td>42.9</td>
</tr>
<tr>
<td>Australian Hookworm Campaign</td>
<td>5</td>
<td>19.8</td>
</tr>
<tr>
<td>Commonwealth Department of Health</td>
<td>5</td>
<td>26.7</td>
</tr>
</tbody>
</table>

There were more Indigenous health MJA publications with no address in each decade than for any institution. But not when considering the number of pages of text – reflecting that these predominantly anonymously written articles were shorter than other articles. There was a similar proportion of these publications without an institutional address in the control sample (54% or 15/28) as in the Indigenous health publications (47% or 53/113) (p>0.05, t test).

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4 The most publications for each decade were: AITM (4) in 1910s, Australian Hookworm Campaign (4) in 1920s, University of Adelaide (9) in 1930s, and University of Sydney (4) in 1940s. Whereas most pages of MJA text for each decade were: AITM (16.9 pages) in 1910s, University of Adelaide (12.3 and 39.3) in both the 1920s and 1930s, and CSL (15.5) in the 1940s.
Of the 69 acknowledgements of a specific institution, 36% (25/69) were of a university, 9% (6/69) of a hospital, 7% (5/69) of a government health department, and 14% (10/69) of other research institutions. Of the acknowledgements of a specific institution, the Indigenous health publications had statistically significantly higher proportions of acknowledgements that were of a university (36% versus 11%) or a government health department (7% versus 0%) and a statistically significantly lower proportion that were of a hospital (9% versus 39%) (p<0.05, t test). The proportion of acknowledgements that were of other research institutions was not statistically significantly different in the control sample (6%) (p>0.05, t test).

In the control sample only 2 of the 29 (7%) publications acknowledged a university compared with 22% (25/113) of the Indigenous health publications (Adelaide University, Sydney University, and the University of Hawaii). This difference was statistically significant (p<0.05, t test). There were statistically significantly more publications that acknowledged a hospital in the control sample (21% or 6/29) than in the Indigenous health publications (5% or 6/113) (p<0.05, t test). But some caution with the interpretation of these results is necessary as about half of both the Indigenous and the other MJA publications (including the large number of anonymous publications) did not include an institutional address.

The prominence of institutions may represent merely the affiliations of the most prominent authors. This might suggest that no institution developed a range of research groups in Indigenous health or just that a single researcher or research group into Indigenous health was sufficient to make an institution stand out. It is probably the latter, reflecting the low level interest in Indigenous health at this time. At Adelaide University there was a large group of researchers involved in Cleland’s expeditions to Central Australia. But Cleland was almost always acknowledged as the principal author when this group wrote for the medical audience of the MJA.
Titles

The words (excluding prepositions, determiners, conjunctions ⁶) used in titles 10 or more times (of the 809 words in the 113 titles) are shown in table A2.6. With those words used in titles of more than 20 pages of text are shown in Table A2.7. In contrast, there were no words other than prepositions, determiners, or conjunctions that were used more than once in titles of the control MJA publications. Different words were used in the titles of the control publications. Of the words listed in tables A2.6 and A2.7, only ‘Natives’ was used in the titles of control publications (although ‘Disease’ but not ‘Diseases’ was also used).

Table A2.6. Most common words (excluding prepositions, determiners, conjunctions) used in titles of MJA publications about Indigenous people, 1914-1949

<table>
<thead>
<tr>
<th>Words</th>
<th>Number of times used</th>
</tr>
</thead>
<tbody>
<tr>
<td>Australian</td>
<td>31</td>
</tr>
<tr>
<td>Australia</td>
<td>29</td>
</tr>
<tr>
<td>Aborigines</td>
<td>21</td>
</tr>
<tr>
<td>Aboriginal</td>
<td>15</td>
</tr>
<tr>
<td>Medical</td>
<td>10</td>
</tr>
<tr>
<td>Natives</td>
<td>10</td>
</tr>
</tbody>
</table>

⁶ In the titles of the Indigenous health publications, the most commonly used of these words were – the (used 72 times), of (62), in (61), and (22), to (16), a (15), on (10). These words were similarly ranked in the control sample.
Table A2.7. Most common words (excluding prepositions, determiners, conjunctions) used in titles of *MJA* publications about Indigenous people by pages of associated text, 1914-1949

