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Gateways

Exploring the Legacy of the Jesup
North Pacific Expedition, 1897-1902



Igor Krupnik and
William W. Fitzhugh, editors



GATEWAYS

Exploring the Legacy of the Jesup North Pacific Expedition

1897-1902



1/ Jochelson's caravan of reindeer sleds crossing the Verkhoyansk Mountain Range, Siberia, winter 1902
(AMNH 1749)

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Exploring the Legacy of the Jesup North Pacific Expedition, 1897-1902

IGOR KRUPNIK AND
WILLIAM W. FITZHUGH, EDITORS



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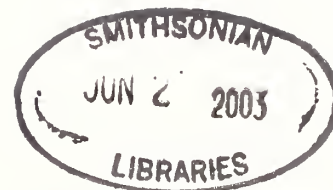
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Front Cover: Siberian Yupik (Eskimo) girls dancing in the village of Ungazik (Indian Point), Chukotka, Siberia, Spring 1901. Waldemar Bogoras, photographer (AMNH 1344)

Back Cover: Skidegate, a Haida village, Queen Charlotte Islands, British Columbia, 1900. John Swanton, photographer (AMNH 330387)



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contributors

Judith Berman is a research associate in the American Section, University of Pennsylvania Museum of Archaeology and Anthropology, Philadelphia. Her various publications, as well as her Ph.D. thesis "The Seals' Sleeping Cave: The Interpretation of Boas' Kwak'waka'wakw Texts", examine the legacy of Kwakwaka'wakw research and publications by Franz Boas and George Hunt, the significance of the Boas-Hunt cooperation, and aspects of the Kwak'waka'wakw language and folklore.

Douglas Cole (1938-97) was professor of history at Simon Fraser University, Vancouver, B.C. He was known for his numerous writings on the contact history of the Native peoples of the Northwest Coast, on scientific exploration and museum collecting in British Columbia, and on art and literature in Canadian colonial society. His most influential contributions to North Pacific anthropology include three major volumes: *Captured Heritage: The Scramble for Northwest Coast Artifacts* (1985); *An Iron Hand upon the People: The Law against the Potlatch on the Northwest Coast* (1990, co-authored with Ira Chaikin); and *Franz Boas: The Early Years, 1858-1906* (published posthumously in 1999).

William W. Fitzhugh is director of the Arctic Studies Center and curator at the Department of Anthropology, National Museum of Natural History, Smithsonian Institution. He has spent almost 30 years studying circumpolar archaeology and publishing on Arctic peoples and cultures in Canada, Alaska, Siberia, and Scandinavia. Special interests include prehistory and environmental archaeology, circumpolar maritime adaptations, and culture contacts. At the Smithsonian,

he has produced several exhibits that resulted in major catalog volumes, such as *Inua: The Spirit World of the Bering Sea Eskimo*; *Crossroads of Continents: Cultures of Siberia and Alaska*; *Ainu: Spirit of a Northern People*; and *Vikings. The North Atlantic Saga*.

Michael Harkin is associate professor of anthropology and American Indian studies at the University of Wyoming. He received his M.A. and Ph.D. in anthropology from the University of Chicago and has taught at Emory University and Montana State University. He is the author of *The Heiltsuks: Dialogues of Culture and History on the Northwest Coast* (1997).

Richard Jantz is a professor in the Department of Anthropology and director of the Forensic Anthropology Center at the University of Tennessee, Knoxville. He received his Ph.D. from the University of Kansas. His research interests are ancient and modern human quantitative variation in anthropometrics, dermatoglyphics, and skeletal morphometrics.

Sergei Kan is professor of anthropology and Native American studies at Dartmouth College, Hanover, N.H. Most of his publications, including his recent book *Memory Eternal* (1999), deal with the Tlingit Indians' culture and the history of Russian Orthodox Christianity among Native people in southeastern Alaska. Recently he has been working on a new book on Russian anthropologist Lev Shternberg that will cover Shternberg's life, his public and scholarly career, and his relationships with Franz Boas, Waldemar Bogoras, Waldemar Jochelson, and other members of the Jesup Expedition project.

Richard Keeling, formerly with the University of California, Los Angeles, worked for several years on documenting and analyzing early recordings of traditional Native American music, primarily of native groups of northern California. He has published several papers on Yurok, Hupa, and Karok music as well as an extensive annotated catalog of music and voice recordings collected between 1900 and 1938, *A Guide to Early Field Recordings (1900-1949) at the Lowie Museum of Anthropology* (1991). He now lives in the Bay Area.

Igor Krupnik is an ethnologist at the Arctic Studies Center, Smithsonian Institution, Washington, D.C. Born and educated in Russia, he has done extensive fieldwork among the Siberian Yupik people in the Bering Strait area, in the Russian Far East, and recently in Alaska. He is currently coordinator of various international projects studying the impacts of global climate change and the preservation of the cultural heritage and ecological knowledge of Native peoples. He has published and co-authored several books and catalogs, and he writes extensively on Arctic Native peoples, Native heritage resources, modernization, and minority issues.

Barbara Mathé is senior Special Collections librarian at the American Museum of Natural History in New York. In 1997 she co-curated (with Thomas Miller) the Jesup Centenary Exhibition at the AMNH, *Drawing Shadows to Stone: Photographing North Pacific Peoples, 1897-1902*. She is currently working on a book of photographs of the anthropological exhibitions at the Louisiana Purchase Exposition in 1904.

Thomas R. Miller is a doctoral candidate in anthropology at Columbia University. His Ph.D. dissertation, "Songs from the House of the Dead," explores the role of the early phonograph in the history of museums and anthropology through a study of shamans' songs recorded by Franz Boas, James Teit, Waldemar Jochelson, Waldemar Bogoras, and others, during the Jesup Expedition and beyond. He worked for several years with the Asian ethnographic collections at the AMNH in New York, including the original collections of the Jesup Expedition. In 1997, he was a guest cura-

tor (with Barbara Mathé) of the Jesup Centennial Exhibit *Drawing Shadows to Stone* at the AMNH.

Stephen Ousley is the director of the Repatriation Osteology Laboratory in the Department of Anthropology, National Museum of Natural History, Smithsonian Institution, Washington, D.C. He received his Ph.D. from the University of Tennessee, Knoxville. His research interests include the history of anthropology, morphometrics, quantitative genetics, and forensic anthropology.

Brian Thom is a Ph.D. candidate in anthropology at McGill University, Montreal, Canada. His M.A. thesis at the University of British Columbia (1995) was focused on the history of archaeological excavation of burial mounds and cairns along the Northwest Coast. He worked for several years with Coast Salish communities in British Columbia on the problem of aboriginal titles to land and cultural resources.

Nikolai Vakhtin is a professor at the European University, St. Petersburg, Russia. He teaches courses in field linguistics, sociolinguistics, and the cultural anthropology of Siberia. He received his Ph.D. (1977) and full doctorate (1993) in Siberian Yupik linguistics from the Institute of Linguistic Studies of the Russian Academy of Sciences, St. Petersburg. He has done extensive research in Native languages and modern culture change among the minority peoples of northern Russia, including the Yupik, Aleut, Chukchi, and Yukagir. He has written several books and over 80 articles on the languages and cultures of the northern indigenous peoples of Siberia.

Paula Willey is currently a project manager for Gallery Systems, a provider of collections management software for museums, galleries, and private collections. Previously, she worked at the AMNH Library as Special Collections manager.

Saskia Wrausmann is a undergraduate student at the College of William and Mary, Williamsburg, Va. Her primary interests are in cultural and biological anthropology. In 2000 and 2001 interned at the Arctic Studies Center. She is fluent in German and French.

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Note: All photographs (unless marked by an asterik above) are courtesy Department of Library Services, American Museum of Natural History. Maps were produced by Marcia Bakry, National Museum of Natural History. Other figures were provided by the respective authors and are separately credited.

abbreviations

AAA	American Anthropological Association	IASSA	International Arctic Social Sciences Association
AAAS	American Association for the Advancement of Science	InV-JC	Institut Vostokovedeniia (Institute of Oriental Studies), St. Petersburg, Waldemar Jochelson Collection
AAN	Arkhiv Akademii Nauk (Archives of the Russian Academy of Sciences)	JNPE	Jesup North Pacific Expedition
AFC	American Folklife Center, Library of Congress, Washington, DC	MAE	Peter the Great Museum of Anthropology and Ethnography, St. Petersburg
AMNH	American Museum of Natural History, New York	MJC	Melville Jacobs Collection, University of Washington, Seattle
AMNH-DA	American Museum of Natural History, Department of Anthropology	NAA-BAE	National Anthropological Archives, Smithsonian Institution, Bureau of American Ethnology Collection
AMNH-L	American Museum of Natural History, Library, Special Collections Division	NMNH	National Museum of Natural History, Smithsonian Institution, Washington, DC
APS	American Philosophical Society, Philadelphia	NYPL	New York Public Library, New York
APS-C	American Philosophical Society, Franz Boas Professional Correspondence	RAS-B	Russian Academy of Sciences, Archives, St. Petersburg Branch, Waldemar Bogoras Collection
ATM	Archives for Traditional Music, Indiana University, Bloomington	RAS-F	Russian Academy of Sciences, Archives, St. Petersburg Branch, Fonoteka (Phonographic Collection)
BAAS	British Association for the Advancement of Science	RAS-J	Russian Academy of Sciences, Archives, St. Petersburg Branch, Waldemar Jochelson Collection
BAE	Bureau of American Ethnology, Washington, D.C.	RAS-S	Russian Academy of Sciences, Archives, St. Petersburg Branch, Lev Shternberg Collection
CUL	Columbia University Library, Rare Books and Manuscripts, New York	SI	Smithsonian Institution, Washington, DC
HBC	Hudson's Bay Company		
HBCA	Hudson's Bay Company Archives, Winnipeg, Manitoba		
HUA	Harvard University Archives, Boston		



2/ The Jochelsons' team with the expedition boat at the Kolyma River, Siberia, 1901 (AMNH 1679)

foreword

This book, *Gateways: Exploring the Legacy of the Jesup North Pacific Expedition, 1897–1902*, honors anthropology's most prominent founding father, Franz Boas. It follows the historical trails of Boas' first (and last) attempt to produce a comprehensive and synthetic panorama of native cultures of the North Pacific Rim. As part of a decade-long retrospective of Boas' signature contribution to the science of anthropology and to the construction of a regional culture history, this book has been an academic exploration through space and time.

Our involvement with the Jesup Expedition began in the 1980s when our team at the Smithsonian's National Museum of Natural History was working on the exhibit *Crossroads of Continents: Cultures of Siberia and Alaska* and its accompanying catalog (published 1988). Inevitably, the *Crossroads* project looked both to the past and the future, since it explored the history and prospects of both the peoples of the North Pacific and the discipline of anthropology. The central issue for Boas in the 1890s, as for our team in the 1980s, was whether contemporary anthropology could answer the fundamental questions about the origins and history of Native Americans and their relationships to Siberian peoples and cultures. To Boas, the traditional disciplines of history and anthropology of his time seemed inadequate for the task, as there was neither written history for the North Pacific prior to the 1740s nor a competent ethnography, archaeology, folklore, or linguistics for most of the Native nations in the area.

In the early 1990s, as the centennial of the Jesup North Pacific Expedition (1897–1902) was approaching, we anticipated the opportunity to reopen the questions posed at the start of this early anthropological rite of passage in the North Pacific. New perspectives based on a full century of advances in anthropological methods and theory could be combined with hopes for a new political geography. This would allow trans-Beringian research and cultural exchange to commence after decades of denial. We hoped that new scholarship and rapprochement might lead to a reevaluation and renewal of Boas' goals for the Jesup Expedition. With some temerity, we decided to give an appropriate name to the undertaking and called it "Jesup 2." We organized a session on the subject in 1992 at the First Congress of the International Arctic Social Sciences Association (IASSA) in Quebec, which was itself a creature of the new détente between East and West. And we proposed there a neo-Boasian effort to take up the task of North Pacific anthropology and culture history more or less where Boas and his Russian, German, American, and Canadian colleagues had left it when their careers and lives expired in the 1930s–1940s.

This volume represents one of the several tributaries of the *Jesup 2* stream that we imagined might flow from the resurgence of North Pacific cultural studies. Originally the plan was crafted for a panel discussion organized at the 1993 American Anthropological Association meetings held in Washington, D.C. At that session we intended to explore new perspectives on

the original Jesup Expedition through the study of its record of unpublished manuscript materials, photographs, personal papers, notes, and ledgers, from collections in both Russia and North America. Many of these documents were not available to the original expedition team (or its successors), and they add measurably to our understanding of their efforts, as well as to what did not get accomplished. We also felt that a thorough reevaluation of the Jesup Expedition legacy would serve as added mortar to the scholarly structure we hoped would be soon forthcoming.

As it has turned out, the opportunities for a coordinated *Jesup 2* program produced some pleasant surprises. Through much hard work we were able to engage a new group of curators and institutions in Alaska and the Russian Far East to produce a smaller version of the *Crossroads* exhibit. It traveled to rural Alaska and the Russian Amur-Sakhalin region, spreading its message of cultural exchange and cooperation to the peoples responsible for these cultures in the first place. Another surprising development was the opportunity to produce a major exhibition on the Ainu people, one of the cultures targeted by Boas for the Jesup Expedition. As it happened, very little ethnographic work on the Ainu was accomplished during the expedition, and their exclusion from its collections and publications resulted in an ambiguous status for this culture as a North Pacific people for the remainder of the 20th century. Fortunately we found a way to correct this deficiency in 1999 through a special exhibition, *Ainu: Spirit*

of a Northern People, and a book featuring this culture and its history and art.

In addition to such opportune windfalls, we also found our *Jesup 2* voyage marked by unanticipated shoals and navigational hazards. Wiser heads from the 1992 IASSA meeting were right to caution us about planning such an optimistic program in the absence of a Smithsonian “Morris Jesup,” or some suitable institutional or philanthropic replacement to sponsor new research and publications, and we have had to refocus and adapt. I would like to thank all those people and institutions who have contributed by participating as symposia and panel members, correspondents, contributors, and supportive bystanders in our various efforts of the past decade to forward the Boasian goal of a more integrated and inclusive North Pacific anthropology. Although it has not been possible so far to launch a multi-institutionally, orchestrated centennial *Jesup 2* program, many elements of this concept are nevertheless moving forward in the broader international anthropological community. We can, belatedly and with only slightly chastened optimism, report progress on many fronts, as reported in the following Introduction. Not the least of these advances is the current volume, which suffered several untimely publication setbacks before reaching this happy conclusion. Though Boas’ team made little use of written history in its “Jesup 1” project, it is proving an invaluable component of the “Jesup 2” effort some one hundred years later.

*William W. Fitzhugh, Director
Arctic Studies Center*

note on Cyrillic transliteration

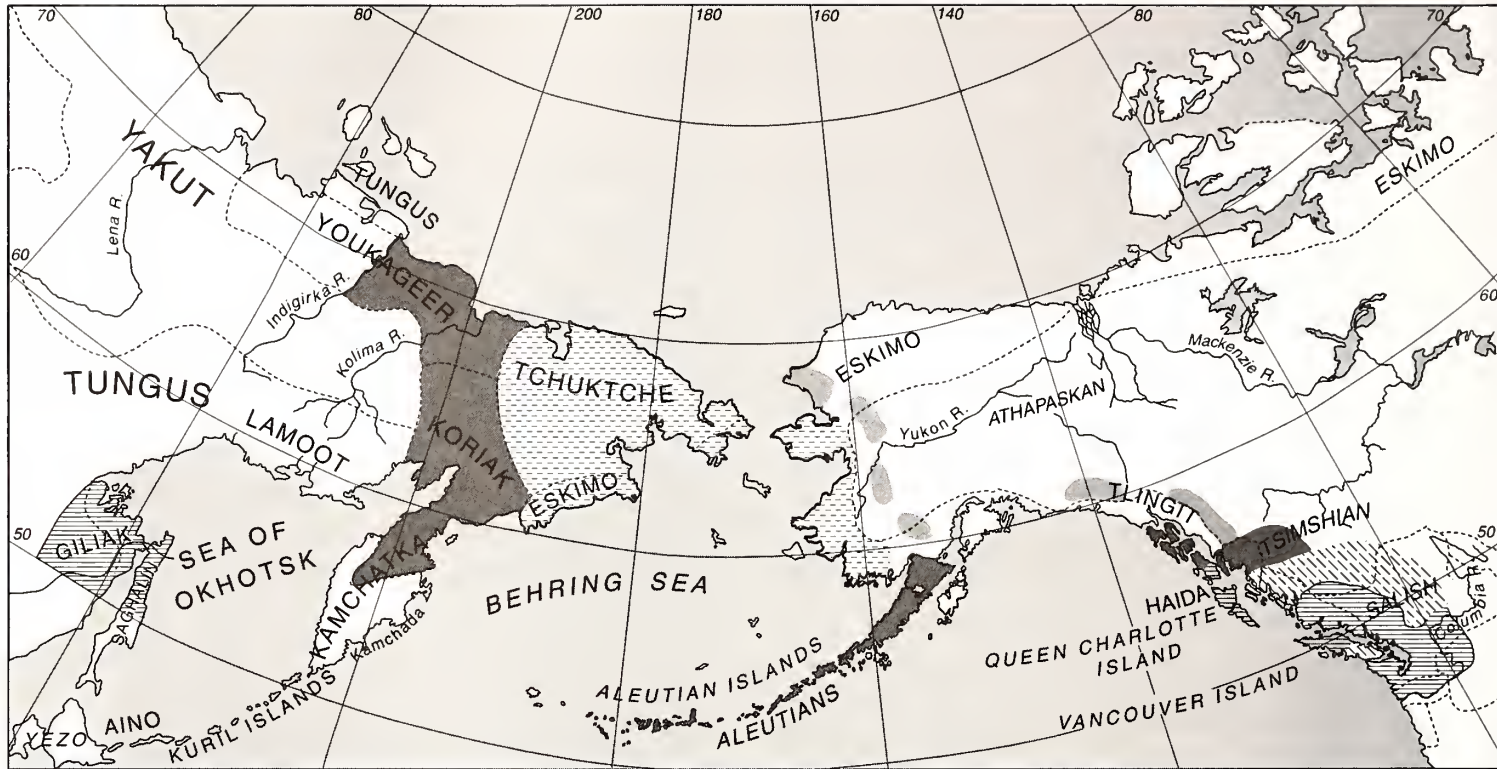
Two coexisting systems are in use in the United States for transliterating Russian Cyrillic letters into English: that of the Library of Congress (LC), and that of the National Image and Mapping Agency (NIMA, formerly the U.S. Board of Geographic Names). The LC system is used for bibliographic references; the NIMA system applies to geographic names (place names) and to most ethnic names.

All Russian or Siberian geographic names are transliterated here according to the NIMA system, which uses *ya*, *yu*, and *yo* for Cyrillic *я*, *ю*, and *ё* (Yakutsk, Yuzhno-Sakhalinsk, etc.). Throughout this volume, Native Siberian ethnic names are transliterated in accordance with the *Peoples of the Soviet Union* map produced by the National Geographic Society in 1989, which basically adheres to the NIMA system (Yakut, Yukagir, Koryak, Nanay, etc.). Most of these ethnic names are already established in Western anthropological literature—thanks largely to the Jesup Expedition’s pioneering publications. This system also results in names reminiscent of several Native American group titles familiar to North American readers: Yurok, Maya, Yup’ik, Eyak, Yokut, Yakutat, Tlingit, and so on. Furthermore, the NIMA-based spelling of ethnic and geographic names is similar to the Russian/Cyrillic transliteration system adopted in England and Canada and to the one commonly used by modern Russian authors when writing papers in English. The NIMA-based system is also applied here for transliterating a few Russian or

Native Siberian personal names, words, and ethnographic terms in individual papers.

In contrast to the NIMA system, the Library of Congress transliteration system uses *ia*, *iu*, and *io* for the Cyrillic *я*, *ю* and *ё* and an apostrophe for the Russian soft sign (*ь*). Because today’s highly standardized electronic library catalog formats are based on the LC system, names of Russian authors and all titles of items in the bibliographic reference sections in this volume adhere to the LC system. Using two transliteration systems in a single book may be inconvenient, but every effort has been made to adhere strictly to each of these patterns in its designated application in order to establish a high level of consistency for all future Arctic Studies Center publications. For the convenience of readers, an alternative NIMA-based transliteration of Russian authors’ names is sometimes provided in parentheses in those cases where such a pattern has been established by earlier publications (for example, the original Jesup Expedition series, *Anthropology of the North: Translations from Russian Sources*). Despite all our efforts, we may not have been able to eliminate all potential cases of confusion or the occasional idiosyncratic usage.

We are grateful to our colleagues Pavel Ilyin (U.S. Holocaust Museum), Michael Krauss (Alaska Native Language Center, University of Alaska), and Marjorie Mandelstam Balzer (editor, *Anthropology and Archeology of Eurasia*) for their advice on transliteration practices for ASC publications.



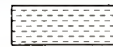
1897



1898



1899



1900



1901

3/ Field of Proposed Operations of the Jesup North Pacific Expedition, 1898 (adapted from American Museum of Natural History. *Annual Report of the President for the Year 1897*)

introduction

WILLIAM W. FITZHUGH
AND IGOR KRUPNIK

Ever since the European discovery of America, the question of the origins and history of Native Americans has been a subject of ardent public interest and scholarly debate. Theories of Asian origins, first advanced by José de Acosta in 1598, remained eclipsed for centuries by Eurocentric theories of Phoenician, Egyptian, or Celtic migrations across the Atlantic. But with the emergence of academic anthropology in the late 19th century, the idea of an Asian/Siberian route to the Americas prevailed and was elaborated into major research initiatives. Of these, the most crucial was the Jesup North Pacific Expedition (1897–1902), the first, and as yet the most coordinated, single study ever undertaken of the peoples and cultures of the North Pacific region.

Throughout most of the 20th century, politics has been the most difficult stumbling block for trans-North Pacific scholarship. Although Asia and North America are clearly visible from each other's shores at the Bering Strait, during most of the 20th century this narrow 56-mile waterway was both a symbolic ideological barrier and a bristling frontier of military and political confrontation. The struggle not only separated Native families from their relatives across the Bering Strait; it also had a crushing effect on scholarly cooperation. Previous experience demonstrates that meaningful research in the North Pacific requires active international collaboration between American, Canadian, Russian, European, and Japanese scientists. Such research expands dramatically with open communication, including data

exchanges and comparative study, and it progresses best within a framework of multidisciplinary perspectives and close linkages between the social and natural sciences.

Anthropological research conducted by participants in the Jesup Expedition between 1897 and 1902 began with these principles in mind. After many decades of embargoed communications and stifled scholarship, we now may reexplore the opportunities that were originally pioneered by the Jesup Expedition team.

This volume is an outgrowth of such an attempt to pursue the study of peoples and cultures across the North Pacific area a full century after the Jesup Expedition crews were sent to the Northwest Coast of North America and the shores of Siberia. This new initiative is called *Jesup 2* in honor of its predecessor and because it follows in the steps of the original Jesup Expedition surveys and publications. With borders reopening and exchange resumed, the time may be opportune for new research and partnerships. If history and current trends are a guide, the 21st century will bring renewed life and importance to the Alaskan-Siberian crossroads, a region that has been a breeding ground for cultural development and intercontinental human links for thousands of years.

Shared Lands, Common History

The Greater North Pacific Region has special importance in the study of Native American and Siberian cultures. As far as is known, the Bering Strait was

the major (if not the only) proven entryway for movements of human populations from the Old into the New World before A.D. 1500, and it has been host to many subsequent Asian-American interactions. For this reason, the vast region around the Bering Strait is usually called "Beringia," and it has a very special importance for the culture history of the Americas.

During the Ice Age, lowered sea levels produced a broad land bridge that enabled intercontinental dispersal of animals and plants, either through the harsh continental interior or following a milder Pacific coastal route. Even after the disappearance of the last land bridge about 11,000 years ago, prehistoric communication across the Bering Strait continued by boat or over winter ice. Unlike the North Atlantic region, where thousands of miles of ocean and uninhabited lands separate Europe from North America, in the North Pacific region Beringia acted both as a channel and as a "quality control" point for contacts and exchange. Other possible routes exist along the Aleutian Chain, across the open waters of the Bering and Chukchi Seas, or, for the more hapless, across the expanses of the North Pacific, pushed by westerly currents and winds. Thus, people as well as artifacts large and small found their way from Asia to America (and back) on a sporadic basis. Regular contacts and exchanges between hunters from neighboring tribes situated around the entire coastal margin of the North Pacific Rim would have been even more influential over the long run.

Historically, the North Pacific region was one of the last large areas of the world to be contacted by Europeans, and it is still one of the world's best-preserved cultural regions. As the Russian, British, Spanish, and American explorers witnessed in the 1700s, its productive lands and waters supported indigenous peoples and cultures with highly developed technologies, social structures, and art. On their first encounters, many European observers reported that Native groups from the Kamchatka Peninsula in Asia to the Northwest Coast of North America exhibited certain similarities in culture, language, and physical type.

Suggestions of common origins or shared ancestry were made on the basis of these early observations and anecdotal evidence. Similar observations were made about the region's natural history, since both sides of the North Pacific have a common set of marine mammal, avian, and fish species and share many comparable environments and climate regimes.

The Jesup North Pacific Expedition, 1897–1902

Despite similarities noted by explorers, early ethnological studies in Alaska and Northeast Siberia throughout the 1700s and most of the 1800s were oriented toward description of regional and even individual ethnic cultures. In that halcyon era of "natural history" scholarship, detailed observation and systematic recording, rather than theorizing, carried the day. For this reason, the launching of the Jesup North Pacific Expedition in 1897 marks a milestone in the history of North Pacific studies. Its objectives, field program, and subsequent publication activities were all designed by Franz Boas, then assistant curator in the Anthropology Division at the American Museum of Natural History (AMNH) in New York. Boas and others realized that science would never solve larger questions by studying cultures and regions in isolation.

Funded privately by Morris K. Jesup, president of the AMNH, the Jesup Expedition had as its purpose the investigation of the history of Native cultures and their relationships throughout the North Pacific region. Among the questions posed were some of the oldest and most exciting in the history of American anthropology: the origins and migration routes of the American Indians and Eskimos; cultural relations between Asia and the Americas; and the histories, ethnology, and material culture of the complex tribes of the Greater North Pacific Region.

The objectives of this broad regional synthesis called for field studies on a scale never before attempted in anthropology. In drafting the Jesup Expedition program, Boas skillfully integrated a number of

scholarly resources of his time. The project was built on his previous work among Northwest Coast tribes in the late 1880s and early 1890s (see Cole, this volume); on the successful record of the earlier Smithsonian naturalists' work in Alaska (Fitzhugh 1988), and on whatever bits of information about the Native peoples of Siberia were then available to western anthropologists (see Vakhtin, this volume). As a newly appointed museum curator, Boas also saw the Jesup Expedition as a vehicle for building museum collections for scientific and exhibition purposes.

Ideally the Jesup Expedition was to be conducted by teams of anthropologists (or other trained professionals) who specialized in ethnology, archaeology, folklore, linguistics, and physical anthropology. Careful collections were to be made, and the geographic distribution of cultural elements—ethnographic and archaeological objects, language, physical traits—was to be thoroughly documented, following newly formulated principles of diffusion and cross-cultural studies. By utilizing this plan, Boas expected to produce a broad regional synthesis that would be a model for his method of detailed comparison and multidisciplinary field research.

As might be expected of the founder of American anthropology, Boas was decades ahead of his time. He instructed the members of the team he assembled to gather masses of ethnological data, including facial casts, body measurements, photographs, folklore texts, wax recordings, archaeological artifacts, and linguistic records. He dispatched his field crews to the Northwest Coast, Alaska, and Siberia with the imprimatur of the AMNH and with funds provided by Morris Jesup, together with his own detailed instructions on data collecting. Fieldwork lasted from several summer months (for Boas, Dixon, and Farrand, in North America) up to two full years (for the Jochelsons in Siberia). The researchers then returned and prepared monographs under Boas' direct supervision.

The AMNH's coffers soon filled with thousands of ethnographic specimens, and its archives burgeoned

with documents, field notes, and photographs. Eventually, 11 Jesup Expedition volumes, comprising 31 separate reports on detailed ethnographic descriptions, folklore, and physical anthropology, were published, as were several dozen external articles and other monographs. All this made the Jesup Expedition one of the most extensively published anthropological projects ever (see Krupnik, this volume).

As project leader, Boas had the task of completing the final monograph and synthesizing its field results. But despite heroic efforts, his team had barely succeeded in scratching the surface, and even Boas became daunted by the immensity of the task and by the dragging performance of many of his associates (see Ousley and Jantz, this volume; Kan, this volume). To the dismay of his sponsor, Morris Jesup, he never completed what was to have been the final monograph in the JNPE series. Boas and his partners did present some of their conclusions in numerous summary papers (Boas 1897, 1903, 1905, 1910a, 1910b, 1912, 1925, 1933; Bogoras 1927, 1929; Jochelson 1926), but to many later critics, this was too little and too late (Krupnik 1998).

In retrospect, the expedition's greatest accomplishment was to gather invaluable collections and publish masses of ethnographic data that documented cultural practices of the North Pacific peoples at a transitional time in their history. Working relationships were also forged between North American and Russian scientists and institutions that benefited subsequent generations of scholars. The principle became established that cultural relations between Asia and North America had deep roots and could not be understood by researchers working in isolation. The Bering Strait actually never was a significant geographic or cultural barrier to prehistoric communication and exchange, and neither should it be for scholars who wish to understand its regional history. The tangled political realities of the 20th century, however, imposed harsh limitations on the spirit of partnership and cooperation exemplified by the Jesup Expedition.

Post-Jesup Research

From our perspective, the Jesup Expedition was a huge success. The voluminous series, dozens of other publications in English, German, and Russian, presentations at international meetings, and large collections of museum artifacts, photographs, and other resources that it fostered attracted interest and stimulated new research (Krupnik and Vakhtin 1997a). While Boas went on to assume a professorship at Columbia University, forsaking his curatorial duties (and, eventually, his promised Jesup Expedition summary volume), he continued to publish the expedition's field materials. Some of his Jesup associates—the Jochelsons, Swanton, Dixon, and Smith—expanded their research in the North Pacific to areas not covered (although originally envisioned) by the Jesup Expedition (Fig. 3). A few, particularly Jochelson and Bogoras, developed new support for their earlier theories. But no new joint projects of a similar magnitude were to follow, and as Soviet power and Stalinist policies took hold in the Russian Far East, communication, travel, and collaborative research across the Bering Strait gradually ceased. By the late 1930s and the early 1940s, Russian (Soviet) and western studies of the North Pacific cultures, restricted by national borders and ideological constraints, diverged and went their separate ways (Krupnik 1998).

As integrated cross-cultural research across the Bering Strait came to a virtual standstill, the plight of international scholarship produced an eloquent plea for cooperative studies by the famous Danish Arctic explorer Knud Rasmussen. He himself had once been expelled from Siberia while on a field trip because he lacked proper visa papers. Calling for a multinational research program in northeastern Siberia and Alaska at the Fifth Pacific Science Congress in 1933, only a few months before his death, Rasmussen predicted, "I am quite aware that a task like this cannot be brought to realization in the twinkling of an eye. . . . It is, however, my firm conviction that one day there will be a great co-operative undertaking of this kind, and that this plan will be carried out" (Rasmussen 1934:2772).

Sadly, his proposal, like many others, died as a result of the harsh political regimes to come.

Although both Russian and American scholars continued ethnological surveys in their respective regions, they had begun to recognize the critical need for archaeological evidence for their general scenarios of prehistoric connections and culture change. Soon, archaeologists took the lead, thanks to the advances in archaeological techniques, the numbers of sites excavated, and the sheer amount of prehistoric artifacts recovered across the Arctic. Boas had included archaeology in the original Jesup Expedition program, but practical problems, including the relatively early state of development of archaeological techniques and theory, limited its contribution (see Thom, this volume). Fortunately, the Jesup Expedition had stimulated an awareness of the importance of archaeological investigation in the Arctic. It was by this means, and through the later work of Jochelson in the Aleutians, Collins on St. Lawrence Island, Hrdlička at Kodiak Island, Jenness at Cape Prince of Wales, and Larsen and Rainey at Point Hope, that a more detailed story of North Pacific prehistory began to unfold during the 1920s and 1930s.

With the onset of the Cold War mentality in the late 1940s, all research cooperation, as well as human contacts, across the Bering Strait ceased. The minimal and declining competency in the Russian language on the part of American scholars, and Soviet censorship, ensured that little information entered academe across the Soviet-American frontier. As a result, Russian-American scholarly communications had all but evaporated by 1950 (Krupnik 1998). Nevertheless, important surveys dealing with trans-Beringian archaeology and physical anthropology by Russian scholars (e.g., Debets 1951; Levin 1958 [1963]; Rudenko 1947 [1961]) and Western scholars (e.g., Collins 1937; De Laguna 1947; Larsen and Rainey 1948; Laughlin 1952) continued. These studies clearly documented the divergence of Russian and Euro-American scholarship in that they involved minimal direct exchanges and recorded few compatible results.

During the 1950s, new theories on the origins of the North Pacific peoples and cultures were advanced that redrew or even rejected the old scenarios of the Jesup Expedition (see, for example, Chard 1960; Drucker 1955; Levin 1958 [1963]). None, however, was based on coordinated field research or on a compatible set of field data collected on two continents, which had been the inspiration for Boas and his partners.

As a result of post-Jesup research, the “Paleoasiatic” peoples of northeastern Siberia (called “Americanoids” in some of the Jesup Expedition-based publications) are no longer believed to have originated in North America or to constitute a coherent entity of their own. Nor are the Eskimo [Yup’ik and Iñupiat] people in Alaska and Siberia considered to be a relatively recent Canadian “wedge” that split the initial continuum of coastal North Pacific groups from Kamchatka to Oregon. Cultural similarities between the Native peoples across northeastern Siberia, the Northwest Coast of North America, and southern Alaska exist, but their origin—by migration, cultural transfer or diffusion, or convergent development—is not known.

As these examples show, the complexities of North Pacific cultural history are now recognized as immense, especially since Alaska has been occupied for at least 12,000 years and eastern Siberia for 40,000 years or more. Given this demonstrated complexity and the probability that people have been moving back and forth across the Bering Strait with ease for at least 10,000 years (see Fortescue 1998), it is ironic that many archaeologists, bio-anthropologists, and linguists continue to be impressed by three-stage models of New World prehistory (see Greenberg 1987; Turner 1988). There is hardly a possibility of simple migration theories or scenarios of massive population or cultural transfers across the North Pacific, such as those advanced by the Jesup Expedition team a century ago.

Gateways to *Jesup 2*

Beginning in the 1970s, initiatives by the International Research and Exchanges Board (IREX) began to rebuild

a bridge for bilateral Russian-American exchange in the North Pacific. The effort included research visits, conferences, publications, and even some limited instances of joint fieldwork (Campbell 1976; Gurvich 1981; Laughlin 1980: 70–4, 1985; Laughlin and Okladnikov 1975; Michael 1979; Michael and VanStone 1983). These events drew North American and Russian researchers in Arctic and Pacific studies into their first substantial contacts since the 1930s. Personal friendships were forged and research partnerships were once again established, although lengthy visits and joint field surveys were all but impossible. These early connections eventually culminated in the exhibit *Crossroads of Continents: Cultures of Siberia and Alaska* (1988), produced jointly by the Smithsonian Institution’s National Museum of Natural History and the Soviet (now Russian) Institute of Ethnography and Museum of Anthropology and Ethnography in St. Petersburg (Fitzhugh and Crowell 1988). The exhibit traveled throughout North America during 1988–92. Featuring an integrated view of North Pacific cultures, *Crossroads of Continents* served as a visual ethnography and a preliminary synthesis of the area first covered by the Jesup Expedition surveys. It also highlighted the expedition’s principal findings and the outcomes of anthropological research of the intervening 90 years.

In addition to incorporating Jesup Expedition collections from the AMNH, the joint exhibit and its catalog featured early Russian collections from Alaska of the 1800s and Alaskan materials from the Smithsonian and other North American museums. The *Crossroads* project served as a meetingplace for large numbers of American, Canadian, Russian, and European scholars over a 15-year period from 1978 to 1993. This long-term exhibit venture, its numerous symposia, and its curatorial, conservation, publication, and education programs (Fitzhugh and Chaussonnet 1994; Fitzhugh and Crowell 1988; Johnson et al. 1991; Sadler and Greenberg 1989) offered new possibilities for direct communication among dozens of researchers working on both sides of the North Pacific divide.

As Smithsonian scientists were building their *Crossroads* exhibit and scientific collaboration network, curators at the AMNH in New York launched their own venture in the Jesup Expedition legacy. Their efforts were focused on exploring and exhibiting the magnificent AMNH collections of the indigenous cultures and art of the Northwest Coast. The AMNH program, which started in the 1980s, produced the *Chiefly Feasts* exhibit on the vibrant spiritual traditions of the Kwakwaka'wakw [Kwakiutl] people, based on the objects and data collected by Boas and his partners during the Jesup Expedition. It also generated several volumes and papers focused on the expedition's activities, collections, and participants (Freed et al. 1988a, 1988b, 1988c; Jonaitis 1988, 1991, 1992, 1999). Other research projects were soon to follow or were advanced independently (Cole 1985; Jacknis 1984; Jantz 1995; Jantz et al. 1992; Ousley 1995). This triggered a revived interest in Franz Boas' academic legacy and career (Berman 1992; Stocking 1992), including a special issue of *Études/Inuit/Studies* (*Sur les traces de Boas: 100 ans d'anthropologie des Inuit/In Boas' Footsteps: One Hundred Years of Inuit Anthropology*, 1984), and led to the first detailed studies on JNPE participants such as George Hunt and James Teit who had received little attention during their lifetimes (Berman 1994; Cannizzo 1983; Jacknis 1991, 1992; Maud 1989; Wickwire 1988). By the early 1990s, the Jesup Expedition saga, its collections, and the life stories of its team members had emerged as a thriving field of research and museum activity in North America and Russia alike.

Additional trans-Beringian exchanges and scholarly and exhibit projects were launched in the early 1990s (see, for example, Dürr et al. 1992; Smith and Barrett 1990; Varjola 1990). In 1991 the Alaskan Office of the U.S. National Park Service initiated the Shared Beringian Heritage Program for research and cultural exchanges along the Siberian and Alaskan shores of the Bering Strait, under the framework of the proposed Beringia International Park. In 1993 a new "mini-Crossroads" traveling exhibit, *Crossroads Alaska/Siberia*, was organized

by the Smithsonian's Arctic Studies Center together with key Alaskan and Siberian museums. For several years (1993-97), it toured to many regional centers in Alaska and the Russian Far East (Chaussonnet 1995; Chaussonnet and Krupnik 1996; National Museum of Natural History 1997).

Today, a new generation of scholars is actively recharting the course of North Pacific/Beringian studies, and an impressive volume of archaeological research has been amassed. Still, despite much new work, the larger perspectives of culture history, the origins of North Pacific cultures, and the dynamics of prehistoric culture change in the Greater Northern Pacific Region remain almost as subject to dispute as they were at the end of the Jesup Expedition. (Of course, the same can be said of other anthropological fields.) We are left today with hardly any firm evidence beyond the past 500-1,000 years for interpreting the culture history of this region and of its amazing linguistic, biological, folkloristic, and ethnological diversity. Despite volumes of new scholarship, the basic documents on which we rely for North Pacific ethnography date back to the classic 19th-century studies on the Northwest Coast and Siberia alike. It is obvious that the ground has been laid for reassessment of the Jesup Expedition legacy in the light of modern knowledge. We now face the need to build new relationships and to train and equip new students in the field. A shared scientific language needs to be created, after two generations of scholarly isolation, and new sources of funding for joint research ventures must be secured.

As official barriers to communication across the Bering Strait were relaxed after 1988, new airline routes and connections, joint commercial and educational enterprises, direct phone and fax lines, e-mail, and many other developments emerged. A steady stream of Native and scholarly contacts across the North Pacific area was soon to follow, paralleling the pattern, if not the intensity, of ancient trans-Beringian contacts. The North Pacific is a natural and active crossroads between Asia and North America; it must have been so

ever since the first peoples migrated into what was then, 12,000–15,000 years ago, truly a “new world.” Since then, meetings, migrations, intermarriages, trading, fighting, exploring, and getting lost and being found by neighbors have occurred more or less continuously over the millennia, except for some brief periods of isolation. The 50-year-long break of the past century was probably the most effective barrier ever imposed, and the hardest to overcome.

Jesup 2 Beginnings

In 1992—almost 100 years after Boas, Frederic W. Putnam (head of the AMNH Department of Anthropology), and Jesup had begun their first discussions on the proposed survey of the North Pacific region—the Arctic Studies Center of the Smithsonian Institution advanced a blueprint for new long-term research toward these same goals. The proposed venture was called *Jesup 2*, as a centennial and intellectual successor to the (first) Jesup North Pacific Expedition of 1897–1902 (AAAS 1992; Fitzhugh and Krupnik 1994).

The new initiative, which was undertaken concurrently with the approaching (1997) centennial of the Jesup Expedition, was submitted in 1992 at a special session at the First Congress of the International Arctic Social Sciences Association (IASSA) in Quebec (Fitzhugh and Krupnik 1992). The symposium’s title, “Jesup 2: Survival, Continuity, and Culture Change in the North Pacific Region,” became the core framework for several individual and joint research ventures now commonly recognized as “Jesup centennial activities.”

Three successive symposia were organized following the initial panel of 1992. The first, “Gateways to Jesup 2: Evaluating Archival Resources of the Jesup North Pacific Expedition, 1897–1902,” took place in 1993, at the 92nd Annual Meeting of the American Anthropological Association in Washington, D.C. Participants from the United States, Canada, and Russia reviewed unknown or poorly studied museum, archival, photographic, manuscript, and other collections and raw data originating from the Jesup North Pacific Expedition (AAA

1993). This volume is the result of the “Gateways” symposium. The second session, “Cultural Continuity and Change in the North Pacific Region,” was organized at the “Bridges of Science” joint conference held by the American Association for the Advancement of Science (AAAS) and the Russian Academy of Sciences (RAS) in Anchorage, Alaska, in 1994. In November 1997 a conference celebrating the Jesup centennial, “Constructing Cultures Then and Now. Celebrating Franz Boas and the Jesup North Pacific Expedition, 1897–1902,” was held at the AMNH, the birthplace of the Jesup Expedition. This five-day international conference brought together an impressive team of over 50 scholars, museum curators, and Native cultural workers from North America, Russia, Europe, and Japan and was by far the largest and most representative gathering of people active in “Jesup area” research (Graburn 1998; Lee 1998). An exhibit of historical photographs and some ethnographic objects collected by the expedition was organized at the AMNH, and a wonderful catalog, *Drawing Shadows to Stone* (Kendall et al. 1997), was produced for the opening of the centennial celebration. The volume of conference proceedings is now in preparation for the same AMNH series that also contains the volumes of the original Jesup Expedition (Kendall and Krupnik n.d.). A similar Russian conference took place in Vladivostok, in the Russian Far East, in April 1998 (Artem’ev 1998).

The new venture was initially designed to be sustained by a scattered community of international scholars (Fitzhugh and Krupnik 1994:2), instead of being, like its famous predecessor, a centralized project with an established budget and defined responsibilities. As a result of the loose structure, many research and public activities have been initiated or have been supported by individual research and museum institutions. A new bibliography is gradually being accumulated (e.g., Fitzhugh 1996; IARPC 1995; Krupnik 1998, 2000; Mandelstam Balzer 1996; Vakhtin 1993). Four recently published books are outgrowths of the Jesup centennial efforts (Artem’ev 1998; Jochelson 1997; Kendall et

al. 1997; Shternberg 1999), and two more volumes are in press or in preparation (Ivanov-Unarov and Ivanova, in press; Kendall and Krupnik n.d.). Several international projects documenting cultural continuity and the modern revival of Native nations first surveyed by the Jesup Expedition were completed during the 1990s, including an international seminar, "Development and Self-Determination among the Indigenous Peoples of the North," held in Alaska in October 1996 (see reviews in Stern et al. 1997). Scores of new publications have been directly linked to or inspired by the Jesup centennial agenda (e.g., Fitzhugh and Dubreuil 1999; Kan 2000; Kasten 1996; Krupnik 1996, 1998, 2000; Krupnik and Vakhtin 1997b; Ousley 2000; Roon 2000; Schweitzer and Golovko 1995; Thom 2000).

These successful public activities and exchanges relating to the Jesup centennial brought together scholars, museum curators, and Native cultural activists from the two sides of the North Pacific in an effort that perhaps deserves the name *Jesup 2*. Progress in communication and broad network-building is clearly the biggest current advantage over our "First Jesup" predecessors, who often needed months (and sometimes years) to get their messages from New York to Russia/Siberia or Alaska and back.

New Research Targets

As the world enters the new millennium, scholars and the public alike are concerned with the dramatic outcomes of the past century and the legacy it will leave to future generations. Issues of environmental degradation, pollution, loss of species, and ecosystem integrity are currently of major concern to the broad constituency of natural scientists, public activists, and politicians. Both Native leaders and social scientists express a similar set of concerns with regard to human cultural diversity and the rights of local populations and cultures. Government policies, industrialization, and the spread of consumerism have damaged indigenous subsistence and languages worldwide; they have also undermined traditional arts and crafts and distorted

the cultural continuity and ethnic diversity of Native peoples on an unprecedented, global scale.

Despite the differences in political systems, in many respects 20th-century developments in Siberia and in Alaska and the Northwest Coast produced surprisingly similar results. Both areas have recently experienced revivals of indigenous cultures and sweeping drives for Native political empowerment, land rights, and self-determination. The movement has been far more successful in Alaska and Canada than in Siberia but is also gaining momentum there. Both in the Russian Far East and along the Northwest Coast of North America, cultural and language survival, Native rights, education policy, and economic and political issues are looming as major concerns on local agendas for the new century. The challenges to Native cultures are mounting, since in many northern communities Native languages have been weakened or lost, poverty has increased, subsistence economies have been weakened, and alcoholism and social disorders remain significant threats to physical and communal well-being.

As a tool for evaluating the current pace of change, the North Pacific region already has a baseline data set produced by the Jesup Expedition exactly a century ago. A new effort should be made to produce a summary of indigenous cultural continuity (and losses) during the past century. Through the example of the JNPE's method and organization, new efforts can be initiated to conduct a reanalysis of the JNPE field and its archival data, to concentrate new surveys in the same geographic area, to ensure data comparability, to facilitate studies of centennial culture change, and to encourage cross-cultural comparison.

A centennial-focused assessment of old and new data on cultural relationships and continuity may provide invaluable assistance to native communities and policy groups. It is now axiomatic that such studies should be carried out in cooperation with and on behalf of Native constituencies, with the aim of encouraging local educational, cultural, and professional development. The standard practice is certainly to take

ethical considerations into account in such work. Such studies, and concrete implementation of their major outcomes, are particularly urgent throughout the Russian part of the North Pacific region, where the recent political transition and the shift to a market economy have left many Native communities in a more destitute situation than under the Soviet communist regime. We hope this volume will serve as a catalyst for these scholarly and practical endeavors.

The Focus of This Volume

As noted, Boas never completed his assigned task of synthesizing data from the Siberian and Northwest American field surveys into a final volume for the Jesup Expedition publication series. For this reason, the JNPE has been viewed as an inconclusive, though significant, event in the history of North American anthropology (Cole 1999; Darnell 1998). Unfortunately, Boas' last (and practically his only) general review of the expedition's outcomes, methodology, and theoretical framework was presented in German in 1908 as the opening address at the 16th International Congress of Americanists in Vienna (Boas 1910b). It is still unknown why such a milestone paper has never been published in English. Whatever the reason, it remained out of sight for generations of English-speaking scholars in North Pacific research. These and other factors eventually sidelined the JNPE from the mainstream of scholarly advances in anthropological theory and field practice. To restore a rather belated justice to the JNPE efforts, and for the record of its founding father, we include here a modern translation of Boas' seminal Vienna address of 1908 (see Boas, this volume). There is no doubt that Boas was fully aware of the great methodological and theoretical value of the JNPE multidisciplinary approach and of its input to the study of human cultural development in the most general sense.

It is uniformly recognized, however, that the Jesup Expedition did achieve a more restricted goal of producing a set of "classical" ethnographic monographs on many groups of the North Pacific region. With Boas'

resignation from his position at the AMNH after increasing tensions between him and Jesup led to his departure for Columbia University in 1905 (see Cole, this volume; Darnell 1970:211–4), the "final chapter" and the overall evaluation of the legacy of the JNPE have been left for others to complete.

At the centennial of the Jesup Expedition era, a more dedicated and multifaceted appraisal is needed. What can be said now about Boas' theoretical motivations in organizing the Jesup Expedition? How can this be tested against the general intellectual discourse and the dominant anthropological paradigms of the era? In particular, the Boasian perspective on "culture" has sparked a new debate and is currently the subject of extensive scholarly reevaluation (see, for example, Berman 1996; Bunzl 1996; Darnell 1998; Jacknis 1996; Liss 1995; Stocking 1992, 1996). In this sense, the results of the century-old Jesup Expedition surveys across Beringia and the Greater North Pacific Region are as fresh in our own time as they were in Boas' day.

This volume is the first summary of such a centennial reappraisal effort. It is an outgrowth of the "Gateways to Jesup 2" panel that was organized by the volume coeditors at the 92nd Annual Meeting of the American Anthropological Association in Washington, D.C., in 1993. A few of the nine original papers from the AAA panel do not appear in this volume, while some new contributions—those by Cole, Thom, Krupnik, and Willey—were submitted after the session. Abridged versions of two papers from this collection appeared earlier in a special issue of the *European Review of Native American Studies* under the editorship of Christian Feest (Kan 2000; Thom 2000).

This volume thus initiates the process of a modern reappraisal of some of the less recognized aspects of the JNPE legacy that extend far beyond its voluminous publications and ethnographic collections of a century ago. The task leads us into three separate aspects of JNPE historiography: (a) the origins and intellectual background of the expedition; (b) a critical assessment of its fieldwork and collection practices; and

(c) its various archival legacies, which provide a lasting trove of documentary evidence for analysis of the expedition's results. The contributions in this volume are organized to emphasize such a progression.

Part 1, "The Expedition," with contributions by Cole and Vakhtin, explores the intellectual roots of the Jesup Expedition and presents an informative historical counterpoint that aids in assessing the complexity of the project and its final outcomes. Douglas Cole—who passed away in August 1997, a few months before the Jesup Centennial Conference in New York (see "In Memory of" below)—produced the most detailed update of the complete JNPE multiyear saga. His approach proceeds from the perspective of the Boas-Jesup relationship and what each was hoping to achieve. "Pure science" and museum goals were clearly juxtaposed, and ultimately, the outcome favored the museum's priorities more than those of science. Nikolai Vakhtin offers valuable insights on the less-known record of the assembly of the Siberian portion of the JNPE, which was in certain ways a more radical scientific venture than what was done on the American Northwest Coast. Of the two, Cole's perspective is more critical of Boas' motivations and tactics, and of the end results that leaned in favor of Morris Jesup's expectations.

Part 2, "The Collectors," with papers by Harkin, Mathé and Miller, Thom, and Berman, presents modernist perspectives on the field approaches of the Jesup Expedition, particularly from the point of view of researcher-Native relationships. It is hard to imagine less congruent sets of data—textual records, photographs, and archaeological excavations—yet combined, they reinvigorate the image of the interdisciplinary and pioneering JNPE research. Harkin examines Boas' fieldwork among central Northwest Coast groups in the context of his training in German Romantic and liberal social science thought, in which texts and myth rather than history determine cultural content. Mathé and Miller review the photographic practices of the JNPE team members from the modern perspective, which directs attention to the "framing" (even the "staging") of the

ethnographic reality, as well as the power/status interplay of the photographers and their human subjects. Thom's paper is a modern archival chronicle of archaeological work conducted by Harlan Smith along the southern Northwest Coast between 1897 and 1899. This was a region in which Boas expected archaeological data to reveal significant evidence of historical change. Smith's finds importantly reinforced Boas' mistaken view that archaeology, linguistics, oral history, and culture could be combined into a single unified thesis of North Pacific (pre-)history. Smith's previously unstudied archival documents reveal a human context for this early archaeological research and shows how social relationships, good and bad, shaped his and Boas' conclusions about regional prehistory in ways that are not evident in the published JNPE reports. Finally, Berman offers a new perspective on the Boas-Hunt collaboration and on George Hunt's contribution to the JNPE and later documentation efforts. In a detailed review of the unpublished manuscripts from Boas' and Hunt's monumental corpus of Kwakwaka'wakw [Kwakuitl] texts, she unveils an intricate and often conflicting play of human and professional relationships that were never disclosed in their voluminous folkloric and ethnographic writings.

