

HUMANITIES RESEARCH

Vol XIV. No. 1. 2007



The journal of the Research School of Humanities
The Australian National University

HUMANITIES RESEARCH

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Humanities Research is published by the Research School of Humanities at The Australian National University. The Research School of Humanities came into existence in January 2007 and consists of the Humanities Research Centre, Centre for Cross-Cultural Research, National Europe Centre and National Dictionary Centre.

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Published by ANU E Press
Email: anuepress@anu.edu.au
Website: <http://epress.anu.edu.au>

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Printed in Australia

VOL XIV. NO. 1. 2007

ISSN: 1440-0669 (PRINT), ISSN: 1834-8491 (ONLINE)

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HISTORICIZING “CROSS-CULTURAL”

BENJAMIN PENNY

In 2000, a few years into the 10-year history of the ANU's Centre for Cross-Cultural Research, a new field of research for the Centre was announced: “Conceptualising Cross-Cultural Research”, which in later years became “Interrogating Concepts of the Cross-Cultural”.¹ The 2007 iteration of the website summarizes it in this way:

By “cross-cultural research” we mean scholarship that is oriented towards tracing patterns of transaction and translation between cultures. Methodologically, such scholarship transcends conventional national and area studies frames of reference by recognising the increasing porousness of cultural boundaries. This program examines both the disciplinary and interdisciplinary ramifications of the term “cross-cultural” in Humanities research. It does so by exploring the theoretical links between the notion of the “cross cultural” as it has emerged in the disciplinary fields of anthropology, history, literary studies and linguistics, and contemporary conceptualisations of “cultural difference” in the transdisciplinary fields of postcolonial, migration and globalisation studies.²

Although this description locates the particular interest of “cross-cultural research”

in recent changes in the state of the world and of academic disciplines, it is clear that “transaction and translation between cultures” has been going on for as long as there have been people, and the “tracing of patterns” in this process is by no means only a recent phenomenon. The essays in this volume are concerned with examining how such patterns were traced before the middle of the twentieth century, when the term “cross-cultural” was coined. They therefore involve studies both of particular encounters between people of different cultures and investigation of the disciplinary categories in which those studies took place.

The literature of encounter between people from different cultural backgrounds is, of course, vast and the essays here only address a few examples of the rich legacy of work left by generations of explorers, traders, missionaries and consular officials, as well as people who thought of themselves as scholars. Some small amount of this work is well known but more of it is much less read than it should be and, in general, deserves rediscovery and reassessment. The people who conducted this research worked within the paradigms of their own eras: the ways they thought through what they saw and heard may sound unfamiliar, if not simply odd, to a contemporary ear, but such perplexity is all to the good, as it makes us ponder the earlier forms — indeed, often

the foundations — of the disciplines that currently hold sway.

However, just as the essays in this volume seek to historicize “cross-cultural research”, it is also possible, and illuminating, to historicize the word “cross-cultural” itself, and the major part of this essay will be concerned with the first significant academic project to use the term “cross-cultural” in its title. It is important to do this to expose the difference in conceptions between its use today and what it meant at the time of its coining, in order to lay bare the foundations of the field.

The first citation the *Oxford English Dictionary* gives for “cross-cultural” comes from Malinowski’s *A Scientific Theory of Culture* (1941) in a chapter outlining “Concepts and Methods in Anthropology”. Having discussed “evolutionism” and “diffusionism”, and mentioning functionalism in passing, Malinowski says:

Thus there is the comparative method, in which the student is primarily interested in gathering extensive cross-cultural documentations, such as we see in Frazer’s *The Golden Bough*, or in Tylor’s *Primitive Culture*, or in the volumes of Westermarck on marriage and morals. In such works the authors are primarily interested in laying bare the essential nature of animistic belief or magical rite, of a phase in human culture or a type of essential organization. Obviously, this whole approach presupposes a really scientific definition of the realities compared. Unless we list, in our exhaustive inventories, really comparable phenomena, and are never

duped by surface similarities or fictitious analogies, a great deal of labor may lead to incorrect conclusions.³

There are two points that I want to focus on from this passage. First, Malinowski saw Frazer, Tylor and Westermarck — he was probably referring to Westermarck’s *The History of Human Marriage* and *The Origin and Development of the Moral Ideas*⁴ — as using comparison to reveal “the essential nature” of a particular phenomenon, much as nineteenth-century classical philologists sought the underlying Indo-European language by comparison of known tongues. In such projects, it is often the origin or source of a specific cultural activity that is the primary goal. This presumes, of course, that there is a shared cultural substratum in humanity; indeed, it may have been argued, that substratum is made up of those essential characteristics that make us human. Secondly, comparison, for Malinowski, meant “cross-cultural documentations” — but it was imperative that those comparisons be made between “really comparable phenomena” specified by a “really scientific definition”. He was obviously reacting against some earlier excesses of the comparative method here, but nonetheless we might baulk, some 65 years after Malinowski’s death, at the notion that cultural phenomena can be defined with such accuracy and precision, and at the tendency towards circularity in so reducing the set of items we might compare to only such things that we define as being “really comparable” in the first place.

Even so, it is important to recognize that at its appearance in academic writing at least, “cross-cultural” collocated most

comfortably with the idea that insights into the nature of the human condition could best be drawn through comparing the various forms that particular features of people's lives took in different places and, as we shall see, at different times. The *OED*'s definition for "cross-cultural" indicates that it appeared before 1940 and, indeed, in 1937 Yale University launched a major project under the name of "The Cross-Cultural Survey", later incorporated into the Human Relations Area Files.⁵ This survey, headed up by George Peter Murdock (1897–1985), produced both the *Outline of Cultural Materials*, with its first edition in 1938, and the supplementary *Outline of World Cultures*, first published in 1954. Both works continue to be revised and published and are now available electronically. Murdock explained the genesis of the project in an article from 1940:

For a number of years, the Institute of Human Relations at Yale University has been conducting a general program of research in the social sciences, with particular reference to the areas common to, and marginal between, the special sciences of sociology, anthropology, psychology, and psychiatry. In 1937, as one of the specific research projects on the anthropological and sociological side of this program, the Cross-Cultural Survey was organized.

A year of previous experience in collaborating with other social scientists in research and discussion had made it clear to the anthropologists associated with the Institute that the rich resources of ethnography, potentially of inestimable value to workers in adja-

cent fields, were practically inaccessible to them. Working in the laboratory, the clinic, or the community, the psychologists, sociologists, and others made frequent requests of the cultural anthropologists for comparative data on various aspects of behavior among primitive peoples. Sometimes they wanted perspective, sometimes suggestions, sometimes a check on their own scientific formulations. In trying to assist them, the anthropologists found that they could usually cite a limited number of cases from their own knowledge and give an impressionistic judgment as to the general status of ethnography on the question. For scientists, however, this was often not enough. What guarantee was there that the remembered cases were representative, or the impressions valid? What was needed was access to a dependable and objective sample of the ethnographic evidence. Only rarely was it possible to refer the seeker to an adequate summary of the evidence; in the great majority of instances, he could satisfy his scientific curiosity only by resorting to the vast descriptive literature itself and embarking on a research task of discouraging magnitude.⁶

To overcome this problem, Murdock established the Cross-Cultural Survey, which was designed to be "a representative sample of the cultural materials on the various societies of the world...organized for ready accessibility on any subject".⁷ This was an encyclopaedic project; one that sought an Olympian view of all hu-

manity, a kind of grand ethnographic panopticon with each discrete unit of culture defined and arranged for easy comparison. The foreword to the first edition of the *Outline of Cultural Materials* states that it was “designed primarily for the organization of the available information on a large and representative sample of known cultures with the object of testing cross-cultural generalizations, revealing deficiencies in the descriptive literature, and directing corrective fieldwork”.⁸ By the third edition, the goal was significantly more ambitious: the “large and representative sample of known cultures” had become, by 1950, “a statistically representative sample of all known cultures, primitive, historical, and contemporary”.⁹ Thus, there were two processes necessary for this project to be fulfilled. First, materials needed to be gathered; secondly, they needed to be classified. Murdock explained the progress in collection in 1940 in this way:

Since the publication of the manual, in 1938, the staff of the Cross-Cultural Survey has been engaged in the actual assembling of materials. To date, the descriptive data on nearly a hundred cultures have been abstracted, classified, and filed. It is hoped ultimately to assemble and organize all the available cultural information on several hundred peoples, who will be adequately distributed with regard to geography and fairly representative of all major types and levels of culture. Although primitive cultures will preponderate numerically, because they reveal the widest range of human behavioral variations, there

will be a fair representation of the historical civilizations of the past, of modern folk cultures, and of the communities studied by contemporary sociologists.¹⁰

The single-minded collection of data was not something that Murdock simply delegated to his staff. An obituary by John W.M. Whiting, one of his former students, in the *American Anthropologist* recalled that,

When I was a graduate student at Yale in the 1930s, Pete [as he was known to friends and family] would spend nearly every weekday night from 8.00 p.m. to 5.00 a.m. in the Yale library examining every possible source of ethnographic information, identifying the group described and listing all the references. As a consequence he was able to publish a listing of all known cultures of the world — The Outlines of World Cultures. This served as an approximation of the *universe* of known peoples of the world, which was necessary if the aim of the files was to produce a representative sample of this universe.¹¹

The completeness of Murdock’s files is indicated by his description of the methods of collection:

For each of the cultures analyzed, the entire literature is covered, including manuscript materials when available. In some instances, more than a hundred books and articles have been combed for a single tribe or historical period. All material in foreign languages has

been translated into English. The information, if of any conceivable cultural relevance, is transcribed in full — in verbatim quotations or exact translations. The object has been to record the data so completely that, save in rare instances, it will be entirely unnecessary for a researcher using the files to consult the original sources.¹²

Classification of the data was according to two broad criteria. The first was geographical: the world was divided into continents or their equivalent, then countries or large portions of countries, then specific groups. Thus, Australia is found under Oceania, with the sub-classifications: "Australia [in general], Historical Australia, Norfolk Island, Prehistoric Australia, Australian Aborigines [in general], Andedja, Arabana, Aranda, Barkindji, Dieri, Kabikabi, Kamilaroi, Karadjeri, Kariera, Kawadji, Kurnai, Murngin, Narrinyeri, Tasmanians, Tiwi, Ualarai, Wikmunkan, Wogait, Worimi, Yiriyoront, Yungar".¹³ The second criterion was according to content and is much more complex. In a system reminiscent of *Roget's Thesaurus*, the entirety of human activity is broken down into 79 sections and 619 subsections. The editors of *Outline of Cultural Materials* write:

The reader must expect to find classified under the same heading such superficially divergent phenomena as the Indian medicine man and the modern psychoanalyst under Category 756 (Psychotherapists), and the primitive quarrying of flint and the contemporary activities of the Anaconda Copper Company under Category 316 (Mining and Quarrying). Sim-

ilarly, there can be no special category like "Christianity," pertaining to only a limited number of cultures, but only general categories like 779 (Theological Systems).¹⁴

The editors remark that, "any element of culture may have as many as seven major facets any one of which may be used as the primary basis of classification", and proceed to list these facets as being:

1. a "patterned activity" (travel, conversation, crime),
2. only occurring under certain circumstances (rest days and holidays, disasters, menstruation),
3. being associated with a particular subject (division of labour by sex, sibs, priesthood),
4. being commonly directed towards some object (poultry raising, kin relationships, child care),
5. being accomplished by some external means (telephone and telegraph, weapons, mutual aid, agency),
6. being normally performed with a purpose or goal (mnemonic device, sorcery, techniques of inculcation),
7. having a concrete result (shipbuilding, sanctions).¹⁵

Systems of classification are, of course, challenging to develop but this one does seem both arbitrary and, in the character of those examples thrust together in parentheses, to approach self-parody.¹⁶ However, even if we discount the problems associated with developing a taxonomy, the subsequent process of classification of any given cultural phenomenon is itself often complex, difficult and as arbitrary as the classificatory categories themselves.¹⁷

However, for the present purposes the details of the classification scheme that Murdock and his colleagues developed are less important than the fact that he did attempt to encompass “the *universe* of known peoples of the world” and to produce “a representative sample” of them for the purposes of making comparisons. This striving towards a rigorously defined taxonomy of the entirety of human experience was by no means a new goal. Murdock and his colleagues were clear methodological descendents of Edward Tylor, whose “On a Method of Investigating the Development of Institutions; Applied to Laws of Marriage and Descent” provided the model for this variety of research. As George Stocking remarked, in relation to Herbert Spencer’s *Descriptive Sociology*, “Spencer may thus be regarded as the ultimate source of the later Human Relations Area Files; Tylor, of the systematic comparative cross-cultural study of the data they contain.”¹⁸ The purpose of comparison underlying the Cross-Cultural Survey was to find systematic correlations between cultural variables, as Tylor had done. Thus, to take one example, for Guy E. Swanson to answer the question “From what experiences do the ideas of the supernatural and its myriad forms arise?”, he analyzed data from a “randomly” selected set of 50 societies from Murdock’s list, correlating various aspects of social organization with particular varieties of religious belief: monotheism, polytheism, ancestral spirit belief, reincarnation, the immanence of the soul, witchcraft and the interaction of the supernatural and morality.¹⁹ His conclusions — in the case of monotheism — are indicative of the style of the whole:

1. Monotheism is positively related to the presence of a hierarchy of three or more sovereign groups in a society.
2. There is no relationship between the number of sovereign groups in such a hierarchy, and the likelihood that the monotheistic deity will be seen as active in earthly affairs including the support of human moral relationships. High gods do tend to be active in societies having two or more sovereign communal groups.
3. A variety of other indices of social complexity are not related to the presence of monotheistic beliefs in a society.
4. The data seem to run counter to the expectation of certain anthropologists that a highly developed monotheism would be likely to appear in the simplest and most isolated societies.²⁰

During the late 1930s and 1940s others used “cross-cultural” in this same sense, notably Margaret Mead,²¹ and after the Second World War its use spread widely in Anthropology, Psychology, Education and related fields. At some point in the mid-1950s another sense of “cross-cultural” started to appear. Rather than standing for a type of work that surveyed a range of cultures for examples of a particular phenomenon, it focused on differences between the perceptions two particular peoples held of each other, or the perceptions two particular peoples had of some specific event, or set of circumstances, or object. Thus, for example, the two theses “Military Government and the German Press: an Experiment in Cross-Cultural Institutional Change” and “The Japanese Student’s View of America: a Study in Cross-Cultural Perception” were both ac-

cepted in 1954, the former from Columbia University and the latter from Ohio State University.²²

This new meaning of "cross-cultural" arguably marks the origin of its use in the context of the encounter between two peoples of different cultures. In the post-war world that saw the start of long-term occupations of defeated countries by their victors — and the continuing presence of their military bases — the increasing presence of foreign students and staff in the universities of the first world, the formation of initiatives such as the Peace Corps and the burgeoning of disciplines like Social Psychology, discussions of how people of different cultures understood each other gained a new relevance. One manifestation of this interest was the development of the field of "cross-cultural training" in the late 1960s and early 1970s, where people about to be posted to another country were sensitized to the different ways their new hosts perceived the world and behaved.

As the decades progressed, there were two fields in which the term "cross-cultural" became preponderant in book titles: one represented the stream concerned with cross-cultural comparison and the other was represented by applied studies of encounter situations. The first was found in medical research where particular diseases or treatments or syndromes were studied, often statistically, in different parts of the world for the purposes of comparison; the second is apparent in the field of "cross-cultural communication". By 1997, the year the ANU's Centre for Cross-Cultural Research was founded, the books with "cross-cultural" in their titles in the Library of Congress catalogue still largely

comprised works with a strong comparative and statistical bias, notably four volumes of Philip M. Parker's *Cross-Cultural Statistical Encyclopedia of the World*, or studies of cross-cultural communication — for instance, *Cross-Cultural and Interdisciplinary Aspects of Teaching Languages for Professional Communication* and *Cross-Cultural Communication and Aging in the United States*.²³ It is interesting, however, that lurking amongst these titles, one book undoubtedly hailing from the humanities appears: Claudio Gorlier and Isabella Maria Zoppi's edited volume *Cross-Cultural Voices: Investigations into the Post-Colonial*. Despite its title, the essays in this book actually differ little from the traditional study of "Commonwealth Literatures"; indeed, one of its editors disclaims any desire to enter "into the heart of the vital and multi-faceted debate concerning a 'post-colonial discourse'".²⁴ However, the mere juxtaposition of "cross-cultural" and "post-colonial" marks a shift to another variety of "cross-cultural research" — the kind the Centre for Cross-Cultural Research pursued over the decade of its lifetime. One of its five "key programs" was, in fact, "Postcolonialism and Cultural History".²⁵ Another, as I noted at the beginning of this essay, was "Interrogating Concepts of the Cross-Cultural", the rubric under which the original series of seminars on "Historicizing Cross-Cultural Research" was given that led to this volume of essays.

ENDNOTES

¹ I would like to thank Henrika Kuklick for her helpful suggestions in the preparation of this essay.

² <http://www.anu.edu.au/culture/research/interrogating.php> (accessed 7.4.07)

³ Malinowski, B., *A Scientific Theory of Culture and Other Essays* (Chapel Hill: The University of North Carolina Press, 1944), p.18.

⁴ Westermarck, E.A., *The History of Human Marriage* (London: Macmillan, 1891) and subsequent editions; *The Origin and Development of the Moral Ideas* (London: Macmillan, 1906–08) and second edition.

⁵ In 1971, when he was at the University of Pittsburgh, George Murdock and others founded the Society for Cross-Cultural Research, under whose aegis the journal *Cross-Cultural Research* is published. The website of the society says: "Whereas early members were heavily involved in hologetic research, often in conjunction with the Human Relations Area Files (HRAF), the methodological perspectives of the membership have broadened over the years to include a wide range of cross-cultural interests and approaches" (<http://www.sccr.org/description.html>, accessed 13.4.07). Hologetic — "whole world" — research is usefully discussed in Richard W. Thompson and Roy E. Roper, "Methods in Social Anthropology: New Directions and Old Problems", *American Behavioral Scientist* 23, 6 (July/August, 1980), 905–24, pp.907–11.

⁶ Murdock, G.P., "The Cross-Cultural Survey", *American Sociological Review* 5, 3 (June 1940), 361–70, p.361.

⁷ Murdock, "The Cross-Cultural Survey", p.362.

⁸ Murdock, G.P., et.al., *Outline of Cultural Materials* (New Haven: Yale University Press, 1945), p.v, reprinting the foreword to the first edition of 1938.

⁹ Murdock, G.P., et.al., *Outline of Cultural Materials*, 3rd revised ed.(New Haven: Human Relations Area Files, Inc., 1950), p.xii.

¹⁰ Murdock, "The Cross-Cultural Survey," pp.362–3.

¹¹ Whiting, John W.M., "George Peter Murdock (1897–1985)", *American Anthropologist* 88, 3 (September, 1986), 682–6, pp. 684–5; my emphasis.

¹² Murdock, "The Cross-Cultural Survey," p.363.

¹³ Murdock, G.M., *Outline of World Cultures*, 3rd revised ed. (New Haven: Human Relations Area Files, Inc., 1963), pp.125–7.

¹⁴ Murdock, *Cultural Materials*, p.xviii.

¹⁵ See, Murdock, *Cultural Materials*, pp.xix–xx.

¹⁶ And, indeed, to echo Borges's taxonomy of animals from the "Celestial Emporium of Benevolent Knowledge" (famously cited by Michel Foucault in the preface to *The Order of Things*): "(a) those that belong to the Emperor, (b) embalmed ones, (c) those that are trained, (d) suckling pigs, (e) mermaids, (f) fabulous ones, (g) stray dogs, (h) those that are included in this classification, (i) those that tremble as if they were mad, (j) innumerable ones, (k) those drawn with a very fine camelhair brush, (l) others, (m) those that have just broken a flower vase, (n) those that resemble flies from a distance." (J.L. Borges, "The Analytical Language of John Wilkins", in *Other Inquisitions 1937–1952*, trans., Ruth L.C. Simms (Austin: University of Texas Press, 1964), p.103.)

¹⁷ For a critique on these grounds, see Köbben, A.J., "New Ways of Presenting an Old Idea: The Statistical Method in Social Anthropology", in Frank W. Moore (ed.), *Readings in Cross-Cultural Methodology* (New Haven: HRAF Press, 1966), 166–92.

¹⁸ Stocking, G.W. Jr., *Victorian Anthropology* (New York: The Free Press, 1987), p.316.

¹⁹ Swanson, G.E., *The Birth of the Gods: The Origin of Primitive Beliefs* (Ann Arbor: The University of Michigan Press, 1966, first ed. 1961), p.1. For detailed analysis of four other studies from this school — Murdock's own *Social Structure* (New York: Macmillan, 1949), D. Horton's "The Functions of Alcohol in Primitive Societies", *Quarterly Journal of Studies in Alcohol*, 4 (1943), C.S. Ford's *A Comparative Study of Human Reproduction* (New Haven: Yale University Press, 1945) and L.W. Simmons's *The Role of the Aged in Primitive Society* (New Haven: Yale University Press, 1945) — see Köbben, "New Ways of Presenting an Old Idea", pp.180–91.

²⁰ Swanson, *The Birth of the Gods*, p.81. Swanson's "certain anthropologists" are Father Wilhelm Schmidt and his followers: the work Swanson refers to in particular is Schmidt's *The Origin and Growth of Religion, Facts and Theories*, translated by H.J. Rose (London: Methuen and Co., 1935).

²¹ Thus, for instance, "The importance of cross-cultural comparisons in helping to clarify, sharpen, limit, and enlarge the instrumental concepts which are being used in the analysis of our own society", in Mead, M., "Public Opinion Mechanisms among Primitive Peoples", *The Public Opinion Quarterly*, 1, 3 (July 1937): 5–16, p.5.

²² Hurwitz, H.J., *Military Government and the German Press: an Experiment in Cross-Cultural Institutional Change*, Masters Thesis, Columbia University, 1954; H. A. Gould, *The Japanese Student's View of America: a Study in Cross-Cultural Perception*, Masters Thesis, Ohio State University, 1954.

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²³ Parker, Philip, M., *Cross-Cultural Statistical Encyclopedia of the World* (vol.1, Religious Cultures, vol.2, Linguistic Cultures, vol.3, Ethnic Cultures, vol.4, National Cultures) (Westport: Greenwood Press, 1997), Daniela Breveníková et al. (eds.), *Cross-Cultural and Interdisciplinary Aspects of Teaching Languages for Professional Communication* (Bratislava: Ústav jazykov Ekonomickej univerzity v Bratislave, 1997), Hana S. Noor Al-Deen (ed.), *Cross-Cultural Communication and Aging in the United States* (Mahwah, N.J.: Erlbaum, 1997).

²⁴ Gorlier, Claudio and Isabella Maria Zoppi (eds.), *Cross-Cultural Voices: Investigations into the Post-Colonial* (Rome: Bulzoni, 1997), p.13.

²⁵ <http://www.anu.edu.au/culture/research.php> (accessed 7.4.07)

THE LURE OF TEXTS AND THE DISCIPLINE OF PRAXIS

Cross-Cultural History in a Post-Empirical World

BRONWEN DOUGLAS

PROLOGUE: NARRATIVE AND TEXTS

The main aim of this paper is to tell stories about interactions between European voyagers and Aboriginal people in New Holland (mainland Australia) and Van Diemen's Land (Tasmania) at the end of the eighteenth century. I start, however, with the terms in the title. First, "texts" and "discipline of praxis".¹ The discipline of praxis is, of course, history, which by professional convention is empirical and objectivist. In the 1940s, R.G. Collingwood outraged this orthodoxy with his "ideal-ist" proposition that history is inseparable from the historian and "the here-and-now" and that "the past" is a creation of "the historical imagination".² Since the further outing of history as thoroughly text-bound by Roland Barthes, Hayden White and other textual theorists,³ historians with any claim to anti-positivism have been teased by the challenge of how to juggle the tension between narrative and texts: between their core brief to tell interesting stories about the past and the need to incorporate at least some textual analysis — because the past is only accessible through texts of one sort or another.⁴

Textual analysis requires an historically contextualized grasp of the ideologies, discourses, language, protocols and experiences which informed authors' perceptions and thinking, but it can clot a narrative and make it less readable.

Second, "post-empirical world". The discovery of the past by anthropologists and non-empirical "new historicists" in a wide range of "studies" formats — literary, cultural, gender, media, colonial, postcolonial, indigenous, and so forth⁵ — has at once made history fashionable and marginalized conventional practitioners of the discipline as, at best, utilitarian suppliers of historical background and, at worst, boring empiricists devoid of flair or theory. Historians in turn often lament the lack of detailed, particularistic archival research by such interlopers and typecast them as dangerous postmodernists, obsessed with texts at the expense of messy realities and often just plain wrong.

So much for insulting stereotypes. As an historian of cross-cultural encounters in Oceania (including Australia) in the eighteenth and nineteenth centuries, I have a foot in both camps. I derive much of my theoretical and methodological momentum from anthropology, feminism,

literary studies and Subaltern Studies: notably, the concept of culture itself; a concern for the politics of language, representation and narrative construction; and techniques of textual critique. Yet those techniques complement rather than supplant the principles for the collection and rigorous comparative scrutiny of documents which I learned as an apprentice historian. I am committed to writing about particular past human interactions and the gendered ambiguities of agency in actual encounters.⁶ That pragmatic orientation privileges persons and actions over the teleology of imagined structures and outcomes but its inductive logic is analogous to the ethnographic inductivism of anthropologists — typically, our generalizations depend on particularities, either past events or observed human behaviour. Indeed, in cross-cultural research, the relative emphasis on inductive or deductive reasoning constitutes a major fault line. It differentiates the empirical disciplines of history and anthropology from more textualist or formalist approaches in cultural studies, literary critique and art history which focus primarily on the objectified representation of indigenous people in colonial texts, images and collections — on signifiers rather than referents, the indigenous settings and the personal interactions represented which are of special interest to historians.

Furthermore, my theoretical perspective proposes an intimate liaison between indigenous actions or contexts and their representation by foreign observers — between referents and signifiers. Such representations should be read not merely as reflexes of dominant metropolitan discourses and conventions but also as personal productions generated in the stress

and ambiguity of actual encounters. By this reasoning, the behaviour, appearance or lifestyle of particular indigenous people attracted, intimidated or repelled observers, affected their perceptions, challenged or confirmed their predispositions, and left distorted countersigns in what they wrote and drew. Without empirical grounding, history tends to be little more than *a priori* background noise. Yet weaving an imaginative, accessible, but faithful narrative out of the fragmentary gleanings of archives and memories is hard work, especially if you care about indigenous agency and therefore also need ethnographic expertise and sensitivity — which is also useful in trying to decipher past Europeans. I, too, find it easier to stick to texts, with their built-in limits to enquiry, but writing stories about actual pasts remains a key goal.

CULTURE AND CIVILIZATION

The final problematic term in the title is “cross-cultural”, which I overuse because it is a handy shorthand for encounters, interactions and mutual (mis)conceptions between indigenous people and foreigners. Yet “cultural” is among the least transparent of words and has at least three strikes against it in this context. First, it is abstract: “culture” is a concept, not a thing; and cultures don't meet or encounter each other, people do. To reify such interactions as cross-cultural assumes that dramatic differences in language, thinking, history and way of life between two seemingly homogeneous communities are what matters when their members come together. This distanced binary perspective has its points, especially politically, but in principle it essentializes each side as permanent

and monolithic and in practice privileges elite male perspectives, taken as opposed. By contrast, a close look at particular past situations may also reveal multiple alliances between local inhabitants and foreigners whose respective unstable groupings intersected ambiguously and fractured internally along lines of gender, age, vocation, place, and rank, class or status. I still use “culture” strategically, but pluralized to imply flux and diversity rather than fixity or uniformity.⁷

Second, cultural is ethnocentric: in the social sciences and increasingly in popular usage, culture has the naturalized anthropological connotation of a bounded, collective pattern of belief, thought and behaviour. Yet, so far as we can tell, Oceanian people did not usually objectify their total way of life in this fashion, even in confrontation with Europeans, though indigenous people these days often appropriate the term in oppositional political rhetoric.

The third problem with cultural is anachronism: in English, culture only acquired its naturalized modern anthropological meaning (Edward Burnett Tylor’s “complex whole”⁸) from the mid-to-late nineteenth century. This usage emerged out of an ambiguous raft of earlier senses, literal and metaphoric, substantive and abstract, as traced in Raymond Williams’s outline of the convoluted history of the word and its cognate term “civilization”.⁹ Already multivocal at the end of the eighteenth century, culture denoted the process of “cultivation”, both literally in animal or crop husbandry and metaphorically in development of the human intellect: “The mind is strengthened by the cultivation of the arts and sciences”, pro-

nounced the English translator of La Pérouse’s narrative in 1798.¹⁰ However, a detailed search turned up few uses of culture or its derivatives by Oceanic voyagers before the 1830s and those always in the primary physical sense of husbandry. It is entirely absent from the *Endeavour* journals of the Englishmen James Cook (1728–79) and Joseph Banks (1743–1820) and from the *Investigator* and other journals by Matthew Flinders (1774–1814).¹¹ In the published narrative of the *Endeavour* voyage, though, Cook’s editor John Hawkesworth (1715?–73) replaced Cook’s wording “rais’d with very little labour”, said of the “produce” of Tahiti, with the phrase “with so little culture”. *Culture* also occurs in passing in the published narratives of the French voyagers Jean-François de Galaup de La Pérouse (1741–88) and Antoine-Raymond-Joseph de Bruni d’Entrecasteaux (1737–93), with the same incorrect and demeaning implication that the people in question — the Samoans and the Kanak of New Caledonia, respectively — had no or little familiarity with “the art of culture”.¹²

Embedded in these casual assertions that Pacific Islanders ignored agriculture is a tacit shift from purportedly empirical fact to loaded judgement. This verbal slippage betokens a universalist but profoundly ethnocentric developmentalism which was given detailed expression with respect to Oceania by Johann Reinhold Forster (1729–98), the German naturalist who sailed on Cook’s second voyage of 1772–75 and in 1778 published a treatise on natural philosophy. For Forster, the “cultivation” — or its synonym “culture” — of crops and animals was a prerequisite for “progress” in “civilization”:

...mankind, in a pastoral state, could never attain to that degree of improvement and happiness, to which agriculture, and the cultivation of vegetables, will easily and soon lead them. I do not, however,...insist that mankind should entirely neglect the culture, and domestication of animals;...it is the joint care of animals and agriculture, which leads mankind to the highest degree of content, and paves the way to perfect happiness.¹³

The presumption of a critical causal nexus between agriculture and civilization was a standard trope in developmentalist or social evolutionary theories from the Enlightenment onwards.¹⁴ Forster's version drew on the idea of a common stadial or graded development of civil society from savagery to civilization proposed by Scottish philosophers such as Henry Home, Lord Kames (1696–1782), whose treatise on the "progress towards maturity of knowledge and civilization...in different nations" included an equally loaded allegation about "Negroes": "[T]hey live upon fruits and roots, which grow without culture."¹⁵

By the late-eighteenth century, the abstract noun "civilization" denoted both the Enlightenment idea of a general secular process of human development from a primordial state of savagery and the ultimate outcome of that trajectory: a condition of refinement or social order, of being "cultivated" or "civilized" — "civil society" in English — which was supposedly realized in (European) modernity and was set in binary opposition to "savagery" or "barbarism". In German, *Zivilisation* and

Kultur were synonymous whereas by the early-nineteenth century the English term culture was increasingly reserved for intellectual, spiritual and aesthetic advance, in opposition to the perceived materialism of civilization. However, the anthropological application of culture to mean a particular way of life came via the German conflation of civilization and culture as a general human process: indeed, Tylor's celebrated definition referred to "culture or civilization, taken in its wide ethnographic sense".¹⁶

Men of their time, European travellers undoubtedly regarded Oceanian people, as Roy Porter put it, "through eyes already trained in seeing stereotypes about the savage and the civilised".¹⁷ Yet the term "civilization" was not often used by British voyagers before 1830,¹⁸ though from time to time they mentioned "civil" behaviour or "civility", often connoting relief that nothing nasty had happened. Thus Sydney Parkinson (1745?–71), Banks's artist, commented that some Maori men "behaved very civil to us" in New Zealand (Aotearoa) in 1769.¹⁹ Banks, the well-bred naturalist, often used the word "civil" and showed his concern for refinement of manners and social rank in frequent references to the exchange of "civilities", usually with "the Better sort of people".²⁰ On the other hand, a familiar ambivalence about the civilized state — "we Europeans" — was implicit in Banks's well-known primitivist description of the inhabitants of eastern New Holland in 1770 as "these I had almost said happy people, content with little nay almost nothing, Far enough removed from the anxieties attending upon riches, or even the possession of what we Europeans call common necessities".²¹ Cook, the farm

labourer's son, endorsed the sentiment but added feelingly: "They live in a Tranquility which is not disturb'd by the Inequality of Condition", a reminder of the entanglement of the ideas of civility and class. In the journal of his second voyage, Cook explicitly referred to "our Shame [as] civilized Christians" in venting an elegiac outburst against the negative impact on the Maori, especially on their sexual morality, of "the commerce they had with Europeans".²²

In contrast to the British, French voyagers made far greater use of *civilisation*, in both its abstract senses.²³ They explicitly located particular Oceanian groups along a universal trajectory bridging the opposed poles of savagery and civilization — but always towards the savage end. However, the moral implications of that opposition were fiercely contested, ranging between triumphalist acclaim for civilization as unequivocal progress and nostalgic disgust for aspects seen as degenerate and contrary to nature. Experience of Oceanian people provided grist to both rumour mills; indeed, both extremes were enunciated at different stages in the course of a single narrative, that of Bruni d'Entrecasteaux's voyage in search of La Pérouse in 1791–93.

I have argued elsewhere that the rhetorical somersaults in d'Entrecasteaux's evaluations of particular indigenous people were at least in part a product of their perceived behaviour towards the French — that the content and wording of his narrative were infused with indigenous counterevidence, that referents could impinge on signifiers.²⁴ Thus, in Van Diemen's Land, the inhabitants' "peaceable dispositions" showed him that "these men

so close to nature...are good and trusting" and provided "the most perfect image of the first state of society, when men are not yet troubled by the passions or corrupted by the vices which civilization sometimes brings in its wake". These infantilized people were at once "less advanced in civilization" than the Maori of New Zealand (Aotearoa) but also less "fierce".²⁵ In contrast, though Tongans were not "naturally ferocious", the seemingly arbitrary brutality of chiefs towards ordinary Islanders horrified d'Entrecasteaux and produced the global assertions that "sentiments of humanity are unknown to them" and they "attach no value to human life".²⁶ For their part, the Kanak of New Caledonia so appalled him with a single "act of ferocity" — cannibalism — that he denied them "the least degree of civilization" and deemed the Tongans "much more advanced".²⁷ Yet, in Tonga, advance was an equivocal blessing which had produced a "feudal"-style government with "weak", "effeminate" chiefs whose "voluptuous" lifestyle and arbitrary "abuses" led to a "state of anarchy" and forced the ordinary people into dissimulation, theft and "acts of cruelty".²⁸ Finally, his colleagues' accounts of vivid insults exchanged between two warring parties in the Louisiade Archipelago (Papua New Guinea) saw d'Entrecasteaux damn entire groups as "cannibals" and deplore "the excesses in which the human species can indulge when customs are not moderated and softened by civilization". Rhetorically, this was a long way from the natural charms of the "simple and good" inhabitants of Van Diemen's Land.²⁹

These fluid, late-eighteenth-century representations of particular Oceanian people were moulded by cumulative exper-

iences of indigenous reception of foreigners — local actions and demeanour — which the author tried to square with his general values, preconceptions and desires, and with place-specific precedents derived from reading voyage literature. The moral universalism of d'Entrecasteaux's developmentalist discourse remained intact across the spectrum of his representations but the specific moral valence of his words shifted dramatically in response to particular indigenous actions. However, his vocabulary did not yet signify the racialization of observed human differences and is inappropriately read in terms of the now familiar named racial phenotypes into which the people of the region were shortly to be classified. In principle at least, eighteenth-century humanism, both neoclassical and Christian, allowed the potential for progress or salvation to all human beings while construing both in thoroughly ethnocentric ways.

VOYAGERS AND TEXTS

I now turn to narrative history, to stories about encounters between particular Aboriginal people and outsiders during three voyages. My main focus is the young Englishman Matthew Flinders, then second lieutenant on HMS *Reliance*. In six expeditions between 1795 and 1799, some in open boats, Flinders and his friend George Bass (1771–1803?), the ship's surgeon, between them explored half the east coast of New Holland, from Hervey Bay (Queensland) to Westernport (Victoria), plus Van Diemen's Land.³⁰ I discuss episodes during their joint visit to Van Diemen's Land in December 1798 and during Flinders's fifteen-day stay at Moreton Bay (Queensland) in July 1799

accompanied by Bungaree, a Broken Bay man who became the key protagonist in what ensued.