<table>
<thead>
<tr>
<th>Words</th>
<th>Pages of text</th>
</tr>
</thead>
<tbody>
<tr>
<td>Australian</td>
<td>103.2</td>
</tr>
<tr>
<td>Australia</td>
<td>102.4</td>
</tr>
<tr>
<td>Aborigines</td>
<td>67.0</td>
</tr>
<tr>
<td>Blood</td>
<td>40.9</td>
</tr>
<tr>
<td>Aboriginal</td>
<td>39.4</td>
</tr>
<tr>
<td>Diseases</td>
<td>37.9</td>
</tr>
<tr>
<td>Queensland</td>
<td>31.2</td>
</tr>
</tbody>
</table>

The most common words in the titles of Indigenous health *MJA* publications were those describing Aboriginal people (Australian, Aborigines, Natives, and Aboriginal) and their location (Australian, Australia, Queensland, and North). Few of the commonly used words in titles describe specific research topics or diseases. More common were general medical words – Medical, Diseases, Health, and Public (as in Public Health). One exception was the word ‘Blood’. Its use in titles (and also the use of the word ‘Groups’) mainly referred to research of Aboriginal people’s blood groups. Only two titles used variations of the word ‘Blood’ to describe race-based classifications of Aboriginal people: ‘full-blood’ and ‘White-Aboriginal mixed bloods’.

Mickey Dewar’s PhD thesis (but not her book based on the thesis), which examined the representations in non-academic books about the Northern Territory, similarly examined the words used in the titles of these books. Whilst she was initially optimistic, she found this analysis not very illuminating. I have come to a similar conclusion and have avoided any further interpretation of the analysis of these words as it now appears either too tentative or too speculative.

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1 The next most common words (associated with between 20 and 30 pages of text) were – Insects (28.0 pages), Health (23.9), Public (23.9), Man (22.9), North (22.1), Animals (21.9), Attributable (21.9), Injuries (21.9), Pathological (21.6), Natives (21.3), and Groups (20.4). Of these words, only ‘Injuries’, ‘Pathological’, and ‘Natives’ were used (all only once) in titles of control *MJA* publications. None of these words or those in table A2.6 or A2.7 were used more than once in any title, so the number of times used was the same as the number of titles of publications in which they appeared.
References

Chronological list of selected MJA publications about Indigenous people, 1914 – 1949

20. Tebbutt AH, McConnel SV. On human iso-haemagglutinins with a note on their
distribution amongst some Australian Aborigines. Med J Aust 1922;1:201-
209.
23. Anon (Burkitt AN & Haddon AC & Buck PH & Gullberg & Kenyon). Physical
24. Anon (Tebbutt AH & Haddon AC & Burkitt). Comparative Iso-Agglutinin Index
27. Sweet WC. The intestinal parasites of man in Australia and its dependencies as
235.
29. Sweet WC. The activities of the Australian Hookworm Campaign. Med J Aust
1924;1:Suppl 319-322.
30. Hogg CA. Twelve cases of insanity in Australian Aboriginals with a
31. Cleland JB. Injuries and diseases in Australia attributable to animals (except
32. Hogg CA. Twelve cases of insanity in Australian Aboriginals with a
33. Bostock J. Insanity in the Australian Aboriginal and its bearing on the evolution
36. Lee DHK. Blood groups of North Queensland Aborigines, with a statistical
collection of some published figures for various races. Med J Aust
1926;2:401-410.
1926;2:454.


Other references cited in this appendix


117. Cleland JB. Injuries and diseases of man in Australia attributable to animals (except insects). Australasian Medical Gazette 1912;32:269-274.


120. Dewar MS. In search of the 'Never-Never': the Northern Territory metaphor in Australian writing 1837-1992 [PhD]. Northern Territory University, 1993.

Quantitative analysis, 1950 – 1969

This analysis includes 200 *MJA* publications about Indigenous people from 1950 to 1969 (see chronological list of these publications at the end of this appendix).1-200 One hundred and sixty-nine publications were found using Moodie and Pederson’s bibliography and a further 29 using Harrison’s thesis and 2 using MEDLINE.201,202 Another publication was identified by the bibliography but not included as it did not explicitly mention Aboriginal or Torres Strait Islander people. These 200 publications were compared to a random sample of 50 control *MJA* publications stratified by decade of publication and whether the publications were in supplements or not.

The publications about Indigenous people accounted for 374.9 pages of *MJA* text. The median length of these publications was the same as for publications before 1950, 1.3 pages (interquartile range 0.3 to 2.8), and the mean length was less at 1.8 pages. As in earlier years, there were a large number of very short publications (21% of publications were less than a quarter of a page long) – especially letters to the editor and reports of papers given at medical meetings.