Part 3, "The Resources," with papers by Kan, Ousley and Jantz, Keeling, Krupnik, and Willey, presents a series of new studies of both known and "rediscovered" materials produced during or after the expedition. The list ranges from Kan's story of the painful saga of Leo Shternberg's manuscript on Gilyak [Nivkh] social organization that was produced for, but never published in, the JNPE series, to the modern appraisal by Ousley and Jantz of the monumental JNPE corpus of anthropometric measurements that was duly collected but neither processed nor published in Boas' time, to Keeling's analysis of the expedition's ethnomusicological legacy, preserved on old wax-cylinder recordings.

Next, Krupnik demonstrates the overall impact of the Jesup Expedition in a comprehensive bibliography of its published and unpublished writings. This record

alone fully validates the JNPE's preeminent role in the history of regional and theoretical studies in anthropology and culture change. It furnishes strong support to those of us who believe that the JNPE did establish an unprecedented and monumental record of anthropological documentation. In a similar way, Willey's paper provides a comprehensive review of the expedition's photographic record of some 3,500 historical images that are now catalogued and organized thematically in the AMNH Special Collections files. There is clearly more grist to grind here, both for those interested in regional scholarship and for those seeking to build on the vision of trans-Beringian contacts and history begun by Boas and his partners.

Editors' Notes

A few technical comments will be helpful to readers in matching the old realities of the Jesup Expedition era with today's practices in anthropological publications. The names of the Russian members of the expedition, including Waldemar Bogoras, Waldemar Jochelson, his wife, Dina Jochelson (Jochelson-Brodsky), and Leo Shternberg, have been spelled in many different ways in various languages. Throughout the volume, we follow the long-established spelling used in their English publications and in major reference bibliographies: Bogoras, Jochelson, Jochelson-Brodsky, and Shternberg. Some references to Russian or German publications and personal correspondence, however, preserve the forms used in the respective languages: Bogoraz, Jochelson-Brodskaya, and Sternberg. In certain sections the Russian spelling—for example, Bogoraz instead of Bogoras—is preserved to underline the Russian setting of the story.

In this volume, as well as in several other publications by the Arctic Studies Center, we adhere to the commonly accepted practice of presenting Native ethnic and tribal names as singulars (Eskimo, Chukchi, Koryak, Aleut, etc.) instead of plurals. We generally use modern names in contemporary text; for example, Chukchi, Yukagir, and Nivkh instead of the names

Chukchee, Yukaghir, and Gilyak that were common during the Jesup Expedition era. We do keep the old names when referring to the Jesup Expedition volumes and to the subsequently produced publications. Several ethnic names, both in Siberia and in North America (for example, Eskimo, Tungus, Kamchadal, Kwakiutl, and Thompson), have been abandoned or have become obsolete since the time of the Jesup Expedition. We introduce modern names (Inuit or Yup'ik, Even, Itelmen, Kwakwaka'wakw, Nlaka'pamux) wherever appropriate, but we usually allow the authors to follow the name patterns now accepted by local communities in their respective areas.

This book is illustrated by numerous original photographs from the Jesup Expedition era, including many taken by the expedition field crews in Siberia and North America. We are grateful to the JNPE's host institution, the American Museum of Natural History in New York, for permission to reproduce these precious historical images. Special thanks go to Barbara Mathé, head of the Special Collections division at the AMNH Library, who was instrumental in selecting and securing most of the illustrations on the AMNH files. Some other illustrations—individual photographs, copies of field sketches, personal notes, etc.—are reproduced from the originals on file at the American Philosophical Society in Philadelphia; the Archives of the Russian Academy of Sciences, St. Petersburg, Russia; the Peter the Great Museum of Anthropology and Ethnography, also in St. Petersburg; and the Hudson's Bay Company Archives in the Provincial Archives of Manitoba. (See the List of Illustrations.) We thank all the institutions that granted us permission to use these documents as illustrations in our Jesup centennial collection.

Acknowledgments

This volume's progress was by no means smooth and easy, and, like many of the proceedings of the JNPE, it suffered several setbacks—although, of course, for different reasons. The full manuscript was completed and submitted for publication in 1997. Dori Stewart

provided invaluable assistance in preparing that version. We praise her heroic contributions in formatting into a single editorial cast the 11 original papers (and disks), delivered with unimaginable individual variations. Three years later, Elisabeth Ward of the Arctic Studies Center undertook the challenging task of breathing new life into a long-dormant venture. She skillfully coordinated the uneasy process of updating the papers and again editing the volume, with professional assistance from Nancy Levine. Last but not least, we want to thank all the contributors for their support, patience, and dedication. Their trust and commitment helped us persevere through years of delays and withered hopes to see this volume finally published under the Arctic Studies Center's newly established *Contributions to Circumpolar Anthropology* Series.

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The Results of the Jesup Expedition

Opening Address at the 16th International Congress of the Americanists, Vienna 1908

FRANZ BOAS

Translated by Saskia Wrausmann

If I accept the honor of this invitation to report on the results of the Jesup North Pacific Expedition, I must first express my deepest regret about the loss of a man without whose heartfelt interest in ethnological research we would not have been outfitted for this expedition. This spring Morris K. Jesup, president of the 13th International Americanist Congress [in 1902—ed.] and president of the American Museum of Natural History in New York, passed away. He lent his continued interest to the development of American science and dedicated his great organizational skills and rich resources toward the loftiest goals of humankind. For more than 25 years, Mr. Jesup was head of administration at the American Museum of Natural History. During those years, he was able to advance not only the natural sciences but also anthropology in every possible direction and to foster the value of scientific inquiry in his fellow citizens.

I had the honor to lead the largest comprehensive study that Mr. Jesup sponsored, the North Pacific Expedition. Starting in 1897, we conducted extensive ethnological and archaeological research on the East Coast of Siberia and the Northwest Coast of America. The goal was to investigate how the peoples of this vast territory interrelate. The expedition itself was completed in 1902. The results of our observations are accessible to the scientific community in a large number of volumes, which should all be completed in about four to five years. Mr. Jesup himself paid for the expenses incurred by the expedition and the publications, a total cost of about \$100,000.

Before I discuss the results of the expedition, it seems appropriate to delineate the aspects that guided the development of my research plan. In my view, the goal of the expedition was to resolve the question of how the cultures, languages, and races of the Old World relate to those of the New. Of particular interest was the extent to which Old World influences extend into the heart of North America, and vice versa. These questions naturally are connected in a fundamental way to the larger problem of the place of Native Americans among the peoples and races of the world.

Our theoretical framework is directly linked to new perceptions on the evolutionary history of humankind that have been recently developed by a number of researchers.

In all anthropological investigations, we face the difficult fundamental question of how to explain similarities in cultures that are geographically distinct. Some scholars, in particular those of the British school of anthropology, consider similarities as evidence for parallel evolution of humankind in all parts of the globe. In its most extreme form, their view proposes that human culture everywhere always follows the same path. Thus, currently existing circumstances represent different stages in the course of human development. Other scientists do not feel that similar ethnological appearances prove the existence of different developmental stages but that they are the effect of analogous psychological laws which appear with great uniformity in all parts of the world and at all levels of development.

In opposition to these views is a more individualized theory in which culture is a product of a specific history and development that relies not only on the mental and physical accomplishments of a people but also acknowledges that new ideas and modes of living arise through contact with neighboring peoples and external forces. Supporters of this theory tend to attribute cultural similarities between discrete areas to a common history. Cultural similarities that occur in neighboring areas are interpreted as fairly recent borrowings and adaptations. A common ancient culture core (*Kulturgut*) explains parallels in distant regions.

Obviously, approaching the issue of cultural resemblances from a strictly psychological and developmental-historical standpoint makes a research question such as the one posed by the Jesup Expedition seem impossible to solve. If all the differences of a restricted region are ascribed only to faster or slower cultural development, or if all the similarities are attributed only to common psychological processes, a historically based examination of mutual influence in cultural development is futile.

We must now admit that many of the cultural similarities in remote areas are so sporadic and unrelated that it is extraordinarily difficult to defend a common origin. The wealth of ethnographic information that has come to light in the past decades points to the inescapable fact that the customs and innovations of one people—say, in North America—can be paralleled by those of another people in any other part of the globe. Thus, it is undeniable that convergent developments and inventions of a people cannot be unequivocally used as proof of a historical connection because each would have to be linked historically to all others. On the other hand, we have a wealth of proof regarding the dissemination of ideas from one people to another. We have convincing evidence that the *Verbreitungskreise*, the expanding circles of cultural achievements, of early, prehistoric times extended very far. The spread of cultivated plants and domesticated animals in the New and the Old Worlds, and the

enormous distribution of complicated folktales, provide clear testimony for widespread diffusion.

These considerations were imperative in working out the complete plan and in choosing a particular question for investigation. It seemed that our method of research had to be based on the recognition of historical lines of diffusion but should also acknowledge similar psychological tendencies in remote areas and among all peoples, which result in parallels that are not historically caused. If we keep in mind both sources of cultural similarity, we must demand, in a careful, methodical investigation, that an isolated similarity of singular traits should never be seen as proof of a historical connection. We may only, if there are other influential reasons, work with the hypothesis of lost geographic links. A historical connection, then, can only be considered established when a number of complicated cultural forms appear evenly over a contiguous region, outside of which they are either missing or are disjointed and fragmentary.

This does not yet demonstrate that certain appearances emerge from a single center. However, it is extraordinarily likely that related cultural forms blend over the total area. The question of where a cultural trait was acquired should be left out of further discussion because the present concentration of its distribution provides only very unreliable evidence for the location of its first appearance.

For this research technique, the negative element—the absence of typical cultural forms outside an isolated region—carries a lot of weight. This circumstance calls for special care in the collection of evidential materials. I want to emphasize sharply that from this point of view, the evidence for a connection between the Melanesian-Malay culture and the northwestern American culture in the sense of Friedrich Ratzel is not convincing. Instead, one should demand a gradual exploration of a problem that is of such sweeping importance.

These observations provide the foundation for the entire JNPE research. Exactly determining the geographic

distribution of ideas and cultural forms, carefully analyzing assumed cultural acquisitions, and studying the connection between contemporary and prehistoric populations within the land stretches in question are all necessary in order to obtain a clear picture of the development of culture and the distribution of peoples in all areas.

Most of all, it seemed imperative not to shirk any effort in explaining the historical development of the culture of each area. Aside from the geographic comparison, no research method now seems more promising than surveying how different tribes perceive their own customs and interpret their own traditions. If it is true that a large part of every tribe's culture is acquired, then it is no less true that the acquisition only becomes a genuine part of the culture if it fuses with native perceptions into a comprehensive whole which has a more or less expressed character. In other words, the foreign element in a culture becomes native by being permeated by the spirit or style of the native culture. Because of this, knowing the spread of an objectively similar form in connection with its subjective interpretation gives us the best method for explaining the processes that made each culture, in form and content, what it is now; both are naturally in a relationship of the closest reciprocity. This point of view gave weight to study of the people's own interpretation of their traditions. It thus seemed supremely important to document the anthropological material through uncensored accounts of natives in their own words and in their own language, to preserve the original meaning. In addition, text material gained through such a process provides an unparalleled basis for purely linguistic research. Such research is clearly superior to a simple collection of vocabulary and grammar and is absolutely necessary for our purposes. The division of languages into dialects, word borrowings, tonal and grammatical influences, and the presence of large morphological groups are all manifestations that are vital in answering our question.

It hardly needs to be mentioned that the study of

body types of different peoples should not be neglected. A study of the kind I have outlined seems worthwhile for two reasons. First, we hoped to make an important contribution to our knowledge of the relationships between the indigenous peoples of America and those of the Old World. Second, we hoped that studying the historical development of a large area inhabited by peoples of a simpler culture would give us the means to treat with greater methodological precision the vexing ethnographic problem of independent invention versus acquisition.

It appears to me now that this method of carefully analyzing a geographically connected region through our work with the goal of clarifying historical associations is vividly justified. We found clear proof of cultural acquisition everywhere. Not only that; this method allowed us to reconstruct population migrations. Thus, the people of the North Pacific coastal region no longer appear to be unchanging, ahistorical entities. We see cultures as changing constantly, each people influenced by its proximal and distal, spatial and temporal, neighbors. We recognize that although in the historical sense these peoples are certainly primitive, the structure of their thoughts and beliefs should not be interpreted as comparable to that of our earliest ancestors and thus placed neatly within the developmental-historical order. Instead one should look for their origin in the complex ethnic relationships of all people.

We do not deny that the acquired cognitive material with which these populations operate has the tendency, through certain psychological laws, to take on forms that remind us of distant peoples. We regard the clarification of these psychological factors as one of the most important tasks of ethnology. It should result not in a simple evolutionary formula that can be applied to all humankind but rather in laws of perception, thought, feeling, and desire that affect the various cultural forms of humankind.

Thus, the results of our research complement the conclusions drawn from detailed studies of Africa, Oceania, and early European history.

After this brief presentation of our research assumptions, I would like to go on to discuss my course of action and the more specific results of the expedition. The people we studied in Asia can be grouped together as the isolated tribes of East Siberia. Four independent language families are spoken there, from Japan northward to the Bering Strait and westward to the Kolyma River: Ainu, Gilyak, Chukchee, and Yukaghir. Tungus and Turkish tribes have invaded the region from the south, considerably changing the cultural and physical characteristics of some natives. Only the Ainu of Sakhalin Island and Hokkaido Island were not covered by the original expedition proposal because older reports are numerous. Messrs. Waldemar Bogoras, and Waldemar Jochelson, Mrs. Jochelson, Ph.D., and Mr. Berthold Laufer, Ph.D., worked on the Asian side. Later, we were fortunate to receive the assistance for the expedition of Dr. Leo Shternberg, whose extensive knowledge of the peoples of the Amur River area is of great importance for our problem. Unfortunately, it was not possible to extend our work to the isolated tribes of West Siberia.

The area covered in North America stretches from the Bering Strait south to the Columbia River. The tribes there speak 10 independent languages. We did not consider two of these tribes, the Eskimo and the South Alaskan Tlingit, because other research was available. Unfortunately, it was impossible to include in our observations the Aleut, a tribe whose position is still unclear. Our main focus incorporated the tribes of the province of British Columbia and the state of Washington. Considering our prior knowledge of the Haida, Tsimshian, Kwakiutl, and Bella Coola, it seemed advantageous to focus our attention on these and other Salish tribes. John R. Swanton, Ph.D., James Teit, Professor Livingston Farrand, and I conducted this work. Gerard Fowke, on the Asian side, and Harlan I. Smith, on the American side, were responsible for archaeological research. Mr. Bogoras, Mr. Jochelson, Mr. Swanton, and I were especially dedicated to linguistic research, with Mr. Bogoras working with the Chukchee

and Koryak, Mr. Jochelson with the Yukaghir, Mr. Swanton with the Haida, and myself with the Tsimshian, Kwakiutl, and Salish tribes. Mrs. Jochelson, Ph.D., primarily collected anatomical and anthropological material in Siberia, and I did the same in America. This material consists of large collections of photographic types, skeletons and skulls, and measurements taken both directly from humans and from plaster casts, which I value greatly. Naturally, the skeletal material is inconsistent, since skulls and skeletons are hard to obtain in areas where cremation is practiced or other obstacles occur.

In recounting the results of the entire enterprise, I will first remark on the stark contrast that has developed between the tribes of the American North and Northwest and those of the southern parts of North America. A comparison of our results with two new, detailed studies of the Plains Indians and the tribes of California and the Western Plateau area of North America convinced me that the tribes of the Pacific states, including the larger part of California and the Arctic Coast, are marginal populations, in the sense of Friedrich Ratzel. Undoubtedly, these cultures originally covered a much larger part of northern North America. The more we familiarize ourselves with the specific ritual culture of the southern Indian tribes of the Union, and the more details of their art we understand, the better we can disregard the secondarily formed perceptions of the world that distinguish the culture circles of North America, and the clearer it becomes that they appear to be gradually attenuating from south to north and that we may in all likelihood be seeing the northern ripples of the Central and South American culture circle. Two *Kulturströme*, or cultural flows, seem to have advanced northward from Mexico across the deserts and steppes of the Southwest, as well as from the Antilles. These migrations make North American culture and the central areas of both North and South America appear to be a unique entity.

I am therefore impelled to assert that the culture circle of the Arctic and Pacific Coasts, including the

larger part of California, represents an older American culture type that has been barely touched by the expansion of the civilized American peoples. What we usually call typically American represents a blending of the cultures of the larger part of the continent, which took place relatively recently, without reaching the southernmost and northernmost parts of the hemisphere. The expansion of Indian maize cultivation, to the extent that it is independent of climatic conditions, clearly demonstrates the aforementioned contrast.

The northwestern peoples of the American coast seem to have originally had an intimate connection with the isolated peoples of East Siberia. The problem that confronts us here is perhaps one of the most difficult and strangest that we encountered during our research expedition. In superficial observations, similarities in the cultural lives of East Siberian coastal peoples such as the Chukchee and Koryak and of the Eskimo are noticeable. It would seem as if the coastal Chukchee and Koryak possessed the main characteristics of the Eskimo culture. On the other hand, a significant contrast appears between the inhabitants of the coast of British Columbia and the shores of the Sea of Okhotsk. Nevertheless, a comparison of the mythological repertoire of these peoples teaches us that a widespread concurrence is found between East Siberia and the southern parts of the North Pacific Coast. Mr. Jochelson, Mr. Ehrenreich, and I discussed a number of these parallels at length, so that I can point out here that an association between American and Asian motifs undoubtedly exists. I will mention here only magic flight, a complicated myth motif that is common and well developed only in northwestern America, although it does seem to have reached far into the prairie and the South.

It is certainly not insignificant that the Raven plays a prominent role as ancestor and sometimes as creator in the mythology of East Siberian peoples. The Raven has the same role with the North Pacific peoples of America. The interpretation of this similarity is made

considerably more difficult by the fact that neighboring Indians heavily influence the Eskimo from Alaska east to the Mackenzie River. We know that Indian animal mythology plays an equally important role. Undoubtedly, the mythological repertoire of the entire North Pacific Coast, starting from the Sea of Okhotsk east and south to the Columbia River, contains many common elements.

However, there is an important difference here. While Eskimo animal mythology, as far as I know, is largely newly acquired, the Indian-like myths of the East Asian peoples such as the Koryak are much older. I am impelled to assume a very old association in that direction, and it does seem likely that an ancient connection existed between the peoples of the Sea of Okhotsk and the Indians of British Columbia—a connection that could be older than the arrival of the Eskimo at the Bering Strait.

At present, we are not able to answer this important question with full conviction. We do hope, however, that archaeological explorations along the coast of Alaska will give us a definitive answer. This hope relies mainly on the sharply defined physical type of the Eskimo, which differs so much from that of neighboring peoples that a row of Eskimo skulls can be readily recognized as such. If other types were to be discovered in older layers on the shores of the Bering Sea, we would have proof that the Eskimo of Alaska should be looked at as later migrants from the more eastern regions of the (North) American Arctic.

I would like to point out a phenomenon that I find significant and that reopens in a certain sense the Boyd-Dawkins theory of a possible connection between prehistoric Europe and the Eskimo. As is well known, Boyd-Dawkins compared the occurrence of Paleolithic harpoon types and carvings with those of the Eskimo and concluded that their obvious similarities meant a possible connection between the two. One must now recognize that although harpoons are found almost everywhere, the unique tools with holes in them, and the tendency toward artistically realistic objects, are

hardly found in the same pattern of association. Now one must emphasize that the ornamental designs of prehistoric Europe also show a distinctive similarity to the designs of Arctic America. For example, a design of two carved parallel lines with short alternately placed grooves that point inward at a right angle and, when wide enough, create a zigzag pattern can be found in both areas, on ivory or bone. As far as I know, this design has not been discovered in any other part of the globe. I do not want to draw from these similarities the conclusion that the Boyd-Dawkins theory is proved. I do believe, however, that these similarities deserve our continued attention by way of keeping in mind the possibility of cultural links.

Given the importance of this question, it seems appropriate to examine more closely the range of the mentioned ornamental types. When compared with the older collections from Europe, the collections made during the Jesup Expedition prove that the types and designs in Siberia evidently occur up to the Lena River, and in North America all the way east to northern Greenland. I myself have had the opportunity to inspect large collections from southern Greenland. However, my friend Dr. Thalbitzer tells me that he has seen no types of this sort in the Greenlandic collection in Copenhagen. I do not know whether these ornamental types exist in prehistoric western Siberia and in Russia.

Let us now turn to a discussion of the relationship of languages and the anthropological types of Northwest America and Northeast Asia.

According to the data collected by Mr. Bogoras and Mr. Jochelson, it seems safe to say that the isolated languages of Northeast Asia cannot be separated from the American languages on the basis of phonetic and morphological characteristics. However, one must remember that a unity of all American languages, in the form proposed by earlier researchers, does not exist. Instead, we can group the colorful multitude of American languages into a number of morphological categories that display significant, even fundamental, differences among each other. Neither

incorporation nor polysynthesis can be considered a specifically American language trait. The interpretation of these language family groups whose genetic relation cannot yet be determined causes the same problems in America as in other continents. I assume that this phenomenon is similar to the one that led Mr. Wilhelm Schmidt to group so many of the languages of Southeast Asia together and on which Lepsius already focused in his study of African languages.

Whatever the later interpretation of this problem may be, it does seem confirmed that the eastern group of the isolated Siberian languages leans more toward America than toward Central Asia and that if one must draw a line, they are best categorized with the American languages.

Physical anthropology studies in the area in question reveal similar conditions. Because of intrusions by Tungus and Yakut [Sakha—ed.] tribes, Northeast Siberian tribes undoubtedly underwent assimilation, so that, for example, the Yukaghir have strong blood relations with the Tungus and Yakut. The Mongolian features of the Northeast Siberian peoples, which are especially expressed in the shape of the eye and nose, are thus strongly developed. On the other hand, the development of the cheekbones seems at the very least less prominent in the tribes of the Far East, such as the Chukchee and Koryak, than in the Yakut.

In America, the purely Mongolian features increase significantly toward the Northwest. First and foremost, the strong development of the nose in the American Northwest disappears, as is typical among the peoples of Asia. The "Mongoloid eye" is more strongly developed, although not with the same intensity as in Asia. The face shape approaches the flat Asiatic shape more and more, and even the skin color varies little between Asiatic and American peoples.

So, it seems that the native Siberians and the Americans of the Northwest Coast constitute one entity.

I am perhaps permitted to rephrase the problem of the position of the Native American population in light of this new information. Everything leads me to

believe that humans have inhabited America for a long time. It has not yet been decided whether the migration occurred before or after the last Ice Age, but all criticism by geologists notwithstanding, an early migration may be supported in all probability. If we may assume such an early migration in America, it does not seem impossible that the isolated peoples of Siberia represent a postglacial back-migration out of America. On the other hand, it may also be possible that the white race, which has flooded the entire globe over the course of time, originally appeared as a localized variety.

Let us turn from these general questions, which inevitably lead to more or less uncertain hypotheses, to the specific results of the Jesup Expedition. I would first like to note that we were able to prove a significant number of shifts of populations and culture in America and Asia. Mr. Harlan I. Smith's archaeological research and the linguistic studies conducted by Mr. James Teit and myself all led to the same conclusion. The distribution of peoples in southern British Columbia has been changed by a wave of migration that brought Salish tribes from the interior across the Rocky Mountains to the coast. The coastal inhabitants have culturally assimilated these tribes almost completely. We have here the interesting theoretical example in which a totem and clan organization was acquired by a tribe that previously had a simple family organization. This transition has been found in a number of tribes that were subjected to the cultural influences of the coast. Thus we cannot assume the typically unspecified development from totem organization to a simpler form.

Our in-depth anthropological study of the residents of northern Vancouver Island supports the fact that the Native tribe here originally had a close connection to the tribes of the Columbia River. These relations would have been subsequently interrupted by the immigration of inland tribes.

A second interesting migration wave can be followed in northern British Columbia. The Tsimshian are

part of the groups of the Alaskan coastal regions, which are characterized by strongly expressed high culture. Their myths and basic religious beliefs, however, point to a close association with the population of the northern section of the West American Plateau area. More specifically, these data point directly toward a connection with the cultural group represented by the Northern Shoshone. To fully clarify the matter, we will look at the extensive collection of material from the western parts of the Mackenzie River basin. It has already been determined that the Tsimshian can be considered new settlers in the coastal region. It is remarkable that their type of language is completely isolated and that it seems to be most closely related to the Shoshone and Kutenai groups.

I have already discussed the probable shift of the Eskimo westward.

Unfortunately, there is a total lack of precise information on the Aleut—information that is imperative for a comprehensive solution to the problem we are discussing here. We should therefore greet with joy Mr. Waldemar Jochelson's preparations to study the Aleut in connection with the large Raboushinsky Expedition that has been planned.

Aside from the more recent and documented invasions by the Tungus and Yakut in Northern Siberia, no similar larger movements can be proved in Asia. Mr. Bogoras and Mr. Jochelson have shown that the Kamchadal, the Koryak, and the Chukchee comprise a linguistic unit and that their original range of distribution reached as far west as the Kolyma River. Mr. Jochelson has finally determined the Chuvan people to be a branch of the Yukaghir.

One of the most important cultural-historical facts emerging from the expedition's research relates to the domestication of reindeer in East Asia and the conspicuous and complete lack thereof in America. To put it briefly, it seems that West Siberian peoples use reindeer in the same way as their neighbors use cattle. The Central Siberians use the reindeer like Turkish people use the horse. In contrast East Siberians now

use reindeer more like they once used the dog. From these and other facts, we may draw the conclusion that rein-deer breeding everywhere adjusts to the older culture of a people or to the cultural forms of its neighbors. With the East Siberian peoples, everything seems to support the idea that perhaps only a few centuries ago the Chukchee, as well as the Koryak, were purely coastal inhabitants with economic practices not unlike those of the Eskimo. A strong proliferation of these tribes and a peopling of the interior seem to have happened only after the reindeer gradually started replacing the dog. Considering the lively exchange between Asia and America in the area of the Bering Strait, the complete lack of the reindeer culture alongside a lively trade in other cultural attainments in America can hardly be explained otherwise. A confirmation of this view also results from the unusual lack of adaptation of the Chukchee dwelling to the demands of the nomadic lifestyle. The Chukchee tent is to be understood architecturally as an adaptation to the nomadic lifestyle of

the Eskimo-type subterranean dwelling. In its heavy clumsiness, it differs surprisingly from the easily mobile tent of the Eskimo.

I cannot discuss here in detail every conclusion of our whole endeavor. As expected, the members of the expedition have collected a wealth of ethnological, linguistic, and anthropological data. We originally estimated that these materials would be published in 12 quarto volumes. Now that about 7 volumes have been published, we realize that there is too much information for the planned size of the publication. However, I hope we will reach a satisfying conclusion for the exploration Mr. Jesup so generously organized by publishing its complete results.

Note

This is a translation of Franz Boas (1910), *Die Resultate der Jesup Expedition. Internationaler Americanisten-Kongress 16, 1908. Erste Hälfte*, pp. 3–18 (Vienna and Leipzig: A. Hartleben's Verlag). Printed as a separate issue in 1909.

In Memory of Douglas Cole, 1938–1997

IGOR KRUPNIK

Douglas Lowell Cole, of Simon Fraser University, died suddenly of a heart attack on August 18, 1997. He was not quite 59, and his death came as an unexpected tragedy. It happened three months before the Jesup Expedition Centennial Conference in New York in 1997, where Cole was to deliver a plenary review paper with the same title as his chapter in this volume. His life and professional career have been covered at length by several posthumous publications, (see Cole, this volume), to which an interested reader can turn.

In this era of virtual communications, personal connections are built quite differently than in the time of Boas and the Jesup North Pacific Expedition. I never met Douglas Cole in person, and we spoke by phone but once. Introduction to each other, progress in understanding, and building of mutual respect all took place in cyberspace. The communication lasted for about a year, and it left a file of some 40 e-mail letters and messages. This is, of course, not much, but the result is this contribution of Cole to the Jesup volume and our deep sense of a sad loss.

In April 1996, Douglas Cole sent me a short letter expressing his interest in our forthcoming collection of papers on Boas and the Jesup Expedition. Of course, we knew of his book on the history of the Northwest Coast museum collections, *Captured Heritage* (1985), and of his many other publications on Northwest Coast history and Franz Boas. In response, I wrote to him about the *Jesup 2* program and invited him to exchange some materials of mutual interest. Intrigued, Cole offered to send us a rough cut excerpt of sections on the Jesup Expedition from his forthcoming biography

of Boas for comments, advice, and criticism. As we read this first pasted draft, I invited him to rework it into a review paper on the expedition's history for our collection of Jesup essays. Within four months, we received a 60-page manuscript.

This is however only part of the story. Douglas Cole had his special and quite distinctive view of Boas as a person and a scientist and of Boas' interactions with other prominent personalities of the time, and he did not flinch when his revisionist opinions contradicted many a popular perspective. In any convention of modern Boasian admirers, Douglas Cole was an indispensable and a challenging ingredient. His initial evaluation of the Jesup Expedition as an artificially inflated venture and merely a Boas failure was highly provocative, at the least, and it was largely unfair, to our minds.

In underlining this, I offered to include Cole's paper in our *Jesup 2* volume as a "voice of dissent," reserving our right as editors to submit an editorial rejoinder. Although tough as an opponent, Douglas Cole was very keen in accepting criticism. Several letters followed, and many comments and materials were exchanged. The final result of this interaction is presented in the next chapter. It preserves Cole's original critical stand, though moderated to mutual satisfaction.

Douglas Cole did not live to see the publication in 1999 of his major scholarly volume, *Franz Boas. The Early Years, 1858–1906*, or to personally meet the network of *Jesup 2* researchers. This loss to our common studies of the Jesup Expedition history and legacy is indeed irreplaceable. We will miss Douglas Cole and his insights tremendously for many years to come.

4/ Camp of the Reindeer Koryak and herd of reindeer, with the Jochelsons' field tent in the middle, 1901 (AMNH 4168)



part 1

THE EXPEDITION: HEMISPHERIC PERSPECTIVES

“The Greatest Thing Undertaken by Any Museum”?

Franz Boas, Morris Jesup, and the North Pacific Expedition

DOUGLAS COLE

Franz Boas was a curator in the Department of Anthropology at the American Museum of Natural History (AMNH) for almost 10 years, from January 1896 until May 1905. From this Central Park West locale, he initiated numerous projects, some of which, such as an African and Asian missionary collection, were fruitless and forgotten. He invested his greatest ambition in three major museum initiatives: an East Asiatic project which, beginning with China, would move to the Philippines and Malaya; a North American “Vanishing Tribes” project that hoped to salvage ethnological and linguistic information from the scores of North American Native groups endangered by Euro-American settlement; and the Jesup North Pacific Expedition to investigate groups on both sides of the Bering Strait.

The East Asiatic project placed Berthold Laufer in China from 1901 to 1904 but then collapsed. “Vanishing Tribes” went on fruitfully, though never at Boas’ desired pace, both under him and under his successor, Clark Wissler. The Jesup Expedition, the most cherished of Boas’ museum projects, ran for its full five years, produced a large quantity of publications, and exercised a continuing influence on research, especially on the western side of the Bering Strait. It was the showpiece of Boas’ association with the AMNH. Recent evaluations of the Jesup Expedition have been kind. The expedition was “an anthropological tour de force,” a “grandiose, brilliantly conceptualized, and masterfully orchestrated attack on one of the most important problems in American anthropology”

(Fitzhugh and Crowell 1988:14) that “still ranks as the foremost expedition in the history of American anthropology” (Freed et al. 1988b:7).

The prime instigators had more ambivalent feelings. To AMNH President Morris K. Jesup, the expedition had, by the time of his death in early 1908, become a matter of “many disappointments,” “an enterprise that has involved expense and anxiety out of all proportion to the representations that were originally made” (Jesup to Osborne, 30 April 1906, AMNH, File 293b). Boas, too, faltered in his faith. Although he publicly praised Jesup and the expedition, he privately expressed a wish to “simply dump the whole Jesup Expedition and concern myself no further with it” (Franz Boas to Sophie Boas, 18 March 1909, APS).¹

Background

Born in Prussian Westphalia and educated at Heidelberg, Bonn, and Kiel, Boas began his anthropological work during a yearlong expedition to Baffin Island. He sought a position, preferably in the United States, but could find nothing except a temporary assistantship at Berlin’s Royal Ethnological Museum. There he encountered its recent, rich Northwest Coast collections and had an opportunity to study briefly a group of touring Bella Coola [Nuxalk]. All the more intent on an American career (and, *cherché la femme*, on seeing his New York fiancée), he traveled to New York and, borrowing money from relatives, made a first visit to the Northwest Coast. He was then asked by the

Northwest Tribes Committee of the British Association for the Advancement of Science (BAAS) to survey the Native tribes of British Columbia, which were threatened by settlers brought in by the recently completed Canadian Pacific Railway. Boas made five more trips to the Northwest, on behalf of the BAAS or with the support of the American Bureau of Ethnology. In the meantime, he had secured a position with *Science*, a weekly New York journal, and had married Marie Krackowitzer, the American-born daughter of an Austrian "Forty-Eighter," one of the liberal-minded Germans who had left after the disappointment of the Revolution of 1848.

Boas had come to the United States in part because of the opportunity it offered as a raw scientific field. But rawness carried, as he soon found, the problem of there being few positions. He suffered a series of false starts: at *Science*, at the new Clark University [in Worcester, Mass.], at the Chicago World's Fair [the 1893 World's Columbian Exposition], and at Chicago's new Field Museum. In 1896, however, his chief at the Chicago Fair, Frederic W. Putnam, who had become curator of anthropology at the AMNH as well as director of Harvard University's Peabody Museum, wedged him into an assistant curatorial position at AMNH and a lecturer's appointment at Columbia College. From these posts, Boas' training, disciplinary breadth, ability, and incredible industriousness allowed him to become a commanding presence in his field.

Boas had arrived at the AMNH at a bad time. The country was in a severe depression, with the museum's trustees and donors made all the more nervous by the growth of the populism, free silver, and single tax movements. The Anthropology Department received no acquisition budget in 1896, and the museum's president, Morris K. Jesup, soon had regrets that he had taken on Boas' salary commitment. Jesup had, however, already decided that the Anthropology Department, along with vertebrate paleontology, should receive priority treatment. To this end he had hired Putnam, the best man he could get as curator, and had agreed to take on Boas as an associate curator.

The accidental arrival of a damaged collection of British Columbian artifacts in New York allowed Boas to break Jesup's budget restrictions, although the president expressed surprise that the museum's Northwest Coast collections, among its strongest areas, should need supplementing. Boas assured him that it would be the easiest matter in the world to spend \$3,000 on that region (Boas to Putnam, 18 December 1896, HUA, Box 8). Since this area of the Kwakiutl [Kwakwaka'wakw], Bella Coola [Nuxalk], and Salish was his special interest, Boas was anxious to fill gaps. The salvage purchase was a mere tidbit. Boas had his eye on much more.

He realized immediately the value to research and collecting represented by the wealth of the AMNH's trustees and friends. Late in 1896, he drafted a letter to Henry Villard, sponsor of the museum's Peru and Bolivia expeditions, proposing that Villard, the former president of the Northern Pacific Railway and now proprietor of the *Evening Post* (and a fellow German American), contribute toward filling the gap. With several thousand dollars over the next two years, Boas wrote, the museum "should have the most thorough and I may say a complete collection from the region between Columbia River and Mt. St. Elias" (Boas to Villard, 23 December 1896, AMNH, Acc. 1897-30). The letter proved unnecessary. Jesup himself soon took up a much more extensive proposal for an elaborate exploration of the anthropological affinities between Asia and North America.²

Boas put this idea, which had matured for well over a year, before Jesup on January 19, 1897. Describing the question of the influence between Old and New World cultures as one of the most important problems of American anthropology, Boas proposed in his letter to Jesup a systematic ethnological and archaeological investigation of both sides of the North Pacific. (See Appendix A, this chapter). Fragmentary study, he wrote, had demonstrated the commonality of certain cultural elements in the two regions. Bows, body armor, and canoes, for example, had common features. The great

diversity of language along both coasts was striking, but since the languages on the Asian side were practically unknown, it was unclear whether there were any actual linguistic similarities. Particular points of mythological coincidence suggested early communication. Northwest Coast Indians physically resembled the Asians more than did any other American stock.

In short, there are so many points of similarity between the tribes of this whole region that we are justified in expecting that here a mutual influence between the cultures of the Old and of the New World has existed. Thus a foundation for the solution of this important problem with all its important bearings upon the ancient civilisation of America may be laid in this region. (Boas to Jesup, 19 January 1897, HUA, Putnam Papers, Box 16)

Conveying his ingrained sense of salvage urgency, Boas noted that everywhere, but especially on the Asian side, the culture of the people was rapidly disappearing “and the whole work is becoming more difficult from year to year.”³

Jesup’s imagination was struck by the great problem of Asian-American contacts. He “got very much interested in that question” (Putnam 1902) and, in his annual report written in January 1897 commented that “the theory that America was originally peopled by migrating tribes from the Asian continent” was a subject of great interest to scientists. Opportunities for solving this problem were rapidly disappearing, Jesup continued, and he then asked that friends of the museum contribute toward a systematic investigation of the problem (Jesup 1897:24-5).

Before there was an opportunity for a response to his appeal, Jesup himself jumped. On February 9 he told Boas that he wanted personally to take up the plan and asked for a detailed scheme for carrying it out. Boas was overwhelmed. “Mr. Jesup looks at the proposed expedition in the light that it will be *the greatest thing ever undertaken by any Museum* either here or abroad and that it will give the Institution an unequalled standing in scientific circles” (Boas to Putnam, 11 February 1897, HUA, Box 8; emphasis added).⁴ Thus

began the Jesup North Pacific Expedition to investigate affinities between the peoples of Northeast Asia and Northwest America.

Jesup’s move was not uncharacteristic. A self-made man of considerable wealth, generous with his time and money, he had always been sympathetic to grand designs and large-scale ideas: he had underwritten the Jesup Collection of North American Woods, some 10 years in acquisition, and the Jesup Collection of Economic Entomology and was now supporting the polar aspirations of Commander Robert E. Peary. Boas had put before him a vast project that promised to address the fundamental question of the relationship between Asia and aboriginal America. He accepted the challenge.

Jesup’s decision launched Boas into frenzied action. He visited Leonhard Stejneger, a Smithsonian naturalist familiar with the Siberian coast, in Washington, D.C., and wrote to some orientalists to ask about young men suitable for Siberian work. The matter was made all the more urgent because Jesup had seized on the expedition as a lever for securing another museum wing from the New York state legislature. The public announcement, made a little too hastily for Boas’ taste but dictated by the state assembly’s calendar, was released for March 12 newspaper editions. (Boas had to provide details and corrections to reporters over the next two days.) “The main object of the expedition is to investigate and establish the ethnological relations between the races of America and Asia, and is intended as a contribution to the solution of that question.” Field parties would work on the American West Coast, along the coast of the Sea of Okhotsk, and in the northern portion of the Bering Sea. “The expedition will be the greatest, it is said, in point of time spent and territory traversed ever backed by private individuals in this line of research” (*New York Times*, 13 March 1897, 2:5).

The roots of Boas’ intercontinental project, now Jesup’s, reached back to well before Boas’ employment in Jesup’s museum. In 1895 Boas had sounded

out people in Berlin about a prospective fieldworker and had then investigated, through Stejneger, transportation routes to Siberia's Amur River region (Stejneger to Boas, 16 November, 11 December 1895). An expedition, he told Berlin sinologist Wilhelm Grube, "had in the last year almost come to fruition twice" (Boas to Grube, reported in Boas to Laufer, 5 May 1896, AMNH, Acc. 1900-12). What Boas meant, at a time when he was without a position, is unclear, but he certainly foresaw an investigation of the relationship between Siberian and Northwest American groups.

During that same Berlin summer he had raised, more explicitly than ever, the question of the probable connections between Asian and American peoples. A number of complicated British Columbian myths, he told the Berlin Geographical Society, showed such similarity with Old World myths that a cultural connection between the two continents was very probable. The distribution of other phenomena, including physical type, pressed toward the same conclusion and made it probable that firm links between the cultural areas of both worlds would be found (Boas 1895b:266-70).

Boas' interest in the question of intercontinental relationships arose in large part from the publication that summer of his book *Indianische Sagen* (Boas 1895a). Breaking up myths into elements, he showed the mixture of these among the coastal and interior groups of the Northwest and traced some far beyond, to the Mackenzie and Mississippi River basins, the North Atlantic coast, and along the Arctic, to Greenland. The mythologies of the Northwest tribes also incorporated foreign elements from the Old World.⁵

According to a letter Boas wrote to a German editor in 1897, he had long collected collaborating data for the mutual influences of the coastal inhabitants of these areas. His reading of Georg Steller's 18th-century description of Kamchatka "transposes me almost directly into familiar Northwest American surroundings," but he had been especially struck by Grube's recent article in *Globus* on shamanism among the Nanay people of Siberia's lower Amur River (Boas to Andree,

4 May 1897, AMNH-DA, Jesup Ex. File). Some legends recounted there coincided almost exactly with those of the Northwest Coast, which, more importantly, were limited in North America solely to those coastal groups.⁶ Other data argued emphatically for an early influence on Northwest Coast cultures.

The Jesup Expedition would be pursued within the research strategy that Boas had now developed. This was to be an explicit demonstration of the efficacy of the historical method of anthropological research. "I believe," he wrote to *Globus* editor Richard Andree, "that our science urgently requires an investigation of the historical development of the cultures of primitive peoples in order to obtain a clear understanding of the laws of cultural development." The Jesup Expedition would cover an area "unusually favorable" for such a method since "the major influences have occurred along a direct coastline" (Boas to Andree, 4 May 1897, AMNH-DA, Jesup Ex. File). This would be an opportunity, Boas told Edward. B. Tylor, for a rigid adherence to the historical method, whose superiority over the comparative method he had recently asserted in a paper at the Buffalo meeting of the American Association for the Advancement of Science (AAAS). "I want to investigate the geographical distribution of certain customs and characteristics over continuous areas." The historical method meant that "we shall not obtain dazzling results, but I hope such as will stand the criticism of later times" (Boas to Tylor, 13 April 1897, Balfour Library, Oxford, Tylor Papers). The Buffalo paper, "The Limitations of the Comparative Method of Anthropology," had been a reassertion of Boas' decade-old point that generalization must come from careful investigation and induction, not from a priori assumptions. The method required a limitation to a restricted and well-defined territory, with comparisons that did not extend beyond the limits of the cultural area itself.

A detailed study of customs in their relation to the total culture of the tribe practicing them, in connection with an investigation of their geographical distribution among neighboring tribes, affords us almost always

a means of determining with considerable accuracy the historical causes that led to the formation of the customs in question and to the psychological processes that were at work in their development. (Boas 1896b)⁷

Boas' criticism was methodological and was concerned in large part with the weakness of the "comparative method" (Carneiro 1973; Leopold 1980; Stocking 1987), but he did mention the research area that he already had in mind. While no one believed that slight similarities between Central American and East Asian cultures were satisfactory proof of a historical connection, "no unbiased observer will deny that there are very strong reasons for believing that a limited number of cultural elements found in Alaska and in Siberia have a common origin" (Boas 1896b, 1940:277).⁸

Fieldwork in America's North Pacific Region

The first season's work of the Jesup Expedition would be confined to British Columbia in order to give Boas time to organize the Siberian work for the following year. He had already been planning a summer trip to the coast, partly in the museum's interest, partly to prepare a final report for the BAAS's Northwest Tribes committee. He had originally arranged for only a two-month trip, one month of which would be without museum pay, although with BAAS assistance.⁹ Now it became a four-month first field season of the Jesup North Pacific Expedition.

"I go west better equipped than ever before," he wrote before his May departure (Boas to parents, 9 April 1897). More money was part of it; so too was his new intimacy with the museum's collection. Equally satisfying was the presence of collaborators and companions who, though often pursuing their own assigned work, would be with him much of the summer.

He was mostly with Harlan Smith, the taciturn young man from East Saginaw, N.Y., whom he had known since the Chicago Fair. Smith was just 25. A boyhood interest in Indian remains had led him to Putnam and archaeology. Boas liked the bachelor archaeologist. "His heart is in the right place and he is absolutely

reliable," but he doubted that Smith would ever amount to much in archaeology. Although resourceful, clever, and good with his hands, Smith lagged behind in anything to do with real scholarship. The "many gaps" in his knowledge were obvious, his questions were "unbelievably simple," and he was unable to "see the connection between his work and the general broad questions of anthropology" (Boas to parents, 15 August 1897; F. Boas to M. Boas, 21 August 1897; Boas to Putnam, 10 April 1900, HUA, Putnam Papers).

A second companion was 30-year-old Columbia psychologist Livingston Farrand, who now lectured in ethnology as well. Farrand, totally inexperienced in fieldwork, wanted to apprentice with Boas and volunteered to go west at his own expense. That had not gone over well with Jesup, who, taking a "narrow-minded" view, wanted no outsiders on his great expedition (Boas to parents, 9 April 1897). Boas' long letter turned the situation, and although Farrand's field assignments were largely separate from his own, Boas found that Farrand's gaiety, unassuming naturalness, and good manners made him a pleasant companion (F. Boas to parents, 9 April, 27 May, 15 June 1897; reproduced in Rohner 1969:206).

The three New Yorkers arrived in British Columbia at the beginning of June and traveled immediately to Spences Bridge in the southern interior. There they rendezvoused with James Teit, the Scotsman whom Boas had first met in 1894. Teit had prepared things well, securing local Nlaka'pamux [Thompson Indians] for the physical measurements that Boas wished to take. While Smith went on to dig in Kamloops and Lytton (see Thom, this volume), Boas and Farrand, guided by Teit, began a long horseback trip northwestward along the Fraser River, across the Chilcotin plateau, and over the Coast Range to the Bella Coola [Nuxalk] on the Pacific. Farrand detached himself at Puntzi Lake when Boas decided that the Chilcotin were so interesting that they deserved a month of Farrand's time. The overland journey took 38 often unpleasant days: rain poured over the 10-horse pack train in the usually dry

interior, bogging down the horses. Rations seldom strayed from beans and bacon. Natives along the way were not keen to allow themselves to be measured. Only the beauty of the mountains and valleys made much of the journey rewarding.

Bella Coola, remote as it was, came as a relief. There Boas found a welcome bed at the home of John Clayton, a local storekeeper, and enjoyed the dietary change to fresh salmon. More important, George Hunt, Boas' collaborator from Fort Rupert, had done his advance work well. The two worked together every morning, going over the Kwak'wala texts that Hunt had been sending East, with the balance of the day spent investigating Bella Coola religious ideas.

Boas then went north to Port Essington on the Skeena River to measure, make casts, and identify museum pieces. There he met Charles Edenshaw, a Haida artist, and hired him to identify items from the museum's collection and to show him something of northern art and the basics of Haida ethnology. Boas then spent two weeks with Hunt among the Kwakwaka'wakw [Kwakiult] of Rivers Inlet. That concluded Boas' fieldwork. He met up with Farrand, and the two left for New York, while Smith stayed on with his excavations until winter rains drove him home in mid-November.

Boas was pleased with the season. They had made over a hundred plaster-of-paris facial casts and many more body measurements. Boas had enough information from Edenshaw to write the first contribution to the Jesup Expedition series, "Facial Paintings of the Indians of Northern British Columbia" (Boas 1898a), which enlarged on the place of geometric design in Northwest Coast decorative art. He had also corrected and revised over 300 pages of Hunt's texts and had gathered new material, all of which was published as "The Mythology of the Bella Coola Indians" (Boas 1898b), on the peculiar cosmology of that group. Farrand, unfortunately, "had not done *very* much" (F. Boas to M. Boas, 13 September 1897). The Chilcotin had been less than cordial, and Farrand had not been able to

find a good interpreter. His collection of legends, however incomplete, did show "a not very rich independent mythology, but a surprising receptivity to foreign influences" (Farrand 1900:4).

Smith's archaeological results seemed very important. The older shell mounds of the coast revealed a skull type resembling that of the interior, while yet older ones contained deformed skulls related to those of the Koskimo Kwakiutl. This seemed to indicate that at an earlier time a rather uniform population had prevailed along the coast from the Columbia River to northern British Columbia and that the various types now found on the coast stemmed from migration of Indians from the interior, with the earlier population prevailing now only on the Columbia River and northern Vancouver Island (Boas, unpublished lectures, February 1898:17; Beattie 1985).

Boas did not participate in the next two Jesup Expedition field seasons. Farrand returned to the coast in 1898 to investigate two Olympic Peninsula groups, the Quinault and the Quileute. Despite considerable disappointment, he collected enough to show a myth transition from the Northwest Coast toward the Chinook (Farrand and Kahnweiler 1902:79-80). In the same season, Smith made excavations in Puget Sound and at Lillooet and then continued his archeological work in 1899 on Vancouver Island. The results seemed to confirm an early migration from the interior to the coast and to Vancouver Island that carried with it the art of stone chipping and geometric decoration (Smith 1907:439).

Boas himself went west in 1900, the fourth year of the expedition. His field season in British Columbia was relatively simple: six days with Teit in the Nicola Valley and then two full months at Alert Bay.

Teit had proved to be the treasure that Boas had anticipated at their first meeting in 1894. At that time Teit, age 30, had been in British Columbia for 12 years. Raised in the Shetland Islands, he had left school at 16 and two years later had joined an uncle who ran a store in Spences Bridge. Teit was soon drawn into the

Native world: within three years of his arrival he was living with Lucy Antko, a Nlaka'pamux woman whom he officially married in 1892 (Wickwire 1993). He made his living by a variety of frontier occupations: packing, guiding, freighting, and serving as a big-game hunting guide. By the time Boas met him, Teit was already seriously studying the Indians around him. By 1900 he had finished, in addition to several small pieces, a volume containing Thompson [Nlaka'pamux] texts and a review of their ethnography, which was now in press as a Jesup Expedition publication (Teit 1898, 1900).

Boas' purpose in meeting Teit on this trip was largely to take anthropometric measurements of the Indians south of Spences Bridge. Boas—soon stiff and sore—rode on the horse familiar from the Bella Coola trek from village to village with Teit, then survived the eight-hour, 41-mile return to Teit's home. Furnished with only a table, two chairs, and a bed, the one-room cabin was filled with books about Indians and the Shetlands. "Mr. Teit can give us all an example of great industry and of the unassuming fulfillment of duty," Boas wrote his children (29 June 1900). After looking through Teit's notes, Boas boarded the train for Vancouver and then the boat to Alert Bay.

At Alert Bay he enjoyed comfortable accommodation with George Hunt's brother-in-law, the merchant S. A. Spencer, and had the daytime use of a small cabin where he could work with the Kwakwaka'wakw. He found a good interpreter in William Brotchie for the language and a painter to explain details of the art. Older men came by to tell him stories, and he sought out recipes and information on food preparation and medicines from the women. The sole difficulty was that Hunt was kept busy in Spencer's cannery, and so, for most of the time, he could help Boas only in the evenings and on Sundays.

It was during this Jesup Expedition period that the collaboration between Boas and Hunt solidified. Although Boas had worked with Hunt since 1888, particularly for the Chicago Fair and then at Fort Rupert in the winter of 1894–95, and Hunt had long been

sending Kwakwaka'wakw stories to Boas, the British Columbia Native had never gained his full confidence. Both at the Chicago Fair and at Fort Rupert, Boas had found Hunt difficult to deal with and too lazy to use his brain. In 1897, however, Hunt had come to Bella Coola and prepared things well for Boas' arrival. Boas did find Hunt unbelievably clumsy with the Rivers Inlet dialect of Kwak'wala, but he had time to improve Hunt's general orthography (Berman 1991; Cannizzo 1983; Jacknis 1991; Rohner 1969:183, 214, 211, 236).

