The Potential for a Canadian Anthropology:

Diamond Jenness's Arctic Ethnography

by

Robert Lorne Alexander Hancock
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the Requirements for the Degree of

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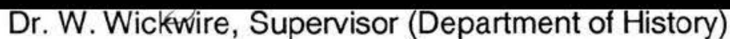
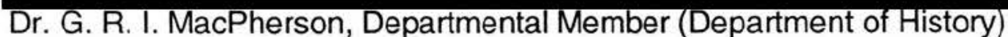


Dr. W. Wickwire, Supervisor (Department of History)



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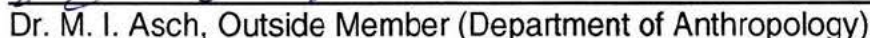
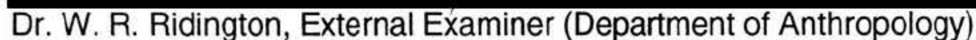


Dr. M. I. Asch, Outside Member (Department of Anthropology)



Dr. W. R. Ridington, External Examiner (Department of Anthropology)

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ABSTRACT

Supervisor: Dr. Wendy Wickwire

Diamond Jenness (1886-1969) was an important figure in the development of professional anthropology in Canada. However, the theoretical content of his ethnographic publications has not been fully recognised. Drawing from both his fieldwork diaries and his published ethnographies on the Arctic, this thesis attempts to understand Jenness in context. This context is constructed through an examination of the following: the influence of his anthropological mentor, R. R. Marett; the significance of his early fieldwork in Papua New Guinea; and an analysis of his exposure to the then-current anthropological innovations, including Bronislaw Malinowski's discussion of fieldwork methods, A. R. Radcliffe-Brown's contributions to the development of functionalist theory, and Franz Boas and Edward Sapir's development of the Americanist theoretical paradigm. This thesis concludes that when compared to these contemporary developments in the discipline, Jenness's Arctic work appears outdated, particularly in its reliance on an evolutionist framework rejected by each of these three new approaches. As such, while his approach was distinctive it was not a viable source for a uniquely Canadian anthropology.

Dr. W. Wickwire, Supervisor (Department of History)

Dr. G. R. I. MacPherson, Departmental Member (Department of History)

Dr. M. I. Asch, Outside Member (Department of Anthropology)

Dr. W. R. Ridington, External Examiner (Department of Anthropology)

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My research has been facilitated by a number of archivists and librarians. Mr. Benoît Thériault, Reference Archivist, Library, Archives, and Documentation Services, Canadian Museum of Civilization, Hull, shared his extensive knowledge of Jenness and of the CMC collections both before and during my research trip to Hull in May, 2001. Mr. Simon Bailey, Keeper of the Archives, University of Oxford, assisted me in procuring copies of the Jenness–Marett correspondence. As well, the reference librarians and circulation staff at the McPherson Library, University of Victoria, made the finding and collecting of interlibrary loan materials less of a chore.

A number of individuals also shared their time and knowledge with me. Dr. Barnett Richling, Chair, Sociology / Anthropology Department, Mount Saint Vincent University, Halifax, generously shared some of his expertise on Jenness, and copies of some Jenness correspondence. Mr. Stuart E. Jenness graciously took time to talk to me about his father while I was in Hull. Dr. Regna Darnell, Professor, Department of Anthropology, University of Western Ontario, London, has always been willing to answer my questions about the history of anthropology in Canada.

Additionally, this thesis benefited from constructive feedback when a very early formulation of its arguments was presented as part of a session entitled, "Revisiting Anthropologic Histories: Nineteenth and Early Twentieth Century Ethnography" at the 101st Annual Meeting of the American Anthropological Association, 28 November 2001, in Washington, DC. I am also grateful for the feedback I received from my graduate student peers when I presented part of my research at a joint Department of History Workshop / Department of Anthropology Colloquium at the University of Victoria, 18 January 2002. In particular, I would like to single out Marc Pinkoski, Interdisciplinary Ph.D. student, University of Victoria, for his unending willingness to engage in discussions of anthropological theory and its political contexts and implications.

Finally, without the love and support of my parents it would have been impossible for me to undertake, let alone complete, this thesis.

I. INTRODUCTION

Diamond Jenness (1886-1969) was one of the most prominent professional anthropologists in Canada before the Second World War. However, his theoretical approach appears to have been at least a generation out of date. In the early 1920s, a period marked in Britain by Bronislaw Malinowski's fieldwork innovations and A. R. Radcliffe-Brown's novel theoretical approach, and in America by Boasian refinements of the culture concept, Jenness was stuck in an evolutionary framework ascendant two decades previously. Jenness had been exposed to both national traditions in his education and his early professional life; and his Arctic ethnography was largely contemporaneous with the developments in British and American anthropological theory and methods. In spite of this, his work displays little awareness of the new approaches, leading to his marginalisation in the history of the discipline.

Educated at Oxford and employed at the National Museum of Canada in Ottawa, Jenness was never located at the centre of either tradition. He was, however, one of the first anthropology students of Robert Ranulph Marett, a leader of the British folk-lore movement and an important figure in the development of British anthropology; afterwards, his direct superior in the Museum was Edward Sapir, one of Boas's most outstanding students and a leading figure in the development of the Boasian paradigm. As a result of his training and employment, Jenness occupies an interesting position at the intersection of the two traditions. Notwithstanding his central role in the history of the discipline in Canada, this position alone would give him an interesting role in

the history of anthropology.

My objective in this thesis is to examine the extent to which Jenness's Arctic work represents a distinctly Canadian approach to anthropological method and theory. Jenness was in a key position, having been trained in the British tradition while working in an Americanist context; he went into the Arctic equipped with British theories and methods, and returned to an Americanist setting to craft his fieldnotes into ethnography. I am interested in the ways in which Jenness's works display the influences of these disparate traditions, and whether or not he represents a distinctive combination of the two.

As well, I am interested in the ways in which his work is similar to or differs from that of his contemporaries, both in Britain and in the United States. Though Jenness came into contact with younger scholars in the 1930s, for example Frederica de Laguna, William Fenton, and Thomas McIlwraith, he did not supervise the training of any students.[1] Nor did he contribute directly to the construction of academic anthropology in Canada. However, he is a significant figure in the history of Canadian anthropology.

This thesis is organised in five parts. First, I outline the historiography of anthropology and assess Jenness's place in the history of Canadian anthropology. Second, I consider the work of Jenness's academic mentor at Oxford, Robert Ranulph Marett, and examine Jenness's first fieldwork, in New Guinea. Third, I analyse Jenness's Arctic ethnography. Fourth, I provide a

[1] For an example of his interaction with younger scholars, see Jenness's correspondence with Fenton (Canadian Museum of Civilization Archives, Ethnology Records, Diamond Jenness Professional Correspondence [I-A-164M] [hereafter CMCJ], box 644, file 25 [William N. Fenton, 1932-1942]).

context for his Arctic ethnography by examining the approaches of Jenness's contemporaries, in particular Bronislaw Malinowski, A. R. Radcliffe-Brown, and Franz Boas and Edward Sapir. Finally, I draw some conclusions about Jenness's method and theory.

The Historiographical Context

George Stocking, the pre-eminent historian of anthropology, first introduced the notions of *presentism* and *historicism* to the discussion of the discipline's past. Presentism, Stocking argued, is a Whiggish approach to history, "a search for the origins of certain present phenomena."[2] This method combines abridgement (that is, not all parts of the past are relevant, and therefore worth studying) with "a normative commitment to the phenomena whose origins are sought"[3] (that is, the choice of relevant materials is based on the investigator's current theoretical orientation). Stocking identified three problems with this method: it seeks to judge, rather than understand, historical developments; it decontextualises elements of the past, leaving itself "prone to anachronistic misinterpretations";[4] and it emphasises progress, making it "less interested in the complex processes by which change emerges than in agencies which direct [change]."[5]

In contrast to presentism and its emphasis on tracing the development of

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[2] George W. Stocking, Jr., "On the Limits of Presentism and Historicism in the Historiography of the Behavioral Sciences," in his *Race, Culture, and Evolution: Essays in the History of Anthropology* (New York: Free Press, 1968 [essay originally published in 1965]), 3.
[3] ibid., 4.
[4] ibid.
[5] ibid.

ideas with current prominence, Stocking offered the concept of historicism, which he defined as "the commitment to the understanding of the past for its own sake."[6] This approach includes those concepts which have lost their relevance over time, to the point that they have no intellectual descendents in the present. Presentists would argue that, because they no longer have any currency, these ideas have no explanatory potential. Stocking, on the other hand, saw value in a fuller understanding of the contemporary contexts of earlier works. For example, he argued that

> E.B. Tylor may speak to present anthropologists, but they will be better able to understand him if they are able to distinguish between the questions he asked which have long since been answered, the questions which are still open, and the questions which we would no longer even recognize as such. ... [T]o approach Tylor in these terms requires a standpoint in the present. But it also requires that we know what the questions are to which Tylor's ideas were answers, and the alternatives which his answers were designed to exclude.[7]

In his formulation, Stocking posited that, when looking at the history of a discipline, an historian (an outsider) will tend to take the historicist view while a practitioner (an insider with vested interests in the discipline) will be more likely to take a presentist approach.[8] "[A]nthropologists," he maintained,

> are more likely to be committed to one side or another, and historians to be (relatively) disinterested observers, and the histories they write are likely to reflect this fact. Each approach has advantages and disadvantages. If historians are less likely to be blinkered by theoretical bias, they are also more likely to suffer from a lack of technical sophistication and relevance; and if an anthropologist's commitment may inhibit understanding of the "losing" side, it can also illuminate issues that remain below the threshold of a more disinterested concern.[9]

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[6] ibid.
[7] ibid., 11.
[8] ibid., 5-6.
[9] George W. Stocking, Jr., "History of Anthropology: Whence / Whither," in *Observers Observed: Essays on Ethnographic Fieldwork*, ed. by George W. Stocking, Jr. History of Anthropology Vol. 1

Throughout, however, he privileged the historicist perspective of the outsider, following Kuhn's notion of paradigms which, in Stocking's interpretation, encourages the researcher "to understand the 'reasonableness' of points of view now superseded, to see historical change as a complex process of emergence rather than a simple linear sequence — in short, to understand the science of a given period in its own terms.[10]

More recently, Regna Darnell has offered a more nuanced analysis of the concepts of presentism and historicism. She argues that an acknowledgment of the context in which ideas are formulated and applied makes for more sophisticated theory. "Chronology matters," she argues; "ideas do not emerge in a vacuum. It is easy enough to criticize earlier work by applying contemporary standards, but at the price of eclipsing the context and continuity of ideas; this price is too high."[11] Her goal is to show that current anthropological theory in North America, which is based in part upon a rhetoric of discontinuity with previous approaches, has strong affinities with the forebears it seeks to disown. As part of this development, she maintains that "[p]resentism in this reflexive sense, choosing issues for historical attention because they still matter today, is fully commensurate with historicism. It is only when we fail to distinguish the contexts of our own theoretical positions from those of the past that presentism becomes a methodological millstone."[12] This is less a rejection of Stocking than

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(Madison: University of Wisconsin Press, 1983), 6.

[10] Stocking, "On the Limits of Presentism and Historicism," 8.

[11] Regna Darnell, *Invisible Genealogies: A History of Americanist Anthropology* (Lincoln: University of Nebraska Press, 2001), 7.

[12] ibid., 1.

a recognition that historical inquiry is guided by both presentist and historicist concerns.

A concern for context has in fact been central to the historiography of anthropology since it first emerged as an area of research. Reporting on the first major meeting of scholars working on the history of anthropology, in 1962, Dell Hymes outlined the characteristics of "truly professional historians of anthropology": "use of out of the way and unfamiliar sources, including unpublished ones, such as letters; attention to textual detail; horizontal sectioning, relating an author to contemporary, including non-anthropological, figures and ideas; in general, a clear sense of historical context, and of historical problem."[13] In an early survey of work on the history of anthropology, Darnell also outlined the relationship between anthropologists and historians, or at least historical methods. She argued that, taken as a whole,

> [t]his literature provides an interesting balance between historians, who tend to set the scholarly standards, and anthropologists, who tend to define the topics of interest. It appears certain that the field will continue to grow both in quantity and sophistication of its productions. Out of this interdisciplinary collaboration a new paradigm for the history of anthropology is emerging.[14]

I envision my current research in the history of anthropology as fitting into this interdisciplinary conception.

The emphasis on historical context has been picked up by more recent anthropological theorists, as they look back to the discipline's past, in a presentist way, to find clues to a way forward. Though they are not interested in the

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[13] Dell Hymes, "On Studying the History of Anthropology," *Kroeber Anthropological Society Papers* 26 (1962), 83.
[14] Regna Darnell, "History of Anthropology in Anthropological Perspective," *Annual Review of Anthropology* 6 (1977): 414.

historiography of anthropology, and while their views might not be accepted wholeheartedly, some members of this generation, including Johannes Fabian,[15] George Marcus and Michael Fischer,[16] and James Clifford,[17] have examined earlier theoretical approaches as they construct their current formulations.[18]

Diamond Jenness and the History of Anthropology in Canada

In the history of anthropology Diamond Jenness can be seen as one of the "dead ends" in the development of the discipline, interesting to historicists but not presentists because he "founded no formal school of Canadian anthropological thought and headed no band of followers."[19] Though he was the one of the most prominent anthropologists in Canada during the period between the two World Wars,[20] he is now largely forgotten. As a result, a strictly presentist approach to the career and works of Jenness would offer little or no insight or explanatory power.

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[15] Johannes Fabian, *Time and the Other: How Anthropology Makes its Object* (New York: Columbia University Press, 1983).
[16] George E. Marcus and Michael M. J. Fischer, *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences* (Chicago: University of Chicago Press, 1986).
[17] James Clifford, "On Ethnographic Surrealism," in his *The Predicament of Culture: Twentieth Century Ethnography, Literature, and Art* (Cambridge, MA: Harvard University Press, 1988 [essay originally published in 1981]), 117-151; Clifford, "On Ethnographic Self-Fashioning: Conrad and Malinowski," in his *The Predicament of Culture: Twentieth Century Ethnography, Literature, and Art* (Cambridge, MA: Harvard University Press, 1988 [essay originally published in 1985]), 92-113.
[18] Darnell, *Invisible Genealogies*, 289-301.
[19] Jim Lotz, "Introduction," in *Pilot Not Commander: Essays in Memory of Diamond Jenness*, ed. by Pat Lotz and Jim Lotz (special issue of *Anthropologica*, n.s. 13:1-2 [1971]), 18.
[20] Henry T. Epp and Leslie E. Sponsel, "Major Personalities and Developments in Canadian Anthropology, 1860-1940," *Na'pào* 10:1-2 (September 1980), 10; Moreau S. Maxwell, "Diamond Jenness, 1886-1969," *American Antiquity* 37:1 (January 1972), 86; Peter Kulchyski, "Anthropology in the Service of the State: Diamond Jenness and Canadian Indian Policy," *Journal of Canadian Studies* 28:2 (Summer 1993), 23. However, Andrew Nurse maintains that Marius Barbeau "was arguably the most prominent anthropologist in Canadian history," or at least "[h]e may certainly have been the best known" (Nurse, "'But Now Things Have Changed': Marius Barbeau and the Politics of Amerindian Identity," *Ethnohistory* 48:3 [Summer 2001], 436, 466n11.).

Born in New Zealand in 1886, Jenness took a degree in classics at Victoria University in Wellington.[21] As a Rhodes Scholar, he went to Oxford to continue his education in Latin and Greek. Upon arriving there, he met a number of anthropology students, including Wilson Wallis and Marius Barbeau, who induced him to take some courses in that field. After receiving a Diploma in Anthropology in 1911, he embarked upon a university-sponsored research trip to New Guinea. On his return to New Zealand a year later he received an offer from Edward Sapir in Ottawa to go to the Arctic as a member of a Canadian government expedition. This was the beginning of Jenness's career in Canada, which culminated in his appointment as Chief of the Anthropological Division of the National Museum after Sapir's departure in 1925.

Barnett Richling, who has studied Jenness in depth, offers three hypotheses for the relative invisibility of Jenness in disciplinary histories. First, he notes, Jenness "worked in the shadow of Sapir"; second, he spent his career in the National Museum of Canada, where the range of his research and his contact with colleagues and students was limited; and, third, he made "contributions to anthropological knowledge [which] were primarily substantive, not theoretical."[22]

However, to say that Jenness is not known as a theorist does not mean

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[21] Jenness's biographical details are drawn from Maxwell, "Diamond Jenness," 86-88; Asen Balikci, "Bio-Bibliography of Diamond Jenness," *Anthropologica* 4 (1957), 37-46; Frederica de Laguna, "Diamond Jenness, C. C.," *American Anthropologist* 73:1 (February 1971), 248-254; Henry B. Collins and William E. Taylor, Jr., "Diamond Jenness (1886-1969)," *Arctic* 23:2 (June 1970), 71-81.

[22] Barnett Richling, "An Anthropologist's Apprenticeship: Diamond Jenness's Papuan and Arctic Fieldwork," *Culture* 9:1 (1989), 71-72; Stuart E. Jenness, "Prologue," in *Arctic Odyssey: The Diary of Diamond Jenness 1913-1916*, by Diamond Jenness, ed. by Stuart E. Jenness (Hull: Canadian Museum of Civilization, 1991), xxvii-xliii.

that his work is atheoretical; rather, it means that his theoretical orientation is less transparent and must be reconstructed from his works.[23] Writing of Marius Barbeau, a colleague of Jenness at the National Museum, Derek Smith has argued that the work of this generation of scholars, the first generation of professional anthropologists employed in Canada, must be analysed and assessed:

> Our task ... is to evaluate Barbeau's work strenuously in terms of critical social theory. We should be able to do this without sentimentality or hagiography. His work demands that attention now — and it deserves the best analysis and evaluation that we can bring to it, for it is being used uncritically and simplistically.[24]

Even though his approach is heavily presentist, I agree with the general thrust of his argument. While both Barbeau and Jenness have been central to the history of anthropology in Canada, they have not been subjected to sustained critical analysis according to their particular approaches and their influence on the wider world around them.[25]

One of the few scholars to examine Jenness in this way is Peter Kulchyski. Writing from a critical-Marxist perspective, and limiting himself to an analysis of Jenness's Arctic diaries and single-volume survey of the *Indians of Canada*, Kulchyski situates his critique in a larger discourse of nation-building. Taking note of the relative paucity of theoretical work on Jenness, Kulchyski

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[23] At the Canadian Anthropological Society Conference, held in Montréal in May 2001, I spoke to a fellow presenter who, when he heard that I was doing work on Jenness, praised Jenness's ethnographies, particularly the Arctic ones, because they were collections of data unmarred by theoretical interjections. This exchange solidified in my mind the need to examine Jenness's theoretical approach.

[24] Derek G. Smith, "The Barbeau Archives at the Canadian Museum of Civilization: Some Current Research Problems," *Anthropologica* 43:2 (2001), 198.

[25] To a certain extent, however, this has begun to change. See, for example, Nurse, "'But Now Things Have Changed'"; Kulchyski, "Anthropology in the Service of the State."

argues that

> [t]his relative silence over a figure of such stature is suspicious in its own right. It is not only an amorphous change in times and attitudes that demand [*sic*] such an examination: the current level of political struggle engaged in by Native Canadians requires historical rereadings. ... Jenness's intellectual project was not unrelated to a project of Canadian national definition that excluded Native peoples. It is also important that the roots of the antagonism between Natives and anthropologists be laid bare, not to exacerbate the rift further, but in the hope that understanding and openness can help to produce a meaningful *rapprochement*. And intellectual history, in which Jenness must occupy a crucial position, is no incidental aspect of this program.[26]

Like Smith, Kulchyski takes a presentist approach to his subject. Before undertaking the sort of work advocated by Smith and Kulchyski it is necessary to understand Jenness in the context of his times, and not simply in the context of current concerns about relations between Aboriginal peoples and the state. While this is obviously important, I will concentrate on understanding Jenness in contemporary terms, with reference to contemporaneous developments in anthropology. I shall leave the analysis of his impact on later developments for others.

Focussing on Jenness's Arctic ethnography, my goal is to assess his contributions to Canadian anthropology. I have chosen to concentrate on his Arctic works because they are the result of his most extensive fieldwork project, and they comprise the largest portion of his published output. As a civil servant working in a period of severe fiscal restraint Jenness was unable, for the remainder of his career, to undertake fieldwork lasting more than two or three months. Additionally, his administrative duties prevented him from devoting as much undivided time and energy to the writing up of later fieldwork. However, I

[26] Kulchyski, "Anthropology in the Service of the State," 24.

should add that I am interested primarily in examining Jenness's work as reflected in his books, diary, and letters. While issues of his character are interesting, I will raise them only in my conclusions, and only as adjuncts to my main argument.

In trying to create a context for Jenness's work, it is impossible to take a strictly historicist view of his work. By its very nature, my contextualisation is based on ahistorical considerations. While I try to avoid questioning why Jenness did not think like his contemporaries, I recognise that this concern is never far from the surface. At the same time, I am interested in Jenness *because* his approach was distinct from that of his contemporaries, and also because it was not passed on to a subsequent generation. With no students to explicate retrospectively his theory and methods, there is an opportunity for me to undertake a kind of detective work, seeking answers to questions which until now have not been asked.

I recognise that however Jenness's approach was viewed in its own time, it needs to be critically assessed to be useful in a current context. To charges that I have engaged in a presentist exercise, I would respond thusly: if Jenness's work is only to be viewed as an historical artefact, then an historicist reading of it is appropriate. From this perspective, his ethnographic writings appear remarkable: he undertook intensive, long-term fieldwork in an era when this was not the norm; he named his informants in his publications; and he published the first major ethnography of the Copper Inuit. If, however, his work is to contribute to current debates, then an analysis in modern terms should be brought to bear

on it. This perspective requires a consideration of more than his ethnography, drawing upon other sources in an attempt to understand the underpinnings of his published work. It is such an analysis which I undertake in this thesis.

This thesis contributes to a wider understanding of the development of professional anthropology in Canada between the two World Wars. Jenness and his works have often been overshadowed by his colleagues at the National Museum, for example Sapir and Barbeau. Although he corresponded with Boas, and worked with both Sapir and Harlan I. Smith, Jenness was never a part of Boas's academic network. Overall, Jenness remained largely a marginal figure in the small world of interwar American anthropology.

Jenness's Arctic work, however, is important in its own right. David Damas has argued that "[t]he most important source for the Copper Eskimo is Jenness ..., whose nine volumes [in the *Report of the Canadian Arctic Expedition 1913-1918*] probably represent the most complete ethnography of any Eskimo group,"[27] while David Riches maintains that Jenness's *The Life of the Copper Eskimo*[28] "might well be regarded as the first recognisably modern anthropological production on the Eskimo, its format of closely researched socio-cultural material, coupled with snippets of local colour and personal experience anticipating such later landmarks as *We the Tikopia*."[29] While it might not be widely remembered or read, it is obviously central to the anthropological canon

[27] David Damas, "Copper Eskimo," in *Handbook of North American Indians, Volume 5: Arctic*, ed. by David Damas (Washington: Smithsonian Institution, 1984), 413-414.
[28] Diamond Jenness, *Report of the Canadian Arctic Expedition 1913-1918, Volume 12, Part A: The Life of the Copper Eskimos* (Ottawa: King's Printer, 1922).
[29] David Riches, "The Force of Tradition in Eskimology," in *Localizing Strategies: Regional Traditions of Ethnographic Writing*, ed. by Richard Fardon (Edinburgh: Scottish Academic Press, 1990), 81.

on the Inuit.