The control *MJA* publications were slightly longer but this difference was not statistically significant (*p* > 0.05, Mann Whitney test). The median length of the control *MJA* publications was 1.7 pages (interquartile range 0.3 to 2.8) and the mean length 1.9 pages. A similar proportion (24%) of publications were less than a quarter of a page long.
Table A3.1 shows the increasing numbers of Indigenous health *MJA* publications at the end of the 1950s, decreasing in the mid 1960s and then increasing at the end of the 1960s – with more publications in the 1960s (115) than the 1950s (85). Table 1 also demonstrates that the most number of publications in any one year was in 1969, with 25 publications and 40.3 pages of text. Other years with large numbers of publications and pages of text were 1957 (18 publications, 38.1 pages), 1959 (14 publications, 22.3 pages), 1964 (13 publications, 28.2 pages), and 1968 (14 publications, 11.2 pages).

Table A3.1. *MJA* publications about Indigenous people by year, 1950-1960

<table>
<thead>
<tr>
<th>Year</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>1950</td>
<td>4</td>
<td>16.2</td>
</tr>
<tr>
<td>1951</td>
<td>5</td>
<td>19.9</td>
</tr>
<tr>
<td>1952</td>
<td>7</td>
<td>8.6</td>
</tr>
<tr>
<td>1953</td>
<td>9</td>
<td>25.1</td>
</tr>
<tr>
<td>1954</td>
<td>1</td>
<td>0.7</td>
</tr>
<tr>
<td>1955</td>
<td>7</td>
<td>7.9</td>
</tr>
<tr>
<td>1956</td>
<td>11</td>
<td>14.7</td>
</tr>
<tr>
<td>1957</td>
<td>18</td>
<td>37.7</td>
</tr>
<tr>
<td>1958</td>
<td>9</td>
<td>38.1</td>
</tr>
<tr>
<td>1959</td>
<td>14</td>
<td>22.3</td>
</tr>
<tr>
<td>1960</td>
<td>11</td>
<td>14.3</td>
</tr>
<tr>
<td>1961</td>
<td>10</td>
<td>14.2</td>
</tr>
<tr>
<td>1962</td>
<td>9</td>
<td>7.4</td>
</tr>
<tr>
<td>1963</td>
<td>7</td>
<td>12.1</td>
</tr>
<tr>
<td>1964</td>
<td>13</td>
<td>28.2</td>
</tr>
<tr>
<td>1965</td>
<td>9</td>
<td>12.5</td>
</tr>
<tr>
<td>1966</td>
<td>8</td>
<td>17.5</td>
</tr>
<tr>
<td>1967</td>
<td>9</td>
<td>26.0</td>
</tr>
<tr>
<td>1968</td>
<td>14</td>
<td>11.2</td>
</tr>
<tr>
<td>1969</td>
<td>25</td>
<td>40.3</td>
</tr>
</tbody>
</table>

Of the 200 Indigenous health *MJA* publications, 109 (55%) were articles, 48 (24%) were letters to the editor, and 43 (22%) were reports or reviews. All the 13 publications to appear in supplements were published after 1963 – with seven published in 1968. The 109 articles accounted for 343.5 (92%) of the total 374.9 pages (letters: 16.9 pages (5%) and reports: 14.5 pages (4%)).
There were statistically significantly less letters to the editor in the control publications (10% of total) than in the Indigenous health publications \((p<0.05, t\) test). There were similar proportions of articles (56% of total publications) and slightly but not statistically significantly more reports (34%) in the control sample as in the Indigenous health sample \((p>0.05, t\) test).

There were statistically significantly more letters to the editor and less reports than in Indigenous health *MJA* publications before 1950 (when 12% of publications were letters and 33% reports) \((p<0.05, t\) test). But the proportion of Indigenous health *MJA* publications that were articles was very similar (56% before 1950).

**Authors**

Table A3.2 shows the most prolific authors of Indigenous health *MJA* publications in this period. The most prominent author in the period was Barry Christophers, a general practitioner from Richmond in Melbourne who was a member of the Council for Aboriginal Rights (Victoria) and the Federal Council for Aboriginal Advancement.\(^4^4,4^7,6^0,7^2,7^6,9^1,9^4,9^8,9^9,1^0^2,1^0^8-1^1^0,1^1^2,1^2^4,1^2^5,1^2^8,1^3^7\) He was the author of the most publications (and of the most publications as a first author) in both the 1950s and 1960s. As he only published letters to the editor which were usually quite short, these 19 publications only accounted for 7.2 pages of text.