The son of an English-born Hudson's Bay Company employee and his high-born Tlingit wife, Hunt grew up in Fort Rupert, where his father was normally the only white man. Although he could not necessarily consider himself Kwakwaka'wakw, he was raised almost as one. His knowledge of the Fort Rupert language needed little qualification. He was an initiate in the *Hamatsa*, the highest Kwakwaka'wakw dance society, he acquired shaman credentials, and he might have participated in a cannibal ceremony. For the latter he suffered a penalty: though he was acquitted of the charge, the trial cost him over \$400 (Cole and Chaikin 1990:73–5). He twice married high-born Kwakwaka'wakw women and raised his large family within Indian society.

By 1900, Boas was satisfied with Hunt and his command of language and tradition. His experience with him that summer, when he was able to check Hunt's versions against Brotchie's, confirmed Hunt's ability. "I find him quite dependable, more than I had thought" (F. Boas to M. Boas, 16 August 1900). While retaining reservations about Hunt's linguistic idiosyncrasies, his tendency toward a formal style, and his command of Kwak'wala grammar, Boas felt confident with Hunt's material (Berman 1991:27–36). Hunt would continue to send texts to Boas for the rest of his life.

Boas left Alert Bay and British Columbia satisfied. He had a much clearer understanding of the "terribly difficult" Kwak'wala language and was now, after working with Hunt in 1897 and again in 1900, in a position to publish many of the texts he had been collecting

for six years. He thought he also had enough material for a detailed description of the manners and customs of the Kwakwaka'wakw. "That," he wrote, "will make a very peculiar cultural picture" (F. Boas to S. Boas, 16 August 1900).

Boas' 1900 trip was virtually the last on the American side of the Jesup Expedition. Hunt and Teit would work in their own areas over the next two years, but the only visitor was John R. Swanton, whom Boas had assigned to the Queen Charlotte Islands. Swanton was a Putnam student, a well-trained Harvard Ph.D. who had studied linguistics under Boas at Columbia. Swanton worked for the Bureau of American Ethnology, which paid his salary on this trip while the AMNH paid expenses. He was instructed to study the Haida language, religion, and social organization while he collected specimens for the museum (Boas to Swanton, 5 June 1900, AMNH, Acc. 1901-31). He left much of the artifact collecting to the Victoria physician turned museum collector C. F. Newcombe so that he could concentrate on language, mythology, and ethnology.

Fieldwork in Siberia

The Asian side of the expedition was more difficult to organize. Boas had had one man, Berthold Laufer, in mind for the southern portion of the work since 1895, when Grube had mentioned his name to Boas as a promising young scholar. Laufer, son of a Cologne confectioner, was nearly finished with his degree and came with strong recommendations from Leipzig and Berlin, where he had studied Eastern languages, religions, and cultures. He had, moreover, sat in on lectures by Berlin anthropologists Adolf Bastian, Felix von Luschan, and Eduard Seler. Unfortunately, Laufer still had before him his military obligation. Boas, even though he as yet had no expedition arranged, suggested that Laufer complete his service as soon as possible so that he would be available should a Siberian worker be required. Laufer did so, receiving his degree, magna cum laude, while in the army. Formally appointed in May 1897, he came early next year to New York to prepare

for his Siberian work. In March, just as he was scheduled to depart, the museum received word that his visa had been refused by the Russian Interior Ministry (see Vakhtin, this volume). Laufer was a Jew, and Jews were not allowed into Siberia [by the Russian government—ed.].

It was all very difficult and embarrassing. Boas had just arranged a large farewell reception for the traveler, and Laufer might never be able to leave. Working with urgency, Boas went to Washington to meet with officials at the State Department, where, in Jesup's name, he pulled all the possible strings. He touched base with Andrew White, the U.S. representative in Berlin, but first reliance was put on the American minister to St. Petersburg, Ethan A. Hitchcock, who spoke with the interior minister. The minister, Ivan Goremykin, remained immovable, replying, in every instance, "simply that it was against the law to grant such request—Dr. Laufer being a German Jew—who were prohibited from entering Siberia." Vasily V. Radloff of the Imperial Russian Academy of Sciences accomplished what diplomats could not. He called on Grand Duke Constantine, who served as president of the academy, and on the governor of Siberia, then in the capital. Suddenly, word reached New York that Laufer had, by special permission of Tsar Nicholas II, been authorized to visit Sakhalin and the Amur River (Zvolianski to Olarovsky, 12 March 1898, AMNH-DA, Jesup Ex. File; Hitchcock to Jesup, 4 April, 23 April 1898; G. Dewollant to Jesup, 26 April 1898). Laufer was aboard the next steamer. He arrived in Yokohama on May 23.

Accompanying Laufer was an archaeologist, Gerard Fowke. Fowke was one of Putnam's unschooled protégés, although he had most recently worked for W. H. Holmes and the Bureau of American Ethnology in Washington. Already in his forties, he had been digging mounds and other sites in the eastern United States for over a decade. The two men were a mismatch: Fowke, the unrefined American outdoorsman, almost 20 years older than Laufer, with scant university training; Laufer, the aesthetic, urbane, and

scholarly European (Ohio Archeological and Historical Society 1929). Laufer, Fowke judged, was a “book-student, 25 years old” with “no *practical* sense, but any amount of *theoretical knowledge*”—“Can’t tie a string, drive a nail or whittle a stick; hell of a man for a wilderness trip!” (Fowke to W. H. Holmes, 5 March 1898, NAA, Folder 44).¹⁰ Fowke’s attitude carried on into Siberia. “Laufer is a good fellow,” Fowke told Boas, but, as a fieldworker, “he is *helpless*.” That tone enraged Boas, who was partial to the young German. Fowke had been sent to work with Laufer, not to sneer at him. Even more, Fowke’s archaeology had been, on his own admission, a “dismal fizzle.” He found nothing on the Amur River, complaining that the banks were too densely covered with vegetation to dig and that the river had constantly changed its course. Boas was disgusted but recommended that Fowke remain in Japan for three months of excavation on shell heaps. Even that hope of salvaging something from Fowke’s expense was a failure (Fowke to Boas, 15 September 1898, AMNH, Acc. 1900-17; Boas to Fowke, 12 September 1898; Fowke 1899; Boas to Jesup, 19 January 1899, AMNH, Jesup Ex. File).

Laufer attributed the difficulty to Fowke’s unwillingness to adjust. “As a true American he cannot and will not set himself into the new Russian relationships and rejects everything that comes his way.” Laufer was certain that, with energy and concentration, things would be found on the Amur River (Laufer to Boas, 4 March 1899, AMNH, Acc. 1900-12).

While Fowke was dabbling on the Amur River and then in Japan, Laufer spent eight months, from July 1898 to March 1899, on the east coast of Sakhalin Island working among the Nivkh, Tungus [Uilta—ed.], and Ainu peoples. Field conditions were difficult; travel was by horseback, reindeer sledge, and dog sled; and for two and a half months Laufer was ill with influenza that turned to pneumonia. Worse yet, he could find no interpreter for his ethnological work: no Nivkh knew more than the most common Russian phrases, and the Ainu were not very familiar with Japanese. Having traveled

down the east coast of the island, he returned north to Nikolayevsk in time to cross the ice to the mainland before the spring breakup. Here he settled at Khabarovsk on the Amur River to study the Nanay, with whom Grube had also worked. With the spring thaw, he traveled downriver, stopping at various Nanay and Nivkh villages until he reached the river mouth in August. By October he had finished the season, traveling over Vladivostok to Yokohama, where he spent the remaining weeks of 1899 packing his collection before sailing for New York (Boas 1903:93–8).

Boas found Laufer’s huge assemblage of art and artifacts exceptionally interesting. So too was Laufer himself. Looking forward to Laufer’s February arrival, Boas confessed, “I take a great interest in the young fellow as if he were my own young brother.” Once in the city, Laufer became the Boases’ frequent guest, often for dinner twice a week. “It is amusing,” Boas commented, “to see how my earlier feelings return with this young fellow. He told me today that he wanted to tear up all his Siberian work and begin it all over again.” That, Boas observed, was just the same as he had been with his Baffin Island research. (F. Boas to S. Boas, 12 January, 20 February 1900).

Laufer’s Siberian difficulties paled before those of the Jesup Expedition’s northern researchers. Boas had had problems even finding someone for the job. He had initially been in touch with Freiherr Erwin von Zach, an Austrian studying in Leiden.¹¹ Boas was impressed by his credentials and engaged him in May 1897, only to have the arrangement collapse in August. There were doubts about von Zach’s ability to endure Siberian hardships, but Boas blamed Leiden museum director J. D. E. Schmelz for the Austrian’s withdrawal (unknown correspondent to Boas, 21 September 1897; F. Boas to M. Boas, 21 August 1897). Boas then fell back on Vasily Radloff, who was later to help with Laufer’s visa problem. Radloff recommended two experienced Siberian fieldworkers, Waldemar Jochelson and Waldemar Bogoras (Radloff to Boas, 23 May 1898, AMNH, Acc. 1901-70). In the summer of 1898, Boas

met in Germany with Radloff and Jochelson and confirmed arrangements for the two Russians, who after making equipment purchases, sailed to New York to secure Boas' instructions and receive tutoring in anthropometrics (see Vakhtin, this volume).

Boas found them "very curious" men, "so different" in personality from western Europeans. Marie did not particularly like either, in part because they kept Franz until late in the evening and everything was put on hold at home "until the Russians go" (F. Boas to S. Boas, 6 March 1900; M. Boas to S. Boas, 23 March 1900). The Russians left for San Francisco in late March 1900, sailing then to Nagasaki and finally to Vladivostok.

Siberia was familiar territory to both Jochelson and Bogoras. Their experience there was initially as political exiles, and their friendship was cemented in a common attachment to *Narodnaia volia* (Peoples' Will), a radical populist group that did not shun violence. Both used their exile to study the local indigenes—avocations that became a profession. Jochelson, the elder of the two, had spent three years in isolated confinement before being transferred to Yakutsk and then to the mouth of the Kolyma River on the Arctic Ocean (F. Boas to S. Boas, 27 August 1903). He then worked with the Yukagir with the Imperial Geographical Society's expedition. Bogoras, with the Sibiryakov Expedition, did research on the Chukchi, which he was now seeing through publication. At the time of his engagement, Jochelson was registered for a doctoral program in Switzerland, where his wife, Dina Jochelson-Brodsky, was studying medicine, but he was willing to interrupt his work, and his wife's.

The Jochelson-Bogoras expeditions can only be described as heroic. Arriving in Vladivostok in May 1900, the party split. The Bogorases went to Mariinsky Post on the Anadyr River, the most remote Russian settlement in Northeast Asia, to study the Reindeer and Maritime Chukchi and the Asiatic Eskimo [Yupik]. Mrs. Bogoras remained there while Waldemar Bogoras traveled to the Sea of Okhotsk to meet Jochelson. There he lent his Chukchi linguistic ability to studying the

language of the Koryak, a related group. After their midwinter work among the Kamchatka Koryak, Bogoras left on his own for the west coast of the Kamchatka Peninsula to collect material from the Itelmen [Kamchadal] and then, after more study of the Chukchi and Yup'ik on the Chukchi Peninsula, traveled to St. Lawrence Island in the Bering Strait. He returned to Anadyr by Native boat, a voyage of 28 days, to meet Mrs. Bogoras, who had remained there to make collections along the Anadyr River valley. They left in August for Vladivostok by steamer and, after shipping their collections to New York, returned to St. Petersburg by rail. Illness delayed their departure to New York; the couple arrived there only in April 1902.

Jochelson and his wife Dina had an even more difficult trip. Half the winter was spent among the Maritime Koryak in underground dwellings filled with smoke, stench, and lice. The other half was spent among the interior camps of the Reindeer Koryak in bitter cold. They had had to search out the Koryak, who had fled to the mountains to escape an epidemic. That necessitated a difficult trek by horse across the boggy tundra. Summer boat trips to Tungus [Even] and Maritime Koryak groups were accompanied by privation. The Jochelsons stayed on, as planned, for another year to study the Yukagir of the Kolyma region.¹² That required a difficult 56-day trip across unmapped mountains to famine-plagued villages, then on to Yakutsk before returning to St. Petersburg via Irkutsk in the summer of 1902. They had traveled some 8,000 miles by foot, sled, boat, or horse.

The research portion of the expedition ended in 1902, although Boas sought to fill in and round out parts of it after that. Hunt continued to work on the Northwest Coast, gathering texts and other information and collecting objects for the museum not only from the Kwakwaka'wakw but also from the Nuuchah-nulth [Nootka]. Teit labored on among the interior groups, collecting material for later volumes on the Lillooet and Secwepemc [Shuswap] and on Thompson myths. Otherwise, field activities for Jesup's great

expedition were over when the Jochelsons arrived in Irkutsk late in the summer of 1902 (Boas to Jochelson, 5 December 1898, AMNH-DA, Jesup Ex. File; Boas to Jochelson, 24 March 1900, AMNH, Acc. 1901-70).

Working Up the Results

Boas wished for more. He requested money to pay a missionary for work among the Nuu-chah-nulth [Nootka] and, in 1905, money for Jochelson to visit the Asian Eskimo and Aleut in order to follow up “fundamental questions” raised by his earlier work. Boas also sought an appropriation for research to investigate his theory that the Tsimshian were recent arrivals on the coast (Boas to Bumpus, 22 December 1902; Boas to Jesup, 25 November 1905, AMNH, File 293). He was unsuccessful in securing funds for any of these projects. Jochelson, however, independently succeeded in his Aleutian ambition, with the [Russian-funded—ed.] Riabushinski Expedition to the Aleutians and the Kamchatka Peninsula in 1909–11.¹³

Long before then, Boas had become disillusioned with Jesup and the AMNH. The Jesup Expedition was but part of Boas’ grandiose ambitions for anthropology in New York, and things in the museum were not as they should be. He had problems about his own status and salary and about museum assistance, his “Vanishing Tribes” of North America was underfunded, and his East Asiatic project had failed. The enormous effort he had to spend on installation, labeling, and cataloging, in addition to his teaching responsibilities at Columbia University, meant that he made little progress on his own scholarly work. His dissatisfaction grew as research support stagnated or declined. Things were going backward, with less done daily, he wrote, yet the material was disappearing “day by day.” “I have capacity for work, but am dissatisfied at frittering away my energies in vain attempts to reach a settled policy of work to be pressed. If the Museum cannot assist me in these plans, my interest lags.” While his dissatisfaction included the lack of support and planning in the museum, the Jesup Expedition publications

lay “especially on my heart” (Boas to Jesup, 9 January 1902, AMNH-DA, Reports File; Boas to Bumpus, 21 February 1902, AMNH-DA, Bumpus File; F. Boas to S. Boas, 28 February 1902).

The Jesup Expedition memoirs had been ambitiously projected at some 30 contributions in 12 volumes. Many of those from the American side were prepared quickly. An album of photographs, Farrand’s paper on Salish basketry designs and on the Chilcotin and Quinault, Teit’s Nlaka’pamux ethnology, Smith’s work on British Columbian archaeology, Boas’ facial paintings and Bella Coola myths, and his and Hunt’s first Kwakiutl [Kwakwaka’wakw] texts were ready by the beginning of 1902. Still to come were further reports by Smith, Teit’s Lillooet and Shuswap [Secwepemc] ethnologies and Thompson [Nlaka’pamux] texts, several volumes of Kwakiutl work, and Swanton’s Haida ethnology and texts. For the Asian side, Laufer had completed his slender volume on Amur decorative art, but Jochelson and Bogoras were, after their arrival in New York in 1902, only beginning their writing.

Publication costs had never been included in the expedition budget, although Jesup agreed to finance the first set of publications, at a cost of \$2,000. Boas feared that without a special appropriation, the museum’s limited publication budget, which had to cover all competing departmental requests, would hopelessly delay the dissemination of his valued results. In February 1902 he pleaded with museum director Hermon C. Bumpus for extraordinary money. “The danger is again imminent that the whole enterprise, the appreciation of which has constantly increased as its publications progressed, will fall flat.” He found it unbearable to think that the Jesup Expedition should be another example of an enterprise started with great vigor but ending in disappointment. He wanted a decision, once and for all. His estimate of costs was \$20,000 (Boas to Bumpus, 21 February 1902, AMNH-DA, Bumpus File). Boas got some of what he demanded. Jesup agreed to finance the expedition publications then in preparation, at an estimated cost of \$4,425,

should museum funds be insufficient. This was a relief, but, all in all, Boas wrote in July 1902, it had been a bad year: "nothing has worked out—or only a little" (Bumpus to Jesup, 19 May 1902; Jesup note, 19 May 1902, AMNH, File 293a; F. Boas to S. Boas, 2 July 1902).

Worse yet, relations between President Jesup and Curator Boas were becoming tense. Jesup now seemed disappointed with his expedition, acting as if "nothing will come of it." He was reluctant to agree to new plans before results were complete, something Boas regarded as nonsense. With this went Boas' growing view that there was "a minimal understanding for actual scientific work" in Jesup's museum. Then, in early 1903, the president changed his mind on the Jesup Expedition publications: in future they would have to be paid for from the museum's general publications fund. Boas was devastated. "It was perhaps a harder blow than all those that I have received in recent years" (F. Boas to S. Boas, 4 September, 5 September 1902; Boas to Jesup, 20 February 1903, AMNH, File 293).

Boas pleaded with Jesup to reverse a decision that would reduce the publication program to a role entirely out of keeping with the work accomplished. His whole scientific reputation, he said, was at stake, and "I cannot afford to have an enterprise for which I have the responsibility, fail." He had done his part, and now he asked Jesup "to see me through, that I may come with honor out of the undertaking." Jesup remained immovable. Boas had not told him at the beginning about the large sums required for publication. The expedition was over, and it was for the museum to see to publishing the results. He would allow enough money in the museum budget to keep the publications in progress, but no more. "All is now being done," wrote Bumpus to Jesup, "that is imperatively necessary." At least, said Boas at year's end, the publications go on (Jesup to Boas, 24 February [1903; microfilmed as 1900]; Bumpus to Jesup, 28 April 1903, AMNH; F. Boas to S. Boas, 23 December 1903).

Boas made things somewhat easier by cutting costs. He had long thought that the museum was

paying too much to publish its memoirs. He suggested that instead of the museum acting as its own publisher, the memoirs go to E. J. Brill in Leiden (Boas to Bumpus, 20 February 1903, AMNH, File 293). Bumpus followed up the suggestion, and future volumes were published by that house, with G. E. Stechert & Co. acting as American agents. The contract cut costs substantially (Boas to Winsor, 28 July 1905, AMNH. File 1905:B).¹⁴

Jesup's reluctance to expedite publication stemmed in large part from the accumulating costs of his expedition. One thing after another had contributed to overruns. Boas' initial estimate had projected the expedition costs at \$5,000 a year over six years, a total of \$30,000 from Jesup's pocket. In his haste to prepare the proposal, Boas had assumed that the museum and not the expedition would bear transportation expenses. He had also not realized that salaries of museum staff, such as himself and Smith, when in the field, would have to be borne by the expedition's budget and not by the museum's. These costs upset budget projections. Then the engagement of Bogoras and Jochelson brought an embarrassing crisis. Boas had expected to employ young men, like Laufer, just out of university. The two experienced Russians would do the work much better than untried newcomers, but they were much more expensive. Jochelson and Bogoras were, at ages 45 and 35, mature scientists who deserved long-term contracts and salaries commensurate with their standing. That meant \$1,200 a year, compared with young Laufer's \$500. It was all very embarrassing for Boas, but Jesup agreed to proceed with the Russians despite the enormous overrun his expedition was suddenly facing (see also Vakhtin, this volume). Boas now expected that work on the Asian side alone would cost \$27,667, with the entire expedition running to almost \$50,000, excluding publication costs (Boas to Jesup, 2 November, 18 November 1898, AMNH-DA, Jesup Ex. File; Boas to Putnam, 1 December 1898, AMNH-DA, Putnam File). "The whole thing is somewhat unpleasant," he confessed, "since it appears as if I made

a false estimate, though I can show Jesup where and how the large expenditure comes." Jesup complained in 1900 that he could not keep the business part of the expedition in his head: "I only know I am advancing a pile of money in this affair & time will prove the success of it." By 1901, Jesup's obligations, not including publications to date, were already \$53,470. Boas was estimating that the cost, including publications, was likely to be \$75,000; he later raised it to \$100,000 (Boas to Papa [Meier Boas], 31 October 1898; Jesup to Winsor, 19 July 1900, AMNH-DA, Jesup Ex. File; Winsor to Jesup, 1 April 1901, AMNH-DA, Jesup Ex. File; Boas to James H. Lamb Co., 9 November 1900, AMNH-DA, L File; Boas 1910b).

The toll, financial and personal, continued to mount as relations turned sour. When Jesup made remarks critical of the expedition, Boas was outraged: "Seldom do I get excited in conversation," he wrote, "but I became quite angry, so much so that it was difficult for me to remain within the borders of propriety." In Boas' mind, Jesup's intention was to restrict his obligation so that he would "not have to put out money for publications" (F. Boas to S. Boas, 26 November 1903).

Printing was not the only continuing cost. Bogoras and Jochelson had been contracted to write up their results at a monthly salary of \$150 each. For over a year, they worked at the museum. An attempt to get them fellowships with the Carnegie Institution failed, and both returned to Europe in 1904, their contracts altered to \$150 per chapter. Jochelson settled in Zurich, where his wife was completing her medical training; Bogoras went to St. Petersburg.

Before their return to Europe, Boas had seen them frequently, and both spent a good deal of the summer of 1903 with him at his Lake George retreat. Boas revised his earlier ambiguous opinion of Bogoras, "who became very attractive upon longer acquaintance." He was a man of fine sensitivity, intelligence, and enthusiasm, Boas wrote, and his whole life and aspiration were directed to political ideals, a drive to implement them and, if necessary, to sacrifice for them. Jochelson, too,

became likable on closer acquaintance. He went out every day to pick up the newspaper because, as Jochelson himself said, "In Russia the unexpected may happen at any time and I think that any day a constitution could be promulgated" (F. Boas to S. Boas, 26 October 1902, 9 October 1903; F. Boas to S. Boas, 27 August 1903).

Jochelson's writing was slow but regular. Bogoras, caught up in revolutionary 1905 St. Petersburg, stopped his entirely. For long periods, he ceased even to write letters. "I have had nothing from Bogoras for a month," Jochelson wrote Boas, and "that concerns us very much." Boas finally received a letter from Bogoras that excused his neglect. "But you will understand that an epoch like this happens only once in many centuries for every state and nation and we feel ourselves torn away with the current even against our will." Boas lectured him about priorities: "If events like the present happen only once in a century, an investigation by Mr. Bogoras of the Chukchee [Chukchi] happens only once in eternity, and I think you owe it to science to give us the results of your studies."

April brought another long silence. Boas was again concerned, especially because he had read in the paper that Bogoras had been arrested but then released. Boas' worry was not merely for the man's safety. "During the present excitement in Russia I am sure he will not give any time to his scientific situation." Boas would have liked to have had him out of Russia so he could concentrate on his work. A letter in November from Bogoras brought renewed regrets at the lack of progress but no change of mind. "Events that are going on in Russia request from all citizens their best attention and ability." Things were so dreadful, victims so numerous, that he felt no right to retreat from the struggle. At 40, he had time ahead to finish all yet to be written. He would have to be forgiven: "my mind and soul have no free place to let in science." On December 4 Boas received a cable from Moscow that Bogoras had been arrested. He wired St. Petersburg, asking Radloff's assistance in securing the revolutionary anthropologist's

release. Slowly the details came out. Bogoras had been arrested as a participant in the Peasants' Congress but had been released on bail after two weeks. He had returned to St. Petersburg and had then gone on to Finland, where he gradually returned to his scholarship. Jochelson, too, was affected by the revolution, and, lacking "the necessary calm," his writing slowed. Even from afar, Russia's internal turmoil had an upsetting influence. "You know, of course, that next to the researcher stands in me a citizen" (Jochelson to Boas, 7 March 1905, AMNH-DA, Jochelson File; Bogoras to Boas, 6 April 1905; Boas to Bogoras, 22 April 1905, AMNH-DA, Bogoras File; Boas to Jochelson, 28 September, 13 October 1905; Bogoras to Boas, 23 November 1905, AMNH-DA, Bogoras File; Jochelson to Boas, 7 March, 10 June, 8 May, 29 August 1905, AMNH-DA, Jochelson File).

If 1905 was a memorable year for the Russians, it was also for Boas. The previous summer, he had traveled to Europe, where he had an opportunity to consult with E. J. Brill about the Jesup publications, to visit Stuttgart for the 14th Congress of Americanists (1904), and to meet there with Jochelson, Bogoras, and others. The last day of the congress was largely taken up with papers on the Jesup Expedition from Boas and Jochelson and a complementary one from Leo Shternberg. "I presided that day," Boas wrote the museum's director, "and feel very well satisfied with the reception that the works of the Expedition received." On his return to New York, however, he determined that he could no longer carry on both his museum and university responsibilities. "I simply can no longer fill both posts" (Boas to Bumpus, 30 August 1904, AMNH, File 293; F. Boas to S. Boas, 25 October 1904).

Much as he was attached to the museum projects he had initiated, and no matter how integral the museum had become to his teaching program, the institution had lost its allure. The prospect of meaningful activity there was hopeless. He no longer had faith in Jesup. The parting was complicated, and in the end Boas angrily resigned from the museum in April 1905,

but with continuing responsibilities for the Jesup Expedition publications. Difficulties between Boas and Director Bumpus, however, required a more precise delineation of Boas' role and led to an even greater breach in the strained cordiality between Boas and Jesup.

Boas insisted that payment to him, irrespective of the published amount, should never fall below the \$4,000 he had counted on as his annual museum remuneration. This insistence touched a sensitive Jesup nerve. All Boas' previous appeals had been expressed, the museum president noted, as concern for the means to sustain his scientific work and for funds to support his scientific reputation. The tone had altered, and Jesup expressed his great disappointment at "the present condition of an enterprise that has involved expense and anxiety out of all proportion to the representations that were originally made." Jesup was confident that he had himself always acted with "the utmost liberality and fairness" and felt that Boas was not now living up to his commitment. He felt sorrow at "the many disappointments that have come to me in connection with this expedition" (Jesup to Osborne, 30 April 1906, AMNH, File 293b).

The final agreement contracted Boas to complete the expedition series by 1911 for a stipulated payment per published signature, the total cost not to exceed \$25,000 (Agreement of 31 May 1906, signed by Boas on 8 June 1906). Boas, for his part, was scornful of the whole business. The contract, he wrote, "is like that for building a house; goods to be paid on delivery, and the shoddier my work, the better financially for me! True Bumpus-Jesup style" (Boas to Putnam, 23 June 1906, HUA, Putnam Papers, Box 14).¹⁵ The new arrangement might have expedited publication—all involved were now being paid according to results—but it did not.

Expedition Publications' Later History

Relations between the AMNH and Boas were chilly, even frigid, after 1906. His difficulties with the museum had destroyed his desire to get on with the Jesup

publications. Two volumes were about to appear, but there would follow a long pause, since he had done no work for two years. "The fault lies in the obstructionism in the museum" (F. Boas to S. Boas, 23 June 1910). Indeed, "if I could do so in a way consistent with my scientific commitments, I would simply dump the whole Jesup Expedition and concern myself no further with it" (F. Boas to S. Boas, 18 March 1909).

But he could not drop it; he had too much invested and too many commitments to it. The material from the Russian side came in fitfully, and Boas worked on it, sometimes just as fitfully. Despite delays, some of the Russian material was so extensive that Boas had to find outlets beyond the restricted confines of the Jesup Expedition Series, under the AMNH Memoirs. Bogoras was certainly the most productive. His Chukchi ethnology had come out in three installments by 1909; the Chukchi mythology was published in 1910 and the Siberian Eskimo [Yup'ik] folktales in 1913. His Koryak texts and Chukchi grammar were essentially complete by 1914. Jochelson's Koryak ethnology was in print by 1908, but his Aleutian-Kamchatka expedition of 1909-10 delayed his work on the Yukagir volume. The most remiss was Shternberg, who had been added belatedly to write on the Amur River groups he knew from exile and expeditionary study. He did send the first part of his manuscript to Boas in 1912, but even it was never published (see Kan, this volume).

Then the outbreak of World War I [in 1914] made communications between New York and Russia almost impossible and severely interrupted mail to and from E. J. Brill, the Dutch publisher. The AMNH extended Boas' contract to 1916 and then again. The Russian Revolution and its aftermath disrupted things even further. Boas' contact with his Russian collaborators was reestablished only in September 1921. Boas gathered food and clothing in New York for Jochelson, Shternberg, and Bogoras, and the latter two were given \$300 toward their work. The following year, the Jochelsons came to the United States, where their scholarship was supported by the AMNH, the Carnegie Institution, and

private assistance arranged by Boas, largely through financier Felix Warburg. During this time, Jochelson was able to publish part of his Aleutian Islands archaeology (Jochelson 1925), to see his Jesup Expedition Yukagir volume through the press (Jochelson 1926), and to write a handbook, *Peoples of Asiatic Russia* (Jochelson 1928), for the AMNH. Mrs. Jochelson was given money and space in the museum to continue her anthropometric work, although no publications seem to have resulted. [Dina Jochelson-Brodsky's manuscript, "On the Anthropometry of the Native Peoples of (Northeast) Siberia," was prepared for the Jesup Expedition Series as Part 2 of Volume 11 but was never published; see also Krupnik, this volume—ed.]

The war and postarmistice conditions in Europe absorbed a great deal of Boas' attention and robbed him of scholarly concentration. Like Bogoras and Jochelson, he could not sever himself from political concerns, as a patriotic American with strong German sympathies and commitments. The Jesup publications limped along, hampered by war, revolution, and reconstruction and squeezed in among Boas' many other concerns, none of which included the writing of a concluding volume.

When Jesup died in 1908, his widow expressed a wish to see the final volume soon, but Boas was uninterested. "I have sworn to myself that I will not write the volume until all material is published" (F. Boas to S. Boas, 9 July 1909). It is doubtful that by 1909 he was any longer committed to writing it. He could maintain a workman's duty to scientific responsibilities, but his passions were elsewhere.

Such a project, moreover, ran against Boas' temperamental difficulty with the sustained treatment of the broad sweep. At least as much of a factor was his deep hostility to the AMNH, which endured beyond Bumpus' departure and Jesup's death. This combination of temperament and hostility was enough to prevent the completion of a summary volume, but the delayed Siberian results allowed Boas to procrastinate. As his other commitments

multiplied, the nonappearance of a fitting conclusion was almost predetermined.

Evaluation

Assessment of Boas' Jesup North Pacific Expedition is difficult. The research was never as complete as Boas would have wished, and new problems arose that could not be explored. The results were never fully published, introducing another complication. Moreover, evaluation must tread the fine line between legitimate historical perspective and superficial hindsight.

The Jesup North Pacific Expedition was, in many ways, two quite different projects: a North American one, and a Siberian one (Krupnik 1996). On the American side, the expedition can be viewed as a well-endowed continuation of Boas' previous research. AMNH support and Jesup's money allowed Boas to add archaeology to his research tools; otherwise the American work was an extension of his previous methods and strategy. "I am going to continue my previous work without practically changing my plans at all," he told W. J. McGee in 1897, "but since I have ampler funds than heretofore, I shall be able to work to better advantage" (12 April 1897, NAA-BAE).

His old collaborators, Teit and Hunt, went on in much the same way as they had before the Jesup Expedition and as they would continue to do after its close. Research concentrated on Boas' Central Coast and southern interior interests, stretching only slightly northward to include the Haida and, quite superficially, the Chilcotin, Quinault, and Quileute to the south. The areas touched on lightly by the expedition—those of the Nuu-chah-nulth, Quinault, Quileute, Tsimshian, and Southern Athapaskan groups—were those on which he had done little or nothing before 1897.

But most serious was the neglect of Alaskan groups. The Alaska Eskimo and Aleut had earlier been designated as part of the expedition, but no research appropriation was listed beside them (see Fig. 3; Boas to Jesup, 2 November 1898, AMNH-DA, Jesup Ex. File). The justification for the omission was that accounts of

other investigations among these groups were accessible (Boas 1901:357, 1908:1298). The reference presumably was to Smithsonian work, probably to W. H. Dall's work on Alaskan groups, especially the Aleut, in the 1870s; more certainly to John Murdoch's work in the 1880s on the Point Barrow Eskimo (Murdoch 1892); and, most importantly, to E. W. Nelson's then unpublished study of the Bering Strait Eskimo (Nelson 1899). Boas did seek some "ancient" Alaskan Eskimo material, especially skulls and bones, from Captain Minor Bruce in 1899 and bought part of his existing collection (Boas to Bruce, 1 April 1899, AMNH-DA, Jesup Ex. File; AMNH, Acc. 1899-13). In 1901 Boas expressed the hope that it might still be possible for the expedition to do a systematic investigation of prehistoric sites along the Yukon River and the neighboring coastland in order to discern whether a pre-Eskimo culture or type existed in the area (Boas 1901). By then, however, the expedition was all but over, and Jesup was unwilling to extend its scope.

Essentially, however, Boas did not consider the Eskimo to be part of the Jesup Expedition problem. The Siberian Eskimo [Yupik] were themselves interesting, and Boas asked Bogoras to survey them and make collections from among them, but only if the opportunity offered, since they were "not primary objects" of the expedition (Boas to Jochelson, 26 March 1900, AMNH, Acc. 1901-70). In all this, there is a consistent lack of interest in the Eskimo. At the AMNH, Boas continued his interest in the Eastern Canadian Inuit that had been his first love, working with visiting Labrador and Greenland Natives and using his old friends George Comer and James S. Mutch to gather material, but he never seriously considered using the Jesup project to study the place of the Eskimo and Aleut in connections between Siberia and North America.

The Indians of southern Alaska had been included in the initial plans, with Boas apparently intending to do the work there himself (Boas to Jesup, 19 January 1897; Boas to Putnam, 11 February 1897, HUA, Putnam Papers). In 1898 Fowke was to do archaeological

excavations in northern British Columbia and southern Alaska, but he was dispatched to Siberia instead (Boas to Fowke, 7 April, 11 April 1898, AMNH, Acc. 1900-17). There were few accounts of the Tlingit except for a limited yet very good one by Boas' old Berlin friend Aurel Krause (Krause 1885). The museum did have "a mass of manuscript material" on that southeastern Alaska group, but it belonged to G. T. Emmons and was not accessible even to Boas. Emmons seemed "to know a great deal," and his manuscript would ultimately become the museum's property, but Boas knew, or soon came to think, that he could provide information only on "industries and history" and little pertaining "to their arts or to their inner life," let alone anthropometrics, linguistics, or even mythology. Yet Boas did not "feel like spending money in that country as long as this work has been done" (Boas to Swanton, 4 April 1901, AMNH, Acc. 1901-31; Boas to Bumpus, 11 November 1903, AMNH-DA, Bumpus File; Boas to Farrand, 20 June 1903, AMNH-DA, Farrand File).¹⁶ A factor in the neglect of the Tlingit may simply have been that the museum already had rich artifact collections from that group. The same was true of the Alaska Eskimo, but the main reason for their omission was that Boas thought the Eskimo a late arrival in the area and thus irrelevant to ancient North Pacific problems.

The American research itself, then, was very uneven. The published results form no coherent corpus. Boas' facial painting piece (1898a) was entirely concerned with problems of decorative art, something that was then a major concern of his. His Bella Coola mythology (1898b) did attempt to place that anomalous Salish-speaking group within its central coastal relationships, but it was almost as much a methodological study on acculturation and diffusion, and it led nowhere near intercontinental relationships. The Kwakwaka'wakw texts he published with Hunt were enduring salvage contributions to the primary materials of anthropological interpretation but, again, were part of his long-term interest in that group and did little to elucidate any broad generic relationships. His

Kwakwaka'wakw ethnography dealt almost exclusively with industrial and domestic pursuits and is much more a complementary volume to his earlier *The Social Organization and the Secret Societies of the Kwakiutl Indians* (Boas 1897) than a contribution to broader questions.

Farrand's work was thin and peripheral. His Salish basketry design piece was concerned with decorative art, and his Quinault study (Farrand and Kahnweiler 1902) made a minor contribution toward placing that small Salish-speaking group in context. His work on the Chilcotin (Farrand 1900)—the only Athapaskan group at all studied—revealed only a receptivity to neighbors' traditions. Boas only later realized that more attention needed to be given to the wide-ranging Athapaskans, especially those of the far north (Boas 1910a, 1940:336). Smith's Salish archeology was suggestive, but misinterpreted (see Thom, this volume). His cranial finds reinforced Boas' propensity to think the Salish a coastal intrusion from the interior, most likely a mistaken idea.¹⁷

In contrast, no burden of history—neither Boas' previous interests nor existing museum collections—disturbed the expedition's objectives on the Asian side. There the expedition was much more productive and suggestive of relationships. Laufer, Boas' favored "younger brother," contributed little except for collections. This, too, was in large part Boas' fault. He was so eager to keep the young man in New York as part of his East Asiatic project that Laufer was, in June 1901, sidetracked to a quite separate Chinese expedition that occupied him for years. His single substantial Jesup Expedition publication, *Decorative Art of the Amur Tribes*, was "disappointingly spare" (Kendall 1988:104).¹⁸ Even his excellent collection, largely undescribed by its collector, remains relatively mute.

Enormously more substantial were the contributions of Bogoras and Jochelson. Both Jochelson's *Koryak* (1908) and Bogoras' *Chukchee* (1904-09) were extended ethnographic treatments, and Bogoras went on to compilations of Chukchi, Asian Eskimo, and other

myths and an extended treatment of the Chukchi language in later contributions. The two had also returned with huge accumulations of artifacts—collections for their groups that remain superior to any others, even those in Russia (Fitzhugh and Crowell 1988:15). As important, some of the Russians' findings allowed Boas to draw far-reaching conclusions on the great problem that was the expedition's focus.

On the American side, only Boas was involved enough to take a comprehensive view. The Russian collaborators, to whom Boas had introduced American material, were much more attracted to the fundamental problem of Boas' project. Even if they pursued their own research agendas (Krupnik 1996), their Jesup work coincided, over the long term, with Boas'.

The two Russians were struck at least as much as Boas by the closeness of northwestern American to northeastern Asiatic folklore. They became certain that there had to have been either close contact or a kindred origin, and probably both in earlier times (Bogoras 1902:669; Jochelson 1906:125). Bogoras found ideas characteristic of the American Northwest Coast prevailing far into Siberia, so much so that he wrote, "from an ethnographical point of view, the line dividing Asia and America lies far southwestward of Bering strait" (Bogoras 1902:579).

Boas reviewed the Siberian evidence, compared it, as Bogoras and Jochelson had, with his own collections of Northwest myths, and reached the same conclusion. The Koryak, Chukchi, and Itelmen formed one race with the Northwest Coast tribes. The unity had been much greater in earlier times, but "enough remains to lead us to think that the tribes of this whole area must be considered as a single race, or at least that their culture is a single culture, which at one time was found in both the northeastern part of the Old World and the northwestern part of the New World" (Boas 1903:115). Traditions showed far-reaching conformity between the two regions and the interrelationship of motifs was beyond doubt.¹⁹ Boas cited particularly the "magic flight" theme and the widespread

prominence of Raven as ancestor and creator. Nor could the languages of the two areas be separated: the speech of the Asian groups inclined more toward American than toward Central Asian, and if a linguistic division were to be made, eastern Siberian languages were best grouped with those of America. All evidence from physical anthropology tended toward the same conclusion (Boas 1908, 1910b).²⁰

Later events had broken the ancient homogeneity. Just as Tungus and Sakha [Yakut] people had reduced the area once occupied by these related tribes of Siberia, migrations had broken the continuities on the American side. The Salish along the Fraser River and adjacent coasts were a recent intrusion; so too were the Tsimshian, who seemed originally to have been an interior people more akin to the Shoshone and Kootenay. Both, however, had been assimilated into general Northwest Coast culture. The Eskimo, on the other hand, were a more obvious intrusion, a sharply defined physical type, essentially different from their neighbors, who further broke the North Pacific continuum. Though Eskimo material culture was very close to that of the Chukchi, their language and physical type were quite different from those of the Siberians and Americans. The Eskimo did have elements of mythology in common with other coastal people, but these appeared to be an "essentially recent acquisition" (Boas 1908, 1910b).

Some of these conclusions are plausible so far as anthropology and archaeology are able to interpret the obscure past. A school of recent scholarship argues for a tripartite division of Americans: Northwest Coast groups, along with neighboring Athapaskans, may be the descendents of a separate migration from Asia; other American Indians are seen to be descended from a Paleo-Indian group, likely the earliest migrants, who formed the initial, widespread, Paleo-Indian Clovis population; and the Eskimo and Aleut, descendants of Eskaleut ancestors, constitute the third broad group.²¹ This would support the view of the Eskimo as a discontinuity, although the thesis is increasingly contested

by others using different evidence. The Tsimshian and Coast Salish discontinuities are more dubious.

Within this general schema, however, Boas was led to several other conclusions. He was persuaded, apparently on the basis of Jochelson's and Bogoras' findings, supported by the research of Leo Shternberg, that the commonality of the Northwest Coast and Siberia came from a reverse postglacial migration. Boas seemed convinced that the Siberian groups were an offshoot of American peoples (Boas 1910a, 1912, 1940:325, 337; Shternberg 1906:138). This idea, the "Americanoid" theory, receives no current support.²² Boas was even more certain that the Eskimo were an American people, recent invaders from the eastern Arctic. They had, he thought, been driven northward by the Athapaskan and thus descended to the Arctic coast (Boas 1891, 1908:1301). "The much discussed theory of the Asiatic origin of the Eskimo," he wrote in 1910, "must be entirely abandoned" (Boas 1910a:537). However, the dogmatism was usually tempered with a wish for archaeological confirmation that an earlier, non-Eskimo type had inhabited Alaska (Boas 1902, 1908, 1910b, 1936). Boas' insistence is curious. He recognized the strongly "mongoloid" physical type of the Eskimo, their very strong maritime cultural similarity with the Koryak and Chukchi, and the possible connection of Yukon pottery with Siberia (Boas 1904, 1910a, 1940:341), but he never committed himself to any detailed sorting out of the relationships, and his insistence on a central Arctic origin for the Eskimo goes back to his conclusions of the mid-1880s (Boas 1883:118, 1888). The view was endorsed by Bogoras and Jochelson, both of whom wrote of the Eskimo as a wedge that split the trunk of the common tree (Bogoras 1902:670; Jochelson 1908:359). Eskimo origin was, as a later anthropologist noted, Boas' *idée fixe* (Drucker 1955:60). Boas could be a stubborn, even opinionated man: once he grasped a notion, he tended not to let it go.

Part of his difficulty was understandable ignorance. The Alaskan Eskimo were imperfectly known. He noted

the paucity of knowledge of Eskimo mythology west of the Mackenzie River that prevented "a clear insight into the main characteristics of the folklore of the western Eskimo" (Boas 1902, 1904, 1910a:530). Boas probably thought the Alaskan Eskimo to be more similar to his Central Eskimo than they actually were. The uniformity of Eskimo culture was "remarkable," and although he cited "a certain amount of differentiation" west of the Mackenzie River, he attributed it to influence from Indian neighbors (Boas 1910a:537).

Another difficulty was that Boas was working without adequate archaeology, and, had he pursued archaeological research in Alaska and northern Siberia, the methods of the time would probably not have revealed the necessary data. He was also hampered by a too-recent view of ethnic relationships. He tended, understandably, to project historical entities back into remote prehistory. He continued—despite his concern with acculturation and diffusion, despite his attempt at historical depth—to lapse into thoughts of migrations of peoples more or less congruent with historical divisions. Although he made salient the idea that tribes were not stable units lacking in historical development but cultures in constant flux, each influenced by its nearer and more distant neighbors in space and in time (Boas 1908:1296-7, 1910b:8), he could not totally free himself from that fallacy. While northwestern Indian ancestry reaches back to the Old World, recent archaeology has shown the great age of culture in the region and its continuity from its first discernible forms to its appearance at European contact. Current thought tends to the view of stability of population in the region over a long time, with an emphasis on continuity that almost discards migration models (Carlson 1990:69, 115; Fladmark 1986:5).

The expedition did establish some of the affinities it sought between Paleoasiatic groups in Siberia and the Northwest Coast Indians and their interior neighbors. Similarities of bows, housing, watercraft, harpoons, and body armor, for example, could be found on each side of the North Pacific. Elements, even structures, of

mythologies were strikingly similar. That much seemed true. On the other hand, Boas was blinded by his idea of Eskimo origin and remained ignorant of the complexities of Alaskan relationships. He (and Jochelson) willfully dismissed counterevidence of Eskimo participation in North Pacific culture.²³

The Siberian expeditions led Boas to important intercontinental hypotheses. They also, in the work of Bogoras and the Jochelsons, made permanent contributions with long-term effects. Events hindered the full completion of the Russians' projects. Shternberg's work on the Amur tribes never reached publication, nor did Bogoras' on the Itelmen. Only a small portion of the Siberian anthropometry was published. Yet the corpus was significant, probably far more than the Northwest American material, and, as important, the Jesup Expedition spurred Bogoras and Jochelson into continuing activity. Moreover, theirs was the only concern with intercontinental connections for a generation or more.

Since no final summary volume appeared, we have only sketchy and fragmentary suggestions of Boas' conclusions. His comparisons drew on similarities of material culture and mythology and on vaguely described physical and linguistic similarities. Even these did not entirely support his conclusions: he was forced to acknowledge but dismiss the importance of Chukchi and Eskimo similarities. The conclusions that he published in conference papers or journal articles after the expedition's end ventured only a little beyond the evidence he had used between 1895 and 1897 to urge it. The material gathered, important as it was and is, probably could not have sustained much more. That, as much as any other factor, may have determined the nonappearance of the summary volume.

Conclusion

The Jesup Expedition proved a disappointment for Morris K. Jesup and for his museum. Boas, too, was disillusioned, much more by the museum and Jesup than with the expedition itself. While he remained proud of its accomplishments, it had not unfolded in the way

he foresaw, and its publications went on interminably, inconclusively. Worse, he never was able to fill in the research gaps. It has taken almost a century for the resuscitation and redemption of the Jesup project.

The Jesup Expedition's limitations are clear. In a perhaps ironic way, Boas had foreseen that the slow, steady results of his "historical method" would not be dazzling. Even measured by its aspirations and prospectus, however, its success was limited. The answers to its research questions never went much beyond the postulates that formed the question. On the Northwest Coast, it was an extension, "by ampler means," of his earlier program, one which then continued, in Hunt and Teit's ethnological gatherings, in Leo Frachtenberg's painstaking linguistic research, and in Hermann Haeberlin's precociously brilliant essays on art. The Siberian story was somewhat different. There, the expedition sustained the work of two, or even three, pioneering anthropologists. Jochelson and Bogoras, almost alone among Jesup participants (Boas himself being the only other), not only practiced their "historical method" but extended their imagination to embrace the intercontinental context of the project. The impact of the Jesup Expedition had its limitations within scholarship on the North American area, but the consequences for Siberian scholarship have been much more significant and enduring.

Appendix A

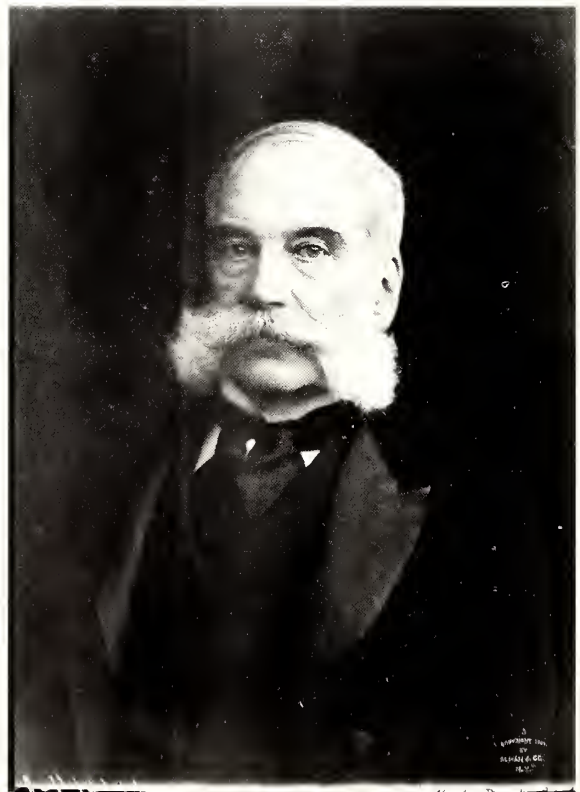
*Franz Boas to Morris K. Jesup,
President,
American Museum of Natural History,
Jan. 19th, 1897*

Dear Sir,

One of the most important problems of American anthropology is that of the influence between the cultures of the Old and of the New World. Investigations on this problem have mostly been confined to comparisons between the ancient cultures of Central America and of South Eastern Asia. The comparative study of that region in which contact and transmission of



5/ Franz Boas, 1858-1942 (AMNH 2A5161)



6/ Morris K. Jesup, 1838-1908 (AMNH 114141)



7/ Waldemar Jochelson, N. G. Buxton, and Waldemar Bogoras in San Francisco before their departure for Siberia, spring 1900 (AMNH 38343)



8/ Dina Jochelson-Brodsky and Waldemar Jochelson on the eve of the Jesup Expedition, spring 1899 (AMNH 338671)



9/ Franz Boas posing for exhibit group displaying Canadian Eskimo clothing and harpoon (AMNH 3220)

7013
4194



10/ Jochelson's team rafting down the Korkodon River, Siberia, fall 1901 (AMNH 4194)



11/ Waldemar Bogoras, with his native guides on the Kolyma River, Siberia, 1895 (AMNH 22402)



12/ Waldemar and Sofia Bogoras, with the group of expedition freight at Mariinsky Post, Siberia, summer 1901 (AMNH 1380)



13/ Dina Jochelson-Brodsky, 1862-1941, on the eve of the Jesup Expedition, 1899 (AMNH 2A23780)



14/ Dina and Waldemar Jochelson in their field tent in Eastern Siberia. Photo ca. 1896. Note the drying negative plates on a small rack on the table (AMNH 2A13549)



15/ Dina Jochelson-Brodsky emerging from native sod-covered hut, summer 1900 (AMNH 337626)



16/ Waldemar Bogoras and his native guides in Chukotka, spring 1901 (AMNH 11117)



17/ Bogoras (in the middle) and his guides preparing for winter sled trip, 1901 (AMNH 2421)



18/ Bogoras and Russian Cossacks on the Anadyr River, summer 1900 (AMNH 2654)



19/ James Teit, 1864-1922, and his wife Lucy Antko, ca. 1896 (AMNH 11686)



20/ George Hunt, 1854-1933, and his second wife T'lat'ławidzanga at Beaver Harbour, British Columbia (AMNH 328734)



21/ N.G. Buxton in Gizhiga, Siberia, flanked by the local Russian officer and his secretary, spring 1901 (AMNH 22089)



22/ Harlan I. Smith during his excavations at the Great Fraser Midden, Eburne, British Columbia (AMNH 42964)



23/ Dina Jochelson-Brodsky and native guides in the Jochelsons' field camp among the Reindeer Koryak, 1901. W. Jochelson, photographer (AMNH 4148)

culture has most probably taken place has never been taken up in a thorough manner.

Fragmentary studies of the Ethnology of the tribes of the North Pacific Coast reaching on the Asiatic side from the Amoor [River] to the Behring Strait and on the American side from Columbia River to Behring Strait have proved beyond reasonable doubt, that there are certain cultural elements in common to all the tribes of this region. The bows, the armors, the method of building canoes may be given as instances. The mythologies of the people of this extensive region show also very peculiar points of similarity which suggest an early communication. Close analogies between Siberian tales and such from British Columbia and particularly tales collected among the Ainos of Yezzo [Hokkaido Island—ed.], the Kamchadeles and the Indians of Vancouver Island have been noticed. The whole question, however, is by no means definitely settled and cannot be solved until all the tribes of this region have been thoroughly investigated. We also know that the physical type of the inhabitants of the North Pacific coast of America resembles Asiatic types more than any other American race.