The remainder of this thesis consists of four chapters. In the next chapter, I will outline the background of Jenness's research by discussing his training, emphasising Marett's contributions to the discipline and Jenness's New Guinea research. The third chapter will examine Jenness's Arctic research, including a description of the Canadian Arctic Expedition, a discussion of his fieldwork methods, and an analysis of his Arctic ethnography. This will be followed by a consideration of the contemporary anthropological context, focussing on Malinowski's fieldwork prescriptions, Radcliffe-Brown's theoretical innovations, and Boas's current approaches to the method and theory of ethnological research. Finally, the conclusion will consist of a discussion of Jenness's work as a potentially distinctive Canadian approach to anthropological research. Throughout, my emphasis will be to approach Jenness's work on its own terms. My research on Jenness has not been motivated solely by historicist concerns, however; I have definite ideas about what I consider to be appropriate ways to investigate Indigenous cultures, and these will certainly influence my readings of Jenness's work.

II. TRAINING AND EARLY WORK

Jenness was born 10 February 1886 in Wellington, New Zealand, the youngest of George and Hannah Jenness's fourteen children. He won a scholarship to Victoria University College in his hometown in 1904, and after graduating, in 1908, with first class honours in classics, he went to Balliol College, Oxford, as a Rhodes Scholar to continue his studies in Latin and Greek. Among the people he met when he arrived at Oxford were Barbeau, a Rhodes Scholar from Québec, and Wilson Wallis. Both were anthropology students, and they convinced Jenness to take courses in that field. In 1911 Jenness was awarded an Bachelor of Arts with honours (*Lit. Hum.*) and a Diploma in Anthropology; five years later he was also awarded a Master of Arts degree.[1]

Jenness left only a fragmentary record of his experiences at Oxford, and of the demands placed on him in the Diploma programme. Writing from France, where he was stationed during the First World War, Jenness recollected to Barbeau his impressions of life as a student:

> I laugh sometimes when I think how "staid & grown up" we were in the Oxford Days — solving problems of heaven & earth & letting life slip by under our feet. You in particular burned the midnight oil over Haida crests & clan totems etc. while Wallis showed the Australian Blacks how their society ought to be organised. The only society I shall ever organise will be beside my own hearth — where you & other friends will come to expound your dreams by the firelight.[2]

Though Jenness does not seem to have discussed his training in anthropology, his contemporary, Wallis, published an article outlining the

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[1] Balikci, "Bio-Bibliography," 37; Richling, "An Anthropologist's Apprenticeship," 72-73.

[2] Canadian Museum of Civilization Archives, Marius Barbeau Fonds, Marius Barbeau Professional Correspondence [hereafter CMCB], box B206, file 27 (Diamond Jenness, 1912-1914; 1917-1924), Jenness to Barbeau, 18 May 1918.

programme at Oxford. While Wallis's description is somewhat vague, it does offer a sense of what was expected of the students.[3] Their studies were largely self-directed, with instruction in social anthropology from Robert Marett, in material culture from Henry Balfour, and in physical anthropology from Arthur Thompson. Each student took both oral and written examinations in each of the three topics. As well, students took a wide range of electives, "in such subjects as human geography, comparative religion, psychology, the European Bronze Age, and Egyptology; I think all of us attended the osteology lectures in Medical School."[4] The wide range of instruction would stand Jenness in good stead when he undertook his fieldwork.

R. R. Marett

Jenness's main influence at Oxford was the classicist, philosopher, and anthropologist, Robert Ranulph Marett. Marett had begun his career under the guidance of Edward Tylor, though he would later call into question some aspects of Tylor's evolutionary framework.[5] Though little-known now, Marett commanded respect both from his contemporaries and from commentators. Robert Lowie, in a survey of ethnological theory generally critical of European developments, singled Marett out for praise, asserting that "in post-Tylorian England for poise in the judgment of theories or for a sympathetic grasp of primitive values there is no

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[3] Wilson D. Wallis, "Anthropology in England Early in the Present Century," *American Anthropologist* 59 (1957), 781-790.
[4] ibid., 787-788.
[5] George W. Stocking, *After Tylor: British Social Anthropology 1888-1951* (Madison: University of Wisconsin Press, 1995), 126, 167-168.

superior to this philosophical humanist."[6] George Stocking has called Marett "[s]urely one of the most underestimated figures in the history of British anthropology,"[7] because of Marett's critique of Tylor and James Frazer and because of his role in bridging the gap between British anthropology and French sociology.

There are a number of reasons why Marett does not occupy a prominent place in the history of early British anthropology. On the one hand, Henrika Kuklick argues that Marett "anachronistically practiced nineteenth-century armchair anthropology in the first third of the twentieth century."[8] James Urry, on the other hand, maintains that Marett's classicist approach was overtaken by one based more on the natural sciences, even though the latter approach was not nearly as well suited to the types of studies that were undertaken:

> Although for a time it looked as if those anthropologists with a background in the classical humanities, of whom R. R. Marett ... was the leading representative, would develop the "cultural" side of anthropology, in fact it was natural scientists such as Haddon, Rivers, Seligman and their students who came to dominate the study of society and culture in the years before 1920, often abandoning the study of physical anthropology for the study of "ethnology" in the process. Although poorly qualified to understand the diversity of human social and cultural life, often lacking a background in languages, history or textual analysis, these men were determined to make anthropology a subject in its own right, not merely an adjunct to the comparative study of European cultures. Many classicists were interested in other cultures only in as much as they helped enlighten

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[6] Robert H. Lowie, *The History of Ethnological Theory* (New York: Holt, Rinehart and Winston, 1937), 111. It is important to keep in mind that, at this time, the terms *primitive* and *savage* were (pseudo)scientific descriptions and not necessarily explicit evolutionary judgments of the culture. For example, Lowie, as a leading student of Boas's, would not have been using the term *primitive* here in an evolutionary sense.

[7] George W. Stocking, ed., "Dr. Durkheim and Mr. Brown: Comparative Sociology at Cambridge in 1910" in *Functionalism Historicized: Essays on British Social Anthropology*, ed. by George W. Stocking, Jr. History of Anthropology Vol. 2 (Madison: University of Wisconsin Press, 1984), 109.

[8] Henrika Kuklick, *The Savage Within: The Social History of British Anthropology, 1885-1945* (Cambridge: Cambridge University Press, 1991), 265.

> their established sources; for the natural science anthropologists, savage life and custom became the primary focus of fascination.[9]

Like most of his contemporaries, Marett was, broadly speaking, an evolutionist, though for him "evolutionism was only an ultimate point of reference, not a central organizing concept."[10] In fact, one of the main distinctions between the British and American approaches to anthropology is the former's lack of critiques of the evolutionary framework; "for a long time," Urry argues, "evolutionism remained an implicit aspect of many theories, including those of both Malinowski and Radcliffe-Brown,"[11] the first members of the next generation of British theorists.

Marett's evolutionism was tempered, however, by his emphasis on history. He saw value in the combination of historical and evolutionary approaches, with the historical focussing on an analysis of the formation of current conditions and the evolutionary, which he associated primarily with psychology, concentrating on "the spontaneous origination, the live ... moment of spiritual awakening, that ensues upon the fact of cultural contact and cross-fertilization."[12] Marett emphasised change and movement in his theoretical approach to cultures, privileging psychology as the central framework for understanding humanity. "Human nature, whether savage or civilized," he argued,

> is subject to perpetual transformation. This means that something is

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[9] James Urry, "The Search for Unity in British Anthropology, 1880-1920," in his *Before Social Anthropology: Essays on the History of British Anthropology* (Chur, Switzerland: Harwood Academic Publishers, 1993), 7.
[10] Adam Kuper, *Anthropology and Anthropologists: The Modern British School* (London: Routledge, 3rd ed., 1996), 4; Stocking, *After Tylor*, 126. This marks a significant divergence in the approaches of Marett and Jenness, as I will discuss below.
[11] Urry, "The Search for Unity," 14.
[12] R. R. Marett, "Presidential Address: The Psychology of Culture Contact," *Folk-Lore* 28 (1917), 34.

> always disappearing while something else is coming into being. This law holds good of the most backward of societies no less than of the most advanced. It follows that survivals are no mere by-products of a latter-day civilization, but form an essential feature of human history taken at any of its successive stages and in any of its branching forms. The rate of change may vary according to the special conditions, but not the nature of the process.[13]

Marett's emphasis of the Tylorian notion of survivals marked him as a pre-functionalist theorist, though he took a unique approach to the concept, arguing that the only way to understand them is through a psychological approach, one allowing researchers "to apprehend the present not as an envisaged state but as a felt movement."[14]

Marett's preferred method of psychological analysis was for the anthropologist to attempt to project her- or himself into the collective mind of the primitive society being studied. In a passage that also displays his particular evolutionary approach, Marett outlined his argument in favour of his method on the basis that if his assumptions were faulty then the whole discipline of anthropology would be impossible to conceive. He defended the right of the anthropologist to place himself into the mindset of a different culture on the grounds that all people experience common feelings and emotions:

> Now the fundamental characteristic of savage society and savage mentality is that they are mobbish. But every civilized man has at some time felt and cared as a member of a mob. Granted that the difference of conditions is considerable. For instance, a mob is occasional amongst civilized people; whereas the savage mob is permanent and has a tradition. At the same time, there is enough of the savage in the civilized man, or of the civilized man in the savage — for as much is to be said for putting it in the one way as in the other — to render possible a genuine introjection, that is, a sympathetic entry into the mind and spirit of another.

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[13] R. R. Marett, "Psychology and Folk-Lore," in his *Psychology and Folk-Lore* (London: Methuen, 1920), 14.
[14] ibid., 18-19.

> ... If this were not possible, because the mentalities of different men at different levels of culture are in all respects as incommunicable as their passing dreams, then Social Anthropology would be utterly inconceivable.[15]

Marett's approach was based on a kind of psychic unity, the notion that all forms of human life are open to all humans because of some essential component of the human mind. Most importantly, he recognised the common humanity of all peoples, valorising different modes of life. While he was at least nominally an evolutionist, he did not arrange cultures into a hierarchy of value.

Marett's main area of interest was religion, and he conceptualised his work in this field in terms of translation. His goal, as he formulated it, was "to translate a type of religious experience remote from our own into such terms of our consciousness as may best enable the nature of that which is so translated to appear for what it is in itself."[16] This work would contribute to "a generalized history of the evolution of Man."[17] However, Marett did not offer a comprehensive definition of the term "religion," arguing that "it matters less to assign exact limits to the concept to which the word in question corresponds, than to make sure that these limits are cast on such wide and generous lines, as to exclude no feature that has characterised Religion at any moment in the long course of its evolution."[18] The main factor, for Marett, was that religion is a function of culture; there is no such thing as an innate or inborn religion,[19] and he

[15] R. R. Marett, "The Birth of Humility," in his *The Threshold of Religion* (London: Methuen, 4th ed., 1929 [essay originally published in 1910]), 174-175.
[16] R. R. Marett, "Introduction," in his *The Threshold of Religion* (London: Methuen, 4th ed., 1929, xxiii.
[17] ibid., xxiv.
[18] R. R. Marett, "Pre-Animistic Religion," *Folk-Lore* 11 (1900), 163-164.
[19] R. R. Marett, "A Sociological View of Comparative Religion," in his *The Threshold of Religion* (London: Methuen, 4th ed., 1929), 135.

concluded that material, as opposed to psychological, explanations of religion would always be "palpably incomplete and arbitrary."[20]

For all of his theorising, Marett did not outline a research method as such. Cautioning that primitive societies do not have "a theology, or thought-out scheme of beliefs," he warned researchers to avoid "Why?" questions in favour of "What?" questions lest the researcher, on the assumption that the people have a systematised understanding of their own beliefs, "unawares extract from the native a sort of mock theology, made on the spot, and divorced from the facts of his real life."[21] At the same time, he argued against a theoretical separation of the concepts of religion and magic on the grounds that a firm distinction only becomes apparent "at a later stage of human progress"; as the goal of the researcher is to capture the native "point of view, quite uncoloured by his own," Marett suggested that the term "magico-religious" be used with regard to inquiries and explanations in this area.[22]

The closest Marett came to outlining a method was in an entry on "The Study of Magico-Religious Facts" in the fourth edition of *Notes and Queries on Anthropology*,[23] a handbook for anthropologists and amateur fieldworkers.[24] Marett offered a counter-intuitive prescription, asserting that until a researcher is in "complete sympathy" with the psychology of the native group she or he is

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[20] ibid., 129.
[21] R. R. Marett, "The Study of Magico-Religious Facts," in *Notes and Queries on Anthropology*, ed. by Barbara Freire-Marreco and John Linton Myres (London: Royal Anthropological Institute, 4th ed., 1912), 255.
[22] ibid., 251.
[23] Barbara Freire-Marreco and John Linton Myres, eds., *Notes and Queries on Anthropology* (London: Royal Anthropological Institute, 4th ed., 1912).
[24] James Urry, "*Notes and Queries on Anthropology* and the Development of Field Methods in British Anthropology, 1870-1920," *Proceedings of the Royal Anthropological Institute of Great Britain and Ireland* (1972), 45-57.

studying, "direct questioning of natives can only defeat the attainment of genuine results" in an understanding of the religion.[25] Rather, he suggested that

> [t]he observer must watch quietly for the thousand-and-one little signs that betray the general state of mind, and manage, as it were, to overhear the unspoken feelings and thoughts that attend on the savage when he is intent on his own business. Of course, when it comes to putting these things down on paper, the observer will be obliged to render his impression of the mental attitude of the savage in the terms of civilized thought. Let him, however, take great care to discount the influence of the concepts and categories indispensable for himself as a civilized man, yet unexistent for the savage.[26]

Besides his work on religion, Marett is perhaps best known for his early championing in England of the work of Durkheim. A number of commentators have noted Marett's role in the introduction of Durkheim and his students to British anthropologists, with Stocking going so far as to state that "[a]lthough the anglicization of Durkheim is appropriately associated with the name of Radcliffe-Brown, there was before him another important mediator: Robert Ranulph Marett."[27]

What impressed Marett most was Durkheim's emphasis on the resiliency of the social group.[28] Marett outlined his interpretation of Durkheim's approach, identifying it as a worthwhile new development and also noted with approval Durkheim's attempt to account for an entire social system:

> More significant still is the widespread movement, ... led by Professor Durkheim ..., in support of a method of Anthropology that lays due

[25] Marett, "The Study of Magico-Religious Facts," 257.
[26] ibid.
[27] Stocking, *After Tylor*, 163; Wendy James, "'One of Us': Marcel Mauss and 'English' Anthropology," in *Marcel Mauss: A Centenary Tribute*, ed. by Wendy James and N. J. Allen (New York: Berghahn, 1998), 8-9; R. R. Marett, *A Jerseyman at Oxford* (London: Oxford University Press, 1941), 162-163; Wallis, "Anthropology in England," 789. However, not all observers acknowledge the influence; for an alternative view, see Kuper, *Anthropology and Anthropologists*, 37.
[28] Wallis, "Anthropology in England," 789.

> emphasis on the social factor. The old way was to arrive at the savage mind by abstraction. The sociologist of yesterday was content to picture what the outlook of a man like himself would be, should the whole apparatus of civilization have been denied him, including a civilized man's intellectual and moral education. Naturally his results bordered on romance. The new way, on the contrary, is to proceed constructively. Whilst full account is taken of the effects both of heredity and of the physical environment, yet the effects of the social environment are reckoned to be determinate in an even higher degree. The mass of cultural institutions, it is held, embody and express a kind of collective soul. In this social selfhood each individual must participate in order to realize an individuality of his own. It is a corollary that no isolated fragment of custom or belief can be worth much for the purposes of comparative science. In order to be understood, it must first be viewed in the light of the whole culture, the whole corporate soul-life, of the particular ethnic group concerned. Hence the new way is to emphasize concrete differences, whereas the old way was to amass resemblances heedlessly abstracted from their social context. Which was is the better is a question that well-nigh answers itself.[29]

Like Durkheim, however, Marett never undertook any ethnological fieldwork himself. When Stocking asserts that "Marett's work contributed much to the reformation of British social anthropology; much of what we associate with Malinowski and Radcliffe-Brown is in fact foreshadowed in Marett — whom around 1910 both had read,"[30] he means in a theoretical, not a methodological, sense. Marett saw himself working in a Tylorian tradition, where "[t]he man in the study busily propounded questions which only the man in the field could answer, and in the light of the answers that poured in from the field the study as busily revised its questions."[31] In his section in *Notes and Queries* on collecting magico-religious information, Marett was more direct in his injunction to keep

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[29] Marett, "The Birth of Humility," 173-174. For other examples of Marett's interpretation of Durkheim, see: Marett, "A Sociological View of Comparative Religion," 129-130; R. R. Marett, "A Sociological Review of Comparative Religion," *Sociological Review* 1:1 (1908), 52.
[30] Stocking, *After Tylor*, 170, 172-173.
[31] R. R. Marett, "The Diffusion of Culture," in *The Frazer Lectures 1922-1932 by Divers Hands*, ed. by Warren R. Dawson (New York: Macmillan, 1932), 173-174.

ethnographic data and ethnological theory separate:

> the observer should not intrude, into the actual account of what he has seen and heard, any explanations of the meaning that he has read or imagined, much less any comparisons with our own practices, or with those of which the pages of the comparative anthropologist are so full. Of course, he is at perfect liberty to offer such explanations and conjectures in their proper place, but he should keep them rigidly apart from his descriptions of fact. He should even be cautious in the use of descriptive epithets; thus it is better to write "So and so was said" than "The following prayer was uttered," "The medicine man then pronounced a spell."[32]

More recent commentators, however, have remarked upon the artificiality of this distinction, associated with both Tylor and Frazer, between ethnographic facts and speculative comparison.[33] Fieldworkers needed a strong grasp of theory in order to select the relevant data from the masses they collected, and theorists needed a strong grasp of ethnographic materials in order to select the relevant materials from the masses of data at their disposal.

However, by the end of the first decade of the twentieth century most of the British anthropologists responsible for the training of students recognised the importance of extended field research for the development both of young anthropologists and of the discipline.[34] In fact, Marett asserted in his preface to Jenness's New Guinea ethnography, that "[t]ouring, indeed, proves the ideal method of anthropological research,"[35] though Marett himself did not undertake any extensive ethnographic research; in a letter to Barbeau, Jenness commented that Marett was "a good philosophical anthropologist, but I don't imagine he

[32] Marett, "The Study of Magico-Religious Facts," 253-254.

[33] James Urry "'Facts' to Argument: Structure and Function in the History of Ethnographic Writing in the British Tradition, 1890-1940," in his *Before Social Anthropology: Essays on the History of British Social Anthropology* (Chur, Switzerland: Harwood Academic Publishers, 1993), 43.

[34] James Urry, "A History of Field Methods," in *Ethnographic Research: A Guide to General Conduct*, ed. by R. F. Ellen (London: Academic Press, 1984), 48.

[35] R. R. Marett, "Preface," in *The Northern D'Entrecasteaux*, by Diamond Jenness and Andrew Ballantyne (Oxford: Clarendon Press, 1920), 7.

would score very highly on field-work."[36] In any event, by the 1910s, the strict division between the armchair theorist and intrepid fieldworker was rapidly being replaced by something new: "fieldworker academics"[37] such as Malinowski and Radcliffe-Brown. Despite his experiences in New Guinea and the Arctic, however, Jenness seems to have been entrenched in the former division.

New Guinea Fieldwork and Ethnography

Jenness characterised the beginning of his fieldwork as the end of his time as a student. After leaving Oxford, he wrote to Marett, "[m]y varsity career is all over now. Will you let me say how grateful I am to you for all your kindness through-out — for your staunch championship of my anthrop[ology?] 'mania' & for all the trouble you have had over the expedition. I must succeed in it, if only to justify your faith in me."[38] The troubles that Jenness mentioned were related to the financing of the expedition. As a research student, he needed £250 for his expenses, and Marett worked to raise the funds at the University. One appeal for money emphasised the growing importance of fieldwork to anthropological training:

> The Committee for Anthropology ..., holding that such field-work forms an integral part of its educational programme, and mindful of the fact that other Universities have brought much honour to themselves by equipping

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[36] CMCB, box B206, file 27 (Diamond Jenness, 1912-1914; 1917-1924), Jenness to Barbeau, 25 February 1918.
[37] George W. Stocking, Jr., "The Ethnographer's Magic: Fieldwork in British Anthropology from Tylor to Malinowski," in *Observers Observed: Essays on Ethnographic Fieldwork*, ed. by George W. Stocking, Jr. History of Anthropology Vol. 1 (Madison: University of Wisconsin Press, 1983), 80.
[38] Oxford University Archives, Papers of R. R. Marett, secretary to the Committee [for Anthropology], relating to the anthropological expeditions made by Diamond Jenness to New Guinea and the Canadian Arctic (1904-1920) (DC 1/3/1) [hereafter OUAM], Jenness to Marett, 14 August 1911.

> Anthropological expeditions, makes appeal to the University as represented by the colleges to contribute the not unreasonable sum of money required.[39]

Fortunately for Jenness, the funding eventually materialised, enabling him to finance a year of fieldwork, from December 1911 to December 1912, in the D'Entrecasteaux Islands of New Guinea. His intention was to learn the language and then to collect a variety of things: material culture for the Pitt-Rivers Museum at Oxford, anthropometric data, and ethnographic data.[40] In the last case, his emphasis was on

> the social institutions of villagers scattered around Goodenough and nearby Fergusson Islands, their ritual and economic ties with Trobriand Islanders and other neighboring peoples, and aspects of intellectual culture: religion, mythology, and morality. In these latter subjects his approach was influenced by Marett's predilection for examining the psychology of "primitive cultures," primarily belief systems.[41]

Jenness outlined two main reasons, albeit contradictory ones, for his selection of this part of New Guinea for his field research. First, he characterised it as a locale which had not been subject to previous anthropological research, or, in fact, even to much exploration by white people at all. He pointed out that "this district [when he arrived] was almost entirely unknown. ... [F]ew white men have ever visited its shores. The place, in fact, had an evil reputation, for the natives were reputed to be amongst the worst cannibals in Papua."[42] Second, it was the location of a Methodist mission run by his brother-in-law, Andrew

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[39] OUAM, blank form letter, 01 March 1911.

[40] de Laguna, "Diamond Jenness," 248; Richling, "An Anthropologist's Apprenticeship," 73.

[41] Richling, "An Anthropologist's Apprenticeship," 73.

[42] Diamond Jenness and Andrew Ballantyne, *The Northern D'Entrecasteaux* (Oxford: Clarendon Press, 1920), 11.

Ballantyne.[43] Ballantyne, who spoke Bwaidogan and Dobuan, was familiar with the local cultures and peoples; he would become Jenness's interpreter and collaborator,[44] though he died before the ethnography was completed.

Even though he claimed that this region was largely unknown to the outside world, Jenness selected a cultural group that had already been drawn into the colonial sphere. Richling argues that there is "little indication that [Jenness] expected to meet pristine 'primitive' peoples, untouched by Western culture, in New Guinea,"[45] which may certainly be the case; but Jenness's rhetoric of an undiscovered corner of the world shows that he was willing at least to imply to his readers that these were poorly-known people who had had few visitors from the outside world, rather than address the facts that he spent "most of his time around the Bwaidoga mission"[46] and that "his native informants and acquaintances were typically well-versed in European ways, and heartily suspicious of colonial authorities, traders, and the like."[47] As well, many villages, even those "no white man had even been in," were home to men who had spent time throughout the region labouring for foreign companies.[48] This was not a region cut off from the larger world around it, an issue which will arise again with regard to his Arctic ethnography.