Table A3.2 also includes three authors associated with the revival of the Board for Anthropological Research's expeditions from Adelaide after the Second World War – Andrew Abbie\(^6^2,8^9,9^6,1^1^3\) and John Casley-Smith\(^6^1,6^6,6^7,8^0\) from Adelaide University and John Miles\(^1^7,2^4,2^5,2^9,3^8\) from the Institute of Medical and Veterinary Science (IMVS) in Adelaide. The psychiatrist John Cawte also wrote three articles about these expeditions\(^1^2^6,1^3^5,1^3^9\) The other author mentioned in table A3.2 is John Crotty, a pathologist with the Northern Territory Medical Service\(^6^9,9^5,1^1^7,1^2^2,1^3^6\) Only

\(^a\) Christophers published an additional six letters to the *MJA* editor about Indigenous health that were not cited in the Aboriginal health bibliography or Harrison's thesis.\(^2^0^3-2^0^8\)
Christophers (19 times) and Crotty (4 times) were listed as the first author of more than three Indigenous health *MJA* publications.

**Table A3.2. Most prolific authors of *MJA* publications about Indigenous people, 1950-1960**

<table>
<thead>
<tr>
<th>Author</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>Barry Christophers</td>
<td>19</td>
<td>7.2</td>
</tr>
<tr>
<td>John Miles</td>
<td>5</td>
<td>19.3</td>
</tr>
<tr>
<td>John Crotty</td>
<td>5</td>
<td>12.4</td>
</tr>
<tr>
<td>Andrew Abbie</td>
<td>4</td>
<td>11.5</td>
</tr>
<tr>
<td>John Casley-Smith</td>
<td>4</td>
<td>11.9</td>
</tr>
</tbody>
</table>

Of the 200 Indigenous health publications, 14% (27 publications, 7.2 pages of text) were anonymously written short publications that simply reported a paper or work by a named author. This sort of anonymously written report was more common in the 1960s (18) than the 1950s (9) because of reports of a large number of papers about Indigenous health at two conferences: the Australian Medical Congress of the BMA and AMA in 1968 and 1969 seminar on Aboriginal Health organised by the Post-graduate Committee in Medicine of the University of Adelaide. There was a similar proportion of such anonymous reports in the control sample (6/50 or 12%) (p>0.05, t test); but a (not quite statistically significantly) higher proportion of such reports about Indigenous health in the *MJA* before 1950 (23%).

There were 18 (9% of the total publications) (16.7 pages of text) other anonymously written Indigenous health *MJA* publications. Most of these (14/18) were published in the in the 1960s. This type of anonymous publication accounted for the same proportion of Indigenous *MJA* publications before 1950. There was a not statistically significantly larger proportion (10/50 or 20%) of such anonymous publications in the control sample (p>0.05, t test).
Institutions

The University of Adelaide was the again the most commonly acknowledged institution by authors *MJA* publications about Indigenous people in the period - in 20 publications (48.8 pages of text). It was the most frequently acknowledged institution in the 1950s (7 publications) and 60s (13 publications) and had the most pages of text in the 1950s. However, when the various names used to describe the government health services in the Northern Territory (Department of Health NT, NT Medical Service, and the Commonwealth Department of Health) were combined, these organisations were acknowledged by the authors of a similar number of publications as Adelaide University – 21 publications (49.9 pages). Most of these NT publications (17/21) were in the 1960s. Table A3.3 shows that the other institutions acknowledged in five or more publications were: IMVS (9), University of Western Australia (UWA) (7), University of Sydney (6), Royal Perth Hospital (5), and the University of New South Wales (UNSW) (5).