Both the Asiatic and the American sides of the North Pacific Ocean have one important peculiarity in common. They are inhabited by numerous tribes speaking a great diversity of languages, only a few of which are known. I have indicated on the accompanying sketchmap the distribution of tribes and languages. Those spoken on the Asiatic side are practically unknown, and all of them are disappearing. We do not know if any similarity of structure between these languages and American languages exists, but we must admit the possibility of this being the case. The interior of the Asiatic side is inhabited by people speaking allied languages. The diversity of language does not extend beyond the coast region. The same is the case in America. In short, there are so many points of similarity between the tribes of this whole region that we are justified in expecting that here a mutual influence between the cultures of the Old and the New World

has existed. Thus a foundation for the solution of this important problem with all its important bearings upon the ancient civilisation of America may be laid in this region.

A systematic investigation of the whole problem will have to include the following points:

1. An ethnographical study and the making of ethnographical collections of the tribes on the American side.
2. An ethnographical study and the making of ethnographical collections of the tribes on the Asiatic side.
3. An exploration of the immense shell mounds, and of ancient monuments on the North Pacific coast of both continents.

The study of this subject on the Asiatic side requires a thorough knowledge of Chinese and Mongol ethnology and languages. That in the region of Behring Strait a thorough knowledge of American ethnology and of the Eskimo language. Farther south work is particularly needed in southern Alaska and in the States of Oregon and Washington.

So far as collecting is concerned, this region is one of the few, where a vast amount of material may still be gathered at comparatively slight expense. This is true particularly in the region of Behring Strait, among the Chukchee, the Koryak, and more than anywhere else on the Amoor River. But in all these regions the culture of the people is disappearing rapidly and the whole work is becoming more difficult from year to year.

I have made an approximate estimate of the expense of exploration in this region and judge that at an expenditure for field work of \$5000 a year for six years the whole region may be covered with fair thoroughness. [HUA, Putnam Papers, Box 16].

Appendix B

*Franz Boas to Frederic W. Putnam,
February 11, 1897*

This letter would have to be about 10 pages long, if I wanted to say all I have to say; but I want to be brief and leave all details until your next visit here.

Mr. Jesup called me down to his office the day before yesterday and told me that he could not give me any money for this year's trip to the North Pacific Coast, except that he would give me 2 months leave of absence—and that very reluctantly only—and place at my disposal \$250.00 with which to make collections for the Museum and that he would get me free transportation. I have to give up one month's salary.

Furthermore he told me that he wished to take up the general plan of exploration on the North Pacific Coast and instructed me to consult with you and to propose a detailed scheme of work for the carrying out of the plan. He also asked me, if anything could be done this year and I requested that I might do some things, but that it would be best probably to begin systematic work in Siberia not until next spring.

Now there are two matters for which I must work. The first and less important (although very important for me) is, that I stay away longer and utilize my time, because it would be absurd to go to B.C. for 2 months. I wish to make a plan which I can present to Mr. Jesup putting the matter in such a way that I keep the work for the B.A.A.S. [the North-Western Tribes Committee of the British Association for the Advancement of Science] entirely distinct of all the rest and then put in a couple of months or at least six weeks on work for the proposed Jesup Expedition, which will be a great thing, if it is to embrace the whole work of ethnological exploration of the North Pacific Ocean. Mr. Jesup looks at this proposed expedition in the light that it will be the greatest thing ever undertaken by any Museum either here or abroad and that it will give the Institution an unequalled standing in scientific circles. I will not make any proposition in this letter but must talk the matter over with you in detail when you come here. My general idea is to present the matter in such a way that I commence the work on this side this summer, that at the end of each year enough material should be accumulated to allow us to make a report of the collection which will be a material addition to our knowledge and thus to keep the interest in the subject.

The second point is the making of a detailed plan of work. In order to do this intelligently I must go to Washington to get certain information which I want to present to you when you come here. But first of all we must find the men to do the work when the matter comes to the point. My idea is almost as follows: Judging from what you said you might include Mr. Dixon to prepare specially for ethnographic work among the Chukchee, Eskimo and Yukageer.²⁴ Would he be ready to take the field for a whole year beginning next spring? (a year from May). Then we must engage a student of Mongol languages who must be imported in order to do the work on the Amoor; and at present I am the best man for southern Alaska & B.C. and farther south. Our prime endeavor now must be to impress Mr. Jesup with the necessity of having *trained specialists* do the work, and not give it to adventurers or people with superficial knowledge. I have written a bunch of letters to American Orientalists asking, if there is any good young man who has devoted himself to the study of Mongol Ethnology. And I have written abroad for this purpose. You are aware that I have a certain young man in mind who I think will be first class, but I shall wait until I obtain full information.²⁵ These are the two fundamental points I wished to write about.

Mr. Jesup instructed me to ask your consent to my proposed trip. I hope you will not object to my going away for 2 months and I trust you will show Mr. Jesup that it is desirable for me to stay away for four months. . . .[HUA, Putnam Papers, Box 8].

Notes

1. All subsequent correspondence that is uncited as to repository is from the Boas Papers, American Philosophical Society, Philadelphia. Marie Boas (Boas' wife) and Sophie Boas (his mother) are abbreviated as M. Boas and S. Boas, respectively.

2. But compare Boas' own statement: "I interested Jesup only through Villard" (F. Boas to S. Boas, 27 November 1900). In discussing several issues throughout the paper, I am indebted to previously published work on the Jesup Expedition: Jonaitis (1988); Freed et al. (1988a, 1988b).

3. See also Boas to parents, 19 January 1897.

4. The letter is reproduced in Appendix B.

5. The English version, translated by Dietrich Bertz from German for the British Columbia Indian Language Project, remains unpublished, although typescript copies are available in a few repositories. Boas' conclusions were summarized and elaborated in the *Journal of American Folk-Lore* (Boas 1896a) and reprinted in Boas 1940:425–36.

6. Grube's article is cited in Boas 1897:663n. The Steller description is also in the conclusion of *Indianische Sagen* (Boas 1895a).

7. See his more forceful conclusion and insistence on research in "contiguous areas" in Boas 1896b.

8. Both Boas' paper in the *Folk-Lore Journal* (1896a) and his AAAS paper (1896b) were intended in part as a refutation of Daniel Brinton's ultraorthodox view of independent invention and cultural evolution (see Ousley 2000). Mixed in, however, are a number of other themes, such as concerns about the psychological process of acculturation of cultural elements, the complexity of origins, and the need for strict induction.

9. The BAAS contributed 480 Canadian dollars (G. M. Dawson to Boas, 14 May 1897, National Archives of Canada, Geological Survey of Canada, 63.94).

10. I am indebted to David J. Meltzer of Southern Methodist University, who brought Fowke's letter to the attention of Stanley Freed, and to Freed, who kindly passed it on to me.

11. Born in Vienna in 1872, von Zach later served with the Austrian consular service in East Asia and then in the Dutch government in Indonesia. He published a number of Chinese linguistic studies and translations of Chinese literature before his death in 1942.

12. The Jochelsons' work on the Yukagir was not originally to be part of the expedition, but Boas later accepted the addition of this group to the program (see Vakhtin, this volume).

13. Boas initially was ambivalent about this latest trip of Jochelson's. He welcomed the long-sought research, but it delayed Jochelson's completion of his Jesup writing.

14. Costs were reduced to \$2 per page, below even Boas' estimate. The first part of Bogoras' *Chukchee*, which came out in 1904, was the first

volume published by E. J. Brill.

15. Boas seems himself have recommended the piecemeal idea in order to avoid conflicts with Bumpus. See memo by Boas, 25–27 April 1906, although this is contradicted in Boas to Osborne, 28 April 1906.

16. See also Boas 1901:357 and Boas 1903:77, where Boas writes that, because of Nelson and Emmons, the principal work of the expedition had to be done in British Columbia and Washington State. Swanton did do four months of work in 1904 in southeastern Alaska, but that was under Bureau of American Ethnology sponsorship. He published a long account for the bureau's 26th report on Tlingit society, beliefs, and linguistic relationships in 1908 and a collection of Tlingit myths and texts the following year. The Emmons material was published only in 1991, after almost heroic editorial work by Frederica de Laguna (incidentally, a Boas student).

17. Subsequent studies suggest that the Coast Salish arise from a very ancient technology, the Pebble Tool tradition, that inhabited the coastal region for 9 or 10 millennia (Robinson 1976).

18. Laufer did publish some short contributions, including "Petroglyphs on the Amoor" (Laufer 1899) and "Preliminary Notes on Explorations among the Amoor Tribes" (Laufer 1900).

19. This was based largely on Jochelson's comparative analysis in *The Koryak* (1908:354–82), the purport of which had been published earlier in Jochelson 1906.

20. These somewhat repetitive reports are perhaps the best summary of Boas' conclusions in the years following the expedition.

21. This remains a difficult and controversial area in which new evidence undermines old models while increasing the complexity of the problems. Nevertheless, much of Boas' general conclusion remains plausible.

22. The term "Americanoid" was used in this connection by at least 1904. Stephen Ousley [Ousley 2000—ed.] has pointed to its earlier, but disparaging, use by Daniel Brinton.

23. Jochelson, for example, did not include Nelson's Alaska Eskimo myths in his evaluation because "a large part of the episodes of the latter cannot be considered as genuine Eskimo

elements" and would only "have caused confusion." Yet the Eskimo influence on Koryak culture—myths, religious rites, and material culture—pointed to a direct intercourse between Koryak and Eskimo at some period. When, and under what circumstances could only remain an open question (Jochelson 1908:359). See Chowning 1962.

24. Roland Dixon was a Harvard student. He made a brief trip to the West Coast for the Jesup Expedition but never went to Siberia. His dissertation on the Maidu was supervised by Boas. He received his Ph.D. degree in 1900, after which he began a long career at Harvard.

25. Obviously, Boas refers here to Berthold Laufer, with whom he maintained an extensive correspondence.

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Franz Boas and the Shaping of the Jesup Expedition Siberian Research, 1895-1900

NIKOLAI VAKHTIN

To ensure the productivity of their research, I am convinced that American scholars who nowadays venture on research projects in Siberia must study the history of the Jesup North Pacific Expedition (JNPE), 1897–1902, as a prerequisite. Likewise, Russian academics considering participation in joint projects supported by American grants should familiarize themselves with the historical background of the JNPE. All—especially those of us participating in the *Jesup 2* project (see Fitzhugh and Krupnik 1994)—are well advised to study the achievements, challenges, mistakes, and limitations of their predecessors as they arranged international cooperation 100 years ago. The astonishing similarity between political, social, and scholarly paradigms then and now makes this task not only necessary but also emotionally powerful.

Although this alone would justify interest in the history of the JNPE, there is another reason for such interest. Extensive American literature on the subject focuses, quite understandably, on the “American” side of the JNPE—on its influence in shaping American anthropology and on its American participants. The expedition, however, had two sides, and its “Russian” side is of no less importance to the development of Russian anthropology.

It is a fact that the JNPE played an important role in shaping Russian scholarship, especially the development of Russian (and, later, Soviet) research in social anthropology, ethnography, and linguistics of the Siberian Native peoples. A miraculous interplay of favorable circumstances—for the development of

anthropology—led to the emergence of a “school” that proved to be extremely productive and fruitful. To some extent, to study the roots of Russian northern research after 1897 is to study the history of the JNPE. In an excellent review paper by Freed et al. (1988), the description of preparations for the Jesup Expedition and the obstacles it had to overcome takes about two pages. Of these, the authors have given the Siberian side two lines: “In Siberia, the principal problems were politics, climate, terrain, logistics, miserable living conditions, and the enormous distances . . .” (Freed et al. 1988:9). Was there, then, anything that was not—is not—a problem in Siberia?

This paper tries to fill in at least the broadest “Russian gaps” in the early history of the JNPE, largely on the basis of archival resources in the United States and Russia. More specifically, I relied on vast collections of correspondence between the members and organizers of the expedition and the dozens of other people who were in one way or another involved in this monumental enterprise.¹

Developing The Project, 1895-1897

There is a well-known, though certainly somewhat unfortunate, tradition of naming buildings, halls, universities, book series, and projects not after those who built, wrote, or invented them but after those who provided the funding. This is understandable: good architects, writers, and scholars will, with some luck, be remembered, but for the rich, this may be their only opportunity.

The role of Morris K. Jesup in establishing the American Museum of Natural History (AMNH) is, of course, fundamental. Similarly, without his support the North Pacific Expedition would hardly have been possible. Nevertheless, the *Boas* North Pacific Expedition might be a better name: the amount of time, talent, and energy that Boas invested in this project was incredible.

Franz Boas' Employment at the AMNH

Franz Boas was born in 1858 in Minden, Westphalia. He chose a university career in natural sciences and mathematics, and from 1877 to 1881 he studied in Heidelberg, Bonn, and Kiel. After a year of military service, Boas spent some time in Berlin studying the "reaction of the human mind to [the] natural environment." In the summer of 1883 he went to Baffin Island and spent more than a year with the Inuit. After several more years in Berlin at the Ethnographic Museum (Museum für Völkerkunde) and more fieldwork (on the West Coast of North America, in 1887), he moved to the United States and took a position at Clark University. After resigning from Clark in 1892, Boas spent the next two years in Chicago, first as chief assistant to Frederic W. Putnam, a leading anthropologist at Harvard whom Boas helped to organize anthropological exhibits at the Chicago World's Columbian Exposition, and later as curator of anthropology at the Field Museum. Boas' resignation from the Field Museum in 1894 was followed by a year of unemployment (Stocking 1973).

Putnam, who was hired as part-time curator, Department of Anthropology at the AMNH in New York, began working on a plan to invite Boas to the department. As early as December 1894, he wrote to Jesup:

Complying with your request that I put in writing the substance of our conversation of yesterday . . . I respectfully make the following suggestions: First,—that I be authorized to propose to Dr. Franz Boas that he shall so arrange his plans as to be able to accept a position in the department as early as possible next Fall. . . ." [Putnam to Jesup, December 8, 1894, AMNH-DA].

Putnam used every meeting with Jesup to persuade

him that they needed Boas in New York. This persistence eventually bore fruit. In March 1894, AMNH Secretary John H. Winser had informed Boas that there was no position for a curator of anthropology at the museum (Winser to Boas, 3 March 1894, AMNH-DA). Five months later, however, the likelihood of a position already appeared to have increased. Putnam encouraged Boas, writing that he hoped that the cloudy period of Boas' life was over and there was sunny weather ahead (Putnam to Boas, 3 August 1894, APS-NYPL; see also Dexter 1976).

During this time, Boas was not simply waiting passively for other people to decide his destiny. In May 1895 he wrote to the U.S. National Museum in Washington [later renamed the Smithsonian Institution's National Museum of Natural History, NMNH—ed], offering to enlarge, describe, and sort out its American Indian collections in order "to make a systematic exhibit covering the whole North Pacific coast" (Boas to NMNH, 27 May 1895, APS-NYPL). In this letter, the concept of the North Pacific Coast included only the four American Indian groups from the Yakutat [Northern Tlingit] to the Salish.

During the summer of 1895 Boas was in Europe (Germany, England, and France). While there, he received an offer from J. W. Powell for a permanent position in Washington with the Bureau of American Ethnology (BAE). Simultaneously, he received a letter from Putnam:

I wrote to President Low [of Columbia University] about getting you for Columbia College, after a consultation with Mr. Jesup. Mr. Jesup thought if we could manage to keep you in New York through the winter somehow or other, that next year would open better for us in many ways, and between Columbia College and the Museum we could be pretty sure of giving you a satisfactory position. (Putnam to Boas, 19 June 1895, APS-NYPL)

Putnam asked Boas to postpone the decision until things clarified in New York. With Boas, Farrand, and Ripley, the AMNH would have had an "unbeatable anthropological team" (Freed et al. 1988:9). They could establish there, Putnam wrote, "a great

anthropological institution," whereas in Washington, he argued, Boas would not be so free in his actions.

Thus, by the summer of 1895 Boas had two offers, one from the AMNH and one from the BAE. He was uncertain which to choose. The position at the AMNH looked more attractive, but the one with the BAE was more secure and could be taken right away.

The North Pacific Expedition Idea Emerges

Before 1895, Boas never discussed field research in Asia or in Siberia in his letters. He wrote several letters describing his plans for future work in British Columbia and along the Northwest Coast (e.g., letter to G. M. Dawson, 15 May 1894, APS-NYPL), but he never spoke about expanding beyond the Bering Strait, nor is any mention of Siberia to be found in his early correspondence with Putnam or Jesup.

It was probably during his trip to Europe in the summer of 1895 that the idea of the North Pacific Expedition—a comparative study of the American and Siberian Native people—struck Boas' mind. This idea went beyond anything Boas had envisioned before. Whether he was inspired by something he had read or heard in Europe or by the forced idleness of the seven-day transatlantic trip back to New York, Boas obviously landed on American soil with an idea that was to become the nucleus of the North Pacific Expedition.

Boas had acquired unique experience on the Northwest Coast, particularly in British Columbia, and he was well equipped to address the problem of contacts between the Old and the New Worlds.

The types of man which we find on the North Pacific coast of America, while distinctly American, shows a great affinity to North Asiatic forms; and the question arises, whether this affinity is due to mixture, to migration, or to gradual differentiation. (Boas 1898b:2)

This was put into an even broader and more challenging context:

We have come to understand that before we can build up the theory of the growth of all human culture, we must know the growth of

cultures that we find here and there among the most primitive tribes of the Arctic, of the deserts of Australia, and of the impenetrable forests of South America; and the progress of the civilization of antiquity and of our own times. We must, so far as we can, reconstruct the actual history of mankind, before we can hope to discover the laws underlying that theory. (Boas 1898b:2)

Soon after coming to New York (or perhaps while still in Europe), Boas must have written to Leonhard Stejneger, an old friend in Washington who had visited the Russian Far East, to ask for advice. In November 1895, Stejneger answered; another letter from him followed in December. Inviting Boas to visit Washington, Stejneger wrote:

We can then better talk of the various things you write about. As a matter of fact, without knowing how it is proposed to travel "in the Amur region and further North" [evidently a quotation from Boas' letter], I can have no idea as to costs . . . my experience has been in such a different quarter of that part of the world that they would be of but little use. I have today written, however, to a friend in San Francisco . . . who could provide necessary information. (Stejneger to Boas, 26 November 1895, APS-NYPL)

A month later, Stejneger described the means of transportation from Vladivostok to Petropavlovsk and to small towns such as Gizhiga, Okhotsk, and Tigil along the Sea of Okhotsk. This information was obviously based on the letter from the "friend in San Francisco" he had mentioned earlier (Stejneger to Boas, 21 December 1895, APS-NYPL).

In the meantime, Boas accepted the AMNH position, on January 3, 1896. Along with his everyday activities at the museum, he began to dig trenches around Jesup. Since Putnam, as head of the Department of Anthropology, was his superior, there was no way for Boas to leave him out of the project. In fact, it is unlikely that he had such intentions; Putnam was a friend, and the two thought largely along the same lines, whether the museum structure or anthropological fieldwork was at issue. Putnam had demonstrated this clearly in his memorandum to Jesup (see *Annual*

Report on the Department of Anthropology for 1894;
Putnam to Jesup, 11 August 1894, AMNH-DA).

I believe that the original idea for the North Pacific expedition was developed by Boas and later promoted by Putnam. The two, however, worked closely together. Putnam was the boss, and Boas naturally did not have a chance of persuading Jesup to pay for the expedition without Putnam's support, authority, and name. Although an original letter addressed to Jesup describing the plan of the North Pacific Expedition was not discovered by myself nor other researchers (Brown 1910; Dexter 1976; Freed et al. 1988:9; Hinsley and Holm 1976; Kennedy 1969), there are some indications that such a crucial letter was written.² For example, Putnam wrote to Augustus Lowell:

. . . you have probably noticed in a newspaper . . . some account of the Expedition to the North Pacific which is to be carried on under my direction for the American Museum of Natural History in New York. Mr. Jesup, who is the President of Board of Trustees of that Museum, will personally pay all the expenses of the expedition. Dr. Franz Boas . . . will take charge of a party to make explorations on Vancouver Island this summer. . . . In order that you may understand the scope of the above-mentioned expedition . . . I enclose a copy of my letter to Mr. Jesup on this subject. (Putnam to Lowell, 20 March 1897, APS-NYPL) [See also Appendix A to Cole, this volume—ed.].

Note the phrases "under my direction," "my letter", they clearly indicate that the letter was signed (or perhaps cosigned) by Putnam. In any case, the fact that the North Pacific Expedition was organized and financed "at the suggestion of Boas and F. Ward Putnam" (Rohner 1969:199) can be regarded as proved.

One can get a clear idea of the contents of this letter from another letter written by Boas to Jesup in November 1898 in which he tries to "restate the objects of the expedition, the original plans, and the changes that seem desirable at the present time." Boas formulates the two goals of the expedition as follows:

1. Is there a racial affinity between the Asiatic race and the American race, which

will compel us to assume a common origin of both? 2. Can we prove by archaeological and ethnological investigation the existence of historical contact between the tribes of the two continents? (Boas to Jesup, 2 November 1898, AMNH-DA)

He then explains at length the information that led him to expect a positive answer to these two questions.

The Expedition Takes Shape

For several months, the idea was discussed in many meetings. Jesup soon became an ardent supporter of the proposal and tried to raise money for it. When it seemed that nobody was willing to sponsor the project, Jesup made a bold decision to cover the expenses out of his private funds. John Winser wrote to Putnam in February 1897:

Mr. Jesup has about concluded to take up the cost of the Bering Sea explorations. He . . . would like you to have the matter in mind and be prepared to give your views on his return. Entering into this work is however entirely dependent upon the discovery of the right man for the work . . . (Winser to Putnam, 12 February 1897, AMNH-L)

In March 1897, the first public announcement of the expedition appeared in the form of an anonymous article in *Science* (Proposed Explorations . . . 1897:455–7) [presumably written by Boas]. It was followed by numerous articles in the *New York Times* and other national and local papers. The papers flashed tempting headings:

Round the World for Science. Morris K. Jesup to Send an Expedition for the Museum of Natural History to Search America First. Anthropologists Will Gather Evidence as to First Men on This Continent, Will Cross to Asia Then . . .

When the expedition was announced, dozens (perhaps hundreds) of letters poured into the AMNH. All kinds of people begged to be allowed to take part— young and old, adventurers and doctors, journalists and students, and even a shorthand expert. (The last-named offered, rather boldly, to write 150–200

syllables per minute in any language, including those not previously known to exist.) Most of the letters, which typically began, "it has always been my dream," were written in 1897; all or most were answered negatively: "At present we have completed the research parties; your letter will be filed for the future". A letter from a W. F. Brock is worth quoting as an example:

For several years I have devoted all of the time that I could spare from my profession in gathering together Indian history and legends. . . . As a newspaper correspondent, I have traveled over ALL of Oregon, Washington and Idaho. I have visited many parts of Montana, British Columbia, Alberta and Assinaboia. . . . I was with the Piute Indians of Nevada for four months. . . . I lived among the Yakimans. . . . I converse freely in the Chinook Jargon. . . . I can handle a train of packed horses and manage canoe with a skill which has been acquired by a life residence in a new country. . . . I should like to work under you or in one of your divisions, in any capacity in which I can be the most useful. (Brock to Jesup, n.d. 1897, AMNH-DA)

In a letter to Jesup complaining, hypocritically, that he was besieged by reporters eager to learn details about the expedition, Putnam indicated that, on the whole, "[i]t again shows the great interest which the people take in everything anthropological and especially in all research relating to the ethnology of America" (Putnam to Jesup, 16 March 1897, AMNH-DA).

Now that the expedition had the necessary funding and wide publicity, Boas realized, as Winsor had put it, that beginning the JNPE project in earnest was "entirely dependent upon the discovery of the right man for the work." Boas began to look for the man.

Looking for the Man: Von Zach and Baily

Through his German and American contacts, Boas soon came across two names. The first person was a V. Baily, recommended by Stejneger. Very little is known about him except that he "has had the intention for some time to go to Eastern Siberia collecting" (Stejneger to Boas, 27 April 1897, APS-NYPL).

The other was a young German scientist from Leiden, Edwin von Zach, who was recommended by Professor Gustav Schlegel. Boas' letter to von Zach in April 1897 is probably the earliest source available from which one can judge how Boas had envisioned the proposed expedition before it actually began:

From what Dr. Schlegel writes me, I suppose that you will be well prepared to undertake linguistic and anthropological work, both of which will be of great importance for the undertaking; but . . . it is also necessary to pay particular attention to the collection of ethnological and anthropological material. I desire to have particularly good collection of crania, when such can be obtained, and of all the objects used in the daily life and religious life of the people. Besides these, I lay particular stress upon the collection of good linguistic data, of collection of myths and other traditions in the original language, of songs, etc., and furthermore I want extensive service of measurements of the people; that is to say, I want to cover the whole field of ethnological, anthropological, and linguistic research as fully as possible. . . . You will understand that this letter is not a definite and final proposition on my part, but this letter is written in order to inform you of our proposed work. (Boas to von Zach, April 1897, AMNH-DA)

Von Zach's answer was prompt and enthusiastic:

I am much obliged to you for your flattering proposition . . . , and I am perfectly satisfied with the conditions. . . . Although I am not a man of means, a scientific investigation of this kind is not a question of making money; but I am doubtful if I am able to adequately carry out the proposed work. I have studied medicine and the Chinese language and literature, but I have not paid much attention to the isolated languages of eastern Siberia. . . . All I can claim, therefore, as special acquirements, is a general knowledge of the subject and a deep interest in every thing pertaining to the same. If you should finally decide to engage me, I should propose to discontinue my special work on Chinese language and literature, in order to prepare, so far as feasible, for the proposed expedition. I should study in detail the linguistic and ethnographical literature of Siberia, and visit the collections at Berlin, London, and St. Petersburg. I should also take up with greater vigor my practical studies of the English and Russian languages. Finally I beg to ask you

to inform me if the work that I would be expected to do is confined to the Koryak and Youkageer, or if you intend to take up other tribes of eastern Siberia as well. (von Zach to Boas, 24 April 1897, AMNH-DA)

Boas was obviously impressed by the young man's response. On May 7, 1897, he wrote to both Gustav Schlegel and Morris Jesup stating that the recommended candidate was "excellent." "I do not believe from what I hear," he added, "that we can find a better man than him for the work north of the Okhotsk Sea, and I would suggest that he be engaged for doing this work" (AMNH-DA).

By mid-May, the proposed expedition began to take shape, as Boas wrote to Jesup:

It will be possible to send two parties to Asia next spring. One of these would go to Arctic Siberia . . . the other party would go to the Amoor River. It would be best for both parties to stay away for a whole year. I have engaged Prof. Von Zach to go to Arctic Siberia, and another gentleman [Boas is probably referring here to Laufer—N.V.] who seems to be very well prepared for the work has been recommended to me. (Boas to Jesup, 16 May 1897, APS-NYPL)

On May 19, 1897, shortly before leaving on the field trip to Victoria, British Columbia, Boas sent an official letter to von Zach and offered him a position on the expedition team, with the task of studying the Chukchi, the Koryak, and the Yukagir tribes of Siberia. For this, he suggested a salary of \$500 per year, with all expenses in the field to be covered by the AMNH (Boas to von Zach, 19 May 1897, AMNH-DA).

At the same time, steps were taken to secure the cooperation of the Russian government. On March 15, Morris Jesup signed a formal letter to Russia's Envoy Extraordinary to the United States, E. Kotzebue. In describing briefly the aim of the expedition, he expressed hope that "the Imperial Russian Government will give us authority to carry on explorations in its territory" (Jesup to Kotzebue, 15 March 1897, AMNH-DA).

Relations with Russian government authorities developed slowly but steadily. On September 19, Dr. E. O. Hovey, a geologist employed by the AMNH who

had taken part in the International Geological Congress in St. Petersburg, submitted to Jesup a report on his consultations with Russian officials (conducted at Jesup's request) about the possibility of sending an expedition to Siberia. The Russian government, regarding the whole proposition quite favorably, requested a list of the people who were to take part in the expedition, with their titles and positions, "without which nothing could be done." No foreign expedition would be allowed to enter Siberia unless its personnel was known and approved in advance. Dr. Hovey also talked to General Dubrovin, of the Imperial Russian Academy of Sciences (RAS), and he met with Dr. Amstant, the assistant to the permanent secretary of the RAS, Professor Vasily V. Radloff. (Radloff himself was away on vacation.) In addition, Hovey called on Grand Duke Constantine, president of the RAS, leaving with his secretary a letter explaining the plan for the Siberian expedition. His conclusion was "that the Russians are or will be thoroughly interested in the investigations in north-eastern Siberia and that the government will authorize and assist the expedition" (Hovey to Jesup, 19 September 1897, AMNH-DA).

Change of Plans: Jochelson Appears

Everything seemed in order, but later that year something must have happened with von Zach. There are no more letters to or from him in Boas' correspondence collection, and the leadership of the JNPE fieldwork in northeastern Siberia was again uncertain. The sequence of the Siberian work suddenly changed; the Amur River area would now first be investigated by Berthold Laufer (on Laufer, see Kendall 1988). On January 4, 1898, Boas wrote to Radloff:

For the Spring of this year we have planned an expedition to the Lower Amoor [Amur] River. We have requested and have been granted authority from the Imperial Russian Government to conduct our investigations in that region, and I have selected Dr. Berthold Laufer of Cologne, who has studied Asiatic languages in Berlin and Leipzig, to study the language of the Gilyak; he will be accompanied

by Mr. Gerard Fowke. . . . I hope to extend our work in 1899 towards the more northern regions, but I have not yet found a man well fitted for this work. . . . I am desirous of finding a young man who will spend a year or two in Northeastern Siberia, with a view to studying the customs, manners, languages and physical characteristics of that district. Could you recommend to me a young man fitted to undertake this work? (Boas to Radloff, 4 January 1898, AMNH-DA)

Radloff promptly responded on February 23, 1898:³

I have found a gentleman willing to take part in your expedition, a Mr. Jochelson, who has just returned from an expedition to the Yukagirs, and among whom he has lived for two and a half years. . . . He consents to take part in the expedition for one year only, and only to the Yukagirs. For the expedition to the Chukchee he recommends a friend of his, a Mr. Bogoraz, who has lived two years among them and knows their language. . . . It is my opinion that you would do well to secure the services of these two gentlemen, since they are both well acquainted with the countries to which they will have to go, and have already made special studies of the languages as well as the habits and customs of the peoples. . . . Unfortunately I have not yet been able to receive the consent of the latter gentleman, since he is living in Eastern Siberia, but I have written to him and hope to have his answer in about two months. (Radloff to Boas, 23 February 1898, AMNH-DA)

Radloff also rendered to Boas the conditions upon which Jochelson consented to undertake the work. All travel expenses should be paid, as well as a sufficient salary starting on the date Jochelson left St. Petersburg and continuing until he had fully prepared his field materials for publication. Jochelson was willing to give Boas full benefit of all the materials he had already gathered, as well as those yet to be collected, but he reserved the right to publish in Russian as much of these findings as he wished.

This is the first time the names of Waldemar (Vladimir) Jochelson and Waldemar (Vladimir, also called Nathan) Bogoras (or Bogoraz) appear in the correspondence.⁴ A question that is often asked—why Radloff did not mention the third potential participant, Leo Shternberg—has, in my opinion, an obvious answer.

The original letter from Boas indicated that he already had a person for the study of the Gilyak [Nivkh] people in the Amur River area. Boas was asking for help in identifying one man to do research in northeastern Siberia for two years. Radloff instead suggested two men, each for one year. For Shternberg, there just was no vacancy at the time (see also Kan, this volume).

Radloff also wrote to Jesup informing him that “the Academy of Sciences at St. Petersburg has consented to assist in every possible way the scientific expedition.” In addition, he requested official information: the names of all persons who were to take part in the expedition, when they expected to arrive in Siberia and the duration of their stay, and what parts of Siberia they intended to visit. This information, he explained, was necessary for a letter of recommendation to the governor-general of Eastern Siberia, so that each member of the expedition could be supplied with an open letter from the minister of the interior to all the administrative powers of that part of the empire (Radloff to Jesup, 23 February 1898, AMNH-DA).

However, the matter of acquiring permission from the Russian government did not proceed smoothly. On April 4, the U.S. Embassy in St. Petersburg informed Jesup that Laufer would not be able to get a Russian visa to conduct fieldwork in Siberia. The visa was refused by none other than Minister of the Interior Ivan Goremykin, who was perfectly familiar with the whole project and was much interested in the matter. Goremykin’s position was that this would be against Russian law: Laufer, as a German Jew, was prohibited from entering Siberia [according to the Russian anti-Jewish regulations—ed.] (U.S. Embassy to Jesup, 4 April 1898, AMNH-DA).⁵

Boas wrote to a contact in Germany, a Mr. Grundwedel, to discuss the possibility of influencing the Russian government. The answer was pessimistic:

. . . the Russian government seeks totally to thwart all scientific investigations by non-Russian scholars on Russian territory. . . . I see no other way but that the expedition make itself directly available to the Russian Academy.

Of course the Imperial Russian Academy would have first rights to both the collections as well as any literary output. For science it would be all the same, of course, but not for you. (Grundwedel to Boas, 2 May 1898, AMNH-DA)

The matter was settled only after Radloff addressed Grand Duke Constantine, titular president of the RAS, who appealed to no less than his nephew, Tsar Nicholas II.

By June 1898, everything was more or less ready. In July 1898 Berthold Laufer and Gerard Fowke began their work among the Nivkh [Gilyak] and Ulch [Tungus] people of the Amur River region, as well as among the Ainu of Sakhalin Island. They remained in the field until March 1899 (Freed et al. 1988:13–14; Kendall 1988; Segel n.d.). By that time, the other half of the Siberian JNPE expedition had also been arranged.

The JNPE Siberian Team: Two Populist Revolutionaries

It is now time to explain who those two “Russian gentlemen,” Vladimir Jochelson and Vladimir Bogoras, were. To use Radloff’s wording, they “had just returned from an expedition” to Eastern Siberia and were recommended by the RAS to Boas on the strength of their two-year experience of fieldwork in the area, their good command of Native languages, and their deep knowledge of the “habits and customs of the people.” In fact, the two people in question were dissidents.

Vladimir Jochelson was born in 1855 and had joined the revolutionary movement, the People’s Freedom party, at a rather young age.⁶ Between 1875 and 1881, he was an underground party activist. In 1881 he emigrated to Switzerland, where he worked at the party printing house and studied social sciences and economics at the University of Bern. In 1885 he returned to Russia and was immediately arrested and imprisoned. He spent 1885–87 in solitary confinement, and in 1887 he was exiled to Eastern Siberia for 10 years of *ssylka* (political exile).⁷ While in Siberia, he became interested in the Yukagir, a small Native nation living in

the area of his exile. He later took part in the Sibiriyakov Expedition (1894–98) organized by the Russian Geographical Society [and sponsored by Russian gold-mining tycoon Alexandr Sibiriyakov—ed.]. Jochelson returned to European Russia in 1898 and immediately went to Switzerland, where he enrolled at the university in order to finish his education (RAS-J).

Vladimir Bogoras was born in 1865 in the small town of Ovruch in Volyn Province, western Ukraine. In 1880, at the age of 15, he entered St. Petersburg University. He took courses in mathematics but later switched to law. Like Jochelson, Bogoras was a member of the People’s Freedom party. In 1882 he was exiled to his hometown and then, in 1883, arrested. After serving a short term in prison, he again became very active in party affairs. In December 1886 he was arrested for the second time, sent to prison for three years, and afterward exiled for 10 years to the Kolyma Region of eastern Siberia, where he lived from 1890 until 1898. Around 1894, he too became a member of the Sibiriyakov Expedition and worked on the ethnography of the Chukchi. He returned to St. Petersburg in 1899 and was employed as a fellow of the Museum of Ethnography (Al’kor 1935:5–7; Krader 1968:116).

A third person, Leo (Lev) Shternberg, became connected with Boas and the JNPE project several years later. Since his name will be mentioned many times below, and since Shternberg’s earlier years were so strikingly similar to those of Bogoras and Jochelson, it is appropriate to say a few words about him here. (A more detailed account is found in Kan, this volume.)

Born in 1861 in Zhitomir, Ukraine, Shternberg studied at St. Petersburg University in 1881, enrolling in the Department of Natural Sciences. He soon joined the Central Student Circle, the main branch of the People’s Freedom party among the students. There, he met Bogoras for the first time. After being involved in large student demonstrations and clashes with the police, Shternberg was exiled from St. Petersburg in 1882 and became a law student at Novorossiysk University in Odessa. He studied and continued his “revolutionary

activities" in Odessa for four years until his arrest in 1886, when he was in the middle of his graduation exams. After being imprisoned for three years, in 1889 he was exiled for 10 years to Sakhalin Island. He became interested in the language and culture of the Gilyak [Nivkh] people and published his first paper on the Gilyak in 1893. In 1899 he returned to St. Petersburg (Bogoras helped him get permission to live in the capital) and in 1901 became an ethnographer at the Museum of Anthropology and Ethnography, rising to the level of senior ethnographer several years later (Kan 1978; Ol'denburg and Samoilovich 1930:7–8).

Jochelson, Bogoras, and Shternberg: Early Anthropological Interests

During their years in Siberia, these three members of the People's Freedom party wrote to each other as frequently as they were permitted, exchanging whatever news they had, words of support for each other, opinions on the books they had read, and the books themselves. The letters are full of complaints about the unbearable conditions of life and the idleness and boredom. This is especially true for Bogoras who, being the youngest and the most energetic of the three, obviously suffered most from living "on the sidewalk of the road of life," as he put it, and watching life go past. This excerpt of a letter from Bogoras in Sredne-Kolymsk to Shternberg in Sakhalin captures his mood:

Your warm-cold and wet-dry island is still part of the globe, and lives and moves together with it, if not forward, then at least backwards. Kolymsk is a different planet, even less connected with Earth than the Moon, completely alien to Earth, a block of ice cast out into space and suspended there above the emptiness, where every accidental spark of life freezes down and suffocates. (20 June 1894, RAS-B).

The reasons why the three exiles become interested in the ethnography of the Siberian Native peoples are rather complicated. To some extent, it was a continuation of their interest in "the people"—a central

concept in the People's Freedom party ideology. Another reason was the immense demand for educated people in those remote areas. The services of Jochelson, Shternberg, and Bogoras were soon engaged by the local administration and by the Sibiryakov Expedition for the purposes of conducting censuses, recording statistics, and describing the life of the people. Of course, they were political exiles and could not be trusted, but they were also educated people—former university students—and thus could be useful. To some extent, the idleness and boredom of their everyday lives impelled them to find something to do in order to "preserve their sanity and will to live," as Kan (1978:11) put it. Ten years, after all, is a long time.

Initially, they might not have taken their ethnographic pursuits seriously. For example, Bogoras, after two paragraphs of the usual complaints about his boredom and idleness, rage at being cut off from life, and irritation, wrote in a letter to Shternberg:

I am now flirting with ethnography. I traveled through the area, lived for seven months with the Chukchi, goddamn them, rode on reindeer back, went downstream on rafts—well, this is hardly interesting to anyone but an ethnographer. (Bogoras to Shternberg, 4 November 1895, RAS-B)

Shternberg himself, after several months of isolated life at a distant military post (he had to share a hut with his guards), established friendly relations with residents of a neighboring Gilyak [Nivkh] settlement. Going there almost every day, he began to learn their language and to document their customs.

Thus it happened that almost simultaneously (Jochelson in 1898, Bogoras and Shternberg in 1899), three men experienced in studying Siberian ethnology and languages and willing to publish the materials they had collected arrived in St. Petersburg. Of course, in many ways they were quite naive about how science was done. In 1899, for example, Shternberg, still in Sakhalin, wrote to Bogoras, who was already in St. Petersburg, asking him to find "an international Gilyak alphabet" and "a reader in comparative philology."

Bogoras wrote back quickly, "There is no such thing as a Gilyak alphabet. What you have to do is copy a couple of Gilyak texts and send them to the Academy with detailed grammatical commentaries" (Bogoras to Shternberg, 22 February 1899, RAS). Shternberg mailed his manuscript on the Gilyak language to Bogoras, who persuaded K. Zaleman, a well-known linguist working with the academy, to publish it (see Kan, this volume).

Obviously, Bogoras, Jochelson, and Shternberg were using their ethnographic and linguistic materials and the unique knowledge they had acquired in Siberia as a means of recapturing their standing in life. In 1899 Bogoras wrote to Shternberg that he had visited Radloff, who promised to support Shternberg's intention to come to St. Petersburg to work on his collections, which would be donated to the Museum of Anthropology and Ethnography (MAE). Bogoras advised Shternberg to write to Radloff immediately that he, Shternberg, had a certain collection from a certain land and was willing to present it to the museum but needed time to organize it. That would require his presence in St. Petersburg. "Advertise yourself with reserve but intensively," Bogoras wrote, not without a hint of irony (Bogoras to Shternberg, n.d. 1899, RAS).

They seem rather surprised themselves at how their lives were turning out. Before their exile to Siberia, they never dreamed of becoming ethnographers. Political activism, journalism—these were the stuff of real life. Bogoras somewhat sarcastically joked, "Ah, this is what the Achaeans went to conquer Troy for! So that they could afterwards take apart Chukchee, Yukaghir, Gilyak and other texts. *Mais tu l'a voulu, George Dandin!*" (Bogoras to Shternberg, n.d. 1899, RAS). But another obvious undertone of Bogoras' letters of the period was sheer pride. He was proud of himself and his comrades because they had managed not to perish, physically and mentally, during those 10 extremely harsh years in Siberia. Instead, they had found something there that helped them reestablish their social standing. These former convicts and exiles had collected copious data previously unknown to scholars,

and they were publishing their works in the prestigious proceedings of the RAS and the Imperial Geographical Society. The RAS had no one but the two (or even three) of them to recommend to Boas as experienced ethnographers with considerable knowledge of Siberia. "By God, attaboys, those old Siberian Jews!" (Bogoras to Shternberg, 19 August 1899, RAS).

Boas Employs Jochelson

In the fall of 1898, Boas went to Berlin, where, for the first time, he had an opportunity to meet Radloff in person and to make the acquaintance of Jochelson, who was still in Switzerland working on his doctoral examinations (Boas to Jesup, 4 October 1898, AMNH-L). After meeting Boas and securing his own position, Jochelson began to promote Bogoras persistently, reminding Boas about him in almost every letter. For instance, he wrote:

I just received word from Yakutsk, from Mr. Bogoraz, that he agrees to study the Chukchi for the Museum and travel to the Bering peninsula for that purpose. He is satisfied with the conditions I had stated. Mr. Bogoraz should have arrived in Irkutsk by now, and in November we hope to meet in Russia . . . (Jochelson to Boas, 23 September 1898, AMNH-DA)

And:

I beg to repeat that he is by far the best man for the investigation of the Chukchi and the other tribes of the Bering peninsula. . . . Mr. Bogoraz speaks Chukchi fluently. He is well prepared to conduct ethnological work, and he is willing to start at once, if so required. (Jochelson to Boas, 3 November 1898, AMNH-DA)

On October 28, 1898, Boas mailed to Jochelson a letter containing the terms of the latter's employment for the expedition: the AMNH offered to employ Jochelson for a period of three and a half years at a salary of \$100 a month, with an additional \$4,000 set aside for field expenses. Jochelson had to come to New York on or around February 1, 1899, in order to receive special instructions in regard to his fieldwork. He was required to then proceed to the north coast of

the Sea of Okhotsk in spring 1899. He was to devote his time from summer 1899 until late winter 1900 to the study of the local Koryak people and then pay a visit to the eastern groups of the Yukagir. The scope of his work was defined as follows:

You would have to make collections of specimens illustrating the customs and the physical characteristics of the people. These collections should include ethnographical specimens of all kinds, skeletons and skulls, so far as these can be obtained, photographs, and casts in plaster-of-Paris. Your studies would be devoted primarily to the ethnology of the people, including a thorough study of language and mythology and anthropometric measures. After you have completed your studies, you will return to New York. Your return will be expected approximately in the beginning of 1901. The following year and a half you would engage to work up in the American Museum of Natural History the scientific results of your field work. The scientific results, as well as collections made during the journey, would become the exclusive property of the American Museum of Natural History. No results could be published except according to directions given by authority of the Museum. (Boas to Jochelson, 28 October 1898, AMNH-DA)

In addition, the AMNH would furnish photographic equipment and supplies for the journey and pay for transportation to and from Vladivostok via New York.

Jochelson replied from Bern that he could accept the conditions if the AMNH were ready to consider what he called "changes and clarifications in detail."⁸ These included an increase of his monthly salary to \$150 for the 18 months in New York in 1901-02; provision of additional resources for shipping the collections from the town of Gizhiga on the Sea of Okhotsk to New York; payment of \$100 extra for acquisition of ethnographic literature on Siberia; insurance to be paid by the museum; and some other financial conditions. But far more important were Jochelson's "clarifications" regarding his future rights as a collector and author:

I don't want to process the results of the anthropological research (measurements, masks, etc.) myself, but prefer to leave it to the

Museum to give to an anthropologist to do. . . I would like to evaluate the Koryak ethnographic, ethnologic and linguistic material myself. The finished work which will belong to the Museum will be published under my name. The Yukagir material is mine, I collected it during three years of field work. . . . I can give the old Yukagir information to the Museum on the condition that I can also give the combined old and new material at the same time to the Russian Geographical Society, in Russian (both publications must naturally appear under my name). (Jochelson to Boas, 10 November 1898, AMNH-DA)

He also discussed minor details of purchasing supplies and shipping equipment to Vladivostok (the RAS agreed to pay for the latter) and indicated that it would be better for him to postpone the expedition for two years and complete his doctorate. He was, however, ready to abandon that and leave for St. Petersburg in early December of 1898 if Boas insisted. Boas replied on December 5, 1898. He accepted some of Jochelson's "clarifications" while declining others.

You must consider it as the primary object of your journey (1) to study and to collect among the Koryak, and (2) to make ethnological collections among the Yukagheer. Everything else is secondary. . . . On the whole, your proposed modifications of my propositions seem to imply a fear that this Museum might interfere with your rights as an author and investigator. There is no inclination on our part to do so. On the contrary, we hope that the expedition, when carried out, will materially contribute to your reputation, and assist you in obtaining a satisfactory station in life. (Boas to Jochelson, 5 December 1898, AMNH-DA)

By this time, Jochelson's and Bogoras' participation in the Jesup North Pacific Expedition had already been decided by Boas. But he still had to persuade Jesup that this choice was the best one, even though employing two men was more expensive than one. As Boas wrote to Jesup, "These two men acquired such familiarity with work in that region, that it appeared unwise to employ any one else to do work there" (2 November 1898, AMNH-DA). This new decision, however, implied certain complications:

For the immediate purpose of the Jesup Expedition, it would have been sufficient to collect a certain amount of information on the tribes of the Sea of Okhotsk and of the west coast of Bering Strait, without going into certain details. Mr. Jochelson, however, is not willing to take up work in eastern Siberia unless he can exhaust the field, besides, he asks to be employed for a considerable length of time, and his salary represents a very considerable sum of money. The same would be true of Mr. Bogoraz, although to a less extent. Thus we are placed in the following position: we might adhere to our old plan to send a young man to the region referred to, and try to obtain what we want. If we do so, the work will be done less thoroughly, and not so well as it would be done by Messrs. Jochelson and Bogoraz. Besides, since these two men exist, and as their work is appreciated by European scientists, there is no doubt that efforts will be made to give them an opportunity to carry out the proposed work. . . . If, therefore, we should not employ them, but send another man, we should be exposed to danger of doing imperfect work, which in the course of a few years might be superseded by the much better work. . . . A difficult choice is, therefore, presented to us, in that we need information from the region in question, and that we cannot wisely employ any one but the two Russian gentlemen. (Boas to Jesup, 2 November 1898, AMNH-DA)

It is difficult to say whether this letter was just political or if, in fact, Boas was really impressed by the extensive knowledge Jochelson had of the area and the Natives. In any case, he allowed Jochelson to influence the original plan of the expedition by expanding its area to encompass the Yukagir and "exhaust the field." A semiofficial letter was written on December 6, 1898, proposing that Bogoraz survey the Chukchi beginning in 1900 for 12 to 15 months, on conditions similar to those offered to Jochelson.

A month later, Boas received a letter from Jochelson in Paris. Jochelson accepted all the proposed conditions and agreed to leave Switzerland in September 1899 to start preparations for his departure. He once again reminded Boas of Bogoraz: "It should be advisable that my departure and Mr. Bogoraz' should take place at the same time" (Jochelson to Boas, 4 January 1899, AMNH-DA).

In January 1899 Bogoraz returned to St. Petersburg and began working at the Museum of Anthropology and Ethnography under the direction of Radloff. In the first week of March, Boas received a letter from Bogoraz in which he accepted all the conditions. "I am happy," Radloff wrote to Boas, "that my mediation had such positive results and you can now go ahead with the arrangements for the expedition in Asia" (Radloff to Boas, 27 February 1899, AMNH-DA).

Siberian Expedition Preparations, 1899–1900

For several of the months that followed, the correspondence between Boas and his Russian partners focused mainly on purchasing supplies and equipment for the expedition. Both parties tried to do this as inexpensively as possible; they wrote numerous letters and made dozens of inquiries about the prices of flour, canned milk, barter items, and gifts for local people. The whole plan was beginning to take tangible shape, although the organizers had to overcome all sorts of problems, some of them rather peculiar. For example, the U.S. Customs had no classification entries for "ethnographic objects"; if they were "Specimens of Natural History," no tax was due, but customs officials were not sure. An officer cited a letter by the auditor for the Treasury Department and then presented his own interpretation:

The articles are classified as specimens of Natural History, free, under Paragraph 666 New Tariff. This classification however would appear to be erroneous. In the opinion of this office, the term "Specimens of Natural History" applies only to natural objects, and does not apply to any artificial product or manufacture. . . . As to the "Anthropological Specimens" it is impossible to tell from the description whether they were natural or artificial. . . . I think the Auditor right in his claim that the plaster casts and the Indian ladder are not specimens of Natural History . . . [and are to] be classified under Paragraph 702 N. T. . . . because, in my judgment, Ethnology is a science, viz.; the science which treats of the division of mankind into races, their origin, distribution and relations, and the peculiarities which distinguish them.

If they are to be classified under paragraph 702, then a bond is required. (Official to Winsler, 10 November 1897, AMNH-DA)

At the turn of the century, even customs officers were discussing the definition of ethnology. But, along with answering letters from the U.S. Customs, Franz Boas had much more serious decisions to make.

Where to Go and What to Study

In the shaping of the content and route of the Siberian portion of the Jesup Expedition, the very different backgrounds, training, and experience of Boas and his two Russian partners had unforeseen consequences. In social science research, it is almost impossible to investigate one's ideas in a purely technical manner or even to collect data according to a rigid, standardized questionnaire. The interference of the researcher's personality—the "observer's paradox"—sometimes is so strong that two people who study the same phenomenon might get very different results. What Boas expected the Russians to do was to become his eyes, ears, and arms. They had to go to specific areas, make anthropometric measurements, record folklore texts, collect objects, and return to Boas in New York. He wanted to train them specially for the job. He wrote to Radloff:

My intention is to have both Mr. Jochelson and Mr. Bogoras here for a few months, in order to make sure that the work on physical anthropology will be done according to the same methods, so that our results may be comparable. (18 April 1899, AMNH-DA)

Boas aspired, within the limited funds he had, to carry out the maximum research to both satisfy his scholarly interests and give Jesup and the AMNH as much prestige and publicity as possible. But it became clear from the start that the Russians had their own ideas as to where and how to do research in Siberia.