Jenness left behind no field notebooks, so it is difficult to reconstruct his field methods.[49] However, his correspondence with Marett contains some insight

[43] ibid.
[44] Richling, "An Anthropologist's Apprenticeship," 73.
[45] ibid.
[46] ibid.
[47] ibid.
[48] OUAM, Jenness to Marett, 26 July 1912.
[49] Richling, "An Anthropologist's Apprenticeship," 74.

into his approach to gathering materials. Soon after his arrival, he explained that he accompanied Ballantyne on a series of "short excursions" to a variety of regions, including one to take a census and another to investigate charges of cannibalism,[50] setting a pattern of work around the mission station interspersed with short journeys around the islands he would continue for the rest of his time in New Guinea.[51] Three months later, he outlined some of his strategy in undertaking his research:

> My plan is to work out as thoroughly as possible the customs etc of the natives around the [mission] station here. There is quite a large population — this being in fact the most populous part of Goodenough [Island] — & it seems to be very homogenous. Then note the most interesting features of the natives of other parts of Goodenough and Fergusson [Islands], especially the points in which they differ from the natives here. Dobu and Normanly, generally, though it differs fairly considerably in some respects from the N. D'Entrecasteaux — I shall barely touch upon.[52]

In the same letter, Jenness also outlined his typical daily activities when he was working in and around the mission station:

> This is how I'm working at present. Rise about 6:30, breakfast about 8. When possible we have 2 or more of the old men in at the station to talk of their customs etc. (For the last week however they have all been away looking for food in the bush or fishing on the reef or visiting round the coast trying to buy food with tobacco we supply them with. For many of the natives have been sorely pressed, & some of the children & old folks would certainly have died had we not fed them with our own rice and biscuits. ...). About 1 pm we lunch then I go off map-drawing or visiting the villages or taking photos or something. The evening is taken up with writing and reading. Every now & then we take a whole day & go off to more distant villages. We are hoping soon to visit the Amphlettes; also the people in the hills in the middle of Fergusson who have never really been visited. Once or twice a government officer has tried to get at them

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[50] OUAM, Jenness to Marett, 20 January 1912.

[51] OUAM, Jenness to Marett, 11 April 1912. For example, at one point, travelling with the policeman, Jenness claims to have visited thirty villages in fifteen days (OUAM, Jenness to Marett, 26 July 1912).

[52] OUAM, Jenness to Marett, 11 April 1912.

> but they have invariably fled.[53]

Generally speaking, Jenness relied upon a small group of informants, "2 or 3 men who best knew the customs" at a time, to discuss a given topic. Sometimes he used a question-list developed by Frazer,[54] though Jenness recognised that "it only supplied broad lines of enquiry."[55]

Something largely absent from Jenness's field correspondence is mention of anything that might be recognised as participant-observation methodology. The closest Jenness seems to have come to this level of involvement in the life of the Bwaidogans was at a campsite on one of his tours:

> I seemed to get right down into native life. We had sing-songs at night — I copied down many of them. It was weird to sit in the circle round the fire with 20-30 natives about me swaying their heads & bodies to the tune of some mournful chant. Living & sleeping with them the barriers appeared to be broken down. They spoke quite freely of their customs — in fact, took pains to point them out to me. As for songs & legends I have quite a notoriety among them. ... I think this proves the natives have confidence in me & regard me more as one of themselves.[56]

However, this confidence on Jenness's part was short-lived. Later in the same letter, he admitted that "[s]ometimes I fear I have not got into the real native life — it all seems too open & straight-forward but I think I have. I can't think 'native' tho' as I suppose one ought to, much as I try. Oxford skepticism is too much for me."[57] Obviously, he was unable to apply Marett's method.

It is easy to see why Jenness thought that he had not grasped the complexity of the Bwaidogan culture. In a letter written three months before the

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[53] ibid.
[54] J. G. Frazer, *Questions on the Customs, Beliefs, and Languages of Savages* (Cambridge: Cambridge University Press, 1907).
[55] OUAM, Jenness to Marett, 26 July 1912.
[56] ibid.
[57] ibid.

comments above, he asserted that "[s]ociety here seems very simple. There are practically no traditions — their memory reaches at the best to a vague recollection of the days of their grandfathers."[58] He was able to ascertain the presence of classificatory kinship, of patrilineal descent, and of taboos and their methods of inheritance.[59] As well, he found that "[p]roperty seemed at first equally simple — descending regularly to the eldest son. But in working out two or three cases genealogically curious anomalies arose which so far remain unfathomed,"[60] in particular, cases of exogamous marriage without prescriptive rules for post-nuptial residence. In any event, he was able to conclude that "I believe the information I am getting is sound. Ballantyne knows the language well & we are taking great pains to check."[61]

His relationship with his brother-in-law seems central to have been central to the success of his research project. Jenness frequently acknowledged the value of Ballantyne's facility with the local languages.[62] In fact, within a month of his arrival, he had asked Ballantyne to collaborate on the research.[63] Ballantyne's experience with the language made him well-suited to undertake a philological analysis, which Jenness says he himself "could not touch."[64] Jenness's reticence in undertaking philological work, and his difficulty learning the local languages, is curious given his extensive training in Greek and Latin.[65]

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[58] OUAM, Jenness to Marett, 11 April 1912.
[59] ibid.
[60] ibid.
[61] ibid.
[62] OUAM, Jenness to Marett, 11 December 1911, 29 January 1912; CMCB, box B206, file 27 (Diamond Jenness, 1912-1914; 1917-1924), Jenness to Barbeau, 06 May 1912.
[63] OUAM, Jenness to Marett, 29 January 1912.
[64] ibid.
[65] Richling, "An Anthropologist's Apprenticeship," 74.

Jenness's experience working with his brother-in-law was an unexpected help to his research. Upon arriving in New Guinea, Jenness discovered an equally unexpected hindrance to his work. The islands on which he concentrated his research were undergoing one of the worst famines on record.[66] Jenness lamented that "[t]he famine makes anthropological work very slow. The natives spend every hour of the day in their gardens or hunting for food in the bush or fishing on the reefs. Still it is going slowly ahead."[67] At one point he asked Marett plaintively, "[w]hy did not I come a year earlier? — it would have made anthropologizing much easier."[68] Richling argues that this famine led Jenness to develop "an appreciation for the precariousness of local subsistence, the fine line between well-being and disaster, and the mitigating role of mutual aid."[69] These were all lessons which would come in handy for Jenness during his time in the Arctic.

Preparation of the D'Entrecasteaux manuscript for publication was delayed by several unforeseen events: the Arctic trip, participation in the Canadian Expeditionary Forces from 1917 to 1919,[70] and Ballantyne's untimely death.[71] On leave from the CEF during demobilisation, Jenness returned to Oxford to finish the manuscript.[72]

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[66] OUAM, Jenness to Marett, 26 July 1912.
[67] OUAM, Jenness to Marett, 04 May 1912.
[68] OUAM, Jenness to Marett, 26 July 1912.
[69] Richling, "An Anthropologist's Apprenticeship," 73. Some of the other problems Jenness faced in New Guinea seem more prosaic. For example, he asked Marett to "[t]ell Prof. Thompson not to be too critical over my [anthropometric] measurements. The long frizzy hair makes it difficult to be correct within a millimetre or so & one is sometimes puzzled how much to allow for the hair" (OUAM, Jenness to Marett, 06 December 1911).
[70] Marett, "Introduction," 8.
[71] Jenness and Ballantyne, *The Northern D'Entrecasteaux*, 12.
[72] Collins and Taylor, "Diamond Jenness," 74.

In the end, *The Northern D'Entrecasteaux* turned out to be a rather unremarkable ethnography, an assessment shared by Jenness himself.[73] Its most interesting feature is what it reveals about Jenness's views and values: his desire to depict the New Guinea peoples in metaphorical terms that reflected "civilised" ways, on the one hand, and "savage" ways on the other hand. Jenness emphasised the latter, based on what he saw as a lack of common-sense knowledge and a preference for cannibalism.

Jenness's metaphors cover all and sundry practices. Villagers rouse wild pigs from their hiding places "with their musical shouting, like English boys who call out in the early morning to frighten the birds away from the corn."[74] Men (and, as it turns out, women as well) tally their successes at romance in the same way that war veterans and Indians count their victories in war medals and scalps, respectively.[75] A man with ceremonial knowledge or power "may set up his name-plate and advertise his special department."[76] In his discussion of land ownership, Jenness used English concepts such as "land titles," "alienation," and "usufruct"[77] to describe native practices, rather than attempting to interrogate the native concepts. The latter should not have been difficult with an interpreter as capable as Ballantyne.

At times, the congruency between the native culture and the "English" culture with which Jenness so readily identified began to crumble. For instance, he asserted that "[e]ven from our standpoint the natives would be regarded as

[73] CMCJ, box 648, file 6 (A. L. Kroeber, 1930-1939), Jenness to Kroeber, 11 January 1932.
[74] Jenness and Ballantyne, *The Northern D'Entrecasteaux*, 20.
[75] ibid., 61.
[76] ibid., 73.
[77] ibid., 71, 72.

exceptionally clean people, but for one or two customs that seem repulsive."[78] He highlighted certain groups' "improvidence", for example: "[t]he Olegana people allowed their domesticated pigs to root up the sweet potatoes, though they knew that food was scarce. The Bwaidoga natives, again, planted sweet potatoes when the yam crop failed, but omitted to erect any fences around them; later they complained that the wild pigs ravaged their gardens."[79] Again with regard to yams, Jenness presented the natives as acting like impudent children:

> If she were vexed a woman could neutralize the singer's [yam] incantation and rob his magic of its power. She would be injuring herself, of course, for her own yam crop would suffer with all the rest. But the native does not stop to think of this. He wants an outlet for his anger, and vents it on the nearest object.[80]

At the same time, Jenness never questioned the insights or purported superiority of his own "English" approach to the world. If only the natives were more like Englishmen they would be more successful:

> Nuatutu offers an almost ideal site for a group of hamlets, being the central point where north and south converge to face the trade region of the east. The soil is fertile, and the reefs along the coast and round the Barrier Islands closeby teem with fish. But for two miles on either side there is no stream, and this has effectively prevented the place from being occupied. Probably a well sunk at the foot of the hill would strike good water, but the natives have no knowledge of well-drilling.[81]

It would appear that Jenness was practicing a variant of what Marett called "old sociology," where the researcher "was content to picture what the outlook of a man like himself would be, should the whole apparatus of civilization have been

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[78] ibid., 206.
[79] ibid., 208.
[80] ibid., 125. I find Jenness's shift in gender from the specific "she" to what I consider a generic "he" to be evidence that he felt all the natives capable of such a reaction — it is no longer just the province of an hysterical woman (which would be one interpretation of the continued use of the pronoun "she"), but a typical response of any of the natives.
[81] ibid., 45.

denied him, including a civilized man's intellectual and moral education."[82] In contrast to this, Marett argued, based on his reading of Durkheim, that

> no isolated fragment of custom or belief can be worth much for the purposes of comparative science. In order to be understood, it must first be viewed in the light of the whole culture, the whole corporate soul-life, of the particular ethnic group concerned. Hence the new way is to emphasize concrete differences, whereas the old way was to amass resemblances heedlessly abstracted from their social context.[83]

Jenness's use of metaphor, as a function of his perception of the natives as capable of being just like Englishmen with the aid of a little civilisation, shows how much his approach differed from that proposed by his teacher.

I will offer one final example. In discussing the mental faculties of the natives, Jenness paid them rather a backhanded compliment, saying that

> it seemed to us that, taken in the mass, they are not markedly inferior to white people whenever their interest is aroused. They are keenly observant of all natural phenomena, and there are few birds or fish or plants whose name even a small boy does not know. This closeness of observation is especially noticeable in all that pertains to fishing and to the gardens, and has led to the creation of an extensive vocabulary connected with these pursuits; the varieties of yams and shell-fish for example appear to the foreigner numberless. Some natives, again, display wonderful accuracy in locating sounds, and without the slightest hesitation will lead the way through half a mile of dense forest to the exact tree on which a blue pigeon sits cooing.[84]

These were obviously not stupid people — they had come to terms with their environment, and developed a set of tools with which to make the most of their surroundings. Yet Jenness called them stupid when they showed no desire to learn the ways of his world:

> No one, however, can be more stupid than an uninterested native. Many things which appeal to us have no interest whatever for him. At the

[82] Marett, "The Birth of Humility," 173; see n. 58, above.
[83] ibid., 174.
[84] Jenness and Ballantyne, *The Northern D'Entrecasteaux*, 52.

> mission station the best boat boy and the best hunter could never be taught his twice times tables. He tried, and tried hard, but somehow it did not appeal to him, and he would forget to-morrow all that he learned to-day. Another lad, of a rather similar type, by sheer application and force of will learned his arithmetic tables, because he knew that otherwise he could not go up to the mission station at Ubuya.[85]

We are left with a paradox — the natives were untouched by civilisation but needed math and writing to succeed. Jenness interpreted the lack of desire to learn math as stupidity, and did not recognise that he was there in a particular historical moment, when there was no exigency to learn the tools of the white world because the hunting and boat-building boys could still survive with the tools suited to their world. The other boy, perhaps, had more to gain and less to lose by grasping onto the new ways rather than the old ways.

While disturbing, this discussion of learning shows how Jenness felt that the natives still held the possibility of becoming English, provided they set their wills to the task. However, when he shifted his attention to cannibalism the natives ceased to be Englishmen *manqué* and became complete and total savages. Writing about a famine which hit the area in 1900, Jenness told of a time when cannibalism was so prevalent that "[i]t was dangerous for a child to leave his parent's side for a single moment lest he should be carried off to swell the cannibal pots."[86] He detailed "the last case" of a cannibal feast to occur in one of the villages he visited:

> the victim ... was trussed with ropes and bound round with torches. He knew he was going to be burnt alive, so he began to count aloud all the enemies he had slain. Not a groan escaped him in agony, though he wriggled once or twice, and even his *inhuman torturers* could not but admire his courage. As a rule the body was cut into segments and laid

[85] ibid., 52-53.
[86] ibid., 32.

> upon a mat; nothing, not even a drop of blood, was allowed to escape, lest the victors should be cheated of part of their revenge. Each family received a portion, which it cooked like *ordinary meat*, and every one down to the smallest child shared in the feast.[87]

In the process of killing and eating a victim, the natives turned from average, if stupid, people into inhumans, from people who ate normal food into inverted people who treated the least natural type of meat, human flesh, as if it were any other kind of meat. The only other people less human were the women who engaged in cannibalism outside of the sanctioned revenge feasts:

> Certain women, the Mud Bay natives say, often try to exhume the body during the night, and cut off part of the flesh for food. They call these women *kwala*, and hold them in great abhorrence. A Kukuya woman even now is much given to this practice; one of her kinsmen refuses to eat her food on the ground that her pots are tainted. In 1911 a woman was discovered at Kabuna one night trying to dig up the corpse of a man named Tomobwaina, but she managed to make her escape without being recognised. They might have killed her had they seized her; in any case she would have been roughly handled. No desire for vengeance actuates these women, only a deprived appetite for human flesh; the practice therefore will soon die out, now that no cannibalism is ever allowed to occur. Men say these *kwala* cut off the thighs and shoulders, which they cook and eat in secret; the blood they catch in bamboo tubes and drink. *They derive no magic power apparently from their ghastly banquets.*[88]

These women operated outside the standard system of value, seeming to gain nothing from their "ghastly" routine (therefore making them inhuman?); even the participants in the sanctioned cannibal feasts gained the satisfaction of revenge against a vanquished foe. Jenness made no attempt to understand what motivated these women; given, however, that even other members of the community did not seem to understand what motivated the women, it seems unlikely that Jenness would have been able to come to an understanding of

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[87] ibid., 88 (emphases added).
[88] ibid., 119 (emphasis added).

these women as human beings motivated by certain human needs. On the other hand, they might have been unwilling to share their knowledge with Jenness, a problem he encountered during his time in the Arctic.

However, Jenness's ethnography was largely ignored when it was published. Stocking has evaluated it in positive terms. He views Jenness as one of the few ethnographers to undertake a Haddonian "intensive study of a limited area" prior to the First World War. Placing Jenness in a group that includes Radcliffe-Brown, Malinowski, and another student of Marett, Marie Czaplicka,[89] Stocking notes that in the years that followed Malinowski and his work overshadowed all these others. He ascribes this dominance of Malinowski's work in the history of anthropology to "biographical accident," to the "institutional marginality" of the other scholars, and, most centrally, to the fact that the others' "monographs were slow in appearing and conventional in style, and they seem not to have thought of or represented themselves as ethnographic innovators."[90] These factors, among others, affected the international reception of Jenness and his work.

This is not to say that Jenness was not read at all, though. Malinowski cited his work. In a disparaging tone he took Jenness to task, in *Argonauts of the Western Pacific*,[91] for the latter's interpretation of missionary influence on the cessation of religious customs which had no meaning, writing that "[i]t is strange to find a trained ethnologist, confessing that old, time-honoured rites have no

[89] Stocking, *After Tylor*, 119.
[90] ibid.
[91] Bronislaw Malinowski, *Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelagoes of Melanesian New Guinea* (London: Routledge and Kegan Paul, 1922), 467n.

meaning! And one might feel tempted to ask: for *whom* it is that these customs have no meaning, for the natives or for the writers of the passage quoted?"[92]

On the other hand, Alfred Kroeber was more positive in his assessment, sending Jenness a short note in early January of 1932: "I want to congratulate you belatedly on an unusually fine piece of work. Not only are the data good, but the writing is compact, pregnant, and well-rounded. I feel you have done easily one of the best pieces of work extant on Melanesia, and am sorry I had not made its acquaintance before."[93] In his response to Kroeber, Jenness characterised his New Guinea manuscript as being the hurriedly written and revised result of his first fieldwork experience.[94] He would come to see the results of his second fieldwork trip, to the Arctic, as much more substantial.

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[92] ibid.; for Jenness's reaction to Malinowski's critique, see CMCJ, box 648, file 6 (A. L. Kroeber, 1930-1939), Jenness to Kroeber, 11 January 1932.
[93] CMCJ, box 648, file 6 (A. L. Kroeber, 1930-1939), Kroeber to Jenness, 04 January 1932.
[94] ibid., Jenness to Kroeber, 11 January 1932.

III. THE CANADIAN ARCTIC EXPEDITION

For almost a century, between the 1840s and 1920s, a number of scientific exploring parties joined whalers in bringing the wider world to the Arctic. Before this time, commercial needs had motivated Arctic exploration; one scholar notes that "[e]arly attempts at [finding] a Northwest Passage had been undertaken to get around the obstruction that North America represented to European trade with the Far East, and early attempts to reach the North Pole had been undertaken to reach the Far East by that direction."[1]

However, the fact that most of these expeditions took place under foreign flags raised serious issues about who held sovereignty over the Arctic, particularly given that so much of it remained unexplored.[2] By the 1870s, the British government was receiving requests for land grants in the Arctic, usually from Americans. The British did not want to get involved, but at the same time were afraid that if they "disclaimed jurisdiction, the United States would immediately claim the territory for itself and interfere with future Canadian expansion in that direction."[3] Canadian politicians were divided over a British offer of jurisdiction over the area, and formal transfer was delayed by indecision on both sides, with the British wondering about the most desirable way to transfer control and the Canadians wondering if they wanted control at all.[4] Finally, in 1895, after more than twenty years of discussions and delays, a bill was passed

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[1] Alan Cooke, "A Gift Outright: The Exploration of the Canadian Arctic Islands After 1880," in *A Century of Canada's Arctic Islands 1880-1980*, ed. by Morris Zaslow (Ottawa: Royal Society of Canada, 1981), 53.
[2] Morris Zaslow, *The Opening of the Canadian North 1870-1914* (Toronto: McClelland and Stewart, 1971), 251.
[3] ibid., 251-252.
[4] ibid., 252-254.

in the Canadian parliament "constituting the Provisional Districts of Ungava, Franklin, Mackenzie, and Yukon."[5] There were still questions about jurisdiction, however, as the Canadian government had replaced a series of vague proclamations and laws with one that was equally unclear about what, precisely, was being claimed.

Concerns over the potential of continued challenges to Canadian sovereignty in the Arctic finally forced the federal government into action. In 1897, the government sent an expedition to Hudson's Bay and Baffin Island;[6] in 1903, it established North West Mounted Police posts on Hudson's Bay and at the mouth of the Mackenzie River;[7] and in 1906 it sent a ship north to enforce recent legislation calling for all whalers to be licensed by the Canadian government.[8]

One by-product of the continued concern for strengthening Canada's claim to sovereignty over the Arctic was the Canadian Arctic Expedition. Vilhjalmur Stefansson, an "ambitious, headline-hunting anthropologist"[9] who had just returned from leading a four-year expedition to the Arctic,[10] was searching in 1912 and 1913 for financing to mount his next trip north. He had received offers of funding from the American Museum of Natural History and the National Geographic Society,[11] but feeling that this was not enough he turned to the

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[5] ibid., 255.
[6] ibid., 255, 259.
[7] ibid., 262.
[8] ibid., 265-266.
[9] ibid., 247; Collins, on the other hand, offers a more sympathetic portrait of Stefansson (Henry B. Collins, "Stefansson as an Anthropologist," *Polar Notes* 4 [1964], 8-13).
[10] Zaslow, *Opening of the Canadian North*, 246; Richard J. Diubaldo, *Stefansson and the Canadian Arctic* (Montreal and Kingston: McGill-Queen's University Press, 1978), 57.
[11] Diubaldo, *Stefansson and the Canadian Arctic*, 58-60.

Geological Survey of Canada,[12] a sub-section of the Department of Mines. Rather than just contribute to the expedition, the federal government took over entire responsibility for the endeavour, renaming it the Canadian Arctic Expedition.

This decision was motivated largely by politics. The recently-elected Conservative government under Robert Borden wanted a programme of northern research that matched the one that the previous Liberal government had undertaken in the Eastern Arctic.[13] Sovereignty was still an issue. The government was afraid that Canadian claims to the territory would be adversely affected by expeditions under foreign flags finding uncharted land in the north; the mandate of the Expedition was explicit in its emphasis on the search for new land.[14] Finally, the Geological Survey wanted to expand its scientific research into new areas of Canada.[15] Morris Zaslow offers a concise description of the government's new priorities:

> Government interests after 1911, now that the situation in Hudson Bay and the Eastern Arctic was relatively stable, was diverted to the western Arctic where considerable activity by American whalers, fur-traders, and prospectors was still under way, and furthermore, keen scientific interest was being displayed in the remaining primitive Eskimo bands not yet completely transformed by contact with the white man. The Geological Survey, under its dynamic young director R. W. Brock, was keenly interested in developing the anthropological and ethnological sides of its museum activity, as well as pushing geological mapping and studies beyond their present northerly limit.[16]

The expedition was marked by divisions and conflict from the beginning.

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[12] ibid., 62.
[13] ibid., 63.
[14] ibid., 64; Morris Zaslow, "Administering the Arctic Islands 1880-1940: Policemen, Missionaries, Fur Traders," in *A Century of Canada's Arctic Islands 1880-1980*, ed. by Morris Zaslow (Ottawa: Royal Society of Canada, 1981), 63.
[15] Zaslow, "Administering the Arctic Islands," 63.
[16] Zaslow, *Opening of the Canadian North*, 272.

Planning it was a tremendous undertaking, and the necessity of departing by May 1913 meant that many corners had to be cut, and many of the decisions were deferred to Stefansson, much to the later regret both of the government and of the scientific staff. As it was, responsibility for organisation was divided between the Naval Service and the Departments of Marine and Fisheries, Interior, Customs, and Mines (the home department of the Geological Survey).[17] The Expedition was broken into two parts: the Northern Party, under the command of Stefansson, which was to focus on exploration and the discovery of new land; and the Southern Party, under the command of the zoologist R. M. Anderson, which was to concentrate on scientific work and whose staff was to report to the Geological Survey.[18] This division of duties led to conflict both in the field and at the administrative level, where the Naval Service emphasised the exploration aspect and the Geological Survey stressed the scientific component; Stefansson's priorities laid with the former.[19]

The scientific staff, under the auspices of the Geological Survey, was impressive in both size and scope. It included marine biologists, botanists, meteorologists, topographers, a photographer, and anthropologists.[20] Anderson, the highest-ranking member of this group in the field, served as commander. Jenness and Henri Beuchat were hired as the anthropologists on this team.

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[17] Diubaldo, *Stefansson and the Canadian Arctic*, 66.
[18] ibid., 74.
[19] Morris Zaslow, *Reading the Rocks: The Story of the Geological Survey of Canada 1842-1972* (Toronto: Macmillan, 1975), 321.
[20] For more on Beuchat, see: CMCB, box B170, file 30 (Mme E. Beuchat, 1913-1919), "[Autobiography of Henri Beuchat]"; C. M. Barbeau, "Henri Beuchat," *American Anthropologist* 18 (1916), 105-110. Beuchat was a colleague of Marcel Mauss, the leading student of Emile Durkheim in France. It would be interesting elsewhere to speculate on the differences in Jenness's approach should Beuchat have survived and the men worked together.