The texts used are undoubtedly both ethnocentric and élitist. They comprise contemporary journals and later, more polished narratives: manuscript copies, seemingly abridged, of Flinders's journals of his two voyages;³¹ accounts of the same voyages "taken from" the journals of Bass and Flinders and published in volume two of *An Account of the English Colony in New South Wales* by the Marine lieutenant-colonel David Collins, who had been judge-advocate and colonial secretary at Port Jackson and would be the founder and lieutenant-governor of Hobart Town; Flinders's brief coastal *Observations* published in 1801 to accompany the charts of his early surveys; and, finally, the long historical introduction to his *Voyage to Terra Australis*, on "Prior Discoveries".³²

The third voyage, mentioned only briefly for comparative purposes, is that of the Frenchman Nicolas Baudin (1754–1803), who explored western and southern New Holland on the *Géographe* and the *Naturaliste* in 1801–03, in direct competition with Flinders, who was then surveying the New Holland coast in HMS *Investigator*. I refer to an episode during the French visit to southeastern Van Diemen's Land in early 1802, drawing on Baudin's shipboard journal (1974), a contemporary official report by Baudin (1978), and the later published narrative of the voyage by the young naturalist François Péron (1775–1810).³³

FIRST HISTORY: VAN DIEMEN'S LAND, DECEMBER 1798

Late in 1798, Flinders and Bass in the 25-ton colonial sloop *Norfolk*, with a volunteer crew of eight, sailed through Bass Strait and around Van Diemen's Land, thereby proving it to be an island. They saw signs of human presence at several points but interacted with only one local inhabitant. At Port Dalrymple — the Tamar estuary — they saw three or four people "at a great distance", who according to Flinders walked away, "most probably at our approach", whereas Bass said that they "ran off into the woods" and made the incident emblematic of the "extreme shyness" of the inhabitants which "prevented any communication".³⁴ But in the upper Derwent they came face to face with two women and a man. The women "scampered off" (said Bass) "screaming" (said Flinders) but the man showed no "signs of fear or distrust" and accepted a dead swan "with rapture". Apparently "ignorant of muskets", his only interest was the swan and the Englishmen's red neckerchiefs. He did not know their smattering of Port Jackson and Tahitian words but seemed to understand their signs and agreed to show them his habitation. However, his "devious route and frequent stoppages" convinced them that he sought only "to amuse [himself] and tire them out" — Bass read caution in this strategy and "jealousy" about "his women" — but they parted "in great friendship".³⁵

Exegesis: This fleeting individual contact — so typical of the serendipitous, almost spectral quality of early voyagers' reports of meetings with the elusive and enigmatic inhabitants of Van Diemen's

Land — was loaded with considerable interpretive weight by Bass and Flinders, as indeed it is by this historian given the relatively few such meetings reported. In a classic slippage, they made a single human specimen stand for an entire group: the man's "frank and open deportment" produced a "favourable opinion of the disposition" of the inhabitants of Van Diemen's Land.³⁶ Their reportage is a prime sample of a rhetorical trajectory I have previously identified in voyage texts:³⁷ from relief at approved conduct by indigenous people to positive depictions of their essential character or appearance and explicit distancing from a standard compendium of supposedly Negro traits. Such representations are oblique reflexes of actual indigenous behaviour, processed by European travellers in the light of the profound insecurity of sailing in unfamiliar waters and their usual distaste for Negro physiognomy. Consider these sequences in Flinders's two extant reports of the meeting at the Derwent. In 1801: the man "seemed to be devoid of fear"; "his countenance was more expressive of benignity and intelligence, than of ferocity or stupidity"; "his features were less negro-like than is usual in New South Wales". And in 1814: "the quickness with which he comprehended our signs spoke in favour of his intelligence"; his hair "had not the appearance of being woolly" — code for "Negro".³⁸ It is difficult to say much about the actual encounter except that the local man was evidently alert, wary and sought to control and profit from the meeting on his own terms while the women avoided it, possibly through fear of the strangers, or the man, or all three.

Flinders's reference to New South Wales exemplifies a persistent sub-text in

all these accounts: a comparative — what would now be called ethnological — agenda which sought empirical evidence of the relative “condition” of different groups, always pivoting on the authors’ claim to expert knowledge of Port Jackson. Thus, the young men in a party encountered at Twofold Bay en route to Van Diemen’s Land were “better made, and cleaner in their person than the natives of Port Jackson usually are”. Even the invisible people of Port Dalrymple were deduced to be “much inferior in some essential points of convenience to...the despised inhabitants of the continent”, a judgement based on only three elements of material culture: the leakiness of their habitations, their apparent lack of canoes and the “roughness” of the marks they left on trees, suggesting a less “sharp-edged tool” than that used on the mainland. “But”, added Bass, yoking pragmatic relativism to a tacit developmentalist philosophy, “*happiness...exists only by comparison with the stage above and the stage below our own*”.³⁹

SECOND HISTORY: MORETON BAY, JULY 1799

Six months later, Flinders set out in the *Norfolk* to examine the coast north of Port Jackson, without Bass but accompanied by Bungaree,⁴⁰ “whose good disposition and manly conduct” had attracted Flinders’s “esteem” and who for the next 30 years would be among the best-known and oft-portrayed Aborigines in the colony.⁴¹

On 16 July, at a sandy point east of the mouth of Moreton Bay — Flinders’s Point Skirmish, still so named, on the southern tip of Bribie Island⁴² — Flinders and

Bungaree conversed “by signs” with several apparently unarmed local men. Bungaree went ashore, also unarmed, and engaged in the first of several exchanges — his yarn belt for a kangaroo fur band — by which both parties presumably sought to establish, maintain or develop a relationship. Bungaree was the key figure in these transactions. Flinders eventually landed, armed against “treachery” with a musket, but his own efforts at exchange failed when he refused to give up his cabbage-tree hat on demand. As Flinders and Bungaree retreated to the boat, crowded from behind by the men, one tried good-humouredly to take the hat by ruse but failed. The situation then deteriorated. Firewood was thrown at the boat, fell short, and was “treated as a joke” but one man hurled a spear, which narrowly missed. Alarmed, Flinders shot at “the offender” and continued to do so through two misfires until he finally wounded him. Another man was reportedly shot in the arm by a seaman and the Aborigines fled.⁴³

Although Flinders professed satisfaction at “the great influence which the awe of a superior power has in savages”, his journal also tells another story, of ongoing apprehension and jumbled emotions: insult at the “impudent” and “very wanton attack”; regret that he had been provoked into firing; hope that it would deter further attacks by “the enemy”; anxiety nonetheless; and vulnerability because he had to remain in the bay to do his survey and repair the sloop. For five days, he cautiously avoided further contacts despite repeated Aboriginal invitations. His prudence seemed justified on 18 July when the *Norfolk* was assailed by “a party of natives...who appeared to be standing

up in their canoes, and pulling toward them, with all their strength, in very regular order...after the manner of the South Sea islanders". Then, as "about 20 of them were counted, and seemed to be coming on with much resolution", the decks were cleared, the men armed, and the sloop bore away towards the attackers who had surprisingly come no closer. Flinders recounted the denouement with wry retrospective appreciation of its absurdity: "this hostile array turned out to be a few peaceable fishermen" standing on a sand flat and "driving fish into their nets".⁴⁴ Yet dark imaginings about savage hordes were standard fare for sailors in a region offering notorious precedents in the real or assumed fates of Cook, La Pérouse and numerous lesser figures. Flinders knew from personal experience as a midshipman with William Bligh (1754–1817) in the Torres Strait Islands in 1792, when the ships were twice attacked by men in canoes and a seaman died, how lethally unstable the equation between the "superiority of our arms" and "great differences of numbers" could be.⁴⁵

From 21 July, Flinders's tension gradually eased as Bungaree, "in his usual undaunted manner", facilitated relations with the local people, who welcomed him enthusiastically but remained apprehensive of the white men, their muskets and, especially, Flinders. Hardly any women were seen. During the last two days of the visit, with the sloop detained by bad weather near Skirmish Point, the exchanges expanded to include the Europeans and featured much singing and dancing, presumably an Aboriginal strategy to pacify or control the dangerous strangers who thought they were being "entertained". Flinders found their dan-

cing "not ungraceful", especially in contrast to the "clumsy" efforts of three Scottish sailors who had earlier been ordered to dance a reel without "musick" and had not impressed the local audience. Their singing was "musical and pleasing" in contrast to Bungaree's reciprocal offering, which "sounded barbarous and grating" and "annoyed his auditors" — but he was accounted "an indifferent songster, even among his own countrymen". These "friendly interchanges" culminated in a name exchange — they called Flinders "Mid-ger Plindah" and he recorded three of theirs — which he took for an important "ceremony" on the analogy of Cook's account of a similar practice at Endeavour River in 1770.⁴⁶

Exegesis: In a later brief history of his 15-day visit to Moreton Bay, Flinders attributed the eventual "friendly" relations to "a salutary change" induced in Aboriginal attitudes by "the effect of our fire-arms". But the content and wording of his own journal suggest that the most potent element in local responses to the strangers and repeated expressions of eagerness to communicate with them was Bungaree. Though he "could not understand" the Moreton Bay language, the local people persistently sought him out, while his mediatory skills were much valued by the Europeans with whom he did share a *lingua franca*.⁴⁷ Flinders represented him as the key agent in three of the four exchange situations which succeeded the initial violence. On 21 July, "about six miles" from Skirmish Point, two men signalled for them to land but fled when Flinders approached, only to return when they saw Bungaree. He "made a friendly exchange" with them and went to the boat for additional items, "to make the exchange

equal".⁴⁸ There was a more elaborate transaction four days later, with Bungaree again the main player:

Presents were made them of yarn caps, pork, and biscuit, all of which they eagerly took, and made signs for Bong-ree to go with them, and they would give him girdles and fillets, to bind round his head and the upper parts of his arms. So long as their visitors consisted only of two, the natives were lively, dancing and singing in concert in a pleasing manner; but the number of white men having imperceptibly increased to eight, they became alarmed and suspicious.⁴⁹

On 28 July, members of the crew chopped down a tree and the noise of its fall greatly "startled" several local men. Flinders, ever pragmatic, thought it "might probably assist in giving them a higher idea of the power of their visitors". Bungaree — "gallant and unsuspecting" according to Flinders but the second epithet is surely wrong — made amends for their fright by giving them a spear and a throwing-stick and showing them the use of the latter, of which they appeared "wholly ignorant".⁵⁰ I take this tutorial as a genuinely *cross-cultural* act which symbolized a reciprocal rather than a hierarchical relationship and belies the reified idea of the cross-cultural as a binary divide between opposed, homogenized cultures. It is likely that the Moreton Bay people took Bungaree for the leader of the expedition and the white men for his followers — which might explain why the modern town near Skirmish Point is called Bongaree and not Flinders.

Bungaree also served Flinders as a datum point in the continuation and extension of the comparative agenda previously noted with respect to Van Diemen's Land. At the mouth of the Clarence River, en route to Moreton Bay, they had seen three large, well-built habitations which Bungaree "readily admitted...were much superior to any huts of the natives which he had before seen". A fishing net taken from a house in Moreton Bay was "proof of the superior ingenuity of these over the natives of Port Jackson". Their singing, too, was more complex: "not merely in the diatonic scale, descending by thirds, as at Port Jackson: the descent of this was waving, in rather a melancholy soothing strain". On the other hand, Bungaree's weaponry was superior and, although the inhabitants of Moreton Bay bore a general physical resemblance to those of Port Jackson, there was none "whose countenance had so little of the savage, or the symmetry of whose limbs expressed strength and agility, so much, as those of their companion Bong-ree" — a classic instance of a personal relationship transcending a demeaning stereotype.⁵¹

These piecemeal contrasts were specific and empirical rather than systemic. However, at the end of the account of his stay in Moreton Bay, Flinders outlined an inductive environmentalist theory of the development of civil society which is pertinent to this paper. In his 1814 narrative, he summarized the situation thus: "They fish almost wholly with cast and setting nets, live more in society than the natives to the southward, and are much better lodged."⁵² Here is his contemporary explanation of why this should be so as rendered by Collins, but the ideas are clearly Flinders's:

[T]he inhabitants of this bay appeared to possess in general a very pointed difference from, if not a superiority over, those of New South Wales, particularly in their net-works...There was no doubt but they were provided with nets for catching very large fish, or animals...[T]his mode of procuring their food would cause a *characteristic difference between the manners, and perhaps the dispositions*, of these people, and of those who mostly depend upon the spear or fiz-gig for a supply. In the one case, there must necessarily be the *co-operation of two or more individuals*; who there, from mutual necessity, would associate together. It is fair to suppose, that this association would, in the course of a few generations, if not much sooner, produce a favourable change in the manners and dispositions *even of a savage*. In the other case, the native who depends upon his fiz-gig or his spear for his support depends upon his single arm, and, requiring not the aid of society, is indifferent about it, but prowls along, a *gloomy, unsettled, and unsocial being* [Bungaree?]...

The net also appearing to be a more certain source of food than the spear, *change of place will be less necessary*. The encumbrance too of carrying large nets from one place to another will require a *more permanent residence*; and hence it would naturally follow, that their houses would be of a better construction...; this *superiority* Mr. Flinders attributed to the

*different mode of procuring fish which had been adopted by the inhabitants. He likewise supposed that the use of nets...arose from the form of the bay...*⁵³

**THIRD HISTORY:
SOUTHEASTERN VAN
DIEMEN'S LAND,
JANUARY–FEBRUARY 1802**

I leave commentary on this passage to the conclusion and turn to my brief third history of incidents during Baudin's six-week sojourn in southeastern Van Diemen's Land in January–February 1802. Since I have discussed the episode in some detail in other papers,⁵⁴ I limit myself here to a few relevant points.

Baudin arrived in Van Diemen's Land with favourable preconceptions about the people he would meet, derived from the Cook and d'Entrecasteaux voyage reports, and bound by both his instructions and his inclinations to avoid violence against them except in extreme self-defence. He wrote at the outset that "the people of this country do not appear to be savage, except when provoked".⁵⁵ His contemporary journal gives a dispassionate empirical account of frequent, mostly amicable relations with local people, broken by two sudden, unexplained assaults on shore parties by men at Bruny Island who had been amicably interacting with the French and been "loaded with presents": the first time, a single spear wounded a midshipman in the neck; the second time, a "hail of stones" wounded Baudin "fairly sharply" on the hip.⁵⁶ Despite these contretemps, the tone of the journal is matter-of-fact and even-handed about the indigenous people, including the attackers.

In sharp contrast, in an official report written later in the year, Baudin emphasized the violence of the encounters and deplored the “fickleness” of “primitive men of nature...at the furthest degree possible from civilization”, whose unpredictable mood shifts back and forth from amity to aggression made it impossible to form “a clear idea of their character” and left sailors dangerously exposed.⁵⁷

However, even Baudin’s report is relatively restrained and empirical in comparison to the exaggerated language of the official voyage narrative written by Péron, the expedition’s zoologist and anthropologist. Before the voyage, he had professed a qualified primitivist idealization of people “closer to nature”, contrasting “degenerated and debased man of [civilized] society” to the “robust majesty of natural man”.⁵⁸ In the event, any residual primitivism was rapidly dispelled in fears provoked by trying experience of so-called “natural man” in Van Diemen’s Land. Within a few pages, the rhetoric of Péron’s narrative shifts from romantic approval of the “affectionate” and “frank” demeanour of “our good Diemenlanders” to vilification of “these fierce men”. Within the text, this discursive shift is a direct response to the spear- and stone-throwing incidents and suggests a tortuous passage from referents to signifiers. “Men of nature” are no longer “good and simple” but “wicked”. “After all we have seen”, he proclaimed, “one cannot sufficiently mistrust *men whose character has not yet been softened by civilization.*” He wrote subsequently of the people of Maria Island, with whom Baudin had found no fault, that “all their *actions* bore the stamp of treachery and ferocity”. These actions goaded him to a diatribe on “the diffi-

culties faced by travellers in communicating with savage peoples, and the impossibility of overcoming the natural ferocity of their character and their prejudices against us”.⁵⁹ Péron’s ambivalence and outrage were epitomized in his reaction to the man the French knew as Bara-Ourou, whom Péron praised as “the handsomest man in the band” but also damned as the most threatening (see Figure 1).⁶⁰

CONCLUSION

These particular histories of interactions between shipborne strangers and Aboriginal people in Van Diemen’s Land or New Holland at the end of the eighteenth century both mirror and illuminate the problematics of my title. My artificial and partial separation of stories from exegesis is a metonym for the tension between narrative and texts that plagues anti-objectivist history. Narrative is necessary because small histories speak to large issues. So too is textual analysis but it must be contextualized. In this paper, historicizing authors, their ideas and their experiences highlights the ambiguity of the concept “cross-cultural” with reference to periods and contexts in which it was clearly anachronistic or inappropriate. Close attention to the words voyagers used to describe their experiences and the indigenous people they saw makes it clear that whatever they thought were doing, it was normally *not* engaging in cross-cultural encounters. Their key trope was not culture but *civilisation* (in the French case) and civility or civil society (in the British case).

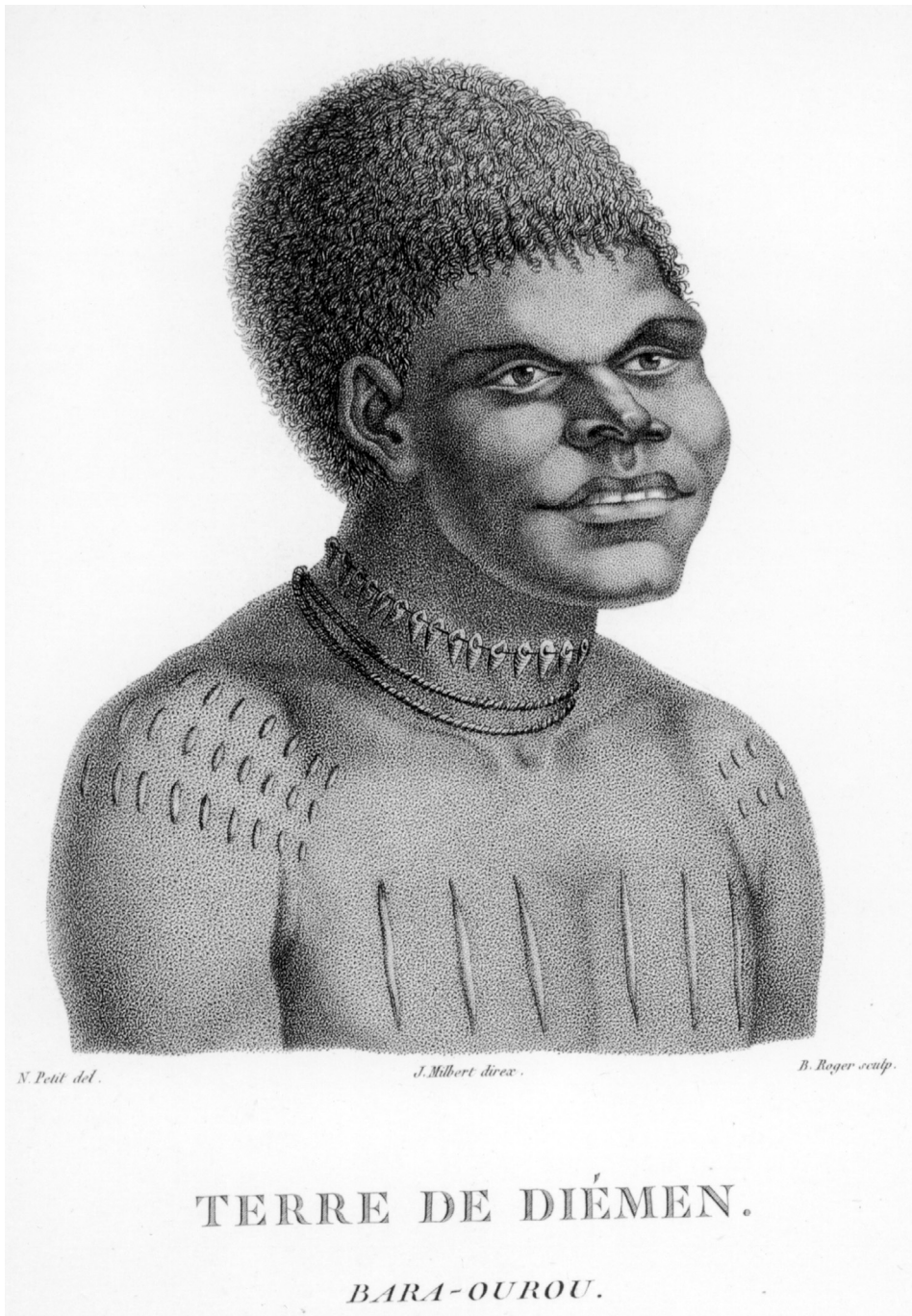


Figure 1: Barthélémy Roger after Nicholas-Martin Petit, 'Terre de Diémen. Bara-Orou', stipple engraving, 31.8 x 24.2 cm, in Charles-Alexandre Lesueur and Nicholas-Martin Petit, *Voyage de découvertes aux terres australes exécuté par ordre de S.M. l'Empereur et Roi. Partie Historique. Atlas* (Paris : Imprimerie impériale, 1807): plate 8.

That said, civilization is no more transparent than culture. Its discursive instability, in conjunction with that of the idea of “race”, was nicely captured by George Stocking: “in the later eighteenth century, the idea of ‘civilization’ was seen as the destined goal of all mankind, and was in fact often used to account for apparent racial differences. But in the 19th century more and more men saw civilization as the peculiar achievement of certain ‘races’”.⁶¹ I have written elsewhere about broad discursive transitions at the end of the eighteenth century with particular reference to race.⁶² The texts considered here are on the cusp of this shift in the meaning of civilization which is exemplified in the similarity and contrast between d'Entrecasteaux's and Péron's narrativized responses to volatile indigenous behaviour in Oceania. It was the spectre of cannibalism — an offence against humanity — which led d'Entrecasteaux (writing in 1793 but edited for posthumous publication in 1808) to deplore “the excesses in which the human species can indulge when customs are not moderated and softened by civilization”; it was particular insult at the “violent aggression” directed against his colleagues that saw Péron (publishing in 1807 about events in 1802) use the same trope: “[O]ne cannot sufficiently mistrust men whose character has not yet been softened by civilization.” Both envisaged the need to respond with force but for d'Entrecasteaux it was a council of despair rather than a prescription: compare his lament that “we must renounce visiting [Pacific Islanders]..., or we must inspire respect in them by very great severity” with Péron's dogma that “one must only approach these peoples armed with sufficient means to curb their ill will or repel

their attacks”.⁶³ “Curb” is a key term which spoke to a paradox at the heart of Enlightenment humanism: that its moral universalism was at once inclusive, philanthropic, and optimistic about all human beings, including so-called savages, but also ethnocentric, hierarchical, paternalist, prescriptive and acquisitive. These latter strands, which would not accommodate other people's assessments and exercise of their rights, desires and autonomy, came steadily to dominate the discourse of civilization. Colonization was in the air and in September 1803, 18 months after Baudin's visit, it became a grim fact in Van Diemen's Land.

The particular wording of these passages also signals a semantic instability in the word “civilization”, noted by Williams: a slippage from the idea of civilization as “refinement of manners and behaviour” to its preferred modern connotation of “social order”.⁶⁴ D'Entrecasteaux used the term in the earlier sense, lyrically celebrating indigenous sociality in Van Diemen's Land as “evidence” of the “first natural affection” and a “school of nature”.⁶⁵ Péron did so in the later sense: these same people — “so close to the zero point of civilization”,⁶⁶ the “children of nature *par excellence*” — epitomized “non-social man” who must be “curbed”. There is, moreover, overt racialization in his assertion that they “differ *essentially* [and perhaps originally?] from all other known peoples” and in his conclusion that they were “the most savage [people] of all”, consigned by physical deficiencies to the bottom of a hierarchy of races whose relative “physical strength” he claimed to have established “by direct experiments”. A passionate advocate for “the progress of civilization” and the superiority of “civil-

ized" over "savage man", Péron argued for a close causal nexus between "physical constitution" and "social organization" or its purported "absence" — between race and civilization: the "peculiar conformation" and the alleged physical "weakness" he discerned in the inhabitants of Van Diemen's Land were products of inadequate diet and lifestyle which were in turn "an immediate and necessary result of the savage state in which these unhappy peoples vegetate".⁶⁷

Williams, furthermore, suggested a national difference in the usage of "civilization": "From e[arly] C19 the development of *civilization* towards its modern meaning...is on the whole earlier in French than in English."⁶⁸ The shift in French was presumably fuelled by the experience of revolution, whereas its later English manifestation related more to colonialism. My sample of voyage texts, though too small to be conclusive, partly bears out Williams's observation. Civilization is only implicit in most of the British texts but for Bass it meant relative "convenience" and "happiness", in Forster's sense, which were corollaries of refinement. Flinders's primary concern was to explain "a characteristic difference between the manners, and *perhaps* the dispositions" of the Port Jackson and the Moreton Bay people — "manners" came first — and he did so in terms of a simple environmentalist developmentalism which also reads like a distillation of Forster: it was ultimately "the form of the bay" which produced "more...society" at Moreton Bay.⁶⁹

Yet the distinction between refinement and social order was not simply linear but was also one of emphasis, degree and pragmatic context: if manners mattered

more aesthetically and in the abstract, "superior power" — Flinders's phrase — came to the fore when the always-lurking spectre of savagery materialized into real or threatened action. In an ironic passage, Flinders acknowledged the intimate linkage of power and refinement: having failed to impress the Moreton Bay people with "the effect and certainty of his fire-arms" when he shot at a hawk and only broke its leg, he recalled wryly how:

...ineffectual had been some former attempts...to impress them with an idea of the superior refinement of his followers. Bong-ree, his musician, had annoyed his auditors with his barbarous sounds, and the clumsy exhibition of his Scotch dancers...had been viewed by them without wonder or gratification.⁷⁰

I approached these texts with the working hypothesis that they would disclose a broad contrast between English pragmatism and French abstraction. It was partly confirmed in the distinction between explicit French and implied English usages. However, again the question is more complex and ambiguous. Class and occupational differences were at least as salient as national ones. In many respects, sailors like Cook and Baudin — an *officier bleu*, of non-noble birth, he had no real career in the French *Marine* until after the Revolution — had far more in common with each other, as their pragmatic, empirical language showed, than either did with their more sophisticated naturalists Banks and Péron. Furthermore, if French sailors fulminated more about *civilisation*, they also fired less often on indigenous people than did the British. Unlike Flinders at

Moreton Bay, Baudin was “not obliged to fire” on his stone-throwing assailants in Van Diemen’s Land because when he aimed his firearm at one man, they all scattered — they had prior experience of muskets whereas the Moreton Bay people seemingly did not. But for Baudin, it was also a matter of principle: “experience” had taught him that “superior force” was not always the only guarantee against “the traps of the man of nature” and that “prudence” could avert endless alarms.⁷¹

As they made their way around the New Holland coast, voyagers of both nations evinced a keen predatory interest in the resources offered by the land and its potential for pasture and agriculture but the British, already ensconced, did so more systematically, persistently and, in the end, effectively.⁷² The initial British settlement in Van Diemen’s Land was placed at Risdon Cove on Bass’s recommendation and the definitive settlement at Hobart Town that followed in February 1804 was led by Collins, the amanuensis of Bass and Flinders. I conclude by suggesting that whereas “cross-cultural” is in principle an egalitarian, relativist concept which acknowledges the specificity and validity of particular ways of life, the idea of civilization, in all its manifestations, is hierarchical, universalist and assimilationist. From this perspective, the only named cross-cultural actor in my histories was Bungaree.

ENDNOTES

¹ For aesthetic reasons, I make minimal use of inverted commas: they are included on first mention of an English term in its contemporary sense and then omitted, except for direct quotations; non-English words are italicized; inverted commas are implied in the case of now problematic terms such as “race”, “civilization”, “savage”, “Negro”.

² R.G. Collingwood, *The Idea of History* (Oxford: Clarendon Press, 1946), pp.242, 248.

³ Roland Barthes, “Le discours de l’histoire”, *Informations sur les sciences sociales*, vol. 6 (1967), 65-75; Hayden White, *Tropics of Discourse: Essays in Cultural Criticism* (Baltimore, MD: Johns Hopkins University Press, 1978); Hayden White, *The Content of the Form: Narrative Discourse and Historical Representation* (Baltimore, MD: Johns Hopkins University Press, 1987).

⁴ In principle, I use the term “text” in the widest sense to mean any vehicle for representation — written, drawn, photographed, made, performed, spoken, remembered. In practice in this paper, the texts considered are all written and visual representations are used mainly for illustration.

⁵ See, for example, Aletta Biersack (ed.), *Clio in Oceania: Toward a Historical Anthropology* (Washington, D.C.: Smithsonian Institution Press, 1991); Frederick Cooper and Ann Laura Stoler (eds), *Tensions of Empire: Colonial Cultures in a Bourgeois World* (Berkeley: University of California Press, 1997); H. Aram Veeser (ed.) *The New Historicism* (New York: Routledge, 1989).

⁶ The problematic concept of “agency”, particularly that of indigenous, female and other historically suppressed categories of persons, here neither necessarily infers intention nor presumes a modernist notion of the individual as a bounded, autonomous subject. Rather, I take it as given that there is a general human potential to desire, choose, and act strategically which must be historicized within the limits and possibilities of unstable assemblages of ideas, systems, personalities and circumstances.

⁷ The anthropologist Francesca Merlan proposed the term “intercultural” as the most effective way to conceptualize “difference-yet-relatedness within an increasingly expanding social field” in a “globalized world” (Francesca Merlan, “Explorations Towards Intercultural Accounts of Socio-Cultural Reproduction and Change”, *Oceania*, vol. 75 (2005), 167-82). However, attentiveness to “complex articulations within and across particular social groups” rather than emphasis on “an ‘interface’ between separately conceived domains” is an equally apt strategy for historians of early encounters between indigenous people and Europeans (Melinda Hinkson and Benjamin Smith, “Introduction: Conceptual Moves towards an Intercultural analysis”, *Oceania*, vol. 75 (2005), 157-66, pp.157-8).

⁸ “Culture or civilization, taken in its wide ethnographic sense, is that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society” (Edward B. Tylor, *Primitive Culture: Researches into the Development of Mythology*,

Philosophy, Religion, Art, and Custom, 2 vols. (London: John Murray, 1871), vol. 1, p.1).

⁹ Raymond Williams, *Keywords: a Vocabulary of Culture and Society*, Rev. ed. (New York: Oxford University Press, 1983 [1976]), pp.57–60, 87–93; see also Claude Blanckaert, “La naturalisation de l’homme de Linné à Darwin: archéologie du débat nature/culture”, in A. Ducros, J. Ducros, and F. Joulain (eds), *La culture est-elle naturelle? Histoire, épistémologie et applications récentes du concept de culture* (Paris: Editions Errance, 1998), 15–24, pp.19–23.

¹⁰ Anon., “The Translator’s Preface”, in J.-F. de Galaup de La Pérouse (L.-A. Milet-Mureau, ed.), *The Voyage of La Pérouse Round the World, in the Years 1785, 1786, 1787, and 1788 ...*, vol. 1 (London: John Stockdale, 1798), [1]–[7], p.[3].

¹¹ See the transcriptions of Cook’s and Banks’s journals by the Australian National Library’s South Seas Project, <<http://southseas.nla.gov.au/>>, and Flinders’s journals in the Matthew Flinders Electronic Archive of the State Library of NSW, <<http://www.sl.nsw.gov.au/flinders/archive.html>>.

¹² James Cook (J.C. Beaglehole, ed.), *The Journals of Captain James Cook on his Voyages of Discovery*, vol.1, *The Voyage of the Endeavour 1768–1771* (Cambridge: Hakluyt Society, 1955), p.121; Antoine-Raymond-Joseph de Bruni d’Entrecasteaux (E.-P.-E. de Rossel, ed.), *Voyage de D’Entrecasteaux envoyé à la recherche de La Pérouse...*, 2 vols. (Paris: Imprimerie impériale, 1808), vol.1, p.355; John Hawkesworth, *An Account of the Voyages Undertaken by the Order of His Present Majesty for Making Discoveries in the Southern Hemisphere...*, 3 vols. (London: W. Strahan and T. Cadell, 1773), vol.2, p.186; [Jean-François de Galaup de La Pérouse] (L.-A. Milet-Mureau ed.), *Voyage de la Pérouse autour du monde...*, 4 vols (Paris, Plassan, 1798), vol. 3, p.236. In the Endeavour journal, Cook elaborated his assumption that Tahitians did not need to practise agriculture: “in the article of food these people may almost be said to be exempt from the curse of our fore fathers; scarcely can it be said that they earn their bread with the sweet of their brow, benevolent nature hath not only supply’d them with necessarys but with abundance of superfluities” (*Journals*, vol. 1, p.121). By contrast, in the journal of his second voyage on HMS *Resolution*, Cook defended the Tahitians in this respect in a careful empirical passage challenging the ethnographic expertise of his French predecessor Louis-Antoine de Bougainville (1729–1814): “it is true some things require but little labour, but others again require a good deal, such as roots of every kind and Bananas and Plantains will not grow spontaneously but by proper cultivation, nor will the Bread and Cocoa nutt trees come to perfection without” (James Cook, *Journals*, vol. 2, *The Voyage of the Resolution and Adventure 1772–1775* (Cambridge: Hakluyt Society, 1961), p.235).

¹³ Johann Reinhold Forster (N. Thomas, H. Guest and M. Dettelbach, eds), *Observations Made During a Voyage Round the World* (Honolulu: University of Hawaii Press, 1996 [1778]), pp.234–5, 238.

¹⁴ Thus, for example, the French comparative anatomist Georges Cuvier (1769–1832) discerned: “very different degrees in man’s development...Man has really only succeeded in multiplying his species to a high degree, and in advancing very far his knowledge and his arts, since the invention of agriculture...Mild climates, soils naturally watered, and rich in plants, are veritable cradles of agriculture and civilization” (Georges Cuvier *Le règne animal distribué d’après son organisation, pour servir de base à l’histoire naturelle des animaux et d’introduction à l’anatomie comparée*, 4 vols (Paris: Deterville, 1817), vol.1, pp.91–4); Charles Darwin (1809–82) applied similar logic to the particular case of Aboriginal Australians: “they appeared far from such utterly degraded beings as usually represented. — In their own arts they are admirable... — They will not however cultivate the ground, or even take the trouble of keeping flocks of sheep which have been offered them; or build houses & remain stationary. — Never the less, they appear to me to stand some few degrees higher in civilization, or more correctly a few lower in barbarism, than the Fuegians” (Charles Darwin (R.D. Keynes, ed.), *Charles Darwin’s Beagle Diary*, (Cambridge: Cambridge University Press, 1988), p.398).

¹⁵ Henry Home, Lord Kames, *Sketches of the History of Man*, 2 vols. (Edinburgh: W. Creech and London: W. Strahan and T. Cadell, 1774), vol.1, pp.32, 43.

¹⁶ Tylor, *Primitive Culture*, vol.1, p.1; Williams, *Keywords*, pp.58–9, 89–90.

¹⁷ Roy Porter, “The Exotic as Erotic: Captain Cook at Tahiti”, in G.S. Rousseau and R. Porter (eds), *Exoticism in the Enlightenment* (Manchester: Manchester University Press, 1990), 117–44, p.122.

¹⁸ A notable exception was George Vancouver (1757–98), who twice sailed as a midshipman with Cook and commanded a major surveying expedition to the Pacific Ocean in 1791–95. In a journal passage extolling a Hawaiian man who had retained a piece of Vancouver’s hair given to him four years previously, Vancouver invoked two ethnocentric tenets of contemporary humanism: first, that the man’s “pledge of friendship” arose from “principles innate and common to the species” and showed the “similarity in the human mind” in “every stage of civilization”; and second, that “the untaught inhabitants of...the uncultivated world” and “the civilized and polished states of the world” represented starting point and culmination of a unilinear historical trajectory (George Vancouver (W. Kaye Lamb, ed.), *A Voyage of Discovery to the North Pacific Ocean and Round the World 1791–1795*, 4 vols. (London: Hakluyt Society, 1984), vol.3, p.862).

¹⁹ Sydney Parkinson (Stanfield Parkinson, ed.), *A Journal of a Voyage to the South Seas in his Majesty's Ship, the Endeavour... Embellished with Views and Designs, Delineated by the Author, and Engraved by Capital Artists* (London: Stanfield Parkinson, 1773), p.97.

²⁰ For example, in Raiatea (French Polynesia) in August 1769, "we all went to see the great king [of Borabora] and thank him for his civilities" (Joseph Banks (J. C. Beaglehole, ed.), *The Endeavour Journal of Joseph Banks 1768–1771*, 2 vols. (Sydney: Public Library of NSW with Angus and Robertson, 1962) vol.1, p.327; vol.2, p.124).

²¹ Banks, *Endeavour Journal*, vol.2, p.130.

²² Cook, *Journals*, vol.1, p.399. The later passage runs: "we debauch their Morals already too prone to vice and we interduce among them wants and perhaps diseases which they never before knew and which serves only to disturb that happy tranquillity they and their fore Fathers had enjoy'd" (Cook, *Journals*, vol.2, p.175).

²³ For example, the idea of *civilisation* as a universal human process underwrote the acknowledgement by La Pérouse's editor that the piecemeal introduction of its trappings was a mixed blessing for people he placed at the level of "savages". His preferred strategy was "to raise them by degrees in order to civilize them, by making orderly communities [*des peuplades policées*] before making polished people [*des peuples polis*], and only giving them new needs and new procedures along with the means to supply the first and make effective use of the second. This will prepare their descendants for and guarantee them the happy results of the development of the human faculties" (Louis-Antoine Milet-Mureau, "Discours préliminaire du rédacteur", in La Pérouse, *Voyage*, vol.1, xix–lxviii, p.lxvi).

²⁴ Bronwen Douglas, "Art as Ethno-historical Text: Science, Representation and Indigenous Presence in Eighteenth and Nineteenth Century Oceanic Voyage Literature", in N. Thomas and D. Losche (eds), *Double Vision: Art Histories and Colonial Histories in the Pacific* (Cambridge: Cambridge University Press, 1999), 65–99, pp.73–83; Bronwen Douglas n.d., "In the Event: Indigenous Countersigns and the Ethno-history of Voyaging", in M. Jolly, S. Tcherkezoff, and D. Tryon (eds), *Oceanic Encounters* (in preparation).

²⁵ d'Entrecasteaux, *Voyage*, vol.1, pp.230–2, 242, 243.

²⁶ Ibid, p.308.

²⁷ Ibid, pp.333, 343.

²⁸ Ibid, pp.298, 305–12.

²⁹ Ibid, pp.421–3, cf. p.234.

³⁰ For a chart of Flinders' voyages, see: 'Matthew Flinders, General chart of Terra Australis or Australia showing the parts explored between 1798 and 1803 by M. Flinders Commr. of H.M.S. Investigator', State Library of NSW, Sydney, <http://image.slnsw.gov.au/cgi-bin/ebind-show.pl?doc=flinders_maps/a125;seq=16>.