<table>
<thead>
<tr>
<th>Institution</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>University of Adelaide</td>
<td>20</td>
<td>48.8</td>
</tr>
<tr>
<td>IMVS</td>
<td>9</td>
<td>27.1</td>
</tr>
<tr>
<td>NT Medical Service</td>
<td>9</td>
<td>20.0</td>
</tr>
<tr>
<td>UWA</td>
<td>7</td>
<td>15.9</td>
</tr>
<tr>
<td>University of Sydney</td>
<td>6</td>
<td>23.7</td>
</tr>
<tr>
<td>Commonwealth Department of Health</td>
<td>6</td>
<td>21.7</td>
</tr>
<tr>
<td>Department of Health NT</td>
<td>6</td>
<td>8.2</td>
</tr>
<tr>
<td>UNSW</td>
<td>5</td>
<td>23.7</td>
</tr>
<tr>
<td>Royal Perth Hospital</td>
<td>5</td>
<td>8.7</td>
</tr>
</tbody>
</table>

There were many more publications with neither address nor institution acknowledged (51 publications) or for which only an address but no institution.

_b_ This does not include addresses which were merely the name of a capital city (but did include addresses that were only the name of a non-capital city). These capital city addresses were treated as if no address or institution was provided.
were given (37) than publications which acknowledged any individual institution. Of the 128 acknowledgements of a specific institution, 31% (40/128) were of a university, 23% (29/128) of a hospital, 22% (28/128) of a government health department, and 10% (13/128) of other research institutions. Compared with Indigenous health MJA publications before 1950, there were statistically significantly greater proportions of acknowledgements that were of a government department (7% before 1950) and of a hospital (9%). These changes can be largely explained by the increased number of publications by authors from the government health services in the Northern Territory and from hospitals in both Western Australia and Queensland. A shift was beginning to occur from academics in universities in the south-east of Australia to doctors involved in the delivery of health care to Aboriginal people in northern, central, and western Australia.

In the control MJA publications, as in the Indigenous health publications, there were many publications with neither an address nor an institution acknowledged (24/50 publications) but only three for which only an address but no institution were given. Of the 30 acknowledgements of a specific institution in control publications, 63% (19/30) were of a hospital, 17% (5/30) of a university, 10% (3/30) of other research institutions, and none were of a government health department. There was a statistically significant smaller proportion of these acknowledgements of a government health department than in the Indigenous health MJA publications; the other differences were not statistically significant.

**Titles**

The words (excluding prepositions, determiners, conjunctions) used in titles 10 or more times (of the 416 different words and 1423 total words in the 200 titles) are shown in table A3.4. Those words used in titles of more than 30 pages of text are shown in table A3.5. The words used in the titles of Indigenous health publications

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5 In the titles of the Indigenous health publications, the most commonly used of these words were – the (used 136 times), of (102), in (100), and (53), a (27), an (12), for (12), on (10). These words were similarly ranked in the control sample.
in the 1950s and 1960s were not much different from those used before 1950. Words describing Aboriginal people — ‘Aborigines’, ‘Aboriginal’, ‘Australian’, and ‘Natives’ — and location — ‘Australian’, ‘Northern’, ‘Australia’, ‘Territory’ — were again commonly used. There were a few words that reflected either a specific disease (‘Leprosy’) or specific research. ‘Blood’ and ‘Serum’ were used in the titles of virological, haematological and cardiovascular research that used Aboriginal blood — rather than to describe the blood group research which had been so prominent between the wars.

Different words were used in the titles of the control publications. ‘Treatment’ was the only word other than prepositions, determiners, or conjunctions that was used more than twice in titles of the control MJA publications. Of the words listed in tables A3.4 and A3.5, only ‘Australian’, ‘Blood’, ‘Children’, ‘Medical’, and ‘Serum’ were used in the titles of control publications.

Table A3.4. Most common words (excluding prepositions, determiners, conjunctions) used in titles of MJA publications about Indigenous people, 1950-1960

<table>
<thead>
<tr>
<th>Words</th>
<th>Number of times used</th>
</tr>
</thead>
<tbody>
<tr>
<td>Aborigines</td>
<td>54</td>
</tr>
<tr>
<td>Aboriginal</td>
<td>46</td>
</tr>
<tr>
<td>Australian</td>
<td>46</td>
</tr>
<tr>
<td>Northern</td>
<td>33</td>
</tr>
<tr>
<td>Australia</td>
<td>32</td>
</tr>
<tr>
<td>Territory</td>
<td>30</td>
</tr>
<tr>
<td>Blood</td>
<td>12</td>
</tr>
<tr>
<td>Children</td>
<td>11</td>
</tr>
<tr>
<td>Future</td>
<td>11</td>
</tr>
<tr>
<td>Serum</td>
<td>11</td>
</tr>
<tr>
<td>Leprosy</td>
<td>10</td>
</tr>
<tr>
<td>Medical</td>
<td>10</td>
</tr>
</tbody>
</table>