There was division in the ranks of the Expedition, as the scientific staff did not respect Stefansson's severe manner of command. Discontent with Stefansson reached such a point that he accused the scientists of mutiny at least twice. To make matters worse, while Stefansson and some others were off hunting, the expedition ship *Karluk* became embedded in the encroaching ice and was carried off, eventually foundering off Wrangell Island north of Russia. Sixteen of the twenty-eight people on board died, either before the ship sank or while trying to cross the ice to land afterwards. Among those left on the ailing ship were most of the scientific members slated to join the Northern Party.[21] Stefansson attempted to commandeer most of the remaining resources for his Northern Party, again leading to a great rift with the scientific staff. Richard Diubaldo asserts that

> [b]y the spring of 1914, it was obvious that the Canadian Arctic Expedition was turning into a nightmare. The expansion of the expedition, the shoddy, hasty preparations, and the fuzzy and contradictory nature of the official instructions, all conspired to bring more fundamental tensions to the breaking point. Differing points of view regarding the expedition's purpose had created two camps, pitting Stefansson against virtually the entire Southern Section. No compromise was possible.[22]

Due to ice conditions and the loss of the *Karluk*, the Expedition spent its first winter in the Arctic among the Eskimo of far north-eastern Alaska. Jenness began his research in earnest, working on the language and culture of the local groups with an interpreter named Alfred Hopson, or Brick, the fifteen-year-old son of the shopkeeper in the settlement at Point Barrow. He found this work in Alaska difficult due to the negative effects of years of contact with whites.

The Copper Inuit had been selected for this project because it was

[21] Diubaldo, *Stefansson and the Canadian Arctic*, 83.
[22] ibid., 101.

believed, contrary to the experiences of Alaska Eskimos, that they were largely free from such outside influence. Jenness himself stated upon his return that the Copper Inuit were of particular interest because they were "the only branch of the Eskimo race which still retained its primitive mode of life unaffected by the great world beyond."[23] He saw his goal as making a rapid, comprehensive recording of "traditional" Copper Inuit life and culture, before it was lost. Anderson argued that

> [w]ith the present rapid advance of civilised ideas and customs into this particular region, it is certain that much of this information could not be obtained at a later time. The habits of the Eskimos are changing with a rapidity which is astonishing to those not conversant with the situation; improved weapons and methods of trapping reduce the game and compel shifting of tribal localities, while from the history of the past, it seems very likely that contact with the fringe of civilization will rapidly decimate the numbers of the Copper Eskimos as it has done to the Eskimos farther west.[24]

It requires close reading to see that these changes were already occurring. Jenness records them in his diary and in his published reports, but usually only in passing. There exists a contradiction in his ethnography — sometimes the Copper Inuit are presented as pristine examples of a pre-contact primitive society; and other times they are depicted as debased examples of culture contact.

Jenness's research was not only of scientific value; his knowledge also offered practical assistance to the Southern Party. In one of his annual reports,

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[23] Diamond Jenness, "Ethnological results of the Canadian Arctic Expedition," in *Summary Report of the Geological Survey, Department of Mines, for the Calendar Year 1916*. Sessional Papers Vol. 17, No. 26 (Ottawa: King's Printer, 1917), 392.

[24] R. M. Anderson, "Canadian Arctic Expedition 1916," in *Summary Report of the Geological Survey, Department of Mines, for the Calendar Year 1916*. Sessional Papers Vol. 17, No. 26 (Ottawa: King's Printer, 1917), 329.

Anderson praised Jenness's "linguistic abilities and acquaintance with the Eskimo character," which made him most suitable to act "as the official purchasing agent for the expedition in practically all business transactions with the local natives, including the purchase of meat, fish, and clothing."[25] He also acted as the Expedition's interpreter.

On account of the war in Europe, the Southern Party of the Expedition was ordered home in 1915. However, due to the difficulties of communication and travel in the far north, it was the summer of 1916 before the order was obeyed[26] and the scientists ended their three-year trip. Stefansson and the Northern Party stayed in the Arctic for another two years, travelling and mapping the islands of the Arctic archipelago. Jenness worked in Ottawa for a while, writing up his reports, before enlisting in the Canadian Expeditionary Force and serving in France from 1917 to 1919.

Jenness's Fieldwork and Arctic Diary

It is unclear why Jenness chose to participate in the Canadian Arctic Expedition. He had shown no familiarity with, or even interest in, Canada,[27] preferring instead to work in the South Pacific. His goal was to return to the latter after the war.[28] However, the difficulty of finding satisfactory employment after returning from New Guinea, combined with the opportunity to work with someone

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[25] R. M. Anderson, "Canadian Arctic Expedition 1915," in *Summary Report of the Geological Survey, Department of Mines, for the Calendar Year 1915*. Sessional Papers Vol. 22, No. 26 (Ottawa: King's Printer, 1916), 224.
[26] Zaslow, *Opening of the Canadian North*, 275.
[27] Richling, "An Anthropologist's Apprenticeship," 75.
[28] ibid.; OUAM, Jenness to Marett, 17 October 1913, 06 January 1915.

of Stefansson's stature, made the opportunity to go to the Arctic attractive.[29] "The salary," he reported to Marett, "was not princely — expenses +500$ a year while in the field & a salary (unstated) while working up the report. But it was the only thing except teaching that was offering, and I understand the Expedition is rather important and likely to lead to something afterwards."[30]

Sapir, who, through connections with Barbeau, had invited Jenness to participate in the Expedition,[31] seems to have been an enthusiastic supporter of the Expedition. Its focus on ethnography fit with his goals as Chief of the Anthropological Division of the Geological Survey:

> Now or never is the time in which to collect from the natives what is still available for study. In some cases a tribe has already practically given up its aboriginal culture and what can be obtained is merely that which the older men still remember and care to impart. With the increasing material prosperity and industrial development of Canada the demoralization or civilization of the Indians will be going on at an ever increasing rate. No shortsighted policy of economy should be allowed to interfere with the thorough and rapid prosecution of the anthropological problems of the dominion. What is lost now will never be recovered again.[32]

In his instructions to Jenness, Sapir emphasised salvage. He urged Jenness to undertake a comprehensive survey of traces of precontact Inuit life, physical characteristics, and material culture:

> The main part of your work is to be the collection of a full ethnographic material, based on study and observation among the Eskimos of the Arctic region. In connection with your research work, it would be advisable for you to assemble rather full ethnographical collections from the various tribes visited, these collections to be forwarded to the Victoria Memorial Museum at Ottawa. As complete data as possible should also be obtained on the physical characteristics of the natives visited, including

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[29] Richling, "An Anthropologist's Apprenticeship," 75.
[30] OUAM, Jenness to Marett, 09 March 1913.
[31] Richling, "An Anthropologist's Apprenticeship," 74.
[32] Edward Sapir, "An Anthropological Survey of Canada," *Science* n.s. 34:884 (08 December 1911), 793.

> systematic anthropometric data. ... Inasmuch as the technology of the Eskimo has been more fully studied than any other phase of their culture, it is suggested that you concentrate as much as possible on the non-material side of culture, including such topics as religion, shamanism, social organization, and various beliefs and customs.[33]

This work was intended to be divided between the Expedition's two anthropologists — Jenness and Beuchat. Jenness had asked Sapir about undertaking linguistic work, and Sapir responded enthusiastically, in the process outlining his views of the value of such work:

> I had not written to you in regard to linguistic matters, as I had imagined perhaps mistakenly that your training and interested had not been along those lines, and I am therefore doubly pleased to find that you expect to pay attention to this aspect of the work. Of course, the very best sort of ethnological material that you can get would be texts obtained from dictation. Such texts are apt to be extremely valuable, not only in studying mythology, but also other aspects of ethnology, particularly rituals and religious ideas.[34]

Less than six weeks later, however, Sapir wrote to Jenness stating that if the two ethnologists were forced to work in the same area, "[i]t is perhaps as well that M. Beuchat is to do most of the linguistics while you are to undertake all the anthropometric work."[35] In light of later comments on his lack of "any special training in linguistics,"[36] Jenness likely found this arrangement most agreeable;[37] the disappearance of the *Karluk* and the subsequent death of Beuchat made these arrangements moot, though.

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[33] Canadian Museum of Civilization Archives, Ethnology Records, Edward Sapir Correspondence (I-A-236M) [hereafter CMCS], folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to Jenness, 06 March 1913.
[34] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to Jenness, 07 May 1913.
[35] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to Jenness, 19 June 1913.
[36] Diamond Jenness, "The Ethnological Results of the Canadian Arctic Expedition, 1913-1916," *American Anthropologist* 18 (1916), 612.
[37] In fact, given his trepidation about linguistic work, I wonder how enthusiastic he was about undertaking such research; perhaps his offer to Sapir was an attempt to curry favour with the latter, as Jenness must have known the theoretical emphases of the man who could later play a role in the securing of a permanent position in Ottawa.

Because none of Jenness's fieldnotes are extant, the only way to assess his fieldwork is through analysis of his diary[38] and correspondence. Diary keeping became one of his strengths, as he revealed to Marett:

> Of the other work I did [during his first winter in Alaska] besides miscellaneous notes and jottings, there was the keeping of a regular diary, wherein many ethnological notes became embedded. I wish I had done the same in Papua; many things which otherwise would pass unnoticed are thus recorded, and it has taught me to look more for things. Millions of things escaped me in Papua because I had not the wit to see them.[39]

The Arctic diary is rich in detail, containing descriptions of incidents and activities which allow insight into Jenness's methodology and theoretical orientation. In his preface to the diary, Jenness's son explains the nature of the material within:

> Not simply a routine account of a series of chronological events, my father's daily entries also provide a view of the feelings and responses of an idealistic, sensitive, and dedicated young scientist thrust into dire living conditions in a culture totally foreign to any he had known previously. The three-volume diary [manuscript] is also, of course, an extraordinary account of a very modest man's industriousness and perseverance in carrying out far more than was expected of him, in spite of a multiplicity of delays, frustrations, perilous experiences, and recurring ailments.[40]

The diary also offers a personal view of a generally private man. Though Sapir had promised Jenness that the contents of the diary would remain confidential,[41] Jenness "carefully refrained from [recording] anything personal against members of the expedition, or anything of that nature."[42] Countermanding earlier instructions from Ottawa, Stefansson demanded access to the private journals kept by the members of the Expedition so that he could gather ethnographic data

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[38] Diamond Jenness, *Arctic Odyssey: The Diary of Diamond Jenness 1913-1916*, ed. by Stuart E. Jenness (Hull: Canadian Museum of Civilization, 1991).
[39] OUAM, Jenness to Marett, 29 June 1914.
[40] Jenness, "Preface," xx.
[41] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to Jenness, 20 May 1913.
[42] OUAM, Jenness to Marett, 02 August 1914.

from them.[43] In spite of this constraint, Jenness's diary allows readers access to his relatively unguarded reactions to daily life and events during his research.

Stuart Jenness's romantic view of his father's time in the Arctic is characteristic of the general view of Jenness's fieldwork, which has emphasised the difficulties associated with long-term residence among the Inuit. For example, Henry Collins and William Taylor argue that the disappearance of the *Karluk* "was the inauspicious beginning of Jenness's Arctic career. Few young anthropologists have faced such difficulty in beginning field-work in a new and unfamiliar area; yet none, surely, has emerged from the test with a more brilliant record of work accomplished."[44] They describe his work in Alaska in all of its arduous detail:

> Jenness's first winter's field-work on the Arctic coast of Alaska that led to this impressive list of publications was conducted under conditions that many an ethnographer would have found intolerable. The people he lived with most of the time were inland Eskimos from the Colville River who spent the winter on the Arctic coast trapping white foxes to trade for ammunition and other necessities .... Food was never plentiful, indeed often insufficient for their needs, and it was frequently necessary for the group to pack its belongings on sleds and set out for some other locality where the prospects for food were more promising. Jenness shared this precarious existence with his Eskimo hosts, living with them in their tiny over-crowded wooden cabins and travelling with one companion over many miles of frozen tundra and sea ice in the coldest and stormiest months of the arctic [*sic*] year.[45]

S. Jenness contrasts the harsh living conditions his father faced with the latter's moderate descriptions of his time in the Arctic, arguing that

> [t]hroughout the diary there is a genteelness of prose in his descriptions of the primitive living conditions he was experiencing and few expressions of complaint or criticism (although these would have been perfectly

[43] Jenness, "Preface," xxxiii.
[44] Collins and Taylor, "Diamond Jenness," 72.
[45] ibid., 72-73.

understandable considering his almost daily hardships, repetitious and often dreary routines, frustrations, and interpersonal irritations).[46]

I take a different view. Following the argument put forward by Kulchyski, I suggest that the diaries themselves constitute an "ideologically rich text, frequently providing glimpses and interpretive threads that seem to go against the grain of its overall impulses; a text that often offers its revelation in spite of itself."[47] Rather than focussing on the details of Jenness's travails in the Arctic, I highlight the implications of what he chose to elide and downplay. This permits not only a better understanding of the Inuit with whom he lived, but also provides insight into his research methods and theoretical orientation.

Collins asserts that in undertaking this research among the Copper Inuit, Jenness "faced a challenge and an opportunity rarely offered [to] a 20th century anthropologist,"[48] namely, the chance to study "a virtually unknown people who had been brought to the attention of the scientific world only two years previously."[49] Jenness's Arctic fieldwork differed from his New Guinea research in a significant way: while his New Guinea fieldwork offered him limited opportunities to undertake participant-observation research,[50] in the Arctic, "social intimacy, like cooperation in the daily round of subsistence activities, was inseparable from the work of anthropology."[51] This immersion in Inuit life caused Jenness some difficulties.

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[46] Jenness, "Preface," xxi.
[47] Kulchyski, "Anthropology in the Service of the State," 39.
[48] Henry B. Collins, "Diamond Jenness: An Appreciation," in *Pilot not Commander: Essays in Memory of Diamond Jenness*, ed. by Pat Lotz and Jim Lotz (special issue of *Anthropologica*, n.s. 13:1-2 [1971]), 9.
[49] ibid.
[50] Richling, "An Anthropologist's Apprenticeship," 74.
[51] ibid.

Jenness's exposure to Inuit life began almost immediately upon his arrival in the Arctic. He spent a part of his time during the first winter and spring in Alaska living with Eskimo families near Point Barrow,[52] an approach which met with Sapir's approval: "I was glad to learn from your last letter that you had at last settled down to fairly continuous work with some Eskimo informants, and I hope that by the time you get this [letter] you will feel thoroughly at home in things Eskimo, both ethnologically and practically."[53]

The first major problem with which Jenness had to contend was a lack of equipment. He had left most of his anthropological instruments, papers, and books on the *Karluk*,[54] which caused him some difficulties in his early fieldwork. Jenness himself wrote that as a result his research was handicapped,

> more especially in dealing with the Eskimos of this region, who have lost many of their ancient customs under the influence of the whites, and with whom therefore observation alone does not yield very profitable results. The only alternative method is through the language, but unfortunately the Eskimo language is an extremely difficult one, both structurally and phonetically.[55]

In spite of the complexity of the language, Jenness set to work trying simultaneously to learn it and describe its grammar.[56] In a letter to Marett, he outlined the approach he was taking:

> Would you like to hear of the phonology? You know what an ignoramus I am about everything relating to that. Yet here I have to record Eskimo where scarcely a single sound coincides with any known to Europeans or to the rest of the world. Beuchat gave me some help on the Karluk. I

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[52] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], "Summary Report Covering the period from Sept. 1913–July 1914"; Jenness to Sapir, 30 May 1914.
[53] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to Jenness, 22 June 1914.
[54] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Jenness to Sapir, 26 October 1913, 30 May 1914.
[55] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], "Summary Report Covering the period from Sept. 1913–July 1914."
[56] OUAM, Jenness to Marett, 29 June 1914.

> started out, like any novice with a sort of script, and have been changing it all the time, whether for the better or not I don't know. I wish I could have you here for a few days. It troubles me rather sorely, as there another [*sic*] two years ahead. Stefannson [*sic*] used what seems to me a rather poor script, very inadequate. I shall probably modify mine when I get backand [*sic*] can discuss it with someone who knows. At present I am content to indicate any difference in sound I can detect by what seems to be the most convenient symbol to preserve that difference, e.g. a "q" for a uvular guttural. I have a few short stories written down in this manner which at present I am not able to translate; also a number in English.[57]

To assist him, Jenness engaged a fifteen-year-old boy, Alfred Hobson, also known as Brick. The son of mixed parentage, Brick was characterised by Jenness as "a very good lad in many ways."[58] He spoke conversational English, and could read and write, "though much in a newspaper would be quite unintelligible to him. He is more at home with Eskimo."[59] However, late in his first winter in Alaska, Jenness had to let him go, explaining that "[t]he lad was very useful in many ways, but his defective knowledge of English & total ignorance of grammar, together with the necessity of attending to his fox-traps, prevented my making as much progress as I had hoped."[60]

Working with Brick, Jenness came to appreciate the difficulties of his research. Brick transcribed a story told by a man, who told them that

> Stefansson had obtained [a story] as a caribou charm. The [Cape] Halkett people were laughing about it while Brick was present — it was a popular story which Stefansson had recited to them as an example of one of their charms. He had asked them to tell him any more they knew, for he would write them down and they would always be remembered — otherwise they would soon be forgotten. At the time they sat quiet and said nothing, but laughed heartily when Stef[ansson] had gone — such is the fate I fancy of

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[57] OUAM, Jenness to Marett, 02 December 1913.
[58] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Jenness to Sapir, 02 December 1913.
[59] ibid.
[60] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Jenness to Sapir, 27 February 1914.

many an ethnologist — more often than is suspected.[61]

Like Brick, many of the Eskimos whom Jenness met in Alaska reflected the blending of Eskimo and outside cultures. Of the first Eskimos he saw, while searching for the *Karluk*, he noted that "[a]ll of these people had flour, tea sugar matches etc primus stoves, kerosene, frequently sewing machines, besides of course rifles and shotguns,"[62] showing the extent to which outside items had entered the far north. He commented on the influence of Christianity, remarking that no work was done on Sundays[63] and that crosses marked recent graves.[64]

Jenness also frequently hypothesised about the racial composition of individual Eskimos, based on their physical appearance. For example, he noted that "[o]ne child appeared to be half-Eskimo, half-Polynesian, judging from its appearance."[65] Later, he mentioned a young woman who "was very good looking — very different from the ordinary Eskimo type .... She resembled rather the Arab or North African type. Probably she has foreign blood in her."[66] The most striking comment was his assertion that "Aksiatak has quite a Roman nose, his face is long and flat, the chin almost pointed, but there is no doubt that he is of pure Eskimo descent."[67] It is unclear how Jenness was able to make such definitive judgments based on so little time in Alaska.

However, Jenness was careful to note that he was working with a family relatively free from these influences:

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[61] Jenness, *Arctic Odyssey*, 122.
[62] OUAM, Jenness to Marett, 17 October 1913.
[63] Jenness, *Arctic Odyssey*, 40.
[64] ibid., 10.
[65] ibid., 32.
[66] ibid., 37.
[67] ibid., 67.

> [t]he Eskimos about here are losing most of their old customs. The tattooing of women is not continued among the younger generation. Mr. Brower [a trader] tells me that they even come to him to have sleds & umiaks made by his workmen. However, the family I am staying with has its headquarters on the Colville River & has been very little in contact with the whites.[68]

Jenness alluded here to a theme that would dominate his Arctic research, that is, the discovery of islands of cultural "purity" in an otherwise inundated world. In the sea that is culture change — culture loss, for the Eskimo — he found an island relatively free from the deluge of the outside world. In a letter to Sapir, for example, he explained that "[t]he two families with whom I am staying are inland Eskimos from the Colville River region, & have come less into contact with the whites than most of the Eskimos here. One of them Aluk is reputed to be well acquainted with the old songs & traditions, but is said likewise to be unwilling to talk about them."[69] These two themes — finding informants who were less affected by outside influences than their fellow Inuit, but who were less than willing to impart all that they knew — would recur throughout his time among the Copper Inuit as well.

A third theme that Jenness highlighted in his Alaskan work was the ways in which the Eskimo culture compared with English culture. For example, although "Eskimo manners at 'table' seem rather strange to a European,"[70] he explained that "[m]ost families appear to have a small 'table cloth' (more correctly perhaps 'food cloth' for it is laid on the floor) of what we commonly call oil-cloth.

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[68] OUAM, Jenness to Marett, 17 October 1913.

[69] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Jenness to Sapir, 02 December 1913.

[70] Jenness, *Arctic Odyssey*, 129.

It is kept very clean, as cleanliness goes here."[71] Like Europeans, they also displayed a "marked" affection for their young children. "They play with them, hug them, and in general behave towards them just as English parents do,"[72] he noted.

At other points, Jenness was a harsh critic of the Eskimos, regularly lapsing into broad generalisations, particularly about Eskimo morals. In these cases, he often referred to the Eskimo as children, or as being childlike. For example, he wrote that

> I have noticed this repeatedly among all the Eskimos I have been with — it seems to be typical of the people as a whole. They are unable, I suppose, to project themselves out of themselves — to love their neighbours as themselves. This perhaps explains their cruelty — or so it seems to us — to birds and animals — a *child-like* thoughtlessness which permits them to torment an injured bird or thrash unmercifully a dog which has provoked them.[73]

Later, he accused them of an acute lack of foresight, nearly calling them stupid for not planning ahead. The men and boys, he wrote,

> fire about 10 rounds for every bird they kill. However, they have plenty of powder and shot and brass shells, so I suppose it matters little; but it is a trait of their character. Similarly they burn a lot of coal oil in primus stoves when it is altogether superfluous, then live in darkness part of the winter for lack of lighting.[74]

This theme recurred frequently during his later stay with the Copper Inuit, whose country Jenness finally reached in late July, 1914. Collins and Taylor remark in retrospect that he had arrived "just in time,"[75] as the Inuit were on the cusp of change:

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[71] ibid., 44.
[72] ibid., 45.
[73] ibid., 244-245 (emphasis added).
[74] ibid., 216.
[75] Collins and Taylor, "Diamond Jenness," 73.

> The few brief encounters with the early explorers, and even the contacts with the whalers who wintered among them from 1905 to 1907 had in no way affected the Eskimo's way of life. The coming of the *Teddy Bear* [a trading schooner], however, with its store of white man's goods, was an event of far more importance. Other changes were to follow.[76]

However, these later commentators argue, the arrival of outside influences did not have an impact upon the Copper Inuit among whom Jenness would work:

> Fortunately, these beginnings of change in the Eskimo's economy had no serious effect on Jenness's work. The rifle was coming into use, to be sure, and a few of the Eskimos were beginning to trap white foxes, but caribou were still being hunted with bow and arrow or speared in the water from kayaks. And in its nonmaterial aspects their culture remained unchanged. Thus in the two years that he lived among them Jenness was able to observe and record the life of the Copper Eskimos as it had existed for centuries or millennia before the white man's "civilization" had reached them.[77]

Jenness's goal was to do something no other ethnographer had done before — to spend a summer accompanying a family through its summer rounds. As a result of his plan to travel with the Inuit during "the long period in which small, flexible, highly mobile family groups rely upon fish and caribou for their livelihood,"[78] he "was confident that his observations would make a significant and original contribution to northern ethnology."[79]

To this end, he spent the period between April and November, 1915, living with an Inuit family. He justified this approach on the grounds that "[i]t is better ethnologically to spend a summer with the band I am with and watch their summer life than to run around the country, now meeting them, now alone."[80]

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[76] ibid., 73-74.
[77] ibid., 74; see also Leslie H. Tepper, "The Expedition Diaries of Diamond Jenness, 1913-1916," *The Beaver* 341:1 (July 1983), 4.
[78] Richling, "An Anthropologist's Apprenticeship," 79.
[79] ibid.
[80] Jenness, *Arctic Odyssey*, 447.