Jochelson was the first to resist Boas' plan. In a letter quoted above, Radloff informed Boas that Jochelson consented to go "only to the Yukagirs" (Radloff to Boas, 23 February 1898, AMNH-DA), although Boas needed information on the peoples of

the North Pacific coast—the Koryak, Chukchi, and Nivkh. The Yukagir were located too far in the interior to be of special interest, according to Boas' vision of the expedition. Jochelson eventually yielded and agreed to go first to study the Koryak. But even after this incident, he continued to suggest various side trips, such as a trip to visit the ancient Yukagir burial sites. To that, Boas had to answer rather bluntly, "I think that the journey to the ancient graves of the Yukagirs is practically out of the question on account of the additional expense" (Boas to Jochelson, 5 December 1898, AMNH-DA). Then Bogoras proposed a similarly unwelcome side trip. He suggested a route for his expedition that was obviously designed not so much to meet the goals of Boas and the JNPE as to satisfy his personal scientific interests. After consulting with Nikolay Gondatti, the former governor of the area, Bogoras wrote to Boas regarding the route of the expedition:

The best starting point should be Markovo on the river Anadyr . . . [from there] to the Chaun Bay and . . . along the coast to Bering Strait . . . [then] Naukan and Welen, the greatest villages of the littoral Chukchee, [and] return to Anadyr by *baidara* [skin boat] in the next summer. In that way I can visit all the littoral villages of both oceans. (Bogoras to Boas, 22 March 1899, APS-NYPL)

This was an ambitious and clever plan. Bogoras was, quite understandably, more interested in the Maritime (or coastal) Chukchi than in the Reindeer people whom he already knew, so he tried to convince Boas of this plan. He seemed also unaware at that time that Naukan was not a Chukchi village but a Yupik one.

The study of Chukchean language had been made by me before and needs now but for some supplement, the more that in the Chukchee there exist but very scarce difference of dialect. I have also collected materials concerning the material state of life, folklore, rites and myths, family and tribe life etc. of the reindeer Chukchee. In my further study I must firstly complete all these informations and secondly get corresponding investigation of the littoral part of the people. (Bogoras to Boas, 22 March 1899, APS-NYPL)

The timing of the expedition was also disputed. Both Bogoras and Jochelson were busy publishing their materials, and on top of that, Jochelson was planning to complete his doctoral exams in Switzerland. As late as July 1899, Bogoras asked for Boas' consent to postpone the start of the expedition until 1901 (Bogoras to Boas, 9 July 1899, APS-NYPL). But the expedition, for both Bogoras and Jochelson, was obviously too attractive to risk missing the chance. Four days later, Bogoras wrote another letter and said that he would leave the decision in Boas' hands. He was ready to start right away: it was just that 1901 would have been better for him.

Boas was ready to postpone the expedition but was not happy about it. In a letter to Radloff, he wrote:

I have agreed to his [Bogoras'] request to delay his expedition until 1901, although I should be glad to get the whole matter started. . . . If you do not consider the delay necessary, I beg to ask you kindly to suggest to him the desirability of not delaying the expedition any longer than is absolutely necessary. (Boas to Radloff, 8 August 1899, AMNH-DA)

Eventually, the whole matter was settled. Shortly before leaving Switzerland, Jochelson informed Boas that he had convinced Bogoras not to postpone the expedition (20 August 1899, AMNH-DA). In a joint letter 11 days later, Bogoras and Jochelson informed Boas that they had had a conference, that Radloff insisted that Bogoras go together with Jochelson, and that they would come to New York in mid-February 1900. The "mutiny" was suppressed; the Russians were now ready to go at the time and to the area decided by Boas and to become students. "We would like to know how much time will be required to get acquainted with your anthropometrical methods, as well as with other goals of the expedition" (Bogoras and Jochelson to Boas, 31 August 1899, AMNH-DA).

Why was the idea of such an expedition so attractive to both Bogoras and Jochelson? We will probably never know; perhaps they wanted to return as free people and scholars to the land of their exile to prove

something to somebody, or perhaps they believed that this expedition would, as Boas put it, "materially contribute to their reputation and assist them in obtaining a satisfactory station in life," or perhaps they had fallen in love with ethnography.

New Scenario for the Expedition

Boas began advising Jochelson and Bogoras on the literature they should acquaint themselves with before departing. He sent them copies of the first publications on the Jesup Expedition (Boas 1898a, 1898b, 1898c), Hoffman's monograph on the art of the Eskimo (Hoffman 1897), and Petitot's book on the Canadian Indians (Petitot 1886). He also referred them to Aurel Krause's volume on the Tlingit (Krause 1885), to his own *Indianische Sagen* (Boas 1895), to his newly published contribution on Kwakiutl social organization and secret societies (Boas 1897), and to some other books. "The most important literature on the Pacific Coast of North America," Boas added, "is contained in the early descriptions of Veniaminoff [Veniaminov 1846—N.V.], the early Russian missionary, which you will certainly find in St. Petersburg" (Boas to Jochelson, 19 September 1899, AMNH-DA).

After many discussions, a new plan for the expedition was drawn up jointly by Jochelson and Bogoras and approved by Boas. According to this plan, the two Russian participants were to do research on the Koryak as a team. They were planning to go first to the small Russian town of Gizhiga on the coast of the Sea of Okhotsk and spend half a year together working among the nearby groups of Koryak. Jochelson was to take the photographs and anthropological measurements and make the plaster-of-paris masks, while Bogoras was planning to study the Koryak language (using his previous knowledge of the closely related Chukchi). Ethnographic work was to be done jointly, but mostly by Jochelson, since it would be his task to write a book on the Koryak for the Jesup Expedition series. After that, they proposed to go to the Anadyr River together and to share the work among

the Chukchi in the same manner: Bogoras would document the language and folklore, while Jochelson would handle the anthropometry and photography. By the end of spring 1901, Jochelson would go back to Gizhiga to complete the work on the Koryak, while Bogoras would proceed to the Arctic coast and on to the Bering Strait. On their return to the United States, Bogoras would complete two volumes: a study of both languages, Koryak and Chukchi, and a monograph on the Chukchi (for the JNPE series). Jochelson would present the bulk of the photographs and anthropometry and would write a monograph on the Koryak, working from the data collected by both of them. Jochelson also proposed that he write a detailed monograph on the Yukagir and their language on the basis of both existing and new materials (Bogoras and Jochelson to Boas, 30 October 1899, AMNH-DA).

This seemed a good plan, although it was somewhat removed from Boas' original research program for the JNPE Siberian division. In any case, this exact plan did not materialize in the field; instead, Bogoras and Jochelson came to New York, met Boas face to face, and sorted out numerous minor disagreements and misunderstandings. I believe that they must have personally liked each other, for the final plan of the expedition bears visible traces of compromise, collective thinking, and consensus.

In late November 1899, before departing for New York, Bogoras went to the Caucasus to attend to some personal matters, and Jochelson paid a short visit to Zurich. They agreed to meet in Antwerp by the end of the year and informed Boas that they were coming to New York around February 1900.

Formal Contract and the Final Plan

In late March 1900, after Jochelson and Bogoras arrived in New York, a formal contract between Morris Jesup and Vladimir Jochelson was signed [and cosigned, probably later, by another Russian, Alexander Axelrod, a junior friend and assistant of Jochelson and Bogoras, who was hired as the Siberian team field assistant—

ed.]. Under this contract, Jochelson was appointed to take charge of JNPE activities in northeastern Asia. The expedition consisted of four people: Jochelson; Bogoras; N. G. Buxton, a zoologist in charge of zoological collecting for the AMNH; and Axelrod. In addition, the two wives, Mrs. Jochelson [Dina Jochelson-Brodsky, 1864–1941] and Mrs. (Sofia) Bogoras were allowed to accompany the expedition in the field, although the expenses were to be deducted from their husbands' salaries at the expedition's end. The object of the expedition was formulated as "ethnological and biological survey of northeastern Asia, in accordance with special instructions given to you under this date by Professors J. A. Allen, Franz Boas, William Beutenmuller, and L. P. Gratacao" (Jesup to Jochelson, 24 March 1900, AMNH-DA).

Two days later, Boas wrote the letter containing the final instructions. It was a good example of a compromise between the two parties: it combined the original plans Boas had envisioned for the northeastern Asian research and numerous (and often contradictory) suggestions and amendments put forward by the Russian scholars. The document is very carefully worded; every expression, every word, even the order of some words, was evidently the result of many discussions. This final plan was written to satisfy everyone. As Boas stated:

The principal object of your work will be a thorough investigation of the Koryak, maritime Chukchee, and eastern Yukagheer from all points of view, ethnological, linguistical, and somatological. You will use every effort to collect as full information and as full collections as possible from these tribes. Your collections are to embrace, so far as feasible, the whole range of objects manufactured by the tribes enumerated above. You will endeavor to represent fully in your collections objects that are new to science. You will also make special efforts to obtain a good collection of anthropological photographs and plaster casts. You will make studies and collections among the Lamoot, reindeer Chukchee, Eskimo, and Kamchadal if opportunity should offer; but these are not the primary object of the

expedition. You will use your judgment in determining the movements of the expedition in the field, and you are expected to arrange the movements of the party in such a way as will secure the best results. (Boas to Jochelson, 26 March 1900, AMNH-DA)

It seems that after meeting with the two Russians, Boas gained a wider perspective and saw greater potential in ethnographic work in Siberia. Now, instead of insisting that they do only what was assigned to them by the JNPE plan, he tried to exploit the sudden opportunity of learning and acquiring more than he had expected. The rather liberal instructions quoted above as regards the schedule, the route, and the list of Native peoples that the expedition had to explore can be seen as confirmation that Boas' attitude toward the project had changed slightly. Some time later, learning that Jochelson was planning to return from northeastern Asia to St. Petersburg not via New York but by land across Siberia and that on his way he would be passing the land of the Yakut [Sakha], Boas wrote a special letter to Jesup. In it, he stated that, although the Yakut people were, of course, "beyond the scope of the JNPE, it would be a shame to miss such a rare opportunity and not to acquire, with Jochelson's help, his Yakut collection for the Museum" (Boas to Jesup, 26 March 1900, AMNH-L).

"Double-Faced Janus"

In the meantime, all the necessary steps were taken to secure the cooperation of the Russian government. Boas wrote to Radloff in March 1899, "I beg you to inform the Imperial Academy of Sciences of our plans, and to solicit the assistance of the Academy in carrying out the work" (Boas to Radloff, 24 March 1899, AMNH-DA). Letters were also written to everyone concerned. Jesup wrote a special letter to Governor-General Grodekof of Amur Province thanking him for his "valuable assistance" to Laufer and asking for further assistance to Jochelson's team in regard to transportation to Gizhiga (Jesup to Grodekof, 9 March 1900, AMNH-DA). In October 1899 Radloff wrote to Jesup:

I am very glad that the affairs regarding the expedition to North-eastern Siberia are in good shape, and I shall do my best that the Messrs Bogoraz and Jochelson shall receive all possible aid from the Russian Government. (Radloff to Jesup, 26 October 1899, AMNH-DA)

Both Jochelson and Bogoraz received open letters from the Russian government that ran as follows:

All institutions and persons under the jurisdiction of the Ministry of the Interior are herewith commanded to render the bearer of this all possible aid within their lawful powers, to enable him to discharge his mission. [Dated November 11, 1899 and signed Head of the Ministry of the Interior etc., etc. Sipyagin; Director of the Department of General Affairs . . . Trepov.]

Five months later, when Jochelson and Bogoraz were already on their way to Vladivostok, the Russian Ministry of the Interior issued a completely different message (28 April 1900). Confidential instructions were sent to the local Siberian officials in charge requesting that secret surveillance be established to monitor the actions of both Bogoraz and Jochelson. It was stated that, due to their earlier antigovernment activities, it was "entirely unwarranted to render them assistance of any kind in the scientific work assigned to them" (for discussion, see Freed et al. 1988). As Bogoraz put it in one of his letters to Boas several years later, and in a different connection, "this is Russia, you know."

The whole story became known several years later when a Russian-language newspaper, *Osvobozhdeniye* ("Liberation"), based in Stuttgart, published an article entitled "The Double-Faced Janus." The story was actually written by Jochelson himself in January 1903 in St. Petersburg but was published under the alias "Docent." The article was later translated into English for Morris Jesup's attention and information. In a cover letter, Boas wrote:

I think the loyalty of Mr. Jochelson, who knew about all these matters while in Siberia, and the energy and skill of both Messrs. Jochelson and Bogoraz, deserve special commendation under these circumstances. . . . You will appreciate how difficult the work

of both Mr. Bogoras and Mr. Jochelson was made by these secret orders; and the full success of their investigation deserves, for this reason, the highest praise. (Boas to Jesup, 4 March 1903, AMNH-DA)

Epilogue: The Beginning

Five years after the idea of a full-scale anthropological and linguistic expedition in the North Pacific area first struck Franz Boas, the second Siberian party of the Jesup Expedition, led by Vladimir Jochelson and Vladimir Bogoras, was set to leave for fieldwork on the North-east Coast of Siberia.

On May 16, 1900, Jochelson and Bogoras arrived in Vladivostok. Here they met Axelrod, who had preceded them. Everything that had been shipped from Russia and Europe arrived safely, and they began getting the equipment ready. In his first letter to Boas, Jochelson wrote that Governor Grodekov was very obliging and had promised to give them any help they needed (Jochelson to Boas, 20 May 1900, AMNH-DA). Obviously, the governor had not yet received the secret memorandum from the Ministry of the Interior circulated two weeks earlier.

On June 14 Bogoras and his wife Sofia left for Mariinsky Post at the mouth of the Anadyr River on board the ship *Baikal*. About a month later, on July 24, Jochelson and his wife Dina Jochelson-Brodsky (accompanied by Buxton and Axelrod) followed them. The main work of the JNPE in Siberia thus began.

The history of the JNPE Siberian fieldwork in 1900–02, as well as the long and painful story of the publication problems, took place against the backdrop of, and was illuminated by, the many dramatic events of the first third of the 20th century. These included World War I, the three Russian revolutions and the Russian Civil War, the Great Depression, and other milestone events in the history of the two countries (see also Kan, this volume). As such, it deserves to be the subject of a special study and is more than this one paper could hope to encompass.

Acknowledgments

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Notes

1. Part of the Boas-Bogoras-Jochelson-Shternberg correspondence is currently held at the Archives of the Russian Academy of Sciences in St. Petersburg (RAS-J and RAS-B). Some of Bogoras' and Shternberg's personal collections are stored at the Archives of the Museum of Anthropology and Ethnology in St. Petersburg (MAE); Jochelson's collection is mostly at the Institute of Oriental Studies (IOS), St. Petersburg (see the description of the Aleut section of the latter collection in Bergsland and Dirks 1990). Originals of the Franz Boas Professional Correspondence are at the American Philosophical Society (APS) in Philadelphia. Microfilms of Boas' correspondence are available at many institutions; I used the New York Public Library copy (APS-NYPL). The major institution that houses the papers and correspondence related to the Jesup North Pacific Expedition is, naturally, the American Museum of Natural History in New York, in the Library, Special Collections Division (AMNH-L), and in the Archives of

the Department of Anthropology (AMNH-DA).

2. The text of this letter is reproduced in Appendix A of Douglas Cole's paper, this volume—ed.

3. All dates for the Russian letters are "New Style" (referring to the Gregorian calendar that was adopted in Russia in 1918, replacing "Old Style," based on the Julian calendar). For example, this letter from Radloff has two dates: February 11/23, 1898.

4. The usual spelling in English is "Bogoras". In his Russian publications, it is always spelled "Bogoraz" or "Bogoraz-Tan" (Tan-Bogoraz), the latter having being his political and academic pen name since the early 1900s. Judging by his letters of the JNPE years written in English, he preferred that his name be spelled in the Russian way (Bogoraz), although in all his JNPE publications he is listed as Bogoras—ed.

5. A detailed discussion of this episode is available in Freed et al. 1988:12–13.

6. The party's name in Russian was *Narodnaia volia*, conventionally and quite correctly translated into English as "People's Freedom." However, the word *volia* can mean both freedom and will (see Vladimir Dahl, *The Dictionary of Russian*, Moscow, 1956). The name of the party can thus be understood as "People's Will."

7. Two types of political exile were in use in Russia before the Revolution of 1905, both determined either by courts or by the local administrative authorities. Exile *to* a certain area (*ssylka*), usually to Eastern or Western Siberia, meant that one had to live in a small, remote town or village, had to report to the local police every week or month, and had no right to leave the place without special permission. Exile *from* a certain area (*vysylka*) usually meant that one was forbidden to live in the capitals, big cities, or central provinces of Russia but otherwise was free to move. Jochelson, Bogoras, and Shternberg were sentenced to *ssylka*—the worst kind of exile.

8. I quote here from the available English translation of Jochelson's letters, originally written in German. These were translated in 1986 by Renate Khambatta and Laila Williamson; the translation is now kept at the AMNH Department of Anthropology in New York.

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24/ Kwakwaka'wakw (Kwakiutl) woman at Fort Rupert demonstrates cedar spinning for a museum life group, as Franz Boas and George Hunt hold up a backdrop. O.C. Hastings, photographer, 1894 (AMNH 11604)



part 2

THE COLLECTORS: MODERN PERSPECTIVES ON
JESUP FIELDWORK

(Dis)pleasures of the Text

Boasian Ethnology on the Central Northwest Coast

MICHAEL HARKIN

The ambitious project of the Jesup North Pacific Expedition (1897–1902) was historically significant for many reasons: the cooperation between anthropologists and capitalists (see Freed et al. 1988); the expedition's rich legacy of Siberian ethnography (see other papers in this volume); and its contribution to the understanding of important ethnological issues in the North Pacific Rim. However, the ethnographic legacy of the Jesup Expedition on the Central Northwest Coast, by which I mean the area populated by the Bella Coola [Nuxalk], the Oowekeeno, and the Heiltsuk [Bella Bella], is rather meager. The three cultures are closely related, and the latter two possess very similar languages. The poverty of the Boasian record in this region is possibly attributable in part to the practical difficulties Franz Boas had in getting the Jesup materials published. A survey of the archival materials, however, is equally unsatisfying; the American Philosophical Society indexes are surprisingly silent on Oowekeeno, Bella Bella, and Bella Coola material from the Jesup Expedition period.

What we do have are a volume of Bella Coola myths collected by Boas and several Heiltsuk myths collected by Boas' Columbia colleague, the psychologist Livingston Farrand (Boas 1898a, 1898b, 1916:883–8, 1932).¹ Boas, assisted by George Hunt and Harlan Smith, conducted research at Bella Coola from mid-July to late August 1897. Boas and Hunt were occupied primarily with the collection of myths, while Smith made cranial measurements and completed a valuable photographic portfolio of the area (Boas 1898a; see Tepper 1991; Thom, this volume). Farrand, along with George

Hunt, spent about a month from mid-August to mid-September 1897 in the village of Bella Bella; they were briefly joined by Harlan Smith, who made cranial measurements (Boas 1898a).

In 1897, when the research was being carried out, both these societies were undergoing rapid, radical culture change and were displaying renewed cultural and political vitality. After decades of suffering the scourge of introduced diseases, these groups were relatively healthy, their populations were resurgent, and they were enjoying unprecedented prosperity. They were experimenting with new artistic and architectural styles, and they were attempting to reconcile evangelical Christianity with traditional belief systems. They were coming into contact with more than just the evangelical and commercial aspects of European and Canadian society. For instance, the Bella Coola had an unusual opportunity to observe European culture when a group of dancers was invited to tour Germany in 1885–86 (Tepper 1991:142–9). They brought back many new ideas that they incorporated into their culture. Most strikingly, Gothic architectural forms appeared in at least one chiefly house, where spires were used to represent a nearby mountain (Mc Ilwraith 1948:194; Tepper 1991: 7).

The Heiltsuk, in the village of Bella Bella, were engaged in what may be described as a “revitalization movement” based on enthusiastic Methodism (see Harkin 1993).² The Heiltsuk combined Methodist moralism and work ethic with traditional concepts of personal power to create a powerful new ethos relevant

to contemporary problems. Although this resulted in the curtailing of many traditional practices, there was also a large element of syncretism present. Christmas celebrations bore a strong resemblance to potlatches and even, to some degree, to winter dances (ceremonies). Moreover, the self-initiated changes in Heiltsuk society had resulted in a level of prosperity unprecedented in any native community in British Columbia. Local businesses, including a cooperative general store, flourished, providing the Heiltsuk with a reasonable supply of luxury items, as well as staples. While such changes may strike the romantic anthropologist as distasteful, they nevertheless were central to the evolving Heiltsuk identity at the turn of the century (Harkin 1997).

Ironically, the Methodist missionaries, not the ethnographers, are the ones who give a full account of these changes. Although their reports are strongly biased, missionaries such as C. M. Tate, the founder (in 1881) of the Bella Bella mission, were sensitive to cultural dynamics. Tate, along with his wife Caroline, kept a close journalistic record of changes in Heiltsuk culture. Later missionaries, such as the first medical doctor to minister to the Heiltsuk, R. W. Large, were likewise extraordinarily sensitive to a range of issues concerning culture change. It is relatively easy to factor out their biases and to derive a fairly good picture of Heiltsuk culture in this transitional period. Change did not always proceed smoothly; it was often resisted in ways both subtle and direct. The missionaries were, arguably, the very best observers of such things, as resistance was a threat to their authority. By drawing on missionary sources such as diaries, articles published in denominational journals, and membership and financial records, it is possible to gain some understanding of fin de siècle Heiltsuk society (Harkin 1993).

The missionaries were biased against traditional culture, but they were nevertheless engaged in it. Boas, however, harbored a long-standing opposition to missionary activities and a strong distaste when forced to rely on missionaries for linguistic data (Berman 1996:

221–3; Stocking 1974:68–9). In part, this arose from ethical concerns generated by cultural relativism; in part, it reflected an unrealistic methodological stance that asserted several related principles (discussed below)—foremost among them, a positivist assertion that it was possible to be an unbiased and objective observer, in the fashion of the natural sciences. This stance was taken to great lengths, to the degree that Boas systematically disguised the identity of George Hunt and his role in actively generating ethnographic data for the Kwakwaka'wakw [Kwakiutl] and other groups (Berman 1996:228–9). The idea was to reach some overarching, static, ideal type of culture, detached from its pragmatic and socially positioned moorings among real people. This stance proved difficult for Boas' Kwakiutl ethnography and simply unworkable for his ethnography of the Central Coast. Ultimately, and ironically, the obviously positioned observer, such as the missionary—provided he or she is reasonably sympathetic—is more reliable than the objective scientist.

Boasian Fieldwork: Objects and Methods

In contrast to missionary accounts, Jesup materials (and Boasian ethnography more generally) give little sense of a living community in transition. Indeed, Boas' often-affirmed commitment to empiricism notwithstanding, it is difficult to view Boasian texts as transcriptions of actual experience. Of course, Boas was driven by contemporary concerns, such as the evidence for diffusion that myth and physical anthropology could provide. Moreover, the rich legacy of Kwakiutl ethnology, resting on its "five-foot shelf," is not to be dismissed (Darnell 1992:44–5). Although we cannot agree with Radcliffe-Brown that Boasian texts are utterly useless—indeed, the rich ethnographic minutiae of the Kwakiutl work is its great strength—it is undeniable that Boasian materials fail to address any of the important and interesting cultural transformations that occurred under the very noses of the Jesup ethnographers (Berman 1996:216–7; Darnell 1992:41). While it would be unfair to criticize Boas for failing to comprehend

and thematize in his anthropology issues of social change that would not be addressed systematically until the first wave of acculturation studies in the 1930s, we can allow ourselves to wonder why all evidence of history and change was systematically suppressed in Boasian texts. After all, the Bureau of American Ethnology ethnographer James Mooney did produce historically and culturally sensitive work during the very same decade of the 1890s.

The situation is analogous to the position of Alfred Kroeber with respect to California ethnology and ethnohistory. As Buckley (1996) has pointed out, Kroeber consistently underrepresented the importance of history in understanding the contemporary California Indians. What is more, he denied the severity and significance of genocidal policies and actions, which continued even after Kroeber was established in California. Clearly, Kroeber's failure to account for history and culture change, especially in comparison with contemporaries such as T. T. Waterman and J. P. Harrington, was a moral as well as epistemological one. Boas' failing was not primarily moral, as he spoke out against Canadian government actions that were certainly much less destructive than the California genocide. Nevertheless, his systematic ethnography, like Kroeber's, failed to take account of such matters in the way that others, less systematic but more sympathetic, did.

As a young anthropologist working the village of Bella Bella (Waglisla) in the mid-1980s, a village that my professional founding ancestor had visited in 1897 and 1923, I was naturally interested to collect any stories that might persist. But few stories about Boas remained. The only information I ever heard—from several people—was that during his 1923 visit Boas spent much of his time going to the post office. The post office was located several miles from the main village site, over low mountains; a round trip took two hours or more. The large investment of time in this activity illustrates the rather peculiar Boasian methodology—Berman aptly calls it “epistolary ethnography”—which relied heavily on postal services and was devoted

above all to the production of texts (Berman 1996:235).

Three characteristics of Boasian fieldwork are worth examining, for they explain the dearth of information on dynamic social processes, particularly on the Central Coast: framing, textualism, and “Kwakiutlism.” In fairness, these characteristics explain some of the strengths of Boasian anthropology as well, such as the rich legacy of Kwakiutl ethnography and of myths and stories from other groups.

Framing

Framing refers to Boas' method of sorting out the aboriginal from that which was tainted by white contact and, generally, by the modern world (see Goffman 1974:10). In his experiments with ethnographic film and in his principles of museum display, the object is strictly framed; it is recontextualized in an artificial frame that nevertheless purports to represent ethnographic reality (Jacknis 1985). Such simulacra allow for detailed description and (perhaps) analysis of the ethnographic object. They separate the object from its background, the semantic message from pragmatic “noise.” In textual ethnography, this goes beyond the problem of anachronism. Unlike Edward Curtis, who wished to recapture a lost world that was in large part a product of his own and a collective national imagination, Boas observed what was actually present. However, what he observed was only an increasingly small part of the actual world and was, moreover, often dependent on the ethnographic frame itself. As Berman points out, the conditions of production of the Kwakiutl texts were crucial to their existence (Berman 1996:232). Hunt prepared texts in response to questions from Boas and after consulting with several informants. The end product is a distillation of Hunt's interpretation of both Boas' interests and the diverse testimony of informants.

Even when Boas was carrying out his own fieldwork, the object of collecting texts that represented a whole culture's shared beliefs tended to filter out information that was not consistent with such a holistic and traditional picture. This ethnographic Heisenberg

effect is to some degree an unavoidable part of fieldwork. However, the complete reliance on formal interviews of elderly and "traditional" individuals, characteristic of Boas' Heiltsuk and Bella Coola research, severely limited the type and quality of data. The constitution of the ethnographic object by its frame may serve a useful pedagogic or scientific purpose, as in high-energy physics, but it does not provide much information about the everyday world, or about processes common to the readers and objects of ethnographic texts.

The peculiar framing device known as the "ethnographic present" is central to Boasian anthropology. It is a distancing technique, one that "denies coevalness" with the ethnographic object (Fabian 1983). All action, apart from speech, takes place in a Neverland of un-lived time. The ethnographic present is founded on the linguistic and logical paradox of past action that is recorded as if it were taking place in the present, ongoing, and unaffected by normal relations of before and after. Not only are the ethnographic objects not to be found in the same historical epoch as the anthropologist and his readers; their world appears to be temporally constituted outside normal human time and being. In a rhetorical move opposite to Barthes' "reality effect," which rests on the verisimilitude created by temporal sequencing in historiography, the effect of reading texts cast in the "ethnographic present" is distinctly one of unreality (Barthes 1986:141-8).

In the introduction to his Jesup volume *The Mythology of the Bella Coola Indians*, Boas essays an ethnographic synopsis of the Bella Coola that epitomizes some of the distancing tropes employed throughout the Boasian corpus:

The Bella Coola are a small tribe inhabiting the coasts of Dean Inlet and Bentick Arm, two long and narrow fiords situated in about latitude 52° north, in British Columbia. . . . The name "Bella Coola" is a corruption of the word "Bilxula" by which name the tribe is known to the Kwakiutl. There is no term in their own language embracing all the tribes speaking the Bella Coola languages. It seems

that at a former time the tribe was quite populous; but, owing to various epidemics and the introduction of other diseases, its numbers have dwindled down, so that at present time it has been reduced to only a few hundred souls. (Boas 1898b:26)

By various rhetorical means, Boas reduces the complexities of the lives of "a few hundred souls" to the abstract questions pertaining to "a small tribe." The first and most extraordinary linguistic act is a naming. As elsewhere in North America, ethnonyms are assigned to groups that have none, often using terms borrowed from other groups (see Harkin 1988). Such a name is essential for the anthropologist, who, after all, studies tribes. For Boas, this naming was equivalent to designating a *Volk*, with all that entailed.³ Such baptism was necessary to the overall framing strategy.

The *Volksgeist* method originated by J. G. Herder and adapted by Boas relied on a certain degree of abstraction from observed reality. Questions of cultural psychology and group mind superseded the directly observed fact. Although Boas at times strongly defended his approach as one of strict methodological individualism, it clearly was not that (Berman 1996:218; Liss 1996:171). Rather, it gave the researcher license to structure information in such a way as to demonstrate the "genius" of individual cultures (Bunzl 1996:69). Clearly, such a model deflects the immediate interests and concerns of real people in favor of themes chosen by the researcher as indicative of the timeless truths of that culture.

Boas alludes only briefly to the problem of change in Bella Coola society. He is forced to admit that "their numbers have dwindled down," but this does not prevent him from accepting the present as a true representation of the past, nor indeed of systematically doing away with any evidence of temporality. The remainder of the text is constructed after the manner of its inaugural statement: "The Bella Coola are . . ."

Perhaps most striking about the quoted fragment is its emotional detachment from the physical suffering of the people who constitute the purported

subject of Boas' text. Populations in the region declined from infectious disease by as much as 80 percent over the 50-year period prior to Boas' fieldwork (Boyd 1990; Harkin 1994). Although health and population levels were temporarily on the rise again at the turn of the century, fresh memories of great suffering would surely have been expressed to Boas and other Jesup ethnographers. It is a measure of their sangfroid and the perceived duties of the scientist that all this would have rated merely a token reference. Like his student Alfred Kroeber, who spoke of the genocide of California Indians as "the little history of pitiful events," Boas was relatively unconcerned with the hardships and anguish the people had experienced in recent memory (Buckley 1996).

Boas' emotional detachment is in great contrast to the other main observers of Native cultures, the missionaries, who were, if nothing else, *engagé*. On the matter of death and dying (of obvious concern to those professing the existence of a glorious afterlife), we hear the wailing and feel the sorrow of the death of children. A typical example of missionary writings during the plague years is by the wife of the first missionary to the Heiltsuk:

They brought her home, and, seeing that she was seriously ill, we brought her to the Mission House; tried all within our power to restore her to health. But the delirium set in, and after three nights and days watching all that was mortal of Jane lay with folded hands in the sitting-room of the house, there to await Christian burial. One of her last conscious acts was to take her Bible from under her pillow, and kissing it lovingly she exclaimed, "Oh how I love Jesus!" (Tate 1883:111)

The pathos of this passage is representative of missionary rhetoric. It is interesting that Native peoples should have received two sets of white visitors at the same time, with such opposite interests and textual strategies.⁴

Of course this was no coincidence. Boas' detached language represents, above all, an attempt to distinguish his writings from those of others interested in

Native cultures: missionaries, "do-gooders," Indian agents, and so on. Boas professes an interest that is, unlike those of other whites, disinterested. Again, the contrast with the ethnographer James Mooney is instructive; Mooney, in his work on the Sioux Ghost Dance, never considered ethnography and empathy to be contradictory (see Mooney 1896).

Social, temporal, emotional, and geographic distance is indeed essential to Boas' view of anthropology as a science. It is Claude Lévi-Strauss who has most explicitly formulated this position. For Lévi-Strauss, *le regard éloignée* (the distant, or distanced, view) is the sine qua non of anthropology (Lévi-Strauss 1976:55, 1985; Todorov 1988). This would seem especially true for Boas, who felt the need of distancing in his early "psycho-physical" research among the Eskimo of Baffinland. There he hoped to achieve the "simplest possible circumstances" in which to conduct his research into perception of the environment (Stocking 1968:140). Lévi-Strauss—and, arguably, Boas—equated the scientific status of ethnography with "the relative simplification which affects every mode of knowledge when it is applied to a very distant object" (Lévi-Strauss 1976:47). Ironically, it was also Lévi-Strauss who pointed out the connection between such distancing and the legacy of brutal conquest: "Anthropology is the daughter to this era of violence: its capacity to assess more objectively the facts pertaining to the human condition reflects, on the epistemological level, a state of affairs in which one part of mankind treated the other as an object" (Levi-Strauss 1966:126, quoted in Buckley 1996).

Boas certainly thought of himself as a scientist and placed great value on objective methods (Stocking, ed. 1974:11–2). Especially in Boas' time, the distinction between science and hobbyism was crucial. Not only was anthropology just beginning to be professionalized in the United States and Canada, but the strong claims that evolutionary anthropology made to scientific status, based on its connection to evolutionary biology, were not available to diffusionist

Boasians. If Boasian anthropology could not claim to apply to cultural data “the methods and the instrumentalities of the biologist” (according to Otis Mason, as quoted in Stocking, ed. 1974:12), then it seemed that there was little, other than techniques of objectification and distanciation, that prevented it from sinking into an antiquarian bog.

Framing was, above all, an attempt to get at culture as opposed to civilization, local as opposed to universal truths. This distinction is central to the German Counter-Enlightenment and laid the foundation for both Boasian anthropology and German ethnology (Bunzl 1996:20; Stocking 1992:11; see also Kuper 1988:149). It was an increasingly untenable position. The tension between the idea of local cultures as pure founts of the “genius” of a *Volk* and the reality of the colonial and postcolonial world resulted in increasingly radical framing devices. Boas’ earlier published work among the Kwakiutl (1897), ethnographically dense and admitting some questions of change, contrasts with his later, austere publication of “texts” (e.g., Boas 1928). This increasing tension perhaps accounts in part for the irony Krupat (1990) has noted in Boas’ work, which he attributes merely to the tension between theory and fact.⁵ This tension is nicely epitomized in a famous photograph showing Franz Boas and George Hunt arranging a field photograph in Fort Rupert in 1894. Hunt and Boas are holding up a backdrop behind a Kwakiutl woman dressed in traditional attire, spinning cedar (Fig. 24). The backdrop hides a picket fence and Victorian frame house, which would have “spoiled” the shot (see Berman 1996:237).

Textualism

Textualism, a quality of all Boasian anthropology, is a type of framing that masks itself. Textualism is a strategy designed to quarantine the object from lived reality. Texts are presented as if unmediated, as if the ethnographer has done nothing other than record and publish texts that exist independently. The role of the anthropologist in eliciting the texts, and the role of the

narrator in creating and performing them, are suppressed. Above all, the role of translation, both cultural and linguistic, is denied. The mediation provided by “informants,” and especially by the supremely mediational figure of George Hunt, is never fully acknowledged (Berman 2000). These allegedly unmediated “genuine, difficult, confusing, primary sources” (Sapir, quoted in Darnell 1992:42) constituted the foundation of linguistic and ethnographic description and analysis.⁶

And yet texts were thought to be more than metonymic fragments of a culture. They were metaphors of that culture, standing for a culture in toto. The idea of the text is little changed from that of the brothers Grimm, who saw folktales as the texts that would reveal *der Geist* (the spirit) of the *Volk*. Texts were viewed as standing in an “organic” relationship to society itself (Ziolkowski 1990:108). They revealed a distinctive genius that, as Hegel believed, animated all aspects of society and through which one could approach specific social institutions (Ziolkowski 1990:14). Wilhelm von Humboldt formulated this connection between text and *Geist* most explicitly; for him a “radical identity” obtained between language and “the ideal totality of spirit” (Steiner 1992:86). It was this Humboldtian concept of language as text that framed the basic problematics of Boasian methodology (Bunzl 1996:69–70).

Boas, of course, realized that there were other expressions of culture, other types of data he might collect, but these were, in this sense, supplemental to the texts, which would reveal all. In his Kwakiutl ethnography Boas did indeed collect and publish data on a large range of matters, in large part to bolster his diffusionist arguments on descent, totemism, and kinship (Berman 1996:215–7; Kuper 1988:135–40). These data were given a form which mimicked the canonical myths that he and Hunt also collected by systematically erasing traces of their construction. This was not the case, however, for the Heiltsuk and Bella Coola, for which Boas provided ethnographic descriptions—

ranging from very brief to nonexistent—appended to the texts.⁷ Paradoxically, he comments that Heiltsuk culture had “practically disappeared” as he was collecting the texts—a statement that calls into question both the usefulness of his concept of culture and the posited connections between culture and myth (Boas 1928:ix).

In his study of myths, Boas laid the foundation for the modern anthropological culture concept, although, as we have seen, this idea was borrowed directly from the German *Volksgeist* tradition (Bunzl 1996:21–9; Stocking 1968:214). In examining myths, the anthropologist gained access to a “deeper” level of culture that was partly unconscious, unrepresented, or underrepresented in manifest behavior, perhaps even the remnant of elements of culture that had disappeared (Jacknis 1996:198). Moreover, myths provided the basis for ethnological comparison and even the possibility of reconstructing histories of the region (Stocking 1968:206). By collecting complete sets of tales from different cultures in a region and statistically tabulating the results, Boas believed that he could answer all important questions about culture contact and diffusion (Boas 1896; Stocking 1968:207–8).

This concern with myth has become characteristic of American anthropology in general, and yet the underlying Boasian assumptions have attenuated considerably. For Boas, the burden placed on myth is such that it is made to bear the entire weight of a culture. In practical terms, this meant that for the non-Kwakiutl cultures Boas studied or on which he commissioned studies, myth is the only data published, even if other sorts of data were collected. When Boas and Hunt returned to Bella Bella in 1923, they collected a variety of data on religion and social organization (Boas 1923). Several hundred pages of notes deal with beliefs and practices that were rapidly disappearing, especially the Winter Ceremonial. Very little of this material, however, was published (see Boas 1924, 1928, 1932).

The only justification for the view that myth stands for culture in toto—which, if never expressed so baldly,

was nevertheless the operating principle of Boasian research—is a form of neo-Kantian idealism that subordinates all factors to mental ones. This is seen most clearly in the work of the German psychologist Theodor Waitz, which was read and cited extensively by Boas and Boasian anthropologists (Smith 1991:49). Waitz held that human cultures were united by a shared psychic unity but that important cultural differences were expressed in myth. Cultural variation was a product of environment, history, and the existence of individual geniuses—ideas clearly influential in Boasian anthropology, although Boas preferred to talk about *the* genius of culture, in the Humboldtian vein.

While not evident in all aspects of Boas’ work, these ideas permeate his research on myth, which was strongly influenced by other anthropological idealists, such as Bastian and Tylor (themselves influenced by Waitz), who were interested in the “psychic life” of primitive peoples (Bunzl 1996:49–51; Stocking 1968:152, 207). It is on this ground that Boas and Lévi-Strauss, so different in other respects, meet. Like Waitz, Boas, in his desire to distance himself from the racist elements of German romanticism, exaggerated the significance of myth as a mental phenomenon in the constitution of culture (Smith 1991:50; Stocking 1992:92–113). He was so engrossed with the collection of myths that he viewed performed culture, such as the Winter Ceremonial, as a hindrance to the collection and transcription of texts (Jacknis 1996:199).

The problems with such a view from a philosophical position have been addressed repeatedly in the social sciences. For present purposes, it is appropriate to address the issue on a more pragmatic level. The relation between myth and social change is worth exploring, for my initial critique of Boas rested on his habit of ignoring dynamic processes.

Myth may give us a sort of “window” into the past, as Boas says, in the sense of providing data on migrations and diffusion, but this is Ratzelian history on a very large scale that is not likely to be relevant to the people telling the myths. Using the very Boasian

concept of culture as it has been adapted by modern American anthropology, we can say that it is precisely an *acultural* history that is thus provided. Oral tradition can, indeed, provide data and insight into remembered historical events on a human scale, a truly cultural history (Harkin 1988). There is, however, a time lag of a generation or more between the event itself and the appearance of a myth—as opposed to anecdotal narratives—about the event. Moreover, as time passes, the myth becomes more “mythlike,” more canonical, and less anecdotal. After a few generations, the new myth may be indistinguishable in form from other myths (Vansina 1985:24).

A methodological problem arises. If researchers are interested only in collecting “texts,” or canonical myths, they will entirely miss the embryonic myth that speaks of relatively recent events and changes. Moreover, they will deny themselves the opportunity to study the process of myth-making and its relation to changing cultural contexts. How many of the “idiotic stories” Boas complained about (as quoted in Stocking 1968:204) were such incipient myths, we cannot know. We do know that stories depicting the arrival of the white man, the effects of European disease, the fur trade, and Native warfare were in circulation at the time and constituted the most important means of understanding and coping with change available to the people of the Central Coast. The Boasian failure to treat these materials seriously calls into question the *Volkgeist* conception of texts and culture that Boas bequeathed to modern anthropology.

Kwakiutlism

A third critique of Boas applies especially to his work with the Heiltsuk and Oowekeeno. Boas' ideas about ethnic groups and boundaries revolve around a central feature of his ethnography, which we may term “Kwakiutlism.” This is problematic in two senses. First, the term “Kwakiutl” does not properly denote even the groups that it primarily refers to—the Kwak'wala-speaking people of Fort Rupert, Alert Bay,

and adjacent mainland and island groups. The Alert Bay group has adopted the ethnonym Kwakwak-a'wakw. These various groups do not recognize the common identity that is implied in the use of the ethnonym “Kwakiutl.” A second, related problem is in the extension of the term to incorporate all the northern groups speaking North Wakashan languages, including the Heiltsuk, Haisla, and Oowekeeno. It is impossible now to eliminate the term “Kwakiutl” from our vocabulary, but I will use it selectively to refer to the core groups that Boas studied.

The ethnography of the Kwakiutl was Boas' life work. As such, it is understandable that the Kwakiutl constituted a fixed point of reference for him and that he would compare other Northwest Coast groups with them. It is even unsurprising that he would accept the Kwakiutl view of the social landscape and their central place in it. As Buckley (1989) has cogently argued with reference to Kroeber's Yurok-centrism, the assumption that the group an anthropologist studies is in some way central is borrowed from that group's own ethnocentric self-assessment. I would add that this intersects with the ethnographer's egocentrism to create a powerful concept that is reinforced both objectively and subjectively. While Kroeber's Yurok became a cultural climax, Boas' Kwakiutl became a cultural empire.

Like any empire—the German, for example—the Kwakiutl (as an ethnographic concept) could only “expand” at the expense of their neighbors, the Heiltsuk and Oowekeeno. This augmentation was made on the basis of points of ethnographic correspondence. There are indeed a number of important similarities between Kwakiutl and Heiltsuk cultures. Most clearly, the Winter Ceremonials in the two cultures share many elements. In large part, this is because the Kwakiutl borrowed many elements from the Heiltsuk, including the *hamatsa*, or cannibal dance (Boas 1966:258, 402). This diffusion is attested by oral traditions prominent in the region, as well as by Boas' own data. However, the Kwakiutl Winter Ceremonial is somewhat different in that it combines two distinct traditions into a single

performance: the *tsaiqa*, or shamanic dances, and the *dleáa*, or crest dances (Boas 1924). The Kwakiutl performance loses the dialectical element so obvious in the Heiltsuk version. This also strongly suggests a north-to-south direction of diffusion, as Boas himself readily admits (Boas 1924).

Despite his awareness that the Heiltsuk, far from being peripheral to the culturally climactic Kwakiutl (to borrow Kroeber's terminology), were correctly seen as the originators of much that the Kwakiutl had borrowed, Boas still insisted on referring to them as "northern Kwakiutl" and on viewing them officially as the Kwakiutl's poor relations. Even when faced with the seemingly insurmountable obstacle of language, Boas failed to grant the distinctiveness and "genius" to the Heiltsuk that he does to other groups such as the Bella Coola.

The boundary between Kwak'wala and Heiltsuk is one of language, not dialect. They are both members of the North Wakashan subfamily, along with Oowekyala and Haisla. The two languages are approximately as close as Dutch and German; there are a large number of cognates, but little mutual intelligibility. It is a testament to George Hunt's linguistic skills that he was able to communicate at all with Heiltsuk consultants, even though much of the 1923 fieldwork was conducted in English (Boas 1923). The transcription of Heiltsuk terms reflects a consistent Kwak'wala bias. While Boas acknowledges these difficulties, he never admits that they cast doubt on his Kwakiutlist assumptions (Boas 1924). Language, in theory, is not itself sufficient to constitute cultural boundaries, but it is significant that nowhere else on the Northwest Coast does Boas see cultural wholes not coterminous with linguistic boundaries.

In fact the Kwakiutl constitute a special case in which the judgment of cultural boundaries was a priori. The Heiltsuk certainly do not consider themselves to be Kwakiutl. If Boas had asked them, he would have learned that they consider themselves to be closely related to (although not identical with) the Bella Coola,

Oowekeeno, and Haisla. So, if Boas wanted an example of strong cultural affinity crossing linguistic boundaries, the Heiltsuk and Bella Coola provided such a case. He mentions a number of similarities between the Bella Coola and the "Kwakiutl" but never fully examines the issue of Heiltsuk and Bella Coola affinities, apart from the Fort Rupert tribes (Boas 1898b:124-5). Certainly, there would be much more justification for considering these two groups to be a "single culture" than for thinking of the Heiltsuk as Kwakiutl.

There is a fundamental epistemological problem underlying the designation of groups as cultures. Since Fredrik Barth's important work, modern anthropologists need no longer trouble themselves with finding perfect matches between social groups and cultures, even in tribal societies (Barth 1969). However, for Boas ethnic boundaries enclosed unique and autonomous lifeworlds, replete with their own modes of thought, their own "genius," revealed especially in their myths. This is the relativism of Herder and the German Counter-Enlightenment (Berlin 1991:37-9). The danger of strong forms of relativism is, of course, solipsism. Certainly, the various groups Boas encountered on the Northwest Coast were very different from European cultures, and Boas' relativistic assumptions could easily be justified in this context. However, could each individual group be its own self-contained lifeworld, in opposition to all others? Boas himself was never clear on this; he seemed to waver between ideas of the genius of cultural wholes and the diffusion of cultural traits (Liss 1996:171-5; Stocking, ed. 1974:4-6, 1996).

Designating a group as "a culture" was something that could be done only after the analysis of cultural elements and their paths of diffusion revealed that this "accidental accretion" resulted in "an integrated spiritual totality that somehow conditioned the form of its elements" (Stocking, ed. 1974:5-6). In the face of this rather paradoxical criterion, Boas seemed to fall back on two basic strategies: resorting to linguistic boundaries as de facto ethnic boundaries, and establishing something like Barth's "plural societies." The first, more

common, strategy acknowledged the genius of individual groups, while the latter was a useful way of looking at cultural similarities and borrowings.⁸

The latter was applied to the case of the Kwakiutl and other North Wakashan groups. Ironically, this represents potentially the greater theoretical advancement. In his construal of the inhabitants of 200 miles of British Columbia coast as “Kwakiutl,” Boas erred in a number of respects, but the concept of individual groups speaking different languages yet sharing an overarching culture is a valuable one. Of course, the idea is not original but has precedents in the German geographic tradition, especially in Friedrich Ratzel’s concept of *Lebensraum*, or “living space” (Smith 1991:219–33). In fact, this area was a poor candidate, since a variety of cultural differences in, for example, descent, marriage, and kinship pertained among the Wakashan-speaking groups. The problem went beyond a poor application of the concept; rather, it lay in Boas’ failure to comprehend that this concept of culture was different from the idea of a distinctive cultural “genius.” To call the Heiltsuk Kwakiutl is absurd; to say that there is a North Wakashan cultural sphere is not absurd and is, moreover, empirically testable. Although the results of such testing would be less than reassuring, the idea could be usefully applied to other groupings such as the Heiltsuk, Oowekeeno, Bella Coola, and Haisla or the Gitksan, Nishga, and Coast Tsimshian.

It is unfortunate that unreflective “Kwakiutlism” caused Boas to give short shrift to Central Coast groups. The paucity of Heiltsuk and Oowekeeno ethnographic material that we have, especially from the Jesup period, is due in large part to the assumption that these groups were in fact Kwakiutl. As the Kwakiutl had been treated extensively in earlier works (e.g., Boas 1897), there was no need to provide another complete ethnographic corpus.

Conclusion: The Central Coast as a Limit Case of Boasian Anthropology

It should be clear from the foregoing discussion that

Boas’ concepts and methods of anthropology were tested and found wanting on the Central Coast. This is only partly because we have the rich Kwakiutl material with which to compare it. The contradictions and weaknesses inherent in many of the concepts borrowed directly from the German Romantic and liberal social scientific traditions, and indeed the paradoxical manner in which Boas applied some of these ideas and methods, became more evident in his peripheral work. Boas as the ethnographer of the Kwakiutl was guided in large part by praxis—by his pragmatic association with the Kwakiutl—although this dimension was systematically suppressed in his published work. In peripheral regions, such connections were lacking, and he was forced to fall back on “first principles,” which included the idea of objective science, the privileged place of texts, the autonomy of cultural wholes, and the idea of culture as a mental phenomenon, all borrowed directly, and with little change, from their German sources.

German social theories of the middle to late 19th century were constructed within a specific political context, against the background of two broad issues: the Counter-Enlightenment revolt against French universalism, and the unification of Germany, with the attendant problems of minority populations, especially Poles and Jews. Boas imported these theories into the new world of American anthropology, in many cases with relatively little self-awareness of the fact. The political context of North American internal colonialism was much different from that of post-1848 Germany. It is only logical that tools honed in the study of Thuringian peasants would be found less than optimal for the study of Northwest Coast Indians.

Boas’ strengths as an ethnographer, and especially a linguist, which were unparalleled in anthropology at least until Malinowski, were somewhat undermined in the case of the Central Coast by these theoretical weaknesses and contradictions. It would be presentism of the worst sort to find fault with Boas for not operating with the full complement of modern anthropological

concepts and methods, some of which he helped to develop. This, however, does not preclude a critical reading of his work, or a comparison of his work on one group with that on another. By any reasonable standards, Boasian ethnography on the Central Northwest Coast will be found wanting, instructive as the failure may be.

Notes

1. Farrand later became a university administrator and was president of the University of Colorado and of Cornell University.

2. In calling the Heiltsuk embrace of Methodism a “revitalization movement,” I am extending the sense in which the term is generally used. Several elements, however, suggest the usage: the importance of Native “prophets” and preachers, the centrality of ideas of disease, health, and purity in both missionary and Native discourse, and, of course, the background of sickness, cultural dislocation, and rapidly changing morals against which the movement appeared. By the standards of such things, this movement was quite successful.

3. Boas’ designation of the Bella Coola as a *Volk* is especially interesting in view of the fact that he did not ascribe such status to the Heiltsuk.

4. On the rhetoric of missionary writings, see Harkin (1993:7–10).

5. Pragmatically, the shift in the nature of Boas’ Northwest Coast publications reflects his attenuated engagement with the Northwest Coast—his personal distancing—and his practice of publishing George Hunt’s materials with little editorial change.

6. This belief in unmediated perception of truth, with its roots in Reformation theology, is characteristic of German Romantic philosophy, especially that of Johann Gottlieb Fichte (see Berlin 1991:190–1).

7. Boas provides a single systematic, although extremely brief, description of Heiltsuk social organization (Boas 1924:329–32). The unpublished field notes from Boas’ and Hunt’s second, post-Jesup, visit to Bella Bella (Boas 1923) are a rich source of data on social structure (although not on social change).

8. This opposition between culture and re-

gion—between *Volk* and nation—was precisely the central political problem in the Germany of Boas’ youth (see Smith 1991:94–5).

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25/ Kwazi'nik, a Nlaka'pamux woman from Spences Bridge, British Columbia, 1897. Harlan I. Smith, photographer (AMNH 11661)

Kwazi'nik's Eyes

Vision and Symbol in Boasian Representation

BARBARA MATHÉ
AND THOMAS R. MILLER

The photographer crouches behind a tripod, head under a black cloth. Emerging, he steps forward and closes the lens, sets the shutter, loads the film holder into the back of the camera, and pulls the slide away from the plate. When all is ready, the photographer stands next to the apparatus, reviews the scene, makes any adjustments, and issues last-minute instructions. Finally, the photographer presses a cable or pulls a string, the shutter is released, and the picture is taken. In the instant of exposure, the shutter opens, and the mechanical eye meets the gaze of the subject. What the camera records is the subject watching this photographic performance (Fig. 25).