*d These words were used twice: appointment, babies, bladder, blood, case, childhood, children, chronic, ear, general, headache, report, review, surgery, syndrome, therapy, and throat.
Table A3.5. Most common words (excluding prepositions, determiners, conjunctions) used in titles of *MJA* publications about Indigenous people, by pages of associated text, 1950-1960.*

<table>
<thead>
<tr>
<th>Words</th>
<th>Pages of text</th>
</tr>
</thead>
<tbody>
<tr>
<td>Australia</td>
<td>104.5</td>
</tr>
<tr>
<td>Australian</td>
<td>102.2</td>
</tr>
<tr>
<td>Northern</td>
<td>91.1</td>
</tr>
<tr>
<td>Aborigines</td>
<td>87.6</td>
</tr>
<tr>
<td>Territory</td>
<td>75.7</td>
</tr>
<tr>
<td>Aboriginal</td>
<td>71.6</td>
</tr>
</tbody>
</table>

*The next most common words (associated with between 20 and 30 pages of text) were – ‘Children’ (29.5), ‘Central’ (25.9), ‘Serum’ (24.3), ‘Survey’ (24.1), ‘Natives’ (24.0), ‘Observations’ (23.8), ‘South’ (22.2), ‘Medicine’ (21.5), and ‘Medical’ (20.5). ‘Australia’ was used twice in one title. Otherwise, none of these words or those in Table 5 or 6 were used more than once in any title, so the number of times used was the same as the number of titles of publications in which they appeared.*
References

Chronological list of selected MJA publications about Indigenous people, 1950 – 1969


358


Other references cited in this appendix


Appendix 4

Quantitative analysis, 1970 – 1999

This analysis describes the 709 MJA publications about Indigenous people from 1970 to 1999. These were found using the MEDLINE (the most recent version searched was dated September 2000), Australian Medical Index (AMI) (dated May 2000), and ATSIhealth databases (May 2000); and Thomson and Merrifield’s 1988 Aboriginal health bibliography.

The Indigenous health publications accounted for 1540.0 pages of MJA text. The median length of these publications was 1.7 pages (interquartile range 0.4 to 3.4), and the mean length was 2.2 pages. Compared with earlier years, there were a smaller number of very short publications (11% of publications were less than a quarter of a page long).

Table A4.1 shows the peaks in the annual number of publications in the mid 1970s and mid 1990s – with more publications in the 1990s (283) than either the 1970s (222) or 1980s (204). Table A4.1 also demonstrates that the most number of publications in any one year was in 1993, with 57 publications and 141.4 pages of text. Other years with larger numbers of publications were 1975 (38 publications, 120.5 pages), 1983 (34 publications, 49.7 pages), and 1994 (40 publications, 55.5 pages).
Table A4.1. *MJA* publications about Indigenous people by year, 1970-1999

<table>
<thead>
<tr>
<th>Year</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>1970</td>
<td>17</td>
<td>70.1</td>
</tr>
<tr>
<td>1971</td>
<td>8</td>
<td>35.7</td>
</tr>
<tr>
<td>1972</td>
<td>20</td>
<td>53.1</td>
</tr>
<tr>
<td>1973</td>
<td>26</td>
<td>61.6</td>
</tr>
<tr>
<td>1974</td>
<td>24</td>
<td>42.1</td>
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<tr>
<td>1975</td>
<td>38</td>
<td>120.5</td>
</tr>
<tr>
<td>1976</td>
<td>18</td>
<td>52.1</td>
</tr>
<tr>
<td>1977</td>
<td>23</td>
<td>50.9</td>
</tr>
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<td>1978</td>
<td>26</td>
<td>51.8</td>
</tr>
<tr>
<td>1979</td>
<td>22</td>
<td>39.1</td>
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<tr>
<td>1980</td>
<td>24</td>
<td>50.3</td>
</tr>
<tr>
<td>1981</td>
<td>23</td>
<td>33.9</td>
</tr>
<tr>
<td>1982</td>
<td>11</td>
<td>10.3</td>
</tr>
<tr>
<td>1983</td>
<td>34</td>
<td>49.7</td>
</tr>
<tr>
<td>1984</td>
<td>18</td>
<td>25.2</td>
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<td>1985</td>
<td>22</td>
<td>31.5</td>
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<td>1986</td>
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<td>21.2</td>
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<td>1987</td>
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<td>23.5</td>
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<td>1988</td>
<td>21</td>
<td>41.5</td>
</tr>
<tr>
<td>1989</td>
<td>20</td>
<td>41.0</td>
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<tr>
<td>1990</td>
<td>23</td>
<td>48.2</td>
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<tr>
<td>1991</td>
<td>30</td>
<td>75.5</td>
</tr>
<tr>
<td>1992</td>
<td>31</td>
<td>73.1</td>
</tr>
<tr>
<td>1993</td>
<td>57</td>
<td>141.4</td>
</tr>
<tr>
<td>1994</td>
<td>40</td>
<td>55.5</td>
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<tr>
<td>1995</td>
<td>27</td>
<td>48.3</td>
</tr>
<tr>
<td>1996</td>
<td>25</td>
<td>80.2</td>
</tr>
<tr>
<td>1997</td>
<td>16</td>
<td>24.7</td>
</tr>
<tr>
<td>1998</td>
<td>14</td>
<td>39.2</td>
</tr>
<tr>
<td>1999</td>
<td>20</td>
<td>48.8</td>
</tr>
</tbody>
</table>