³¹ Matthew Flinders, Narrative of the Expedition in the Colonial Sloop Norfolk, from Port Jackson through the Strait which Separates Van Diemens Land from New Holland; and from thence round the South Cape back to Port Jackson, Completing the Circumnavigation of the Former Island, with some remarks on the Coasts and Harbours, MS copy, State Library of NSW, Sydney, 1798–99, viewed 24 April 2004 <<http://www.slnsw.gov.au/flinders/manuscripts/1.html>>; Matthew Flinders, A Journal in the Norfolk Sloop, 8 July–12 August 1799, MS copy, State Library of NSW, Sydney, 1799, viewed 24 April 2004, <<http://www.slnsw.gov.au/flinders/manuscripts/2.html>>.

³² Bass's original journal of the voyage to Van Diemen's Land in 1798–99 is to my knowledge no longer extant and so the accuracy of Collins's rendition cannot be verified. However, a comparison of parallel passages in Collins's version of Flinders's 1799 journal and in the manuscript copy of this journal suggests that Collins did not take undue liberties with his material. Strikingly, the copies of Flinders's journals, made for Governor Philip Gidley King, only refer to the *absence* of indigenous people at particular places. They entirely omit any mention of the two episodes of interaction between voyagers and local inhabitants at the River Derwent in 1798 and Moreton Bay in 1799 which are the main focus of this paper. The episodes are described in detail in Collins's published version of Bass's and Flinders's journals (David Collins, *An Account of the English Colony in New South Wales, from its First Settlement, in January 1788, to August 1801: with Remarks on the Dispositions, Customs, Manners, &c. of the Native Inhabitants of that Country. To Which are Added... An Account of a Voyage Performed by Captain Flinders and Mr. Bass; by which the Existence of a Strait Separating Van Diemen's Land from the Continent of New Holland was Ascertained. Abstracted from the Journal of Mr. Bass*, 2 vols (London: T. Cadell Jun. and W. Davies, 1802), vol.2, pp.187–8, 231–56) and briefly by Flinders in his two publications (Matthew Flinders, *Observations on the Coasts of Van Diemen's Land, on Bass's Strait and its Islands, and on Part of the Coasts of New South Wales; Intended to Accompany the Charts of the Late Discoveries in Those Countries* (London: John Nichols, 1801), p.8; Matthew Flinders, *A Voyage to Terra Australis; Undertaken for the Purpose of Completing the Discovery of that Vast Country, and Prosecuted in the Years 1801, 1802, and 1803 in His Majesty's Ship the Investigator, and Subsequently*

in the *Armed Vessel Porpoise and Cumberland Schooner...*, 2 vols. (London: G. and W. Nicol, 1814), vol.1, pp.clxxxvi–clxxxvii, cxcvi–cxcviii).

³³ François Péron and Louis de Freycinet, *Voyage de découvertes aux terres australes...sur les corvettes le Géographe, le Naturaliste, et la goëlette le Casuarina, pendant les années 1800, 1801, 1802, 1803 et 1804. Historique*, 2 vols. (Paris: Imprimerie impériale, 1807–16).

³⁴ Bass in Collins, *Account*, vol. 2, p.168; Flinders, *Narrative*, p.[21].

³⁵ Bass in Collins, *Account*, vol. 2, p.187; Flinders, *Observations*, p.8; Flinders, *Voyage*, vol.1, p.clxxxvi–clxxxvii.

³⁶ Bass in Collins, *Account*, vol. 2, p.188.

³⁷ Douglas, “Art”, pp. 70–3; Bronwen Douglas, “Seaborne Ethnography and the Natural History of Man”, *Journal of Pacific History*, vol. 38 (2003), 3–27, pp.12, 17.

³⁸ Flinders, *Observations*, p.8; Flinders, *Voyage*, vol.1, p.clxxxvii.

³⁹ Bass in Collins, *Account*, vol.2, pp.169–70, my emphasis; Flinders, *Voyage*, vol.1, p.cxl.

⁴⁰ For a later portrait of Bungaree, see: Philip Parker King, ‘Boong-gar-ree Aboriginal of New South Wales 1819 who accompanied me on my first voyage to the NW coast’, watercolour, 16.2 x 10 cm., PXC 767 f.48, State Library of NSW, <<http://img.e.s.l.n.s.w.g.o.v.a.u/c.g.i-b.in/e.b.i.n.d-show.pl?doc=pxc767/a1084;seq=2>>.

⁴¹ Flinders, *Voyage*, vol.1, p.cxciv.

⁴² For Flinder’s chart of Moreton Bay, see: Matthew Flinders, ‘Chart of part of the coast of New South Wales from Ram Head to Northumberland Isles by M. Flinders, 2nd. Lieut. of H.M.S. Reliance, 1800’, State Library of NSW, <http://img.e.s.l.n.s.w.g.o.v.a.u/c.g.i-b.in/e.b.i.n.d-show.pl?doc=flinders_maps/a125;seq=14>.

⁴³ Flinders in Collins, *Account*, vol.2, pp.232–4.

⁴⁴ Ibid, pp.239–44.

⁴⁵ This episode took place off Darnley Island in September 1792 and led Flinders to reflect: “Had the four [canoes] been able to reach the cutter, it is difficult to say, whether the superiority of our arms would have been equal to the great differences of numbers; considering the ferocity of these people, and the skill with which they seemed to manage their weapons” (Flinders, *Voyage*, vol.1, pp.xxi–xxvi).

⁴⁶ Flinders in Collins, *Account*, vol.2, pp.243, 245, 249, 250–3, 255.

⁴⁷ Flinders, *Voyage*, vol.1, p.cxcviii.

⁴⁸ Flinders in Collins, *Account*, vol. 2, p.243.

⁴⁹ Ibid, p.245.

⁵⁰ Ibid, p.249.

⁵¹ Ibid, pp.228, 238, 249, 250, 252.

⁵² Flinders, *Voyage*, vol.1, p.cxcviii.

⁵³ Flinders in Collins, *Account*, vol.2, pp.253–5; my emphasis.

⁵⁴ Douglas, “Seaborne Ethnography”, pp.19–26; Bronwen Douglas, “Slippery Word, Ambiguous Praxis: ‘Race’ and Late 18th-Century Voyagers in Oceania”, *Journal of Pacific History*, vol. 41, 1–29, pp.23–5, 27–9.

⁵⁵ Baudin to Freycinet, 14 January 1802, in Nicolas Baudin (C. Cornell, tr.), *The Journal of Post Captain Nicolas Baudin Commander-in-Chief of the Corvettes Géographe and Naturaliste Assigned by Order of the Government to a Voyage of Discovery* (Adelaide: Libraries Board of South Australia, 1974), p.302.

⁵⁶ Baudin, *Journal*, pp.300–50, 304–5, 321, 345–6.

⁵⁷ Nicolas Baudin, “Des naturels que nous trouvions et de leur conduite envers nous [Baudin to Jussieu, 18 Nov. 1802]”, in J. Copans and J. Jamin (eds), *Aux origines de l’anthropologie française: les mémoires de la Société des Observateurs de l’Homme en l’an VIII* (Paris : Le Sycomore, 1978), 205–17, pp. 207–14.

⁵⁸ Péron, François, “Observations sur l’anthropologie, ou l’histoire naturelle de l’homme, la nécessité de s’occuper de l’avancement de cette science, et l’importance de l’admission sur la flotte du capitaine Baudin d’un ou de plusieurs naturalistes, spécialement chargés des recherches à faire sur ce sujet”, in Copans and Jamin (eds), *Aux origines de l’anthropologie française*, pp.183–4.

⁵⁹ Péron and Freycinet, *Voyage*, vol.1, pp.231, 237, 238, 285, 287; my emphasis.

⁶⁰ Ibid, p.287.

⁶¹ George W. Stocking, Jr., *Race, Culture and Evolution: Essays in the History of Anthropology* (New York: The Free Press, 1968), pp.35–6.

⁶² Bronwen Douglas, “Science and the Art of Representing ‘Savages’: Reading ‘Race’ in Text and Image in South Seas Voyage Literature”, *History and Anthropology*, vol.11 (1999), 157–201; Douglas, “Seaborne Ethnography”; Bronwen Douglas, “Notes on ‘Race’ and the Biologisation of Human Difference”, *Journal of Pacific History*, vol.40 (2005), 331–8; Douglas, “Slippery Word”.

⁶³ d’Entrecasteaux, *Voyage*, vol.1, pp.359, 423; Péron and Freycinet, *Voyage*, vol.1, pp.238, 285.

⁶⁴ Williams, *Keywords*, p.58.

⁶⁵ d’Entrecasteaux, *Voyage*, vol.1, p.234.

⁶⁶ François Péron, Île Maria: observations anthropologiques: entrevue avec les naturels de cette île et description d'un tombeau trouvé sur la côte nord de la Baie de l'Est. Ventose an Xe [February–March 1802], MS 18040, Collection Lesueur, Muséum d'Histoire naturelle, Le Havre, France, p.2.

⁶⁷ Péron and Freycinet, *Voyage*, vol.1, pp.285, 446, 448, 452, 457, 458, 466, 471; vol.2, p.164; my emphasis.

⁶⁸ Williams, *Keywords*, p.58; original emphasis.

⁶⁹ Bass in Collins, *Account*, vol.2, p.169; Flinders in Collins, *Account*, vol.2, pp.253–4, my emphasis; Flinders, *Voyage*, vol.1, p.cxcviii.

⁷⁰ Flinders in Collins, *Account*, vol.2, pp.234, 252–3.

⁷¹ Baudin, *Journal*, pp.x–xi, 346; Baudin, “Des naturels”, pp.210–11.

⁷² For example, “The account of the Derwent river being now closed, and the whole of what was learned of Van Diemen’s land related, it may not be improper, says Mr Bass, to point out the manner in which this country and New South Wales appear to differ in their most essential quality, that of their soil” (Collins, *Account*, vol.2, p.189).

MORE THAN ONE ADAM?

Revelation and Philology in Nineteenth-Century China

BENJAMIN PENNY

From Marco Polo to Richard Nixon, narratives of the encounter between Chinese and Westerners have been defining texts of European cultures and their descendants. Successive but sporadic reports from travellers, missionaries, diplomats, traders and others have provided a model of an alternative way of arranging people, of organizing their lives, of thinking about the state of being human; one that described a government that was, or at least was represented as being, as authoritative as anything at home, with military power that could challenge any other, and with cultural achievements as profound. Traditionally labelled “inscrutable”, China nonetheless possessed a written literature, an esteemed bureaucracy, technological achievements, complex financial systems, codes and courts of law, and religions that had texts, buildings and hierarchies of priests. In other words, though not like us at all, they were exactly like us.

The voluminous literature of the encounter with China is above all, and consistently, a literature of *comparison*. From eating manners, to the rigging on boats, from city design to imperial customs, reports of the Chinese exotic have been seized on by centuries of eager western readers and, latterly, viewers. But the thrill these stories generate is possible only

as a response to a condition where recognition and bafflement are mixed in equal parts, where things are close enough to be familiar but far enough away to be bizarre. This is psychically exciting but it is also discomforting and unstable, and one of the effects of this has been to move the Chinese to the discursive comfort of one extreme or the other; to find a way of welcoming them into the fold or to define the conditions of their exclusion. Neither move is unproblematic: if, fundamentally, the Chinese are like us then their very obvious differences must be accounted for or, less satisfactorily, elided; if they are basically not like us, the reverse is the case. What these two moves have in common, however, is that they have sought the fundamental similarity — or difference — between China and the West in features deemed to lie at the core of what it means, or meant, to be Chinese and whatever it is, or was, that we conceived ourselves to be at that moment in history: early on it was religion; later, language came onto centre stage; now perhaps it is in conceptions of the rights of individuals.

This paper focuses on a largely forgotten chapter in this history in the form of a book that attempts to show, in the words of its subtitle, that *the Languages of Europe and Asia have a Common Origin*, and in

doing so that the people of China and Europe, too, share a common descent. The book is *China's Place in Philology*, written by the Reverend Joseph Edkins, Doctor of Divinity, who lived from 1823 until 1905 and was resident in China from 1848 until his death.¹ Edkins left an enormous legacy of work across the whole range of topics in the history, religions, literature, geography, philosophy, and economy of China (as well as its language) in English, apart from his copious translations into Chinese — not least of the Bible — and original works in that language. Published in 1871, *China's Place in Philology* did not meet with universal acclaim; indeed, in some quarters it was derided, but his work in this field remained, in Edkins's own opinion, his most valuable and far-reaching.

Edkins was sent to China by the London Missionary Society or LMS, an evangelical Protestant society based in London established in 1795 as The Missionary Society, changing its name in 1818. This was by no means the only mission society active in China through the nineteenth century: there were representatives of most of the Christian denominations, Roman Catholic and Orthodox as well as Protestant. Among the Protestants were missionaries from across the English-speaking world, usually attached to their own national and denominational groups, and also from many European countries, each with their own goals and emphases. Even amongst the British evangelical societies, there were clear demarcations: not only in the region, or mission field, but in strategy and theology as well.²

For many years, mission history was an unfashionable field of research, bedev-

illed as it was, and to a certain extent still is, by people for whom conversion was not just a phenomenon to be studied but a goal to be prayed for. However, in Chinese Studies at least, to ignore missionary writings is to ignore a vast and valuable archive. And to understand the nature of these writings, the particularities and specific contexts of each author have to be understood: to regard them all as having the same ideologies, the same attitudes to Chinese people, the same project, is much mistaken. From the 1950s to the 1980s, a standard textbook on modern Chinese history was Teng and Fairbanks's *China's Response to the West*.³ This title reflected the commonly accepted totalizing binary of the time. Fortunately, however, in more recent years a pluralizing tendency has gained ground, with both of the categories "China" and "the West" gradually becoming disaggregated in the scholarly literature. In Edkins's time, under the category "the West" there existed a web of heterogeneous possibilities of involvement with all sorts of different Chinese people. Europeans of many kinds, Americans, Australasians; missionaries as well as traders, customs officials, military personnel and diplomats; and bureaucrats and scholars who worked on China based in western capitals — to aggregate all these into a single entity that had a unified project is to grant, perhaps, more credence to justifications emanating from the metropolitan capitals for foreign adventurism of various kinds than the complex situation on the ground might warrant.

Thus, it is important to place Joseph Edkins in his place and time, to grant him his individuality and idiosyncrasy, and to allow him his disputes with colleagues, fellow nationals and co-religionists.

Edkins, along with most of his colleagues — with the major exception of James Legge — has received only passing scholarly attention.⁴ One of the goals of this paper, and the larger project of which it is a part, is to rescue Edkins and his scholarly colleagues from the academic obscurity into which they have fallen. It is my contention that this notable group of scholar-missionaries — not that they would have seen themselves as a group — laid down the analytical categories for understanding aspects of Chinese society that stood for decades in the West and in various Chinese societies across the world, including the People's Republic, and indeed to some extent still stand. Before moving on to a detailed discussion of Edkins, his work and its reception, and Edkins's conception of his own position in relation to the Chinese people amongst whom he lived most of his adult life, it may be useful to review and discuss some of the vocabulary of encounter.

CULTURE AND CIVILIZATION

While we may judge that what Edkins was doing in *China's Place in Philology* — which was completed in 1870, the year Tylor's *Primitive Culture* appeared in London — was what we would call “cross-cultural research”, the word “culture” in its common usage does not appear in his book. Indeed it was not until 1912 that the title of a book in English about China used the word “culture” in this sense — in Ernst Boerschmann's pamphlet *Chinese Architecture and its Relation to Chinese Culture*.⁵ Boerschmann was a German photographer resident in China who is not generally recognized as a writer in English and this sense of culture is of German de-

rivation, so Boerschmann's case is complicated. The first clear case of a work by an English native speaker is Maurice Price's *Christian Missions and Oriental Civilizations, a Study in Culture Contact; the Reactions of Non-Christian Peoples to Protestant Missions from the Standpoint of Individual and Group Behaviour: Outline, Materials, Problems, and Tentative Interpretations*, privately printed in Shanghai in 1924.⁶

What word — what category — did Edkins and his colleagues use instead of “culture”? Or did they simply get by without one? One candidate for this task was “civilization”, but if culture is complicated, civilization is perhaps even more so, in this context at least. Raymond Williams' *Keywords* proves a useful starting point. Starting life as a term that described a process, originally “to make a criminal matter into a civil matter, and thence, by extension, to bring within a form of social organization”, by the latter part of the eighteenth century “civilization” had acquired the sense of “a state of social order and refinement, especially in conscious historical or cultural contrast with *barbarism*”.⁷ This sense of civilization places it at one end of a unilinear scale against which all societies, and activities, can be placed and compared. The fact that this unilinear scale was generally accepted at the time did not mean that there was general acceptance of what societies occupied what positions on the scale. In the case of China, it managed to occupy positions corresponding to both barbarism and to civilization according to different people at different times. Thus, while its criminal justice system with its public executions, torture and physical punishments like the canue was deemed barbaric in the extreme by some outraged expatriates, its

court and ritual code could equally be held up at the same time as the epitome of civil human relations.⁸ The sense of civilization as “an achieved condition of refinement and order” finds its way into discussion of China by at least the early-nineteenth century. In 1804 Sir John Barrow, Secretary to the Admiralty and founder of the Royal Geographical Society, published his account of Britain’s first embassy to China of 1793, on which he accompanied Earl Macartney, the appointed envoy. This book is called *Travels in China: Containing Descriptions, Observations and Comparisons Made and Collected in the Course of a Short Residence at the Imperial Palace of Yuen-min-yuen, and on a Subsequent Journey from Peking to Canton. In which it is Attempted to Appreciate the Rank that this Extraordinary Empire may be Considered to Hold in the Scale of Civilized Nations*. In this book, Barrow claims to show the Chinese as they really were, as opposed to the view of them commonly held on the basis of reports from the Jesuit missionaries which had held sway for decades. Thus, he writes:

The voluminous communications of the missionaries are by no means satisfactory; and some of their defects will be noticed and accounted for in the course of this work; the chief aim of which is to show this extraordinary people in their proper colours, not as their own moral maxims would represent them, but as they really are...and to endeavour to draw from such a sketch...as may enable the reader to settle, in his own mind, the point of rank which China may be considered to hold in the scale of civilized nations.⁹

Barrow’s discussions of China’s position in this scale of civilization begin by asserting that “civilization” depends to a large extent on material progress: science, arts, manufactures, the conveniences and luxuries of life, to use his measures. On this scale he judges China “greatly superior” to Europe “from the middle to the end of the sixteenth century”. Indeed, “when the King of France introduced the luxury of silk stockings, which, about eighteen years afterwards, was adopted by Elizabeth of England, the peasantry of China were clothed in silks from head to foot.”

However, “the Chinese were, at that period, pretty much in the same state in which they still are; and in which they are likely to continue”; that is, they had not developed further in the previous two centuries and had been overtaken by Europe during that time.¹⁰

For Barrow, this civilization is a matter of social attainment rather than being defined or limited by descent. Thus, he asserts that while the Chinese and those he calls “Malays” were both “unquestionably descended from the ancient inhabitants of Scythia or Tartary,” the Malays’ conversion to Islam “first inspired, then rendered habitual, that cruel and sanguinary disposition for which they are remarkable”.¹¹ Thus while the Chinese have bettered themselves on the scale of civilization, people of the same ancestry, the Malays, have regressed.¹² For Barrow, then, civilization is a state that societies achieve or lose, and on the basis of which societies can be compared, like to like, favourably or unfavourably on a single scale, taking into account attributes such as material progress or the propensity to spill blood.¹³

In 1840, some 36 years after Barrow's book had appeared and, importantly, after the first wave of British Protestant missionaries had made their way to China, the Reverend W.H. Medhurst, who had arrived in Malacca in 1817 to work on the mission to the Chinese — moving to Shanghai after the First Opium War — and who was, like Edkins, employed by the London Missionary Society, published his *China: its State and Prospects, with Especial Reference to the Spread of the Gospel, Containing Allusions to the Antiquity, Extent, Population, Civilization, Literature, and Religion of the Chinese*.¹⁴

Medhurst begins his chapter on "The Civilization of China" in this way:

In seeking to evangelize the heathen world, two descriptions of people claim our attention: namely, the barbarous and the civilized. China belongs to the latter class. Instead of a savage and untutored people — without a settled government, or written laws, — roaming the desert, and living in caves, — dressed in skins, and sitting on the ground, — knowing nothing of fashion, nor tasting luxuries; we behold in the Chinese a quiet, orderly, well-behaved nation, exhibiting many traces of civilization, and displaying them at a period when the rest of mankind were for the most part sunk in barbarism.

We see here the same evaluation of China as a civilized nation, familiar from Barrow but, unlike him, Medhurst tempers his enthusiasm with an explicit appeal to religion: "Of course we must not look for that high degree of improvement, and those

well-defined civil rights, which are in great measure the effects of Christianity."¹⁵

With Medhurst, then, the categories "civilization" and "barbarism" are overlaid with another set, namely "heathen" and "Christian". That these categories do not necessarily map onto each other is clear from the evaluation of China as both civilized and heathen — distinguishing it from much of the mission field where "heathen" and "barbarism" collocated comfortably. Indeed, China stood as the exemplum, if not the only case, of a civilized and heathen nation of the present though it had precursors in the ancient world in pre-Christian Greece and Rome. "Christian" and "barbarism", needless to say, is not a possible combination.

Williams notes, in his article on "civilization", that "there was a critical moment when civilization was used in the plural", noting that the English use is later than the French.¹⁶ This use of "civilizations" approaches the contemporary meaning of "cultures", at least insofar as it implies that different places have distinctive ways of life and thought that are organically whole. What distinguishes this meaning of "civilization" from the comparable meaning of "culture" — as in "Chinese civilization" and "Chinese culture" — is a question of register: discussions of "Chinese civilization" usually begin with the ancient philosophical systems and include examples of artistic and technological achievements arranged in historical sequence. "Chinese culture" on the other hand tends to be less historical and more concerned with the lives of ordinary people. Of course, there are no firm lines of demarcation between civilization and

culture, as there equally are not between the senses of civilization in the singular and the plural. It is worth stressing that such changes in meaning are gradual and uneven and single authors may shift almost imperceptibly from one sense to another; indeed, we should acknowledge that the use of the singular form “civilization” and the plural “civilizations” sometimes overlaps.

In writings on China in English the plural sense of civilization seems to appear in the latter part of the 1880s, well after Edkins’s cogitations on the nature and origins of the Chinese language, and his understanding of the meaning of civilization seems close to Medhurst’s. It is interesting, though, given Williams’s observation on the earlier French use of the plural form “civilizations”, that perhaps its first clear use in relation to China is in a translation from that language: Pierre Laffitte’s *A General View of Chinese Civilization and of the Relations of the West with China*, published in French in 1861, and in English translation not until 1887. Laffitte, who revelled in the wonderful title “Director of Positivism”, was Auguste Comte’s direct disciple but was no specialist on China. This did not stop him in his ambitious undertaking, in three lectures:

Gentlemen, We are to enter to-day upon a survey of the whole field of Chinese civilization. In view of the importance of such a study, both in itself and in its bearings on the problems of the science of society, we shall devote to it three lectures...At the base of the farthest East is a noteworthy civilization, which, say what we may about it, is in constant devel-

opment and in full activity, and is being brought day by day into closer contact with the West. This civilization, in so many respects so much misunderstood, is that of China.¹⁷

In these lectures, Laffitte treats “Chinese civilization” as a discrete entity that possesses certain distinctive features, has specific traits and manifests a particular pattern of development. A civilization, for Laffitte, is a kind of entity made up of selected elements of a nation’s lifeways, rather than an attribute a nation has more or less of, as it was for Barrow and Medhurst. Civilizations, so conceived, can still be judged against each other in terms of their attainments or levels, but Laffitte’s approach also pointed to the possibility of a model of human development that moved away, potentially at least, from an uncompromising unilinearity. With this model, the possibility is raised of the ways of life and systems of thought of different places developing along their own tracks to equally civilized points but remaining thoroughly distinct. That such a possibility was conceived in the middle of the nineteenth century is, of course, no accident, parallel as it is to the rise of nationalist movements across Europe with their conceptions of specific national essences and peculiarities. Aligned to this distinction, though different from it, are discussions related to whether humankind — or particular features of people’s lives — had a single origin or multiple origins. Arguments about monogenetic and polygenetic theories, as they are called, featured crucially in the study of the origins of language and the history of specific languages, as will be discussed below.

Along with the two models of development, leading respectively to “civilization” and to “civilizations”, a third story should be considered. Specifically Christian, and, in relation to studies of China, usually Protestant, this story is found most explicitly in works of those highly educated and thoroughly modern scholar-missionaries (including Edkins) who we would now also refer to as scriptural literalists; that is, people who took the words of the Bible as literally true. So with the book of Genesis in one hand and a knowledge of recent scientific advances in the other, these scholars set about to demonstrate as well as they could that the ultimate monogenetic hypothesis, namely that we all derive from Adam and Eve, was not only compatible with the state of knowledge of the time but could be proved with academic rigour. In Edkins’s words — about language but it could equally apply in many other fields — this work was “for the vindication of Scripture and the progress of knowledge”.¹⁸

Positing Adam and Eve at the root of the tree of humanity, as this position did, the process of change that produced human diversity often became understood as one of degeneration, as moving away, step-by-step from the point of our common origin and God’s first revelation, both literally in geography and metaphorically in culture. From this point of view, however savage or barbaric the people you might meet in your travels, their origins were the same as yours and, though subject to different conditions since the original revelation, you and they were all part of a common brotherhood and their forebears had, therefore, received the same revelation from God as had yours. One attraction, then, for the study of ancient societ-

ies and languages in the nineteenth century — Egyptian, Accadian, Sanskrit, Chinese — was to try to recover those remnant parts of the original revelation preserved in non-Semitic textual traditions. As Max Müller, Professor of Sanskrit at Oxford, editor of the Sacred Books of the East series and doyen of comparative philology, wrote in 1878: “The more I see of the so-called heathen religions, the more I feel convinced that they contain germs of the highest truth.”¹⁹

Yet the fact remained that European civilization was only made possible, in some versions of this theory at least, by its Christian character. The revelation of Jesus reversed the degenerative process and not only granted salvation to humanity but also a civilized character to society. As Medhurst wrote: “Of course we must not look for that high degree of improvement, and those well-defined civil rights, which are in great measure the effects of Christianity.”

It was on this theoretical terrain that Edkins produced his work that attempted to demonstrate that “the Languages of Europe and Asia have a Common Origin”. To understand this work — its motivations and its methodologies — we must walk this ideological landscape with him, following the same scholarly maps, observing what lay at his horizon. Setting aside the arrogance of hindsight, we can approach an understanding of how Edkins and his colleagues saw themselves and their work among Chinese people only by allowing argument from a literal reading of Genesis to stand as the unassailable foundation of theory.

JOSEPH EDKINS

Edkins's death at 81, in 1905, produced four obituaries, one in each of the major Chinese Studies journals of his day.²⁰ The overriding impression from them is of an old campaigner who had died in harness, a figure notable a generation or two before who continued to plough his furrow with energy but whose best work had been produced some time earlier. There is, in one at least, the snide tone of a younger competitor keen to prick the bubble of what he evidently saw as an overblown reputation.

Edkins was born in Nailsworth, near Stroud, in Gloucestershire, on December 19, 1823. The son of a Congregational minister who also ran the school where Edkins was first educated, he later entered Coward College for theological training. He graduated in arts from the University of London and was ordained in 1847 at the age of 24 in the Stepney Meeting House, London, a Congregational institution. On gaining ordination, he left England for China under the auspices of the London Missionary Society, arriving in Hong Kong in July 1848 and proceeding to Shanghai soon after. In his first correspondence with the LMS in London in 1848 Edkins started to plead for a Miss Phillips to join him in China. These pleas continued for almost two years, and were evidently never acceded to, as he finally had to let the London office know that his engagement had terminated.²¹ His colleagues at Shanghai included Medhurst, William Lockhart, a notable medical missionary with whom he would later travel to Beijing, and Alexander Wylie. With Wylie, in 1857, he formed the Shanghai Literary and Debating Society that later became the North China

Branch of the Royal Asiatic Society of North China. In 1858 he left for England in order to marry his wife, Jane (nee Stobbs, 1838–61), a Presbyterian minister's daughter from Orkney. Returning the following year, in 1860 he made several famous visits to the leaders of the Taiping Rebellion in Nanjing and Suzhou, not far up the Yangtse from Shanghai.

After the opening of more treaty ports after the Second Opium War, in 1860 Edkins moved to Yantai in Shandong, then to Tianjin in 1861 and finally in May 1863 to live permanently in Beijing, where he spent nearly 30 years. Jane Edkins had died of dysentery in 1861 at the age of 23 but some of her letters home were published posthumously under the title *Chinese Scenes and People, with Notices of Christian Missions and Missionary Life in a Series of Letters from Various Parts of China*. In one of her letters to Edkins's brother she wrote endearingly:

You ask me to tell you about your brother. He is very well indeed, and is busy as a bee. We breakfast every morning at eight, and have prayers before. He spends the morning at home studying, and in the after part of the day he is in the city preaching, and otherwise attending to the work of the Mission. I have got his study all in nice order, and there he is in his glory. From nine till one each day you might take a peep in and find him excogitating, diving deeper and deeper into the mysteries of Buddhism and Confucianism. Seated thus by his study table he puts me in mind of that picture, "As Happy as a King," for he looks

quite that, with all his Chinese books in notable confusion beside him.²²

In Beijing, Edkins spent much of his time preaching in the hospital Lockhart had established and otherwise going about mission business in Beijing and surrounds. In 1862 he requested that a Miss White be sent to marry him and she arrived early the following year. They married on May 9, 1863.²³ The second Mrs Edkins subsequently founded a school for girls and gave birth to three daughters. The family went to England in 1873, when Edkins was honoured in 1875 with a doctorate in divinity from Edinburgh University. They subsequently returned to Beijing in 1876, but his wife died the next year from breast cancer — two of their children had already died and, two years later, the third girl was buried next to her mother and two sisters.²⁴

Relations between Edkins and some younger missionaries from the LMS stationed in Beijing became strained by the late 1870s. In particular, it would appear that Edkins was viewed as being too generous to Chinese converts with the mission's funds. His younger colleagues were rather more suspicious than Edkins of the motivations of new converts who were given to "backsliding" as it was called. Ultimately, as Box wrote in his obituary: "In 1880 he resigned his connection with the L.M.S., not through any lack of interest in mission work, for until his death he was devoted to the cause of missions, but through difference of opinion with his colleagues as to methods of mission work."

There was, however, another side to this story revealed in his unpublished correspondence. For the second time in

his life, head office of the LMS appears to have refused Edkins's request to get married — this time to an expatriate German missionary by the name of Miss Johanna Schmidt.²⁵ After resigning from the LMS, Edkins married Miss Schmidt and began working for the Inspector-General of Imperial Maritime Customs while still active in the life of the church. About 1890 they moved to Shanghai, where they stayed until his death. Little is known about the third Mrs Edkins, including how long she stayed in China, and when and where she died. Box relates Edkins's passing in a superb description of the "good death":

As she [Johanna] sat by his bedside she saw his eyes fixed upward and his face suffused with a strange light. His lips moved, and presently she heard him murmur, "Wonderful! Wonderful!" She asked him what he saw, and he replied, "I cannot tell you, but you will know what it means tomorrow!" It was on the morrow he passed through the gates of death into "the Glory Land", of which he evidently had a vision.²⁶

Throughout his time as a missionary, Edkins was also writing. His scholarly output is extraordinary in its sheer volume, its range and its quality. Henri Cordier's obituary is, in reality, a catalogue of Edkins's works and incomplete though it is, it lists more than 140 books and learned articles. His best-known work today, though it is by no means as well-known as it ought to be, is his *Chinese Buddhism: a Volume of Sketches, Historical, Descriptive and Critical* from 1880.²⁷ However, it is clear that, as Bushell wrote in his obituary, "*China's Place in Philology*

was probably the book nearest the author's heart".²⁸ But he continues:

...the general consensus of opinion is that it hardly suffices to prove his somewhat daring thesis of the common origin of the languages of Europe and Asia. Dr Edkins was always original. His reading of Chinese literature was most extensive, and the words of the other languages cited in the text were actually taken down from the mouths of Tibetans, Koreans, Manchus, and Mongols, yet the theme was almost too discursive even for his power of concentration.

Others, too, marvelled at his proficiency in languages; thus Box: "His knowledge of languages was most extensive — English, German, French, Latin, Greek, Hebrew, Assyrian, Persian, Sanscrit, Tamil, Chinese (in most of its dialects), the Miao dialects (...), Japanese, Manchu, Corean, Thibetan, Mongolian and others."

The anonymous obituarist in the *Journal of the China Branch of the Royal Asiatic Society* was more caustic:

It is only fair to say that in his philological theories Dr Edkins stood almost alone, and that very little sympathy, sometimes even very little patience, was shown to them by other scholars whose study of the Chinese language itself had perhaps been more thorough than that of Dr Edkins. However, it must be said that in combining a knowledge of Eastern languages — of Hebrew, Persian and Sanskrit — with a knowledge

of the modern languages of Europe, Dr Edkins was perhaps the foremost of his generation. The vast scope of his language studies made them all more or less superficial, while at the same time it made it possible for him to make philological comparisons which would have been impossible to anyone else.²⁹

In these comments it is possible to see the emergence of one style of scholarship, and the concomitant decline in another, which has ruled much of humanities scholarship to this day. Edkins was one of the last generation, in Chinese Studies at least, of the grand comparativists. Partly as a result of the decline in the kind of broad linguistic training he received, and partly because of the growth in university departments concentrating on a single subject (the Chairs in Chinese Studies at Oxford and Cambridge date to 1876 and 1888 respectively), scholars of later generations have ploughed much narrower, but much deeper.

CHINA'S PLACE IN PHILOLOGY

By Edkins's time, the shared history of the Indo-European languages had been demonstrated and accepted. The great impetus for this study had been the growth of European scholarship on Sanskrit, and the major figure in the first half of the nineteenth century in this field had been Franz Bopp (1791–1867). Bopp had shown the relationship between Sanskrit, Persian, Greek, Latin and the Germanic languages (and later Old Slavonian, Lithuanian, and Zend — the language of the Zoroastrian Avesta scriptures) through his comparative study of grammatical forms; thus his first

work was on verbal inflexions. He is best known for his *Comparative Grammar of the Sanskrit, Zend, Greek, Latin, Lithuanian, Gothic, German, and Slavonic Languages* which appeared in German from 1833 and in English translation beginning in 1845.³⁰

Edkins's comments on the Indo-European project open his book:

To show that the languages of Europe and Asia may be conveniently referred to one origin in the Mesopotamian and Armenian region, is the aim of the present work. Sanscrit philologists, entranced with admiration of the treasure they discovered south of the Himalayan chain, forgot to look north of that mighty barrier. Limiting their researches to the regions traversed by Alexander the Great, they allowed themselves to assume that there was no accessible path by which the linguistic investigator could legitimately reach the vast area existing beyond their adopted boundary. The result of this abstinence on the part of Bopp and other scholars of high fame has been that the idea of comparing Chinese, Mongol, and Japanese with our own mother-tongue appears to some chimerical, hopeless, and uncalled for.

"Yet," he continues:

...Scripture, speaking with an authoritative voice and from an immense antiquity, asserts the unity of the human race, traces the most general features of the primeval planting of nations, and declares

that all men once spoke a common language. The most revered and most ancient of human books, in making these statements, sheds a bright and steady light on the obscurity of history, and at the same time reveals the imperfection of those views held by some modern thinkers and writers who deny that the languages of the world had one origin and that its races came from one stock.

Edkins was by no means the first to see links between Chinese and languages of peoples far to the west — such discussions go back at least to John Webb's *An Historical Essay Endeavouring a Probability that the Language of the Empire of China is the Primitive Language*, published in 1669.³¹ Most of these works refer to Biblical chronology, a detailed discussion of which will occur below, as the crucial evidence put forward for the truth of Edkins's proposition comes from the beginning of the eleventh chapter of Genesis: "And the whole of earth was of one language, and of one speech." First, however, we should note that Scripture was only the spur to Edkins's work, and did not relieve the scholar from further research, informed by the most advanced studies of his time. Indeed, Edkins placed his work in a thoroughly modern linguistic context and in this book was launching a serious critique of accepted linguistic wisdom. Thus, relying on Max Müller's hypothesis of "dialectal regeneration" — first published in Müller's *Lectures on the Science of Language* in 1864 — to bolster his argument, Edkins contended that the Indo-European inflected languages and agglutinative languages (such as those of Tartary, South India and Japan) were fundamentally related. This

flew in the face of contemporary ideas about language taxonomy and was one reason, Edkins claimed, for the exclusion of Asian languages from comparative philology.

Another was the so-called isolating nature of some of these languages, Chinese being the classic case. In Chinese most morphemes are free-floating and rely on syntax to acquire grammatical function: words neither inflect, as in most European languages, nor glue together — the etymological root of “agglutinative” — as in Japanese. In the case of Chinese, it was obviously impossible to compare its verb endings with those in, say, Sanskrit, because it didn’t have any. Thus, Edkins proposed that the word roots of Chinese and similar languages should be compared to bring them into the comparative fold. This did not find favour with some reviewers but it did represent an attempt to introduce into the discussion an original methodology designed to address a question that had previously simply been ignored.