He assumed the role of an adopted son in the family with which he travelled, though he noted that "[i]n my case the adoption is very special — more in the nature of a business proposition if they understood what that meant."[81] The family had been promised an array of goods, to be given when they returned Jenness safely in the fall.[82]

In a self-congratulatory summary report sent south near the end of his Arctic stay, Jenness outlined what he saw as the advantages of his method of long-term intimate fieldwork. He asserted that

> [m]uch information was obtained concerning the daily life of the natives in summer and winter, both by direct inquiries, but mainly by living in the their midst, observing and taking part in the common routine. Much misapprehension has existed amongst ethnologists concerning their summer life, our knowledge of which has hitherto depended entirely upon the statements of travellers who have come into momentary contact with them during their wanderings. I spent seven months, from early spring until the beginning of the ensuing winter, with a small band of natives on Victoria Land, sharing their life in all its details, living in the same tents, hunting and fishing with them to obtain our common food, and accompanying them in all their movements. The information thus acquired proved beyond doubt that the old theories concerning their social and religious life during this period are entirely erroneous, at least as far as this branch of the Eskimo race is concerned. While it is difficult, perhaps impossible, for a civilized person fully to understand the mental attitude of a savage people towards the phenomena of life, yet the many shamanistic performances which I witnessed, and in many cases took part in, leave a general notion concerning their religious life which cannot be far from the truth.[83]

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[81] ibid., 463.

[82] Anderson recorded the arrangement in a summary of the Expedition's work: "Mr. Jenness made his final start for Victoria island on April 13, with his Eskimo companion, a middle-aged man named Ikpukkuaq. The said Ikpukkuaq was supplied with a Winchester .44 rifle and some cartridges, and was promised that if he did well by Mr. Jenness during the summer he was to keep the rifle on his return and receive a certain number of cartridges" (Anderson, "Canadian Arctic Expedition 1915," 228; see also Jenness, *Arctic Odyssey*, 416).

[83] Jenness, "Ethnological Results," 614-615. Here he is arguing against Marcel Mauss, a student of Durkheim, who maintained that the social organisation and religion of the Inuit changed with the seasons (Riches, "The Force of Tradition," 81; see also Marcel Mauss, with the collaboration of Henri Beuchat, *Seasonal Variations of the Eskimo: A Study in Social Morphology*, trans. James J. Fox [London: Routledge and Kegan Paul, 1979 (originally published in French in

Jenness's notes on his Inuit family display a certain tension. On the one hand, the Inuit have been subject to a great deal of outside influence, both from other Aboriginal groups and from southerners. With regard to the former groups, early in his stay amongst the Copper Inuit Jenness commented on these influences in a letter to Sapir:

> As yet I have seen too little of these people [the Copper Inuit] to learn much about their customs and language. They had regular intercourse with the natives further west until about two generations ago, when it appears to have been discontinued. Now they trade with the Indians to the south, exchanging dogs for guns and ammunition.[84]

Anderson also reported on Jenness's research in this regard, noting that Jenness found "that these groups are not as definite as was formerly supposed, in fact the groups are pretty thoroughly mixed, both by intermarriages and by families shifting from one group to another, nearly every group containing individuals from other groups more or less remote."[85]

Jenness was also aware of the impact of southern culture on the Copper Inuit. Richling argues that "[t]he presence of white men, and of their new tools and ideas, posed serious challenges to traditional Inuit concepts of order and action."[86] Jenness himself noted that

> [s]pecial attention was paid to the material culture of the Copper Eskimo and a large collection made of their weapons, household utensils, and clothing. These are rapidly being changed through the influence of the western Eskimo and of the whites. Already the natives have an abundance of iron to replace their copper; rifles are beginning to

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1906)].). Interestingly, he is also making an argument contrary to Marett, who said that it was possible for researchers to see the world from a native point of view.

[84] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Jenness to Sapir, 15 January 1915.

[85] Anderson, "Canadian Arctic Expedition 1915," 230.

[86] Barnett Richling, "Second Sight: Diamond Jenness's Life Among the Copper Eskimo, 1914-1916," (paper presented at the Sixth Inuit Studies Conference, Copenhagen, October 1988), 16.

> supersede bows and arrows; European pots and tin cans take the place of stone pots; garments of cloth are in great demand; and even the style of clothing is undergoing change. For this reason a special endeavour was made to procure numerous specimens of those objects which were most likely to suffer modification or disappear entirely.[87]

Obviously, Jenness was aware of the significant changes that were occurring in the lives of the Copper Inuit; however, as I will discuss below, his ethnographies rarely reflect the nature of the changes and rather present a static portrait of a pure Copper Inuit culture. An example of this denial on his part can be found in a letter to his former colleague at the Museum, Sir Francis Knowles: "I do not think that the transition from the bow to the gun had affected their archery when I was there. There were only five rifles in the country when we arrived and they had been obtained only two years previously from the trader. The vast majority of the Eskimos had never touched a rifle."[88] However, the vast majority of Inuit Jenness met seemed to have some familiarity with firearms.

During his fieldwork, Jenness was often annoyed and disgusted by the actions of his hosts. He commented that he was constantly reprimanding them, complaining that they "seem not to have developed a sense of gratitude,"[89] and that they "will beg and clamour for anything they fancy, like children without least shame or hesitation."[90] If a man were annoying, Jenness wrote, then the only way he could put up with him was if his wife "is useful sewing and mending."[91]

When he could, he incited his companions to behave in ways he found

[87] Jenness, "Ethnological Results," 613.
[88] CMCJ, box 647, file 55 (F. H. S. Knowles, 1926-1941), Jenness to Knowles, 28 December 1928.
[89] Jenness, *Arctic Odyssey*, 352.
[90] ibid., 341.
[91] ibid., 460.

more appropriate. For example, he recounted an incident that occurred during a caribou hunt:

> Avrunna went caribou hunting and secured one. He cached the meat under stones, as I told him if they threw it away I should give them no ammunition. This perturbed them very much. They said that wanted the skins for clothing, but the meat was too heavy to pack. As a matter of fact, I am certain they have enough summer skins for clothing, but probably they intend to trade with others less fortunate. They said that they would cache the meat under stones and it would do for dog-food next summer, if not for themselves, at least for other Eskimos. I doubt whether any Eskimos will find it before it is consumed by worms and foxes, but let it pass at that.[92]

It is unclear why Jenness forced them to cache the meat if he himself were unsure that it would last; perhaps he was trying to preserve an image in his own mind of a people who had so little they could afford to "waste" nothing.

Jenness was equally concerned with the peoples' treatment of their family members, using his control of "luxury" items to influence behaviour. At one point, he reported that

> I spoke to Ikpuk today about Kannayuk's fearing to sleep in their tent because they beat her. He told the others, and they thought it rather a joke, saying it was the custom; however, I assured him I should be very angry if it continued and believe it will cease. They dare not offend me because I control the supply of ammunition and other desirable things, and can refuse to allow them any this winter.[93]

In both of these cases, Jenness unabashedly tried to influence the Inuit, forcing them to adapt to his preferences lest he withdraw his largesse.

Jenness sometimes spoke explicitly of wanting to teach the Inuit lessons. For example, he seized the rifle of a man suspected in the theft of some

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[92] ibid., 511.
[93] ibid., 507.

ammunition;[94] giving it back after the man had proved his innocence, Jenness remarked in his diary that the man "has received a good lesson, if nothing else, as indeed have all the Eskimos round here."[95] Jenness also refused to trade with individuals or groups suspected of stealing from the Expedition's supplies.[96] He was careful to punish the transgressors in ways which suited their individual characteristics:

> Patsy told me tonight that Niptanaciak was implicated in the stealing of the pemmican the other day. I taxed her with it and she admitted it, saying that she and the others are hungry. I tried to make her feel a little ashamed, the correction which seems to be most suitable in unimportant cases of this kind, for really some of them are like *children*.[97]

Jenness worked to keep himself aloof from the Inuit. Not only did he try to make it clear that his supplies were not to be plundered in times of hunger, as Inuit caches often were, but he sought to exclude himself from certain elements of reciprocity. After a successful hunting trip, he wrote that he "presented two of the caribou skins, heads, leg bones, and carcasses to the *Kanghirjuarmiut*. I told them that it was a free gift, but they each made me a present, one of deerskin socks, the other of winter boots. They offered me more but I declined."[98] I interpret this episode as an attempt on Jenness's part to maintain some distance from the Inuit. He wanted to keep them in his debt, rather than accept some items in exchange, closing the circle of reciprocity and binding him to the group.

Though he was their guest, Jenness often acted as if he were doing the Copper Inuit a favour by being with them. He complained about their willingness

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[94] ibid., 582.
[95] ibid., 584.
[96] ibid., 566; 581.
[97] ibid., 563-564 (emphasis added).
[98] ibid., 449.

to let him contribute more than he thought was his share of caribou to the group, writing that "I don't like the prospect of their depending on me for hunting, but can't very well avoid it. However, I am obtaining some good ethnological notes, and the more there are to travel with the better opportunity there is of seeing native life."[99] Earlier he had noted that

> [n]o caribou appeared near camp, so I did not leave, seeing no reason why I should hunt for the whole community and make them more or less dependent on me instead of hunting themselves. I hope to do my share to support myself and Ikpakkuaq's family this summer. ... At present I should be glad to see some of these people go away.[100]

In Jenness's mind, there was obviously a hierarchy of Inuit personality types. At the bottom were the childlike, insolent individuals who taxed him with their constant demands and petty thievery. At the top were those who were docile and more respectful. The latter group included a married couple who were "very quiet and decent — keeping away from everything but doing any little thing we want,"[101] and others who were "real treasures compared to those we have met west — not officious or bothersome, and perfectly honest."[102]

Jenness's time with the Inuit was also marked by strong challenges to his values. The single incident which had the most personal impact upon Jenness was an exchange of wives he witnessed soon after meeting the Cooper Inuit:

> Itoqunna, I believe, slept with Niq's husband Akhiatak — an exchange of wives for the night .... No words passed between the two women, but when Itoqunna entered, Haviuyaq laughingly asked me "where is Itoqunna?" — alluding to my question of the night before, whereupon everyone laughed. I do not know if everyone exchanged wives last night, though Haviuyaq asked me if I still wanted to sleep alone. The custom is,

[99] ibid., 429.
[100] ibid., 428.
[101] ibid., 574.
[102] ibid., 569.

> of course, well known among savages from books, but strangely enough it shook my nerves more than anything else I have seen in the Arctic .... Itoqanna resumed her usual place in the house today and is sleeping with her husband tonight. I have not dared to enquire yet whether it was in connection with the sealing — though I feel rather ashamed of my weakness in this respect as an ethnologist.[103]

A month later, Jenness was again confronted with this issue, writing that "[o]ne of the women offered to sleep with me tonight, but I declined. Like the others these people cannot understand a man not wishing that sort of thing."[104] The third time the topic was raised, Jenness managed to add a comment expressing his sense of superiority over not only the Inuit but also other southerners whom he was sure would soon be heading north:

> Last night Ikpuk and Tucik were talking about the strangeness of the members of the Expedition not wishing to "marry" any of their women, and I tried to explain to them that we considered it wrong and to warn them of the fate which probably awaits them when other white men, less scrupulous, enter their land — a fate which has overtaken the Eskimos to the west and carried many of them off. It is sad to see the ravages our diseases make among the natives in all parts of the world, but it seems inevitable.[105]

Both Richling and Kulchyski offer commentary on these episodes. Richling, drawing to the fore Jenness's sense of vulnerability, asserts that the latter's discussion of the first episode of wife-exchange

> reveals a deeper conflict between the anthropologist's personal and professional personae. His response to the temporary wife exchange also gives voice to a sentiment of moral offence. Suddenly the mythical character of a customary practice ... became real, and in so doing, engendered a separateness between the scientist reared in an atmosphere of late Victorian mores, and his "subjects". A few days later, witnessing another exchange between the cousins, Jenness commented that "Apparently such incidents are so usual as to pass unnoticed" ..., by all but himself. In the months that followed, his moral ambivalence toward

[103] ibid., 350.
[104] ibid., 370.
[105] ibid., 481.

> Inuit customs actually resulted in a strengthening of Jenness's influence among the Copper Eskimos, and enhanced his usefulness to them in day to day affairs.[106]

I question Richling on this point of moral ambivalence; I see Jenness's assertion that the Europeans view such exchanges as wrong served to show that he was truly troubled by the events. I agree with Kulchyski's point, however, that Jenness's disgust with the practice led him to try to render the exchanges as less upsetting:

> This event, that so shakes the nerve of the ethnologist producing feelings of shame and disquiet, constantly slips free of the objective language Jenness uses to try and contain it. ... Finally, he resorts to the language of otherness: "the custom is, of course, well known among savages from books"; but is it more than that. This last attempt at containment is perhaps the most powerful: it involves positioning Inuit in the category of "savage" in order to simultaneously excuse, explain, contain and reduce to normality what in fact was, for Jenness, an extraordinary event. What he does not know — why it happens, if everyone participates — is greater than what he does know. The event destabilizes Jenness as objective inquirer. ... Jenness's nerves are shaken. He has been invited to forsake his position as objective recorder; he struggles back by classifying the people he lives with as savages, by trying to find available explanations. But somehow, in the end, he cannot quite do his job as an anthropologist and feels ashamed.[107]

This was not the only problem Jenness had with his research. He continued to have difficulties with the language,[108] though he eventually developed enough facility to make jokes. He wrote of a woman named "TamoXuina, whom I have nicknamed 'Tamogluña' (let me eat)."[109] He found that even with the assistance of a translator, it was difficult to induce the people

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[106] Richling, "Second Sight," 9-10.
[107] Kulchyski, "Anthropology in the Service of the State," 42-43. Not surprisingly, Jenness adopted a much more level-headed tone in his discussion of these events in his published ethnography, noting the importance of this practice in the integration of outsiders into the local social group (Jenness, *Life of the Copper Eskimos*, 85-86, 91).
[108] Jenness, *Arctic Odyssey*, 423.
[109] ibid., 577.

to speak, particularly about their religion and folklore.[110] Eventually Jenness resorted to threats to overcome their reticence:

> Uloksak was in my tent during the day, and I told him that he could not expect me to treat him very liberally if he did not tell me any stories. He said there was someone always hanging about the tent and he was afraid to tell. However, he came over late in the evening and told us a few shamanistic stories. I asked him whether he would care to have Ikpuk present, and he said no, Ikpuk would be angry with him.[111]

When his threats were successful, Jenness still had difficulties communicating his objectives. He complained about their inability to understand his directions, and their inability to tell a story in the "proper" way, seemingly without realising that their methods might lend an insight into their culture:

> Higilaq and Ikpuk told us a few items of information, but it is extraordinarily hard to extract anything out of them. There is much I think they could tell, but they don't seem to understand what is wanted. They can't narrate a story completely. Generally they begin in the middle and give the words of one of the personages, then stop as if that explained everything.[112]

Jenness's excitement about his opportunity to live with the Inuit for an extended period eventually waned, and by its end the experience left him wearied. Two months into his summer's journey with the Copper Inuit, he commented on the effects of immersion in Inuit culture. "I am growing Eskimo in many ways—," he wrote, "careless about dirty pots or dirty person — drink more cold water — tend to have my mouth agape when travelling. It requires an effort to keep 'white.'"[113] Clearly Jenness was frustrated with his work. Less than six weeks later, he wrote in his diary that "I am heartily sick of Eskimo life with its filth

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[110] ibid., 549, 581; CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Jenness to Sapir, 26 December 1915.
[111] Jenness, *Arctic Odyssey*, 556.
[112] ibid., 559.
[113] ibid., 451.

and squalor, and long for decent food and rest and quiet."[114] He wrote at one point about the pleasure of having a tent to himself,[115] and he noted with anticipation that "winter will soon be at hand when I can return to the station and enjoy good well-cooked food cleanly served, and the pleasant company of the other members of the Expedition."[116]

Jenness's Arctic Ethnography

In his analysis of some of the characteristics of ethnographic writing, James Clifford discusses a central trope used by practitioners of a certain type of anthropological research. He argues that

> [i]n western taxonomy and memory the various non-western "ethnographic presents" are actually pasts. They represent culturally distinct times ("traditions") always about to undergo the impact of disruptive changes associated with the influence of trade, media, missionaries, commodities, ethnographers, tourists, the exotic art market, the "world system," etc. A relatively recent period of authenticity is repeated followed by a deluge of corruption, transformation, modernization.[117]

In his writings, Jenness presented the Copper Inuit as representing a still-"authentic" culture, while he presented the Eskimo of Alaska as highly acculturated.

Jenness arrived among the Alaskan Eskimos at a point when they had been exposed to southern culture for an extended period of time. He found a situation in which "[v]ery little in the outward culture now differentiates the Eskimo

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[114] ibid., 486.
[115] ibid., 481.
[116] ibid., 480.
[117] James Clifford, "Of Other Peoples: Beyond the 'Salvage' Paradigm," *Discussions in Contemporary Culture* 1 (1987), 122.

from the white."[118] It was so bad, he wrote, that "[l]ife's three great necessities, … food, shelter, and clothing, the Eskimo is no longer able to provide for himself. Remove the supply from without and he will perish within a few years."[119]

He characterised these changes as a function of the primitive nature of the Eskimo culture, and its inability to resist the colonising culture. He argued that

> [t]he changes produced in the life and habits of the Eskimos of Northern Alaska during the last thirty-five years afford an interesting example of the effect European civilization may have upon an uncivilized unprogressive people. … That these Eskimos were incapable of developing internally to any marked degree is fairly evident from the fact that during all the centuries that have elapsed since their separation from the other branches of their race no fundamental change has taken place in either their social or their mental life. In fact, the environmental conditions to which they were subjected were unfavorable to any great development. Year by year the seasons returned unchangingly, each with its different pursuit, but all alike periods of strenuous quest for food. … The great world beyond was too remote ever to reach or affect them, and their own life involved too arduous a struggle for existence to allow them that leisure which alone enables a people to develop.[120]

The Eskimo were unable to evolve, he argued, given the constraints of their harsh environment and the challenges of eking out an existence from it. If Jenness's ethnography seems synchronic, it is only because he believed that the Eskimos were incapable of changing on their own. The only thing that shook them out of millennia of inertia was the arrival of southerners. Jenness theorised that the interaction of "civilised" and "uncivilised" could lead to two outcomes:

> First the old social system breaks down, carrying with it the morality that it supported. This opens the road to self-indulgence and excess of every kind, followed by disease and misery, which, partly directly, partly indirectly, by undermining the virility of the race, cause its decline and sometimes its extinction. … Sometimes, under counteracting influences,

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[118] Diamond Jenness, "The Eskimos of Northern Alaska: A Study in the Effect of Civilization," *Geographical Review* 5:2 (February 1918), 93.
[119] ibid., 91.
[120] ibid., 89-90.

> the people recover, ... and such recovery seems to be going on in Northern Alaska. There the very simplicity of the social organization and its adaptability to new conditions prevented its destruction; it altered without entirely breaking down. It still lends support to the respect with which property and persons are regarded and binds the people together in harmony and goodwill.[121]

Jenness links the Alaskan Eskimos and the southerners in an evolutionary framework, where the presence of the latter groups paradoxically destroys the Aboriginal culture while simultaneously regenerating it out of its simple constituent pieces. A prominent example of this process cited by Jenness is the impact of Christian missionary teachings on the Eskimo communities. Such teaching, he argued, "however imperfectly understood, and however misinterpreted, has been on the whole beneficial to the Eskimos,"[122] as, generally speaking, "[a] native no more than the average white man can reason out a set of moral rules to guide his conduct. He depends on custom to tell him what to do and what not to do, and custom unfortunately prescribes or allows many undesirable practices."[123] Thus he saw religion as a "gift" from a group of conscientious southerners to the Eskimo, and even "if the Christianity of the Eskimo today is very crude and full of superstition, it is nevertheless free from many of the injurious practices of his old religion and contains in itself the germs of a higher development."[124] Once the seed has been planted, he noted, the growth of religion in the Eskimo soul will elevate individuals above their current lot. The progress, as Jenness outlines it, is from superstition to superstitious Christianity to true faith.

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[121] ibid., 98.
[122] ibid., 99.
[123] ibid.
[124] ibid., 100.

His views on evolutionism emerged further in discussions of Inuit "sexual morality."[125] He argued that conditions on the latter have "greatly improved, partly from a growing knowledge of the evils to which loose living gave rise, partly as a result of missionary teaching. Much progress must still be made, however, before the standard of civilization is attained."[126] This passage shows conclusively Jenness's evolutionary bias and makes clear his assumption that there is a single path to be followed, leading at its apex to the culture of which Jenness considers himself a member.

In contrast to the Alaskan Eskimo, Jenness portrayed the Copper Inuit at the time he visited them as still living before the deluge. Addressing this sort of dichotomy, Clifford argues that cultures in situations such as that faced by the Copper Inuit are often described as being caught up in a circumstances they cannot control. He asserts that

> [i]n a salvage/pastoral setup most non-western peoples are marginal to the advancing world system. Authenticity in culture or art exists just prior to the present — but not so distant or eroded as to make collection or salvage impossible. Marginal, non-western groups constantly (as the saying goes) enter the modern world. And whether this entry is celebrated or lamented, the price is always this: local, distinctive paths through modernity vanish. These historiocities are swept up in a destiny dominated by the capitalist west and by various technologically advanced socialisms. What's *different* about peoples seen to be moving out of "tradition" into "the modern world" remains tied to inherited structures that either resist or yield to the new world but cannot *produce* it.[127]

Jenness short-circuited the salvage paradigm by denying that he had to reach back to the past to recover the essential components of Copper Inuit culture. Though elements of outside culture had begun to creep into their territory, he

[125] ibid., 98.
[126] ibid.
[127] Clifford, "Of Other Peoples," 122 (emphases in original).

presented them as being unaffected by these developments. Thus, salvage was obviated by the continued presence of pre-contact culture even as the hallmarks of contact facilitated his presence among the Inuit.

Jenness explicitly targeted his major Arctic ethnography, *The Life of the Copper Eskimos*, at a popular audience,[128] referring readers interested in more detailed scientific descriptions of the topics covered to the other publications of the Expedition.[129] Throughout, he made sure to use concepts and descriptions which would be familiar to this lay readership. For example, he continued to refer to the Inuit as being child-like in their behaviour, particularly with regard to their inability to reason or to control their tempers. He wrote that

> [t]he greatest check on theft is the extreme intimacy of social relations, everyone being aware of what is said, done, or owned by all the rest. Nevertheless a little pilfering does occur, even among themselves; and, in the absence of any established authority, the victim's only redress is by an appeal to physical force, which, with a people whose emotions, like those of children, have not come under the control of a developed temperament, frequently means murder.[130]

Given that at the time he was writing England still supported a system of capital punishment, it is curious that Jenness would view retaliatory murder as a sign of an undeveloped temperament.