Now imagine the scene through the eye of the subject, a Nlaka'pamux woman named Kwazi'nik. The reflected image of Harlan Smith—the photographer—and his tripod can be seen in her eyes (Fig. 26).

Collecting Images

The photo was taken in 1897 at Spences Bridge, British Columbia, as part of Franz Boas' continuing collaboration with the British Association for the Advancement of Science (BAAS) in describing the physical and human geography of western Canada. That year Boas combined his anthropometric work for the BAAS with a new and ambitious project for the American Museum of Natural History (AMNH)—the Jesup North Pacific Expedition.

Between 1897 and 1902 the expedition produced some 3,400 photographs.¹ The pictures were sent from the field to Boas in New York, where they became part

of the AMNH collections from North Asia and the Pacific Northwest of North America. The visual information gathered by the expedition's photographers includes scenes of daily and ritual activity, collected artifacts shown in use, architecture, landscapes, and people wearing traditional costumes. The largest group of images consists of "physical types," photographs in which individuals were pictured from various angles (usually front, side, and three-quarter views; see Fig. 27). These portraits were intended to complement physiognomic measurements, casts, and bones and help establish an anatomical databank of "racial" characteristics. Boas' instructions emphasized photography among the varied field activities. He directed Waldemar Jochelson, leader of the Siberian side of the expedition, to stress physical anthropology, charging him with "special efforts to obtain a good collection of anthropological photographs and plaster casts" (Boas to Jochelson, 26 March 1900, AMNH).²

Morris K. Jesup, president of the AMNH and the expedition's patron and namesake, wanted a sweeping, illustrious scientific achievement for the museum. The search for the first Americans' racial origins had caught the public imagination. In a quest to prove the hypothesis that the first Americans had migrated across the Bering Strait from northern Asia, Jesup found a project of grandeur and scope to suit his Gilded-Age ambitions. As a scientist, Franz Boas was more interested in reconstructing the histories of tribes to demonstrate relationships and historical contacts between North Asians and American Indians.

Boas and others were convinced that colonial incursions into the indigenous societies of the North Pacific had brought traditional cultures to the verge of disappearance. Boas' fieldworkers made a conscious effort to record or reconstruct traditions as remembered from the past. The combination of artifacts, texts, photographs, wax-cylinder recordings, casts, and physical measurements was intended to form an encyclopedic body of data. The collections were chosen to illustrate as many facets of traditional peoples and cultures as possible. As a tool linking the expedition and exhibition phases of the museum enterprise, photography was a valuable means of documentation and re-creation.

Boas' primary goal was accumulation—the collection of racial, cultural, and linguistic information of all types on a massive scale. This project of salvage ethnology was a response to the social conditions of modernity that threatened traditions. Although removing cultural artifacts from their contexts may have hastened the onslaught of change, science could at least record and preserve the past even as it was being effaced (Gruber 1970). The urgent efforts of the photographers to preserve images of a vanishing past convey the anxiety of salvage anthropology, frame by frame. As Smith wrote to Boas from Eburne,

I got the explanation of the house posts I bought as well as they could give them. The large one is interesting the man's figure they say is simply an ornament or a carving made to be a carving and had no meaning. . . . They don't seem to know as much of the old times as we wish they did. (17 May 1898, AMNH)

The museum photographers composed and collected scenes whose corresponding realities they did not expect to survive. Embedded within these idealized, fragmented, metonymic images of culture were visual symbols of native tradition, heritage, and identity. These distinctive features were chosen to represent cultures not only as they then existed but in an imagined and reconstructed “ethnographic present,” situated in the past and staged for the future.

Dictated texts, sound recordings, photographs, and head casts—all objects that in some sense were created by and for science—can be thought of as documentary collections which augment and explain collections of “found” objects (a category that includes most collected art and artifacts, as well as human bones). Although both types of collection depended on a complex negotiation of collaboration and coercion between anthropologists and Native subjects in the colonial-era encounters of the late 19th and early 20th centuries, under certain circumstances documentary ethnographic collections might have allowed participating Native artists and informants a more active voice in deciding how they wished their cultures to be represented. Today, when North Pacific peoples and their cultures have not only survived but are growing more numerous and stronger in the expression of their unique identities, both documentary and “found” collections constitute a powerful and potentially contested resource for the reanimation and reinvention of traditions.

The Jesup Photographers

Boas was an enthusiastic proponent of modern technology in fieldwork, advocating the use of recording devices such as the camera and the phonograph to document cultural traditions. He had studied photography as a university student in Germany and, from his earliest solo trip to Baffin Island, had made use of photographic equipment. Boas himself spent only about four months in the field on the Jesup Expedition, during the summers of 1897 and 1900, and no photographs are attributed to him personally. His ethos, however, pervaded the entire enterprise.

Harlan Smith, then a young employee at the AMNH, was the principal photographer on the North American side, producing more than 500 images on the Northwest Coast.³ He was already familiar with Boas' methodology and ideas on anthropological representation, having worked with Boas and George Hunt (Boas' principal collaborator) at the 1893 Chicago

World's Columbian Exposition. In correspondence from the field, Smith referred to Boas' 1894 work with West Coast photographer O. C. Hastings as a precedent.⁴ Hastings was also hired for the Jesup Expedition in 1898 and worked as an assistant to Smith in Fort Rupert. Gerard Fowke took archaeological pictures in Victoria, British Columbia, in 1898, as well as a small number of images in Siberia and the Russian Far East. Ethnologist Roland Dixon photographed Quinault and Quileute individuals as part of his fieldwork for the expedition in Washington State in 1898.

Two individuals on the American side of the expedition, George Hunt and James Teit, proved invaluable because of their close ties to Native communities; their influence on the photographic work of the Jesup Expedition was immeasurable. Hunt, the son of a British father and a Tlingit mother, was raised in the Kwakiutl [Kwakwaka'wakw] community of Fort Rupert, British Columbia. He worked closely with other members of the expedition team, making their encounters with Indians more relaxed and perhaps more revealing. James Alexander Teit, an immigrant from the Shetland Islands who lived in Spences Bridge, was married to a Nlaka'pamux woman, Susanna Lucy Antko. Kwazi'nik, who posed for Figure 25, was Antko's sister (Wendy Wickwire, personal communication). Teit's insider status allowed him to collect information not easily available to others (see Thom, this volume). Although Teit took up the camera only after the Jesup Expedition years, Hunt started sending photographs back to Boas in New York as early as 1901, and he later went on to produce an important body of pictures (Cannizzo 1983; Jacknis 1985).

In Siberia, most of the photographs were taken by Waldemar Bogoras, Waldemar Jochelson, and Dina Jochelson-Brodsky. [Alexander Axelrod, the Siberian team assistant, probably also took several photographs—ed.] Jochelson wrote to Boas in 1901 about his pictures of Koryak and Tungus, "Half of my photographic plates are of anthropological subjects"—i.e., physical types. Jochelson-Brodsky, who interrupted her

medical training under Rudolf Martin in Zurich to accompany her husband on the expedition, took the anthropometric measurements and assumed responsibility for much of the photography as well. Jochelson wrote to Boas in the summer of 1901, "Mrs. Jochelson has developed the plates and done the other photographic work and acts now as my secretary" (3 August [22 July, old style] 1901, AMNH).

Dina Jochelson-Brodsky measured the faces of some 720 Koryak, Tungus, and Sakha [Yakut] men, women, and children. In addition, she produced anatomical measurements of more than 120 Tungus, Sakha, and Yukagir women's bodies. Together with her photographs and plaster casts of heads, these data formed the basis of her dissertation in medical anthropology, which she eventually completed in Zurich. During the years of the Jesup Expedition, epidemics caused widespread population decline among the Koryak, Chukchi, and Yukagir, so opportunities to measure and photograph individuals were limited.⁵ In a 1907 journal article based on her dissertation research, Jochelson-Brodsky reported that conditions had severely constrained her work:

Unfortunately, I was not able to make special women's measurements of the Koryak. We lived with the Gizhiga Koryak around Primorski region the entire winter of 1900-1901. . . . My husband, myself, the interpreter and other assistants worked in our tight, small canvas tent, heated by a little iron stove. Faced with such arrangements it turned out I was not able to produce special measurements of Koryak women. (Jochelson-Brodsky 1907)

On both sides of the North Pacific, additional pictures were commissioned from local professional photographers. In the Amur River region of the Russian Far East, Jesup anthropologist Berthold Laufer's dismal attempt at photography prompted Boas to urge him to hire a professional photographer instead (Kendall 1988).

Boxes of Light

The elaborate performance of the view camera formally staged and framed the relation between

anthropologist and subject, visually marking the inherent power imbalance that was at other times muted by friendly and casual exchange. The manipulation of scenes before the lens was a collaborative act of theater, a performance engaged in by foreign guest and Native host with varying degrees of coercion and cooperation. The extent to which the composition of pictures was designed and controlled by the photographers and their subjects depended on factors that included the familiarity and relative status of photographer and subject, the didactic purpose of the photograph, lighting and weather conditions, and the technical limitations of the apparatus. The project of salvage anthropology itself was often one of complicity between subject and collector to dramatize tradition. To represent a culture to the public, an image had to be reconstructed in the museum; frequently, this image was in turn based on a scene deliberately composed in the field.

Turn-of-the-century technology imposed strict limitations on field photographers. Correct exposure generally required subjects to hold still in well-lit and carefully arranged poses. The slow film of the period and the large format of the view camera required either strong light or slow shutter speeds for good exposure. A tripod was almost always needed. Most of the photographs were taken outdoors. Although hand-held Kodak box cameras had been in use since the early 1890s, they were mostly relegated to amateur use. Instead, large view cameras with glass-plate negatives, capable of fine detail, were chosen for the expedition.

While basic provisions like food and clothing could be obtained locally, specialized supplies and technical equipment had to be requested by post, shipped from New York to Vancouver or Vladivostok, cleared through international customs, stored in repositories, picked up, and finally transported to field sites. Some shipments never arrived, and others languished in warehouses for months. Writing to museum clerk John Winsor from Victoria in July 1897, Harlan Smith pleaded emphatically:

Please trace at once the phonograph cylinders and the photographic plates sent here to Dr. Boas from the museum in May. They are not here and as a consequence I have to pay big British Columbia prices for photo plates and to do without the phonograph cylinders. I have worked every means to get them from early morning. I have been to every depot, customs and express. This loss is a very serious matter to this year's work. I am bending every effort to try to secure them from some where before my steamer sails. (Smith to Winsor, 30 July 1897, AMNH)

Almost a year later, during the second Jesup Expedition field season, Smith wrote to Boas from Fort Rupert:

At last the photographic plates, sent out here in 1897, have reached me and we have used some of them but find all the pictures taken with them failed. It is too bad they will be a dead loss on our hands. I will try one from each box and so try to use them. If one is good we will try others in the box. They are all speckled. I suppose caused by age or moisture while lying a year at Victoria. (Smith to Boas, 22 June 1898, AMNH)

The temporal and spatial constraints of photography's fixed vantage point and moment of exposure could be partially compensated for by picturing a subject from several angles in succession. In photographs, sequences could string moments together, enhancing time, and panoramas could extend the space of the camera, overcoming the boundaries of the picture frame. These techniques were used to broaden the parameters of the medium, to capture landscapes and views that could not be contained within the confines of an individual photograph (Figs. 28, 29). The conventional front, side, and three-quarter views of 19th-century physical-type photography provide a classic illustration of this approach.

Perceived and Represented "Types"

The search for "types" in descriptions of people was the primary scientific mode of assessing racial and ethnic characteristics in the late 19th century. Amassing physical anthropology data in the form of skeletal material, casts, measurements, and photographs provided

crucial evidence for the racial component of Boas' triadic model of race, language, and culture. Yet much of the physical anthropology data collected on the Jesup Expedition, including Boas' voluminous anthropometric records, remained unanalyzed for more than 80 years (see Ousley and Jantz, this volume).⁶

Early in his career, Boas had been concerned with the effect of the observer's perceptual bias on typology and classification. His physics thesis at the University of Kiel, completed in the early 1880s, dealt with the role of perception in determining variations in the color of seawater. Ranging widely across the German division of scholarly disciplines in his studies, the young Boas evinced a keen interest in methodology, initially proposing a thesis on the problem of random errors in scientific investigations. When this topic was rejected by the faculty at Kiel, he took up the assigned problem of seawater with little enthusiasm, encountering great difficulty in accurately recreating minute natural differences under laboratory conditions (Cole 1999:38-62). In a sense, his efforts showed that scientific errors were not merely random but were often induced by the artificial character of the scientific setting or by unrefined laboratory methods of reconstructing real-world conditions. The notion that the bias of the observer was among the most prominent and deterministic of these effects would later profoundly influence Boas' construction of cultural relativism in his seminal 1889 essay "On Alternating Sounds."

A strikingly similar orientation is reflected in the discussion of the distribution of colored sticks in his 1922 article "The Measurement of Differences between Variable Quantities." In the human sciences, however, the basic framework of classifying data into morphological types was immensely complicated by the particular historical and environmental variables of human migration and intercourse. From the time Boas resigned from the ANMH in 1905 until his death in 1942, he gradually tempered his insistence on the quest for universals of human behavior and fixed racial categories in favor of a historical method that placed local

conditions above universal or evolutionary stages of social development (Stocking 1974:12-15). In contrast to the magisterial certainties of structural functionalism and the rigid hierarchies of social-evolutionary theory, Boasian anthropology developed in a more reflexive mode. As with seawater or colored sticks, the perception and classification of human subjects depended on the point of view of the observer. This counter-social evolutionary position was manifest not only in Boas' physical anthropology but also in his study of representation in cultural artifacts.

For Boas, mere visual qualities could be dangerously misleading if taken as guides to understanding the meaning of cultural objects or physical evidence. When comparing objects of similar form collected from neighboring tribes, he repeatedly cautioned that their true significance could be found only in the context of their originating cultures. Usage and lore, not external similarities of form, were the keys to comparison and classification. The method of museum display he developed between 1886 and 1905 depended on narrative scenes depicting the life of particular cultural groups more than on grouping visually similar artifacts from disparate regions together in exhibit cases.⁷

Boas' cautious analytical attitude toward the extraction of meaning from form was central to his vision of museum display. He concluded a 1904 brochure for AMNH visitors by noting that objects with the same form carried different meanings for different Indian tribes. "This seems to indicate," he wrote, "that the interpretation may also be adapted to the design, or . . . an idea has been 'read into' the design" (Boas [1904] 1995:187). A comparable process of "reading in" takes place when looking at archival photographs. Just as an object's meaning depends on its cultural context, a photograph's meaning depends on its original setting, its subsequent place in a museum or a publication, any accompanying text, and the biases of a viewer's own culture and historical worldview.

Boas retained a basic distrust of the photograph, with its single point of view, lack of perspective,

narrow bracketing of space, and freezing of a single instant. He considered the scientific value of physical-type images to be limited by the perspectival distortion inherent in two-dimensional representation (Jacknis 1984). Characteristically, his solution to the limits of graphic representation was to gather as much evidence of as many types as possible. The visual medium was valued for the degree of completeness it could add to a body of textual or numerical information and to associated collections, as well as for guidance in constructing museum exhibits.

Photographic images were to be a supplementary form of data. The huge corpus of physical-type photographs, for example, was intended primarily to illustrate cranial and body measurements taken in the field. Skulls and bones were determined to be the most valuable evidence, followed by casts, measurements, verbal descriptions, and photographs. Physiognomic resemblances as shown by the camera were surface appearances which, though not analogous to simple racial stereotypes, were nonetheless data to be applied to racial formulae in determining the physical types of individuals and populations. But in human society, classification had to account for highly complex histories over vast areas of distribution. Although they originated in a search for typology, Boas' considerations of race moved him instead toward historical particularism. His insistence on local differentiation stood in contrast to the prevailing evolutionary models of culture. Boas believed that truly scientific explanations could only be based on an immense corpus of detailed ethnographic data. One of the chief aims of the Boasian method during the Jesup Expedition period was, therefore, the extrapolation of general laws, which he still thought possible, from a preponderance of facts.

Exchanging Vision

Photography is in some regards ill suited to the project of idealizing types for classification. Disinterestedly recording every visible quirk and flaw, the camera tends to favor the details of specific corporeal realities over

idealized conceptual forms. This is why medical and biological journals, for purposes of idealization and classification, often prefer drawings instead of photographs as anatomical illustrations. Whereas an artist can depict a model of an organism in diagram or cross-section, showing all the features deemed distinctive and characteristic of its species, the camera can only depict the unique individual specimen.

The statistical profile of North Pacific peoples sought by the anthropologists was to be based on a composite of individual features.⁸ In a circular establishing its guidelines for photographic portraits, the Ethnological Survey of Canada of the BAAS, Boas' employer on the Northwest Coast, instructed its investigators that

facial characteristics are conveniently recorded by means of photographs" taken in the following ways:

(a) A few portraits of such persons as may, in the opinion of the person who sends them, best convey the peculiar characteristics of the race . . .

(b) At least twelve portraits of the left side of the face of as many different adults of the same sex. . . . If the incidence of the light be not the same in all cases they cannot be used to make composite portraits. . . . The distance of the sitter from the camera can be adjusted with much precision by fixing a looking glass in the wall (say five feet from his chair), so that he can see the reflection of his face in it.⁹

The exchange of vision between photographer and subject mediates the act of photography.

The image of Harlan Smith and his camera reflected in Kwazi'nik's eyes is a visible manifestation of what takes place every time a subject looks at a camera. The seeing eye and the camera lens reflect one another; each is mirrored in the other (Fig. 25).

That exchange of vision in which another's point of view gets captured is illustrated metaphorically in a Thompson River tale, "Coyote Juggles with His Eyes," collected by James Teit. The mythic trickster-hero Coyote loses his sight only to steal someone else's:¹⁰



26/ The photographer's figure (Harlan I. Smith), his camera, and tripod reflected in the eye of Kwazi'nik (from AMNH 11661)





27/ Typical "physical type" photographs from the Jesup Expedition databank of physical (racial) characteristics (front, side, and three-quarter views). F. Nehulin, young Chuvan (Chuvantzy) woman from Markovo (?), 1900 (AMNH 1409, 1410, 1411).



28/ First of a two-photo sequence photographs depicting the Kwakwaka'wakw (Kwakiutl) potlatch at Fort Rupert, British Columbia. Harlan I. Smith, photographer, 1897 (AMNH 42968)



29/ Second photo of the same potlatch ceremony at Fort Rupert. Blankets piled on beach, with a speaker in the middle (AMNH 42967)



30/ Sketches with facial paintings of the Nlaka'pamux (Thompson) Indians. Reprinted from *Bureau of American Ethnology, 45th Annual Report, 1930* (Plate 7)



31/ Yukagir shaman's coat from the Jesup Expedition collections being modeled for camera (AMNH 335758)



32/ Yukagir shaman in full costume, with his shaman drum and drumstick, photographed for a mannequin-style museum display (AMNH 1835)



33/ Yukagir shaman in full costume photographed for a mannequin-style museum display (AMNH 1834)



34/ A grave marker in the form of a carved wooden "copper," Fort Rupert, British Columbia. Harlan I. Smith, photographer, 1897 (AMNH 411809)



35/ Native woman in traditional deerskin clothing (probably borrowed for photo session), with a little girl in a gingham dress. Harlan I. Smith, photographer, 1897 (AMNH 11682)



36/ Secwepemc (Shuswap) woman in traditional clothing posing for a root-digging scene. (AMNH 42947)



37/ Emma Simon, a Nlaka'pamux (Thompson Indian) woman posing for a staged life-scene photo to illustrate traditional practice of deer-hide tanning (AMNH 42930).



38/ Nlaka'pamux (Thompson Indian) life group in the Hall of North West Coast Indians' at the AMNH based on staged photographs from the Jesup Expedition (AMNH 333415)



39/ Miniature diorama of the Maritime Koryak winter settlement, American Museum, Hall of Asian Peoples (AMNH 18237). The actions and poses of the human figurines are precisely based on Jochelson's photographs from the Jesup Expedition, including the the two facing photographs.



40/ Koryak hunters dragging killed white whale on sledge, spring 1901 (AMNH 1423)



41/ Koryak men posing for a "dog-offering" ceremony (AMNH 1519)

42745



42/ Chief Petit Louis (Hli Kleh Kan) of the Kamloops Indian Band, Secwepemc (Shuswap) nation, holding a child (AMNH 42745).



43/ Haida painting, possibly by Charles Edenshaw, representing a bear. The painting illustrates the method of split representation whereby different viewpoints of an animal, front and sides, are shown on a single plane. It also shows how the parts of the animal closely identified with a bear, the ears and claws, are used as a symbolic representation of the creature. Published in: Franz Boas. *The Decorative Art of the Indians of the North Pacific Coast*. 1897, p.127 (AMNH 24537)



44/ Yupik (Siberian Eskimo) man from the village of Ungazik (Indian Point, Chaplino) Chukotka, Siberia, 1901 (AMNH 2438, 2437, 2439)

Continuing his travels, he came to a place where he saw Blue-Grouse throwing his eyes up in the air and catching them. Coyote said to himself, "I can also perform that feat," so he pulled out his eyes and threw them up in the air; but Raven caught them and flew away with them, so Coyote was left without eyes and unable to see. He went groping about, and, coming to a patch of *kinnikinnik* or bearberries, he selected two of the berries, and put them in his eye-sockets as substitutes for eyes. He was then able to see a little, but only very dimly. Continuing on his journey, he came to the outskirts of a village where some boys were playing. One boy who was near him called him "red-eyes" and other sarcastic names. Coyote said, "Although my eyes are red, I can see as well as you can. I can see the Pleiades (*nxa'us*)." The boy laughed and said, "How can you see the Pleiades? It is just noon. I know now for a certainty that you cannot see with your red eyes." Then Coyote seized the boy, and, taking out his eyes, put them in his own head, and, putting his bearberry eyes in the boy's head, he turned him into a bird called *tcêla'uin*. (Teit 1912:212)

In the face paintings reproduced on templates in the Jesup archives and publications, the eyes are explained as a site of symbolic visualizations and extraordinary powers of vision. Figure 30 shows a Thompson Indian motif whose meaning was not certain but, according to Teit,

is said to be connected with sight or the expectation to see. Some say the circles represent the eyes and the lines are symbolic of woodworms or strength, and the whole may be a prayer for strength of the eyes. The person using this painting may have wanted his powers of vision increased so that he might see supernatural beings, or he may have wanted sore eyes to be made well. (Teit 1930:424-5)¹¹

Boas collected a large number of face-painting designs from the great Haida artist Charles Edenshaw, and three-dimensional miniature cast representations of George Hunt's face serve as templates for a large collection of face-painting motifs. In his *Facial Paintings of the Indians of Northern British Columbia* (1898), Boas used face paintings to exemplify the problem of mapping designs not only from three dimensions to

two but also simultaneously from a variegated and changing surface to a static representation. In this essay Boas at once classifies the designs from most realistic to most abstract and describes the Indians'

peculiar method of adapting the animal form to the decorative field. There is no endeavor to represent the form by means of perspective, but the attempt is made to adapt the form as nearly as possible to the decorative field by means of distortion and dissection. . . . if I could obtain a series of representations on very difficult surfaces, the principles of conventionalism would appear most clearly. No surface seems to be more difficult to treat and to adapt to animal forms, than the human face. For this reason I resolved to make a collection of facial paintings such as are used by the Indians when adorning themselves for festive dances. (Boas 1898:13)

Visualizing Cultures

Like museum collecting and anthropology itself, photography both records and represents. As a medium of record, photography documents the visual, producing a permanent image of a subject's physical characteristics from a fixed and framed optical perspective at a single instant. Within the constraints of the medium, photography can accurately depict a person's face, an environmental setting, or the detailed surface of an object. But as a representation, the meaning of a photograph is mutable and depends on many factors. The context from which the image sprang fades away, while the context in which it will be viewed changes continuously over time. The anthropological photograph presents a deliberate image of the traditional past, recording a unique moment of contact between science and its object. Subsequent interpretations are attempts to read meaning and context back into these isolated visual fragments.

The Jochelsons, while acquiring shamans' coats, hats, and drums in Siberia, photographed some of the costumes being worn in the field. The poses suggest that the pictures were meant to serve as models for museum mannequins on which the costumes would be displayed. One effect of such comprehensive

collecting of objects was the self-fulfillment of the anthropological prophecy that the ethnographers were witnessing a last performance, since by acquiring these artifacts they were removing them from the sphere of the living culture. Meant to demonstrate processes for purposes of study and display, these photographs also documented the transfer of the shamans' ritual garb to the museum. The photographic ritual marked the desacralization of powerful shamanic vestments as they were transformed into inert museum objects (Figs. 31–33).

In collecting artifacts and creating ethnographic images for the museum, the members of the Jesup Expedition sought out symbols of traditional culture that could represent the past in idealized museum displays. Individual signs of colonialism and acculturation were frequently left out of the collection. Harlan Smith wrote to Boas from Eburne, British Columbia:

I tried to get the big wooden drum cheaper. . . They had two but one showed white contact. It would have interested me as showing contact but I thought Museum would prefer the old style and would not care to see how white men's pipes and hats are drawn by Indian artists. . . . (Smith to Boas, 17 May 1898, AMNH)

Although the anthropologists often strove to avoid documenting obvious signs of modernity, they nonetheless collected many signs of intermingling cultures. Boasian techniques of dramatizing precolonial traditions were more difficult and less relevant in settlements where Russians, English, Canadians, or Americans had lived for centuries than they were among nomadic hunter-gatherers on the tundra (Laurel Kendall, personal communication, 1996). Some photographs, like a wooden "copper" grave marker in British Columbia, clearly show a combination of traditional culture and western influence (Fig. 34). In Siberia, many signs of Russian influence are visible in photographs taken in and around Yakutsk, imperial headquarters for the collection of *yasak* (fur tribute) for more than 250 years.¹² In heavily Russianized areas such as Yakutsk and

Markovo, Bogoras and Jochelson focused on acculturation and collected many objects from groups that they considered ethnically mixed, such as Chuvantsy [Chuvan] and so-called Russianized Natives.

Representing the Past

James Teit amassed a large collection of semiobsolete traditional Indian costumes that many local photographers borrowed for photo sessions throughout the Nicola Valley region (Wickwire 1993; Fig. 35). Harlan Smith, working with Boas, besides acquiring tools for the collection was able to arrange photographic scenes of a Secwepemc [Shuswap] woman stretching deer hide and digging roots (Fig. 36). The scenes were expressly composed to serve as the basis for a life group representing the "Thompson Indians" in the Hall of North West Coast Indians that Boas was curating at the AMNH (Miller and Mathé 1997:39–40, 100–1; Fig. 37):

At Kamloops got 1 pestle or hammer-bone beater, part of a carved digging stick handle. Deer skin, scraper, stone in handle—birch bark basket and stone scraper. For these last 4 I paid \$4.00. This seemed high but I photoed the woman scraping skin and thought you would need a skin and scraper for a group showing squaw scraping skin. Then I photoed woman digging roots and knowing you had a digging stick I only bought basket for I thought you had no old dirty used baskets and would want one for the group so not to take any out of the case collection. Teit says \$5.00 was cheap for them. (Smith to Boas, 27 April 1898, AMNH)

These scenes were used as references, along with Smith's photos of underground Kikulié houses, for a miniature group that has remained on exhibit in the Hall of North West Coast Indians and for a large-scale "Thompson Indian" [Nlaka'pamux and others] life group in the same hall (Fig. 38). The life group shows the deer skin—considerably smaller than that in the original field photograph—and the scraper. The juxtaposition of scales and the combination of authentic artifacts with fabrications to present a seamless vision inside the glass box create a theatrical fantasy of traditional

culture. The pictorial effect created by the view of old costumes, genuine artifacts, architectural motifs, and wax physiognomies was, as a critic wrote in another context, “neither genuine nor spurious, but illusory and fantastic.”¹³

When comparing the life group with the photographs on which it was based, the most obvious difference, besides the altered scale, is that the woman photographed by Smith was wearing western-style clothing, while the mannequin is in traditional dress.¹⁴ At about the same time, Charles Hill-Tout observed that “the old-time clothing has gone entirely out of use, with the exception of the moccasin, which is still almost exclusively worn by the old people of both sexes” (Hill-Tout 1978:51). In a guide to the North West Coast Hall, Boas noted that Interior Salish Indians no longer wore deerskin. Other cases representing the Thompson Indians in the hall also show and describe the older traditional clothing of the Nlaka’pamux and their neighbors. One hundred years later, anthropologist Marianne Boelscher Ignace was able to identify the individuals in Harlan Smith’s photographs as Secwepemc tanner Emma Basil Simon when Simon’s nieces, Christine and Florence Simon of Skeetchestn, British Columbia, recognized their aunt as the figure in the photos. The image itself has attained iconic stature as a symbol of traditional Interior Salish cultures and has been widely reproduced—for example, as a large anonymous mural in the Royal British Columbia Museum in Victoria, the provincial capital.

As guest curators of the AMNH’s 1997 Jesup Expedition centenary exhibition, *Drawing Shadows to Stone: Photographing North Pacific Peoples, 1897–1902*, we were fortunate to be able to name Emma Simon and her family as the individuals behind the images. With the kind permission of the Skeetchestn Band, we were also allowed to include Marianne Ignace’s own contemporary photographs documenting Nellie Taylor—a Secwepemc elder who passed away in 1997—demonstrating the same art of hide tanning, which has endured to this day. The tanning process

has become a symbol of the strength and independence of Interior Salish women, who have sustained the art despite its suppression in government missionary schools in Nellie Taylor’s youth. Harlan Smith’s Jesup Expedition photographs of Emma Simon, placed side by side with the Boasian life group and Ignace’s modern pictures of Nellie Taylor, visually demonstrated for today’s museum visitors the perseverance of the very traditions that Boas and his peers had feared were dying out.

Exchanging Images

The indexical authority of a photograph as historical fact normally seeks to assert itself over the mutable iconic meaning of the picture (Barthes 1977; Sontag 1977). To a certain degree, this equation is reversed in the artifice of museum representation, where patently constructed images stand as models of culture. In “The Museum as a Way of Seeing,” Svetlana Alpers (1991) maintains that a museum can transform anything contained within its walls into an art object. By virtue of its selection for inclusion in the museum, an object takes on a symbolic mantle, signifying a meaning beyond itself. The investiture of artifacts with ethnographic or historical significance manifests itself as a visual trope, spotlighted in isolation and displayed on a pedestal vitrine. The individual object comes to represent an idealized type.

The dramatic reconstruction of precontact life is typical of the museum models based on photographs from the Jesup Expedition. The museum, as a stage for the objects claimed by salvage anthropology, reconstructed their contexts within the visual trope of display. The efficacy of images for purposes of illustration and representation was largely independent of how the images were obtained. Although Koryak people in the remote coastal village of Kamenskoye were reluctant to submit to many aspects of the Jochelsons’ strange anthropological endeavor, they posed for a series of photographs of their village and annual ritual cycle (Miller and Mathé 1997:35–40). These

photographs later served as the basis for a detailed miniature diorama that is still on display in the Hall of Asian Peoples at the AMNH, where it is labeled as a representation of Paleolithic life. The composite of photographic scenes modeled in the diorama employs a surreal juxtaposition of activities and rituals drawn from different times in the ritual cycle of the Maritime Koryak, creating a distorted, theme park-like view of the people's daily lives (Figs. 39, 40, 41).

The process of representation began in the field with the imagining of the museum. Photographs of the museum were useful in the field for anthropologists hoping to acquire collections. To explain their unusual requests, the anthropologists showed pictures of the AMNH.¹⁵ If suitably impressed, people were sometimes more willing to provide objects and images for the museum's collections. Smith wrote to Boas from Eburne:

I have used up all the pictures I have of the Museum persuading the Indians here to let me have houseposts. I show them that the posts are in rain and weather then picture of museum & ask them to let us house the posts. If you can please have sent to me 3 or 4 more pictures each of Museum, lecture hall and a case hall. (Smith to Boas, 19 May 1898, AMNH)

Under certain circumstances, such tactics proved all the more persuasive for being backed by colonial authority, as was the case with Chief Louis (Fig. 42). Chief Petit Louis (Hli Kleh Kan) led the Kamloops Indian Band from 1855 to 1915, a period of cataclysmic changes on the interior plateau. He helped to hold together the Secwepemc [Shuswap] nation when native cultures were under attack, voicing persistent claims to land, sovereignty, and distinct identity. The band had already objected to Harlan Smith's taking human remains when Boas, moonlighting for the crown as an agent of the BAAS, attempted to obtain the chief's consent for anthropometric work. He succeeded only by invoking the authority of the queen of England over the Indians who were legally her royal subjects. In a lantern-slide lecture following the first

Jesup Expedition field season, Boas admitted using coercive pressure to overcome the Indians' resistance to being cast, photographed, and measured:

I am afraid, that, in trying to coax him to submit to the operation, I gave him a rather wrong impression in regard to the character of our work. . . . I told him that the Queen desired to see the great chief of the Shushwap, and since she was too old to visit him, I had been requested to take his portrait and bring it to her, and that at the same time she had asked me to present him with his own bust, which he was to place in his house, so that his people might understand how important a man he was. This argument removed all his objections, and, after he had consented, there was of course no difficulty in getting just as many men of his tribe as I pleased. (Boas 1897a)

Boas showed Chief Louis' portrait as an anonymous classic Shuswap male physical type in the published album of photographs from the Jesup North Pacific Expedition. Subsequent presentations of the same image have varied according to different contexts, including a prominent place in the gallery of the Secwepemc Cultural Education Society located on Kamloopa reserve land and its presentation as an artifact of historical Interior Salish-European relations in the Jesup centenary exhibition *Drawing Shadows to Stone*.¹⁶

Boasian Visions

The logic of Boas' directives to the scientists on the Jesup North Pacific Expedition was to document entire cultures to the greatest extent possible. His vision of anthropology was as a science of inductive method whose aim was the description and historical reconstruction of entire societies. Representing a whole culture by means of fragments vested with iconic significance, the visual ethnographer judged which aspects to emphasize and which to omit. The criteria for choosing which elements were distinctive features of a culture and which were mere acculturations or adaptations were ethnographic litmus tests of tradition and authenticity as seen by the anthropologist.

The features chosen by the ethnographer—most often, those elements considered to represent precontact survivals—were seen as the salient features that could symbolize a culture as a whole.

A comparable method of visual typology is employed by artists on the Northwest Coast. The artists highlight the symbolic elements that signify an animal's totemic character. Boas wrote in 1897:

In consequence of the adaptation of the form to the decorative field, the native artist cannot attempt a realistic representation of his subject, but is often compelled to indicate only its main characteristics. . . . It would be all but impossible to recognize what animal is meant, if the artist did not emphasize what he considers the characteristic features of animals. These are so essential to his mind that he considers no representations adequate in which they are missing. (Boas 1897b:126)

In his January 1897 lecture, Boas asserted that for American Indian artists,

One of the greatest . . . difficulties is the lack of knowledge of the principles of perspective. To most primitive people a picture of a solid object that shows only one side is incomplete. They ask: Where is the rest of the object? . . . [B]y the desire to represent all the parts of the thing pictured, the artist is led step by step to disregard their relations in space. The characteristic design is added as a distinctive feature to the conventional figure representing a type. . . . There is only a short step from this stage to the second characteristic stage of primitive art in which the realistic picture of the object is omitted entirely and only its distinctive symbol is represented. (Boas 1897a)

In his study of Northwest Coast decorative art following the first Jesup Expedition field season, Boas described the Native artist's method of representation:

I conclude . . . that it is the ideal of the native artist to show the whole animal, and that the idea of perspective representation is entirely foreign to his mind. His representations are combinations of symbols of the various parts of the body of the animal, arranged in such a way that if possible the whole animal is brought into view. (Boas 1897b:176)

Nearly two decades later, in 1916, Boas restated and elaborated on the concept:

While in our modern perspective drawing the painter tries to give the visual impression of the object, showing only what we believe we see at any given moment, we find that in more primitive forms of art this solution of the problem appears unsatisfactory, for the reason that the momentary position of the object will not exhibit certain features that are essential for its recognition. For instance, if a person is seen from the back, the eyes, the nose, and the mouth are not visible; but at the same time we know that the eyes, nose, and mouth are essential characteristic elements of the human form. This idea is so fundamental in the view of most primitive people that we find practically in every case the endeavor to represent those elements that are considered as essential characteristics of the object to be represented. It is obvious that when this is to be done, the idea of rendering the momentary impression must be given up, because it may not be possible to see all these different features at the same time. (Boas 1916, 1940:537)

In his monograph *Primitive Art*, Boas finally admitted that perspective representation was an option occasionally employed by "primitive" artists, but he continued to stress the aspects of symbolic representation in their art (Boas 1928:78).

Reasoning that specific techniques of representing a three-dimensional form in two dimensions are culturally determined, Boas developed a theory of graphic representation in his studies of Northwest Coast Indian art. He considered the approach and point of view of the Northwest Coast artist to be essentially different from that of the Euro-American. Whereas the western artist's illusionistic perspective showed a subject from a single point of view at a specific moment in time (much as in a photograph), the Northwest Coast artist's rendering could be read as symbolically showing all the important features of a subject at once, without reference to a fixed vantage point. One such form has come to be called "split representation": an image is divided into two halves splayed down the center, with all aspects of the creature—front, back, top, bottom, and both sides—represented at once on the same plane (Boas 1928:221–51; Lévi-Strauss 1963; Fig. 43).

In archives, multiple points of view can be reconstructed simultaneously to achieve an effect outside the constraints of a fixed vantage point in space and time. Although the individual photograph is limited to a single perspective, viewing collections of photographs allows the construction of symbolic models of cultures. The multifaceted research collections commissioned by Jesup and organized by Boas represent cultures in a manner that recalls the way Northwest Coast artists represent animals: as a combination of distinctive features seen from numerous angles all at once (Fig. 44). Artifacts and images sampled from the greater cultural whole form an inevitably incomplete record of the change over time. As visual archaeology, archival collections are the shards and fragments of history and cultural memory (Miller and Mathé 1997:29–32; see also Blackman 1981; Morris 1994). The photographer represents the scene as he or she has composed it, the camera records the reflection of a subject, and the viewer reads meaning into the image. As time passes, the photograph becomes a *memento mori*.

In contrast to the myriad viewpoints approximated by the collections in the archives, in designing museum exhibits Boas strove for a theatricalized, illusionistic effect more like that produced by the camera in a single photograph. While planning the Hall of North West Coast Indians in November 1896, a few months before he embarked on the Jesup Expedition to collect objects for the hall, Boas described to Frederic Ward Putnam, the chief curator of the AMNH Department of Anthropology, his vision for the life-group models:

It is an avowed object of a large group to transport the visitor into foreign surroundings. He is to see the whole village and the way the people live. . . . the larger the group the more it is necessary to allow ample space around it so that it can be seen from a distance. (Boas to Putnam, 7 November 1896, AMNH)

Boas conceded that a complete illusion was only possible within a panorama building where viewers could be surrounded by an image that filled their peripheral vision, creating the impression of a scene

without boundaries. But although a full panorama was not feasible in the museum hall, he described to Putnam how an illusionistic effect might be achieved:

In order to set off such a group to advantage it must be seen from one side only; the view must be through a kind of frame which shuts out the line where the scene ends; the visitor must be in a comparatively dark place, while there must be a certain light on the objects and on the background. (Boas to Putnam, 7 November 1896, AMNH; see also Jacknis 1985:100–3)

The creation of a pictorial illusion by fixing the viewers' perspective, framing and isolating the scene, and focusing light on the object resembles photography as a mode of seeing. The museum viewer looks through a glass darkly at a bound and boxed image. The life groups, mannequins, and miniatures Boas planned for the display cases would be based on Jesup Expedition photographs that were yet to be taken in the field. The life group is presented as a photographic vision, while the photograph on which it is based aspired to a three-dimensional mode of representation.

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Notes

1. As a whole, the Jesup North Pacific Expedition produced far fewer images of the Pacific Northwest than of Siberia. The collections from both sides of the North Pacific contain fewer ethnographic images than physical types, especially from the North American side. The smaller number of such scenes may be partly attributable to the availability of earlier photographs from the Northwest Coast, including those from Boas' previous trips to the area.

2. In keeping with the anthropological fashion of the time, some parts of the collection are only sparsely annotated. Poor, post facto, or missing notations on objects and images are not unusual. The assemblage of photographs, artifacts, texts, sound recordings, and memoirs is full of cross-references, some documented but many undocumented. The montage effect of the succession of fragmentary images reconstituted as parts of the archival whole reveals the carefully constructed character of Boasian museum collections. See also Willey, this volume.

3. Harlan Smith was acutely aware of the uses of cross-referenced image materials as supplements to the collected artifacts and fieldwork of

all the team members. In a letter to Boas sent from Nimpkish River, British Columbia, he scrawled a note across the top reading, "please save these letters as a portion of my field note" (AMNH). The letter, describing his methodology at that particular site, was annotated with illustrations of a shell heap and sketches of his archaeological finds. On June 22, 1898, Smith wrote to Boas from Fort Rupert, "I take a sample of every foot from a section that is I have chosen two places at this heap, photoed a section at each taken a handful of shell soil etc. from each layer of each of these sections" (AMNH). See also Smith 1903.

4. As Ira Jacknis (1984:10) has noted, while Hastings may have snapped the shutter, Boas "was always by his side, directing his work, choosing subjects and maybe even camera angles."

5. See Krupnik 1993. Because of sharp declines in population combined with seasonal migration, Bogoras and Jochelson encountered fewer natives than they had hoped, but every nomadic Yukagir and Tungus they met was "held, measured, photographed and questioned" (Jochelson to Boas, 17 July [4 July, old style] 1902, AMNH).

6. On physical-type methodology in turn-of-the-century anthropology, see also Miller, in press.

7. On Boas' views about museum display and his criticism of contemporary methods, see Boas 1887; Jacknis 1985; Stocking 1994.

8. In 1885 John S. Billings had assembled actual composite photographs of skulls in order to compare cranial profiles, using a technique devised by Francis Galton in the late 1870s. See Spencer 1992:105.

9. Source document published on Early Canadiana Online Website.

10. One of the most marked differences noted by Boas as distinguishing the coastal North Pacific culture area from that of the interior of North America was the animal identity of the mythological trickster-hero figure in collected traditional tales. The role is played by Raven from Kamchatka and Chukotka eastward across the Pacific Ocean and the Bering Sea as far as Vancouver Island and the Olympic Peninsula. On the North American interior plateau east of the Pacific Coast mountain ranges, the principal trickster character is Coyote, with Raven taking a supporting role.

11. Loss of vision was of special concern to

Teit, who frequently and apologetically complained in his letters to Boas (housed at the American Philosophical Society, APS) that his own productivity was hampered by a painful eye condition and failing eyesight.

12. The Sakha [Yakut], a Turkic-speaking people, originally migrated from the southwest to northeastern Siberia and settled around Yakutsk. Although technically not classified as a North Pacific group, they were included in the Jesup research program principally because Jochelson, as a former exile, had excellent contacts in Yakutia and could provide the museum with a unique opportunity to collect anthropological material. Although the Yakut had themselves absorbed cultural elements from smaller neighboring groups as well as from Russians and Cossacks, they remained culturally dominant over smaller groups in Yakutia. Jochelson's observations led him to characterize some Tungus and others as "Yakutized" subgroups.

13. The quotation is from "Loitering through the Paris Exposition," *Atlantic Monthly*, March 1890, most likely written by Thomas Bailey Aldrich: "in the Rue de Caire . . . minarets, moucharabies, Saracenic roofs, horseshoe arches, and fretted lattices, under a strip of dark blue sky, overhung booths in which a brilliant confusion of Eastern colors, shapes, fabrics, physiognomies, turbans, fezes, perfumes, and sounds, with the more frequent Oriental dress, created a theatrical East, neither genuine nor spurious, but illusory and fantastic, like the hallucinations of anodynes" (p. 364). World's fairs and expositions of the era were in fact the venues for which many of the Smithsonian Institution's early life groups were originally created.

14. In his main publication on the Thompson Indians for the Jesup series, James Teit noted of the Lower Thompsons and Upper Fraser Band that "intercourse with the Hudson Bay Company affected the dress of the tribe, especially of the upper division. Skins, etc. were often exchanged for Hudson Bay pantaloons and coats, colored handkerchiefs and sashes, red blankets, red or blue cloth, colored ribbons, beads, etc., so that . . . all these articles were in common use among the tribe" (Teit 1900:220). On traditional Nlaka'pamux [Thompson] clothing and symbolism, see Tepper 1994.

15. See Miller 1999 on resistance to photography and object collecting in Siberia.

16. For details about the AMNH's traveling exhibition marking the Jesup centenary, *Drawing Shadows to Stone: Photographing North Pacific Peoples, 1897-1902* (Thomas R. Miller and Barbara Mathé, guest curators; Laurel Kendall, project director), see Lee 1998.

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Harlan I. Smith's Jesup Fieldwork on the Northwest Coast

BRIAN THOM

In three consecutive field trips to British Columbia and Washington State between 1897 and 1899, Harlan Ingersoll Smith worked as the leading archaeologist for the Jesup North Pacific Expedition, under the direction of Franz Boas of the American Museum of Natural History (AMNH). Smith's contributions to the Jesup Expedition left an important published legacy for the archaeology of the North Pacific Coast.¹ These published works are well known to the archaeologists whose careers followed Smith and, to some degree, defined much of the next 75 years of research (Ames 1994; Matson and Coupland 1995; Moss and Erlandson 1995). Research excavations have often been undertaken at places Smith documented in his published site maps (Smith 1907:303; Smith and Fowke 1901).

During and after the Jesup Expedition, Boas interpreted Smith's archaeological results as being suggestive of the historical relationship between culture groups of the North American Pacific Coast. Although these archaeological interpretations of Northwest Coast prehistory have long since been superseded, Smith's work continues to be a resource for what it has to say about the material culture of the communities in which he worked. In addition to Smith's published work on the Jesup Expedition, he left an archival legacy of correspondence, photographs, and physical and ethnological collections. This important body of little-known work provides insight into the dynamics of scholarship and research operating around Franz Boas and the Jesup Expedition.

Smith's Jesup work is a highly interesting and relevant tale about the relationships between archaeologists, anthropologists, and the people they study. Unlike Boas' important local collaborators—for example, James Teit and George Hunt—Smith had no knack for picking up Native languages nor any personal, long-term connections with community members.

In 1897, Smith was merely 25, only six years into his professional career. He was prevented from completing his master's degree by the collapse of his father's business. Insecurity about his finances and his position accompanied him throughout his Jesup work and was at first manifested in what Boas characterized as a cautious manner. With his marriage, and with some job security promised in his second field season, Smith acted more boldly, sometimes against his own better judgment, to secure material for the Jesup Expedition.

Smith's worries over the security of his post at the AMNH at times put him at odds with Boas' research methodology. Smith was eager to excavate at sites that would yield quantities of artifacts and human remains so that he could please the benefactors of the museum with his collections. He was loath to spend much time in regions that he felt would not produce many artifacts and was reluctant to leave areas that he found productive. Boas, on the contrary, frequently urged Smith to expand his investigations to cover the entire region so that a broad picture of the archaeology could be obtained. Specific research questions being asked today may be different, but many of the

issues and situations faced by Smith 100 years ago have repercussions for anthropological and archaeological fieldworkers of the present. The following account of Smith's work demonstrates the dilemmas of rapport between himself and the community members he worked with and between himself and his professional colleagues (see also Thom 2000).

Smith's and Boas' correspondence has been kept relatively intact in the accession records of the AMNH, and additional notes made by Smith on photographs record information that supplements his correspondence.² Unfortunately, Smith's field notes cannot be found in the AMNH archives or in the archives of the Canadian Museum of Civilization, where he spent the latter half of his career. The references Smith makes to the notes in a number of his letters indicate that they would have contained a great deal of detail about his investigations and his interactions with Native communities. The archivist at the AMNH has suggested that the notes were probably destroyed once the results of Smith's work had been published (Belinda Kaye, personal communication, September 1995), and indeed, Smith's published works relating to the Jesup Expedition are the other main source of information on his investigations. Although these articles are generally very descriptive of his archaeological investigations, they tell only a small part of the story of his work and almost nothing of the ethnographic work he did recording information on contemporary Native communities. Only by putting all these pieces together can we examine the difficulties and controversies experienced by Smith during visits to Native communities in British Columbia for the Jesup Expedition.

Smith's Early Life

Harlan I. Smith was born in Saginaw, Michigan, in 1872. He attended public school and received his bachelor of arts degree from the University of Michigan in 1893. Between 1891 and 1895 he had several jobs: curatorial assistant at the Peabody Museum, Harvard University; assistant to the Department of Anthropology for

the World's Columbian Exposition in Chicago; curator of anthropological collections at the University of Michigan museum; and researcher in Michigan for the Archaeological Institute of America (*Who Was Who in America* 1942:1142). Although he wished to continue his formal education, when the family business suddenly folded he could not afford to return for his master's degree (Smith to Boas, 17 September 1897, AMNH). In 1895 Smith was hired by the AMNH as assistant curator of the archaeology collections; his initial task was to coordinate research at the Fox Farm site in Kentucky (Wintemberg 1940). When Boas began planning the Jesup North Pacific Expedition in 1896, he always intended to include Smith as the archaeologist who would investigate the prehistoric remains of the people living on the Northwest Coast of North America.

In Boas' first published summary of the Jesup Expedition (Boas 1898:5), he presented his broad questions that would serve as a framework for studying the historical, physical, and cultural connections between the people living in Northeastern Asia and on the Northwest Coast of North America. Boas stated that although a unique "race" of Native people living in North America could be observed, there were many distinct "types" of people within that race, given differences in skin color, form of head and face, and body proportion (Boas 1898:6). He proposed that while this variability in "type of man" indicated "long-continued development by differentiation" of physical type and of cultures, the similarities between these peoples must be carefully explained by ethnological, archaeological, and linguistic evidence:

What relation these tribes bear to each other, and particularly what influence the inhabitants of one continent may have exerted on those of the other, are problems of great magnitude. Their solution must be attempted by a careful study of the natives of the coast, past and present, with the view of discovering so much of their history as may be possible. . . . By following out patiently and in detail the lines of interchange of culture, it is possible to trace the historical development of the tribes inhabiting a definite region. (Boas 1898:6)

Smith's work would be a key component in uncovering the history of these connections, both through the examination of "physical type" represented in skeletons uncovered from graves and through the artifacts that represented the cultures of the people who left them behind. In addition, Smith was charged with making extensive photographic records of the communities he visited and with making plaster cast and photographic sets of the "physical types" represented in the North American regions being studied by the Jesup Expedition (Boas 1897:537). Although Gerald Fowke and Waldemar Jochelson would carry out incidental archaeological investigations in Northeast Asia, the main Jesup Expedition archaeological research would be conducted in North America by Smith.

Boas set out his priority areas for ethnological and linguistic research in those places not already extensively studied and reported on by other contemporary scholars. As systematic regional surveys of archaeological sites on the Northwest Coast had not yet been done, Smith's archaeological research was to be "carried on in the whole region" (Boas 1903:77). Smith's broad focus was intended to provide critical information for Boas' overall scheme of collecting local histories and mythologies to understand long-term relationships between communities. Thus, as shown by the map of Smith's work (Fig. 45), Smith worked in many of the Native communities studied by other members of the JNPE North American contingent. Boas, however, also placed particular emphasis on the archaeology of the Coast and Interior Salish people living in British Columbia and Washington State. This emphasis was inspired by a hypothesis made by Boas in previous work on the relationship between Coast and Interior groups. Several years before the expedition, Boas had corresponded with Charles Hill-Tout, a local ethnographer and archaeologist. Hill-Tout had found, in the shell middens and burial mounds of the lower Fraser River delta, skulls that were, he claimed, "significantly different from the 'type' found among people living in these areas today" (Hill-Tout to Boas, 1895, in Hill-Tout

1978:35-40). If there were indeed two "types," such evidence was what Boas needed to understand the long-term historical "intermixture, linguistic borrowing, and exchange of cultural forms" (Boas 1898:6) between Coast and Interior peoples—an important piece of the larger picture of the peopling of the North Pacific Rim.