Implicit in Edkins’s arguments is his defence not only of Scripture in general but, more specifically, for the position that the languages of humankind had a single origin. For Edkins, with his scientific cast of mind, finding the language of Adam himself was never going to be a viable scholarly project, though he did allow himself some speculations of the nature of “the primeval language”. Rather, in arguing the monogenetic case on purely philological grounds, Edkins, arguably, sought to lay a scientific foundation for faith. In these debates it is worth stressing once more that for Edkins, Scripture was not the proof; rather, it was philological — and other modern scientific — argu-

ment that worked towards a vindication of Scripture. He applies the same attitude to another lively field in nineteenth-century scholarship: “After a careful sifting of recent discoveries by the geologists on the antiquity of man, it will be the duty of the Christian theologian to examine afresh the question of early Biblical chronology. All new light brought upon this subject from unexpected quarters must be cheerfully accepted...”³²

And, similarly, Edkins adopted a model of linguistic evolution pioneered by Max Müller on the Darwinian model. *The Origin of Species* had been published in 1859, and provided linguistics with the tools capable of turning the study into a science, as it was perceived, with linguistic laws being the equivalent of the laws of the natural sciences. Müller adopted a model of natural selection in language with alacrity arguing, in the *Lectures on the Science of Language*, that languages formed, changed and died out through a series of processes corresponding to the biological model, except that:

...natural selection, if we could but always see it, is invariably rational selection. It is not any accidental variety that survives and perpetuates itself; it is the individual that comes nearest to the original intention of its creator, or what is best calculated to accomplish the ends for which the type or species to which it belongs was called into being, that conquers in the great struggle for life. So it is in thought and language.³³

Thus, the imperatives of religion and science were both met: the fundamentals of the faith were safe from being overthrown

by the discoveries of comparative philology and comparative philology would be able to take its place beside astronomy and geology in the scientific pantheon.

This position, was, of course, more than acceptable to Edkins, providing him with a mechanism of linguistic change to apply to his grand model of the development of the world's languages. It should be noted, however, that for Edkins language evolution is not teleological. We are, perhaps, too accustomed to seeing the process of biological evolution leading inexorably to us; that is, from lesser to greater complexity up a developmental ladder. In fact, however, natural selection need not lead to greater complexity, simply to greater suitability to the environment in which the organism finds him or herself. Thus, for Bible-believing linguists, language evolution could simply mean language change as the people who spoke each language found themselves in new environments. This is important from two points of view: firstly, the original language was given by God to Adam and it would be inconceivable to believe that this first language could be improved over time — if anything the reverse should be the case, as in the model of degeneration; secondly, Edkins and his colleagues were linguistically very capable and would have appreciated that languages do not necessarily increase in complexity as time passes. Ancient languages like Latin, Greek and Hebrew were, after all, no less complex than modern English or modern Chinese.

To return to the book itself: *China's Place in Philology* reads as a linguistic and cultural history, from prehistory up to the development of European languages in comparatively recent historical times.

There is not the space here to give a complete summary of Edkins's work, and indeed much of it is complex and needs to be read closely to follow his arguments, so here I will concentrate on the underpinnings of his research and give a broad outline of his views.

Edkins's argument does not, in fact, begin with language but with a comparison between the civilizations of the ancient Chinese and the ancient inhabitants of the Middle East: "The resemblance existing between the old [that is, ancient] Chinese civilization and that of the Hamite race [that is, the descendants of Ham, the second son of Noah] long ago developed on the banks of the Nile and the Euphrates is very remarkable."³⁴

There follows a catalogue of similarities in customs, agricultural methods, and architecture, amongst other topics, and the basic proposition is raised:

So close a similarity in genius between the descendants of Cush and Mizraim [two of the sons of Ham], who founded the first arts of the west, and the Chinese, who on the east of the Indo-European area have always reigned supreme in intellect and manual ingenuity, argues a probable connexion of race.³⁵

Importantly, for Edkins, there were also (as he saw them) close affinities between the worship, sacrifices and religious buildings in the ancient Holy Land and those in China. For him this pointed to an original monotheism in the Chinese, a monotheism that derived from their shared ancestry with the Semitic peoples. This stance echoes throughout the history of

the Western encounter with China, most particularly, of course, in missionary circles where the possibility of conversion was seen to be enhanced if, at the very root of Chinese religion, lay a belief in a single all-powerful deity — especially if that deity was actually, originally, Jehovah. It also had direct consequences for perhaps the longest-running and most bitterly fought controversy amongst the missionary fraternity in nineteenth-century China — the so-called term question. The essence of the “term question” can be easily stated: what is the best translation of the word “God” in Chinese? Which, if any, of the words found in Chinese texts meant what Christians mean by God? Huge storehouses of human effort were expended on these questions, and acrimony was often not far from the surface, as, for Protestants at least, translation of Scripture was at the core of their vocation and it was obviously imperative to get the word for “God” right. So, if the ancient Chinese were truly the descendants of people who had received the original revelation, the mystery and nature of ancient Chinese religion could be understood and the right words could be identified.

Now, as obvious for Edkins that the Chinese were originally monotheistic was the fact, observable about him in Beijing as well as in the most ancient of texts, that Chinese religious practice also included features not found in ancient semitic religion. One of these clearly non-monotheistic practices was the role played by heavenly bodies in astrology as well as in star cults. Edkins uses the term “Sabeanism” to describe this style of worship, explaining: “That the early Chinese should, in addition to their monotheism, have become infected with the Sabeanism that Job

condemned, and with some other heathen usages found to prevail long after in the countries from which they came and through which they passed, need not be wondered at...”³⁶

Thus, the people we know as Chinese originated in the Mesopotamian region and migrated slowly eastward, arriving in China at “nearly 3,000 years B.C.”. They entered that land, “by the usual highway from Mohammedan Tartary, into Kansu and Shensi, founding colonies along the banks of the western tributaries of the Yellow River, where we find the ancestors of the family,”³⁷ then subsequently spread out into those areas of early Chinese settlement we know from the ancient texts.

These Chinese were not, however, the first to enter the territory of China. In Edkins’s scheme, the “migrations of races have been in the direction of radii from a common centre where the first human pair were created”.³⁸ One route was into India through the Punjab and was followed first by the Dravidians “and after them the Hindoos”. Another group — “the Eastern and Western Himalaic races” — crossed Tibet and followed the Brahmaputra, heading south and east into Indo-China and north and east into south-western China. The Chinese, meantime, went north and west along what became known, much later, as the Silk Route. The Himalaic branch that entered China from the south constituted, according to Edkins, the “Miau, Lo lo, Nung, [and] Yau” ethnic groups known under the current dispensation as “national minorities”. This southerly branch met with the northerly branch in various regions across China.

Following this explanation of how the Chinese entered their destined territory, Edkins moves back to postulate on the origins of language itself. He proposes that some elements and characteristics of “the primeval language” are retrievable by philological comparison. Thus, “that it was monosyllabic is deducible from the fact, that in all the families, from the Indo-European upwards, the roots are monosyllables”³⁹ and “the structure of sentences in the primeval language, it may be reasonably concluded, was according to the order of nature. The nominative preceded the transitive verb, and the transitive verb preceded its object. The Chinese, the Hebrew, and the English here agree.”⁴⁰ The other way of determining the nature of the first language, of course, is by recourse to Scripture. The classic statement of language origin in the Bible is from the second chapter of Genesis: “And out of the ground the Lord God formed every beast of the field; and every fowl of the air; and brought them unto Adam to see what he would call them: and whatsoever Adam called every living creature, that was the name thereof.”⁴¹ This, in Edkins’s reading, meant that while “divine assistance” was required to make language, it was not fully developed at that stage. This was so because the initial language act was simply the naming of animals — full language competence was a gradual process aided by divine assistance but not granted complete. Edkins quotes a Dr Magee approvingly in this context: “It is sufficient if we suppose the use of language taught him [Adam] with respect to such things as were necessary, and that he was left to the exercise of his own faculties for further improvement upon this foundation.”⁴²

Having established the essential characteristics of the primeval language, Edkins addresses the important issue of combining Biblical chronology with his scheme of language development. The downfall of the primeval language was, of course, the Confusion of Tongues at the Tower of Babel, an event Edkins dates to 400 years after Noah’s Flood, which itself took place 2,200 years after Creation.⁴³ However, his position on Babel is, perhaps, surprising:

The Scriptural account of the Deluge and of the Confusion of Tongues I suppose to refer particularly to the world according to its dimensions as then understood, the *πᾶσα οἰκουμένη* [*pasa oikoumene*, all inhabited regions of the day]. Colonies that went beyond the limits of the Flood of Noah, if there were such, were lost from view.⁴⁴

What this enables, for him, is the possibility that in some specific cases the primeval language may have survived God’s intervention, if the speakers of the primeval language, or their descendents, no longer lived in the world as known by the Babylonians. He cites two cases of this: first, in Genesis 4 it says that when Cain was expelled from the presence of the Lord, he “dwelt in the land of Nod, to the east of Eden”.⁴⁵ With his wife, he subsequently produced the line of succession that ran from Enoch to Lamech and beyond. Of this, Edkins says:

The Cainites went...to the east. Whether any of them and the other descendents of Adam passed into East Asia and America during those 2,000 years now so little

known, we cannot tell. If they did, they would have there been beyond the reach of the Deluge, which science has shown did not extend to the more distant parts of the continent.⁴⁶

The second case is that of the Cushites, the descendants of Cush, the son of Ham, grandson of Noah and father of Nimrod, the mighty hunter. The Cushites were, then, Nimrod's people who built the Tower at Babel. Edkins proposes, on the basis of the shared culture of the Babylonians and the Chinese that he observed earlier, that the wave of emigration that produced the ancient Chinese left the Cushite region after the Flood — thereby acquiring Babylonian civilization — but before the Confusion of Tongues — to preserve the primeval language. Thus, when these Chinese arrived in China from the north they displaced the people they met there, the Eastern and Western Himalaics who had arrived earlier from the south, and who were the result of migrations from before the Flood, and therefore less civilized. This accounts for why both groups in China spoke monosyllabic languages like the primeval tongue as they were not subjected to God's punishment after the Tower of Babel.

I have spent a good deal of space on Edkins's explanations of the origins of the Chinese people and their language. In the rest of the book, he proceeds to explain in similar terms the Semitic, Himalaic, Turanian, Malayo-Polynesian and Indo-European language families, though I will not cover that ground here. Let me add that, while cataloguing those parts of his work I have neglected in this paper, each step of his developmental edifice is illus-

trated with copious linguistic examples displaying his remarkable breadth of knowledge. The point of the whole enterprise, however, remains a proof of the fundamental unity of the world's languages and of the world's peoples, and especially the original revelation that all peoples received in the beginning. In his conclusion he writes, *inter alia* quoting the seventeenth chapter of Acts and a famous passage from Max Müller's *Lectures on the Science of Religion*:

"God hath made of one blood all nations of men for to dwell on all the face of the earth." When the European goes into the other continents of the world, as traveller, colonist, missionary, and civilizer, he meets everywhere with men of the same race. "But what have we in common with the Turanians, with Chinese, and Samoyedes? Very little it may seem: and yet it is not very little, for it is our common humanity. It is not the yellow skin, or the high cheek-bones, that make the man. Nay, if we look but steadily into those black Chinese eyes, we shall find that there, too, there is a soul that responds to a soul, and that the God whom they mean is the same God whom we mean, however hopeless their utterance, however imperfect their worship." Language proves them to be one with ourselves.⁴⁷

Edkins's radical monogenism is, thus, buttressed on the one hand by his firm belief in the literal truth of Scripture, and on the other by an ethic of the common brotherhood of all peoples; the savage, the barbaric and the civilized. In a kind of

reply essay in *The China Review* to some harsh reviews of *China's Place in Philology*, Edkins describes the two schools of thought relating to ancient China. The first, he claims, "looks upon its old civilization as self-grown, desiderates no connection with the old Asiatic empires of the Old Testament, and detracts in many ways from the credit hitherto allowed to the ancient Chinese". "The other party", of which Edkins was a member, he suggests, "desires to harmonize the safe conclusions of modern geologists and ethnologists with regard to the antiquity of man, both with the historical traditions of Judea and Babylon, and with those of the Chinese." The choice between them, he says, is between the proposition that "religion, language and history are one in origin" and the alternative that, "there was more than one Adam".⁴⁸ In his view, any polygenetic model was, by definition, against science, against Scripture, and against common brotherhood.

CONCLUSION

Edkins's book was ambitious in its scope, taking in all the world's peoples and their languages. There is, however, a striking absence: the living, breathing, speaking Chinese he lived among. This is somewhat strange as his other writings, on Buddhism, on fengshui, on other aspects of folklore and religion, are full of anecdotes and the fruits of his day-to-day interactions. We also know from various sources, including his correspondence, that he spent much of each day while at home preaching and circulating among the Chinese who attended the mission hospital to which he was attached in Beijing.

Even stranger, perhaps, given that the kind of philology Edkins practised stressed seeking out the most ancient of texts and reconstructing the early pronunciation of characters, is his lack of interest in what the classical Chinese texts said themselves about the origins of their language. They are certainly not silent on matters of how writing was invented, how people communicated before writing, and how things came to be named. It must be observed, however, that the Chinese literary tradition always stressed the written over the oral, and speech itself appears to have been taken as a given. With the only written language in their known world, the ancient Chinese do not seem to have been much interested in comparative language studies and since Edkins's project relied on the twin pillars of spoken language and comparison, it may simply have been that the ancient Chinese texts were simply answering different questions from the ones he was asking.

Comparative studies of all kinds on the scale that Edkins undertook, especially the comparative study of languages, are particularly notable for including in their purview both the language (or mythology, or religion, etc.) of the observed people, or peoples, and the language (or whatever) of the observer. Thus, in Edkins's study the Chinese language and the European languages stand at each end of the scheme he sets out of the unrolling of linguistic history. To be sure, the European languages are seen to be the last group to have evolved but they are not, as I explained earlier, regarded as the most complex or most perfect of linguistic creations. By including his own language and Chinese in the same scheme, Edkins's model, and indeed comparative philology

as a discipline, can be seen both as relativizing the language of the analyst and granting the language of study a degree of respect. On the other hand, with the move to the study of single languages and societies at the end of the nineteenth century, and the decline of this kind of comparative study, the scholar became removed from the object of research. The Chinese became discursively disconnected, if not from the rest of the world, certainly from Europe and the West.

With this kind of model — us here and them over there — there developed a sense that we inhabited discrete worlds and ways of being. And from this, perhaps, developed an anxiety that something needed to be crossed to get from one to the other; a psychic metaphor of the vast Eurasian steppe. Nineteenth-century missionary writings on China in English certainly display anxieties on the part of their authors but those anxieties do not, in my reading, appear to include the sense that no matter how hard we try we will never truly understand the Chinese mind. “East is east and west is west and never the twain will meet” is a notion surprisingly absent in this context. It is absent, I would suggest, because these were people of religion, something we must take seriously if we are to approach an understanding of the encounter between Chinese people and Westerners before our times. Edkins and others like him knew exactly what they were doing in China and why they were there. We may not approve of what they were trying to achieve but there is little doubt that the only meaningful thing that divided Europeans and Chinese was that we were Christian and, by and large, they were not — yet.

ENDNOTES

¹ Edkins, Rev. J, *China's Place in Philology: An Attempt to Show that the Languages of Europe and Asia have a Common Origin* (London: Trubner, 1871).

² For an outline of the multitude of missions, see Latourette, K.S., *A History of Christian Missions in China* (London: Society for Promoting Christian Knowledge, 1929).

³ Teng, Ssu-yü and J.K. Fairbank, with E-tu Zen Sun, Chaoying Fang and others, *China's Response to the West; a Documentary Survey, 1839–1923* (Cambridge, Mass.: Harvard University Press, 1954, reprinted until at least 1979).

⁴ On Legge, see Girardot, N., *The Victorian Translation of China: James Legge's Oriental Progress* (Berkeley: University of California Press, 2002), and numerous papers by Laurence Pfister. On Edkins, see my “Meeting the Celestial Master”, *East Asian History*, 15/16, June/December pp.53–66.

⁵ Boerschmann, Ernst, *Chinese Architecture and its Relation to Chinese Culture* (Washington: Govt. Print. Office, 1912). This was an offprint from the Smithsonian report for 1911.

⁶ Price, Maurice, *Christian Missions and Oriental Civilizations, a Study in Culture Contact; the Reactions of Non-Christian Peoples to Protestant Missions from the Standpoint of Individual and Group Behaviour: Outline, Materials, Problems, and Tentative Interpretations* (Shanghai: privately printed, 1924).

⁷ Williams, Raymond, *Keywords* (London: Fontana, 1983), pp.57–8, italics in the original.

⁸ On Chinese punishments, see Mason, G.H., *The Punishments of China, illustrated by twenty-two engravings: with explanations in English and French* (London :W. Miller, 1801). Although he did not use the word “civilized” or its equivalents it is worth noting that Leibniz ranked China in advance of Europe itself in areas of human relations at the very end of the seventeenth century. See, for instance, in his “Preface to the *NOVISSIMA SINICA*” (trans. Daniel J. Cook and Henry Rosemount, Jr.), in Cook and Rosemount, *Gottfried Wilhelm Leibniz: Writings on China* (Chicago: Open Court, 1994), pp.46–7: “...who would have believed that there is on earth a people who, though we are in our view so very advanced in every branch of behaviour, still surpass us in comprehending the precepts of civil life?...certainly they surpass us (though it is also shameful to confess this) in practical philosophy, that is, in the precepts of ethics and politics adapted to the present life and use of morals. Indeed it is difficult to describe how beautifully all the laws of the Chinese, in contrast to those of other peoples, are

directed to the achievement of public tranquillity and the establishment of social order..."

⁹ Barrow, Sir John, *Travels in China: Containing Descriptions, Observations and Comparisons Made and Collected in the Course of a Short Residence at the Imperial Palace of Yuen-min-yuen, and on a Subsequent Journey from Peking to Canton. In which it is Attempted to Appreciate the Rank that this Extraordinary Empire may be Considered to Hold in the Scale of Civilized Nations* (London: T. Cadell and W. Davis, 1804), pp.3–4. Underlining in the original.

¹⁰ Ibid, pp.28–9.

¹¹ Ibid, pp.50–1.

¹² Ibid, p.29.

¹³ This is not to say that Barrow does not dabble in racial theory of a more egregious sort. In a bizarre passage he cites his own *Travels into the Interior of South Africa* of 1802 (a journey he made after returning from China) opining that the structure of the upper lid of the eye of "a real Hottentot" was just like that of a Chinese and, in general, "their physical characters agree in almost every point". Recalling "a Hottentot who attended me," he claims this man was "so very like a Chinese servant I had in Canton, both in person, features, manners, and tone of voice, that almost always inadvertently I called him by the name of the latter": Ibid, pp.48–9.

¹⁴ Medhurst, W.H., *China: its State and Prospects, with Especial Reference to the Spread of the Gospel, Containing Allusions to the Antiquity, Extent, Population, Civilization, Literature, and Religion of the Chinese* (London: J. Snow, 1840).

¹⁵ Medhurst, W.H., *China: Its State and Prospects*, pp.97–8. It is worth noting in this context that also like Barrow, Medhurst points out that "China possesses as much civilization as Turkey now, or England a few centuries ago" and that the Chinese are exaggerated in their self-assessment: "They denominate China 'the flowery nation,' — 'the region of eternal summer,' — 'the land of the sages,' — 'the celestial empire,' — while they unscrupulously term all foreigners 'barbarians,' and sometimes load them with epithets still more degrading and contemptuous, such as swine, monkeys, and devils." (p.98) He concludes with a discussion of the advantages of attempting evangelization in "civilized nations" rather than in those "altogether barbarous": in the latter case he notes, "Instances have occurred of savage tribes falling upon the messengers of mercy; and, immediately on their arrival, proceeding to plunder, murder, and, even eat them. But this is not likely to occur among a people, in a great measure, civilized" (p.120).

¹⁶ Williams, *Keywords*, p.59.

¹⁷ Laffitte, Pierre (trans. John Carey Hall), *A General View of Chinese Civilization and of the Relations of the*

West with China (London: Trübner and Co; Yokohama, Shanghai & Hong Kong: Kelly & Walsh; Tokyo & Yokohama: Z.P. Maruya & Co., 1887).

¹⁸ Edkins, *China's Place in Philology*, p.xii.

¹⁹ Müller, M., Letter to A.P. Stanley, quoted in Girardot, *The Victorian Translation of China*, p.245.

²⁰ Bushell, S.W., "Obituary Rev Joseph Edkins, D.D.", *Journal of the Royal Asiatic Society* (January, 1906), pp.269–71, Box, Rev. E., "In Memorium, Rev. Joseph Edkins, D.D.", *The Chinese Recorder*, 36 (June, 1905), pp.282–9 (see also "Editorial Comment" in the May 1905 issue, pp.261–2), Anon., "In Memorium Rev. Joseph Edkins, D.D.", *Journal of the China Branch of the Royal Asiatic Society*, vol 36 (1905), pp.157–9, Cordier, H., "Nécrologie Joseph Edkins", *T'oung Pao*, VI (July, 1905), pp.359–66. See also, *Memorials of Protestant Missionaries to the Chinese: giving a list of their publication, and obituary notices of the deceased with copious indexes* (Shanghai: American Presbyterian Mission Press, 1867, reprinted Taipei: Ch'eng-wen Publishing Company, 1967), pp.187–91.

²¹ Edkins, letters to Rev J.J. Freeman (13.10.1848) and to Rev. Arthur Tidman (14.1.1849, 13.7.1849, 11.4.1850, 11.6.1850) in the Archives of the Council for World Mission: Central China, London University, SOAS.

²² Edkins, Jane, *Chinese Scenes and People, with Notices of Christian Missions and Missionary Life in a Series of Letters from Various Parts of China, with a Narrative of a Visit to Nanking by Her Husband the Rev. J. Edkins, also a Memoir by her Father, the Rev. W. Stobbs* (London: J. Nisbet, 1863). The excerpt is quoted in Box's obituary of Joseph, p.284.

²³ Edkins, letters to Tidman (6.9.1862, 11.4.1863, 25.5.1863), Council for World Mission: North China, London University, SOAS.

²⁴ Edkins, letters to Tidman (12.9.1865) letters to Mullens (27.2.1866, 14.11.1867, 28.12.1877), Meech to Whitehouse (22.9.1879), Council for World Mission: North China.

²⁵ Edkins, letter to Whitehouse (13.12.1880), letters to Thompson (21.12.1880, 1.2.1881), Council for World Mission: North China.

²⁶ Box, Obituary, p.289. See also the editorial comment from *The Chinese Recorder*: "We said it was with mingled feelings that we write of his death. While his place will be vacant here and his presence missed, yet when one, like this, is gathered in as a shock of corn, fully ripe, when the streets of toil are changed for the streets of gold, when the mortal puts on immortality, one cannot refrain from a feeling of sympathy with the joy of one who has gone up higher, who has stepped across the border and sees his Master face to face."

²⁷ Edkins, J., *Chinese Buddhism: a Volume of Sketches, Historical, Descriptive and Critical* (London: Trubner & Co., 1880).

²⁸ Bushell, "Obituary," p.270.

²⁹ Anon, "In Memorium," pp.158–9. It is probably scholars such as the one that wrote this obituary that Box was referring to when he wrote, "[Edkins's] two pet aversions (and I believe his only aversions) were the Higher Critics and those Philologists who declined to accept his theories on words, their origin and connection with each other. He rightly, I think, applied the laws of evolution to language, but his methods, I must confess, went beyond the limits of my poor comprehension." ("Obituary," p. 288).

³⁰ Bopp, Franz (trans. Edward B. Eastwick), *A Comparative Grammar of the Sanskrit, Zend, Greek, Latin, Lithuanian, Gothic, German, and Slavonic Languages* (London, Madden & Malcolm; James Malcolm: London, 1845–50)

³¹ See, Rachel Ramsey, "China and the Ideal of Order in John Webb's 'An Historical Essay...'" *Journal of the History of Ideas* 62, 3 (July 2001), 483–503.

³² Edkins, *China's Place in Philology*, p.xx.

³³ Müller, M., *Lectures on the Science of Language*, quoted in Harris, R. and Taylor, T.J., *Landmarks in Linguistic Thought: The Western Tradition from Socrates to Saussure* (London: Routledge, 1989), p.166.

³⁴ Edkins, *China's Place in Philology*, p.1

³⁵ *Ibid*, p.2

³⁶ *Ibid*, p.30. Job's condemnation can be found at Job 31:26–28: "If I beheld the sun when it shined, or the moon walking in brightness; And my heart hath been secretly enticed, or my mouth hath kissed my hand: This also were an iniquity to be punished by the judge: for I should have denied the God that is above."

³⁷ *Ibid*, p.31.

³⁸ *Ibid*, p.34.

³⁹ *Ibid*, p.51.

⁴⁰ *Ibid*, p.55.

⁴¹ Genesis 2:19.

⁴² 65, On the Atonement, Dissert. 53. This is likely to be William Magee, successively Bishop of Raphoe and Archbishop of Dublin, *Discourses & Dissertations on the Scriptural Doctrines of Atonement & Sacrifice: and on the Principal Arguments Advanced, and the Mode of Reasoning Employed, by the Opponents of those Doctrines as Held by the Established Church: with an Appendix Containing some Strictures on Mr. Belsham's account of the Unitarian Scheme, in his Review of Mr. Wilberforce's Treatise* (London, 1801).

⁴³ Edkins seems generally to follow the chronology of Dr. William Hales (1778–1821) found in his *A New Analysis of Chronology: in which an Attempt is Made to Explain the History and Antiquities of the Primitive Nations of the World, and the Prophecies Relating to them, on Principles Tending to Remove the Imperfection and Discordance of Preceding Systems*, 3 vol. (London, 1809–12). Hales followed the Septuagint text, unlike the more-famous chronology of Archbishop James Ussher (dating Creation to 4004 B.C) who based his work on the Masoretic text.

⁴⁴ Edkins, *China's Place in Philology*, p.67–8.

⁴⁵ Genesis 4:16.

⁴⁶ Edkins, *China's Place in Philology*, p.68.

⁴⁷ *Ibid*, p.395.

⁴⁸ Edkins, "Chinese Philology", *The China Review* 1, 3 (1872), 181–90, 1, 5 (1873), 293–300, pp.181–2.

THE RISE AND FALL — AND POTENTIAL RESURGENCE — OF THE COMPARATIVE METHOD, WITH SPECIAL REFERENCE TO ANTHROPOLOGY

HENRIKA KUKLICK

INTELLECTUAL RESISTANCE TO COMPARISONS

Each of us has his or her standard academic questions, asked with tedious regularity of the presenters of papers at scholarly meetings. No names need be mentioned, but we all know a person whose usual ploy is to present a speaker with a summary of her paper, ask her if the summary is correct, and then ask for clarification of a point or two after being praised for his accurate summary. We also all know a person whose level of antisocial behaviour is tolerated in few places outside the seminar room, who will ask a speaker some variant of the question, "Why have I been obliged to listen to your stream of sentences that make no evident sense?" My own standard question is, "Compared to what?"

My point is that a comparative approach is always an option for me and for others of like mind. The sorts of papers most likely to provoke my question are those that consider a development (any development) in a specific national context, without any consideration of the possibility that the causal factors assumed

to be critical in said national context may not be so. For example, historians of the United States have of late devoted some attention to the baseline assumption of the existence of "American exceptionalism", which informs a good deal of American scholarship.¹ The ingredients of the American exceptionalist argument that the United States has had a history unlike any other nation-state have been few and straightforward: the United States has been distinctive because it has had no hereditary aristocracy to impede upward social mobility of individuals who earn their leadership positions; the culture of the United States has been formed in a "melting pot" (or sometimes, of late, in a "salad bowl"), in which diverse traits of a population of immigrants have been blended in a fortuitous mixture of a congeries of elements gathered from everywhere; and an abundance of land on which the geographical frontier receded but (notwithstanding official pronouncements) never really closed has provided recurrent opportunities for ambitious individuals and social innovation. At the moment, in the popular version of the American exceptionalist argument, there is a fair amount of conversation that suggests that the

United States is a nation uniquely guided and favoured by an interventionist Christian deity. Indeed, as historians of the development of the discipline of American history have informed us, employment as an academic historian was once contingent on professing this view.² Perhaps international developments will cause this xenophobic line to moderate (if not disappear altogether) in popular discourse in the future.

What's wrong with thinking that there is something exceptional about the thesis of American exceptionalism? Never mind its unwarranted message that the United States has been free from class-based social strife. We also easily recognize that its implicit assumption that the United States enjoys special status in the eye of God is found in other national histories. And comparison with the standard histories of other nation-states reveals many narrative elements that are similar in their particulars. For example, for more than two centuries Russians have argued (not without some apparent justification) that they have had in Siberia a frontier conducive to freedom and innovation.³ Histories of other white-settler societies — such as South Africa and Australia — have pointed to factors very similar to those invoked in many histories of the United States (especially those written for schoolchildren); white-settler narratives commonly celebrate egalitarian styles of personal relationships and unfettered opportunities for upward mobility — although, of course, the populations to which these generalizations apply have been implicitly understood to be males of European extraction — and Western (or at least Central) European extraction at that.⁴ Indeed, school textbook histories everywhere seem

generically designed to impart pride in unique national virtues, and efforts to modify the moral lessons they convey invariably provoke controversies.⁵ In the versions of history taught to schoolchildren, if not also in writings of many professional historians, virtually every nation's history is an exceptionalist one.

THE SPIRIT OF THE AGE

By contrast, however, comparisons among nations seem to have been relatively frequent in the nineteenth century, when it was assumed that national histories fit general patterns. In Britain, for example, contemporary Britons and ancient Phoenicians were frequently equated. As Alexander Wilmot wrote in 1896:

[F]rom the fourteenth to the fourth century before Christ the Phoenicians...sent forth the most daring and successful fleets and colonies of antiquity...Their small territory required outlets for a redundant population... In all history there is no greater analogy than that between the Empire of Britain and that of Phoenicia at its culminating point of glory...⁶

In his introduction to Wilmot's book, the novelist H. Rider Haggard wrote that the Phoenicians were "this crafty, heartless and adventurous race...the English of the ancient world without the English honour".⁷ Indeed, the British-Phoenician analogy was a commonplace in continental Europe throughout the nineteenth century. Moreover, the Phoenicians were not the only ancient peoples to whom Britons once compared themselves. Pondering the condition of their empire, they attempted

to explain why Rome fell, and wondered how to avoid replicating Rome's errors. Considering ancient Greece, they weighed the merits of the social organizations of Athens and Sparta.⁸ (It is of interest that some academics of British origin who have taken positions in the United States, such as Paul Kennedy and Niall Ferguson, have embraced a similarly moral approach to historical analysis, preaching the lessons to be learned from Britain's imperial experience to the citizens of what is now an American empire in all but name.)

There is one obvious reason that Britain's educated classes were once wont to compare themselves with the classical ancients: they were educated in the classics. But this explanation is only a partial one. If one can speak in the intellectually vague term of the *zeitgeist* (and sometimes one must), the nineteenth century was a historically self-conscious age, aware that there was inevitably a "spirit of the age", in John Stuart Mill's phrase.⁹ And one can only understand the development of the comparative method in anthropology if one sees it as the product of an age with historicist sensibilities. Among the most important questions practitioners of this method asked was: Was it possible to achieve better understanding of the classical ancients who were responsible for laying the basis of western civilization by examining then-contemporary non-western peoples, who were presumed to be in at least some particulars analogous to the ancients?

I will discuss the comparative method in anthropology largely with reference to its use by practitioners who fell within the British sphere of influence. This is not because its practice was restricted to that

sphere. The greatest of nineteenth-century American anthropologists, Lewis Henry Morgan (1818–81), worked in a style that resembled nineteenth-century British anthropologists, although he was not an "armchair" scholar in the mould of his British contemporaries; they were able to achieve eminence in anthropological circles without leaving the comforts of home, drawing on information collected by others to produce their generalizations.¹⁰ Morgan was something of a field naturalist, so that direct observation informed his analysis of Native Americans (and of beavers); arguably, he also reasoned with greater intellectual rigour and used more elaborate documentation than was employed by most British scholars; the informants who imparted their knowledge of exotic peoples both to Morgan and to British armchair anthropologists were apt to be rather contemptuous of the generalizations produced by the latter.¹¹ And the greatest of late-nineteenth/early-twentieth century French social scientists, Émile Durkheim (1858–1917) — an armchair scholar whose works are still considered relevant to contemporary disciplinary inquiries by anthropologists, sociologists, and others¹² — also practised a variant of the comparative method. Certainly, differences obtained among practitioners of this method in different national contexts. But the case of British proto-anthropologists who used the comparative method should serve as generally illustrative.

THE SOCIAL STRUCTURE OF COMPARATIVE RESEARCH

The comparative method in nineteenth-century anthropology was born of four

elements, none of which was peculiar to anthropology: one, an approach to collecting information; two, an established style of dividing intellectual labour; three, a methodological orientation; four, an aesthetic of generalization and explanation.

Collecting information: There was a standard way to collect information about any given specific object, creature, or practice that was found in many places — the questionnaire. The most important of anthropologists' questionnaires was the volume jointly published by the British Association for the Advancement of Science and the Royal Anthropological Institute at irregular intervals from 1874 to 1951, *Notes and Queries on Anthropology* (it was originally entitled *Notes and Queries on Anthropology for the Use of Travellers and Residents in Uncivilized Lands*).¹³ This was not the only anthropological questionnaire circulated; individuals such as J.G. Frazer, for example, drew up and distributed their own questionnaires. But it is important to recognize that *Notes and Queries on Anthropology* merely elaborated a general form. This had taken shape no later than the late seventeenth century, when some Fellows of the Royal Society printed and circulated a questionnaire to elicit information about the natural and built features of the environments of England and Wales.¹⁴

Consider the periodical entitled simply *Notes and Queries*, a vehicle for inquiring minds who were curious about virtually anything. On March 18, 1854, for example, it published a query from a reader who seemed bent on acquiring confirmation of his judgement that mackerel were blind (his explanation for their lack of interest in the flies he cast in the waters in which

he wished to catch them).¹⁵ On November 28, 1857, *Notes and Queries* published a request for information about the career of a small child who had at some time been exhibited in London, in the irises of whose eyes was said to be visible the name "Emperor Napoleon".¹⁶ And on February 1, 1862, it published readers' responses to a question about the number of societies past and present in which human corpses were ceremonially buried in the foetal position. One of these responses came from Sir John Lubbock (a baronet, later Lord Avebury), a scientific polymath with special expertise in insects, who knew Charles Darwin as a neighbour, family friend and intellectual mentor, and who was a long-serving member of Parliament as well as a wealthy banker. Lubbock's many accomplishments included being the first president of the Anthropological Institute of Great Britain and Ireland (later the Royal Anthropological Institute), formed in 1871 from the union of the previously warring Ethnological Society of London and the Anthropological Society of London.¹⁷ That Lubbock was among the readers of *Notes and Queries*, along with the incompetent mackerel fisherman, surely indicates that this publication reached a broad audience.

Then, there existed the informal questionnaire, such as the letters of inquiry that Charles Darwin mailed to his global network of naturalist-informants, without whose assistance he could never have gathered the wealth of data from which he produced such works as the *On the Origin of Species* in 1859.¹⁸ Last, but hardly least, there were information-gathering kits, such as those for collecting insects that would-be purchasers of specimens distributed to travellers to foreign

parts (such as naval officers and traders); these included operating instructions, as well as the materials necessary to kill, preserve and store insect specimens without mutilating them.¹⁹ Of course, individual scholars supplemented the material they gleaned from systematic inquiries with information they happened upon in various ways, but it is the existence of a variety of routinized forms of deliberate solicitation of information that is significant.

Division of labour: There was a clear division of labour between collectors of information and analysts thereof, which represented a division along class lines — roughly, the division between players and gentlemen, respectively. Those who received compensation for their scientific activities, as collectors often did, were not considered capable of judging evidence dispassionately — and also were not, in any event, “clubbable”, almost invariably being considered unfit for leadership roles in the societies of enthusiastic amateurs who dominated many spheres of British scientific life until the twentieth century. Consider the case of Paul du Chaillu, a French-born traveller to equatorial Africa in the middle of the nineteenth century (who had various financial supporters, including, for example, the Academy of Natural Sciences in Philadelphia). When he made public appearances in London, his descriptions were greeted with scepticism and he himself was widely believed to be something of a cad: his reports of African peoples and their natural environment, which included the first eyewitness observations of the gorilla, were accepted only after authentication by gentlemen-scientists. The gentlemen-scientists had no empirical basis for their judgements,

basing them on their assessments of du Chaillu’s character. Evidently, their high social status counted for more in scientific circles than du Chaillu’s research experience.²⁰

I must emphasize that to report this standard division of labour is not to suggest that we must identify with the view of the gentlemen-amateurs, or that we must believe that the collectors who served gentlemen-scientists regarded their role as that of mere servants. As Anne Secord has ingeniously documented, working-class collectors who supplied nineteenth-century gentlemen-botanists with specimens had considerable scientific expertise, as well as genuine commitment to scientific inquiry.²¹ But no matter what were the satisfactions working-class collectors derived from their scientific labours, their voices were not audible in prestigious scientific circles.