Of these 709 Indigenous health publications, 454 (64%) were articles, 220 (31%) were letters to the editor, and only 35 (5%) were reports or reviews. There was a marked increase in the number letters published in the 1980s (76 letters - 37% of the 204 publications) and 1990s (108 letters - 38% of 283 publications) compared with the 1970s (36 letters - 16% of 222 publications). There was an even more marked decline in the number of reports or reviews.

More than 13% (95/709) of these publications appeared in supplements – with all but five of these published from 1974 to 1986 (and 21 published in 1975). More
than a quarter (27% or 59/222) of the publications in the 1970s were published in special supplements to the journal. Compared with publications in the 1950s and 1960s, there was a statistically significantly greater proportion of publications that were letters to the editor, articles and published in supplements and a smaller proportion that were reports (p<0.05, t test).

Authors

Table A4.2 shows the most prolific authors of Indigenous health publications in this period and table A4.3 ranks only those authors listed as the first author of publications. The most prominent authors were Michael Gracey from Western Australia throughout the period, Max Kamien for his work in Bourke in the 1970s, Jeffrey Hanna from Central Australia in the 1980s and north Queensland in the 1990s, and Bart Currie from Darwin and Paul Prociv of Brisbane both in the 1990s. There were only 26 (4% of the total publications) (30.9 pages of text) anonymously written Indigenous health publications in the *MJA* in this period. Most of these (21/26) were published in the in the 1970s and there were no anonymous Indigenous health publications in the 1990s. There were no anonymous reports of papers given by named authors in this period.

<table>
<thead>
<tr>
<th>Author</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>Michael Gracey</td>
<td>25</td>
<td>64.8</td>
</tr>
<tr>
<td>Bart Currie</td>
<td>18</td>
<td>28.5</td>
</tr>
<tr>
<td>Jeffrey Hanna</td>
<td>17</td>
<td>39.9</td>
</tr>
<tr>
<td>Max Kamien</td>
<td>15</td>
<td>61.3</td>
</tr>
<tr>
<td>Randolph Spargo</td>
<td>15</td>
<td>41.7</td>
</tr>
<tr>
<td>John Mathews</td>
<td>13</td>
<td>37.1</td>
</tr>
<tr>
<td>Paul Prociv</td>
<td>13</td>
<td>20.7</td>
</tr>
<tr>
<td>Alan Dugdale</td>
<td>12</td>
<td>35.8</td>
</tr>
<tr>
<td>Alan Walker</td>
<td>11</td>
<td>27.6</td>
</tr>
<tr>
<td>John Ca'te</td>
<td>10</td>
<td>25.8</td>
</tr>
</tbody>
</table>
Table A4.3. Most prolific first authors of *MJA* publications about Indigenous people, 1970-99

<table>
<thead>
<tr>
<th>Author</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>Max Kamien</td>
<td>14</td>
<td>58.3</td>
</tr>
<tr>
<td>Jeffrey Hanna</td>
<td>13</td>
<td>29.2</td>
</tr>
<tr>
<td>Michael Gracey</td>
<td>11</td>
<td>24.5</td>
</tr>
<tr>
<td>Paul Prociv</td>
<td>11</td>
<td>16.7</td>
</tr>
</tbody>
</table>

**Institutions**

Table A4.4 shows the most commonly acknowledged institutions by authors of Indigenous health *MJA* publications in the period. Health departments and other institutions from the Northern Territory, Western Australia and Queensland were more prominent than universities from south-eastern Australia, although the University of New South Wales was particularly prominent in the 1970s (24 publications). Menzies School of Health Research in Darwin was similarly prominent in the 1990s (48 publications).