Smith's Fieldwork, 1897

In May 1897, at age 25, Harlan I. Smith accompanied Boas and Livingston Farrand to the interior of British Columbia. Smith's first year of investigation was filled with the enthusiasm and insecurities of a young researcher working under the dynamic Boas. The year also brought Smith his first experiences with working in Native communities on the Northwest Coast.

Spences Bridge

Smith, Boas, and Farrand set out from New York on the Northern Pacific Railway, arriving in Spences Bridge on June 2, 1897 (Boas 1903:78). There they met up with James Teit and worked for five days making collections from archaeological sites and taking photographs and plaster casts of Native people from the Spences Bridge area. Teit, a non-Native who had married into the Nlaka'pamux [Thompson] community, worked with Smith in explaining the processes of photography and casting to community members, who were otherwise reluctant to take part.³ Teit was familiar with the major archaeological sites in the area and guided Smith to several sites along the banks of the Thompson River, where Smith made his first collections. Smith expressed his early thoughts and future expectations in a letter to Marshall Saville, his colleague in the AMNH's Archaeology Department:

I like this region very much. It makes one feel like a man; as if one had a right to live and be free & equal to his fellow men. It strikes me as a bustling region where work is to be had by all who really desire to work. The air is clear cool & rich & puts new life into a fellow. . . . I have seen a number of Indians and last eve found a village which I had not been told of and had a pleasant time looking

at canoes & talking with natives. . . . I very much hope to make a big collection and fill my notebooks so that next winter I will have a good time working up the results with you. (Smith to Saville, 3 June 1897, AMNH)

Boas, Farrand, and Teit soon went to the Chilcotin and Bella Coola regions and left Smith on his own in the Thompson River and Fraser River area of British Columbia (Boas 1903:81). Smith made moderate archaeological collections in the area but did not satisfy his initial desire to make a large collection in the vicinity of Spences Bridge. After about 10 days, he moved his work up the Thompson River to Kamloops, where he thought more profitable excavations could proceed.

Kamloops

In Kamloops, Smith met up with Father Jean-Marie Raphael Le Jeune, a local minister who had extensive knowledge of the Secwepemc [Shuswap] language. Boas had already arranged for the expedition to meet with Le Jeune and have him help explain to the Secwepemc people the procedure of making plaster casts (Boas to Le Jeune, 15 April 1897, AMNH). After making their work clear through Le Jeune, Smith took photographs and made casts of seven people from the area.⁴ Upon completing his work documenting the “physical type” of these people, he began archaeological excavations at the sites on the bank of the Thompson River (Smith 1900d:403–5). He quickly ran into opposition as he began to unearth human remains (Figs. 46–47):

Indians here object to my taking bones away—They are friendly & will allow me to dig graves & take all but the bones. I have seen [Indian] Agent and Indians are on the fence. We hope they will change their minds & allow bones to go to N.Y. for study not for joke as they fear. (Smith to Boas, 18 July 1897, AMNH)

Father Le Jeune explained the purpose of Smith’s research to the Secwepemc people in their own language, and Smith received the community’s permission to proceed. The main concern of the Secwepemc had to do with the respect with which their ancestors would be treated:

They, after holding a big council where my side was presented by the Priest [Le Jeune] telling them I came to get things to use to teach to people in N.Y., decided to let me have a few bones to teach with, but I must cover up all I did not take so as so no bad white men would take them to make fun of the Indians. (Smith to Saville, 11 July 1897, AMNH)

Le Jeune’s role in convincing the community of the validity of the work, although vital, was not revealed in a subsequent publication:

The Indians do not know to what people these burials belong, but they do not like to see the bones of what may have been their ancestors, disturbed. For this reason the chief called a council in which the subject was very fully discussed. Finally the confidence of the people was gained by the help of a number of photographs of the museum, in which it was shown how the people visited the halls in order to see the wonderful works of the Indians, and how they were instructed, by means of lectures, in regard to the meaning of all these objects, and from that time on they rather helped than resisted any endeavour to obtain collections. (Smith 1898a:101–2)

Following this meeting, Smith was able to work intensively through the month of June, making a substantial collection of human remains and artifacts from the Kamloops area (reported in Smith 1900d). He sent the collections back to New York by train before moving on to Lytton, a town at the confluence of the Thompson and Fraser Rivers.

Lytton

Smith camped on the side of the Fraser Canyon near Lytton and worked on a number of archaeological sites that had been exposed by erosion. He was joined by Charles Hill-Tout and a local man, John Oakes. Several weeks in July were spent in Lytton collecting from these exposed sites and photographing pictograph sites in the Stein River valley (AMNH 42818–42823), all of which Smith reported on in his first Jesup Expedition monograph (Smith 1899b). Smith used his “little knowledge of the Chinook language” to get permission to

make archaeological collections and to make contacts with people from whom he could collect ethnological materials. He photographed two young babies from Lytton and the remains of some recently abandoned pithouses.⁵ As he wrote to Saville, he began to make substantial collections in a very short period of time:

Last night we worked until midnight carrying to the depot at Lytton (there is no wagon road) on our backs the 11 boxes of specimens I secured during the 6 preceding days. How is that for one week, eleven boxes? . . . This is a glorious country. One feels so well he can work hard and not notice it any more than play. Saturday I crossed the rapids and climbed up a mountain—and got 6 cradles and a stone pestle and raw material of which pipes are made and with the help of my man carried all that load many miles back over the river in a boat, washed ½ mile down stream by the rapids and in time to carry our 11 boxes of specimens to the depot. At any rate I mean to make so big a collection that it will be my time to catalogue and arrange it or break my leg trying. (Smith to Saville, 11 July 1897, AMNH)

In the 11 boxes Smith packed several skeletons from graves that he had photographed (AMNH 42808–42810, 42817). At the end of July he parted with Oakes and Hill-Tout and headed north to the Skeena River, where he would meet again with Boas.

North Coast of British Columbia

Smith went down the Fraser River to Victoria and then up the coast by steamer to the Skeena River. He met with Boas on August 11. There is, of course, no correspondence from Smith to Boas from this period, and no published reports by Smith. Boas, however, does discuss Smith's work on the coast between the Skeena River and Fort Rupert in several letters and publications (Boas 1903; 1905; Rohner 1969). Smith's catalogue of photographs shows that he spent considerable time with Boas in Prince Rupert photographing the artwork of the Haida and Tsimshian people who came to town and the people themselves.⁶ Very few of these photographs made it into publications of the Jesup Expedition (see Mathé and Miller, this volume).

Smith then moved down to the village of Bella Bella and worked with Farrand for some time, assisting him with making casts and photographs of Heiltsuk [Bella Bella] people and with taking several views of an old house.⁷ Boas and George Hunt met Smith and Farrand at Bella Bella and moved on shortly thereafter to Fort Rupert so that Boas could continue his work with the Kwakwaka'wakw [Kwakiutl]. During this time, Smith was engaged in photographing and making casts of people in the communities at Alert Bay and Rivers Inlet.⁸ After working during the month of August with Boas making casts and taking photographs on the North Coast, Smith took his leave from Fort Rupert and traveled to Fraser River to continue his archaeological research. It is interesting that while with Boas on the Northwest Coast, Smith did almost no archaeology, instead assisting Boas with work in physical anthropology—a pattern consistent with Boas' personal avoidance of field archaeology (Mason 1943:59).

Marriage and Money

Boas' correspondence with his family during the time he spent with Smith on the North Coast sheds light on Smith's enthusiasm for making large archaeological collections in other areas of British Columbia. Boas wrote to his wife, on Smith's arrival at the Skeena River, that Smith had been considering getting married in the fall but was concerned about his financial security:

I have some news for you which will be a surprise. The night before last Smith came to me and told me that he wanted to do something which I would think was very stupid. He wants to get married on the way back. He thinks he could live with a wife on \$60 a month. He wanted to know my opinion. Still waters run deep! He said he had thought over everything carefully and that he has been engaged for many years and now he wants to get married. I told him what difficulties he would have living on such a small amount and that his chances for a major raise were very slim. I told him I could not argue with him, that I could only warn him of all the problems he would have, but that I was convinced he would do whatever he wanted anyway. He asks whether you

think that he could make ends meet . . .
Maybe that explains to a large extent Smith's
curious being and his sensitivity. (Rohner
1969:225-6, and Douglas Cole, 1996)

Boas' impression of Smith's financial situation
caused Smith some concern. Smith quickly wrote
letters to Putnam, the head of the Department of An-
thropology at the AMNH, and to Winsor, the manager
of accounts, regarding his concerns over finances—
letters that, Boas told his wife, were most tactless:

Yesterday I wrote a long letter to Putnam on
behalf of Smith. Smith wrote him that he
wants to get married, and Putnam is very
much worried about it. One cannot give
Smith advice because he is going to do
whatever he wants to do. Putnam told me
about a letter Smith had written to Winsor. I
wish Smith would learn certain things,
especially to hold his tongue with respect to
some people. I don't know but I have doubts
that he will ever amount to anything. His
education has many gaps, and it will always
be apparent because he does not have the
mind to spur him on and help him try to fill
the gaps. He likes mostly activity which he
can do with his hands. He is clever and
resourceful, etc., but where theoretical work
is involved, he lags behind. His attitude in all
possible fields is very naïve, and frequently
the questions he asks are unbelievably
simple. I often tell him to think it over himself
and then give me the answers to his own
questions. On the other hand he is such a
nice fellow that I really feel sorry for him. Well
maybe he will succeed yet. He is only
twenty-five years old. But if he really should
get married with an income of not over \$60,
I don't know what will become of him.
(Rohner 1969:229)

The day Smith was to depart, Boas and Smith had
another discussion. Boas wrote a final note to his wife
about Smith's situation:

Yesterday the *Princess Louise* [a vessel that
carried passengers up and down the coast of
Vancouver Island] arrived, and Smith
promptly made ready and went aboard. Last
night we had an earnest conversation in
which I urgently advised him to wait with his
marriage. I told him he would get more
money after January, I am almost certain. I
also told him that I thought it was dangerous
to get married on \$60. I could see that all
the time he talked with me, he was thinking
about his letter to Putnam. . . . I hope he will

be good in his future work. I wanted him
away from here because there was not much
for him to do, and every day during this
season counts for his work. (Rohner
1969:233, and Douglas Cole, 1996)

Boas' uncertainty about the possibility of Smith and
his wife living on only \$60 a month must have deep-
ened Smith's anxiety about making large, good-qual-
ity collections to satisfy the patrons of the AMNH.
Boas was much less concerned with the size of Smith's
collections than with getting a broad picture of the
archaeology of British Columbia and Washington.
Smith's possible financial insecurity made him want to
concentrate his excavations in productive areas such
as the lower Fraser River and distracted him to a cer-
tain degree from pursuing the broad research agenda
that Boas had set out for him.

Port Hammond

After arriving at the lower Fraser River on September
2, 1897, Smith took room and board near the large
shell heap at Port Hammond. Here he conducted ex-
tensive excavations until the end of October. Smith's
work on the lower Fraser River had been preceded by
the surveys of Charles Hill-Tout, who had investigated
archaeological remains in the area for several years.
Hill-Tout had previously sent Boas descriptions of un-
usual skulls that he had obtained from archaeological
sites in the lower Fraser River area (Hill-Tout to Boas,
1895, in Hill-Tout 1978:35-40). These skulls were long
and narrow, showing evidence of lateral pressure. They
were thought by Boas and Hill-Tout to represent the
remains of an earlier group of people, as the later popu-
lations on the lower Fraser River had wide heads and
broad faces, produced by posterior pressure. The prob-
lem of the age and distribution of this type of skull
was one of the main questions Smith was supposed
to address in his investigations. If, indeed, two differ-
ent "types" of skulls were represented archaeologically
in the lower Fraser River region, Boas' linguistic hy-
pothesis of a recent Salish movement into the coast
area from the interior would be confirmed.

The findings from Smith's excavations at Port Hammond are well described in a number of his publications (Smith 1899a:536–9, 1903, 1904c; Smith and Fowke 1901:60). Smith's archaeological work focused on recovering human remains—skulls, in particular—and on making collections of the artifacts from the shell heap.⁹ In much of his correspondence with Boas about the archaeological work, Smith reported on day-to-day finds and his concerns regarding the packing of this material and its shipment to New York. During these first excavations at a lower Fraser River shell midden, Smith noted the similarity between the skulls and art found in the shell heap and those of the present-day people living on the lower Fraser River (Smith to Boas, 17 September 1897, AMNH). He felt that he had to excavate deeper to get to the more ancient type of people represented by the long, narrow skull collected by Hill-Tout (Smith to Boas, 23 September, 3 October, 5 October 1897, AMNH). Without these deeper investigations in the lower Fraser middens, Boas' hypothesis could not be adequately tested.

Excavations in the shell heap at Port Hammond did not reveal as many artifacts or skeletal remains as had Smith's work in Kamloops and Lytton. At the end of his first week of excavation, Smith wrote a number of concerned letters to Boas in which he expressed disappointment at the quantity of finds from the site:

Got a child below undisturbed shell heap today. The skull was not there. Several bone implements constitute our day's finds. I shall photo a cross section tomorrow. I am a little disappointed in results here. The field looks very rich from the surface and we may yet make a strike. I hope those at N.Y. will not expect too much from this place for I fear they will be disappointed if they do. (Smith to Boas, 7 September 1897, AMNH)

Boas, now in New York, swiftly replied to Smith, again reminding him of the "scientific" objectives of the research. On the same day Smith received his letter, he replied to Boas, "I will try to do the scientific work as you desire in the shell mounds and overcome my fear of not securing sufficient specimens to please

the persons at the museum who look for such eagerly" (Smith to Boas, 15 September 1897, AMNH). After giving the matter further consideration that night, Smith wrote a follow-up note to Boas regarding his insecurities about his situation:

I fear you think I act very strangely at times and I guess I do. I know I have still a trace of the effects of being in father's office during the time everything went to the dogs. It made me have fear of being able to earn a living, fear of being cheated, fear of everything & everybody which was often without the slightest reason and while I could & can reason that there is no sense in such fears I can not even yet escape them. At times they so upset my nerves that I hardly know what I do. I never have been able to escape the fear of losing my situation. I suppose it is all due to seeing everything father had swept away and knowing he was a powerful man compared with me showed me how helpless I was. And at the same time it made me dependent on myself while before I had no knowledge of what that was. I think this accounts for some of my doings that seem strange. (Smith to Boas, 16 September 1897, AMNH)

Smith continued to work over the next several weeks as if walking on eggshells. He asked, in cautious notes to Boas, what other museum staff, including Jesup, thought of him. He looked for advice on whether he should try to write newspaper articles for the McClure Syndicate about the expedition and reassured Boas that he would address the research questions at hand. "I think to get at questions we need deeper shell heaps, but do not care to leave here until we have a more complete collection and hence knowledge of this place, unless you so desire. Kindly let me know" (Smith to Boas, 23 September 1897, AMNH).

In addition to Smith's insecurities about being able to produce satisfactory results for the AMNH, a more immediate concern was a looming situation that had the potential to impede his fieldwork. In his first week at Port Hammond, Smith read in the local papers that two collectors from the Field Museum, George Dorsey and Edward Allen, had been arrested in Oregon for grave robbing and subsequently released (Cole

1985:175–6). Only a week later, the Indian agent from New Westminster visited Smith to discuss the same topic. As Smith reported to Boas:

He said that every Indian Agent here had received notice that there was a liability of parties digging in Indian grave yards and to look out for them as it was against the law. Also he had received a second circular giving him direction to warn the Indians & tell them the law on the subject. (Smith to Boas, 15 September 1897, AMNH)

Smith contacted British Columbia's superintendent of Indian affairs, A. W. Vowell, to thank him for some collections he had sent to the AMNH. Smith also inquired at this time about the Indian agent's warning against grave robbing. Vowell replied that the circulars were not directed toward Smith's work but, rather, were to inform local Native people about non-Natives who were digging up their graveyards so that the land could be preempted for settlement. This reply eased Smith's concern about collecting human remains, so he continued his work in the shell heaps at Port Hammond (Smith to Boas, 3 October 1897, AMNH).

Smith also used this time, especially on rainy days, to make his own contacts in the Katzie and Musqueam communities near Port Hammond and Eburne in order to photograph and make casts of the people there and collect ethnographic objects. In contrast to his experiences with Teit and Le Jeune (and, later, Hunt), Smith did not have prior contacts with these Native communities. Nevertheless, members of the Katzie community near Port Hammond offered him the opportunity to purchase a blanket of mountain goat wool, woven hats, a *sxwayxwey* mask, canoes, spindle whorls, rush mats, and other utilitarian items (Smith to Boas, 15 September, 9 October, 30 October 1897, AMNH). Following his cautious program, Smith did not purchase any of these objects, as he wished Boas to give him direction on such acquisitions first. Smith did eventually purchase one of the beautiful mountain goat wool blankets on November 4, on his way back to New York, when he paid only \$6 instead of the \$10

for which it had been offered on September 15 (Smith to Boas, 10 November 1897, AMNH).

Smith was less cautious when it came to trying to obtain photographs and casts of the people living along the Fraser River. He initially tried to do some photography and casting of Native people at the prison in New Westminster, but his request was denied (Smith to Boas, 15 September 1897, AMNH). Smith spent a number of days during rainy October urging people in the Katzie community to be photographed and cast. Although he offered \$1.00 for each cast, only Archille James, a 19-year-old youth from Katzie, agreed (Fig. 48; AMNH 42886–42889). By the end of the 1897 field season, Smith had not been able to get any other person from the Coast Salish communities in Victoria or the lower Fraser River to agree to be either photographed or cast:

I could not get a single Songish at Victoria, nor can I get any here [at Port Hammond] to submit to be cast . . . All these lower Frazier people seem to object to casting—I must try here again next season when I work at the Great Frazier Midden. (Smith to Boas, 11 October 1897, AMNH)

Victoria

On October 22 Smith shipped crates of his work from Port Hammond to New York and left the lower Fraser River for Victoria. Upon his arrival in Victoria, he met Oregon C. Hastings, a local resident who had worked with Boas in Fort Rupert in the past and was keenly interested in the archaeological sites of the area. The next day, Smith and Hastings set out to examine some of the burial cairns at Cadboro Bay, four miles north-east of Victoria (Smith and Fowke 1901:58).¹⁰ In seven days, he excavated 21 cairns. He was most disappointed to find that there was "only a speck of charcoal and a handful of bone dust" remaining in these cairns, largely because of the highly acidic soil and the shallow depths at which the bodies had been interred (Smith to Boas, 30 October 1897, AMNH). After the cairn excavations, Smith and Hastings set to work at "the deepest shell heap I have seen" in Victoria. But

here again, Smith was disappointed at the scarcity of finds (Smith to Putnam, 4 November 1897, AMNH).

To compensate for the poor excavation results, Smith followed up some leads he had on ethnological collecting. He visited a small island in Esquimalt Harbour, where he was offered a drum used in winter dancing for \$1 and a house post for \$12. Still an archaeologist at heart, Smith commented that he saw “shell heaps in the process of formation” on the island (Smith to Boas, 3 November 1897, AMNH). Upon his return to Victoria, he met four men and three women, none of whom he named, from Kaiuquot on Vancouver Island who agreed to be paid \$1 to have casts made and photos taken (Smith to Boas, 10 November 1897, AMNH).¹¹

On November 10, Smith boarded the train, stopping at Port Hammond before leaving for the East Coast. Despite Boas’ advice, he was married to Helena Oakes in a small ceremony in Saginaw, Michigan, on November 25. He then returned to New York to work on organizing and writing up the 1897 material.

In the first AMNH memoir to come out of the Jesup Expedition, Boas summarized Smith’s first season of work and noted the archaeologist’s important contribution in “clearing up interesting points in the history of the Indians” through his examination of the shell middens of the lower Fraser River:

It seems that the physical appearance of the Indians during the period of deposit of the shell-mounds on the lower Fraser River had undergone material changes. The results that were here obtained are so important, that it will be necessary to continue the researchers during the coming year. (Boas 1898:11)

Smith’s Fieldwork, 1898

During the next season in the field, from April to September 1898, Smith continued investigating archaeological sites, photographing and casting physical types, and collecting ethnological artifacts from the communities where he worked. But he spent a great deal more time and energy on the latter, and less time on photographing and making casts for the study of physical

anthropology. Smith’s new wife, Helena, joined him in the field and drew a number of sketches for his correspondence to Boas. Perhaps because of his marriage to Helena, or because it was his second field season with the Jesup Expedition, Smith showed a new confidence in his work and new enthusiasm for the research. His letters from this season generally discuss in more detail his relations with local Native communities, and his archaeological observations are much less tentative. In spite of this new confidence, Boas still provided firm direction for the research.

Kamloops

Smith left New York on April 13 by railroad via Ottawa to British Columbia. In Ottawa he spent two days sketching and making notes on the collections at the Geological Survey of Canada, under the direction of George Dawson (Smith n.d.). On April 21 he arrived in Kamloops to examine and collect archaeological materials that had been exposed by the wind over the past year.

At Kamloops Smith also had the opportunity to take some useful ethnographic photos, including one of a young girl working on a hide with a stone scraper.¹²

While at the village I saw a little girl scraping a skin with a stone hafted in a handle about 3 ft long similar to the one Teit collected. Closer inspection showed 3 of these hafted scrapers, the skin stretched on a frame. I contemplate photographing her at work tomorrow and then buying the whole outfit for you as I think you will want it for a group. Fr. La Jeune thinks I can get it for \$1.50 i.e. the skin so I suppose I can get skin & sticks from frame and scrapers entire for less than \$5.00. If so I feel you will be glad of them. I know this is hardly in my line to collect Ethnology in this region but the thing seems too good to see go. (Smith to Boas, 21 April 1898, AMNH)

Smith felt that this collection of photographs and deer-hide-scraping equipment would be useful for “the construction of an ethnic group; especially since we have the physical material collected at this place in ‘97” (Smith n.d.). Smith also took photographs of a woman digging roots and of a tepee-like structure.¹³

Spences Bridge

Smith left Kamloops after a week and moved to Spences Bridge, where he again met with James Teit. Teit and Smith spent several days photographing teepees and sweat houses and excavating in pithouses near Spences Bridge.¹⁴ During the previous winter, Smith had sent a number of photographs he had taken to Teit, who was to distribute them to the people who were pictured. After Teit had done so, those whose photos had not been sent were understandably upset, and Teit was under some pressure to give everyone a copy of what had been taken of them. Smith wrote to Boas asking him to send the remaining photos to alleviate the situation. He also asked Boas to send copies of the photographs taken of the pictographs at Lytton (Teit 1900; York et al. 1993), as Teit had agreed to ask local people for explanations (Smith to Boas, 2 May 1898, AMNH).

Eburne

After just over a week, Smith took his leave of Teit and Spences Bridge and headed down the Fraser River to Vancouver, where he set out to explore the large shell heap at Eburne commonly known as the Great Fraser Midden (Smith to Boas, 27 April 1898, AMNH). Smith began his archaeological excavations on May 2. He had three men working with him in the field: O. C. Hastings, W. H. Hindshaw, and Roland B. Dixon, all of Vancouver (Smith to Boas, 2 May 1898, AMNH). The Great Fraser Midden produced a large number of human remains and artifacts from deeply stratified deposits.¹⁵ The finds from these excavations are well reported in Smith's monograph "Shell-Heaps of the Lower Fraser River" (Smith 1903).

Smith was more determined than ever to discover the relationship between the long and broad skulls that both he and Hill-Tout had found in previous seasons. Boas had clearly convinced him of the importance of these skulls to the overall research questions of the Jesup Expedition. Smith believed that by working at the Great Fraser Midden, where Hill-Tout

had found his original long skull, he would be able to provide answers to this question. Soon after Smith began his excavations, however, he became aware that there may not have been only two types: "Everything is going well. We find two distinct types of skulls and it seems also that we find every conceivable intermediate form. In fact as Hastings well expresses it, we get no two alike" (Smith to Boas, 16 May 1898, AMNH). In a later letter he reaffirmed this observation:

I wrote to you of the Hammond type of skull and the long type. By long type I meant the type represented by the Hill-Tout skull. I don't know how many I have of them but at least 6 in good condition and some broken. There seem to be intermediate forms. I feel all mixed up about them as they are so different. There may be 3 or 4 types so far as I can see hastily. . . . The two types seem to be buried alike i.e. with equal care and some of each are deep down, others are high up. (Smith to Boas, 3 June 1898, AMNH)

In the publications of the Jesup Expedition, Smith's field sense of the different kinds of skulls represented were overridden by Boas' own interpretation of the human remains. Neither Smith nor Boas mentioned the uncertainties Smith had in the field about the number of different types of skulls present in the shell heap. Instead, they both reported that there were two types of skulls found in the shell heaps—one narrow and the other broad, both of which were cranially deformed (Boas 1903; Smith 1903). Boas' insights were obviously a powerful force for the Jesup Expedition, and he considered this a highly significant interpretation, whether it was correct or not. Had Boas taken seriously Smith's field observations—that there were not two distinct types of skulls but, rather, many forms in between—he might have reconsidered his long-held, but misguided, interpretation that the Salish were relatively recent arrivals in the area.

Another important aspect of Smith's stay at Eburne was his work among the Musqueam community at the mouth of the Fraser River, which he visited on a rainy May day, looking to purchase ethnological materials for the museum. A man offered to sell him a

“whewhe” [sxwayxwey] mask for \$10, a horn rattle [syiwMéxwtses] for \$10, and an entire shaman’s outfit for \$100 (Smith to Boas, 19 May 1898, AMNH). The outfit was far too expensive for him, and he decided to wait before buying the mask, hoping the man would reduce the price.

I have not yet bought the mask for \$10.00 or the horn rattle for \$10.00. I expect to get the mask in the fall and hope to get it cheaper by delay. Do you want the rattle at \$10.00? It seems to be fine, has goat wool fringe, carving of human head on handle, and the rattle part is carved in their own art. There was at least 6 of the masks all the same in the Delta. The shamans outfit consists simply of mask & feather attachments. I do not think you would care for it at \$100.00 and I think you would prefer the \$10.00 mask & \$10.00 horn rattle to it even if they were equal in cost. I have worked my best to get things from them. Hastings has also. I sent you a list of what we got. Yet I hope to get more later. I have not all there is to get & want to bring you a complete lot from the Fraser Delta. What are shell rattles worth? Several of this kind of shell [sketch of a large Pacific scallop shell] are strung on a hoop. Will make every effort to get all kinds of baskets & uses. (Smith to Boas, 3 June 1898, AMNH)

This was a difficult time for the Native people of the Northwest Coast. The Canadian government’s laws banning the potlatch and winter dancing were in full effect. Missionaries and priests were collecting and burning ceremonial regalia, and Native children were being separated from their families and sent to residential schools. Many of the spiritual activities had to be conducted underground. A shaman’s outfit like the one offered to Smith was clearly a powerful and important ritual object at the time and was not going to be parted with for a small sum of money.

Smith did obtain a house post from “Chief Nuxwhailak,” who accepted only \$10 for it and said that the pole was “part gift to museum” because the museum was going to use it for “educational purposes” (Fig. 49). The AMNH received the post on the condition that it was to be labeled “from house of Kaplänuux,

grandfather of present Chief Nuxwhailak from whom it was obtained” (Smith to Boas, 18 May 1898, AMNH). The chief’s condition about the label on his gift was not (and has not subsequently been) respected by the AMNH. Smith attempted to document the meanings associated with this post, “as well as they could give them,” but he was disappointed by the report given by Chief Nuxwhailak. “The man figure they say is simply an ornament or a carving made to be a carving & has no meaning. They don’t seem to know as much of the old times as we wish they did” (Smith to Boas, 3 June 1898, AMNH).

Had Smith learned to take down accounts in the Halkomelem language, or had he had the assistance of someone like James Teit or George Hunt in the Musqueam community, he might not have been so disappointed and might have found that people knew more than they let on in English.

Smith tried to collect other posts that he photographed at Musqueam during his stay at Eburne.¹⁶ He used his technique of showing community members pictures of the AMNH’s halls, explaining that if the poles were moved there, they would be kept out of the rain and weather. However, he was not able to purchase any of the others that he photographed, as the people from Musqueam “would not sell others at any price except one for which they wanted \$100.00 and it was some broken” (Smith to Boas, 3 June 1898, AMNH).

Fort Rupert

After spending a few days visiting sites in the Boundary Bay area of Vancouver, Smith traveled up the coast to Fort Rupert to work with George Hunt. With Hunt’s assistance, Smith was able to arrange the taking of casts and photographs of a number of men from the community at Fort Rupert, although no women would take part.¹⁷ In addition to the usual array of profiles and poses intended to capture the “physical type” of the people, Smith took photos of a Fort Rupert potlatch, gambling, a woman’s potlatch, several house posts and totem poles, and coppers fastened to trees. These

and a series of “unposed photos” of an old man “clothed in a blanket sharpening a stone adze” (Smith n.d.) form a significant contribution to the ethnological photos of the Fort Rupert area of this time.¹⁸

Smith began his archaeological investigations by excavating a number of shell heaps in the area.¹⁹ He continued to be puzzled by the different excavation results from middens in various areas of the coast. In the Fort Rupert middens he found very few artifacts and no human remains, which was very different from the numerous finds in the shell heaps on the lower Fraser River. In a letter that he intended to be kept as a portion of his field notes, Smith anticipated the need for further careful and thorough investigations to make meaningful interpretations of the archaeological record:

I learn of a new shell heap in every direction almost daily and at best can only hope to see a few of them this year, for were I to visit them all I would have no time to dig in any of them. I have to chose a few locations and work in them to get an idea of the different regions from the few typical representatives. . . . Some shell heaps but a short distance from others present such different characteristics that I feel they may belong to different peoples or be summer residences fishing stations or the like of the same people. To determine all these matters will require considerable further investigation and if that produces as much variety it will again extend the investigation. (Smith to Boas, 6 July 1898, AMNH)

Smith’s concerns had progressed from collecting a large quantity of samples to please AMNH patrons to collecting adequate samples for careful interpretations of each site. Just as Boas had taken issue with Smith’s obsession with large collections, these new difficulties in interpretation were also a problem for Boas, who was seeking to get a broad idea of the historical, cultural, and physical relationships of the Native people of the North Pacific Rim. If archaeology was to provide answers to these questions during the Jesup Expedition, investigations would have to be made over the whole region. This broad goal conflicted with Smith’s methodological desire for thorough investigations of single, deep sites. But careful interpretation of the

remains from each site would not allow excavations at as many sites as Boas wished. Despite Smith’s preference, Boas’ leadership in defining regional research goals pushed Smith on to other areas.

Although archaeological investigations in Fort Rupert did not reveal many human remains, Smith was successful in collecting from more recent graves in tree burials and rock shelters. At the end of the first week in Fort Rupert, he wrote to Boas:

We have secured five complete skeletons and three skulls from tree and box burials. George Hunt got permission to take these bones. We are doing it secretly however, leaving no traces behind us and will use the permission to cover a possible detection. (Smith to Boas, 12 June 1898, AMNH)

Smith later wrote to Boas that although he had permission from Hunt to take these skeletons, he “thought what the Indians did not know about it would not hurt them” (Smith to Boas, 6 July 1898, AMNH). By the end of Smith’s stay in Fort Rupert, 32 skulls had been obtained from tree, box, and cave burials, in addition to several painted boards and boxes from these graves.²⁰

While working in the Fort Rupert area, Harlan and Helena Smith camped on the shell heap near the home of George Hunt’s sisters, Sarah and Jane. Smith was delighted by the hospitality of the Hunt family, who often visited, bringing fresh food and gifts, but the Hunt family came to have very different feelings about him and Helena. In addition to several other grievances, the excavation of the burials was not well received by the community in the winter, a few months after the Smiths had left, when community members discovered what they had done. George Hunt received the brunt of enormous family and community resentment about the Smiths’ stay in Fort Rupert. Hunt wrote (in his particular style) about these problems to Boas:

Now there is one thing that I am sorry to let you know what Mrs. H. I. Smith Done for me and I think for you to now the knight there arrived here. I went and Beged my two sisters Sarah and Jane to let them Have a

Room for the night for Mr. Smith was my friend, so they did give Mr. and Mrs. Smith one of these Rooms in the House free of charges and after that, my sisters was kind enough to let them have Empty cases free of charges and Even Help me in sending the Indians to him to have there casts taken and after Mr. Smith left Fort Rupert he left all his traps in the care of my sister and the thank my sister got from her, or Mrs. Smith. She went to Victoria put something against my sisters, on the newspapers. The it was enough to make Mr. Spencer and wife and all my sisters would not speak to me Ever since they Read the paper of what Mrs. Smith say about them, and Even signed by her. It seems to me that Mrs. Smith asked Sarah and Jane to let her have one each of these photographs, so my sisters did have her that is to Mrs. Smith one Each of these photos, and on the second paper she let the reporters scratch the two pictures and put them into the news paper and the names she called them there I am shame to talk about, so my sisters got that wild about things that they went and Report to the Indians what Mr. Smith done to there Daid and that I was helping them, and the Indians, said that they will never let Mr. Smith come to Fort Rupert again to still there grave again. Now I let Mr. Smith have David Boat, that cost David \$25.00 Dollars, and after it was returned, the keel was all worn away, leeking like a basket for the Bottom was nearly worn through. Yet I am pleased for the things that I got from Mr. Smith. (Hunt to Boas, 10 January 1899, APS)

Hunt's news about the Smiths was accompanied by the further bad news that one of the Fort Rupert chiefs had heard that Boas was making speeches telling of how the Kwakiutl were still "living on the Daid [dead] people." Because of these two incidents, Hunt was told at a feast that neither he nor Boas could ever attend ceremonials again. On hearing this news, Boas responded in defense of Smith and the work of the Jesup Expedition:

Now about the Smiths. I simply cannot understand the things you are talking about. All the letters that I received from Smith and Mrs. Smith while they were in British Columbia were just full of praise of your sisters and you mother, and every time they talk about British Columbia, they say how kindly all of you treated them; in fact, they are taking every opportunity to express how much they

are indebted to all of you. I am quite certain that neither he nor she would willingly hurt the feelings of any of your people. I suppose the whole trouble lies with the meddlesome and nasty newspaper writers. You do not know how they are bothering us all the time, and how every thing they learn is twisted about in the paper so as to make it look exciting to the people. I suppose you remember the nasty figures and the horrible description of the dance that was in one of the newspapers, said to be written by me, but which was simply made up, and stolen out of my book. You may be quite sure that the same thing happened to the Smiths. (Stocking 1974:126)²¹

Boas' response to the accusations by the chiefs is now something of its own legend: he sent Hunt funds to host a feast, and Hunt gave out copies of his previously published Kwakiutl work and made a speech to clear their names. While Boas cleaned up his reputation with Hunt and the Kwakwaka'wakw [Kwakiutl], Smith avoided further controversy by not returning to that community the next year. Such a response could only have reinforced Smith's desire to keep his gravedigging archaeological work quiet.

Nimkish River, Alert Bay, and Comox

Smith continued to work on the northern end of Vancouver Island through the months of July and August in the area around the Nimkish River, Alert Bay, and Comox. Much of his time was spent in archaeological excavations of shell heaps. The results of these archaeological investigations are well reported in his "Archaeology of the Gulf of Georgia and Puget Sound" (Smith 1907:305-30). Smith's concern over methodological bias in his interpretation of the archaeological material continued:

I feel that our finds may not in all cases be correlated with the real losses of these people, but are more or less influenced by our luck, consequently we have to do a great deal [of excavation] and get much in order to eliminate, as far as possible, the luck equation. (Smith to Boas, 1 August 1898, AMNH)

Smith's "luck" in the shell heaps did not include finding many human remains. To compensate for this

apparent lack, he and Hunt continued to collect more recent burials from grave boxes found in trees. Smith and Hunt did consult with members of the Comox community about collecting from a grave site; one member was willing to sell a grave post for \$14.²²

Smith and Hunt were active in collecting additional ethnological specimens for the museum. While working in the Nimpkish River area, Smith was given a large "grease pole" that served as a fountain for fish grease at feasts (AMNH 43019). A human figure was carved into the pole, and fish oil poured into the back of the head came out of its mouth (Smith to Boas, 1 August 1898, AMNH). While in Comox, Smith and Hunt were able to acquire a *xoaxoe* mask, a collection of baskets, and 11 carved posts. Smith reported that the mask was one of two in the area and was purchased for \$12.00 from a man from Comox. The carved posts he collected included several grave markers and some house posts that were standing inside an old longhouse (Fig. 50). This was one of the largest ethnological purchases Smith made during his work with the Jesup Expedition. It took up a substantial amount of his disposable budget, which curtailed further expenditures during the year. Smith made some detailed notes on these posts in his correspondence with Boas.²³

Before leaving Comox, Smith visited Denman Island, where he observed a shell midden in the process of creation. His photograph catalogue reads, "The origin of a shell heap, clam shell thrown away after a meal—the fire, the stones, and the sea weed to hold in steam—all left on beach by a travelling party of Indians" (AMNH 42031).

Nanaimo and Duncan

During the last week of August, Smith made his way down the east coast of Vancouver Island from Comox to Victoria, stopping in the communities of Nanaimo and Duncan, where there were large Indian reserves. He located shell heaps in both areas but determined that "it would be best to devote our remaining time and money elsewhere" (Smith to Boas, 31 August 1898,

AMNH). In Nanaimo, at the mouth of the Chase River, he visited a site containing many petroglyphs. He originally wished to send the rock art to New York by quarrying the sandstone but thought the expense of shipping would be prohibitive. He photographed the petroglyphs and made a plaster cast of one of them for the museum (Smith to Boas, 31 August 1898, AMNH).²⁴ In Duncan, Smith located a shell heap on one of the Indian reserves but was not permitted to do any excavation. He continued to look for house posts in all four Cowichan villages he visited but did not find any. Feeling pressed for both time and money, and disappointed, he continued on to Victoria.

North Saanich, Victoria

Smith arrived in Victoria on August 30 and had a fortuitous meeting with five Native people who were willing to be photographed and cast (AMNH 12074–12092). Significantly, these people were not of local Coast Salish ancestry but were Nuu-chah-nulth [Nootka] from the west coast of Vancouver Island. Smith's further efforts in the local Salish villages around Victoria turned up no one interested in taking part in photographs or casts.

For the rest of the week, Smith and his crew did archaeological work at several sites in the North Saanich area. His main purpose was to explore the cairns that he had heard about from local residents. He also visited many local farmers who had collections of artifacts, making sketches of them for his publications, and spent time drawing and making notes on the artifacts at the Provincial Museum in Victoria.²⁵ Smith left one of his field assistants, Albert Argyle, to continue investigations in the area around North Saanich, where several shell heaps and 12 cairns were excavated (Smith to Boas, 31 August 1898, AMNH; Smith n.d.).

Vancouver and Port Hammond

On Smith's return to Vancouver on September 7, he discovered that the rates for shipping materials to New York had increased three times over those of the

previous year. He canceled his plans to explore Puget Sound, Washington, and the Point Grey area in Vancouver because funds had to be diverted to shipping (Smith to Boas, 7 September 1898, AMNH). He decided to use the last of his funds in the Vancouver area, visiting the Musqueam Reserve in order to collect the objects he had seen the past summer:

Musqueam Indians doubled the price on the rattle making it \$20.00 so I left it. Wanted \$20.00 to be photographed at loom, as did also Duncan Indians—will try it again at Port Hammond. Offered \$5.00 but thought \$20.00 too much & need it for shell heap work. Told me 10 disks game on plate not used & did not know it or have it. It was lost long ago they said. Told me bear tooth game did not exist. Conclude the man with bear teeth meant by “he he” that he was fixing bear teeth for fun. I thought he meant for a game. I secured a blanket (Mt. Goat), ½ made, \$3.00. Cowitchin Indians would not sell loom but I saw how they were made. They would not show us how to weave as it took so long & much work & they wanted \$20.00 to do it. I have tried, & with Hastings help, to get the pictures of weaving at every place we have been and went twice to Musqueam, several times in May and once yesterday. I conclude as I have spent so much for ethnology . . . [I] will use the money for shell heap work. (Smith to Boas, 7 September 1898, AMNH)

Smith’s confusion over the “bear tooth” game came from a poor understanding of the Musqueam Halkomelem term *xáxe* (Smith’s “he he”), which means “sacred,” “taboo.” As was typical for Smith’s work in the Coast Salish communities, he was able to collect nothing from Musqueam except a photograph of “cat tails from mats” (AMNH 43032).

Smith’s last money for the season was spent excavating for a few days at Port Hammond. He visited the Katzie Reserve, where he had previously seen another *Xoæxoe* [*sxwayxwey*] mask, but again, he was unable to purchase it. In September, Smith ended his fieldwork and boarded the train for New York.

Smith’s investigations over 1897 and 1898 generated a number of specific research questions that he wished to address through further archaeological work

in shell heaps. He posed these questions to Boas in a letter written near the end of his field season:

Are the long skulls found elsewhere than at Eburne? Are they found at Hammond? Are the rich shell heaps, like those off Hammond and Eburne, which have a large proportion of black soil and specimens, uncommon to the salt water places such as Boundary Bay, Victoria, Fort Rupert, Comox, etc, where the heaps consist mainly of shells and are barren of specimens except in the much near the top? What is the difference between these two sorts of shell heaps? Is the former type peculiar to rivers, or only to the Fraser, or is it common to a river where tribes could gather to catch fish then go away, let the grass grow to cover lost objects so they would not be again found and where they would loose in moving or discard before moving, where murders and lawlessness would be greater? (Smith to Boas, 31 August 1898, AMNH)

Smith’s musings seem distant from the larger goals of the Jesup Expedition. The problems that concerned him were those of understanding how the archaeological sites were formed and what the different functions of the sites were. His expenditures on ethnology and the increased rates for shipping made it very difficult for him to pursue Boas’ broad vision at the end of 1898. Smith would get one more season under the Jesup North Pacific Expedition to address these questions.

Smith’s Last Fieldwork, 1899

During the summer and fall of 1899, Smith continued his investigations at Kamloops, Puget Sound, Port Douglas, Lillooet, Eburne, North Saanich, Spences Bridge, and Nicola Lake. His excavations in these areas are well reported in his publications. However, the archival record for the early part of this last season is not as complete. The following account is therefore limited to very brief summaries of Smith’s published material and what can be gleaned from the photograph record.²⁶

Kamloops

Smith left New York in early May and arrived in Kamloops on May 16. He paid a brief visit to the sites from which he had previously collected, finding that

the wind had revealed additional features. Here he made several more collections of artifacts and skeletons from exposed deposits before moving on to Puget Sound (Smith to Boas, 17 May 1899, AMNH).

Puget Sound

As in British Columbia, Smith conducted his research primarily by making surface collections at sites where artifacts and human remains were exposed, by visiting and describing existing collections of artifacts in museums and private collections, and by undertaking excavations at selected sites that appeared to be promising for collecting a great deal of material. Of the 25 locations on which Smith reported in his 1907 publication, he only excavated the five sites of Marietta, Stanwood, New Dungeness, Port Williams, and Burton.²⁷ W. H. Thacker, a resident of western Washington who worked with Smith in the Puget Sound region, conducted several excavations of shell heaps and burial cairns in the San Juan Islands (Smith 1907:380-6).

Smith's photograph records show that he was able to obtain only a few photographs in these Coast Salish communities.²⁸ This general lack of participation in photography and casting is consistent with that of other Coast Salish people whom Smith visited. Smith also took a number of pictures of an old shed-roof house at Lummi (AMNH 12129-12133) but did not collect any of the planks or any of the eight carved house posts that were there. The meager results of the shell heap work in Washington prompted Smith to return in late July to British Columbia, where he began his work in the Lillooet-Harrison Lake region.

Lillooet-Harrison Lake

From Smith's investigations in Lillooet-Harrison Lake, there are a few letters from Boas to Smith in the field. It appears that the focus of his work was the acquisition of skeletons, specifically skulls. Boas felt that this area might provide important historical information about the link between Coastal and Interior people:

I did not expect you to confine yourself to skulls, but should have been glad to have had archaeological researches carried on also. . . . You know the Lillooet region is one of those inland districts by way of which coast culture entered the interior, and for this reason it is particularly interesting from an historic point of view. It might be, for instance, that in prehistoric times the culture proved to be much purer interior culture than later on, or it might be that the culture was more closely affiliated to the coast culture than it is now. The Lillooet have adopted the social organization of the coast tribes, and many of their industries, as far north as the town of Lillooet, on Fraser River. At the same time they have many things in common with the tribes stretching from Columbia River through the Cascade Range, up to the Chilcotin Valley. It would be exceedingly interesting to obtain prehistoric skulls from this area. (Boas to Smith, 5 August 1899, AMNH)

Smith was successful beyond his expectations in collecting skulls from the area, but he seems to have lowered his own ethical standards to do so:

When I began work in the Lillooet Valley I said "If I can only get two skulls I will be surprised and pleased" but in this regard I have succeeded beyond my hope. I have (16) sixteen more or less complete skeletons—all of them are so old that the Indians said I might dig. But with nearly all, evidences of white contact were found. Some were under rock piles but not well formed cairns. Nearly all the skulls are entire . . . by taking skeletons out on backs we got them out without Indians realizing the bulk & so free from objections. But when the Indians return from fishing it would not be pleasant to be here. (Smith to Boas, 19 August 1899, AMNH)

Although he was pleased about being able to make such a large collection of material, Smith was concerned about "running some risks" for the expedition:

I consider that no trouble will arise from my work up the Lillooet and yet as the work was done while only a few Indians were there, those who were absent and have since returned might object. Those that were present did not confront me much and I feel that I would rather let the matter be digested by them before taking up more extensive archaeological studies, which must, of necessity to careful work and preservation of specimens, be done more

openly. The skeletons I collected there and at other places are evidence that I am not trying to get out of running some risks on small insurance. (Smith to Boas, 16 September 1899, AMNH)

Smith is surely making reference to the cautious attitude he had after his father's failed business.

Boas may have thought Smith too eager to investigate areas sure to yield quantities of artifacts and human remains for the museum. While Smith was reporting the quantities of human remains being collected from Lillooet, Boas again became concerned as to whether Smith was pursuing the larger questions of the Jesup Expedition by obtaining material from the entire region being investigated rather than spending too much time at any one site. Boas wrote to Smith suggesting that he return to Stanwood, Washington, to further investigate the relationship between the Puget Sound shell heaps and those of the Fraser River:

It strikes me that you have spent very little time at Stanwood, considering the importance of getting information from a different region similar to Eburne. I wish you would consider if it would not be advisable, on your return from Nanaimo, to go back there once more, to continue your studies. I hope you are not too much influenced in your judgment by the number of specimens you find. I consider it of the very greatest importance to do as much as we can towards the solution of the problem of the distribution of the shell mounds of Eburne character and also of the distribution of cairns on the east and west sides of Puget Sound. Of course, I rely on your judgement in all these matters; but I wish to urge you not to feel too much influenced by the consideration of the number of specimens that you are going to send back. First of all, we want to understand the history and distribution of cultural forms. I hope you will consider this matter while you are working in the Lillooet region. (Boas to Smith, 29 July 1899, AMNH)

Although Boas was providing strong guidance on the direction the fieldwork should take, he clearly felt more secure in Smith's judgment than he had in previous seasons. Smith advised Boas that a return to Stanwood would not have been profitable for the expedition:

I fear I did not give you a clear idea of Stanwood. When the very 1st day I noticed the blackness of the shell heap I wrote you it was like Eburne. I referred to the blackness and to the fact that it was a delta. I now think the blackness due to surrounding delta soil instead of clean sand as in the sea beach shell heaps. There was nothing in the finds at Stanwood to suggest it to be more like Eburne than other places except the skulls, several of which were found. If, after you examine the skulls, we find that they resemble Eburne types or differ from types of which we have information; then by all means I think more data should be secured from Stanwood. If however the skulls are of no particular interest, then there is nothing that I know of to lead us to return to Stanwood more than to many other places. (Smith to Boas, 19 August 1899, AMNH)

In spite of Boas' desire to get more material from Puget Sound, Smith did not return to Stanwood to continue excavations there after he had completed his work at Lillooet. Instead, he followed his plans to return to North Saanich, via Eburne, to continue the work on the cairns and shell heaps that he had started in the previous season. Smith felt he could best address the questions of the expedition through thorough investigation of these previously explored sites.

Eburne

Toward the end of August, Smith traveled down the Fraser River from the Lillooet-Harrison Lake area. He stopped for a day in Vancouver and returned to the Musqueam Reserve in an attempt to collect some of the house posts and spindle whorls he had been unwilling to purchase the previous year, partly because he considered them too high in price. But Smith found the people at Musqueam no longer interested in selling any of their objects for any price to someone who was going to take the items out of the country. Smith was not deterred:

At Eburne I got two carved posts for \$15.00 each. They would not sell them last year but I brought photos of them. I considered that carvings from the Lower Fraser are very much to be desired. They would not sell them to New York even this year, but they sold them

to an Eburne friend who turned them over to me for cost. The Indians who had the fine spindle whorl last year were not home so I had that trip for naught. . . . Indians near Eburne have been told not to sell specimens to people who plan to take said specimens out of Canada. (Smith to Boas, 25 August 1899, AMNH)

Through this deception, Smith was finally able to make a collection from Musqueam. It is doubtful that the people from Musqueam who sold their posts to Smith's Eburne friend were ever informed of their being removed from the country.

North Saanich

The next day, Smith left Vancouver for North Saanich and set up his excavations there just before the end of August. He was very interested in continuing the excavation of the cairns that had been first explored the previous year. He excavated 30 cairns at five different locations in the North Saanich area (Smith and Fowke 1901:65–6; AMNH 43109–43112). He also continued his excavations of the previous year at one of the large North Saanich shell heaps (Smith 1907:331). In September he received word from Boas that his archaeological fieldwork was to terminate so that the material could be worked up back at the museum:

My present idea is, that with all the material that you have in hand at the present time, it would be best for you to stay here next summer and write out what you have. I do not believe that it is a good plan to accumulate more material than we can actually manage. In that case, of course it would be best either to do the Lillooet work this year or to defer it until 1901. I wish you would be entirely guided in these matters by your judgement, on which I rely. I do not wish to interfere in any way with your plans, as I cannot judge from a distance what is best to do. (Boas to Smith, 5 August 1899, AMNH)

Smith agreed with Boas that the coming season would be best spent in New York:

I am glad that you feel that I ought to write up the material in hand. I am sure that I have much, to supplement notes, in my mind which will shrink and become confused with other matters if I delay writing it out too

long. It might be well to write out the matter in shape for publication and then lay it aside. Later after all the work on any certain problem or place was done, changes could be made if the later works required that the first impressions written out be revised. (Smith to Boas, 16 September 1899, AMNH)

With the end of the season nearing, Smith concluded his investigations in North Saanich and returned to Spences Bridge to meet with Teit and make a journey into the Nicola Valley.

Nicola Lake

In the last week of September, Smith became reacquainted with Teit in Spences Bridge. Smith had brought copies of his newly printed "Archaeology of Lytton" (1899b) to British Columbia so that he could show the drawings of artifacts to knowledgeable elders: Baptise from Nicola Valley; Michel from Lytton; Salictè, James, and Charlie Tcilaxitca from Nicola Lake. These elders provided extensive, detailed information on the uses of the objects in Smith's book, which he included as an appendix in his next monograph, "Archaeology of the Thompson River Region" (Smith 1900d:440–2).

With a week to spare before Smith had to return to New York, Smith and Teit set out on a hike into the somewhat remote Nicola Valley. They wished to observe and collect from a number of sites where Teit had heard about a particular burial practice. These burials were unusual in that the deceased was laid inside a tent set up beside a steep bank, after which a rock slide was caused, covering the grave with boulders (Fig. 51).²⁹ The remains from these burials were very well preserved and in some cases included impressive copper grave goods (Smith to Boas, 30 September 1899, AMNH). Smith and Teit also photographed the frame of a sweat house, a "kickulie house," and a group of people they met near the mouth of Nicola Lake (AMNH 43100, 43101–43102, and 43106, respectively). After a week of making collections and taking photographs of the area, they returned to Spences Bridge. Smith packed up the last of his collections for shipping and returned to New York.