Methodological orientation: The collection of evidence from thither and yon somehow had to be rationalized as a reasonable procedure. Intellectual historians must begin with the working principle that the figures we study were at least as smart as we are, and so we should not be surprised to learn that many nineteenth-century anthropologists were aware that their informants were not presenting them with what the philosopher of science calls “brute facts” — as she customarily does, immediately prior to declaring that there are no such things. At least some of anthropologists’ disciplinary ancestors recognised that, as the philosopher Mary Hesse has memorably stated, theories are always “underdetermined by facts” — a point especially relevant in accounts of paradigm change (and to which we will return).²²

In 1870, for example, John Lubbock observed that the particular perspectives of individuals shaped their reports, saying, "Whether any given writer praises or blames a particular race, depends at least as much on the character of the writer as on that of the people."²³ Indeed, attention to observer bias had a considerable lineage in science, dating at least to the astronomers' definition of the so-called personal equation in the 1820s, a phenomenon of which Sir John was surely aware, since his father was a distinguished astronomer.²⁴ That is, by the end of the eighteenth century, astronomers had noted that individual observers varied in their reaction times to, say, the transit of a star, and astronomers subsequently undertook to calculate patterns of variations among individuals, specifying individuals' differences, so as to achieve inter-subjective measures by which vital matters such as the setting of clocks could be resolved.²⁵

It is no accident, to use historians' once-canonical locution, that Francis Galton delivered descriptions of some of his most important statistical innovations to the Anthropological Institute, of which he was President from 1885 to 1888, as he did when he described the normal frequency distribution (also known as the bell curve) which should be observable for any variable, ranging from, say, height to life expectancy; he intended his statistics to be useful in describing the phenomena anthropologists examined, including cultural and biological traits. In his history of the development of statistics in Britain, Donald MacKenzie emphasizes that Galton and others, most notably Karl Pearson, who developed such statistical measures still in use as the correlation coefficient, were motivated by their desire to docu-

ment patterns of human heredity so that they could promulgate eugenic strategies for improving the physical and mental condition of the British population.²⁶ But Galton was surely aware of anthropologists' problem in reconciling disparate reports in order to formulate generalizations. And if he did not say explicitly that he expected his statistical techniques to solve anthropologists' *methodological* problem, another sometime president of the Anthropological Institute, E. W. Brabrook, did, stating that anthropologists need not trouble themselves with inconsistencies in the reports they received from "every direction", but could trust "rather to the general laws of numbers than to the skill of individuals to eliminate errors".²⁷

An aesthetic of generalization: The comparative method in anthropology was born in an era in which it was assumed that all satisfactory explanations were historical ones. It was not merely the case that historical analogies were frequently made, and history examined for its moral lessons, as I have already discussed. The general idea of evolution, once called "transformism", antedated Darwin's conceptualization of it and was applied to description of various natural phenomena. Consider the practice of embryology in the last quarter of the nineteenth century, when it was a highly prestigious area of research in the biological sciences. Embryological research was animated by the recapitulation hypothesis, which could be applied more or less strictly. Compatible with Darwinism (although also reconcilable with other developmental schemes), this was the idea that ontogeny recapitulated phylogeny, that the development of each individual recapitulated the history of the development of the species to which

it belonged. Thus, examination of embryonic growth patterns was expected to yield answers to the fundamental questions of species' evolution. For example, a development in an embryo that proved just a transient phase in the progress of an organism to its infant stage could indicate whether *degeneration* was just as important a feature of its evolutionary history as *progress* — a possibility much debated at the end of the century. (In the developmental scheme so influentially promulgated by Herbert Spencer in the late-nineteenth century, progress generally meant a movement from simplicity to complexity.)²⁸ To give another example of the use of the recapitulation hypothesis, Sigmund Freud's Lamarckian explanation of the structure of the individual psyche conjured up real human experiences in the remote historical past that would be recapitulated in childhood maturation — which could constitute degeneration (or at least arrested development) if the full course of the species' development was not followed.²⁹

In the first decades of the twentieth century, all manner of varieties of historical explanations would be dismissed *as such*, as expressions of what philosophers termed the “genetic fallacy” — a pejorative phrase expressing the idea that the origins of any given institution in the past had no necessary relevance to understanding the operations of that institution in the present. Nowadays, facile dismissals of assertions as expressions of the genetic fallacy are rarely heard, although we can easily conjure up illustrations of arguments that might be dismissed as based on the genetic fallacy. For example, we should not devalue Galton and Pearson's statistics because the two men were motiv-

ated by eugenic objectives, since their statistics can be used in all manner of research projects. At the same time, we can also see that the historical antecedents of some present practices may be meaningful in contemporary contexts. And historical explanations are back in fashion. Regardless, appeals to historical explanations are themselves historical phenomena; they have seemed more plausible in some eras than in others.

THE COMPARATIVE ANTHROPOLOGICAL PROJECT

What kinds of questions did anthropologists hope to answer with their comparative method? They wanted to trace the development of institutions, such as replacement of the practice of tracing descent from the mother by the supposedly superior practice of tracing descent from the father. Consider what was arguably the single most important article published by E.B. Tylor, whose appointment as Reader in Anthropology at Oxford in 1884 (the first such position in Britain)³⁰ represented a decisive shift toward the professionalization of the subject. Published in 1888, Tylor's article, “On a Method of Investigating the Development of Institutions, Applied to Laws of Marriage and Descent”, used information gathered about societies all over the world, to which societies Tylor assigned grades on a unilinear scale of evolution, analyzing data with a method he called adhesions” (which we would call “correlations”). Tylor thought he could thus determine how transitions from one stage of evolution to another were effected; his most notable finding was that the practice of *couvade* — in which the father of a child apparently

suffers birth pangs during the course of its birth, and is sympathetically attended — denoted progress toward creation of family structures in which fathers assumed their appropriate responsibilities.³¹

Anthropologists also wanted to determine what relationship obtained between the biological and social development of the human species. Consider the resolution of the problem first raised in 1858 by William Gladstone, the future prime minister: How should one interpret the recurrent use of such descriptions as “wine-dark sea” in Homer? Perhaps it was legitimate to analogize ancient Greeks to contemporary primitives — to assume that all populations negotiated an identical course of *biological* as well as *social* development, and that the ancient Greeks’ perceptual sensibilities had not yet matured to the degree observed among modern peoples. Perhaps ancient Greeks really could not distinguish between the colours of the sea and of wine, a conjecture supposedly confirmed by babies’ apparent initial preference for red over blue (remember that the development of babies’ sensibilities supposedly recapitulated the maturation pattern of the entire human species). This argument was not discredited until W.H.R. Rivers put it to empirical test during the 1898 Cambridge Anthropological Expedition to Torres Straits, which was organized by the man who would occupy Cambridge University’s first position in sociocultural anthropology, A.C. Haddon.³² The Torres Straits expedition was the first venture to take British scientists into the field to do their own anthropological research, and it afforded Rivers the opportunity to observe that the islanders had no difficulty seeing blue — although it was not their favourite colour.³³

Or, anthropologists wanted to use empirical evidence to chart the emergence of superior morality and spirituality — and to compel recognition of “survivals” of earlier, irrational and immoral times — “survivals” which could then be deliberately eliminated. Theirs was anthropology as the “reformer’s science”, as E.B. Tylor often spoke of it. And self-understanding and consequent self-conscious reform was J.G. Frazer’s objective when he disseminated his interpretation of Baldwin Spencer and F.J. Gillen’s 1899 classic *Native Tribes of Central Australia* — Australia’s pioneering contribution to the development of anthropological field-research method — even though, as it happened, he disagreed with Tylor in his interpretation of Spencer and Gillen’s findings. Understanding Ar-rernte ignorance of the facts of procreation and their totem ceremonies to be analogous to Christian belief in the virgin birth of Jesus and the ceremony of the Eucharist, Frazer believed that he had exposed survivals of truly primitive habits, persisting in what was a supposedly rational age.³⁴ I will shortly return to consideration of the international anthropological controversy that Frazer’s views provoked.

Clearly, the unilinear evolutionist model that relied on the comparative method was not really historical but historicist — or, at least, historicist in the pejorative sense. Cross-cultural research was used by anthropologists to document the assumption that differences between societies were matters of degree rather than of kind; that some societies had simply advanced further along the teleological trajectory of human progress than others. Why was this research mode abandoned? It would be easy to say that anthropologists dismissed their model and method

rationally because these were proven defective. Reasoned judgement was not irrelevant to intellectual change, but we should briefly pause in our consideration of anthropology's paradigm shift to ask, "Compared to what?"

Consider the case of economists, political scientists and sociologists who have attempted to formulate guidelines for modernizing non-western societies. Among these, the recapitulation hypothesis remained respectable for roughly half a century longer than it did for anthropologists. In fact, one could say that anthropology differentiated itself from other social science disciplines by being first on the mark to reject this hypothesis — and that "modernization" theory ceased to be respectable as much in consequence of ridicule animated by political concerns as anything else.³⁵ Or consider the contemporary primatologists whose projects represent a residue of the objective that anthropologists abandoned when they jettisoned the recapitulation hypothesis — scientists who imagine that they can reconstruct the behaviour of earliest humankind on the basis of their observations, divided though they may be by certain disputes. Which particular primate species constitutes the best prototype of humans' ancestors? What sorts of observations are reliable — in the laboratory or in the field? If the observer is in the field, should she make the ground her vantage point or can she see natural behaviour while being an intrusive presence perched in a Land Rover? Where is the demarcation boundary between humans and other primates if the latter can be taught to communicate in some form of language? Likewise, what does it mean to be human if primates have been tool-users and have even developed

culture?³⁶ The historian of nineteenth-century anthropology feels weary as she watches intellectual history repeating itself among primatologists.

THE DECLINE OF THE COMPARATIVE METHOD

But I return to the history of anthropology within the British sphere of influence. It reveals widespread dissatisfaction with the theory and method required for comparative analysis by the end of the nineteenth century. In the 1892 edition of *Notes and Queries on Anthropology*, for example, C.H. Read observed that the passing traveller could not obtain "even superficial answers" to the questions anthropologists wanted answered; only persons with "long-continued residence among a native race" were trustworthy informants.³⁷ And when in 1902 A.C. Haddon described Spencer and Gillen's *Native Tribes of Central Australia* as "the best book of its kind about any people", he signalled the claims to authority of a new style of anthropologist, persons such as Baldwin Spencer and himself, trained scientists, whose specialized observational skills were more reliable (and more rapid) means to collect accurate information than the intellectual habits formed during "long continued residence among a native race", and whose primary task was to accumulate detailed knowledge of delimited areas — which might be, but need not be, used in comparative analyses.³⁸ Authoritative judgements entailed personal experience of field research among the peoples the anthropologist wished to describe — periods of research that became possible with the development of academic careers, such as Haddon and Spencer enjoyed.

Field trips of a year or more would become the anthropological gold standard in the twentieth century, when anthropologists had better opportunities for financing them with the patronage of private philanthropies and government agencies, but in earlier times it became possible to do fieldwork for the simple reason that academic lives were punctuated by long breaks.³⁹ Much of the research for *Native Tribes* was done during Spencer's 1896–97 summer vacation from his position as Professor of Biology at the University of Melbourne, when he was able to work with Gillen in Central Australia. There, Gillen, a civil servant who was given leave to work with Spencer, was the effective administrator of the area's Aborigines, and had accumulated a good deal of ethnographic knowledge during two decades' residence.

Haddon urged researchers to understand exotic peoples from the "native point of view", an injunction echoed by Spencer and Gillen when they argued that anthropologists must enter "into the mental attitude of the native". Though the "genealogical method" Rivers developed for the Torres Straits expedition proved remarkably durable, however, Haddon's venture did not provide an imitable model. Taking seven men to spend roughly seven months living for periods of variable length on one or another portion of an island cluster, dividing investigative labour among themselves, the Torres Straits expedition constituted a hybrid genre of anthropology. The six volumes of Reports Haddon edited — five published before World War I, and the last (Volume I) in 1935 — received largely favourable reviews, but they were, in part, exercises in old-style research, relying on evidence

that Haddon solicited from afar to supplement the information his team had gathered in the field. Moreover, while teamwork remained common in natural scientific practice (and one could say that practitioners of the comparative method were members of informally constituted teams), teamwork did not become anthropology's ideal method. Rather, Spencer and Gillen's *Native Tribes* became an exemplar. It represented an approximation of the anthropological method that would soon be conventional: a comprehensive study of a delimited area, based on sustained fieldwork conducted by one or two people (if the latter, often a husband and wife), portraying a population's distinctive character.

The significance of *Native Tribes* in the era of its publication was rather different, however. In 1913, for example, Bronislaw Malinowski said of Spencer and Gillen's studies that "half the total production in anthropological theory ha[d] been based upon their work, and nine-tenths affected or modified by it". *Native Tribes* inspired an intense international debate, arguably the most international of controversies in anthropology's history. As E. Sidney Hartland had observed in 1900, so pervasive was the debate that the "quiet non-combatant student" was "astonished to find himself in the theatre of war", assaulted by "jarring theories and conflicting claims", and "searching in vain" for "a bomb-proof burrow". The debate was framed by J.G. Frazer (broker of the book's publication by Macmillan). It was predicated on the assumption that indigenous Australians were the most primitive of living peoples, whose totemism was (somehow) at the base of civilization's highest achievements — monogamous

marriage and truly spiritual religion. Sustained with intensity through the mid-1920s, the debate proved irresolvable in Frazer's terms. Pondering conflicting interpretations of totemism, anthropologists rejected unilinear models of social evolution like Frazer's; if nothing else, they agreed that it was impossible to grade peoples according to the *rate* at which they were making progress from savagery to civilization because evolution everywhere did not fit a single pattern.

In the early twentieth century, bounded populations of professional anthropologists emerged, developing various theoretical schemes; international intellectual exchanges were impeded by language barriers. Some (although certainly not all) of these anthropologists insisted that the indigenous peoples of their countries — such as those in North America and Australia — were quite distinctive, defying comparison with groups elsewhere; it is interesting to note that these included Spencer and Gillen, who lamented the absence of a special term for the Australian variety of totemism, which they considered unlike any other. Some anthropologists, particularly those in German-speaking areas, as well as those influenced in North America by the German-born and -trained Franz Boas, dedicated themselves to varieties of historical analyses, but their understanding of historical change was informed by attention to *particularities* of time and place. In the United States, for example, sociocultural anthropologists chose a truly historical approach, emphasizing the unique characteristics that distinguished peoples from one another; Boas himself decried efforts to produce profound generalizations from cross-cultural comparisons (his students would later

disagree). In sum, concerted efforts to use the comparative method to solve a problem that all participants in the debate over totemism had initially agreed was important had resulted in consensus that totemism was not a unitary phenomenon wherever it was found, that evolution did not follow a standard course, and that the comparative method was impracticable.⁴⁰

Twentieth-century British anthropologists did not abandon their predecessors' aspirations to formulate scientific generalizations. But they repudiated the notion that laws of development that obtained in all societies. Instead, they attempted to document the postulate that all societies were *fundamentally* identical, all of them sharing certain essential properties, all performing the same basic functions. And they reasoned that the point of studying so-called simple (or technologically underdeveloped) societies was not that these societies were *qualitatively different* from technologically sophisticated ones but that they were relatively simple to study, a point also made by Émile Durkheim (among others).⁴¹

That is, the functionalists who dominated British anthropology from the late 1920s to the 1960s dismissed efforts at historical explanations as nonsensical — as expressions of the genetic fallacy. Consider the 1911 pronouncement of the young Malinowski, who would shortly become a protégé of Baldwin Spencer (although he would see Spencer as his enemy within a decade):

[T]he interest of an exact scientist should focus on understanding and penetrating the mechanism and essence of social phenomena as they exist at present and are

accessible to observation, and not in order that these phenomena should serve as the riddle of a prehistoric past about which we cannot know anything empirically.⁴²

Defining the fundamental principle of functionalist anthropology as that "in every type of civilisation, every custom, material object, idea and belief fulfills some vital function...represent[ing] an indispensable part within a working whole", Malinowski said in 1926 that evolutionary progress "consist[ed] *not* in a sequence of different forms changing one into another, but in a better adaptation of an institution to its function" [emphasis mine].⁴³

A quarter-century later, Haddon and Rivers's most distinguished student, A.R. Radcliffe-Brown, who was Malinowski's (sometimes antagonistic) co-conspirator in the establishment of functionalist paramountcy, was still making similar arguments, saying that there was no point in attempting to chronicle the histories of non-literate societies because such societies did not leave reliable written records. But even if accurate histories could be plotted somehow, they would be irrelevant to the primary goal of social anthropology. As he said: "History, in the proper sense of the term, as an authentic account of the succession of events in a particular region over a particular period of time, cannot give us generalizations." He endorsed cross-cultural comparisons, however, saying that the comparative method was "one by which we pass from the particular to the general, from the general to the more general, with the end in view that we may in this way arrive at the universal, at characteristics which may be found in

different forms in all human societies".⁴⁴ In short, the functionalists' position, as articulated with exceptional clarity by Radcliffe-Brown, was to support cross-cultural research, but to suggest that whatever insights might be gleaned through use of the comparative method would not likely challenge the generalizations the anthropologist could produce from a single case study, since all societies were fundamentally alike.

But then, during the 1960s and 1970s, the functionalist model became a target of ridicule, much as the evolutionist model had been a target before it. Certainly, one might say that reports of functionalism's death have been greatly exaggerated, or, at least, that functionalist analysis persists in truncated form. No self-respecting anthropologist today would open herself to ridicule by postulating that any given society constitutes a bounded whole, in which all component parts are integrated in a mutually reinforcing system; but she will, nevertheless, describe interdependent beliefs and practices (as, indeed, she should).⁴⁵ One might also note that certain of evolutionists' assumptions were never thoroughly dispelled, either; not the least of these being that those societies which it is no longer politically correct to call "primitive" are in some sense doomed, bound to lose their idiosyncratic characteristics as they are caught up in the whirlwind of globalization; my point is that it should not be assumed that these societies experience historical change in a distinctive way.

Regardless, contemporary anthropologists' definition of their purview has changed, returning to one approximating that of the nineteenth century. Once again,

anthropologists study the entire world, not just technologically unsophisticated peoples — although the student who writes his Ph.D. thesis on homelessness in New York City, say, or on the conditions of innovation in a biotechnology company, may find herself hard-pressed to find an academic job as an anthropologist in the United States, since many anthropology departments continue to place high value on fieldwork in remote places. Interestingly, in Britain, unlike in the United States, sociocultural anthropologists who do their Ph.D. fieldwork in their own society are as likely to find academic employment as those who do research abroad.⁴⁶ But, then, our young, non-traditional anthropologist may find herself non-academic employment, as many anthropologists do nowadays, in which her anthropological skills may prove to have commercial value. Not the least of her marketable skills (and certainly not the only one) is her capacity to appreciate cross-cultural variation, an important asset in the global marketplace: consider the anthropologist whose job it is to appreciate local differences in the use of ostensibly culture-free technology, such as computers.⁴⁷ In some disciplines — say, economics, biology and physics — practical application has enhanced the discipline's prestige in the eyes of both practitioners and laypersons. Perhaps academic anthropology will now abandon the haughty disdain for applied work that it has sustained for more than half a century.

POSSIBILITIES OF THE FUTURE

Returning to academe proper, however, we observe that disciplinary boundaries are now being renegotiated in ways condu-

cive to cross-cultural, comparative analysis. Disciplinary genres are blurring. From its text alone, a reader may not be able to judge whether any given article was written by a historian or an anthropologist (or some other academic type), and practitioners of different disciplines have appropriated each other's theories and methods. Anthropology itself is becoming more international, although there are certainly distinct cleavages; British anthropologists who object to their colleagues' increasingly closer intellectual ties to Americans (formed not least because there are now many Americans employed in British departments) have looked for like-minded associates in continental Europe.⁴⁸ And over the past decades there have been widespread intellectual trends. Anthropologists — and others — have encouraged attention to the peculiarities of the local, while historians have been especially concerned to establish patterns of everyday lives among the ordinary folk of the past. Nowadays, it seems that the focus of past decades on accumulating knowledge about the peculiarities of the local has provoked a reaction; various types of scholars are now producing sweeping surveys of times and places. They are asking genuinely comparative questions about differences and similarities. But genuinely cross-cultural, comparative research poses technical difficulties. It requires not only a wealth of accumulated knowledge about a range of places but also particular skills, such as the command of a number of languages and the ability to decipher old styles of handwriting. The most promising way to do cross-cultural research may be to form collaborating teams. Thus, we may revive another structural feature of nineteenth-century

scholarship, albeit one in which the social stratification in the division of academic labour will be based on the professional standards of the modern university, rather than on the general class structure.

ENDNOTES

¹ See, for example, Dorothy Ross, *The Origins of American Social Science*, New York: Cambridge University Press, 1991, which argues that belief in American exceptionalism has been sustained by all varieties of social science practised in the United States.

² See Peter Novick, *That Noble Dream*, New York: Cambridge University Press, 1988.

³ See Mark Bassin, *Imperial Visions: Nationalist Imagination and Geographical Expansion in the Russian Far East, 1840-1865*, Cambridge: Cambridge University Press, 1999.

⁴ See, for example, Richard White, *Inventing Australia*, Sydney: George Allen and Unwin, 1981.

⁵ See, for example, Felicity Barringer "Africa's Claim to Egypt's History Grows More Insistent", *The New York Times*, "Week in Review", February 4, 1990, 6; Clyde H. Farnsworth, "Aborigine Enters History Books, 100 Years Late", *The New York Times*, August 15, 1997, A4; Ethan Bronner, "Israel's History Textbooks Replace Myths With Facts", *The New York Times*, August 14, 1999, A1, A5; Joseph Kahn, "Where's Mao? Chinese Revise History Books", *The New York Times*, September 1, 2006, A1, A6; Hassan M. Fattah, "Beirut Memo: A Nation With a Long Memory, but a Truncated History", *The New York Times*, January 10, 2007, A4.

⁶ Alexander Wilmot, *Monomotapa (Rhodesia)*, London: Greenwood Press, 1969 (facsimile of the original 1896 edition), 118.

⁷ H. Rider Haggard, in *Ibid.*, xvii.

⁸ For one exercise in documenting Britons' tendency to analogize their society to classical ancient societies, see Richard Jenkyns, *The Victorians and Ancient Greece*, Cambridge, MA: Harvard University Press, 1980.

⁹ He first used it in an article that appeared in seven parts in *The Examiner* between January and May 1831.

¹⁰ Figures prominent in anthropological circles in the latter part of the nineteenth century, such as E.B. Tylor, Francis Galton and T.H. Huxley, did undertake journeys to exotic parts when they were young men. In their day, however, high reputations in an-

thropology were not contingent on having had personal contact with the subjects of scholarly analyses, as would be the case in the twentieth century.

¹¹ For example, the Australian civil servant A.W. Howitt and his collaborator Lorimer Fison, a retired missionary, who provided information to both Morgan (whom they called "their chief") and to a number of British armchair scholars, disparaged the work of the latter. See, for example, George W. Stocking, Jr., *Victorian Anthropology*, New York: The Free Press, 1987, 236.

¹² See Karen E. Fields's preface to her new translation of Émile Durkheim, *The Elementary Forms of Religious Life*, New York: Free Press, 1995 [originally 1912], xvii-lxxiii.

¹³ For a discussion of *Notes and Queries on Anthropology*, see Stocking, *Victorian Anthropology*, 258.

¹⁴ See E.G.R. Taylor, "Robert Hooke and the Cartographical Projects of the Late Seventeenth Century (1666-1696)", *The Geographical Journal* 90 (1937): 529-40.

¹⁵ *Notes and Queries* Vol.9, No. 229 (March 18, 1854), 245.

¹⁶ *Ibid.* Vol.4, 2nd ser., No. 100 (November 18, 1857), 434.

¹⁷ *Ibid.* Vol.1, 3rd ser., No. 5 (February 1, 1862), 99.

¹⁸ On Darwin's information-gathering practices, see Janet Browne, *Charles Darwin: The Power of Place*, New York: Random House, 2002, passim.

¹⁹ On the material culture and formalized instructions for collecting natural specimens, see Anne Larsen, 'Not Since Noah: The English Scientific Zoologists and the Craft of Collecting, 1800-1840', Ph.D. Dissertation, Princeton University, 1993.

²⁰ Stuart McCook, "'It may be the truth, but it is not evidence': Paul du Chaillu and the Legitimation of Evidence in the Field Sciences" in Henrika Kuklick and Robert Kohler, eds., *Science in the Field*, issue of *Osiris* n.s.11 (1996): 177-200.

²¹ Anne Secord, "Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire", *History of Science* 32 (1994): 269-315.

²² I am, of course, referring to the dramatic changes that can occur in the theories and practices of scientific enterprises that have been called "paradigm shifts" ever since the publication of Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press, 1962.

²³ John Lubbock, *On the Origin of Civilisation and the Primitive Condition of Man*, London: Longmans, Green, 1870, 296.

²⁴ Stocking, *Victorian Anthropology*, 150.

²⁵ In the received history of experimental psychology, astronomers' recognition and calculation of variation among individual observers marks the beginning of their enterprise. See Edwin G. Boring, *A History of Experimental Psychology*, Second Edition, New York: Appleton-Century-Crofts, Inc., 1950, 31, 134–5.

²⁶ See Donald MacKenzie, *Statistics in Britain, 1865–1930*, Edinburgh: University of Edinburgh Press, 1981, 16–7.

²⁷ E.W. Brabrook, "On the Organisation of Local Anthropological Research", *Journal of the Anthropological Institute* 22 (1892): 271.

²⁸ See, for example, Adrian Desmond, *Huxley: The Devil's Disciple*, London: Michael Joseph, 1994, 90, 125–6, 184, 190–9. For a survey of various British and European notions about degeneration in the late nineteenth and early twentieth centuries, see Daniel Pick, *Faces of Degeneration*, Cambridge: Cambridge University Press, 1989.

²⁹ Freud wrote a number of implicitly Lamarckian analyses of this sort. For one example, see his *Totem and Taboo: some points of agreement between the mental lives of savages and neurotics*, translated by James Strachey, London: Routledge and Kegan Paul, 1950 [originally 1913].

³⁰ In 1896, Tylor was given a personal professorship, retiring in 1909; his successor, R.R. Marett, retired as a Reader.

³¹ E. B. Tylor, "On a Method of Investigating the Development of Institutions, Applied to Laws of Marriage and Descent", *Journal of the Anthropological Institute* 18 (1888): 245–72.

³² He was appointed University Lecturer in Ethnology in 1900; in 1909, he became a Reader, and retired as such in 1926. At the time of the expedition, he was Professor of Zoology at the Royal College of Science in Dublin.

³³ See my *The Savage Within*, New York: Cambridge University Press, 1991, esp. 146–9.

³⁴ For explication of Frazer's views, see my "'Humanity in the chrysalis stage': Indigenous Australians in the anthropological imagination, 1899–1926", *British Journal for the History of Science* 39 (2006): 535–68.

³⁵ See, for example, Michael E. Latham, *Modernization as Ideology: American Social Science and "Nation Building" in the Kennedy Era*, Chapel Hill: University of North Carolina Press, 2000.

³⁶ The pioneering study is Donna Haraway's *Primate Visions: Gender, Race, and Nature in the World of Modern Science*, New York: Routledge, 1989. For a recent survey that attends to these issues, see Robert N. Proctor, "Three Roots of Human Recency", *Current Anthropology* 44 (2003): 213–29.

³⁷ C.H. Read, "Prefatory Note" to Part II, "Ethnography", John Garson and Charles Hercules Read, eds., *Notes and Queries on Anthropology*, Second Edition, London: The Anthropological Institute, 1892, 87.

³⁸ A.C. Haddon to Baldwin Spencer, 5 May, 1902, in the Spencer Papers, Pitt Rivers Museum, Oxford, Box 1. Near-contemporaries, Spencer and Haddon were both trained as biologists, and were professional rivals as such before they became like-minded anthropologists. Haddon came second in the competition for Spencer's Melbourne chair. It is worth noting that when the natural history sciences (ranging from anthropology to zoology) differentiated at the turn of the twentieth century, they embraced a common method — the detailed study of a delimited area. For Haddon's rallying cry to embrace this method (as well as an account of the vicissitudes of his career), see my "Islands in the Pacific: Darwinian Biogeography and British Anthropology", *American Ethnologist* 23 (1996): 611–38. On the field method embraced by the natural history sciences, see my "After Ishmael: The Fieldwork Tradition and its Future" in Akhil Gupta and James Ferguson, eds., *Anthropological Locations*, Berkeley: University of California Press, 1997, 47–65. For one statement of the claim that their training made anthropologists more accurate and efficient observers of native peoples than such long-resident observers as colonial officials, see C.G. Seligman, quoted in Richard Temple, *Anthropology as a Practical Science*, London: G. Bell and Sons, Ltd., 1914, 44. Seligman participated in the Torres Straits expedition.

³⁹ This is not to say that fieldworkers such as Haddon did not require grants to finance their fieldwork, but their patrons were not very generous. They had to exercise considerable ingenuity in order to assemble sufficient funds, and might supplement their grants through personal efforts. Haddon, for example, collected ethnographic artefacts to sell to museums; see my "Islands in the Pacific", op. cit.

⁴⁰ This discussion of the totemism controversy summarizes my "'Humanity in the chrysalis stage'", op. cit., and all of the quotations in my discussion are used in this article. The most famous illustration of Boas's students' efforts to produce comparative generalizations about societies is Ruth Benedict's *Patterns of Culture*, New York: Houghton Mifflin Company, 1934.

⁴¹ For a succinct summary of the differences between British and American anthropology, see George W. Stocking, Jr., "The Basic Assumptions of Boasian Anthropology" in his *The Shaping of American Anthropology*, New York: Basic Books, 1974, 1–20.

⁴² Bronislaw Malinowski, "Totemism and Exogamy" in Robert Thornton and Peter Skalník, eds., *The Early Writings of Bronislaw Malinowski*, translated by

Ludwik Krzyzanowski, Cambridge: Cambridge University Press, 1993 (from the portion of the essay originally published in 1911), 140.

⁴³ Bronislaw Malinowski, "Anthropology", *The Encyclopaedia Britannica*, 13th Edition Supplement, London and New York: The Encyclopaedia Britannica Company, 1926, 133.

⁴⁴ A.R. Radcliffe Brown, "The Comparative Method in Social Anthropology", *Journal of the Royal Anthropological Institute* 81 (1951): 22.

⁴⁵ See, for example, Kingsley Davis and Robert Merton's definitions of functionalist analysis, by which any demonstration of association between one social element and another counts as functionalist analysis. By this token, of course, unilinear evolutionist analyses were also functionalist, since they assumed the interdependence of the component parts of any stage of evolution. But consider Paul Rabinow, *Reflections on Fieldwork in Morocco*, Berkeley: University of California Press, 1977. Recognized as a pioneering example of a new anthropological genre, it purports to challenge all functionalist conventional wisdom, but nevertheless invokes some all-pervasive Moroccan cultural spirit, and accounts for the phenomena Rabinow observed in terms of an implicit whole.

⁴⁶ Jonathan Spencer, Anne Jepson, and David Mills, "Career Paths and Training Needs of Social Anthropology Research Students. ESRC [Economic and Social Science Research Council] Research Grant RES-000-23-0220. End of Award Report", unpublished manuscript, 2005, 9.

⁴⁷ See, for example, the work of Genevieve Bell, who earned her Ph.D. in anthropology at Stanford in 1997, who works for Intel Research. Observing 100 households in 19 cities in seven countries in Asia and the Pacific, she has observed differences in use of technology that Intel hopes to apply to future designs. See Michael Erard, "For Technology, No Small World After All", *The New York Times*, May 6, 2004, "Circuits" section, G5.

⁴⁸ See, for example, Adam Kuper, "Alternative histories of British social anthropology", *Social Anthropology* 13 (2005): 47–64.

CONTENDING CENTRES OF CALCULATION IN COLONIAL TAIWAN

The Rhetorics of Vindicationism and Privation in Japan's "Aboriginal Policy"

PAUL D. BARCLAY

INTRODUCTION

In the following analysis of Japanese survey anthropology's golden age in colonial Taiwan,¹ I argue that the enterprise's historical importance derives from its extra-scientific impact as a discursive intervention. Soon after the colony was annexed in 1895, Japan's small contingent of Tokyo-based anthropologists began making their way south. Quite self-consciously, they sought to replace "pre-modern discourses" that accentuated the Other's lack of civility with a cultural-pluralist framework that affirmed the Other's intrinsic attributes. Within a decade, Japan's survey anthropologists completed a serviceable ethnic map of Taiwan's Indigenous Peoples.² Positioning themselves in sub-bureaucratic "centers of calculation",³ their synoptic vision of a complex and previously inchoate local situation provided the ground for more refined surveys and detailed censuses as well as schema, images and terminologies that proliferated in Japanese propaganda, commercial writing and scholarly production.⁴

However, survey anthropology's academically informed model of human diversity did not enter Japanese colonial discourse uncontested. As it turned out, the cultural-pluralist framework was incommensurate with statist priorities of economy and speed, institutionalized under the leadership of de facto viceroy Gotō Shinpei (r.1898–1906).⁵ In fact, the ultimate centre of calculation in Taiwan was located in the Governor General's office, not on the anthropologist's desk. In the final analysis, I argue, the government anthropologist in Taiwan was an "intellectual middleman",⁶ neither an author of policy nor a scholarly innovator. As intermediaries between field officers with day-to-day contact with Taiwan Aborigines and policy-makers who rarely ventured outside of Taipei, they formed the linchpin in a multi-tiered sifting mechanism that produced the centre's working-knowledge of conditions in the highlands. In the end, their energetic and sophisticated discursive interventions could not prevent the northern tribes of central Taiwan from becoming typecast as unreconstructed savages who lacked the reason or cultural

capacity to respond to any policy but brute force.

BACKGROUND AND DEFINITIONS

Before fieldwork became the *sine qua non* of anthropological research, text-based scholars initiated cross-cultural comparison as a method of writing the universal history of human progress.⁷ Reflecting a non-conformist heritage of engagement with the abolitionist cause, champions of the method postulated the “psychic unity of man” as the ground for considering all peoples candidates for fruitful comparison. This “psychic unity” postulate pitted the comparativists against polygeneticists, who argued for the existence of distinct human races. As post-colonial critics have been quick to point out, however, the comparativists, by ranking peoples on a scale from savagery to civility, also contributed an intellectual justification for ideologies of difference and contempt for non-Europeans. According to the critical tradition, the evolutionists defeated the polygeneticists only to establish a more insidious paradigm for racism, substituting “culture” for “race” on the evolutionary scale, eventually succumbing to a pessimistic belief that cultural divides could not be bridged through the agencies of education and enlightenment.⁸

Like their intellectual forebears and actual teachers, survey anthropologists have left an ambiguous legacy, as champions of causes progressive for their time who also took part in a generalized apparatus of oppression. Survey anthropologists form a sort of historical “missing link” between armchair theoreticians and post-Malinowskian participant-observers. Ad-

mired for their stamina, ingenuity and encyclopaedic knowledge of world ethnology, they also find themselves excluded from the intellectual lineage of anthropology’s exemplary scholars. At the same time, like the comparativists, they remain interesting to historians as shapers and emblems of intellectual life in colonies, metropolises and the places in-between during the period of high imperialism. Unlike the armchair anthropologist, however, survey anthropologists physically confronted cultural variation in its environmental setting. They saw, heard, touched and smelled material and non-material artefacts *in situ*. Field experience, according to some, allowed survey anthropologists to conceptualize practices and objects as integrated ensembles, as components of particular cultures. In other words, their research methods lent themselves to a pluralist outlook. Their comparativist predecessors, in contrast, regarded implements and institutions as decontextualized data from which to distil a speculative history of the whole human race, instead of subdivisions thereof.

Because they ultimately relied upon the existence of a Latourian “center of calculation” to consolidate their findings, this essay considers survey anthropology as an extension and modification of the comparativist tradition, rather than as a precursor to participant-observation. Unlike their descendents, survey anthropologists never sought to view the world through the eyes of the peoples they studied; empathy was never the goal. Rather, survey anthropologists divided populations into intellectually and administratively digestible numbers of sub-units (tribes, races, ethnic groups) to answer questions or solve problems generated in

colonial metropolises. For our purposes, the colonial metropole, where “notes and queries” are authored, sent out from and ultimately collated, are equivalent to Latour’s “center of calculation”, the privileged place from which a totality of local situations can be viewed, abstracted and reduced to system.

Lastly, a word on the term “pluralism”. Nicholas Thomas argues that the modern pluralistic view of culture/ethnicity that informed and was elaborated by survey anthropology should be viewed as the successor of Christian and Enlightenment world-views that considered “heathens”, “infidels” and “primitives” as fundamentally incomplete human beings, either in need of salvation/education or expendable on the chopping block of history. Their negative traits — ignorance, illiteracy, etc. — defined the Other in the eyes of the observer. Building on the work of Johannes Fabian, Thomas argues that the discursive construction of tribes, races and ethnic groups as internally coherent collectivities which can be known, compared and ranked by recourse to study of “ideal types” ushered in the age of anthropological typification. His elegant formulation bears quotation in full:

What I seek to extrapolate from [Fabian] is an argument that in premodern European discourses, non-Western peoples tend to be characterized not in any anthropologically specific terms, but as a lack or poorer form of the values of the centre...My analytical fiction, then, tells of a shift from an absence of ‘the Other’ (as a being accorded any singular character) to a worldview that imagines a

plurality of different races or peoples. The distinctively *modern* and *anthropological* imagining projects natural differences among people that may be rendered at one time as different ‘nations’, at another as distinct ‘races’ or ‘cultures’. The underlying epistemic operation — of partitioning the human species — makes possible a variety of political and ethnographic projects: particular populations may be visible as objects of government; they may serve as ethnological illustrations or subversive counter-examples in comparative social argument; and these reified characters may be available for appropriation in anti-colonialist, nationalist narratives.⁹

This “distinctively modern and anthropological imagining” received much of its impetus, and exerted its influence, in the dialectical circulation of images, goods and people between colonial settings and metropolitan publics. Fortuitously, Thomas’s admittedly simplistic historical sketch, or “analytical fiction”, well describes the rupture in consciousness that Japanese survey ethnologists hoped to bring about in Taiwan. For this essay, the term “pluralism” is defined, following Thomas, as “a worldview that imagines a plurality of different races or peoples” in contradistinction to a worldview that conceptualizes different peoples “as a lack or poorer form of the values of the centre”.