Jenness's description of Inuit life was crafted to appeal to a popular readership. The Inuit behaved in a predictable way. Nothing in his ethnography would have challenged people's expectations of a primitive society. Arguing that the Copper Inuit understanding of shamanism threw "a considerable light on the

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[128] Jenness, *Life of the Copper Eskimos*, 11.
[129] ibid., 13.
[130] Diamond Jenness, "The Copper Eskimos," *Geographical Review* 4:2 (August 1917), 86.

mentality of the people,"[131] he described the rituals in extremely unflattering terms, ostensibly on the grounds that this sort of explanation did not necessarily do justice to the Inuit conception, and called for judgment to be reserved pending further examination:

> To a critical and unsympathetic outsider it may seem that a séance of this type is simply a case of palpable fraud on the part of the shaman, and of almost unbelievable stupidity and credulity on the part of the audience. A little very amateurish ventriloquism, a feeble attempt at impersonation, and a childish and grotesque blending of the human and the animal, all performed in full daylight before an audience incapable of distinguishing between fact and fancy, between things seen and things imagined, or at least so mentally unbalanced that it reacted to the slightest suggestion and hypnotised itself into believing the most impossible things — that perhaps is all there may seem to be in Eskimo shamanism.[132]

After such a careful consideration, Jenness's conclusion was no less patronising to the Inuit point of view than the hypothetical perspective he used to introduce his discussion. On the contrary, he noted that he saw nothing in the shamanistic practices that would be unknown to a European:

> Hysteria, self-hypnosis, and delusion caused by suggestion are well-known to every psychologist and medical practitioner, and everything that I witnessed could be explained on one or other of these grounds. The natives have many more tales of far more wonderful phenomena, phenomena which, if true, would be as mysterious and inexplicable as the much-discussed walking over red-hot stones that is practiced by a certain Fijian tribe. But of these marvels I myself saw nothing, and until we have the evidence of some more critical eye-witness than the Eskimo himself, it is safest perhaps to attribute them to the over-wrought imaginations of a people whose knowledge of the workings of our universe is far more limited than our own; a people who have no conception of our "natural laws," but in their place have substituted a theory of spiritual causation in which there is no boundary between the possible and the impossible.[133]

Not only did Jenness discredit insider knowledge, the emic perspective, but he

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[131] Jenness, *Life of the Copper Eskimos*, 198.
[132] ibid., 194.
[133] ibid., 217.

also demonstrated the impact of his evolutionary approach to the understanding of cultures. Because the Inuit had not attained a high level of scientific knowledge, they tended toward "the over-wrought imaginations" of an ignorant people, striving vainly to make sense of their world.

His ethnographic writings reflect the extent to which Jenness valorised the achievements of his culture at the expense of the Inuit culture. In this evolutionary framework, the Inuit will always fare poorly in comparison to the English; the impact of this form of analysis on his understanding of Inuit culture must be considered. If he were unable to see the culture on its own terms, free of an evolutionary paradigm that shaped his expectations, and therefore his results, what value does his description have?

A striking example of Jenness's evolutionary thinking is his assertion that the Copper Inuit represented a group at the transition point between a stone-age and an iron-age culture. He argued that because the Copper Inuit worked the native copper as a "malleable stone"[134] rather than smelting it, they were only at a "pseudo-metal" stage of evolution.[135] His evolutionary perspective was also evident in his discussion of Inuit custom, as distinguished from law:

> Established authority among the Copper Eskimos is unknown. ... The only law is custom, handed down from generation to generation; it alone upholds the structure of society, maintains the taboos, and regulates the relation of family to family and of man to man. Its sanction is religion, and violation of custom is punished, through spiritual powers, by sickness and death, or ill-success in hunting and fishing.[136]

However, at the same time that Jenness argued that the Copper Inuit

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[134] Diamond Jenness, "Origins of the Copper Eskimos and Their Copper Culture," *Geographical Review* 13:4 (1923), 540.
[135] ibid.
[136] Jenness, "The Copper Eskimos," 86.

were at a low evolutionary level, he also made links between it and western European culture. He noted that "[f]amily organization is, in its general features, very similar to our own,"[137] though he added the important proviso that the "[i]nterchange of wives, however, is common, polygamy frequent, and polyandry not unknown."[138] He also pointed out that "[t]he Eskimos, like ourselves, have that indefinable feeling of home in the country they have known since childhood."[139] In his discussion of hunting and fishing, he mentioned that "[w]hat he lacks in weapons, however, the Eskimo makes up for in craft. All the precautions and tricks of the European hunter are known to him,"[140] and that "[p]rimitive as are the methods of fishing that the Copper Eskimos employ they are nevertheless in most cases very effective."[141] Obviously, at some points during his stay with them, the Copper Inuit were able to transcend their primitiveness and impress him with their skill and knowledge.

Jenness also commented on the similarities between Inuit and English parenting styles and methods. He gave the Inuit a rather backhanded compliment, saying that "[h]owever rude and uncultured these Eskimos may be, the bond that binds the mother to her child is an enduring one, lasting as long as life itself."[142] He also noted in passing that Inuit "[p]arents frequently massage their own children while nursing them, as our own parents do."[143] Jenness's discussion of childrearing, however, contains one of the most remarkable

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[137] ibid., 89.
[138] ibid.
[139] Jenness, *The Life of the Copper Eskimos*, 32.
[140] ibid., 146.
[141] ibid., 152.
[142] ibid., 170.
[143] ibid., 165.

passages of his entire ethnography. It is the only time that he directly confronts the prejudices of his readership, by coming down on the side of the Inuit, showing that their behaviour, while seemingly neglectful, actually makes sense in an English context:

> A casual visitor might gather the impression that children are badly cared for by their parents. Both boys and girls run about in their most wretched clothing, full of gapes and rents, often cut down, indeed, from the worn-out garments of their elders. Even their footgear is of the same description, and often it is soaking wet. It must be remembered, however, that these are their oldest clothes, and that there is always a good warm set of garments carefully stored away for special occasions. Our children do not wear their Sunday clothes at school, nor do the Eskimo children wear their cleanest and finest garments when playing about in the greasy snow in and around their houses.[144]

It is unclear why Jenness chose this area to challenge his readership, when he was happy to pander to preconceptions about primitive spirituality in his discussion of shamanism. In any event, this appreciation of Inuit rationality was an isolated event in his ethnography.

Throughout this description of his evolutionary perspective, Jenness's position is important. His view of Inuit culture relative to his own shaped the way he describes their culture and affects the value and meaning he ascribes. His lack of respect for Inuit knowledge and practices made it easy for him to advocate imposed change and assimilation of the Inuit.

As it is, the changes which were occurring at the time he was living among the Copper Inuit received scant mention in his ethnography. He mentioned the likelihood of a significant decline in population in the recent past,[145] and noted that the arrival of Europeans, while reducing the chances of famine, replaced this

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[144] ibid., 169.
[145] ibid., 37.

threat to life with imported diseases which likely caused just as many, if not more, deaths.[146] In this light, Jenness ended his major ethnography with a passionate, if somewhat misguided appeal to the outside world:

> Rapid changes are taking place in the culture of the natives, and implements of iron and steel, rifles, fish-nets, open boats, European textiles and sewing-machines, European foods, cheap musical instruments and the development of trapping at the expense of hunting and sealing will work at a complete transformation within the space of a very few years. Already the new culture elements and the new teachings that are filtering in from the west have profoundly modified their social and religious ideas, and before the present generation passes away the primitiveness of the Copper Eskimos will have ceased to exist. How many will remain by that time, and whether they will be able to take any part in the development of this region depends largely on the manner in which we fulfil our trust. For in throwing open their country to outside invasion we have incurred a heavy responsibility towards the natives. We may increase the security of life among them by checking infanticide and murder, we may protect them from unscrupulous exploitation and from the ravages of intoxicating liquors, but all this will be of little avail unless we immediately take measures to secure them against the introduction of our diseases. ... The Copper Eskimos have no diseases of their own, or at least none were known up to 1916; but white men and western Eskimos are flocking into their country, and in a few more years perhaps they too will fall victim to some of the scourges of our civilization. It may be impossible to prevent this calamity entirely, but at least we could do something to check it.[147]

Jenness seemed to assume that the Copper Inuit were doomed; either they would cease to exist as a distinct culture as they lost their primitiveness in the face of incursions from the west and the south, or they would succumb to the ravages of imported diseases. Perhaps this rhetoric was simply a way of valorising his own research, positioning it as the only record of their authentic life which was then disappearing; or perhaps he was being pessimistic to the point of fatalism, having realised that the invasion of the north, in which he himself had

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[146] ibid., 43.
[147] ibid., 242.

played a part, would forever change the lives of the Copper Inuit, and certainly not for the better.

IV. THE WIDER CONTEXT

It is important to place Jenness's Arctic research in a wider methodological and theoretical context.

Developments in British Anthropology

Though there is still some debate about the extent and impact of the development of functionalist method and theory on British anthropology of the 1920s,[1] the publication, in 1922, of Malinowski's *Argonauts of the Western Pacific* and Radcliffe-Brown's *The Andaman Islanders*[2] marked a significant shift in thinking from previous British approaches.

Looking back on pre-functionalist British anthropology, a number of observers have outlined its essential characteristics. It was focussed on the collection of data,[3] and it took as its goal reconstructing cultural histories in an archaeological way, based on its assumption "that 'customs' are imperishable artifacts, as hard and enduring as flint tools and sherds of pottery."[4] Theoretical concerns were central to the discipline.[5]

The new approach, however, emphasised field research more than armchair theorisation,[6] and rejected "the whole ethnological enterprise."[7] Another important distinction, "the rejection of survivals[,] was a precondition of the emergence of functionalism, insofar as it facilitated (and necessitated) the

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[1] Kuper, *Anthropology and Anthropologists*, 1; Stocking, *After Tylor*, 283.
[2] A. R. Radcliffe-Brown, *The Andaman Islanders* (New York: Free Press, 1964 [originally published in 1922]).
[3] Kuper, *Anthropology and Anthropologists*, 5.
[4] Edmund Leach, "On the 'Founding Fathers,'" *Current Anthropology* 7:5 (December 1966), 566.
[5] Kuper, *Anthropology and Anthropologists*, 5.
[6] ibid.
[7] ibid., 3.

explanation of sociocultural phenomena without reference to diachronic assumption."[8] In the beginning, this emphasis on synchronic analysis "was not necessarily seen as an approach which would displace evolutionist and diffusionist concerns, but rather as something to be added to them."[9] These two developments, the emphasis on field research and the adoption of a synchronic perspective, were interrelated; it is difficult to untangle which came first and led to the other. In an early survey of fieldwork methods, Audrey Richards outlined the relation of method to theory in functionalist anthropology:

> In the type of fieldwork which I have described as "intensive sociological investigation," the anthropologist is not primarily interested in tracing the relationship between cultures, whether in a limited geographical area or in chronological sequence, but in discovering the intimate interrelations between the different institutions of one particular culture. He is endeavouring to discover the nature of human culture by means of a very detailed study of one individual society, of the organization of its members' activities and interests, and of the forces that keep them united as a group.[10]

The new method and theory were quickly accepted by a majority of British anthropologists, and soon "[e]arlier anthropological life forms, before social anthropology, were viewed as positively antediluvian, and of little value."[11] As the new approach gained momentum, its practitioners produced ethnographies perceived as far superior to those produced by anthropologists working in the earlier tradition:

> The new generation of writers were no longer obliged to prove the value of their methodology as the quality of their material was proof enough of the

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[8] Stocking, *After Tylor*, 320n.
[9] Kuper, *Anthropology and Anthropologists*, 8.
[10] Audrey I. Richards, "The Development of Field Work Methods in Social Anthropology," in *The Study of Society: Methods and Problems*, ed. by Frederic Bartlett, M. Ginsberg, E. J. Lindgren, and R. H. Thouless (London: Routledge and Kegan Paul, 1939), 283.
[11] Urry, "The Search for Unity," 14.

superiority of their fieldwork methods. Their accounts were not dry reports of objectified facts; they could include details of their personal involvement in research and most included sketches of key informants and individual biographies.[12]

Of all the anthropologists responsible for the development of the functionalist approach, Malinowski and Radcliffe-Brown were at the forefront. Adam Kuper explains their contributions to the field in terms useful for my analysis:

> Malinowski brought a new realism to social anthropology, with his lively awareness of the flesh-and-blood interests behind custom, and his radically new mode of observation. Radcliffe-Brown introduced the intellectual discipline of French sociology, and constructed a more rigorous battery of concepts to order the ethnographic materials.[13]

At the same time as I stress the congruency of their approaches, it is important to remember that Malinowski and Radcliffe-Brown also differed in important aspects of their approach to ethnographic materials. Phyllis Kaberry, a student of Malinowski, enumerated some of the similarities in order to explain the differences in the two approaches:

> Both denied the value of speculative reconstruction of history; both emphasized the need to study existing social institutions; both conceived of cultures as wholes; both developed a concept of function in terms of the social effects of any custom or institution. But here they diverged, and the difference in approach is nowhere more evident than in their ethnographic monographs. If, in Malinowski's, the people are always with us (and, some would say, too much with us), in Radcliffe-Brown's they are conspicuous by their absence; they are the invisible facts. One explanation lies not so much in Radcliffe-Brown's preoccupation with structure, but rather in the nature of the "effects" which he thought most significant. They are also the most difficult to document from empirical data.[14]

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[12] Urry, "'Facts' to Argument," 56.
[13] Kuper, *Anthropology and Anthropologists*, 35.
[14] Phyllis Kaberry, "Malinowski's Contribution to Field-work Methods and the Writing of Ethnography," in *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*, ed. by Raymond Firth (London: Routledge and Kegan Paul, 1957), 75.

Concentrating on Malinowski's fieldwork methods and Radcliffe-Brown's theoretical innovations, I will outline their contributions to contemporary anthropological research in the British context. While both Malinowski's theory and the relationship between Radcliffe-Brown's fieldwork methodology and his theories would prove of interest to later scholars, I have chosen to limit my discussion to a single factor of each anthropologist's body of work.

Malinowski's Fieldwork

Though Kuper asserts that Malinowski's fieldwork in the Trobriands represented the beginning of modern anthropological fieldwork,[15] other scholars have taken a more nuanced view. In a comprehensive survey of twentieth-century fieldwork methods, Urry has argued that the shift to intensive residential fieldwork was neither sudden nor the result of the efforts of one person. He maintains that this development

> went hand in hand with other developments both within and outside anthropology: a more critical approach to ethnographic sources, changing theoretical interests, the increasing professionalization of academic anthropology, and the easy access to remote areas of the world due to the expansion of European colonial control and methods of communication.[16]

Stocking echoes this perspective, adding that "the emergence of modern fieldwork was a multifaceted process to which many individuals before and after Malinowski contributed."[17]

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[15] Kuper, *Anthropology and Anthropologists*, 12.
[16] Urry, "A History of Field Methods," 35.
[17] George W. Stocking, Jr., "The Ethnographic Sensibility of the 1920s and the Dualism of the Anthropological Tradition," in *Romantic Motives: Essays on Anthropological Sensibility*, ed. by

However, as Stocking notes, the first chapter of *Argonauts* is remarkable because it contains "Malinowski's deliberate archetypification of the role of 'the Ethnographer' [which] offered, both to prospective anthropologists and to various publics at the boundaries of the developing discipline, a powerfully condensed (yet expansive) image of the anthropologist as the procurer of exotic esoteric knowledge of potentially great value."[18] In attempting to understand the context of Jenness's Arctic ethnography, the fact that Malinowski's first chapter is less a description of his own fieldwork experiences than a prescription for future researchers[19] does not diminish its importance in a wider context. Malinowski was presenting a forward-looking picture of research methods, trying to enunciate a style of fieldwork suitable for collecting the sort of data he felt was necessary for an anthropological understanding of a culture.

On the one hand, when it was published, *Argonauts* was seen "as a useful addition to the literature rather than as a call to revolution."[20] On the other hand, different observers argued that "the type of material Malinowski had collected, and the manner in which he presented it, did amount to a radically new view of a 'primitive culture.'"[21] Importantly, Stocking stresses that this novelty was a result of the fact that the opening chapter

> was a "mythic charter" for what was to become the central ritual of social anthropology. A motivating myth for "apprentice ethnographers," it reassured them that a difficult and even dangerous task was possible, that those who would follow in Malinowski's charismatic methodological

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George W. Stocking, Jr. History of Anthropology Vol. 6 (Madison: University of Wisconsin Press, 1989), 209.

[18] ibid.

[19] Stocking, "The Ethnographer's Magic," 104.

[20] Kuper, *Anthropology and Anthropologists*, 9.

[21] ibid.

> footsteps could in fact "get the work done" — even to the point where it would become a matter of disciplinary routine.[22]

Though some would question the extent to which his work was revolutionary,[23] Malinowski's concept of fieldwork undoubtedly reshaped conceptions of anthropological research.

This concept was remarkably simple. At its root, it "depended ultimately on placing oneself in a situation where one might have a certain kind of experience."[24] Malinowski himself laid it out thusly:

> What is then this ethnographer's magic, by which he is able to evoke the real spirit of the natives, the true picture of tribal life? As usual, success can only be obtained by a patient and systematic application of a number of rules of common sense and well-known scientific principles ... . The principles of method can be grouped under three main headings; first of all, naturally, the student must possess real scientific aims, and know the criteria of modern ethnography. Secondly, he ought to put himself in good conditions of work, that is, in the main, to live without other white men, right among the natives. Finally, he has to apply a number of special methods of collecting, manipulating, and fixing his evidence.[25]

In terms of "real scientific aims," Malinowski focussed on the important role that ethnographers had to play in the description of other cultures. Noting that Europeans had lived in the Trobriands for decades, he argued that even "with constant opportunities of observing the natives and communicating with them" these Europeans "hardly know one thing about [the natives] really well."[26] Into this breach could step the ethnographer, supplied with the theory and

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[22] George W. Stocking, Jr., "Maclay, Kubary, Malinowski: Archetypes from the Dreamtime of Anthropology," in *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*, ed. by George W. Stocking, Jr. History of Anthropology Vol. 7 (Madison: University of Wisconsin Press, 1991), 11.
[23] e.g., Andrzej Paluch, "Malinowski's theory of culture," in *Malinowski between two worlds: The Polish roots of an anthropological tradition*, ed. by Roy Ellen, Ernest Gellner, Grazyna Kubica, and Janusz Mucha (Cambridge: Cambridge University Press, 1988), 72.
[24] Stocking, *After Tylor*, 273.
[25] Malinowski, *Argonauts of the Western Pacific*, 6.
[26] ibid., 5.

method required to undertake a comprehensive study of the culture.

In this area, Malinowski argued that the researcher had to have a firm grasp of current theory in the field. He was careful to point out that knowledge of theory "is not identical with being burdened with 'preconceived ideas,'"[27] asserting that while they "are pernicious in any scientific work, ... foreshadowed problems are the main endowment of a scientific thinker, and these problems are first revealed to the observer by his theoretical studies."[28] Despite his emphasis on theoretical competence, Malinowski still advocated a division between research and theorising, in the vein of earlier anthropologists such as Frazer and Marett: "[t]he field worker relies entirely upon inspiration from theory. Of course he may also be a theoretical thinker and worker, and there he can draw on himself for stimulus. But the two functions are separate, and in actual research they have to be separated both in time and conditions of work."[29] To this end, he outlined his preferred method for anthropological fieldwork, both informed by theory and capable of making contributions to the further development of theory:

> the first and basic ideal of ethnographic field-work is to give a clear and firm outline of the social construction, and disentangle the laws and regularities of all cultural phenomena from the irrelevances. The firm skeleton of the tribal life has to be first ascertained. This ideal imposes in the first place the fundamental obligation of giving a complete survey of the phenomena, and not of picking out the sensational, the singular, still less the funny and the quaint. ... The field Ethnographer has seriously and soberly to cover the full extent of the phenomena in each aspect of tribal culture studied, making no difference between what is commonplace, drab, or ordinary, and what strikes him as astonishing and out-of-the-way. At the same time, the whole area of tribal culture *in all its respects* has to be gone over in research. The consistency, the law and order which obtain within each aspect make also for joining them into one coherent

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[27] ibid., 8-9.
[28] ibid., 9.
[29] ibid.

whole.[30]

Emphasising the difficulty in finding and relying upon a native expert for data, Malinowski stressed that the best research method "for an Ethnographer consists in collecting concrete data of evidence and drawing the general influences for himself."[31]

Malinowski's second rule for ethnographers was that they had to ensure that their fieldwork took place under the proper conditions. In his formulation, these "consist mainly in cutting oneself off from the company of other white men, and remaining in as close contact with the natives as possible, which really can only be achieved by camping right in their villages."[32] He believed that immersion in native culture would lead to better information than short contact with paid informants.[33] Such an immersion in native life, what he called "being really in contact with them,"[34] meant that the ethnographer's "life in the village, which at first is a strange, sometimes unpleasant, sometimes intensely interesting adventure, soon adopts quite a natural course very much in harmony with his surroundings."[35] In this mode, Malinowski wrote about how he woke up "every morning to a day, presenting itself to me more or less as it does to the native."[36]

This short example shows the tenuous position held by the fieldworker. Malinowski's introduction contains the paradigmatic statement of the paradoxical

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[30] ibid., 11 (emphasis in original).
[31] ibid., 12.
[32] ibid., 6
[33] ibid., 7.
[34] ibid.
[35] ibid.
[36] ibid.

position of the participant-observer in native cultures, of being both inside and outside of the native culture, of being both like and unlike the people he is studying.[37] He noted that "as they knew that I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished by regarding me as part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco."[38] The need to learn native standards of etiquette and conduct was central to the success of Malinowski's style of fieldwork:

> I had to learn how to behave, and to a certain extent I acquired "the feeling" for native good and bad manners. With this, and with the capacity of enjoying their company and sharing some of their games and amusements, I began to feel that I was indeed in touch with the natives, and this is certainly the preliminary condition of being able to carry on successful fieldwork.[39]

Coming to such an understanding of the native point of view was central his third rule of ethnographic research, concerning the best method for describing native cultures.

The collection, recording, and manipulation of evidence was the final concern Malinowski addressed in his introduction to *Argonauts*. By choosing to live in the village, the ethnographer would be able to observe "the customs, ceremonies and transactions over and over again,"[40] seeing "examples of [native] beliefs as they are actually lived through, and the full body and blood of actual native life."[41] This attention to detail was the one area where Malinowski thought

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[37] ibid., 21.
[38] ibid., 8.
[39] ibid.
[40] ibid., 18.
[41] ibid.

professional ethnographers had something to learn from earlier pre-professional workers:

> In certain results of scientific work — especially that which has been called "survey work" — we are given an excellent skeleton, so to speak, of the tribal constitution, but it lacks flesh and blood. We learn much about the framework of their society, but within it, we cannot perceive or imagine the realities of human life, the even flow of everyday events, the occasional ripples of excitement over a feast, or ceremony, or some singular experience. In working out the rules and regularities of native custom, and in obtaining a precise formula for them from the collection of data and native statements, we find that this very precision is foreign to real life, which never adheres rigidly to any rules. It must be supplemented by the observation of the manner in which a given custom is carried out, of the behaviour of the natives in obeying the rules so exactly formulated by the ethnographer, of the very exceptions which in sociological phenomena almost always occur.[42]

Malinowski argued that these details, which he termed "*the imponderabilia of actual life*,"[43] could only be gathered through the observation of the ethnographer; it would be impossible to ask people to describe all the multitude of activities they undertake each day.[44] As well, it was necessary to understand the motivations for action, "the natives' views and opinions and utterances."[45] His emphasis on these all these details showed the divergence between professional and amateur ethnographers, and pointed to his ultimate goal in the collection of these materials: "[a]ll these facts can and ought to be scientifically formulated and recorded, but it is necessary that this be done, not by a superficial registration of details, as is usually done by untrained observers, but with an effort at penetrating the mental attitude expressed in them."[46] At the

[42] ibid., 17.
[43] ibid., 18 (emphasis in original).
[44] ibid.
[45] ibid., 22.
[46] ibid., 19.

same time, he argued for the importance of letting these details, these facts, "speak for themselves."[47]

The best way to allow the facts to speak for themselves, according to Malinowski, was to record native speech verbatim and to try to come to terms with native concepts.[48] This approach would move the ethnographer towards the final goal of study, which Malinowski defined as "grasp[ing] the native's point of view, his relation to life, to realise *his* version of *his* world."[49] Malinowski focussed on the individual, asserting that "[t]o study the institutions, customs, and codes or to study the behaviour and mentality without the subjective desire of feeling by what these people live, of realising the substance of their happiness — is, in my opinion, to miss the greatest reward which we can hope to obtain from the study of man."[50] However, he was not interested so much in what individuals thought as individuals; his main emphasis was on the ways in which individuals were shaped by their culture, while simultaneously shaping it.[51]

In the end, Malinowski had a very generous reason for embarking upon the study of other cultures, believing that such study would also at the same time reveal much about Western culture. He hoped that

> [p]erhaps as we read the account of these remote customs there may emerge a feeling of solidarity with the endeavours and ambitions of these natives. Perhaps man's mentality will be revealed to us, and brought near, along some lines which we never have followed before. Perhaps through realising human nature in a shape very distant and foreign to us, we shall have some light shed on our own. In this, and in this case only, we shall be justified in feeling that it has been worth our while to

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[47] ibid., 20.
[48] ibid., 23.
[49] ibid., 25.
[50] ibid.
[51] ibid., 23.

understand these natives, their institutions and customs.[52]

In an article also published in 1922, Malinowski expanded his discussion of anthropology's role in changing Western perceptions of native cultures. In particular, he argued that "it would be much better if ethnographical knowledge could altogether change the average white man's whole outlook on savage morality."[53] For example, Malinowski maintained that a native "belief, which appears crude and senseless in isolation, a practice which seems queer and 'immoral,' becomes often clear and even clean if understood as part of a system of thought and practice."[54]

Urry identifies a shift in emphasis at this point in Malinowski's work from salvage to "the study of social change and culture contact" as wider interests shifted to "practical anthropology," that is, anthropology addressing colonial administration.[55] Malinowski himself argued that "[t]he survival of natives — apart from humanitarian, æsthetic, or moral considerations — is a matter of vital importance for practical purposes."[56] His nascent applied anthropology combined practical and theoretical concerns, while always stressing respect for the natives and their cultures:

> The study of savage and coloured races possesses practical value, in the first sense, for purposes of colonial administration and the management of the relations between white and coloured people. In the second sense, ethnology may be a most powerful means of widening our outlook on human nature, of allowing us to build up a correct theory of society for the future scientific guidance of human affairs.[57]

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[52] ibid., 25.
[53] Bronislaw Malinowski, "Ethnology and the Study of Society," *Economica* 2:6 (October 1922), 211.
[54] ibid., 218.
[55] Urry, "A History of Field Methods," 52.
[56] Malinowski, "Ethnology and the Study of Society," 209.
[57] ibid., 208.