Table A4.4. Institutions most often acknowledged in *MJA* publications about Indigenous people, 1970-99

<table>
<thead>
<tr>
<th>Institution</th>
<th>Number of publications</th>
<th>Number of pages</th>
</tr>
</thead>
<tbody>
<tr>
<td>University of Queensland</td>
<td>63</td>
<td>169.7</td>
</tr>
<tr>
<td>Department of Health NT&lt;sup&gt;a&lt;/sup&gt;</td>
<td>58</td>
<td>132.6</td>
</tr>
<tr>
<td>Royal Darwin Hospital&lt;sup&gt;b&lt;/sup&gt;</td>
<td>53</td>
<td>141.5</td>
</tr>
<tr>
<td>Menzies School of Health Research</td>
<td>50</td>
<td>104.4</td>
</tr>
<tr>
<td>Health Department WA</td>
<td>41</td>
<td>115.6</td>
</tr>
<tr>
<td>Queensland Health Department</td>
<td>33</td>
<td>78.5</td>
</tr>
<tr>
<td>University of New South Wales</td>
<td>32</td>
<td>135.5</td>
</tr>
<tr>
<td>University of Sydney</td>
<td>31</td>
<td>91.1</td>
</tr>
<tr>
<td>University of Western Australia</td>
<td>31</td>
<td>88.6</td>
</tr>
</tbody>
</table>

<sup>a</sup> Includes 3 references to Northern Territory Medical Service in 1970s and 11 references to Territory Health Services in the 1990s.

<sup>b</sup> Includes 15 references to Darwin Hospital in 1970s and 1980s.
There were still a large number of publications with neither address nor institution acknowledged (60 publications) or for which only an address \(^c\) but no institution were given (55) than publications which acknowledged any individual institution.

**Titles**

The words (excluding prepositions, determiners, conjunctions) used in titles 40 or more times (of the 1328 different words and 5809 total words in the 709 titles) are shown in table A4.5. In nearly half (48/112) of the titles in which 'Health' appeared, 'Health' followed the word 'Aboriginal'. So, the expression 'Aboriginal health', which had not appeared in the *MJA* before 1969 (see chapter 6), appeared in the titles of 7% (48/709) of *MJA* publications about Aboriginal and Torres Strait Islander people from 1970 to 1999.

**Table A4.5. Most common words (excluding prepositions, determiners, conjunctions) used in titles of *MJA* publications about Indigenous people, 1970-99**

<table>
<thead>
<tr>
<th>Words</th>
<th>Number of times used</th>
<th>Pages of text</th>
</tr>
</thead>
<tbody>
<tr>
<td>Aboriginal</td>
<td>292</td>
<td>637.4</td>
</tr>
<tr>
<td>Health</td>
<td>112</td>
<td>224.1</td>
</tr>
<tr>
<td>Australia</td>
<td>110</td>
<td>298.0</td>
</tr>
<tr>
<td>Australian</td>
<td>90</td>
<td>237.7</td>
</tr>
<tr>
<td>Children</td>
<td>87</td>
<td>235.0</td>
</tr>
<tr>
<td>Aborigines</td>
<td>72</td>
<td>179.5</td>
</tr>
<tr>
<td>Disease</td>
<td>72</td>
<td>164.3</td>
</tr>
<tr>
<td>Community</td>
<td>49</td>
<td>127.5</td>
</tr>
<tr>
<td>Northern</td>
<td>49</td>
<td>105.8</td>
</tr>
<tr>
<td>Territory</td>
<td>44</td>
<td>92.8</td>
</tr>
<tr>
<td>Western</td>
<td>42</td>
<td>126.4</td>
</tr>
</tbody>
</table>

\(^c\) This did not include addresses which were merely the name of a capital city (but did include addresses that were only the name of a non-capital city). These capital city addresses were treated as if no address or institution was provided.
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CRS A452, 1952/179.
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CRS A6119/78, 1061.
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E 51/0, 1964/594.
F1, 1970/553.
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Ted Milliken, 6 November, 2000.
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