PARTITIONING THE HUMAN SPECIES IN UPLAND TAIWAN

The Qing empire ceded Taiwan to Japan as part of the settlement to end the

Sino-Japanese war of 1894–95. As the Taiwan Government-General began setting up its capital in the face of armed resistance in June 1895, reports describing the curious folkways of the empire's new subjects began to circulate in Japan. Especially prominent in the early wave of "first-encounter" documents were travel accounts of the hill tribes, collectively known as "*banjin*", "*seibanjin*", "*yabanjin*" or "*banzoku*".¹⁰ Even before the Government-General could safely inhabit its capital, Takigawa Miyotarō published "Our New Territory: The Island of Taiwan" to popularize the quasi-ethnographic information contained in Ueno Sen'ichi's famous military intelligence report on conditions among the Aborigines.¹¹ Ueno's report was an amalgam of first-hand accounts and information collected by British light-house-keeper George Taylor.¹² Well into the 1900s, the occupation inspired popular ethnography for Japanese consumption, in the form of newspaper, magazine and scholarly accounts of life in "Darkest Taiwan's" interior.¹³

For the small coterie of anthropologists attached to Tokyo University, the ethnological bounty of the new colony proved irresistible. The intellectual backgrounds and institutional affiliations of the major players, Inō Kanori, Torii Ryūzō and Mori Ushinosuke, have been well documented elsewhere.¹⁴ For our purposes, it is enough to say that Inō Kanori (1867–1925), our major protagonist, set sail for Taiwan on November 3, 1895. At the time, Inō supported himself as an editor of an education journal while contributing notes on folklore to the *Journal of the Tokyo Anthropological Society* and attending the lectures of Japanese anthropology's founding father, Tsuboi Shōgorō. Inō embarked un-

der the auspices of the Japanese Army, thereafter working in the documents section of the Government-General and as an administrator of the Japanese Language schools, pursuing his interest in Taiwan anthropology between assignments.¹⁵

Inō's most remarked-upon contribution to Taiwan anthropology was precisely the kind of "epistemic operation" described by Nicholas Thomas as quintessentially *modern*: an ethnic map *cum* taxonomy of the Taiwan Aborigines. Inō sought to replace the casual observations of his amateur co-nationals and the pre-modern Qing descriptions of Taiwan Aborigines with a scientifically ascertained taxonomy based on the investigation of racial-cultural diversity in upland Taiwan. Inō Kanori succinctly stated these goals in mid-1895:

The people of Taiwan are known by three types: Chinese (*shinajin*), cooked barbarians (*jukuban*), and raw barbarians (*seiban*). As for the Chinese, of course their descendants will become obedient citizens (*kika no min*) — it should not present much difficulty to govern them. However, the raw and cooked barbarians need to be investigated from the perspectives of natural as well as conjectural science (*keijikajō*). Thereafter, an administration and an educational policy can be structured. As for "cooked" and "raw", these are general terms formerly used to reflect degrees of submission to [Qing] government. If we look at it from a scientific point of view, however, there are at least four or five different tribes/races (*shuzoku*) [of Aborigines], as we know from

looking at the articles written by foreigners who have investigated this area. But what about the intrinsic, distinctive (*koyū*) physiologies, psychology and local customs of the various tribes? What about their connections to the Philippine islands and neighboring islanders? To this day, these are unsettled issues. Today, by the hands of our countrymen, the clarification of these questions will, it goes without saying, contribute to our political goals...And we shall also see results in regard to our scholarly aspirations.¹⁶

Inō's manifesto (and subsequent writings) called for Japanese survey anthropologists to identify the unique features of each *shuzoku* (tribe/ethnos) on Taiwan in order to better understand the differences among the groups subsumed under the Qing terms *shengfan* (raw barbarian) and *shufan* (cooked barbarian). Inō also emphasized that anthropology should render faithful service to the state as a form of intelligence gathering. These two goals would come into conflict, I will argue, undermining Inō's ability to construct a coherent account of Japanese relations with the uplanders, in effect forcing him to choose between loyalty to an emerging discipline or obedience to his bureaucratic superiors.

On May 26, 1897, Inō formed an expedition party to begin a 192-day ethnographic survey tour by order of the colony's Bureau of Education. The Government-General ordered Inō and his partner, Awano Dennojō, to devise a portrait of Aboriginal society for the purpose of making recommendations on the subject of Aboriginal schooling. The results were

sent to Gotō Shinpei in early 1899 as a report titled *Taiwan Banjin jijō* (Conditions among the Taiwan Aborigines).¹⁷ Considered Inō's magnum opus, *Banjin jijō* is a rich, descriptive and internally conflicted document that speaks in multiple voices, reflecting Inō's intermediary position in the colonial order of things. Relying on his own observations in the field (though never in any one spot for long), archival research in Chinese records and interviews with Pacification-Reclamation officers,¹⁸ Inō constructed a matrix of defining traits — physical features, everyday usages and implements (*dozoku*), cultural practices (*kanshū*), language and oral traditions — to classify the inhabitants of Taiwan's interior into eight discrete ethnic groups.

The Janus-faced nature of this document, a testament to survey anthropology's ambiguous legacy, is illustrated by Inō's characterization of the Atayal peoples. On the penultimate page of his 283-page report, Inō warned Japanese officials against the temptation to caricature the Aborigines (*banzoku*) as savage headhunters.¹⁹ In Inō's taxonomic grid, "headhunting" comprised a single item out of six elements called "customs", while "customs" themselves stood beside other bundles of defining traits, such as "physical features", "language", "technology" and others. Inō emphasized that many Aboriginal groups had ceased headhunting, but even those who continued, like the Atayal, were also competent agriculturists and weavers. Moreover, continued Inō, the savage custom of headhunting was perpetuated as a form of defence against aggressive Han settlers. Inō finished by asserting that the tribes of Australia and Africa were much more primitive than

Taiwan's headhunting Atayal, thereby relativizing their backwardness by recourse to the accumulating world-wide database of "cultures" put into play by the armchair comparativists of yore.²⁰

Going completely against the grain of his conclusion, Inō began the substantive sections of *Banjin jijō* by fixing the Atayal peoples as Taiwan's least-advanced tribe, describing them as preternaturally xenophobic, bloodthirsty headhunters responsible for over a hundred beheadings annually.²¹ Inō's evolutionary ranking of the tribes, in what we might today call the "bullet-points of the report", attributed the Atayal's bottom position to environmental factors:

Taiwan's most advanced Aborigines are the Peipo tribe (*ping-puzu*), followed by the Parizarizao section of the Paiwan tribe, the Puyuma tribe, the Amis tribe and others who inhabit the plains. The lowest position is occupied by the Atayal tribe, who all live deep in the valleys, whose steep mountain paths have obstructed intercourse and made travel difficult...There is no doubt that this state of affairs is directly related to the degree of intercourse with the Chinese. Especially in those villages located among Chinese settlements, we see the most pronounced progress (*shinpo*).²²

Thus, Inō's evolutionary perspective reproduced elements of the old Qing "Sinocentric World Order" ethos that equated "civilization" with proximity to China's sacral-political centre. Emma Teng, in her analysis of Qing nomenclature, discourse and travel writing vis-à-vis the Taiwan

Aborigines, identifies a persistent strain in Qing documents which demonizes the Aborigines for their lack of civility. She terms such discourse the "rhetoric of privation", in contrast to the more romanticized rhetoric of primitivism (the "Noble Savage").²³ Though Inō equated distance from Chinese influence with savagery in much of his ethnology, he severely criticized the rhetoric of privation as non-scientific in another venue, writing:

When the Chinese first learned of Taiwan's location, they acknowledged the existence of the island's own people, or "the natives". There are many writings that attest to this. But at the time, they only recognized the natives as a different people, with different language and customs, but did not give them a particular name...In Ming times, the name "Eastern Barbarians" (*dongfan*) was used, probably meaning "the barbarians of the Eastern Seas...After the Qing occupied Taiwan, there were two major divisions, based on the presence or absence of political compliance [to the Qing], the *seiban* and the *jukuban*...They did not, [however,] make observations about race."²⁴

In this passage, the term "political compliance" is loaded. From the Sinocentric World Order perspective, the court of the Chinese emperor is the metonymous centre/apex of tradition, refinement, power and learning. The source of humanity-making benevolence is configured as a geographical node of virtue, which radiates outward and downward via the power of attraction, imitation and what we might today call acculturation. The boundaries

of the realm of civilization are extended by bureaucracy, the repository of Confucian learning and instrument of Chinese statecraft. Thus, in the passage above “the absence or presence of *political* compliance” also denotes “cultural” submission to the Chinese centre.²⁵ Thus, Inō’s critique prefigures Thomas’s characterization of pre-modern discourses about the Other; they are distinguished from modern scientific discourses by their fixation on *lack* or *presence*, their overbearing concern with the “values of the centre”.

There are, then, two major contradictions in Inō’s ethnology of Taiwan Aborigines. First, the relativizing rhetoric of the *Taiwan Banjin jijō*’s conclusion contradicts the rhetoric of privation that permeates the body of the report; and Inō’s explicitly modern-pluralist approach to taxonomy is undermined by his ultimate recourse to the Sinocentric preoccupation with the Atayals’ physical and cultural distance from the Middle Kingdom. These glaring contradictions call for explanation, because Inō was, if anything, a deliberate scholar, a man obsessed with establishing himself as a member of the Meiji-period bureaucratic-literary elite.

As an ethnologist in 1899 colonial Taiwan, Inō was writing against a discourse that put the human status of the Atayal into question. Anthropologist David Scott uses the term *vindicationism* for such narratives.²⁶ In other words, if the question is: “Are the Atayal beasts or human beings?” then Inō’s reply, in the vindicationist mode, is “They are human beings.” Inō’s contemporary Torii Ryūzō, fellow survey anthropologist and veteran of Tsuboi Shōgorō’s seminars, also laced his ethnological notes with vindicationist

rhetoric. Moreover, Torii’s interpreter, Mori Ushinosuke, who would himself become a prominent government expert on Aboriginal languages, was an adamant vindicationist as well.²⁷ Thus, it would be fair to characterize Taiwan survey anthropology of the Meiji period (1895–1912) more generally as a vindicationist enterprise.

Banjin jijō was, however, only partly an ethnological study. Primarily, it was edited and abbreviated for practical application as a report submitted to Gotō Shinpei, Taiwan’s Minister of Civil Affairs from 1898 to 1906. Analyzing the interplay between Gotō the powerful administrative superior and Inō the dutiful bureaucrat is as important as it is difficult.²⁸ As a self-styled visionary and actor on the global stage, Gotō, an accomplished physician and public-health administrator, frequently invoked the scientific method as a rationale for his policy proclamations. Gotō’s avowed appetite for research on colonized populations, is matched by Inō Kanori’s reputation as a producer of such knowledge. Inō, the indefatigable, driven and scrupulous editor, compiler, analyst and fieldworker, is commonly regarded as the father of modern Taiwan Studies, and was certainly the government’s acknowledged expert on Aboriginal country around 1900. In addition, both men hailed from the area of northeastern Japan’s Iwate prefecture, giving them common cause as rising men from Japan’s rural periphery. Considering these factors, one would expect Inō’s ethnological labours to have had a large impact on Gotō’s view of Aborigines in Taiwan. Paradoxically, it appears that Gotō influenced Inō’s thinking instead; though Gotō of course had very little specific knowledge about the

Aborigines themselves, and was ostensibly being informed by Inō's work.

E. Patricia Tsurumi has aptly characterized Gotō's rough-and-ready sociology of Taiwan as garden-variety Spencerian evolutionism.²⁹ Tsurumi's judgement finds evidence in a much-reproduced 1901 policy statement entitled "An Opinion on the Necessity of Conducting a Survey into Customary Law for the Governance of Taiwan". Here, Gotō applied Spencerian logic to assert that Taiwan's Chinese population was not ready for the sudden introduction of fully civilized Japanese legal codes, because it had become accustomed to a partially civilized legal regime during 200 years of Qing rule. In other words, the rights guaranteed to Japanese subjects under the 1889 constitution would not be granted to Taiwanese (though, of course, the obligations would) for fear that too-sudden a change would shock the "organism" of Taiwanese society. And as for the Aborigines, Gotō used the general marker for savagery, *yaban*, to degrade them and assert that they also could not be governed through modern law codes.³⁰ Gotō referred to the "savages who dwell in the undeveloped lands" as living fossils from antiquity in a classic example of what Johannes Fabian has called "allochronic" discourse.³¹

On one important point only, it appears that Gotō incorporated Inō's ethnology into his own thinking. His declaration that the Aborigines and the Chinese were distinct populations was of a piece with Inō's 1895 manifesto quoted above. We shall return to the significance of this agreement below. On the whole, however, it appears that Gotō was more hostile to than ignorant of Inō's report of 1899. In what must have

come as a stinging rebuke to Inō Kanori, Gotō applauded the efforts of government employees (like Inō?) to submit their hard-earned local knowledge to the government in the form of reports. Gotō rejected, however, the existing knowledge at hand as too unsystematic and non-specialist. Gotō wrote that Western nations had sufficiently developed scholarly communities to let specialists compete among themselves to study native customs, laws and economy; in these advanced nations, the government only had to convene these scholars and reap the harvest. For Gotō, Japan's civil society (*kokumin*/"national people") was still too immature for its government to take such a laissez-faire approach.³²

Matsuda Kyōko argues that Inō's recourse to Social Darwinism stemmed from his visceral reaction to harsh research conditions. Poor infrastructure, lack of security and forbidding terrain combined to provoke Inō to project his "struggle to conduct a survey" onto the Atayal peoples as a "struggle for survival". In short, Inō reasoned that the Atayal had been pushed to such extreme living conditions because they had been forced into the interior by superior forces (the Chinese).³³ This analysis is attractive, for it shows the survey anthropologist responding to the local environment, yet in such a way that his own relationship to the culture-bearer is reified into an enduring characteristic of that society, exposing both the strengths and weaknesses of the genre. At the same time, Matsuda's analysis does not explain the grave contradiction between Inō's vindicationism and his rhetoric of privation.

My alternative explanation is admittedly speculative, but has the advantage

of clarifying the contradictions within Inō's corpus. I believe that Inō adopted elements of the rhetoric of privation and the language of Social Darwinism in the *Banjin jijō* to anticipate or answer to Gotō's objections to his vindicationism. Inō began and finished his field survey before Gotō came to Taiwan, concluding on December 1, 1897. During a 13-month interval, Inō collated his data, read more deeply in Qing documents and drew his conclusions. He filed his report to Gotō on January 9, 1899, about eight months into Gotō's tenure. During his write-up period, he was fired as part of Gotō's and Governor-General Kodama Gentarō's administrative house-cleaning of March 1898, only to be re-hired soon after. He then quit again in December 1898 to return to Tokyo for a year.³⁴ Considering that Gotō's fondness for evolutionary metaphors was well-known to Inō during a period of intermittent unemployment, it seems not unreasonable to expect that Inō would recast his survey ethnology to meet the expectations of powerful and sceptical readers. If Inō often portrayed himself as the centre of calculation vis-à-vis colonial policemen, military officers and amateur ethnographers,³⁵ he in turn answered to an even more paramount centre of calculation in the person of Gotō Shinpei.

As we have seen, Gotō Shinpei did not think Inō's survey worthy of the name "science" in 1901. Nonetheless, he sufficiently appreciated Inō's skills as an editor and compliant underling to commission him for several more projects, making Inō, in effect, the Government-General's in-house historian of indigenous Administration, for both the Qing and Japanese periods.³⁶ In this new role, Inō would begin a second career in Taiwan as an histori-

an,³⁷ shifting his purview from the mapping of cultures in space to the identification of meaningful segments of linear time.

A BROKEN NARRATIVE: INŌ KANORI'S "10-YEAR HISTORY"

Like the 1900 *Taiwan Banjin jijō*, Inō Kanori's 1905 *Ryō Tai jūnen shi* (10-Year History of the Occupation of Taiwan) was compiled for the Government-General's second-in-command, Gotō Shinpei.³⁸ Gotō's preface stressed that the history of Taiwan was testament to Japan's achievements as a modern colonial power. Inō of course inscribed Gotō's progressive view of history into this historical digest, though he stumbled in his short chapter on indigenous Administration, the topic he knew best. As a government scribe, Inō imposed a linear, progressive narrative structure upon the confused history of Japanese-indigenous relations by making Japan's "punitive policy" into the dynamic element of the narrative. Inō thereby de-emphasized the record of conflict within the administration and the complex story of frontier diplomacy in the earlier period. This simplification, in turn, erased the pluralistic view of Taiwan's internally differentiated Aboriginal population from the official narrative, while maintaining the major distinction between Chinese and Austronesian races.

In its chapter on indigenous Administration, the "10-Year History" ignores the first eight months of martial law on Taiwan (August 1895–March 1896) to open with the Government-General's declaration of civilian rule on April 1, 1896. This opening gambit is important, for it establishes the Confucian subtext of Inō's preferred and intended narrative structure:

civil government is normal, ideal and laudable, while martial law is a last resort, an expedient for failed policies. Again we can detect Gotō's hidden hand here, recalling that the Minister of Civil Affairs insisted, upon taking the portfolio in 1898, that he be paramount to all military men in Taiwan, except for the Governor-General, Kodama Gentarō. To dramatize his much-publicized belief that military rule was ruining the colony, Gotō actually struck a naval officer in front of a military audience to defend his own honour. Kodama, ever Gotō's protector, approved of Gotō's brash action.³⁹

The first event of Inō's history, then, is the establishment of the Pacification-Reclamation Office (*bukonsho*). The Bukonsho, wrote Inō, was chartered "solely to enact 'moral suasion' (*kyōka*) among the Aborigines". Inō then added, contradictorily as it turned out, that the Bukonsho was also charged with overseeing the "economic development" (*kaihatsu*) of the "Aboriginal territory" and "finding useful employments for the Aborigines (*banjin no jusan*)". The tension between *kyōka* and *kaihatsu* becomes clear if we comprehend "moral suasion" as a spatial metaphor rooted in the "Sinocentric" topographical political imagination,⁴⁰ and conceive of *kaihatsu* as a temporal metaphor more appropriate to Enlightenment theories of progress. In the former model, the centre of calculation is the Imperial Centre itself, eternal, patient and inevitably triumphant. In the latter model, the centre of calculation is the state's political leadership, which resolves conflicts and defines efficiency in the context of national interest in a world of competing nation-states.

Perhaps anticipating Gotō's views on the subject, Inō posited headhunting as the defining trait for "certain tribes in the northern half of the island" to introduce the Aborigines in his "Ten-Year history". This stereotype, based on a single trait of the population in question, was precisely the kind of demonizing Inō decried in his vindicationist mode five years earlier. Adding force to the "trope of the savage headhunter", Inō used the contrasting term *ryōmin* to describe their victims. In Qing-period usage, *ryōmin* (Chinese: *liangmin*) referred to tax-paying artisans, merchants and agriculturists: literally, the "good people".⁴¹ Inō attributed *ryōmin* victimhood to the atavistic Aboriginal "custom" of headhunting, initially constructing a culturalist explanation redolent with the rhetoric of privation. In the discursive field of "moral suasion" (*kyōka*), then, such "evil customs" would ideally be reformed by the civilizing, edifying influences of the centre, as transmitted by civil (Chinese: *wen*, Japanese: *bun*) institutions like the Bukonsho.

In the first turning-point in his narrative, Inō recounted that the Bukonsho could not stop Aboriginal attacks on *ryōmin* through moral suasion alone. Therefore, the Government-General formulated a system of punishments (*chōbatsu*) directed at Aboriginal headhunters in late 1897. Despite this concession to expedience (force), Inō assured readers that Japanese policy remained organized on the principle of "reassurance through acts of kindness (*suibu*)", to argue that the "civil" impulse was still ascendant around 1898.

Quite abruptly, Inō then changes tack to describe headhunting incidents as "acts of murder and assault" (*kyōkō*) to explain

the government's expansion of police forces (*keisatsu*) along the Aboriginal border in 1898. The new intolerance of "assault and murder" can be read as headhunting's redefinition from "custom" to "crime". This reconsideration was warranted, according to Inō, by a "fear that headhunting would stop plans for Aboriginal-territory development dead in their tracks". Curiously, Inō neglects to mention any specific commodities or economic activities that might have been connected to headhunting at the time (though he surely knew, as we shall see below).

As violence became unmanageable on the Aboriginal frontier in 1898, the Government-General lacked a unified plan. The argument over whether to consider headhunting as a custom in need of reform or as crime in need of punishment fomented a "clash of opinions" within the bureaucracy. Still maintaining the Confucian perspective, Inō called the "moral suasion" emphasis of Bukonsho civil administration the "positive policy" and referred to "punishments" as the "negative policy".

Then, in what Inō called a "Great Revolution", in June 1898 the Government-General dissolved the Bukonsho, for allegedly leaning too far in the direction of leniency/attraction to the neglect of force/punishment. Henceforth, Aboriginal Affairs was put under the rubric "severity tempered with leniency" (*on'i narabi okonawaru*), a dignified locution for "carrot and stick". Subsequent narratives characterized the dissolution of the Bukonsho as a necessary response to Aboriginal savagery. Inō, however, intimated that perhaps it might have been made effective if given more time. Such a hypo-

thesis would explain why Inō switched back to the vindicationist mode in this narrative, now casting the northern Aborigines as history's victims. Halfway through Inō's account, the Han are transformed from "*ryōmin*" (good people) into "Chinamen". Temporarily abandoning the rhetoric of privation that explained headhunting in terms of "savagery", Inō implied that both Han and Aborigines were to blame for the mayhem that was impeding Japanese development in the highlands. Recalling an early staple of Japanese official rhetoric before the Kodama-Gotō era, Inō now claimed that Taiwan's Chinese settlers provoked Aboriginal bloodlust by taking advantage of their ignorance and stupidity (*gumō*) to perpetrate land swindles.

Ostensibly acting as an impartial broker to stop the revenge cycle, the Government-General decreed that all non-Aborigines (Chinese) obtain permits to reside in "Aboriginal territory" in February 1900. At this point in the narrative, Inō's confusion and discomfort become palpable. Even after situating Aboriginal-initiated violence within a context of Han perfidy, Inō back-tracked to describe Aboriginal "assault and murder" as irrational behaviour. In a tortured locution that I read as a concession to the preferences of the administrators above him, Inō wrote: "As the number of people entering the Aboriginal districts increased, the ignorant Aborigines harboured suspicions of invasion, prompting the Aborigines to perpetrate outrages on an immeasurable/disproportionate scale."

Accordingly, from 1902 onward, heavily armed and staffed guardlines (Japanese: *aiyū*; Chinese: *aiyong*) were exten-

ded to physically separate Aboriginal territory from the rest of Taiwan. In January 1903, all of northern Aboriginal country was placed under police jurisdiction, in a complete concession to the “expedient” of police rule. Bringing his narrative up to the present (1905), Inō wrote that the Aboriginal population north of Puli in Nantou prefecture was now governed solely under the rubric of “force and intimidation” (*iatsu kyōsei*), while the southern Aborigines would be governed under the banner of “education and largesse” (*keihatsu suibu*). In this perplexing document, Inō has the northern Aborigines commit atrocities because they are provoked by cunning Chinese invaders who take advantage of their ignorance. And yet the Japanese government responds by ruling these historical victims under the banner of “force and intimidation”. At the same time, readers are to believe that by 1905 the “southern Aborigines” were willing objects of non-coercive policies of tutelage and guidance. Inō concluded this report with an unconvincing assurance that the Government-General’s two-pronged approach would likely produce good results sometime in the future.

For students of the *Taiwan Banjin jijō*, as well as for Japanese administrators at the time, there could be little doubt that the choice of Puli, Nantou prefecture, as a dividing line in Inō’s narrative meant that Inō was defining the “northern tribes” as the Atayal. In Inō’s taxonomy of 1899, all tribes south of Puli, which is the geographic centre of Taiwan, do not practise the art of facial tattooing (although limb tattoos were found throughout the island). Other than this single distinguishing feature, there was no ethnologic basis for bifurcating Taiwan’s Indigenous Peoples

into southern and northern halves. This division was, instead, political. While Inō’s survey anthropology of the late 1890s demonstrated internal diversity and a welter of varied political conditions in rural Taiwan, his narratology, like the old Qing paradigm he once tried to overturn, employed a typology that sorted Aborigines into “good” (southern) and “bad” (northern) imperial subjects. Inō did not invent this Manichean nomenclature; he merely reappropriated it from the “amateurs” he once ridiculed as unfit to perform ethnological analysis.

From as early as 1896, Japanese officials, notably Nagano Yoshitora, found that Bunun warriors in the environs of Puli could be persuaded to bear arms for the government in its skirmishes against villages north of Puli, who fell under the broad rubric “Atayal”.⁴² From this time forward, the “southern tribes” became symbols of compliance in official documents, while the Atayal became stock villains in Japanese rhetoric. It is important to note here that Bunun were also known as headhunters and, in fact, could be induced to take heads at the behest of the Japanese state. We can thus conclude that Inō’s 1905 narrative located savagery not in the act of murder, but in the act of non-compliance with government demands.

According to several contemporary sources, the Atayal tribes clustered around Wushe (“Musha” to the Japanese) made themselves odious to the Japanese when a few villagers murdered 14 Japanese road surveyors in February 1897, the ill-fated Fukahori expedition. The long saga of investigation, recovery of remains, and economic blockades to punish those responsible is indeed a central thread in the area’s

history under Japanese colonial rule.⁴³ Probably more important, though, is the fact that many of the settlements just north of Puli abutted rich stands of camphor. Several “northern tribes” were active in the lumbering trade, charging outsiders like the Japanese fees for access to the forests in the early years of colonial rule. Disputes with a particularly powerful Saisyatt entrepreneur-chief named Ri Agui erupted into a large-scale war in late 1902. Like the Atayal, the Japanese subsumed the Saisyatt under the term “northern tribes” in later discourse, again suggesting that “northern tribes” was really shorthand for “non-compliant” villages. Mochiji Rokusaburō is considered by many to have been the brains behind Gotō’s Aboriginal policy. In his famous position paper of late 1902, he proposed that the Aboriginal Territory be divided into northern and southern sectors, with the former earmarked for military conquest, the southern for “moral suasion”.⁴⁴ And thus, the rough-and-ready demarcation of operational zones in the war to increase the empire’s wealth obliterated fine distinctions made by survey anthropologists like Inō Kanori, who wrote in 1899 that “the disparities in cultural attainment within tribal divisions is often just as varied as the disparities between tribes themselves”.⁴⁵

The image of the north/south division among Aborigines was cemented in a rather grisly affair on October 5, 1903. Then, Japanese officials induced their Bunun allies from Kantaban to entrap, ambush and slaughter more than 100 Atayal men (from Palaan and Hogo) in a single morning near Puli. These so-called southern tribesmen actually redeemed the heads at a Japanese outpost, and were

photographed doing so. Thus, by 1903, Japanese administrators began to define all Aborigines north of Puli, who were distinctive because of their facial tattoos, as a problem population as a result of economic disputes over rights to camphor stands and local resistance to Japanese attempts to survey remote areas. The empirical poverty of this conceptual apparatus is laid bare when we observe that Japanese army and police forces fought a number of pitched battles with villages of “southern tribesmen” (Paiwan and Bunun especially) well into the 1930s.⁴⁶

BIOPOWER AND NECROPOWER IN COLONIAL TAIWAN

Public intellectual, journalist and parliamentarian Takekoshi Yosaburō visited Taiwan in 1904 to write a book celebrating Japan’s colonial achievements. A committed Whig historian, Takekoshi was the embodiment of progressive thought in Meiji Japan.⁴⁷ In his largely hagiographic portrait of Gotō’s reforming administration, Takekoshi cited Inō Kanori as his authority on conditions in Aboriginal country, while praising Gotō Shinpei’s broader vision for developing the island. Takekoshi’s ambivalent report on indigenous administration, I believe, is evidence that Inō was still articulating a vindicationist rhetoric in his face-to-face dealings with other Japanese, even if his ethnological and historical writing under the auspices of the Government-General deployed the “trope of the savage headhunter” as an explanatory device. Takekoshi’s view of the situation as it existed around 1904 is more candid and less reticent than Inō’s 1905 digest as to why the “northern tribes” had been singled out

for the policy of “intimidate and coerce” after 1903:

Almost everybody who has come in contact with the savages declares that they are all quite capable of being raised from their present state of barbarism... But it is a question *how much longer* the Japanese authorities will be willing to pursue their present policy of moderation and goodwill, and leave nearly half the island in their hands. If there were a prospect of their becoming more manageable in ten or even in twenty years, the present policy might possibly be continued for that *length of time*, but if the process should require a century or so, it is quite out of the question, as we have not that *length of time* spare. This does not mean that we have no sympathy at all for the savages. *It simply means that we have to think more about our 45,000,000 sons and daughters than about the 104,000 savages* [emphasis added].⁴⁸

In Takekoshi's analysis, “the policy of moderation and goodwill” is none other than the Bukonsho ideal of *kyōka*, or “moral suasion”. The Bukonsho's charter stated that officers should learn Aboriginal languages, study their customs and enact the policy of *kyōka* in accordance with this hard-earned knowledge. In fact, the wording of the Bukonsho's charter (March 1896) resembles Gotō's 1901 rationale for a survey of customary law: good policy is based on accurate knowledge of local conditions. But as the Bukonsho project was launched, the enormity of the task as outlined became apparent. The variety and

difficulty of local languages, the complexity of the “late imperial frontier economy”⁴⁹ in the borderlands, and the resistance of armed Taiwanese all revealed that the enactment of “moral suasion” would be a long-term approach. Japanese officials, with few exceptions, did not understand Aboriginal languages, and even lacked accurate information about the location of many northern villages. To learn these languages, and map this terrain, local alliances would be required, and these were always slow in the forging.⁵⁰

For officials in Taipei, however, the rhythms of compound interest on public bonds, Japanese election cycles and the challenges of international diplomacy set the timetable for action. Despite protestations that severity was required as a response to “headhunting”, it is clear that the “metropolitan clock” indeed ticked loudly in Gotō's ears as the Japanese public grew weary of colonial debts to the mother country and stories of rampant corruption in the management of the island. To solve the fiscal problems of empire, Gotō instituted a camphor monopoly in 1899, and it began to pay quite handsomely by 1901. As the Government-General became addicted to this new income stream, policy indeed tipped away from the *kyōka* faction and towards the *chōbatsu* faction. As Antonio C. Tavares has demonstrated, harvesting and processing of camphor under the old system of traditional fees to indigenous strongmen was too slow and complicated to accommodate the high-velocity commodity flows that were now required to balance the colonial books. Thus, the Government-General began to support, rather than restrain, Japanese camphor companies who flouted

local conventions and thereby exacerbated frontier skirmishing over access to resources.⁵¹

Takekoshi's blunt statement of an "us or them" mentality indicates a way out of a key dilemma posed by Japanese survey anthropology under the Gotō regime. That is, it clarifies how Gotō and his chosen coterie could at once be the very emblems of Foucauldian "governmentality" in their zeal for colonial research, infrastructure programmes and public health policy while, at the same time, remaining ignorant and even hostile to survey anthropology, even if done in the name of the state. Yao Jen-to argues that post-colonial critics are off the track when they study literature and fiction to understand colonialist discourse as a series of misreadings, errors and silences. Instead, Yao insists, the statistical-bureaucratic-legal machine that was the Taiwan Government-General is better understood in terms of what it did know about the population it constituted through its statistical compendia, surveys and development projects. In this analysis, the Taiwan Government-General was the quintessential biopolitical regime, because it "forced the Taiwanese to become healthy"; not for humanitarian reasons, but to grow a large labour force of "docile bodies" as the engine of a colonial economy.⁵²

Interestingly, Yao considers the 1905 census of Taiwan to have been a precociously detailed and fine-grained example of surveillance, an example of how far the Japanese state reached into the lives of the populace. However, Yao fails to mention that 60% of Taiwan's territory, the abode of the Aborigines, was excluded from the census because of its sparse population,

poor infrastructure and unsettled political conditions. How can we consider a regime as the essence of "biopower" and "governmentality" when it avoids surveying 60% of its territory? The answer lies in Takekoshi Yosaburō's summary remarks on indigenous administration around 1904. If the "biopolitical" body, the population whose increase and health the government seeks to further, is construed as the whole empire, the home islands of Japan (*naichi*) and all of Taiwan, then the government's willingness to confine, embargo and slaughter Atayal villagers is logical within a governmental framework. That is to say, from Takekoshi's perspective, the northern tribes were jeopardizing the increase, wealth and survival of Japan's "45 millions".

From Inō's, Torii's and Mori's centre of calculation, Aboriginal territory loomed large; it was a treasure-house of ethnic abundance and a forbidding terrain that might require decades to survey properly. For the survey anthropologist, space was everything. For Gotō and Takekoshi, however, population was more important than space; from their centre of calculation in Tokyo, biopolitical considerations doomed the ethnologist's vindicationist rhetoric to the dustbin of history. And yet, as Kobayashi Gakuji has perceptively argued, Inō's taxonomic work was not a completely innocent exercise. In Kobayashi's analysis, it was Inō and the survey anthropologists, with their modern theories of race and ethnicity, who drew the sharp conceptual line between "Chinese" and "Aborigine" in Taiwan.⁵³ This epistemic operation, to use Nicholas Thomas's language, indeed "partitioned the human species" in such a way as to enable the Japanese officials analysed above to treat

camphor-related violence as a distinct category of trouble; namely, the “Aboriginal Problem”.

ENDNOTES

¹ For recent scholarship, see Liao Ping-Hui & David Der-Wei Wang, eds., *Taiwan under Japanese Colonial Rule, 1895–1945: History, Culture, Memory* (Columbia University Press, 2006), and the special colonial Taiwan issue of the *Journal of Asian Studies* 64,2 (2005). Other exemplary monographs, dissertations and articles include: Paul R. Katz, *When Valleys Turned Blood Red: The Ta-pa-ni Incident in Colonial Taiwan* (Honolulu: University of Hawai'i Press, 2005); Robert Tierney, *Going Native: Imagining Savages in the Japanese Empire* (Ph.D. Dissertation. Stanford: Stanford University, 2005); Faye Yuan Kleeman, *Under an Imperial Sun: Japanese Colonial Literature of Taiwan and the South* (Honolulu: University of Hawai'i Press, 2003); Chang, Lung-chih, *From Island Frontier to Imperial Colony: Qing and Japanese Sovereignty Debates and Territorial Projects in Taiwan, 1874–1906* (Ph.D. Dissertation. Harvard University, 2003); Ming-cheng M. Lo, *Doctors within Borders: Profession, Ethnicity, and Modernity in Colonial Taiwan* (Berkeley: University of California Press, 2002); Leo T.S. Ching, *Becoming 'Japanese': Colonial Taiwan and the Politics of Identity Formation* (Berkeley: University of California Press, 2001); Wang Tay-Sheng, *Legal Reform in Taiwan under Japanese Colonial Rule, 1895–1945: The Reception of Western Law* (Seattle and London: University of Washington Press, 2000); Michael Stainton, “The Politics of Taiwan Aboriginal Origins” in *Taiwan: A New History*, edited by Murray A. Rubinstein (Armonk, NY: M.E. Sharpe, 2000).

² “Taiwan Aborigines” is a translation of the Chinese word *Yuanzhumin*, or Japanese *Genjūmin*. Others translate it as “Formosan Aborigines”. The proper noun *Yuanzhumin* was adopted as the official designation of Taiwan's indigenous population in the third revision of the Taiwan constitution (1994), in response to organized efforts by indigenous rights groups. The proper noun “Taiwan Aborigines” will be used interchangeably with “Indigenous Peoples of Taiwan” throughout this essay.

³ This phrase was coined by Bruno Latour, *Science In Action: How to Follow Scientists and Engineers Through Society* (Cambridge: Harvard University Press, 1987); in this essay, I am following Matthew G. Hannah, who applied the concept to population inventories in *Governmentality and the Mastery of Territory in Nineteenth-Century America* (Cambridge University Press, 2000), pp.131–41.

⁴ Matsuda Kyōko, “Ryōtai Shoki no Taiwan Senjūmin Chōsa: Inō Kanori o chūshin ni” (Taiwan Aborigine Surveys During the Early Period of Japanese Rule: A Focus on Inō Kanori), *Taiwanshi kenkyū*, no.14 (1997): 135–48; Kobayashi Gakuji, “Inō Kanori no Taiwan Genjūminzoku Kenkyū”, *Gakushuin shigaku* 37 (1999): 71–87; Paul D. Barclay, *Japanese and American Colonial Projects: Anthropological Typification in Taiwan and the Philippines*, (Ph.D. Thesis, University of Minnesota, 1999).

⁵ Descriptions of Gotō's career, thought and policies are well documented; see: Takekoshi Yosaburō, *Japanese Rule in Formosa*, trans. George Braithwaite (New York and London: Longmans, Green, and Co., 1907. Reprint: Taipei: Southern Materials Center, 1996); E. Patricia Tsurumi, “Taiwan under Kodama Gentaro and Goto Shimpei” in *Papers on Japan*, edited by Albert Craig (Cambridge, MA: Harvard East Asian Research Center, 1967); Hayase Yukiko, *The Career of Gotō Shinpei: Japan's Statesman of Research, 1857–1929* (Ph.D. Dissertation, Florida State University, 1974); Edward I. Chen, “Goto Shimpei, Japan's Colonial Administrator in Taiwan: A Critical Reexamination”, *American Asian Review* 13, no.1 (1995):29–59; Yao, Jen-to, *Governing the Colonised: Governmentality in the Japanese Colonisation of Taiwan, 1895–1945* (Ph.D. Dissertation, University of Essex, 2002).

⁶ This phrase taken from Bruce Kuklick, *Blind Oracles: Intellectuals from Kennan to Kissinger* (Princeton: Princeton University Press), p.40, who described George Kennan as “someone with an interest in ideas and with a knack for conveying them to a less scholarly audience”.

⁷ I base this following thumbnail sketch of anthropology's early history on the following authorities: Fredrik Barth, Andre Gingrich, Robert Parkin and Sydel Silverman, *One Discipline, Four Ways: British, German, French, and American Anthropology, The Halle lectures* (Chicago: University of Chicago Press, 2005); Shimizu Akitoshi, “Colonialism and the Development of Modern Anthropology in Japan” in *Anthropology and Colonialism in Asia and Oceania*, edited by Shimizu Akitoshi and Jan van Bremen (Richmond, Surrey: Curzon, 1999), pp.115–71; Henrika Kuklick, *The Savage Within: The Social History of British Anthropology, 1885–1945* (Cambridge: Cambridge University Press, 1991); Adam Kuper, *Anthropology and Anthropologists: The Modern British School* (London and New York: Routledge, 1983); Curtis M. Hinsley, *The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America* (Washington, DC: Smithsonian Institution Press, 1981).