Radcliffe-Brown's Theory

Radcliffe-Brown's fieldwork methods[58] are less relevant to my study than his theoretical framework. He considered his ethnography of *The Andaman Islanders*, the work upon which I have chosen to focus in this context, "an example of analytic method"[59] based upon fieldwork he characterised "as an apprentice study."[60] In fact, for most of his field data he relied upon the work of an earlier, amateur, ethnographer, E. H. Man.[61]

Radcliffe-Brown's theoretical approach to his Andaman material developed slowly. Stocking argues that the earliest surviving fragments of Radcliffe-Brown's report displayed more influence from "Haddon than by Rivers, and showed not a trace of Durkheim."[62] In fact, Stocking continues, "[i]t seems to have been quite a traditional attempt at historical reconstruction on the basis of comparative analysis of culture elements. Similarly, articles he published in 1909 and 1910 on the religion of the Andamanese reflect little of his theoretical

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[58] Stocking, *After Tylor*, 306-307. Most observers are generally dismissive of Radcliffe-Brown's fieldwork methods, with Kuper arguing that it "belongs firmly in the pre-Malinowskian era of fieldwork" and that "[t]he contrast with Malinowski's work in the Trobriands is striking. Even his [i.e., Radcliffe-Brown's] methods of data collection were inadequate" (*Anthropology and Anthropologists*, 40). Urry also highlights the extent to which Radcliffe-Brown's fieldwork was deficient:

> In fact a great deal of the material Radcliffe-Brown collected in the field [in the Andaman Islands] between 1906 and 1908 proved either insufficient or of little value to him in constructing an ethnographic account, at least in the form which he later considered suitable to express the anthropological ideas he wished to convey. Radcliffe-Brown's thinking about anthropology underwent a major sea-change between his return from the Andaman Islands and the time he completed the manuscript of his book *The Andaman Islanders* in 1913 (although it remained unpublished until 1922). ("'Facts' to Arguments," 51)

[59] Kuper, *Anthropology and Anthropologists*, 41.
[60] ibid.
[61] ibid.
[62] Stocking, *After Tylor*, 308.

viewpoint."[63] In another setting, Stocking maintains that this earliest draft is almost Boasian "in its historical orientation."[64] However, likely in the context of some work he undertook on the religions of the Australian Aborigines after his return from the Andaman Islands, Radcliffe-Brown was exposed to the works of Emile Durkheim and adopted the theoretical approach of the latter.[65]

The published monograph, though informed by Durkheim's theory, remained more focussed on description of ethnographic materials than on the explication of a particular theoretical approach to their explanations. However, *The Andaman Islanders* is the preliminary culmination of a number of years of theoretical thinking and development. In one way, argues David Tomas, it marks

> a subtle transformation in [Radcliffe-Brown's] method from an ostensibly context-sensitive inductive process into an authoritarian deductive methodology: not only was his ethnography reconstructed *post factum* but he never returned to the Andaman Islands in order to open the inductive loop and again observe. In fact his method precluded this possibility, for it presented as its final reconstituted product a closed timeless picture of the integrated organic life of Andamanese culture. It might be said that he in fact transformed the nature of ethnography: from the privileged domain of practice, it became the site for the authoritative work of theory.[66]

Like Malinowski, Radcliffe-Brown argued that researchers must be conversant with contemporary theoretical developments. However, in his emphasis on the necessity of an individual researcher undertaking both observation and hypothesisation, Radcliffe-Brown differed from Malinowski in an important respect, and pointed the way forward from the pre-functionalist division

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[63] ibid.
[64] Stocking, ed., "Dr. Durkheim and Mr. Brown," 109.
[65] ibid., 106-111; Urry, "'Facts to Arguments," 51.
[66] David Tomas, "Tools of the Trade: The Production of Ethnographic Observations on the Andaman Islands, 1858-1922," in *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*, ed. by George W. Stocking, Jr. History of Anthropology Vol. 7 (Madison: University of Wisconsin Press, 1991), 102-103.

of labour. He argued that if ethnology were to develop as a science,

> the elaboration of hypotheses and the observation and classification of facts must be carried on as interdependent parts of one process, and no advantage, but rather great disadvantage, results from the false division of labour whereby theorists and observers work independently and without systematic cooperation. The most urgent need of ethnology at the present time is a series of investigations of the kind here attempted, in which the observation and the analysis and interpretation of the institutions of some one primitive people are carried on together by the ethnologist working in the field.[67]

Radcliffe-Brown outlined two possible approaches for "dealing with the facts of culture or civilisation amongst primitive peoples who have no historical records."[68] The first he termed the *ethnological*, which he characterised as an attempt to "reconstruct hypothetically the past history of a people in its main outlines,"[69] based on "the co-ordinated study of physical characters, language, and the various elements of culture, and with the help of such archaeological knowledge as is available."[70] While he did not deny the fact that many people might find such research interesting, he argued that it "has given rise to a literature of which a large part is of little or no scientific value, owing to the utter disregard of the laws of scientific evidence and the need for the verification of hypotheses."[71] As a result, Radcliffe-Brown continued, this approach "does not often provide, and does not seem likely to provide, results that will be of any assistance to the administrator or the educator in the solution of the practical problems with which he is faced."[72]

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[67] Radcliffe-Brown, *The Andaman Islanders*, 231-232.
[68] ibid., 39.
[69] ibid.
[70] ibid.
[71] ibid.
[72] ibid.

In place of the ethnological method, Radcliffe-Brown argued that a *sociological* approach must be taken. Stressing the fundamental interconnection of sociology and psychology, he outlined the goal of this approach as using sociological and psychological laws to understand the institutions of another culture. This approach would aid administration and education because it "would enable the anthropologist to foretell with some degree of certainty what would be the general effects on the life of a tribe of an attempt to abolish the custom in question."[73]

Though he disavowed historical reconstructions, Radcliffe-Brown's ultimate goal was to develop a picture of the pre-contact social organisation of the Andamanese.[74] He asserted rather confidently that "[i]t is fairly easy ... to discover from the natives themselves what was the constitution of the society in former times,"[75] though he recognised at the same time the degree to which the society had changed: "[t]he changes that have taken place in recent years have been extensive, the most important being the diminution in numbers and the

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[73] ibid., 40. He argued that the sociological method was the most useful for the administrator, and offered advice to the researcher:

> But this does not mean that the social anthropologist is to concern himself with the actual problems that face the administrator and the legislator at the present time. The scientist must always keep himself free from concern with the practical applications of the [sociological] laws that it is his business to discover, leaving that to others specially qualified for such work and devoting their whole energies to it. And this is particularly important in such a science as social anthropology, where the elimination of personal prejudice and bias is already so difficult, and would be impossible if we did not rigorously exclude from our theoretical all *immediate* practical considerations. It is only too easy to find in the facts of social history evidence, plausible enough, for our pet political theories. (A. R. Radcliffe-Brown, "Some Problems of Bantu Sociology," *Bantu Studies* 1 [1922], 39-40 [emphasis added].

He was careful to differentiate between impartial research undertaken as the basis for the formulation of administrative policies, and research undertaken with the goals of administration in mind, arguing that the former offered the best strategy for understanding the ways in which administrative decisions would affect native life (ibid., 40).

[74] Radcliffe-Brown, *The Andaman Islanders*, 22.

[75] ibid.

merging together of what were formerly distinct and often hostile communities."[76] These latter comments show the extent to which Radcliffe-Brown did not problematise the development of theory from ethnographic data.

He did recognise that there were some problems with his data, but this did not stop him from using it as the basis of broad theoretical generalisations. For example, in a discussion the Andamanese system of relationships Radcliffe-Brown offered this disclaimer in a footnote:

> Although I tried to learn all that I could on the subject, it is quite certain that I did not learn all that was to be learnt, and it is possible that further enquiry might have shown that I was mistaken in some of my observations. The difficulty of being really sure on these matters is due (1) to the fact that the breaking-up of the old local organisation has produced many changes in their customs, and (2) to the difficulty of questioning the natives on matters connected with relationships when they have no words in their language to denote any but the simplest relationships.[77]

Radcliffe-Brown attempted to get around these problems by working in an explicitly deductive manner. Starting with the assumption that the explanation of customs or beliefs of a particular group has its roots in "some general psychological hypothesis,"[78] he asserted that "[t]he sound rule of method is therefore to formulate clearly and explicitly the working hypothesis on which the interpretation is based. It is only in this way that [the custom or belief's] value can be properly tested."[79]

He was also willing to extrapolate from his Andamanese data generalised explanations of primitive society. With great self-assurance, he asserted that

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[76] ibid.
[77] ibid., 82n1.
[78] ibid., 232.
[79] ibid.

> I have assumed a certain working hypothesis, and I have shown that on the basis of this hypothesis there can be built up a satisfactory explanation of the customs and beliefs of the Andamanese. But the hypothesis is of such a nature, stating or involving as it does certain sociological or psychological laws and principles, that if it be true for one primitive people it must be true for others, and indeed, with necessary modifications must be true of all human society. Such a hypothesis, it is obvious, cannot be adequately tested by reference only to one limited set of facts, and it will therefore be necessary, if it is to become something more than a hypothesis, to test its application over a wider range of ethnological facts.[80]

The theory that Radcliffe-Brown had developed became generally known as the organic or biological analogy.[81] He outlined his hypothesis about society in this way:

> Every custom and belief of a primitive society plays some determinate part in the social life of the community, just as every organ of a living body plays some part in the general life of the organism. The mass of institutions, customs and beliefs forms a single whole or system that determines the life of the society, and the life of the society is not less real, or less subject to natural laws, than the life of an organism. To continue the analogy, the study of the meaning of savage customs is a sort of social physiology, and is to be distinguished from the study of origins, or changes of custom in just the same way that animal physiology is distinguished from the biology that deals with the origin of species, the causes of variation, and the general laws of evolution.[82]

As well, Radcliffe-Brown applied an assumption often associated with functionalist theory, that useless customs or beliefs do not last long in a society, to argue against the notion of survivals. In a sense, he was arguing that savages were as rational as Westerners, and that they also had a sense of utility in terms of both social organisation and beliefs. He wrote that he found that among the Andamanese

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[80] ibid., 400.

[81] cf. Edmund Leach, "Rethinking Anthropology," in his *Rethinking Anthropology*. London School of Economics Monograph on Social Anthropology No. 22 (New York: Humanities Press, 1961), 6.

[82] Radcliffe-Brown, *The Andaman Islanders*, 229-230.

> [c]ustoms that seem at first sight meaningless or ridiculous have been shown to fulfil most important functions in the social economy, and similarly I hope to prove that the tales that might seem merely the products of a somewhat childish fancy are very far indeed from being merely fanciful and are the means by which the Andamanese express and systematise their fundamental notions of life and nature and the sentiments attaching to those notions.[83]

Like Malinowski, Radcliffe-Brown was arguing that the people he studied were not irrational savages but rather had their own ways of fulfilling the functions required of each society.

In short, Radcliffe-Brown is important to my analysis of Jenness not only because of his theoretical developments, but also because of the ways in which he developed his theory and applied it to his data. He was explicit in his deductive approach, and showed the ways in which it shaped his ethnographic account. He also recognised the innate rationality of the people he studied.

Developments in Americanist Anthropology

Getting a grasp on contemporary trends, and selecting appropriate examples of these trends, is somewhat more difficult in an Americanist context than in the British field. While appreciating the diversity of the Americanist approach developed by Boas and his students in the interwar period,[84] I have chosen for the purposes of my comparison to focus on an article published by Boas in *American Anthropologist* in 1920, and on the contemporary work of Edward Sapir, Jenness's direct superior at the National Museum during the time

[83] Radcliffe-Brown, *The Andaman Islanders*, 330.
[84] Darnell, *Invisible Genealogies*, 35, 12; George W. Stocking, Jr., "Ideas and Institutions in American Anthropology: Thoughts Toward a History of the Interwar Years," in *Selected Papers from the American Anthropologist 1921-1945*, ed. by George W. Stocking, Jr. (Washington: American Anthropological Association, 1976), 1-50.

the latter was completing his Arctic ethnographies. Urry outlines some of the developments in this period, helpfully drawing contrasts with contemporary developments in Britain:

> Unlike the situation in Britain, American anthropologists did not view the revolution that occurred in the their subject in terms of a transformation in field methods but instead in terms of methods of analysis — a victory for Boas' critiques of evolutionary explanations. In terms of ethnographic inquiry there had been no major change, there had never been a major division of labour between collectors and experts in American anthropology and first hand ethnographic research into American Indian cultures had always been the norm. In part, this was a reflection of the essentially pragmatic nature of nineteenth century American life and of the fact that anthropology was viewed as a science rather than an adjunct to a literary tradition as it had been in nineteenth century Britain.[85]

In this section I will outline both Boas's and Sapir's current theoretical approaches, providing the final piece of my context for Jenness's Arctic work.

Boas began his article, "The Methods of Ethnology,"[86] by attacking previous theoretical approaches to the study of culture. Continuing a quarter-century critique of evolutionary thinking, he asserted that the unilineal evolutionary approach "presupposes that the course of historical changes in the cultural life of mankind follows definite laws which are applicable everywhere, and which bring it about that cultural development is, in its main lines, the same among all races and all peoples."[87] On the other hand, he argued, the diffusionist approach "assumed that identity of development in two different parts of the globe must always be due to migration and diffusion. On this basis historical contact is demanded for enormously large areas. The theory demands

[85] Urry, "A History of Field Methods," 55.
[86] Franz Boas, "The Methods of Ethnology," *American Anthropologist* 22:4 (October-December 1920), 311-321.
[87] ibid., 311.

a high degree of stability of cultural traits such as is apparently observed in many primitive tribes."[88]

Boas's main critique of these approaches was that their emphasis on "obtaining a consistent picture of cultural development"[89] led to their arbitrary application to cultures irregardless of their correlation to the data. Without proving, or even trying to prove, the validity of their interpretations, these approaches were, in Boas's terms, "essentially forms of classification of the static phenomena of culture according to two distinct principles, and interpretations of these classifications as of historical importance."[90]

Against each of these approaches Boas offered a major critique. Arguing that there appeared not to be any overriding psychological need that would lead to "uniform evolution the world over,"[91] he asserted that

> each cultural group has its own unique history, dependent partly upon the peculiar inner development of the social group, and partly upon the foreign influences to which it has been subjected. There have been processes of gradual differentiation as well as processes of levelling down differences between neighbouring cultural centers, but it would be quite impossible to understand, on the basis of a single evolutionary scheme, what happened to any particular people.[92]

Boas used a similar method in dismantling the diffusionist approach. Maintaining that there was no evidence that primitive cultures experienced the long periods of stability assumed by the diffusionists, he asserted instead that

> [w]herever primitive conditions have been studied in detail, they can be proved to be in a state of flux .... Periods of stability are followed by periods of rapid change. It is exceedingly improbable that any customs of

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88 ibid., 312.
89 ibid., 313.
90 ibid.
91 ibid., 317.
92 ibid.

> primitive people should be preserved unchanged for thousands of years. Furthermore, the phenomena of acculturation prove that a transfer of customs from one region into another without concomitant changes due to acculturation, are very rare.[93]

Identifying the evolutionary and diffusionist approaches with European anthropology, Boas went on to outline the method then being employed by American anthropologists. Central to the approach of the latter group, in his formulation, was their primary emphasis on "the dynamic phenomena of cultural change"[94] and their attempt "to elucidate cultural history by the application of the results of their studies."[95] Also significant was the fact that American scholars generally "relegate the solution of the ultimate question of the relative importance of parallelism of cultural development in distant areas, as against worldwide diffusion, and stability of cultural traits over long periods to a future time when the actual conditions of cultural change are better known."[96]

In terms of the study of culture history, Boas asserted that this was foremost an historical problem. The solution to this problem, he maintained, came from knowing "not only how things are, but how they have come to be."[97] Given that historical evidence about cultures studied ethnologically is generally limited to archaeological data, it was necessary to use indirect methods, "based on the comparison of static phenomena combined with the study of their distribution,"[98] to examine the history of a culture. While admitting that "we can never hope to obtain incontrovertible data relating to the chronological sequence

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[93] ibid., 317-318.
[94] ibid., 314.
[95] ibid.
[96] ibid.
[97] ibid.
[98] ibid., 314-315.

of events,"[99] Boas maintained that "certain general broad outlines can be ascertained with a high degree of probability, even of certainty."[100] If these indirect methods were used in the study of a culture, he argued, the illusion "of absolute stability which is conveyed to the student who sees a certain people at a certain time"[101] would be shattered.

At the same time as he outlined a methodology based on the examination on the comprehensive examination of cultural features, Boas also pointed out the need to understand cultures on the level of their individual members. In this area, he emphasised "the important problem of the relation of the individual to society, a problem that has to be considered whenever we study the dynamic conditions of change."[102] He outlined a kind of feedback relationship between a culture and the psychology of its members:

> The activities of the individual are determined to a great extent by his social environment, but in turn his own activities influence the society in which he lives, and may bring about modifications in its form. Obviously, this problem is one of the most important ones to be taken up in a study of cultural changes. It is also beginning to attract the attention of students who are no longer satisfied with the systematic enumeration of standardized beliefs and customs of a tribe, but who begin to be interested in the question of the way in which the individual reacts to his whole social environment, and to the differences of opinion and of mode of action that occur in primitive society and which are the causes of far-reaching changes.[103]

Sapir, a former student of Boas, was Jenness's superior at the Museum in Ottawa. A leader in the study of interactions between culture and psychology, he, along with Margaret Mead and Ruth Benedict, moved the discipline in the

[99] ibid., 315.
[100] ibid.
[101] ibid.
[102] ibid., 316.
[103] ibid.

1920s "from trait-oriented survey ethnology to a more processural focus on what Mead and Benedict called culture and personality";[104] Sapir termed this approach the study of "'the impact of culture on personality.'"[105] Darnell argues that Sapir's "interest in the individual Indian was not unrelated to his insistence on the uniqueness in his own culture, what he came to call 'the locus of culture' in each individual."[106]

Sapir's research methodology was based on the need to understand the perspectives of individuals in the culture being studied. Richard Preston describes Sapir's method as comprising "the description of social and cultural terms as they are defined in the lives of specific individuals-in-culture."[107] This emphasis on individual understandings, Preston stresses, however, was only in the context of shared meanings; Sapir was interested in examining the individual as she or he were embedded in their culture.[108]

In Sapir's opinion, his approach required an emphasis on linguistics, in particular the importance of collecting native texts in the original language; the way to understand native ways of classifying the world, in his mind the central purpose for anthropological research, he explained in a letter to Wallis, is through their language.[109] In an article written shortly after his arrival at the National Museum outlining the areas of research he hoped to see undertaken in the coming years, Sapir emphasised the importance of linguistics to all research

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[104] Darnell, *Invisible Genealogies*, 327.
[105] ibid.
[106] ibid., 224.
[107] Richard J. Preston, "Edward Sapir's Anthropology: Style, Structure, and Method," *American Anthropologist* 68:3 (August 1966), 1122.
[108] ibid., 1121.
[109] cited in Regna Darnell, *Edward Sapir: Linguist, Anthropologist, Humanist* (Berkeley and Los Angeles: University of California Press, 1990), 71.

projects: "all investigation of native mythology, rituals, songs and allied subjects, undertaken without the help of linguistic study, must fail to result in a complete understanding of the native concepts involved."[110] He also argued that, while the study of language is important in its own right, such investigation would also contribute to an understanding of other fields:

> While it is perfectly clear that cultural, physical and linguistic units do not need to, and in numerous instances do not, coincide, it should be emphasized that all three classes of units are to a large extent interwoven; not infrequently slim evidence for a point of reconstructed culture-history obtained from the study of one of these may be strengthened and even reduced to certainty by evidence derived from a study of one of the others.[111]

Sapir saw the Arctic as key. Though the Inuit had been studied to an extent, they still "present[ed] many problems of interest."[112] In particular, he asserted, "[s]everal of the less easily accessible tribes are as yet practically unknown. Until these have been investigated it will be difficult to undertake a satisfactory analysis of Eskimo culture as a whole, and, consequently, of its relations to the neighbouring cultures."[113] The anthropological component of the Canadian Arctic Expedition's research programme was intended to fill some of the gaps in understandings of Inuit groups and their relation to other Aboriginal groups in Canada's north.

It is obvious that American anthropology, like British anthropology, was going through significant changes in the years after the First World War. In particular, anthropologists working in both traditions were moving away from an

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[110] Sapir, "An Anthropological Survey of Canada," 791n4.
[111] ibid., 790.
[112] ibid., 791.
[113] ibid.

evolutionary perspective and striving for an understanding of the "native" point of view. These developments were occurring simultaneously with Jenness's production of his Arctic ethnographies. However, as I will discuss next, his works show no evidence that Jenness engaged with such theory.

V. DIAMOND JENNESS AND CONTEMPORARY ANTHROPOLOGY

Jenness's Arctic Oeuvre

Jenness's Arctic ethnography was clearly a significant contribution to scholarship on the Inuit. In the context of other materials produced by his colleagues in the Canadian Arctic Expedition, S. Jenness notes that his father's "share of the reports issued by the Canadian government with the results of the … Expedition … amounted to four volumes, totalling 1436 pages, far exceeding the contribution of any other member of the Expedition. Collectively they constitute the definitive early work on the Copper Eskimos."[1] Riches raises the issue that perhaps the early ethnographers of the Inuit, including Jenness, did so thorough a job as to undermine latter attempts at describing Inuit culture:

> I would suggest the possibility that more recent anthropological research on the Eskimo has been overwhelmingly constrained by the spectacular successes of the early "primary" ethnographers — among them Boas, Rasmussen and Jenness — whose researches underpin an image of the Eskimo as an "exotic" hunting society.[2]

A comparative examination of his Arctic ethnography and his earlier New Guinea work suggests that Jenness had an ambivalent relationship with the methodology and theoretical approaches of both his mentor and his contemporaries. Like his teacher, Marett, Jenness operated within an evolutionary framework; unlike Marett, who emphasised culture, however, Jenness focussed on a racial hierarchy. That is, while Marett was interested in the development of cultural elements over time, Jenness criticised the Inuit and Eskimo cultures for not being as highly developed as his own. Also, unlike

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[1] Jenness, "Preface," 624-625.
[2] Riches, "The Force of Tradition," 72.