⁸ George W. Stocking Jr., “The Dark-Skinned Savage: The Image of Primitive Man in Evolutionary Anthropology” in *Race, Culture, and Evolution: Essays in the History of Anthropology* (New York: The

Free Press, 1968); Johannes Fabian, *Time and the Other: How Anthropology Makes its Object* (New York: Columbia University Press, 1983); Robert J.C. Young, *Colonial Desire: Hybridity in Theory, Culture and Race* (London and New York: Routledge, 1995).

⁹ Nicholas Thomas, *Colonialism's Culture: Anthropology, Travel and Government* (Princeton: Princeton University Press, 1994), pp.71–2.

¹⁰ These terms have extremely pejorative connotations and have been replaced in official and popular discourse with the general term “*Yuanzhumin/Genjūmin*”, which I will translate as “Indigenous Peoples of Taiwan” or “Taiwan Aborigines”.

¹¹ Takigawa Miyotarō, ed., *Shinryōchi Taiwan* (Our New Territory: The Island of Taiwan) (Tokyo: Kokondō, June 12, 1895), pp.160–202.

¹² Ueno Sen'ichi, “Taiwantō jissen roku (A Practical Guide to Taiwan)”, *Tokyo chigaku kyōkai hōkoku* 13,11 (February 1892): 21–48; Sanbō honbu, ed., *Taiwan shi* (Taiwan Gazetteer) (Tokyo: January 1895).

¹³ For examples, see Hashiguchi Bunzō, “Taiwan jijō (Conditions on Taiwan)”, *Tokyo chigaku kyōkai hōkoku* (Journal of the Tokyo Geographic Society) 17, 3 (1895): 313–8; *Tokyo Asahi Shinbun* 9/29/1895; Iriye Takeshi and Hashimoto Shigeru, “Taiwan banchi zatsuzoku”, *Fuzoku gahō* 130 (1896): 29–30.

¹⁴ Terada Kazuo, *Nihon no jinruigaku* (Japanese Anthropology) (Tokyo: Shisōsha, 1975); The Japanese Society of Ethnology, ed. *Ethnology in Japan: Historical Review* (Tokyo: K. Shibusawa Memorial Fund for Ethnology, 1968); Oguma Eiichi, *A Genealogy of 'Japanese' Self-Images*, trans. David Askew (Melbourne: Trans Pacific Press, 2002); Donald Hoon Ko, *The History of Japanese Anthropology from 1868 to 1970s* (Ph.D. Dissertation, Washington University, St. Louis, 2003); Sakano Tōru, *Teikoku nihon to jinruigakusha: 1884–1952* (Imperial Japan and Anthropology: 1884–1952) (Tokyo: Keisōshobō, 2005).

¹⁵ Kasahara Masaharu, “Inō Kanori no Jidai: Taiwan Genjūmin Shoki Kenkyūshi e no Sokuen (Inō Kanori and His Research Activities in Taiwan, 1895–1906)”, *Taiwan genjūmin kenkyū* 3 (1998): 54–78.

¹⁶ All translations of Japanese sources are by the author. Inō Kanori, “Yo no sekishi o nobete sendatsu no kunshi ni uttau” (I Declare My Sincere Intentions to the Honorable Gentlemen who Have Gone Before), originally published in the November 3, 1895 issue of the *Kyōiku hōchi* and the December 5, 1895 issue of the *Iwate gakuji ihō*, according to Ogino Kaoru, ed., *Inō Kanori: Nenpu, shiryō, shoshi* (Inō Kanori: Chronology, Materials, and Writings) (Tōno, Iwate: Tōno Monogatari kenkyūjo, 1998), p.158; 224; the document itself is reproduced on pp.115–7 of this volume. Moriguchi, also an Iwate-ken scholar who has worked with Inō's papers, thinks the paper was

written not too long after the Treaty of Shimonoseki (4/17/1895); the text is also reproduced in: Moriguchi Kazunari, ed., *Inō Kanori no Taiwan tōsa nikki* (Inō Kanori's Taiwan Expedition Journals) (Taipei: Taiwan fūbutsu zasshisha, 1992), p.306; Inō himself reproduced this document as the preface his pioneering political history of Taiwan: Inō Kanori, “Shōin” (Preface) *Taiwan Shi* (*Taiwan Chronicle*) vol.1 (Tokyo: Bungakusha, 1902), pp.1–5.

¹⁷ Inō Kanori and Awano Dennojō, *Taiwan Banjin jijō* (Conditions among Taiwan's Aborigines) (Taipei: Ministry of Civil Affairs, Records Division, 1900).

¹⁸ The men stationed along the border between the hills and plains to facilitate diplomacy, gather information, regulate forestry and license trade. See Paul D. Barclay, “‘They Have for the Coast Dwellers a Traditional Hatred’: Governing Igorots in Northern Luzon and Central Taiwan, 1895–1915” in *The American Colonial State in the Philippines: Global Perspectives*, edited by Julian Go and Anne Foster (Durham, NC: Duke University Press, 2003), pp.217–55.

¹⁹ Robert Tierney has argued that “at the height of aboriginal resistance to Japanese subjugation campaigns, the aborigines came to be defined by the single custom of headhunting”, precisely the type of crude stereotyping Inō, at least in his pluralist voice, was trying to work against: Tierney, “Going Native”, p.36.

²⁰ A classic example is Edward B. Tylor, “On a Method of Investigating the Development of Institutions; Applied to Laws of Marriage and Descent”, *The Journal of the Anthropological Institute of Great Britain and Ireland* 18 (1889): 246, in which Tylor is able to draw on “roughly 350 peoples”, “ranging from insignificant savage hordes to great cultured nations” to conduct his analysis.

²¹ Inō, *Taiwan Banjin jijō*, p.3.

²² Ibid, p.112.

²³ Emma Teng, “Taiwan as a Living Museum: Tropes of Anachronism in Late-Imperial Chinese Travel Writing”, *Harvard Journal of Asiatic Studies* 59, no.2 (1999): 448.

²⁴ Inō Kanori, “Shinajin no Taiwan doban ni kansuru jinshu teki kansatsu”, *Taiwan kanshū kiji* 5, 8 (August 1905): 52.

²⁵ Douglas Howland, *Borders of Chinese Civilization: Geography and History at Empire's End, Asia-Pacific* (Durham, N.C.: Duke University Press, 1996), p.13–5; Thomas, Julia Adeney, *Reconfiguring Modernity: Concepts of Nature in Japanese Political Ideology, Twentieth-Century Japan*, 12 (Berkeley: University of California Press, 2001), pp.4–42.

- ²⁶ David Scott, *Conscripts of Modernity: The Tragedy of Colonial Enlightenment* (Durham, NC: Duke University Press, 2004), p.79. The term “vindicationism” usefully avoids imputing a precocious “relativism” to Inō and his compatriots.
- ²⁷ Barclay, “Japanese and American Colonial Projects”, chapter 4.
- ²⁸ Inō’s voluminous correspondence has been preserved in the Tōno, Iwate-ken municipal library; letters from famous men like Tsuboi and Yanagita Kunio are prominently displayed in several memorial albums of Inō exhibits; a search of the registers in Tōno and my study of these albums has turned up not one single surviving letter from Gotō Shinpei, a curious circumstance indeed.
- ²⁹ Tsurumi, “Taiwan”, pp.107–8.
- ³⁰ Gotō Shinpei, “Taiwan keieijō kyūkan seido no chōsa o hitsuyō to suru iken”, (Tokyo: Tōa kenkyūjo dai roku chōsa iinkai, 1930) [Reproduced from *Tokyo Nichinichi shinbun* nos.8771–3, 1901] p.27.
- ³¹ Ibid, p.13; Fabian, *Time and the Other*.
- ³² Gotō, “Taiwan keieijō”, pp.29–30.
- ³³ Matsuda, “Ryōtai shoki”.
- ³⁴ Moriguchi Kazunari, ed., *Inō Kanori no Taiwan Tōsa Nikki* (Inō Kanori’s Taiwan Expedition Journals) (Taipei: Taiwan fūbutsu zasshisha, 1992), pp.138, 360–61; Inō, *Taiwan Banjin jijō*, preface, p.3.
- ³⁵ Barclay, “Japanese and American Colonial Projects”.
- ³⁶ Matsuda, “Ryōtai Shoki no Taiwan”.
- ³⁷ Matsuda Kyōko, “Inō Kanori’s ‘History’ of Taiwan: Colonial Ethnology, the Civilizing Mission and Struggles for Survival in East Asia”, *History and Anthropology* 14:2 (2003):179–96.
- ³⁸ Inō Kanori, ed., *Ryōtai jūnenshi* (A Ten-Year History of the Japanese Occupation), (Taipei: Shinkōdō, 1905).
- ³⁹ Hayase, “The Career of Gotō Shinpei”.
- ⁴⁰ The concept is taken from Julia Adeney Thomas, *Reconfiguring Modernity*, p.38.
- ⁴¹ Susan Naquin & Evelyn S. Rawski, *Chinese Society in the Eighteenth Century* (Yale, 1987), p.117.
- ⁴² Fred Y.L. Chiu, “Nationalist Anthropology in Taiwan 1945–1996: A Reflexive Survey” in *Anthropology and Colonialism in Asia and Oceania*, edited by Shimizu Akitoshi and Jan van Bremen (Richmond, Surrey: Curzon, 1999), p.95.
- ⁴³ See my “Cultural Brokerage and Interethnic Marriage in Colonial Taiwan: Japanese Subalterns and Their Aborigine Wives, 1895–1930”, *Journal of Asian Studies* 64, 2 (May 2005): 323–60 for details and sources.
- ⁴⁴ Mochiji Rokusaburō, *Taiwan shokuminchi seisaku* (Colonial Policy in Taiwan) (Tokyo: Fuzanbō, 1912), p.370.
- ⁴⁵ Inō, *Taiwan Banjin jijō*, p.112.
- ⁴⁶ The Government-General’s Bureau of Police issued a map in 1912 that indicates clearly the island-wide intensity of anti-Japanese activity among Taiwan’s Indigenous Peoples throughout the so-called Aborigine territory, Taiwan sōtokufu minseibu banmuhon-sho, ed. *Riban gaiyō* (Taihoku, December 1912).
- ⁴⁷ Peter Duus, “Whig History, Japanese Style: The Minyūsha Historians and the Meiji Restoration”, *Journal of Asian Studies* 33, no.3 (1974): 415–36.
- ⁴⁸ Takekoshi Yosaburō, *Japanese Rule in Formosa*, George Braithwaite, trans. (London, New York, Bombay & Calcutta: Longmans, Green, and Co., 1907), p.230.
- ⁴⁹ Antonio C. Tavares, “The Japanese Colonial State and the Dissolution of the Late Imperial Frontier Economy in Taiwan, 1886–1909”, *Journal of Asian Studies* 64, no.2 (2005): 361–85.
- ⁵⁰ Barclay, “Cultural Brokerage”.
- ⁵¹ Tavares, “Japanese Colonial State”.
- ⁵² Yao, “Governing the Colonised”.
- ⁵³ Kobayashi, “Inō Kanori”.

THE GESTATION OF CROSS-CULTURAL MUSIC RESEARCH AND THE BIRTH OF ETHNOMUSICOLOGY

P. G. TONER

INTRODUCTION

This article examines the development of cross-cultural music research, from its earliest days in the collection, notation and analysis of "primitive music" and "folk songs" to the first annual meeting of the Society for Ethnomusicology in 1956. The gestation period was long, and the birth, like all births, was largely unheralded and was most significant to the immediate family. Now that ethnomusicology is entering middle age, its true significance can perhaps be better appreciated.

The history of cross-cultural music research parallels the history of cross-cultural research more generally, with some interesting and significant differences. As in anthropology, the evolutionist perspective of early ethnomusicology gave way to functionalism and then to more interpretive approaches, but it has been suggested that the discipline has dragged its feet theoretically and theoretical change has been slow.¹ Ethnomusicology has been influenced throughout its history by its two parent disciplines, anthropology and musicology,² a process that has at times been harmonious and, at other times, like a custody battle. And, more than many

other disciplines, the development of ethnomusicology has been closely tied to technological changes such as the invention of the phonograph. There are, then, unique and distinctive lessons to be learned by historicizing cross-cultural music research. In this article, I will attempt to take stock of these lessons, and to consider the impact this field of research has had on cross-cultural research more broadly. I will also consider in some detail how the development of ethnomusicology has influenced Australian Aboriginal ethnography, specifically in northeast Arnhem Land, and the early development of Australian Aboriginal studies.

This article will also consider the place of two pioneering ethnomusicologists who were concerned with the study of Australian Aboriginal music, and whose research represents the end-point of the trajectory to be described below. The American ethnomusicologist Richard Waterman was not the first to make field recordings of Aboriginal music in Arnhem Land, nor was he the first to analyze recordings of Aboriginal music. He was, however, the first ethnomusicologist to conduct long-term, primary research in the region, and to make a substantial number of field recordings. Waterman was a student of Melville Herskovits, who was himself a student of

Franz Boas, and so Waterman is firmly placed within the anthropological branch of ethnomusicology. The Australian ethnomusicologist Alice Moyle probably did more in her distinguished career for the discipline's development in Australia than any other scholar. She was a prolific recordist, although her periods in the field were relatively brief. Moyle began her studies of Aboriginal music under Donald Peart, the first professor of music at the University of Sydney, and her research reveals a firm grounding in the musicological branch of ethnomusicology.

These two scholars began their research on Australian Aboriginal music in the early- to-mid 1950s, roughly coinciding with the formation in the U.S. of the Society for Ethnomusicology. In late 1952 the music scholars David McAllester, Alan Merriam, Willard Rhodes and Charles Seeger met to discuss how to facilitate communication between scholars with common interests; in 1953 they sent out a letter to 66 people to solicit interest, which 10 people signed (including Waterman). The first *Ethno-Musicology Newsletter* appeared later that year. The first annual meeting of the Society occurred in September 1956.³ For the purposes of this article, I take the foundation of this society as formally marking the birth of the discipline known as "ethnomusicology", even though the academic study of non-Western musics is much older.⁴

So what do we have? We have the formation of a scholarly society from a number of diverse origins, whose membership is roughly split between anthropologists and musicologists with a common interest in non-Western and/or folk musics. And we have two scholars of Australi-

an music who began their research on that topic during the same formative period, each representing fairly clearly one of the two orientations which are still with us today — although I will demonstrate that their complex research paths cannot be characterized in a simplistic way. In this article I want to examine the intellectual trajectory that led to that formative period, to examine an unusual and lengthy period of gestation which led to this peculiar and hybrid birth.

EXPLORERS AND PHILOSOPHERS

Attention to music in situations of cross-cultural contact occurred very early in the record of European exploration. The Calvinist missionary Jean de Léry visited Brazil for 10 months in 1557-58, and his book *History of a journey made into the land of Brazil, otherwise called America*, first published in 1578, includes numerous descriptions of musical performance, as well as music notations in the third edition of 1585⁵ — surely among the first studies of non-Western music. Léry describes native Brazilian instruments, dancing that accompanied musical performances, and the singing style of the "savages",⁶ and his account was incorporated into Montaigne's 1580 essay "Des cannibales".⁷

Another early work to consider non-Western music was Charles de Rochefort's *The history of the Caribby-Islands*. Translated in 1666 by the Englishman John Davies of Kidwelly, de Rochefort's book includes a number of passages relating to the music of the Caribbean, including the following:

To divert themselves they also make several Musical Instruments, if they may be so called, on which they make a kind of harmony: Among others they have certain Tabours or Drums made of hollow Trees, over which they put a skin only at one end...To this may be added a kind of Organ which they make of Gourds, upon which they place a cord made of the string of a reed which they call *Pite*; and this chord being touch'd makes a sound which they think delightful. The concerts of divers other Savages are no better than theirs, and no less immusical to their ears who understand Musick. In the morning, as soon as they are up, they commonly play on the Flute or Pipe; of which Instrument they have several sorts, as well polish'd and as handsom as ours, and some of those made of the bones of their Enemies: And many among them can play with as much grace as can well be imagin'd for Savages.⁸

John Barrow's 1806 tome *An account of travels into the interior of Southern Africa in 1797 and 1798* includes the following passage:

It has frequently been observed that a savage who dances and sings must be happy. With him these operations can only be the effects of pleasurable sensations floating in his mind: in a civilized state, they are arts acquired by study, followed by fashion, and practised at appointed times, without having any reference to the passions. If dancing and singing were the tests

by which the happiness of a Hot-tentot was to be tried, he would be found among the most miserable of all human beings.⁹

Although amusing when examined retrospectively, passing references to music do not make an important contribution to the development of ethnomusicology as a field of study, any more than old maps with "there be monsters here" attributed to unknown regions made an important contribution to the development of geography. These passages merely give us a taste of the European mindset which was present as the colonizing powers expanded their grasp around the world.

One early thinker, however, does stand out as having delineated at a very early stage some of the key orientations that would come to define the field of ethnomusicology, and that was Rousseau in his *A Complete Dictionary of Music* of 1779. The ethnomusicologist Anthony Seeger has interpreted Rousseau's work as addressing some of the same questions that have occupied those working in the field ever since. One of these issues is that the transcription of musical sounds is a means to understand the physical laws of music; as Seeger writes, "musical transcriptions reveal certain similar sound processes governed by laws of acoustics",¹⁰ and he points out that careful transcription has been a characteristic feature of most ethnomusicological studies. Rousseau's study itself included transcriptions of Chinese, Persian, Native Canadian and Swiss songs.¹¹

A second pervasive ethnomusicological issue identified by Seeger in Rousseau's work is his emphasis on the cultural interpretation of music, exemplified by a cer-

tain Swiss air which provoked such a strong reaction among Swiss troops that it was banned, although Rousseau states that the transcription itself reveals no musical structures which could be responsible.¹²

In other words, Rousseau identified in his eighteenth-century writing the two dominant orientations of ethnomusicology, a discipline that did not begin to take form for another century: the first geared around careful transcription and musical analysis as the basis for understanding the music of the Other; and the second geared around an interpretation of music based on its cultural context. These sometimes-opposed, but never entirely exclusive, approaches have been a feature of ethnomusicology down to the present day, and are well exemplified by the work of Waterman and Moyle, of which more later.

COMPARATIVE MUSICOLOGY

Ethnomusicology, as it is now known, was first manifested as an organized academic field of study under the rubric "comparative musicology" in the late-nineteenth century. A common view is that one of its founders was the tone-deaf¹³ British physicist A. J. Ellis, whose best-known work was his 1885 article "On the musical scales of various nations". As a physicist and musical enthusiast, Ellis' interest was in using the methods of acoustics to measure the characteristic pitches and intervals of musical scales of different musical cultures with the aim of comparing them. Ellis' starting point, however, was his understanding (as a physicist) that the interval between any two notes:

...is measured properly by the ratio of the smaller pitch number to the larger, or by the fraction formed by dividing the larger by the smaller. When these ratios are known for each successive pair of notes, the scale itself is known, for means then exist for tuning the whole scale when one of its notes is given.¹⁴

As Jaap Kunst explains in a discussion of Ellis' paper, an octave is represented by a 2:1 ratio, a perfect fifth by a 3:2 ratio, and a perfect fourth by a 4:3 ratio. When the two pitches have no lowest common denominator, the ratios become very unwieldy and a logarithmic table is used.¹⁵

His long paper was originally a talk given to the Society of Arts with many accompanying illustrations on various instruments, including a dichord, a number of English concertinas specially tuned to different scales, a sitar, a koto and a set of Chinese bells. The research on which the talk was based was done with instruments obtained privately or through museum collections, some of which had fixed tuning (and therefore which could be investigated independently), and others which had to be played by native musicians; these included the Arabic oud, the Scots highland bagpipe, the Indian sitar and vina, Chinese flutes, mouth organs, gongs and stringed instruments, the Japanese koto, and both slendro and pelog gamelan orchestras. His treatise is full of tables and charts showing the exact frequency measurements and intervals of all of these instruments, as well as scales derived from them. Ellis reveals an ethnomusicological sensibility that is somewhat ahead of his time when he writes that it is necessary to

be a native musician is to hear the real pitch of a musical scale, that there might be considerable variation of such a scale from musician to musician, and that, at any rate, there is a significant difference between knowing the notes used in a piece of music and the musical theory which underpins it.¹⁶

In Ellis' conclusions, he compares the principal intervals used in these different scales from all over the world, proposing how particular intervals were altered to produce distinctive scales, and even suggesting how particular musical features from one region of the world influenced another. For his final conclusion, however, Ellis writes: "the Musical Scale is not one, not 'natural,' nor even founded necessarily on the laws of the constitution of musical sound, so beautifully worked out by Helmholtz, but very diverse, very artificial, and very capricious."¹⁷

So, even though Ellis' starting point and methodology were based on the Western conception of the physics of sound, there is also an explicit recognition that the musical practices of different cultures might be based on quite different principles. Most importantly, it has been observed that his work provided the empirical foundations for comparative musicology.¹⁸

Perhaps the most important aspect of Ellis' work was his development of a system of "cents" for the study of intervals between pitches. To measure non-tempered intervals in terms of ratios of the two pitches in question is cumbersome in the extreme, and to use a logarithmic scale is only somewhat less so. Ellis' most enduring contribution to ethnomusicology was to propose the division of each semitone

in the equal-tempered scale into one hundred equal increments which he called "cents"; although a difference of one cent is impossible to discriminate, Ellis felt that most sensitive ears could register a difference of five cents.¹⁹ By freeing comparative musicology from the need to negotiate complex logarithms, Ellis provided a methodological tool that not only facilitated the comparison of different tonal systems, but also maintained a strong orientation around tonality as a primary concern of the discipline.

A prominent influence on early scholars of non-Western music like Ellis was psychological theory — the idea that the study of various aspects of music, like rhythm or tonality, could reveal principles of the functioning of the human mind. Indeed, comparative musicology and psychology were very closely integrated in the late-nineteenth and early-twentieth centuries,²⁰ and many early research problems were oriented around the investigation of the origins and development of music and universal musical principles.²¹ Carl Stumpf's *Tonpsychologie* (1883 and 1890) was particularly influential, developing a theory of tone sensation based on individual cognition: for instance, examining the perception of similarity and difference in tonal stimuli, which led to a major focus on tonal distance and scale formation advanced by Ellis and many others.²²

As Dieter Christensen has pointed out, Stumpf "believed in the unity of the human mind",²³ and required musical data from all cultures to substantiate his theories. Toward this end, and under his direction, a large collection of phonograph recordings — which eventually became the famous Berlin Phonogramm-Archiv —

were amassed that would be analyzed for the purpose of psychological research.²⁴ The Phonogramm-Archiv also became the basis for the development of a unique form of evolutionary theory focusing on music and, in particular, on pitch and intervallic organization.²⁵ As Eric Ames notes, Stumpf and his colleagues used recordings of non-Western music to emphasize musical evolution. However, evolutionary thought at the turn of the twentieth century took a variety of forms beyond a strict linear model, and Stumpf was able to construct an evolutionary discourse that accommodated notions of both development and geographical diffusion.²⁶

Stumpf's colleagues, notably Erich von Hornbostel and Otto Abraham, continued to investigate psychological aspects of music, based primarily upon the examination of tonal systems, scales, intervals, and the like. Regardless of their commitment to the intricacies of psychological theory, other comparative musicologists certainly maintained a dominant interest in scales and melodies well into the twentieth century. As Stephen Blum notes, for many comparative musicologists "'musical system' was in effect synonymous with 'tone system'".²⁷ Blum quotes Robert Lach as an example:

How does a scale originate? How did the human spirit — in various lands, various times, among various peoples and races — succeed in constructing its musical system, i.e., the sequence of individual scale degrees, according to various specific schemata, or "systems of tonal crystallization", so to speak, which differ so fundamentally from one another — as is evident

from the various scales and tone systems?²⁸

This emphasis on scales, melody, pitch, heterophony and polyphony — all aspects of music associated with pitch or tone — was prominent in much comparative musicology, particularly in the work of Hornbostel.

A well-known example is his theory of the cycle of blown fifths, discussed in detail by Jaap Kunst. Hornbostel's theory has it that an ancient Chinese tone-sequence or scale was developed as follows: a tone of 366 Hz (the so-called *huang chong*, or "yellow bell") was produced on a length of stopped bamboo 230 mm in length and 8.12 mm in diameter. When this tube is overblown, it produces a tone a twelfth above the fundamental, which is then transposed down an octave to give the second tone of the sequence, a fifth above the fundamental; an overblown twelfth above this second tone, transposed an octave down, produces the third tone, a fifth above the second tone; and so on. A cycle produced in this way using pure fifths (702 Hz, a "Pythagorean" interval based on string-measurement) would arrive back at the same note from which it started after 12 jumps. However, Hornbostel noted, since the blown fifth is roughly one-tenth of a tone flat, the cycle is not completed until 23 jumps. Hornbostel postulated that before the Chinese began to construct scales based on Pythagorean principles, they must have developed their scale based on this cycle of blown fifths. Hornbostel and others discovered scales from around the world which could be derived from this principle.²⁹ Hornbostel's complex theory has been criticized since its inception, but it

reveals two key concerns of the comparative musicology of his day: first, a concern with the origins of musical features; and second, a belief that such origins may be revealed through the study of tones, scales and intervals.

Another example is Hornbostel's ideas about the concept of pure melody. Unlike European music, which since roughly 1600 has been based on harmony, all other music is based on pure melody. Hornbostel believed that harmony was superimposed on "natural" traits rooted in the "psycho-physical constitution of man": a natural melodic movement downward ("like breathing or striking, from tension to rest"); small melodic steps of at most a major third; a "melodic unity" with a disregard for fixed scale steps; and a concern with the "distance" between notes — "the size of steps" — rather than their consonance.³⁰

As with Stumpf, there is an undercurrent of evolutionism in Hornbostel's speculation on melody.³¹ He states that short, repetitive melodies of a few notes less than a fourth apart are indicative of "an early stage of development" and are due to "a narrow range of consciousness",³² and that the natural evolution of melody is from this level to a larger number of notes, a greater range, and longer phrasing.³³ Furthermore, in his study of African music, Hornbostel believed that polyphony was a natural development from antiphony (the alternation of solo and chorus singing), probably by accident when the two parts unintentionally overlapped. However, he is clear that this polyphony is the result of a melodic, rather than harmonic, principle.³⁴

Hornbostel's theoretical orientation is well-revealed in a 1933 article called "The Ethnology of African Sound-Instruments". Hornbostel was an advocate of a form of evolutionism, but one which was combined with the geographical diffusion of cultural traits.³⁵ In fact, at certain points he seems to deny the validity of evolutionary theory for the purpose of understanding cultural phenomena. Of the idea that we might reconstruct the history of cultural phenomena, such as musical instruments, by arranging them according to differential features which would reveal their relative ages, Hornbostel writes: "Plausible as this reasoning sounds, its utility as a guide to method is doubtful, and theories of evolution, however ingenious, can contribute little to the classification of cultural phenomena in chronological order, which has always been accepted as one of the most important problems of ethnology."³⁶

In support of this critique of evolutionism, Hornbostel cites two instruments familiar to the Australian ethnographic literature: "the bull-roarer and the boomerang impress us by the subtlety of their technique, yet they belong to the Australian aborigines"³⁷ - which certainly qualifies as a case of damning with faint praise.

And yet, a chronological or developmental approach, whether or not we call it evolutionism, is present throughout Hornbostel's work. He states explicitly that if one phenomenon belongs exclusively to a "primitive" culture and a second belongs exclusively to a "higher" culture, then the first phenomenon is chronologically earlier³⁸ — which seems like a case of circular reasoning. Hornbostel also gives cautious approval to Tylor's concept of

"survivals", but does not agree with the full extent of Tylor's reasoning. Hornbostel writes that Tylor felt that "inorganic and uncomprehended phenomena belong to an earlier stage of development",³⁹ but he is more cautious: "[I]norganic and uncomprehended phenomena are therefore merely an indication that we have to do with older cultural phenomena, but in what connexion, and to what culture they must be assigned, can only be decided on its merits in each individual case."⁴⁰

On the subject of monogenesis vs. polygenesis, Hornbostel is unambiguously clear: it is highly unlikely that any specialized cultural phenomenon could develop independently in two different locations:⁴¹ "[T]here has never, so far as I know, been an historically verified instance of polygenesis in different cultures."⁴² Instead, Hornbostel advocates what he calls "the despised 'diffusion theory'",⁴³ arguing that some cultural phenomena may survive for a long time and may travel great distances. Any specialized cultural phenomenon, according to Hornbostel, must only have developed in one place at one time — as he famously tried to demonstrate through his theory of the "cycle of fifths". Subsequent distance of the phenomenon from the central point of origin indicates relative age.⁴⁴

This leads us fairly directly to Kulturkreise theory, of which Hornbostel was an advocate. The idea is that cultures may be defined by a collection of individual characteristics (architectural, ritual, musical, social, etc.); complexes of characteristics shared by several cultures, called "culture-circles" (Kulturkreise), must have a common origin. These culture-circles have moved outwards from their points of ori-

gin, have mixed, overlapped and developed, resulting in the current situation of cultural diversity.⁴⁵

Another prominent figure in comparative musicology, Curt Sachs, betrayed a more obvious evolutionist stance than Hornbostel. In his *The Rise of Music in the Ancient World: East and West*, Sachs writes from the outset of "the plain truth that the singsong of Pygmies and Pygmoids stands infinitely closer to the beginnings of music than Beethoven's symphonies and Schubert's lieder".⁴⁶ He continues by stating that "the only working hypothesis admissible is that the earliest music must be found among the most primitive peoples".⁴⁷ Later, Sachs writes that:

The songs of Patagonians, Pygmies, and Bushmen bring home the singing of our own prehistoric ancestors, and primitive tribes all over the world still use types of instruments that the digger's spade has excavated from the tombs of our Neolithic forefathers. The Orient has kept alive melodic styles that medieval Europe choked to death under the hold of harmony, and the Middle East still plays the instruments that it gave to the West a thousand years ago...The primitive and Oriental branch of musicology has become the opening section in the history of our own music.⁴⁸

Some of Sachs' writing seems almost comical in its evolutionism by today's standards, but he was by no means alone in his thinking among some scholars of his day. What is somewhat more remarkable is that this evolutionist line persisted into the 1940s, long after it had met a timely de-

mise in mainstream anthropology. How is it that comparative musicology lagged theoretically behind cultural anthropology by at least two decades, when the two disciplines dealt with such similar subject matter?

Sachs' interpretive framework reveals not only an underlying evolutionist conceptualisation of music and its development, but also how the evolution of music is revealed through particular musical structures. As with Hornbostel, melody features prominently as one key to understanding the contemporary music of so-called "primitive" people as representing the precursors to European music. Although, "[t]o the evolutionist, one-tone melodies as a first step before the rise of two- and three-tone melodies would almost be too good to be true",⁴⁹ Sachs acknowledges that the best available information suggested that two-note melodies are the earliest that can be traced.⁵⁰

Sachs refers to these basic melodies which alternate between two notes a short distance apart as logogenic, or word-born. He considers them to be "a mere vehicle for words",⁵¹ and they evolved in an additive way; that is, certain other notes became added to the central core.⁵² Opposed to logogenic melodies were pathogenic melodies, which are due to "an irresistible stimulus that releases the singer's utmost possibilities";⁵³ these are characterized by great force and passion at the beginning of a phrase, only to diminish and weaken toward the end. "Descending melodies", Sachs writes, "recall savage shouts of joy or rage, and may have come from such unbridled outbursts".⁵⁴ In pathogenic melodies, evolution proceeds in a divisive way; that is, the larger vocal

range resulting from savage outbursts leads to the octave and larger intervals like fourths and fifths creating a skeleton for melodic development.⁵⁵

Between these two extremes is melogenic music, which represents a kind of middle ground characterized by elements of both logogenic and pathogenic. At this more developed melogenic level, where melodies tend to have a range greater than a third, particular intervals tend to crystallize, "determined by simple proportions of vibration numbers":⁵⁶ the 2:1 ratio of the octave, the 3:2 ratio of the fifth, and the 4:3 ratio of the fourth. Sachs' reasoning is sometimes far from convincing, as when he writes: "The strongest magnetic power emanates from the fourth — for physiological reasons it is here best to accept without attempting discretionary explanations."⁵⁷

This "magnetic attraction" results in the development of tetrachords and pentachords, and ultimately the complex melodic structures of "highly civilized peoples".⁵⁸

In a further extension of evolutionist principles, Sachs asserts that the earliest stages of music, represented by contemporary "primitive" people, also appears in the babbling songs of European toddlers. Sachs concludes: "Thus we cannot but accept their babbling as an ontogenetic reiteration of man's earliest music and, inversely, conclude that the music of today's most primitive peoples is indeed the first music that ever existed."⁵⁹

This was in 1943. It would be easier to dismiss such speculative thinking if Sachs was not so influential on others in comparative musicology — including, as will be

discussed below, the early thought of Alice Moyle.

Ellis, Hornbostel and Sachs are only three, albeit quite influential, figures in comparative musicology, and it would be seriously misleading to suggest that their views are completely representative of an entire emerging discipline. Like any discipline, there was a great deal of internal diversity, which is suggested by the diverse backgrounds of the early practitioners: musicology, composition, physics, chemistry, phonetics. And yet, in their thought we can see some of the broad outlines which would come to characterize the "musicological" half of ethnomusicology. I have already commented on an underlying evolutionist stance, although this was not equally prominent among all scholars. Another feature was a unitary approach, viewing "the world's musics as a group of separate units, stylistically distinct and internally homogenous",⁶⁰ and therefore able to be characterized as wholes on the basis of very small samples which reflect a single set of principles.⁶¹

The focus on melody, polyphony, scalar structures and other similar musical features has been interpreted by Bruno Nettl as reflecting some of the dominant values of Western music in the late-nineteenth century. In Germany and Central Europe, where many of the early comparative musicologists were based, standard musical training tended to stress intellectually difficult musical structures, such as melodic development and the simultaneous interaction of various parts. When the early scholars of non-Western music began their study, we see particular attention paid to these same features.⁶² Nettl continues:

And if normal music, to us, is polyphonic, in the broad sense, the concept of polyphony was used to show on the one hand that the non-Western music was worthy of attention because it did have, one said rather defensively, polyphony, with people performing together in incomprehensible fashion, nevertheless knowing what they were doing; but on the other hand, most of this exotic music was worthy of study precisely because it was so different, had no polyphony...The idea of a systematic polyphonic practice — and of systematic music making in general — was high on the list of values among our forebears...early ethnomusicologists wanted to show that non-Western musics were systematically organized in good part because they had learned their own music with this value in mind.⁶³

Another feature of early comparative musicology, also related to the "musicological" half of ethnomusicology, was an emphasis on the transcription of non-Western music as an end in itself. Transcribing music onto the Western staff, even after the invention of recording technology, was and still is an important aspect of ethnomusicological training and practice,⁶⁴ although the necessity and accuracy of transcription has always been subject to questioning and debate. Exact notation was not even a feature of Western compositional practice until the late-eighteenth century,⁶⁵ and of course the compression of an entire musical system onto five lines and four spaces representing 12 semitones per octave tends to mould

non-Western music to our own image. Any understanding of a non-Western music which is based on a transcription in Western notation will necessarily encode our own musical values.⁶⁶ Nevertheless, if we think of the transcription of music as itself an interpretive orientation, it must be one of the most pervasive in comparative musicology through its first 50 years.

So, if early comparative musicology came substantially to inform the "musicological" half of ethnomusicology, what was the intellectual trajectory that led to the sometimes-opposed, but always intertwined, "anthropological" half?

Two prominent features of comparative musicology as it developed in late-nineteenth-century Europe are an overall lack of contextualization and a universalizing comparative perspective.⁶⁷ This was implicit in Ellis' large-scale comparison of scales, in Hornbostel's meticulous analysis of recordings from around the world, and certainly in Sachs' assumption of a single developmental framework for all the world's musics. These two features also proved to be a key rift within the scholarly study of non-Western music, and led to the development of a distinctive perspective within American cultural anthropology.

THE ANTHROPOLOGY OF MUSIC

What must be one of the earliest substantial references to music within anthropology appears in Franz Boas' 1888 monograph *The Central Eskimo*,⁶⁸ which contained transcriptions and some analysis of two-dozen Eskimo songs, within the context of a comprehensive ethnography. Not

only did this set the stage for an anthropological approach to the study of non-Western music, but it represents a scholarly effort almost as early as Ellis and Stumpf in comparative musicology. Indeed, Boas and Stumpf worked together in collecting and analyzing Northwest Coast indigenous music, subsequently published by Stumpf in 1886 in *Vierteljahrsschrift für Musikwissenschaft*, one of the earliest journals in comparative musicology.⁶⁹ So it would be a mistake to assume that comparative musicologists and anthropologists operated in isolation from one another. Nevertheless, their overall approaches to the study of non-Western music were very different in many ways. The comparative musicologists were motivated by theories about the origins and structure of music, and analyzed their materials accordingly. Boas' work on music lacked a theory of this type,⁷⁰ and instead fitted into a framework of meticulous detail and ethnographic salvage work.

In addition to the music contained in *The Central Eskimo*, Boas published articles on Kwakiutl, Chinook, Eskimo and Sioux music between 1888 and 1925. Each of them, while focusing on music, contains the elements that have come to be associated with Boasian cultural anthropology. The article "On Certain Songs and Dances of the Kwakiutl of British Columbia"⁷¹ contextualizes several musical transcriptions of songs (presumably done by Boas himself) by including accounts of ritual, song texts, and a very lengthy version of mythology relevant to the songs. His article "Chinook Songs"⁷² contains three brief notations along with 38 song texts and a glossary of several dozen words. Two articles, both entitled "Eskimo Tales and Songs",⁷³ present song texts along

with explanations of dozens of words used in them. His article "Teton Sioux Music"⁷⁴ addresses issues of musical form, including rhythm, phrasing, and structure, and includes 11 quite detailed musical transcriptions. Although he does not approach the technical and analytical detail of the work of some comparative musicologists, Boas was on par with many, and his work on music is under-recognized, probably due to the enormous volume of work on other subjects.

Boas' impact on the cross-cultural study of music is felt most strongly not in his own research and publications, but in those of his students and their students, a legacy which has led to a number of the most significant ethnomusicologists working today. Edward Sapir, the ethnographer, linguist and poet, was also a musician and grew up in a musical family (his father was a professional cantor).⁷⁵ His first in-depth work on songs was in 1910, when he transcribed over 200 Paiute (northern Arizona/southern Utah) song texts⁷⁶ and recorded 120 songs on wax cylinders.⁷⁷ Along with this material, much of it unpublished, was copious contextual information on the performances, people and places associated with the songs.⁷⁸ Sapir's recordings were transcribed by his father, Jacob Sapir.⁷⁹ This research led to Sapir's article "Song Recitative in Paiute Mythology",⁸⁰ which included his own musical transcriptions and musical analysis focusing primarily on rhythm. This represents a notable difference, given the strong emphasis in comparative musicology on matters of melody, scales and intervals.

Some of the fundamental differences between comparative musicological and

cultural anthropological approaches to the study of non-Western music can be found in a 1912 review Sapir wrote of a publication by Carl Stumpf (*Die Anfänge der Musik*). Sapir offers a précis of Stumpf's belief that, in primitive music, notice was taken of "the unified effect of tones sung at consonant intervals", while other intervals "dissonant or relatively so, would in time arise by giving the voice free play within the fourth, fifth, and octave".⁸¹ Sapir criticizes this approach as being difficult to prove or disprove, and as not being based on historical data.⁸² As Sapir writes:

In the nature of things any such theory must be purely speculative, as the use of musical tones is far too ancient a heritage of humanity to yield its genesis to historical reconstruction. Failing historical evidence, a theory of origin can be fully convincing only when so well grounded in psychology as necessarily to exclude all other possible theories. This is hardly the case here.⁸³

Sapir is also critical of Stumpf's over-emphasis (which could be extended much more broadly in the comparative musicology of the day) on "the purely intervallic [*sic*] side of music":

Music is neither purely tone nor purely rhythm. Would it not be more suggestive to think of it in terms of an association of tone production, however it might arise, with the rhythmic impulse manifested in all of man's artistic activities? Granted this impulse and the possession of vocal chords, adjustable for changes of pitch, various forms of musical expres-

sion might be expected to arise. Several paths seem possible, the actual course or courses traversed lie beyond our ken.⁸⁴

And in a similar vein, critiquing Stumpf's handling of melodic structure and musical form, Sapir writes:

I am inclined to doubt whether a purely musical study of this problem would be as fruitful as when taken in connection with song-texts, dance forms, and other features as musical execution is wont to be associated with in practice. The peculiarities of melodic forms are often due to factors that have no direct relation to musical problems as such, as witness our masses, lullabies, and bugle calls. These remarks are meant to indicate the necessity of studying the more complicated problems presented by primitive music in connection with associated cultural features. Stumpf's relative neglect throughout the book of all features that are not strictly musical in character is naturally to a large extent unavoidable, but we must not fail to realize that such one-sidedness may lead us astray in our interpretations.⁸⁵

Overall, Sapir's review is quite favourable; these comments, however, indicate certain matters of interpretation and emphasis that differentiated comparative musicologists from anthropologists, matters that still, to some extent, characterize ethnomusicology.

Another of Boas' students who was very influential in the development of

ethnomusicology was George Herzog. Herzog is an interesting character in this narrative, as he really stands with one foot in each of the two camps that I am discussing. He first studied at the Royal Conservatory in Budapest, and was heavily influenced by the folk music research of Béla Bartók and Zoltán Kodály.⁸⁶ From 1923 until 1925, Herzog worked under Hornbostel at the Berlin Phonogramm-Archiv, the most important archival institution for comparative musicology until the Second World War, and thus was part of the so-called Berlin School of Comparative Musicology.⁸⁷ In 1925 he emigrated to the United States and studied anthropology under Boas.⁸⁸ Thus, in one individual we have the intellectual descendant of a variety of important approaches to the cross-cultural study of music. Bruno Nettl, Herzog's most prominent student, wrote the following about Herzog's letters, which give an interesting insight into his place in the development of ethnomusicology:

Throughout the correspondence one sees the hand of Hornbostel, the comparativist and the archive-builder, of Bartók the careful processor and analyst concerned with authenticity, of Boas the methodical fieldworker, of the confluence of folkloristics, linguistics, and ethnography... The multidisciplinary nature of American ethnomusicology is in part due to his many-sided reach.⁸⁹

Another prominent student of Boas' who was greatly influential in the development of the anthropological approach to non-Western music was Melville Herskovits, whose interest in the relationship between

African and African-American culture was stimulated by the role of music.⁹⁰ Although Herskovits did not publish extensively on musical topics, he supervised the doctoral dissertations of both Richard Waterman and Alan Merriam (who would both be very prominent in the new field of ethnomusicology), and he made field recordings of music in Dutch Guiana, Haiti, Trinidad, Brazil, Dahomey, the Gold Coast and Nigeria.⁹¹

WATERMAN AND MOYLE: CONTRASTING SCHOLARS COMPARED

In this section, I would like to examine the particular theoretical and methodological approaches of two of the earliest scholars of Australian Aboriginal music, Richard Waterman and Alice Moyle. As well as any two ethnomusicologists, their work clearly represents the mixed parentage of the discipline, one anthropological and one musicological — although it is difficult to characterize their work in a simplistic way. Their distinctive approaches to the study of music reveal much of the discipline's past, and also the directions it came to take in the subsequent five decades.

Richard Waterman is best known as a scholar of African and African-American music, and had a distinguished anthropological pedigree through Herskovits to Boas. His doctoral research, completed in 1943, looked at African patterns in the music of Trinidad, which he pursued through a clearly anthropological framework.⁹² He later worked with Herskovits at Northwestern University in Chicago, becoming the founding director of the Northwestern University Laboratory of

Comparative Musicology and supervising the doctoral research of Alan Merriam, who was to become one of the leading figures in ethnomusicology.⁹³ Merriam later wrote of Waterman's work: "overriding all else is a basic orientation toward anthropology; [Waterman] was an anthropologist first and foremost, and his ethnomusicological specialization was almost always handled within the anthropological frame of reference."⁹⁴

This orientation is most clear in Waterman's consistent theoretical interest in cultural dynamics, examining how cultural patterns, especially musical ones, are retained, reinterpreted, or fused over time.⁹⁵ His best-known work in this regard examined the degree to which African-American musics maintained African musical elements.

Waterman was most interested in rhythmic styles and their cultural context; this in itself differentiates him from a great deal of comparative musicology which had a primary focus on tonality, melody and harmony, and the analysis of music in its own terms. His 1948 paper entitled "'Hot' Rhythm in Negro Music" examined the varying tenacity of West African rhythmic patterns in a variety of African-American musics in the New World. Waterman describes "hot" as "one of those subliminal constellations of feelings, values, attitudes, and motor-behaviour patterns"⁹⁶ which manifests itself in mixed metres, percussion polyrhythms, off-beat melodic phrasing, and an overall prominence of percussion instruments.⁹⁷ The degree to which these West African musical characteristics were syncretised with musics in the New World, and the particular nature of the syncretism, depended on the music-

al styles to which they were exposed and, crucially, on the cultural context. In North America, where whites discouraged and disparaged African music and culture, African-American music is largely European in nature, with significant but submerged African features. In contrast, in Central and South America, in which there was a more accommodating cultural context, African musical elements were more evenly blended, or even dominantly West African.⁹⁸ A place like Trinidad exhibits the full range of possibilities, with dominantly British and Protestant areas featuring European-derived religious and folk music with a more subtle African flavour in some areas, and music of African religious cults in others.⁹⁹ From the outset, then, Waterman's work on music pertained directly to understanding the broader cultural patterns of which music was a part.

Waterman's work on African and African-American music was also critical of some of the prevailing attitudes in the academic study of non-Western music. He took issue, for instance, with the notion that Africans were not developed enough culturally to have harmony, and that any instances of harmony in recorded samples were purely the accidental result of overlapping polyphony of different vocal parts. He also lamented the methodology that facilitated such an interpretation, where analysts relied on poor-quality recordings made by others, where true harmonic features may have been masked, rather than on recordings they made themselves.¹⁰⁰ Waterman writes: "That a hypothesis concerning the absence of harmony in African music could have been framed on the basis of early data presented, then, is completely understandable;

how the hypothesis came to be accepted as fact and how it managed to persist to this day are less readily understood."¹⁰¹

In his work on Australian Aboriginal music, Waterman retained his overriding theoretical interest in cultural dynamics and social context, even to the extent that any focus on particular musical features is limited. He made around 15 hours of recordings of Yolngu music during a year's field research in Yirrkala in 1952/53. For the most part, they were elicited recordings of short sequences of songs, perhaps a half-dozen to a dozen, by single patrilineal groups. There are several recordings of oral narrative, and at least two sequences of songs recorded in their ritual context.

Waterman's only published work dealing specifically with Yolngu music was an article in 1955 entitled "Music in Australian Aboriginal Culture — Some Sociological and Psychological Implications". Waterman's primary focus was in examining Yolngu music as an enculturative mechanism, and he drew upon functionalist theory in his interpretations.¹⁰² Much of the article examines how music is learned in childhood, adolescence, early adulthood, and late adulthood, and the functions of music are described as providing recreation, improving morale, increasing and relieving emotional tension,¹⁰³ strengthening kin-group solidarity, acting as textbooks of natural history, history and cosmology, and affirming ties between social groups and totemic species.¹⁰⁴

In terms of musical features, it is curious that Waterman's interest in rhythm, as detailed in his African-American research, does not replicate itself in his Yolngu research (despite considerable

rhythmic complexity). Instead, Waterman turns his attention to subject matter closer to the heart of comparative musicology: melody, scales and intervals. Even here, though, his primary interest is to demonstrate the relationship between melody and sociality, by demonstrating the patrilineal identity of various melodic intervals. Thus, he can assert that: "[a] *karma* cycle of the *ridajigo*-speaking lineage uses the first, the flatted second, and the flatted third of scale; a cycle of the *komaitt*-speaking lineage uses the natural second and flatted third of scale, and one of the *magkalili*-speaking lineage the flatted third and the fourth."¹⁰⁵

If the shift to a focus on melody over rhythm is somewhat surprising, the focus on social context is not. At any rate, the article as a whole does not deal with any musical feature in great detail, concentrating on matters of social function and enculturation.

Waterman's interest in cultural dynamics is more obvious in a paper, co-written with his wife, called "Directions of Culture Change in Aboriginal Arnhem Land",¹⁰⁶ a chapter of a book co-edited by him on the subject of cultural change in Aboriginal Australia. The paper is a critique of the view, widespread at the time, that Aboriginal culture is essentially conservative and unchanging under conditions of cultural contact, leading to cultural breakdown instead of accommodation. The Watermans advance the position that, although certain areas of Yolngu culture, such as their experience of missionary education and religion, demonstrated considerable resistance to change, in other areas of culture Yolngu people were remarkably innovative and willing to accept

new things.¹⁰⁷ In support of this position, the Watermans point to Macassan-derived material culture and cosmology, and the discovery of "new" songs in the Yolngu musical repertoire.¹⁰⁸ The authors conclude:

...that Australian Aboriginals, as exemplified by the people of Yirkalla [sic], have earned their reputation for resisting change only in connection with changes whose motivations they do not understand, changes detrimental to their well-being, and changes that would involve behaviour in opposition to their values and to the principles of their world-view.

Actually, in their own terms, the Aboriginals of Yirkalla are people with open and questioning minds, continually making use of every source of good and valuable suggestions for the modification of their behaviour and willing to consider any innovation on its own merits.¹⁰⁹

In his only other publication on the subject of Aboriginal music, Waterman again adopts a stance reminiscent of comparative musicology in two senses: first, through a focus on melodic structures (although he does pay attention to melodic rhythm); and second, by analyzing a collection of music made by Mountford on Groote Eylandt. In both senses, Waterman would seem to contradict the interpretive stance which he took vis-à-vis African and African-American music in the 1940s and 1950s.

Waterman's focus is to draw some tentative conclusions about Groote Eylandt music based on his melodic analyses of a

small sample of songs. The language which he uses and the style of analysis are strongly reminiscent of early-twentieth-century comparative musicology, and mark something of a departure from his other ethnomusicological work. There is a general interpretive assumption that the analysis of melodic structure, in the sense of counting notes and measuring the intervals between them in terms of Western musical theory, can provide material for general conclusions about the music and its place amongst the musics of the world. Thus, to give but one example, Waterman writes about one particular song in the sample:

Its basic materials are two tone levels, scored G and A. In the first measure the initial notes of the half-measure figures rise to B, but I suspect that this is either a species of beginning formula or the singer using a few notes to find his best pitch. A peculiar feature about this song is the "rise" in measure 12, which is echoed by a high note at the beginning of measures 13 and 14. This intrusive high note — intrusive because it is unique in this particular collection, and because it breaks the consistent pattern of alternating and balancing figurations using G and A, of which the present song is almost entirely composed — reaches to a major seventh above the low note, then slides down a half-tone for its other two appearances.¹¹⁰

What is surprising here is not the nature of the analysis itself, which was common enough in the mid-1960s when the paper

was written, nor the value of such an analysis. What is surprising is that it comes from the pen of Waterman, whose other published work is based so strongly upon an anthropological method, copious cultural context, and a much broader approach to the analysis of musical features.

Waterman himself comments on this in the paper, noting that, although he is generally in the "anthropological" camp of ethnomusicology, one may follow the lead of Kolinski — amongst the most "musicological" of ethnomusicologists — in using other people's recordings, analyzing musical structure, and ignoring the cultural context¹¹¹ — an approach which he had previously characterized as "unfortunate for the development of ethnomusicology as a branch of cultural anthropology".¹¹²

Waterman's conclusions in this paper also situate it within a well-established discourse of comparative musicology. Given that Groote Eylandt music (at least his small sample) uses a small number of notes, he suggests that melody is better understood in terms of pitch levels rather than scales. He writes:

...this is a way of making music somewhat different from the creation of melodies using an array of notes drawn from a series that can be arranged in a scale either by the singer himself or at least by some musically trained investigator...I should like to suggest that, with regard to tonal materials, there exists a hitherto unrecognised musical culture area to go with the scale-type areas generally recognized. To the South Asian-North African microtonal area, the

European-African diatonic area, and the Far Eastern-American Indian pentatonic area (which, incidentally, should be divided and re-characterized), I should like to add an area of Oceania characterized not by scales, but rather by the technique of forming songs out of a very few established levels of pitch.¹¹³

Thus, here we have one of the most prominent "anthropological" ethnomusicologists, a student of Boas and Herskovits, shunning the investigation of cultural context and addressing himself to the kinds of questions which had dominated comparative musicology since the mid-1880s. It is obvious that, despite being able to characterize different approaches to the study of non-Western music, some scholars cannot be completely tarred with one brush.

What, then, of Alice Moyle? In many ways, she was as good an exemplar of the "musicological" side of the discipline as Waterman was of the "anthropological" side. But, like Waterman, she cannot be entirely tarred with one brush either.

One of Alice Moyle's first publications was a book called *Know Your Orchestra*,¹¹⁴ but her first excursion into Aboriginal music was her 1957 M.A. thesis at the University of Sydney entitled "The Intervallic Structure of Australian Aboriginal Singing". The subject matter, overall approach and theoretical inspiration place her squarely within the European tradition of comparative musicology. Moyle's objective was the comparative study of intervallic resemblances among different musical traditions in Aboriginal Australia, with an

eye toward describing them in terms of developmental stages.¹¹⁵

Moyle draws upon some of the most prominent figures in comparative musicology, especially Curt Sachs, positing a relationship between the number of notes used melodically and different stages of singing; that is, fewer notes equals an earlier stage.¹¹⁶ She further uses Sachs' framework of logogenic, pathogenic and melogenic singing, stating that Aboriginal song belongs in the last category due to the presence of well-marked intervals.¹¹⁷ Moyle also refers to Alain Danielou's "Introduction to the Study of Musical Scales", in which he states that intervals can either be divided by numbers (like string lengths), or "by their psychological correspondence such as the feelings and images they necessarily evoke in our minds",¹¹⁸ extending such an idea to Aboriginal singing.

Like so much comparative musicology in the first half of the twentieth century, Moyle devotes a significant portion of her study to the subject of the pentatonic scale, and refers to both Hornbostel and Sachs in her analysis. She notes that the sequence A-G-E-D-C is a "typically Australian" mode of descent,¹¹⁹ and examines a number of other possible pentatonic modes.¹²⁰ She writes, however: "while the intention here is not to suggest regular or systematic pentatonism... — the reverse is nearer the truth in Australian singing — the conglomerations of small 'motives' already described do frequently produce resemblances to the above pentatonic sequences."¹²¹

She continues:

What remains to be said here on the subject of pentatonism is that in songs all over the continent both pentatonic, diatonic (heptatonic), chromatic, even microtonic progressions are demonstrated. And it would seem that all of these are purely vocal modes of singing which can exist side by side. Theories of "evolution" or development are probably no more than theories of emphasis, certain instruments emphasising certain scale progressions more than others. The Australian situation in this regard would closely parallel that at the beginning of Western musical history, long before subsequent experiments in harmony gave to the diatonics that "advanced" status that they had in Western musical theory.¹²²

It is obvious that Moyle has not been swept away by an inordinate focus on the pentatonic scale in so-called primitive music, like some of her predecessors. However, her work is clearly marked by a strong concern for scales and intervals as holding the key to understanding universal principles of musical development.

Tonality — that is, the number of tones used and the way they are used in particular intervallic sequences — is at the centre of Moyle's theory of stages of development in Aboriginal singing;¹²³ for, as she writes, "the rise and fall of melody is surely closer to the core of music than features derived from song-texts or from dancing rhythms".¹²⁴ This is combined with a corresponding theory of musical diffusion in order to speculate on the origins and spread of Australian Aboriginal

music. Moyle refers to Sachs' belief that there was an Asian cradle of music which could be demonstrated by attention to melody, modes and scales.¹²⁵ She writes: "Having traced — thanks to Sachs — the major-third-plus-semitone progression right to Australia's back door, we are now able to follow it further into North East Arnhem Land to the popular Wadamiri [*sic*] song-series which features this interval group in a well-established and striking manner."¹²⁶

And she continues:

...judging by the basic tones and intervals aboriginal singers select, it might well be that here in Australia, and for contemporary ears to hear, is music which belongs to the same deep strata as the sources of both Eastern and Western music. In Australian aboriginal music we may be hearing the same germinating cells from which have come every known style of music.¹²⁷

She then goes on to outline, based on melodic structure, a series of stages in the development of Aboriginal music, from a small range with monotonous repetition all the way through to polyphonic rudiments,¹²⁸ and states that diffusion accounts for similarities between musical regions across the country.¹²⁹ It seems to me that the underlying evolutionist implication of Moyle's thesis was probably imported into her thinking by her theoretical reliance on Sachs more than any other single scholar.

Moyle's primary interest in the intervallic features of Aboriginal music continued in subsequent publications. Her 1959

analysis of Baldwin Spencer's wax cylinder recordings, made in 1901 and 1912, concentrates almost entirely on which intervals may be heard and in which order. The universal significance of particular intervals is suggested through numerous comparisons between the intervals sung on Spencer's cylinders and the ancient modes of Western civilization: "the tones of the "Arunji" corroboree...resemble those of the ancient Greek Hypophrygian, or mediaeval Mixolydian mode"¹³⁰ or "a structural interval of a fourth...is linked conjunctly to one of a fifth...after the manner of the ancient Greek Hypomixolydian and mediaeval Phrygian modes".¹³¹

In 1960, Moyle turned her analytical ear to the earliest recordings of Aboriginal music in existence, made in 1899 and 1903 of the Tasmanian singer Fanny Cochrane Smith. Once again, melodic structure provides the material for her interpretive framework, as it had in her earlier work. Comparing these songs with those of the Vedda of what was then Ceylon — the hands-down winner of the early comparative musicological prize for "most primitive" music — Moyle contends that the Tasmanian songs show a much higher level of organization, based on melodic structure, with "a compass of an octave and seven or eight appreciably different tones" and melodies which "proceed upwards as well as downwards".¹³² And, within the sample itself, the legato-style "Birds and Flowers" song is more musically advanced than the syllabic "corroboree" song, by virtue of melodic phrases which are "balanced above and below a tonal centre".¹³³ In both cases, conclusions regarding musical development and sophistication are made solely on the basis of melody and melodic features, with no consideration of

rhythm or timbre, much less cultural context.

Moyle's grounding in the theory of European comparative musicology also leads her to adopt an explicitly comparative approach to her material, as she details similarities and differences between the Tasmanian material and songs from the west coast of South Australia as well as Central Australia. These comparisons are based on scale tables, a method advocated by Hornbostel, which break down a piece of music into its constituent notes and their relative duration; the most frequently occurring note is taken as the tonic of a scale used in the piece.¹³⁴ Moyle concludes that the Tasmanian songs have a clear tonic and an emphasis given to a triad with the tonic in the middle, whereas the other Aboriginal songs emphasize the fifth note above the triad. These and other melodic features lead Moyle to compare the Tasmanian songs with Melanesian styles of singing¹³⁵ — which is obviously drawing a very long bow.

So, early in her career Alice Moyle was strongly informed by some of the characteristic features of comparative musicology: an overriding interest in melodic and intervallic structures; a negligible interest in rhythmic features; an interest in comparison, musical origins and developmental stages; a reliance on a "laboratory" method of analyzing field collections made by others; an overall lack of focus on cultural context; and a general belief in notation and musical analysis as the foundation of methodology. However, as her career progressed she became much less easy to characterize in each of these areas. A single example, an article from 1968 which was a follow-up on the Tas-

manian recordings, reveals some subtle changes in her approach.

One notable change, which reflects an extra decade of thought on matters ethnomusicological, as well as her first-hand research and recording, is a re-orientation away from the scalar arrangement of pitches toward a focus on melodic contour, which may reveal "significant demarcations of musical style".¹³⁶ To this end, Moyle's standard transcriptions in Western notation are accompanied by graphs outlining melodic contour, with pitch represented on the Y-axis and duration on the X-axis. Of this method Moyle writes:

...the tonal level (or levels) with which the progression of vocal tones most often coincides is seen to emerge in the length and disposition of the horizontal lines. Western designations such as "tonic", "dominant" etc. are thus avoided. Vertical lines indicate "broad" (as against "narrow", or accurately measured) melodic steps or intervals...Pitch/duration graphs have the advantage here of directing attention to tonal movement, rather than to precise pitch.¹³⁷

Another notable change is Moyle's much greater emphasis on the historical record to provide some relevant cultural context; in particular, in examining song texts and historical reports of polyphonic singing in parallel thirds. Although she points out that, on the Australian mainland, polyphonic singing was produced by accidental overlapping of vocal parts (the same interpretation criticized by Waterman with regard to African polyphony), she gives some credence to early historical accounts of Tasmanian singing in parallel thirds.¹³⁸

However, despite these changes in her approach, and new information thereby generated, her conclusions remain virtually the same. The innovative pitch/duration graphs depicting melodic contour are used to compare Tasmanian singing not only with mainland Aboriginal singing, but also with singing in the Solomon Islands, again taking melodic features as the sole basis for intercultural musical comparison. Examining these graphs, along with the historical accounts, Moyle is able to conclude:

Compared with songs recorded on the mainland more differences than similarities have been found in the present study of Fanny Smith's songs. Fanny Smith's Spring Song and Hymn Improvisation show some structural resemblance to a style of singing hitherto observed in parts of Melanesia. And if early evidence for singing in "third parallels" be accepted, further support is thus given to a tentative theory of musical connection between Tasmania and places in the South Pacific.¹³⁹

So, up to this point in her career Moyle continues to develop a speculative theory of musical diffusion, reminiscent of Hornbostel's support of diffusion theory and the monogenesis of cultural phenomena.¹⁴⁰ Although her thought changed over the many years of her work, it can be concluded that Moyle maintained a consistent grounding in the theoretical framework of her early career, and therefore is a good representative of the "musicological" side of ethnomusicology.

CONCLUSIONS

And so to return to my titular metaphor: the gestation of the discipline of ethnomusicology lasted for roughly seven decades, from Alexander Ellis' treatise on the musical scales of various nations in 1885 to the first annual meeting of the Society for Ethnomusicology in 1956. There was a courting period of at least 300 years, beginning with some of the earliest accounts of non-Western music by travellers and missionaries. The period of labour seems to have started between the late 1940s, with the formation of the International Folk Music Council, and late 1952, when the "founding fathers" of the Society for Ethnomusicology first met at the American Anthropological Association conference to discuss forming a new society.

Using very broad strokes (and cognizant of the dangers of doing so), it is possible to characterize some of the predominant concerns of comparative musicology as a focus on the transcription and analysis of musical structures, especially melodic, scalar and intervallic structures, as the key to an understanding of non-Western music that allows for cross-cultural comparison, generalization and speculation about the origins and development of music. Although there is considerable internal diversity, most of the main figures in comparative musicology, such as Stumpf, Hornbostel and Sachs, dealt with these issues in some detail. This intellectual trajectory provided the theoretical and methodological foundation upon which Alice Moyle built her early work on Australian Aboriginal music.

It is also possible to characterize the anthropological study of music in the late-nineteenth and early-twentieth centuries as primarily concerned with the cultural context of musical performance, revealed through the process of salvage ethnography. Scholars like Boas, Sapir and Herskovits lacked any grand theory of music, although they did undertake limited transcription and analysis to complement transcriptions of song texts and detailed accounts of rituals. When they did focus on musical detail, they often singled out rhythmic structures for special consideration. This approach to the study of music paved the way for Richard Waterman's work on both African-American and Australian Aboriginal musics.

This article has many weaknesses. In order to cover much ground, I have had to gloss over each of the scholars mentioned with undue haste, very likely ignoring many of the subtleties of their work. I have left out a great many significant early scholars of non-Western musics, such as Helen Roberts, Frances Densmore, Jaap Kunst, Robert Lachmann and Hugh Tracey. I have had to ignore the entire movement of scholars who studied the folk musics of Europe, including Percy Grainger, Béla Bartók, Cecil Sharp, Marius Barbeau, Maude Karpeles and many others. Perhaps most grievously, I have left unexamined, except in a passing way, the impact of two major developments on ethnomusicology: the invention of the phonograph¹⁴¹ and the development of music archives based on phonographic recordings. Both changed the course of ethnomusicology forever, and must wait for another article.

This article has at least pointed back in time to some of the important factors which led to the intellectual situation of both the founding of ethnomusicology as a discipline and of the earliest significant research on Australian Aboriginal music. The intellectual situation then, with a smaller number of musically inclined anthropologists and a larger number of ethnically inclined musicologists, continues to characterize the discipline today. However, as the cases of Richard Waterman and Alice Moyle demonstrate, the intellectual history of ethnomusicology is quite complex and no individual scholar's development can be analyzed in a simplistic way. Waterman and Moyle each began their work on Australian Aboriginal music just as the scholarly study of non-Western music was in the process of becoming formalized as a discipline, and they were subject to a wide range of theoretical and methodological influences. Each went on to become extremely influential on the development of ethnomusicology in their respective countries as they trained future generations of researchers. As a pair, then, they shed some considerable light on the origins of ethnomusicology and the ways in which it has grown during the subsequent half-century. Now that the discipline is into late middle age, with grown-up children of its own, its intellectual trajectory will no doubt continue on a robust interdisciplinary path for generations to come.

ENDNOTES

¹ Timothy Rice, "Toward the Remodeling of Ethnomusicology", *Ethnomusicology*, vol.31 no.3, 1987, p. 471.

² The distinction in ethnomusicology between "anthropological" and "musicological" approaches is

less relevant now than it was in the past, although it still persists in a variety of ways. I maintain the distinction here only as a heuristic device for the purpose of examining the development of the discipline from these initially distinct disciplinary bases.

³ Charlotte Frisbie, "Women and the Society for Ethnomusicology: Roles and Contributions from Formation through Incorporation (1952/3–1961)" in Bruno Nettl and Philip V. Bohlman (eds), *Comparative Musicology and Anthropology of Music: Essays on the History of Ethnomusicology* (Chicago and London: The University of Chicago Press, 1991), pp.245–6.

⁴ The International Council for Traditional Music (ICTM) is the other major international society dedicated to ethnomusicological scholarship. Founded in 1947 as the International Folk Music Council (Erich Stockmann, "The International Folk Music Council/International Council for Traditional Music: Forty Years", *Yearbook for Traditional Music*, vol.20, 1988, p.1), the birth of this organization could also have been used in this article to mark the beginning of organized ethnomusicology. In either case, the founding of these organizations was merely a symbolic milestone marking the coalescence of a number of scholarly trends that had been developing less formally for decades.

⁵ Frank Harrison, *Time, Place and Music: An Anthology of Ethnomusicological Observation c.1550 to c.1800*, (Amsterdam: Frits Knuf, 1973), p.6.

⁶ Ibid, pp.16–9.

⁷ Philip Bohlman, "Representation and Cultural Critique in the History of Ethnomusicology", in Nettl and Bohlman, *Comparative Musicology and Anthropology of Music*, p.131.

⁸ Cited in Harrison, *Time, Place and Music*, p.56.

⁹ Cited in Harrison, *Time, Place and Music*, p.199.

¹⁰ Anthony Seeger, "Styles of Musical Ethnography" in Nettl and Bohlman, *Comparative Musicology and Anthropology of Music*, p.347.

¹¹ Ibid.

¹² Ibid, pp.347–8.

¹³ The claim of tone-deafness is made by Jaap Kunst, *Musicologica: A Study of the Nature of Ethno-musicology, Its Problems, Methods and Representative Personalities* (Amsterdam: Koninklijke Vereeniging Indisch Instituut, 1950), p.9. In the published paper, Ellis states: "I have been assisted throughout by the delicate ear of Mr. Alfred James Hipkins...without his remarkable power of discriminating small intervals between tones of very different qualities...this paper could not have come into existence...The calculations, the arrangement, the illustrations, as well as the original conception, form my part. The judgment of ear, musical suggestions, and assistance in every

way form his." (Alexander Ellis, "On the Musical Scales of Various Nations", *Journal of the Royal Society of the Arts*, vol.33, pp.486).

¹⁴ Ellis, "On the Musical Scales of Various Nations", pp.486–7.

¹⁵ Kunst, *Musicologica*, p.10.

¹⁶ Ellis, "On the Musical Scales of Various Nations", pp.490–1.

¹⁷ Ibid, p.526.

¹⁸ Alexander L. Ringer, "One World or None? Untimely Reflections on a Timely Musicological Question" in Nettl and Bohlman, *Comparative Musicology and Anthropology of Music*, p.187.

¹⁹ Ellis, "On the Musical Scales of Various Nations", p.487.

²⁰ In this sense, early ethnomusicology bears a resemblance to the early anthropology of Franz Boas, whose doctoral dissertation in physics had a definite psychological angle, examining the psycho-physics of colour perception.

²¹ Albrecht Schneider, "Psychological Theory and Comparative Musicology" in Nettl and Bohlman, *Comparative Musicology and Anthropology of Music*, p.293.

²² Schneider, "Psychological Theory and Comparative Musicology", pp.294–5.

²³ Dieter Christensen, "Erich von Hornbostel, Carl Stumpf, and the Institutionalization of Comparative Musicology" in Nettl and Bohlman, *Comparative Musicology and Anthropology of Music*, p.204.

²⁴ Christensen, "The Institutionalization of Comparative Musicology", p.204.

²⁵ Eric Ames, "The Sound of Evolution", *MODERNISM/modernity*, vol.10 no.2, 2003, pp.303, 315 and passim.

²⁶ Ibid, p.316.

²⁷ Stephen Blum, "European Musical Terminology and the Music of Africa" in Nettl and Bohlman, *Comparative Musicology and Anthropology of Music*, p.10.

²⁸ Cited in Blum, "European Musical Terminology and the Music of Africa", p.10.

²⁹ Jaap Kunst, *Around Von Hornbostel's Theory of the Cycle of Blown Fifths* (Amsterdam: Koninklijke Vereniging Indisch Instituut, 1948), pp.3–5.

³⁰ Erich M. von Hornbostel, *African Negro Music* (London: International Institute of African Languages and Cultures, 1930), pp.7–8.

³¹ Also see Ames, "The Sound of Evolution", p.316 and passim.

³² Hornbostel, *African Negro Music*, p.11.

³³ Ibid.

³⁴ Ibid, p.13.

³⁵ Ames, "The Sound of Evolution", p.316.

³⁶ Erich M. von Hornbostel, "The Ethnology of African Sound-Instruments", *Africa: Journal of the International African Institute*, vol.6 no.2, 1933, p.133.

³⁷ Ibid.

³⁸ Ibid, pp.138–9.

³⁹ Ibid, p.140.

⁴⁰ Ibid, p.141.

⁴¹ Ibid, p.144.

⁴² Ibid, p.145.

⁴³ Ibid, p.146.

⁴⁴ Ibid, p.148.

⁴⁵ Ibid, p.150; also see Ames, "The Sound of Evolution", ff.77.

⁴⁶ Curt Sachs, *The Rise of Music in the Ancient World: East and West* (New York: W.W. Norton & Co., 1943), p.20.

⁴⁷ Ibid, p.21.

⁴⁸ Ibid, p.29.

⁴⁹ Ibid, p.31.

⁵⁰ Ibid, p.32.

⁵¹ Ibid, p.41.

⁵² Ibid, p.52.

⁵³ Ibid, p.41.

⁵⁴ Ibid, p.41.

⁵⁵ Ibid, p.52.

⁵⁶ Ibid, p.42.

⁵⁷ Ibid, pp.42–3.

⁵⁸ Ibid, p.43.

⁵⁹ Ibid, p. 44.

⁶⁰ Bruno Nettl, "Western Musical Values and the Character of Ethnomusicology", *The World of Music*, vol.24 no.1, 1984, p.31.

⁶¹ Ibid, p. 31.

⁶² Ibid.

⁶³ Ibid, pp.32–3.

⁶⁴ Ibid, p.35.

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- ⁶⁵ cf. Lydia Goehr, *The Imaginary Museum of Musical Works: An Essay in the Philosophy of Music* (Oxford: Clarendon Press, 1992).
- ⁶⁶ Nettl, "Western Musical Values", p.36.
- ⁶⁷ Norma McLeod, "Ethnomusicological Research and Anthropology", *Annual Review of Anthropology*, vol.3, 1974, p.102.
- ⁶⁸ Franz Boas, *The Central Eskimo* (Lincoln: University of Nebraska Press, 1964 (1888)).
- ⁶⁹ Franz Boas, "On Certain Songs and Dances of the Kwakiutl of British Columbia", *Journal of American Folklore*, vol.1 no.1, 1888, p.52.
- ⁷⁰ McLeod, "Ethnomusicological Research and Anthropology", p.102.
- ⁷¹ Franz Boas, "On Certain Songs and Dances of the Kwakiutl of British Columbia", p.52.
- ⁷² Franz Boas, "Chinook Songs", *Journal of American Folklore*, vol.1 no.3, 1888.
- ⁷³ Franz Boas, "Eskimo Tales and Songs", *Journal of American Folklore*, vol.7 no.24, 1894, and "Eskimo Tales and Songs", *Journal of American Folklore*, vol.10 no.37, 1897.
- ⁷⁴ Franz Boas, "Teton Sioux Music", *Journal of American Folklore*, vol.38 no.148, 1925.
- ⁷⁵ Robert Franklin and Pamela Bunte, "Edward Sapir's Unpublished Southern Paiute Song Texts", in Regna Darnell and Judith Irvine (eds), *Collected Works of Edward Sapir*, vol.4: Ethnology, (Berlin: Mouton, 1994), p.589.
- ⁷⁶ Franklin and Bunte, "Edward Sapir's Unpublished Southern Paiute Song Texts", p.589.
- ⁷⁷ Thomas Vennum, "The Tony Tillohash Wax Cylinder Recordings and Jacob Sapir's Musical Transcriptions", in Darnell and Irvine, *Collected Works of Edward Sapir*, vol.4, p.663.
- ⁷⁸ Franklin and Bunte, "Edward Sapir's Unpublished Southern Paiute Song Texts", p.594.
- ⁷⁹ Ibid, p. 589.
- ⁸⁰ Edward Sapir, "Song Recitative in Paiute Mythology", *Journal of American Folklore*, vol.23 no.90, 1910.
- ⁸¹ Edward Sapir, "Review of Carl Stumpf, Die Anfänge der Musik", in Darnell and Irvine, *Collected Works of Edward Sapir*, vol.4, p.141.
- ⁸² Ibid, p.141.
- ⁸³ Ibid, pp.141–2.
- ⁸⁴ Ibid, p.142.
- ⁸⁵ Ibid, pp.143–4.
- ⁸⁶ Bruno Nettl, "The IFCM/ICTM and the Development of Ethnomusicology in the United States", *Yearbook for Traditional Music*, vol.20, p.21.
- ⁸⁷ Christensen, "The Institutionalization of Comparative Musicology", p.206.
- ⁸⁸ Nettl, "The IFCM/ICTM and the Development of Ethnomusicology in the United States", p.21.
- ⁸⁹ Bruno Nettl, "The Dual Nature of Ethnomusicology in North America: The Contributions of Charles Seeger and George Herzog", in Nettl and Bohlman, *Comparative Musicology and Anthropology of Music*, pp.271–2.
- ⁹⁰ McLeod, "Ethnomusicological Research and Anthropology", p.102.
- ⁹¹ Richard A. Waterman, "'Hot' Rhythm in Negro Music", *Journal of the American Musicological Society*, vol.1 no.1, 1948, p.24.
- ⁹² Alan Merriam, "Richard Alan Waterman, 1914–1971", *Ethnomusicology*, vol.17 no.1, 1973, p.73.
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- ⁹⁸ Ibid, pp.26–7.
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- ¹⁰¹ Ibid, p.209.
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- ¹⁰⁴ Ibid, p.47.
- ¹⁰⁵ Ibid, p.46.
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- ¹⁰⁸ Ibid, pp.107–8.
- ¹⁰⁹ Ibid, pp.108–9.

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