Marett, Jenness did not concentrate on trying to understand the thought patterns of the Inuit, as the former had emphasised. One revealing quotation in this context is Jenness's comment in his diary about three Inuit women: "The three sisters form an interesting trio — the woman just past her prime — the woman in her prime ... — and the girl just preparing for marriage. It would be still more interesting if one could discover their different outlooks upon life."[3] When he did attempt to address Inuit psychology, he tended to describe the people he met as childlike, reflecting a kind of Victorian chauvinism on his part and reducing the insightfulness of his descriptions.

At the same time, Jenness's Arctic research methods would certainly have been recognised by Malinowski as meeting the latter's standard for anthropological rigour. Richling has argued that Jenness's "Coronation Gulf fieldwork is a far better illustration of what Malinowski professed to have done in the Trobriand Islands than what Malinowski himself actually did ... there."[4] Certainly, Jenness's desire to accompany an Inuit family on its summer travels demanded a complete immersion into their life. The high mobility of the groups required that he travel and live with them for seven months, rather than pitching his own tent in a permanent village. Thus, unlike Malinowski, he had no refuge from his companions. Because he also had to pack goods on foot, he could not transport reading material to alleviate what Malinowski called the "periods of

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[3] Jenness, *Arctic Odyssey*, 371.
[4] personal communication, 06 December 2000; Richling, "An Anthropologist's Apprenticeship," 81.

despondency"[5] which often confronted the ethnographer in the field.

In many ways, Jenness's work reflected the split between ethnography and theoretical innovation that was current at the time of his training. In Radcliffe-Brown's terms, it was ethnological, rather than sociological, emphasising hypothetical historical reconstructions rather than descriptions of contemporary social structures. Jenness, for example, did not construct an integrated, functional portrait of Inuit culture at the time that he visited them. As well, Jenness's elision of his own theoretical approach in his ethnographic accounts ran counter to Radcliffe-Brown's position that anthropologists should approach the study of a culture in deductive terms, setting a research hypothesis which could be tested by analysis of the ethnographic data collected in the field.

In the early 1920s, both Malinowski and Radcliffe-Brown examined the potential for applying anthropological knowledge to practical considerations of administration in British colonies. The former emphasised the ways in which the dominant culture could benefit from the study of colonised cultures, while the latter outlined the ways in which careful sociological research could contribute to the development of policy. Jenness also addressed the practical applications of his research, focussing on the roles that the Inuit could play in the economic exploitation of their territory:

> There is the further question as to what use could be made of the Copper Eskimos in the future development of the country. At the present time the only asset which the country possesses is its fur. These Eskimos should be as successful trappers as any in the North, once they have learned the value of the different kinds and qualities of fur. It is not improbable that

[5] Malinowski, *Argonauts of the Western Pacific*, 4. However, Jenness did have a copy of Homer's *The Odyssey*, which he read with great enthusiasm during his travels (Jenness, *Arctic Odyssey*, 420, 422, 427, 431, 455, 535).

> the copper deposits in the region may eventually prove of great value, but little can be expected from a purely hunting people in the way of labour for mines. Nevertheless it might be possible to utilize them in other ways, as in the handling of freight and in transport generally.[6]

Jenness viewed the Inuit as part of the environment — an obstacle in the way of economic development in the north. Their destiny, he argued, was to become pack animals transporting the materials necessary for the full realisation of the area's potential.

His interest in this field was motivated by his perception that Inuit culture was on the cusp of major change, change that would come from the outside. Richling argues that Jenness's views resulted from his concern for the well-being of the Inuit:

> His two years among the Copper Eskimos occurred at a time when they, too, were at a turning point. Faced with steadily growing numbers of traders, policemen, missionaries, and other outsiders, profound changes in native life were inevitable. Jenness had seen the harsher consequences of this process in New Guinea where so many Goodenough Islanders had become strangers in their own land. He lamented the changes that had already unfolded among native Alaskans, and feared their eventual infiltration of Coronation Gulf society.[7]

While this was superficially true for Jenness, the impetus for his approach came from his view of the Inuit as passive, compliant people unable to stand up to the influx of westernisation; they were, he believed, unable to take from the invading culture only those elements which would be of use in their setting while rejecting the rest. In his diaries, Jenness noted the Inuit reactions to outsiders:

> Another point common to them I imagine to be an undeveloped personality or rather individuality. Hence the individualist is the man of note and influence. The easy merging of one man's will into another's makes for the "tolerance" of Eskimo society, where each person does

[6] Jenness, "The Copper Eskimos," 91.
[7] Richling, "An Anthropologist's Apprenticeship," 81.

> what he likes without interference. It would account in part for the ease with which they are dominated by Europeans, their pliant wills yielding submissively to the aggressiveness of the outsider. Perhaps too it accounts in part for the hold that missionary teaching has upon the Mackenzie and Barrow natives, the driving power of the missionary forcing his convictions — in so far as they are understood — upon his auditors. Even with us it is always easier to acquiesce than to oppose.[8]

If they were only able think for themselves, he noted, they would be able to withstand the onslaught of change. At the same time, he neglected the issue of power dynamics, even in relation to the impact of his own Expedition. The Inuit, for example, had little choice but to maintain good relations with the Expedition, in order to reap certain rewards. For example, Jenness made an agreement with the family he accompanied during his research in the summer and fall of 1915, that upon his safe return to the Expedition's camp they would be given a range of desirable goods, including guns and ammunition. In case where the Inuit did not acquiesce, Jenness often grew frustrated and threatened to punish them like children.

Jenness, by taking an evolutionary approach which ranked the Inuit as stone-age, did not follow Boas's model of historical reconstruction. As well, Jenness viewed Inuit culture as static until disrupted by the invasion of an outside culture, whether Aboriginal or European. This approach denied the Inuit the agency to shape their own culture that Boas and Sapir saw both in Aboriginal groups and individuals. For example, Sapir castigated Jenness for his downplaying individual personalities in his descriptions:

> All I would note, rather hesitantly, is this, that you seem to be a little afraid of digging into your people's insides – Eskimo and whites. Could you contrive to give a somewhat livelier sense of the Eskimos as differentiated

[8] Jenness, *Arctic Odyssey*, 331.

> people, also of the more serious aspects of the personalities of your companions? As it is, you depend rather too much, it seems to me, on whimsical anecdotes to give a feeling of humanness – such anecdotes come into their own, after all, when set in a more massive context of serious portraiture than you give.[9]

In a number of ways, however, Jenness's work did follow the Boasian model. Like Boas and his students,[10] Jenness operated on the assumption that Aboriginal cultures were dying. This position led him to neglect the contemporary conditions of Inuit life and to highlight their "ancient" practices.

Jenness, however, seems to have little prolonged contact with students of Boas other than Sapir. The professional and personal relationship between Jenness and Barbeau, his colleague and fellow Oxford alumnus, was deeper and more complex. Although some work has been done on Barbeau's perspective,[11] little has been written about Jenness's perspective. Andrew Nurse, for example, in a recent study of Barbeau's early work, identifies patterns similar to those I have presented here:

> The problems with Barbeau's ethnographic analysis of Huron-Wyandot culture were multiple: he failed to specify his key analytic concept, he failed to consider the complexity and multiplicity of culture, and he used different scales of cultural authenticity for different peoples. But perhaps its greatest weakness was that Barbeau erected a definition of authentic Huron-Wyandot culture in opposition to the ways in which the Huron and Wyandot peoples understood themselves and their cultures. ... Finally, Barbeau's treatment of Huron-Wyandot culture was marked by a

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[9] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to Jenness, 07 June 1927.

[10] For a recent critique of Boas's treatment of contemporary conditions in his ethnographic work, see Charles Briggs and Richard Bauman, "'The Foundation of All Future Researches': Franz Boas, George Hunt, Native American Texts, and the Construction of Modernity," *American Quarterly* 51:3 (September 1999), 516.

[11] Laurence Nowry, *Marius Barbeau: Man of Mana* (Toronto: NC Press, 1995); Richard C. Preston, "C. Marius Barbeau and the History of Canadian Anthropology," in *The History of Canadian Anthropology*, ed. by Jim Freedman. Proceedings of the Canadian Ethnology Society 3 (n.p.: Canadian Ethnology Society, 1976), 130; Marius Barbeau, "Les mémoires de Marius Barbeau," Canadian Museum of Civilization Archives, Canadian Centre for Folk Culture Studies records, box 624, file 1 (Textual Transcriptions, pp. 1-125), 14-17, 95-100.

fundamental ahistoricity.[12]

Given that both Jenness and Barbeau studied at Oxford University at the same time, and then spent their careers working in the same government office in Ottawa, one would expect close interaction. When Jenness was selected over Barbeau to replace Sapir as head of the Anthropology Division of the Geological Survey in 1925, the latter was enraged. He remained bitter throughout their years under the same roof. Consequently, there was very little intellectual exchange between the two.

Jenness's relationship with Sapir was more positive. Jenness remarked in a letter to Marett, for example, that "I like Dr. Sapir very much indeed. He is extremely capable, and seems to make a great success of the department [in the Museum]. Also he is extremely easy to get along with; he trusts you to do your work in your own way, though he is always ready to give you any assistance in his power."[13] It is difficult, however, to assess the extent to which Sapir influenced Jenness's work. Although Jenness's Arctic research was superficially similar to the cultural reconstruction advocated by Sapir, his larger objective to place the Inuit in an evolutionary scheme was far removed from Sapir's approach.

Some insight into Sapir's view of Jenness appears in a letter the former wrote to R. G. McConnell, Deputy Minister of Mines. The object of the letter was to secure a permanent position for Jenness at the Museum in order to keep him from enlisting in the CEF:

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[12] Nurse, "'But Now Things Have Changed,'" 452.
[13] OUAM, Jenness to Marett, 22 November 1916.

> Since his return from the North I have had many opportunities of talking over certain aspects of his work with Mr. Jenness, and of solidifying the general impression I had already obtained of his abilities as a scientist. I can now say quite unreservedly that I not only consider Mr. Jenness as admirably fitted for the position of permanent anthropologist on the Survey staff, but that he combines in quite exceptional degree various types of knowledge and experience, not often found united in a single field man.[14]

The statement probably reflects Sapir's push to gain another position on his research staff more than it does a true evaluation of Jenness.

Jenness's relationship with the wider anthropological community is harder to discern. If he corresponded with Malinowski or Radcliffe-Brown, no record of this communication exists in the Jenness correspondence file in the Archives of the Canadian Museum of Civilization. An examination of the Franz Boas Correspondence turns up a considerable correspondence with Jenness, but these letters included little beyond administrative trivia concerning various later projects Jenness undertook. Any mention of the Arctic Expedition work concerns physical anthropology,[15] cat's cradles,[16] and language,[17] rather than general ethnography. Curiously, Boas's correspondence with Sapir and Marett during the late 1910s and early 1920s includes no mention of Jenness.

Jenness made only incidental references to his relationships with other anthropologists. Upon his return from a major meeting in Boston, he wrote to Sapir that "I was particularly glad to be able to have long chats with Boas and Wissler, both of whom I liked very much. Now that I know them personally I have

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[14] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to McConnell, 14 September 1916.
[15] e.g., American Philosophical Society Archives, Franz Boas Correspondence [microfilm] [hereafter APSB], Jenness to Boas, 19 March 1920, 23 March 1920, 09 April 1920, 04 May 1920, 10 May 1920; Boas to Jenness, 05 April 1920, 25 October 1922.
[16] e.g., APSB, Jenness to Boas, 04 January 1922, 18 January 1922, 03 April 1923, 12 April 1923, 19 April 1923, 03 May 1923; Boas to Jenness, 16 April 1923, 17 April 1923.
[17] e.g., APSB, Jenness to Boas, 12 April 1927; Boas to Jenness, 13 April 1927.

ceased to dread their criticisms and shall rather enjoy it if they take me to task now and again."[18] However, five years later Jenness expressed some reservations about his dealings with North American anthropologists, writing to the Dane, Kaj Birket-Smith, that

> [o]ne of my greatest pleasures here is to be able to correspond so freely and frankly with you and other Danish ethnologists. There are one or two anthropologists with whom I feel always that I must be on my guard, but with my European friends I am able to discuss problems and ask for help as I do from my colleagues here in the Museum.[19]

It is just as difficult to assess the critical reaction to Jenness's Arctic work. Neither *American Anthropologist* nor *Man* published reviews of *Life of the Copper Eskimos*. The closest thing to a review of the Arctic work was a short note on Jenness's section on Inuit music (prepared with Helen Roberts).[20] This is surprising given that both journals had earlier published reviews of Jenness and Ballantyne's work on New Guinea. The review in *Man*, by F. R. Barton, was simply a summary the contents of *The Northern D'Entrecasteaux*,[21] while Robert Lowie, in *American Anthropologist*, noted that "Mr. Jenness has presented his results with obvious care and may be sure that his future publications, both in the same domain and the widely different Eskimo field, will be received with respectful attention by his colleagues."[22] Neither of these were enthusiastic endorsements.

In any event, during a period of significant developments of

[18] CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Jenness to Sapir, 03 January 1922.
[19] CMCJ, box 640, file 31 (Kaj Birket-Smith, 1922-1939), Jenness to Birket-Smith, 30 May 1927.
[20] Derrick Norman Lehmer, "Review of *Songs of the Copper Eskimos* by Helen Roberts and D. Jenness," *American Anthropologist* 29 (1927), 712-714.
[21] F. R. Barton, "Review of *The Northern D'Entrecasteaux* by D. Jenness and the late Rev. A. Ballantyne," *Man* 27:111 (December 1921), 187-189.
[22] Robert H. Lowie, "Review of *The Northern D'Entrecasteaux* by D. Jenness and the late Rev. A. Ballantyne," *American Anthropologist* 23 (1921), 226-227.

anthropological methods and theory, Jenness managed to set himself on an independent course. While his contemporaries in the United States and Britain were moving away from an evolutionary perspective, he remained fully ensconced in the earlier paradigm.

Conclusion: The Potential for a Canadian Anthropology

In examining the origins of Canadian anthropology, Darnell has argued the need to consider influences from both the American and British traditions.[23] She cautions, however, that "[t]he proper question for the history of Canadian anthropology, is not ... what are the sources of its parts. Rather, we should be asking, how are historically-diverse parts of our tradition of anthropology integrated within Canada."[24] She also maintains that "the uniqueness of Canadian anthropology can only be seen in contrast to larger trends"[25] of developments elsewhere.

Though Richling has argued that Jenness's work fits comfortably within the Boasian four-field paradigm,[26] this is not quite true. Jenness himself remained most comfortable within British tradition, as evidenced by his hiring preference for new researchers in the Museum.[27] For example, during one of his

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[23] Regna Darnell, "The Uniqueness of Canadian Anthropology: Issues and Problems," in *Proceedings of the Second Congress, Canadian Ethnology Society, Volume Two*, ed. by Jim Freedman and Jerome H. Barkow. Canadian Ethnology Service Paper 28 (Ottawa: National Museums of Canada, 1975), 413.
[24] ibid.
[25] ibid., 406.
[26] Richling, "An Anthropologist's Apprenticeship," 81. See also Sapir's assessment of Jenness's proficiency in each of the four fields (CMCS, folder "Jenness, Diamond 1913-1919" [microfilm], Sapir to McConnell, 14 September 1916).
[27] Barnett Richling, "Diamond Jenness and the National Museum of Canada: 1930-1947," *Curator* 33:4 (December 1990), 251.

searches for an anthropologist to employ at the Museum, he wrote that

> [t]here seems to be no one in Canada who has the necessary qualifications for a position on our staff, and while we could doubtless obtain someone in the United States, we prefer an English graduate who would be more likely to stay with us permanently than someone from the United States. Moreover, such a man would have the advantage of European training and outlook with which he would be surrounded.[28]

Four years earlier, Jenness conveyed this to T. F. McIlwraith, another British-trained Canadian anthropologist: "English methods differ from American in many ways and an Englishman coming over here to work would be likely to have a broader outlook than an American."[29]

Perhaps Jenness's search for British-trained anthropologists reflected a preference for researchers familiar with the contemporary applied research in the colonies,[30] rather than for the American anthropologists who were increasingly taking a culture-and-personality approach. The former approach would likely be seen by Jenness (as it was by Radcliffe-Brown, but for different reasons) as being better suited for contributing answers to questions about Aboriginal policies than the latter. In fact, both Collins and Taylor[31] and Henry Epp and Leslie Sponsel[32] argue that Jenness was a five-field anthropologist, combining work in the "traditional" four fields with research of an applied nature. I take an inclusive view of "applied anthropology." Rather than limiting it only to research with "a focus on issues with policy implications,"[33] I consider any work that is undertaken

[28] CMCJ, box 654, file 11 (Harold J. E. Peake, 1928-1940), Jenness to Peake, 23 September 1930.
[29] CMCJ, box 649, file 32 (T. F. McIlwraith, 1925-1927), Jenness to McIlwraith, 01 March 1926.
[30] Barnett Richling, "Applied Anthropology and Aboriginal Peoples in Canada, 1910-1939," *Australian-Canadian Studies* 13:1 (1995), 50.
[31] Collins and Taylor, "Diamond Jenness," 71.
[32] Epp and Sponsel, "Major Personalities and Developments," 10.
[33] Richling, "Applied Anthropology and Aboriginal Peoples," 50.

for purposes other than theoretical exploration to fall under this rubric. Because he was a federal government employee, his research obviously had more serious practical concerns than theoretical concerns.

This conception of Jenness's work shows that Richling's thesis, that Jenness was a salvage anthropologist who was committed to the Geological Survey's salvage objectives, does not hold true for the latter's early Arctic ethnography. Richling asserts that "there is reason to assume that most public officials did not regard anthropology pertinent to national priorities because they deemed the aboriginal peoples about whom anthropologists were concerned largely irrelevant to national priorities."[34] However, the mandate of the Canadian Arctic Expedition was largely concerned with exploration. Jenness himself addressed the potential benefits and problems of opening the resources of the north to economic exploitation.

Perhaps the applied nature of his research goes a long way towards explaining why Jenness emphasised description rather than theory in his ethnographic writing. It must be remembered that he was a civil servant, a bureaucrat whose expertise happened to be anthropology;[35] and the government he worked for required information suitable for administration of the Inuit. It did not seek theory-building on the nature of culture in general. As a civil servant with extensive administrative responsibilities, Jenness was not able to devote any significant amount of time to writing up his own work,[36] let alone keeping up-to-date with theoretical developments.

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[34] ibid., 52.
[35] ibid., 57.
[36] CMCJ, box 649, file 21 (T. F. McIlwraith, 1925-1927), Jenness to McIlwraith, 01 March 1926.

Both Richling and John Van West acknowledge that Jenness was less a theorist than an ethnographer.[37] Writing from an historicist perspective, Richling argues that

> it would be wrong to dismiss or undervalue Jenness' numerous contributions to the four sub-fields of anthropology because his experience did not culminate in a synthesizing work, or because no "school" of anthropological thought or practice, Canadian or otherwise, may be credited to him. Instead, we are obliged to consider his professional accomplishments within a socio-historical context defined by the specific conditions under which anthropology existed in Canada, and elsewhere, during the first half of this century.[38]

This thesis builds on this work. I take my lead from Darnell, who maintains that the most fruitful approach to such research is a presentist historicism, or a recognition that current conditions shape our views of earlier research. She has also argued that, "[b]y implication, our history of anthropology must trace the anthropologists through their fieldwork back to the theoretical preoccupations within which they are intertwined."[39] Similarly, Joan Vincent insists that "[w]hat led to the [ethnographic] texts and their making also had a politics. And this, of course, requires that we rethink their historiocity."[40]

My objective has been to show the extent to which Jenness, during the early part of his career, was isolated from developments in anthropological theory. I have also tried to show the extent to which his personal views and preferences may have influenced his observations of the Copper Inuit. Kulchyski accuses him of promoting "outright assimilation" on the grounds that Jenness

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[37] Richling, "An Anthropologist's Apprenticeship," 72; John Van West, "The History of Anthropology in Canada," [unpublished manuscript], 20.
[38] Richling, "An Anthropologist's Apprenticeship," 72.
[39] Regna Darnell, "The Pivotal Role of the Northwest Coast in the History of Americanist Anthropology," *BC Studies* 125-126 (Spring / Summer 2000), 35.
[40] Joan Vincent, "Engaging Historicism," in *Recapturing Anthropology: Working in the Present*, ed. by Richard G. Fox (Santa Fe: School of American Research Press,1991), 46.

"proposed measures even more forceful than those the state had already adopted and was prepared to countenance an approach that was in many respects even more paternalistic than the bureaucrats were advocating."[41] He concludes that "[t]he political bias in Jenness's work was markedly against the people whose cultures he was attempting to represent."[42] Now that we have a better insight into his later politics, it is clear that all of his work, including his early Arctic ethnography, is in need of careful historical scrutiny. It is problematic at the current time to take Jenness's work at face value.

At the centre of Darnell's examination of the uniqueness of Canadian anthropology is an assertion that a simple listing of names, dates, and facts, focussed on a limited number of major figures, is not an appropriate approach. Instead, she argues, it is necessary to understand the contributions of particular individuals to the development of a national tradition.[43] This thesis contributes to Darnell's objectives. Contrary to Epp and Sponsel, who argue that Jenness "[i]n many respects ... is the Canadian equivalent of Boas, except that he was never

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[41] Kulchyski, "Anthropology in the Service of the State," 27-28.

[42] ibid., 46. Jenness's wider political views are difficult to ascertain; he certainly felt constrained by his status as a civil servant in his ability to critique government policy (CMCJ, box 640, file 12 [Julia Averkieva, 1932-1935], Jenness to Averkieva, 05 February 1932). Some items in his correspondence files certainly seem to reflect anti-Semitic views on his part. For example, during his term as President of the American Anthropological Association in 1939, he commented in a letter to the Association's secretary that "I was glad to see that Sapir's resolution was amended to read 'in many countries', without naming any specific ones. I am afraid that the 'Semites' often harm their own cause and create ill-will, through a lack of moderation and tact. However, all is well that ends well" (CMCJ, box 639, file 12 [American Anthropological Association, 1938-1939], Jenness, to Setzler, 05 January 1939). Another, more curious example is a letter of introduction Jenness wrote near the end of his career to Sir Frederick Banting:

> May I introduce to you by this letter Dr. Jankowsky, of Breslau, who, besides being a well known anatomist and physical anthropologist, is deeply interested in genetics, and is officially connected with the sterilization program of the German Government? I am sure that you will find him extremely interesting, and that he on his side will be deeply interested in the work that is being done at the Connaught Laboratories. (CMCJ, Jenness to Banting, 12 July 1937)

Obviously, Jenness's politics are complicated and difficult to pin down conclusively.

[43] Darnell, "The Uniqueness of Canadian Anthropology," 403.

an academic anthropologist,"[44] I maintain that, having never trained students in this country, Jenness cannot be linked to Boas in this way. Jenness, unlike Boas, was certainly not a founding father of a distinctive national approach to anthropology. And, again unlike Boas, Jenness's ethnography did not usher in a new paradigm for the discipline. On the contrary, it reflected an approach that was outdated even at the outset of his early work in the Arctic.

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[44] Epp and Sponsel, "Major Personalities and Developments," 10.

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VITA

Surname: Hancock *Given Names*: Robert Lorne Alexander

Place of Birth: Victoria, British Columbia, Canada

Educational Institutions Attended:

University of Victoria	1999 to 2002
McGill University	1995 to 1997; 1998 to 1999
University of British Columbia	1997 to 1998

Degrees Awarded:

B.A. (Honours)	McGill University	1999

Honours and Awards:

Department of History Fellowship	2000 to 2002
Allan and Elizabeth McKinnon Scholarship	2000
Victoria Historical Society Scholarship	2000

Publication:

"The Hop Yards: Workplace and Social Space," in *A* Stó:lô-*Coast Salish Historical Atlas*, ed. by Keith Thor Carlson. Vancouver: Douglas and McIntyre, 2001, 70-71.

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Author

Robert Lorne Alexander Hancock
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