Smith's Contributions to Archaeology and the Jesup Expedition

Smith spent the next eight years working at the AMNH as assistant curator of archaeology, "receiving, unpacking, cataloguing, repairing, [taking care of] installation or storage, and the labelling of specimens, as well as answering the questions of visitors and correspondents" (Smith to Putnam, 23 December 1902, AMNH). The exhibits Smith set up at the AMNH had plainly written labels intended for the lay public, but he also made concessions to serious scholars. He illustrated the memoirs of his explorations, which he worked on in addition to his regular duties, with pieces that corresponded to the exhibits, thus giving the fullest possible account of the materials to the scholar.

Smith did not make any more field trips to the coast of British Columbia under the auspices of the Jesup Expedition. He did, however, conduct field research for the AMNH in Yakima Valley, Washington, in 1903 (Smith 1905, 1906a, 1906b, 1910a, 1910b) and on the coasts of northern British Columbia and southern Alaska in 1909 (Smith 1909a, 1910c, 1910d, 1910e, 1910f, 1911). He continued on at the AMNH until 1911, when he moved to Ottawa to take up the important position of Dominion archaeologist for the National Museum of Canada. Over the next two decades, he continued his field research off and on in British Columbia and also conducted pioneering research in Quebec and Nova Scotia. He did not restrict himself to archaeology; he also pursued ethnographic filmmaking and photography, ethnobotany, and the education of the public on Native history and culture. His career has left a lasting legacy in these areas.³⁰

Evaluating Smith's Jesup Work

Boas had determined that Smith's primary research objective was to investigate and report on the archaeological remains of the North Pacific Coast of North America, to shed some light on the relationships between people of the New and Old Worlds. Boas hoped that this information would be able to support

linguistic and ethnological evidence that was collected by other members of the Jesup Expedition (Boas 1902:3, 1903). Smith's additional work in photography, physical anthropology, and ethnology also contributed to the goals of the expedition but remain absent from most of the publications relating to the JNPE.

Reviews of Smith's research by his peers indicate that his work was considered important and well done in its day. Otis T. Mason gave Smith and other Jesup team members "hearty praise" for their research (Mason 1900:805); J. A. McGuire felt that Smith deserved "the thanks of all students of archaeology for the thorough manner in which he has performed his task" (McGuire 1903:552); and even George M. Dawson, who did not like having artifacts and human remains leave Canada, congratulated Smith for "illustrating the archaeology of this interesting locality" (Dawson 1899:767). These reviewers all concurred that Smith had done well in his first task, the description of the archaeology of British Columbia and Washington.

How did this archaeological work address the questions posed by the Jesup Expedition? Smith interpreted his archaeological collections found in the interior of British Columbia as reflecting cultures that were, by and large, the same as those of the present-day inhabitants (Smith 1899b:161, 1900d:432-3). For the coastal regions, his published interpretations state the same general point: that "the finds indicate that the prehistoric people whose remains are found in these shell-heaps had a culture resembling in most of its features that of the present natives of the Fraser Delta" (Smith 1903:188). Smith found the artifacts and artwork of the lower levels of the shell heaps to be almost identical to those of the upper levels.

Confusion about Smith's interpretation of the coastal material persist. Smith, following Boas' hypothesis, makes a case for there having been at some point in the past a replacement of the early coastal inhabitants by people from the interior (Smith 1903:190, 1907:438-9). The main basis for this interpretation was the replacement of the long-skull people by the broad-

skull people, as discussed by Boas (cited in Smith 1903:189). Smith looked for further support for Boas' hypothesis by pointing out similarities in chipped points, tubular pipes, and geometric designs on objects found on the coast and in the interior (Smith 1903:190).

In his own publications, Boas also cites Smith's evidence as supporting his ideas about a Salish migration from the interior. The disappearance of stone flaking, the two distinct types of skulls, and the change of burial practices from cairns and mounds to tree burials all indicated this migration of people into the region (Boas 1902, 1905:96). Boas asserted that the migration came from the interior because longer skulls "are decidedly more [common] with the people of the interior and of the Columbia River than with the present inhabitants of the Coast of British Columbia" (Boas 1902, 1940:528). The interior invasion group was "in later times assimilated by the northern coast tribes in bodily form as well as culture." Making much out of little evidence, Boas cited Smith's brief work in the Puget Sound area as showing "that there was a gradual merging of the ancient culture of this area into that of the Columbia Valley, thus agreeing with the ethnological results obtained by Professor Farrand" (Boas 1903:90).

Smith was clearly influenced by Boas in presenting his model for the migration of people from the interior to the coast (Robinson 1976). His interpretations were always cautious and tended to defer to Boas, both in the field and in his publications. This best example of this is that Smith's letters discuss the great many "intermediate types" of skulls coming out of the shell heaps, but the official publications by both Smith and Boas characterize the skulls as falling into only two types (Smith to Boas, 16 May, 3 June 1898, AMNH). Beattie recently summarized the debate on long-skulls and broad-skulls, showing that there is little physical evidence to support this kind of grouping (Beattie 1985). Confusion about this issue might not have arisen had Boas heeded Smith's intuition about the difficulties in creating two distinct "types" out of a great number of intermediate specimens.

Smith's collections of skeletons, photographs, and plaster casts provided further information with which to address the historical relationships between the peoples of the North Pacific Rim. While the Jesup Expedition was under way, Boas cited this material as evidence that the "types of man" living in each geographic region of British Columbia were distinct, yet historically connected (Boas 1903:74). Smith's collections of skeletal remains were left unanalyzed for 20 years until Bruno Oettking undertook the project during and after World War I. Oettking took careful measurements of the skulls and found several different methods of cranial deformation that corresponded generally to different language groups of the Northwest Coast (Oettking 1930; see also Jantz 1995).

With a few exceptions—notably, a short album of Smith's pictures showing typical profiles of people from the Thompson, Shuswap, and Lillooet communities (Boas 1900) and a plate published by Boas showing Tsimshian, Haida, Kwakiutl, Nootka, Thompson, and Quinault "Indian types" of the Northwest Coast (Boas 1903:83)—Smith's photographs and his ethnological collections were not included in the Jesup Expedition monographs.

Smith's few ethnological publications (Smith 1910d, 1910f, 1911) do not discuss in detail the kinds of information he obtained and recorded in his letters and notes. The few notes from his correspondence presented here, and the lists of names and communities in his photograph records, provide some limited insight into the communities in which he worked. His field notes, now missing, would reveal more material of this nature, if they were to be found.

Archaeologist as Collaborator

Smith's relationships with the Native communities he studied had a profound influence on how his investigations proceeded and on his final descriptions and interpretations of the archaeological remains. Through Boas, Smith had connections with James Teit in Spences Bridge, Father Le Jeune in Kamloops, and George Hunt

in Fort Rupert. This network of people around Boas gave Smith a unique opportunity for research, while limiting him to the areas Boas was interested in.

In the Thompson River area, Smith was able to draw on the excellent community contacts of James Teit and Father Le Jeune. His reports from this area are particularly rich in descriptions of the functions of objects and the history of the sites he visited. Good relations with the community produced better archaeological results. In his work with George Hunt on the Central Coast, Smith gained access to large ethnological purchases. However, the community's good will toward Smith was not always well repaid, particularly in the matter of grave digging.

This tenuous rapport can be contrasted with Smith's work in the lower Fraser River and southeastern Vancouver Island regions, where he had no such contacts. His descriptions of the archaeological materials from these areas are based largely on his own knowledge of the finds and draw heavily on information obtained by Teit from people in the interior. He confined his archaeological investigations in these areas to off-reserve sites, where he could work on land owned by non-Natives. When he did try to excavate on reserve in Duncan, he was unable to obtain permission from the Native leaders. As he could only communicate in English or with his limited knowledge of Chinook, he had a difficult time explaining what he wanted to do or recording what Native people tried to tell him about their traditional way of life. The most extreme case of Smith's lack of community contacts was in Lillooet, where he chose to excavate burials at night, knowing that community members would not have approved. This later came back to haunt him, as he could not return to the area as Boas had wished.

Collaboration with people who had long-term relationships with the Native communities in which Smith was interested also opened opportunities for taking photographs and making plaster casts. Teit, Le Jeune, and Hunt all explained to community members what Smith wanted to do and introduced him to people

who were willing to take part. They provided him with detailed information on the families and backgrounds of the people he photographed and cast. Notes on most of the pictures of people that Smith took on his own tend not to include any details about the subject other than linguistic affiliation. In the case of the Central Coast Salish communities on the lower Fraser River and southeastern Vancouver Island, Smith was unable to take any pictures or make casts of people, regardless of the payment he offered. An opportunity to work with people in this area might have provided insights into the problem of the historical relationship between the Interior and Coast Salish groups.

Contemporary Reflections on Smith's Jesup Work

Long after the questions of the Jesup Expedition have been reexamined, Smith's work continues to be relevant. Native people today are concerned about the relationship of anthropologists to their communities, as research continues to raise issues such as repatriation, local control over cultural resources, and the authority of non-Native scholars to interpret Native culture. The growing interest in the revival of traditional cultural practices is another area in which modern anthropologists interact with local communities.

A particularly important lesson is the difference between "access" to a field site and "acceptance" by the community of the research being done. Gatekeepers like Hunt may not always be spokespersons for the community at large, but they ultimately have to bear the consequences of the researchers' actions long after the fieldwork is over. Whereas Smith could simply continue his research without returning to Fort Rupert, the trouble surrounding his visit had more serious repercussions for Hunt and Boas, who wished to continue living and working in that community. In the case of Smith's work in Lillooet, the community members who did not protest his grave digging would have had to answer to the rest of the community when those who had been absent returned.

A second lesson has to do with the frustration Smith endured in trying to gain access to Coast Salish communities to excavate, take photographs, make casts, and purchase ritual objects. There is a striking absence in Smith's correspondence with Boas of any attempt to understand why people were unwilling to collaborate with him. Being able to engage in a dialogue, as both Le Jeune and Hunt had done, may have moved his work forward, or at least saved him time and effort. However, Smith's and Boas' research strategy of making general surveys of the broad region prevented Smith from building the kind of rapport that would make this kind of dialogue possible. When the research questions are as grand as those proposed by Boas for the Jesup Expedition, a team approach, with specialists in each community where work is being done, is clearly preferable.

Finally, Smith's work on the Jesup Expedition leaves the current generation of anthropologists and archaeologists with the dilemma of what to do about collections made under questionable circumstances. Repatriation of skeletal remains collected in secret or with inadequate permission may now be appropriate. Clearly, as regards the house post given by Chief Nuxwhailak, the AMNH must honor his request by properly labeling it for the public. The house posts acquired through Smith's Eburne friend pose a more difficult problem. Should they have been collected even though Smith and Boas both knew that sending them over the Canadian border was against the Musqueam people's wishes? Would it have been better to have left them to rot or burn, like so many other Coast Salish artworks of that era?

The answers to these questions are not clear. I would suggest that the answers lie in the ongoing relationship between the AMNH and the Native communities whose collections it holds. The Musqueam house posts are now among the very few photographed or preserved from this region and have been highly instructive for the current generation of carvers. A good example is Susan Point's interpretation of some of these

Musqueam posts for the artworks she created for the Vancouver Airport. Access to and interpretation of these collections may ultimately be an end that can justify the means. Thus, the legacy of Harlan I. Smith's sometimes problematic work for the Jesup North Pacific Expedition can have continuing relevance for Native communities and the public at large.

Acknowledgments

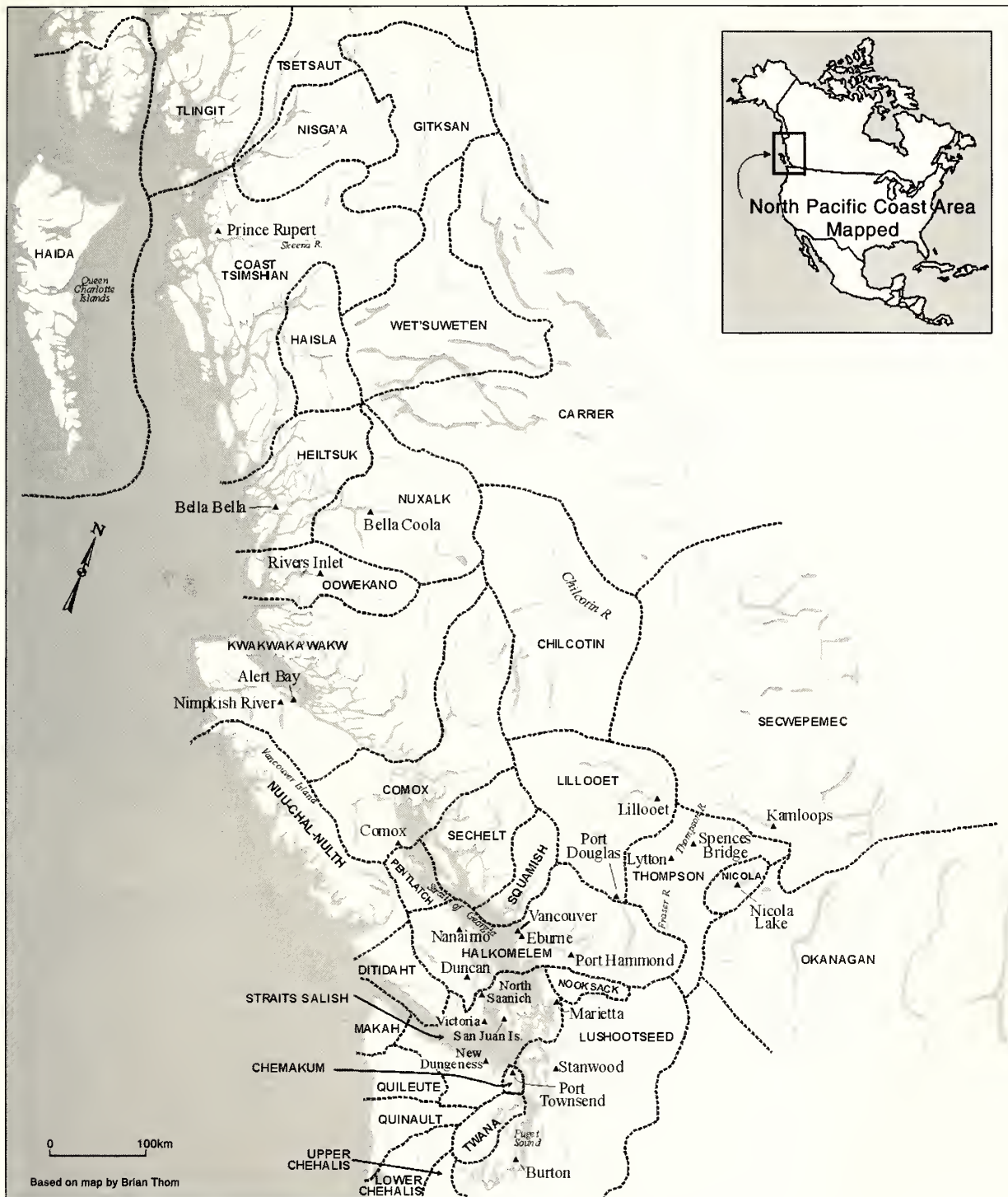
I would like to thank the American Museum of Natural History for the Collections Study Grant I received in 1993 to work on the Harlan I. Smith material. Anibal Rodriguez was particularly helpful with obtaining the correspondence from the accession records. William Fitzhugh and Igor Krupnik at the Smithsonian Institution offered much encouragement and valuable comments on the creation and the subsequent refinement of this paper. The late Douglas Cole also provided valuable comments and suggestions on the final drafts. The University of British Columbia's Travel Bursary subsidized an early presentation of the paper at the 1994 meetings of the American Society for Ethnohistory in Tempe. Finally, I thank my friend Lynn Vanderwekken, who read endless drafts and who gave me so much of her time while I was working on this paper.

Notes

1. The following publications were a direct result of Smith's fieldwork for the Jesup North Pacific Expedition: Boas 1897, 1900; Smith 1898a, 1898b, 1899a, 1899b, 1899c, 1899d, 1900a, 1900b, 1900c, 1900d, 1900e, 1901a, 1901b, 1901c, 1902, 1903, 1904a, 1904b, 1904c, 1904d, 1906c, 1907, 1909b, 1910d, 1911; Smith and Fowke 1901. For a more complete bibliography of Smith's works, see Leechman 1949.

2. Smith's correspondence is in AMNH, Acc. 1897-27, 1898-41, 1899-3.

3. Smith's photograph catalogue at the AMNH records the profiles of people from Spences Bridge as AMNH 11646-11685 and 22634-22695. Smith also photographed sweat houses (AMNH 42754-42755), rock paintings and story rocks



45/ Locations visited by Harlan I. Smith during the Jesup Expedition, 1897-1899



46/ Smith's burial excavation at Kamloops, Thompson River area, British Columbia, 1897 (AMNH 42779)



47/ Detail of Kamloops burial (AMNH 42780)

1898. 41

5/18/98



he let us have
it because we
wanted it for
Educational
purpose and
the 10.00 was
not payment
The pole was
part gift to
museum.

Drawing of house
post
bought for \$10.00 at
Musqueam Reserve
near Eburne B.C.
May 18/98 by Herbert I.
Smith on condition
to be labeled from house
of Kaplanux
grandfather of present
Chief Nuxwhailak
from whom it was
obtained. It was
understood that (over)

48/ House post collected by Smith at Musqueam, British Columbia, 1898, given as "part gift" to the AMNH by Chief Nuxwhailak (AMNH 16/4652)



49/ Archille James, age 19, from Katzie, British Columbia, 1897. Harlan I. Smith, photographer (AMNH 42887)



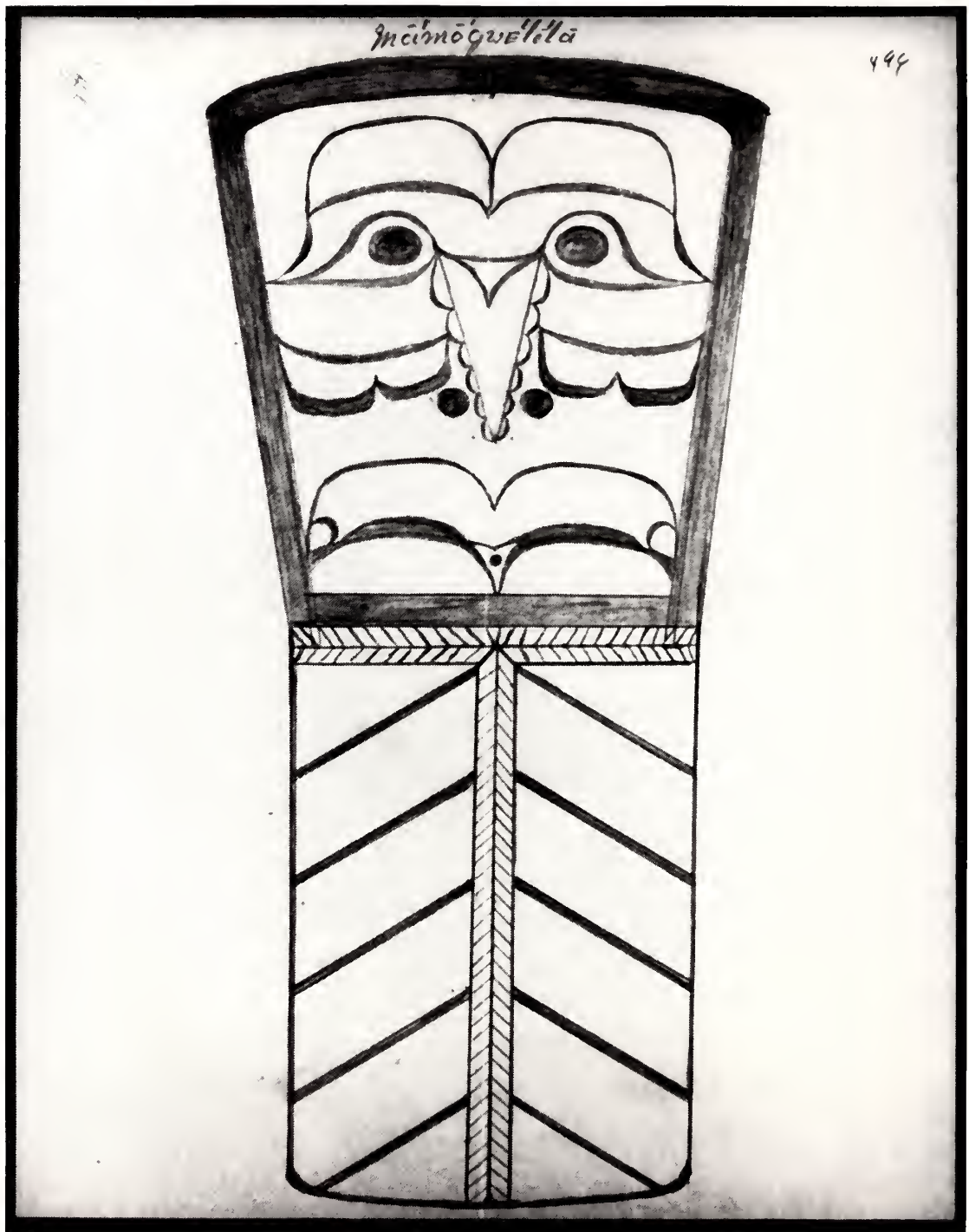
50/ Grave post called "Laxtot," at Comox, British Columbia, 1898. Harlan I. Smith, photographer (AMNH 43022)



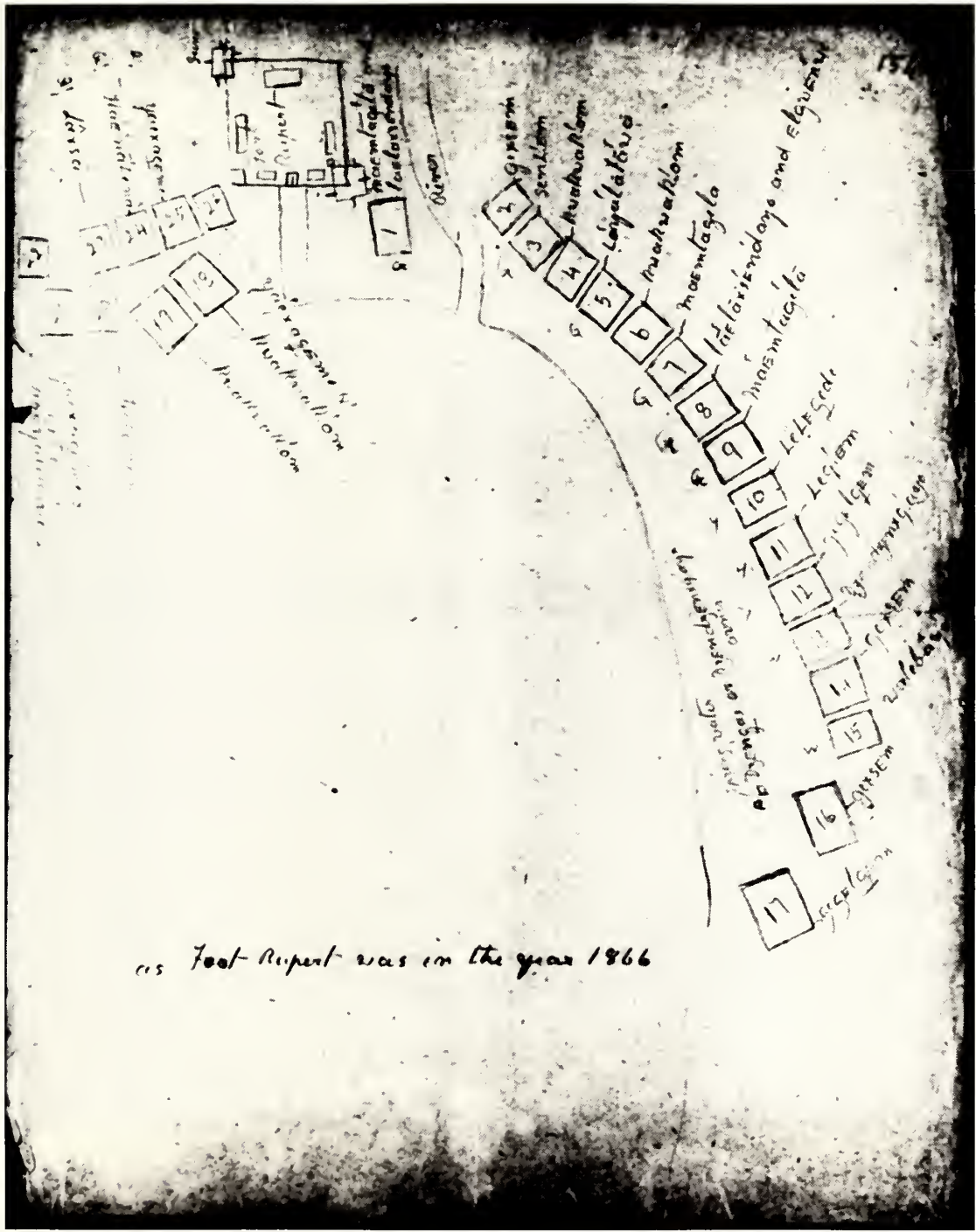
51/ Salish burial ground at Nicola Valley, British Columbia, 1899. Harlan I. Smith, photographer (AMNH 43103)



52/ Map of the Kwakwaka'wakw area in the early 19th century, with Turnour Island and Clio Channel shown as the enlarged area (adapted from *Handbook of the North American Indians*, Vol. 7, 1994)



53/ Sketch of K'odi's copper by George Hunt, 1921 (APS)



54/ Site plan of Fort Rupert (Tsixis) as it was in ca. 1865. Drawing by George Hunt, 1919 (APS)





55/ Fort Rupert (Tsaxis), probably 1865 or earlier. Photographer unknown. HBCA, Provincial Archives of Manitoba 1987/363-F-57/1 (N11778)

Each of the following images of Tsaxis shows the site from a different angle. This earliest image (Fig.55), was taken from the east side of the stream mouth and the fort, near the front of House 18 (as numbered in Hunt's "1866" site plan). Next image (Fig.56) was painted looking north toward the ocean from the higher ground behind the fort. Finally, the third image (Fig.57) was shot in 1881 from the west end of Tsaxis, probably from the site earlier occupied by Houses 16 and 17.



55/ Fort Rupert (Tsaxis), probably 1865 or earlier. Photographer unknown. HBCA, Provincial Archives of Manitoba 1987/363-F-57/1 (N11778)

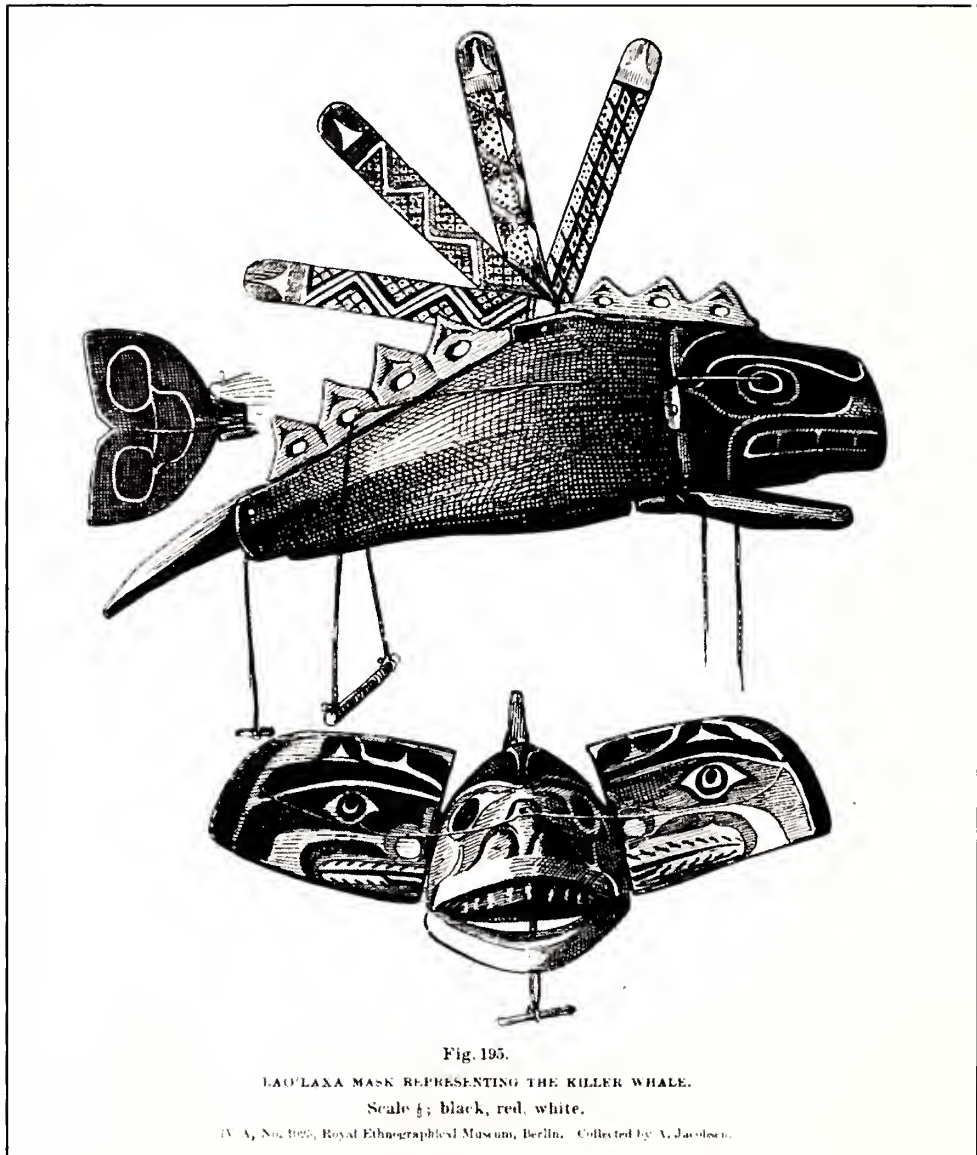
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56/ Watercolor of Fort Rupert, May 8, 1866. Artist unknown. HBCA, Provincial Archives of Manitoba, P-111 (N5296)



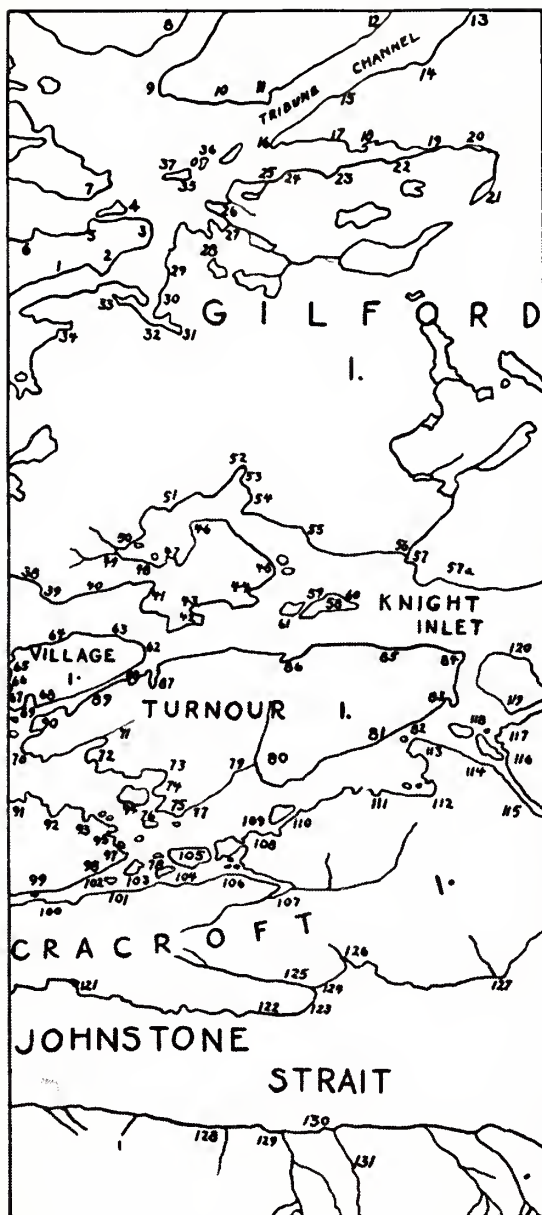
57/ Fort Rupert (Tsaxis), 1881. Edward Dossetter, photographer (AMNH 42295)



58/ Killer whale mask. Reprinted from *Report of the U.S. National Museum for 1895* (Boas 1897:628)



59/ Tlingit seal bowl, southern Alaska (SI 23409)



- | | |
|--|--|
| <p>64. <i>ē s'ēg'es</i> sandy beaches
 65. <i>mā'xmezax</i> place of eating killer whales
 66. <i>*max' mkūma'his</i> round things (islands) in front at beach
 67. <i>*mē' mkūmlis</i> round things (islands) in front at beach
 68. <i>ts'ēnā'ts'ē</i> elderberry receptacle
 69. <i>tl'ē' k'wa</i> stiff (oil, curdled blood) on rock
 70. <i>ō'gwāx'tē'wē'</i> head of passage
 71. <i>dōx'nal</i>
 72. <i>*ya'x'p'ōedēzla</i> bad smell coming up from beach
 73. <i>dzeq'ūzōā'</i> muddy through (clam beach near <i>qā'lō-gwis</i>)
 74. <i>kū' n'was</i> place of thunderbird on rock
 75. <i>qā'logwis</i> bent beach
 76. <i>dēg'a'ts'ē's</i> <i>L'ē'selag'i'la</i> mink's burial place
 77. <i>x'ā'ts'a'dze'li's</i> having great ebb tide
 78. <i>q'ā'bē'</i>
 79. <i>ādap'a</i> village of <i>*wā' laskwā'-g'ul</i>
 80. <i>gū'mbez</i>
 81. <i>laē' lqek'wa'</i> burnt rocks
 82. <i>k'ā' qolē'</i> canoes meeting on water
 83. <i>q'ō' gwadilbē'</i> point having shelter
 84. <i>nō'mas</i> old man, i. e. sea monster; name of many dangerous points
 85. <i>āw'i' L'a'la</i> rocky place stretching inward
 86. <i>ōz'la'li's</i> beach at hind end
 87. <i>g'i' l'ēz'li'li's</i> long behind end beach
 88. <i>ō'gwāx'tē'wē'</i> head of passage
 89. <i>sē'x'sē'wak'</i> paddled through
 90. <i>g'ō'x'dē'ms</i> house site on ground
 91. <i>hānē'</i> hollow thing at rest
 92. <i>L'ē' q'ās</i> canoe building place on rock</p> | <p>93. <i>tl'ō'x'tā'ā</i> small, round opening inside
 95. <i>dēx'sēmā'la</i> grave on surface
 97. <i>ā' lēgēmā'la</i> facing inland
 98. <i>q'wa' lā'ias</i> place of hiding the cedar bark bedding of cradles
 99. <i>*mē'x'stswē'</i> round thing (island) in small hole or opening
 100. <i>dē'mlēwas</i> place of rumbling noise, Baronet Passage
 101. <i>tā'mlēlās</i> trembling point
 102. <i>bēk'w'd</i> having man-of-the-ground, (i. e. a fabulous people)
 103. <i>k'ēq'ūdzē'm</i> young cedars on surface
 104. <i>k'wē' dadē'</i> having barnacles
 105. <i>ā' lēgēmāla</i> facing inland
 106. <i>mā' zās</i> killer whale place (?)
 107. <i>dze'mbar'</i>
 108. <i>nā' Lēwēg'a'laa'</i> turn back to back on rock
 109. <i>mā' ts'ēq</i> stripe in hole
 110. <i>ts'ā' yadē'</i> having eelgrass
 111. <i>q'ā' q'ūx' Lā'his</i> shallow beaches at head
 112. <i>lē' dzadēx</i> <i>qwe'y'i'm</i> having finding of whales
 113. <i>*megw'i' l'ala</i> island being on point
 114. <i>k'ā' k'ēgūnōhis</i> young cedars on side of beach
 115. <i>ō'x' sādēzla</i> beach continuing through
 116. <i>k'ō' k'wa</i> put up on edge on rock
 117. <i>wūxēdats'!</i>
 118. <i>*mā'x' m'x'bē'</i> round things (islands) at point
 119. <i>x'ā' wāp'ēx'dē'mi's</i> open neck place on beach
 120. <i>wā' wūngēwōl</i> deep sides
 121. <i>*nā' lē' wā' x'dzad</i> up river <i>wā' x'dzad</i>
 122. <i>ts'ē' l'x'mēdzēs</i> crabapple trees on beach
 123. <i>L'ā' dzis</i> alder beach
 124. <i>ts'm't'alis</i> trembling beach</p> |
|--|--|

60/ Map of Turnour Island, Clio Channel, and vicinity showing Kwakwaka'wakw historical villages ca. 1840 and a sample of the site names related to this area (from Boas 1934)

(AMNH 42756-42766), a fire drill being used (AMNH 42769-42771), and a storage house (AMNH 42777).

4. Profiles of people from Kamloops are catalogued as AMNH 42745-42755, 22696-22708, 11691.

5. The photograph numbers for Baby Rosie (7 months old) are AMNH 42801-42805; for an unnamed baby, AMNH 42811-42814; for the "Kikulie house ruins," 42815-42816.

6. AMNH 42825-42826 and 11692-11805. Most of the people noted in the photograph catalogue are listed by name and by where the individual is from.

7. These Heiltsuk people, also all named in the catalogue, appear in AMNH 11806-11817 and 42828-42851. The house is shown in AMNH 42852-42857.

8. People from Rivers Inlet are listed by name in AMNH 42862-42885.

9. These skull types are illustrated in Smith 1903:189, 1904c:90.

10. Photographs of these cairns are listed as AMNH 42786-42800.

11. These people, some named and some not, are listed in the photograph catalogue by the community they were from and their age (AMNH 11818-11836).

12. An excellent photograph of this encounter was published in the *Ethnographical Album of the North Pacific Coasts of America and Asia* (Boas 1900). Photograph record numbers are AMNH 42930, 42945, and 43001. See also Mathé and Miller, this volume; and Figs. 37-38, this volume.

13. The woman digging roots is shown in AMNH 42947 and 42957 (Fig. 36, this volume); the tepee structure is shown in AMNH 42931, 42946, and 42948.

14. Pictures of tepees are listed in AMNH 42932, 42938, and 42941; a picture of the sweat house appears in AMNH 42943.

15. Pictures of these excavations include those of human remains (AMNH 42928, 42929, and 42934) and general pictures of the archaeological deposits (AMNH 42927, 42964, 42965, 42975, 42976, and 42995).

16. The pictures of these posts are described in the photograph catalogue at AMNH 42922, 42923, 42924, 42933, 42936, 42937, 42939, 42940, 42942, and 42944.

17. Smith lists the people photographed by name and community in AMNH 11853-11903.

18. Potlatch, AMNH 42967 and 42968; gambling, AMNH 42970 and 42999; women's potlatch, AMNH 42992; totem poles and house posts, AMNH 11905-11907, 42969, and 42991; coppers, AMNH 42984; old man with adze, AMNH 42986-42990 and 42994.

19. Pictures of the shell middens investigated appear in AMNH 42949, 42950, 42952, 42955, 42956, 42958, 42959, 42972-42974, 42979-42983, and 43000; a number of rock carvings were also photographed (AMNH 42953, 42962, 42971, 42978, and 43002).

20. Smith's photographs of these tree burials include AMNH 42951, 42960, 42961, and 42993.

21. Stocking cites this letter as having been written by Boas to Hunt, 3 February 1899.

22. Some of these burials are pictured in AMNH 43022-43026. Smith and Hunt recorded the name of the first of these (AMNH 43022), a grave post, as "Láxktot" and noted that it was "used at potlatch probably as representative of speaker."

23. The posts were photographed by Smith and are listed as AMNH 43022, 43025, 43026, and 43027. Smith wrote to Boas (using letters that refer to a diagram not reproduced here):

I have tried to get posts that were made by Comox people, but I fear northern artists were employed and that northern art shows in some of them. You will be pleased to learn that I secured a story of a flood as an explanation of four of the posts. One post (A) represents a man who made a very long rope of cedar bark. At the time of the flood he took his family, friends, and some animals in his canoe, which he tied to the top of a high mountain by means of this rope. One post (C) represents his friends, another (D) (having a copper carved on it) his wealth, etc, a fourth (E) represents a beaver, perhaps a totem or perhaps simply a tame animal and another friend who represents the carrying aboard of children, etc. . . . One post (F) that was gone represented a bird and other men. I hope to learn more about these and settle a few points, then I will have the full story to go with the poles which, as you say, makes them ten times as valuable. . . .

B, now gone, was a figure of a person like A, but of lesser power. One of the posts from

another house representing a dead man of influence, has a hole in the mouth through which a man spoke. I got all the information I could regarding each pole, but often I find the Indians do not know as well as I. One young woman told me the beaver was a man, but afterwards I found a more intelligent person. (1 August 1898, AMNH)

Of course, from the mythological point of view, animals were in a sense human, and could transform back and forth. Smith's arrogance may have cost him a finer understanding of the stories behind these poles.

24. See also the photographs and descriptions of this site in Smith 1907:323-30 and AMNH 43016-43018.

25. Photographs of objects from the museum are numbered AMNH 43033-43041 and 12063-12073.

26. Smith's correspondence from the end of May to the end of August is almost entirely unavailable from the AMNH accession records. There are, however, two letters that Boas wrote to Smith in the field in the 1899 AMNH accession records.

27. Smith's archaeological findings are well reported in Smith 1907:367-402 and are briefly outlined in Smith 1900a.

28. The photographs show a Nisqually man and a woman sewing a mat in Stanwood: AMNH 12117-12120 and 12134.

29. Some of these burials are pictured in AMNH 43103-43105.

30. For a complete bibliography of Smith's work, see Leechman 1949. Wintemberg (1940) provides an excellent obituary and summary of Smith's research.

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Unpublished Materials of Franz Boas and George Hunt

A Record of 45 years of collaboration

JUDITH BERMAN

Franz Boas, head of the Jesup North Pacific Expedition, was an indefatigable collector of information on the Native peoples of the North Pacific Coast and published many thousands of pages containing a variety of information on these peoples. Thousands more pages, however, remain in the archives, virtually unknown. These unpublished materials are of considerable importance, filling gaps in Boas' published record and inviting fundamental reassessments of some aspects of his work and of the culture and history of the Native peoples he studied.

This chapter on Boas' unpublished North Pacific materials covers only ethnographic, linguistic, and ethnohistorical documents and drawings. It does not discuss his material culture collections, photographs, and phonograph recordings or his physical-anthropology research. This tighter focus allows a more in-depth treatment of materials produced not only during the period of the Jesup Expedition (1897-1902) but also during the years 1894-1942, encompassing the greater part of Boas' professional life.

The greater part of the unpublished materials in Boas' papers was generated in the course of his 45-year collaboration (1888-1933) with George Hunt. Hunt, the son of a Hudson's Bay Company (HBC) employee and a Tlingit noblewoman, married into the Kwakwaka'wakw community at Fort Rupert, British Columbia, and lived most of his life there. Boas hired and trained Hunt to undertake a wide variety of labors, including the assembly of substantial museum

collections. Hunt was responsible, for example, for all the ethnographic material culture collections of the Jesup Expedition for the Kwakwaka'wakw [Kwakiutl] and Nuuchahnulth [Nootka] areas (Fig. 52). Of interest here, however, is the ethnographic, folkloric, and linguistic research that Hunt performed for Boas. The unpublished materials resulting from their collaboration number perhaps 10,000 manuscript pages. Only the most important of the manuscripts that have been identified are considered here.

With one exception—an important document in private hands—the manuscripts considered here are in the three primary repositories for Boas' papers: the American Philosophical Society (APS) in Philadelphia, where the bulk of his professional papers was placed after his death; the Anthropology Archives of the American Museum of Natural History (AMNH) in New York, which contain records from the years 1896-1905, when Boas was employed there (Cole 1985:140, 164); and the Columbia University Rare Book and Manuscript Library, where Boas himself deposited a number of manuscripts. (See Appendix A to this chapter for a list of the individual collections, with the abbreviations for them used here.)

The Unpublished Volume "Kwakiutl Texts"

During his lifetime, Boas published 11 volumes filled largely or exclusively with Kwak'waka language texts written by Hunt (Boas 1909, 1910, 1921, 1925, 1930,

1935, 1943; Boas and Hunt 1905, 1906). Boas' papers include an additional complete text volume that exists in two forms: as a collection of Hunt manuscripts at Columbia University (CU-Hunt xiv) and as a typescript prepared from those manuscripts, with Boas' added translations, at the American Philosophical Society (APS-KTT, see Appendix A). Boas evidently intended to publish the volume but did not manage to do so before his death. Together with the texts in *Religion of the Kwakiutl* (Boas 1930) and another unpublished set of Hunt's manuscripts that will be examined later in this chapter, this unpublished volume represents the bulk of Boas' and Hunt's ethnographic labors in the final decade of their collaboration.

Boas' final typescript volume of Kwakiutl texts is an important supplement to the published record on the 19th-century Kwakwaka'wakw. It also provides a more complete picture of Boas' ethnographic goals, revealing that he was interested in a much wider range of topics than had been recognized.

The volume contains a variety of text materials, most of them produced after 1920. One portion consists of a series of speeches delivered on various public occasions, including those given at feasts and at the Winter Ceremonial. A slightly larger section is devoted to informal conversations, ranging from marital quarrels to a discussion about plant roots between two old basketmakers. Hunt transcribed these conversations in response to Boas' request that he collect some "ordinary, everyday conversations . . . for instance like anything you might say to your wife or to your friends" (Boas to Hunt, 15 December 1927, APS-BPC; see also Boas to Hunt, 18 January 1928).

Boas apparently felt a need to add to the range of speech genres and subject matter represented in the texts he had already edited and published. As he had written earlier in regard to linguistic research in North America, "Up to this time too little attention has been paid to the variety of expression. . .we have hardly any records of daily occurrences, everyday conversation. . .and the like" (Boas 1940a:200-1). Boas' neglect

of the "informal" culture of the Kwakwaka'wakw has been commented on (Codere 1966:xvi; Ray 1980), but the problem was clearly not absent from his mind.

The volume also contains a number of explanatory and narrative texts that are more typical of Hunt's work but that cover specific topics not dealt with elsewhere. These include texts on medicines and on methods of and customs relating to fishing, hunting, and food gathering. One of the latter texts is a brief discussion of the taboos observed in relation to the six kinds of fish that the Kwakwaka'wakw "treated clean" (*a'ikila*): oolachan, halibut, and four species of salmon (sockeye, king, coho, and dog).¹ As Hunt's interlinear English (in his own orthography) states,

they Dont let the . . . young women who have the first monthly Eat any of these . . . fishes . . . [if] have the monthly [the] wife of the salmon fisher . . . [,] the Husband . . . go carry the suck Eye in[to] the House of his Relative for her to Roast it. and they Dont let the wife of his Eat some of it whele she got the monthly. and as soon as she Done . . . then she wash herself . . . and . . . then she Eat the Roasted salmon . . . if [the young woman] Eat the Roasted salmon . . . then it would Disappear . . . and her Father . . . would get into some trouble [*talawafid*, "get into difficulties"] . . . and when they . . . finish Eating the six Defferent kind of fishes. then Right away then the woman go gather up the skin not Eaten and the Bones and she go walk out to the salt water and she throw it into the water . . . [and also] the Entrails and the Blood on the mat they cut the salmon on . . . [to be] washed off . . . in salt water. for it is not allowed the Dog to eat [anything] that came from the six Defferent kind of fishes when it is first caught for . . . [the fish] would Right away Desappear. (CU-Hunt xiv:4359-61)

This text is the only one in the Boas-Hunt corpus to discuss fish ceremonialism among the Kwakwaka'wakw in any comprehensive way. It omits, however, topics that Hunt briefly touches on elsewhere—for example, the prayers addressed to the first salmon to arrive and to any salmon after it is caught, or the resuscitation of the salmon after its skin, bones, and entrails are placed in salt water (CU-Hunt xiv:3919-33;

Boas 1921:246, 609–612; Boas and Hunt 1905:307, 390–2). These short texts show that fish ceremonialism—that is, the ritual, prayer, and taboos surrounding the catching, preparation, and eating of certain species of fish and the disposal of the remains afterward—was pervasive in 19th-century Kwakwaka’wakw life. Given that the ritual and taboos applied to fish that were caught and preserved in huge numbers in order to provide year-round food staples, these texts suggest that fish ceremonialism was the most fundamental form of ritual activity at this time, carried out daily by women in every household. Because of this, the texts cast light on other areas of religious expression, particularly on the far more spectacular Winter Ceremonial, which seems to use the spiritual ecology of fish as its root metaphor (Berman 1991:659–702; 2000).

Other texts in the volume show that Boas and Hunt were interested in the margins of gender among the 19th-century Kwakwaka’wakw. One text tells of women who have taken men’s names and positions in the potlatch system and who thereby “turn into men” (“babēbagwēxa ts!edaq”; CU-Hunt xiv:4135–6). Another recounts a “sham marriage” in which a chief turned his only child, a son, into a “woman on one side” and then gave the “woman half” of his son in marriage to another chief as a fictive daughter. Still another discusses Kwakwaka’wakw transvestites.

The volume also contains unique texts on an assortment of other topics, including a rare description of the great feast pipes of the Kwakwaka’wakw, which were smoked by as many as six men at once (see also Boas to Hunt, 10 March, 23 July 1920; Hunt to Boas 9 July, 2 September 1920, APS-BPC), and texts that Hunt wrote in response to Boas’ request for examples of how children were instructed (Boas to Hunt, 22 May 1928, APS-BPC)—a reflection of Boas’ interest in the socialization of children that emerged at the end of the 1920s.

Among the handful of valuable ethnohistorical texts in the volume is one about Hunt’s trip to the west coast of Vancouver Island in the fall of 1871, when he

was 17, to buy 20,000 tooth shells (dentalium) for the Hudson’s Bay Company (CU-Hunt xiv:2193–290; Hunt to Boas, 20 October 1921, APS-BPC). This long manuscript, which Boas split for publication (APS-KTT:111–3, 150–69, 270–5), has several noteworthy aspects. Hunt’s trip casts light on HBC operations on the coast during this time, especially on the company’s reach into remote areas and the role of Native or part-Native middlemen. The account also describes villages and peoples that had all but disappeared by the time other observers reached the area. Moreover, Hunt’s manuscript gives a detailed account of the methods of gathering and preparing dentalium, a valued aboriginal trade good that was most abundant on the west coast of Vancouver Island. There is no other known account of Kwakwaka’wakw dentalium harvesting. Hunt’s account includes lengthy descriptions of two Kwakwaka’wakw weddings, one of them his own (paraphrased in Boas 1966:56–61). Finally, the text, along with several others in the volume, tells of incidents of war between Nuu-chah-nulth and Kwakwaka’wakw groups and provides other accounts of conflict between coastal groups.

Yet another text tells of missionizing activities in Fort Rupert and the more northerly Kwakwaka’wakw village of Xwamdasbe’, more widely known as Newiti, beginning in 1860 (CU-Hunt xiv:2978–3040; see also Hunt to Boas, 31 May 1924, APS-BPC). Hunt describes a succession of missionaries who passed through these communities with little apparent effect and records a series of Chinook Jargon prayers and hymns dating from 1860. In one of the episodes, the Rev. A. J. Hall, a Church of England missionary, and a Catholic bishop, “Bishop lemons,” battled for souls in Newiti, while the HBC, represented by Hunt, competed with an independent white trader for fur seal pelts.

Again, this text is the only known account to deal with missionary activities in any detail. The relatively detached viewpoint used in describing these activities and their effects is important to note. It is interesting that the Kwakwaka’wakw response to missionary

fervor in those early years seems about the same as their reaction to a “sun dance” movement that Hunt describes elsewhere, which came north from the “Victoria or American Indian” in the 1840s or 1850s (APS-KM ii:68–70). In both cases, the Kwakwaka’wakw seemed willing to try a new form of public ritual but did not find the new ceremonies sufficiently compelling to sustain their interest.

Hunt personally observed most of the events in these accounts, but perhaps the most important ethnohistorical text in the volume describes an incident that occurred before his birth and that has been the subject of debate among historians of the North Pacific Coast (Bancroft 1887:274–5; Fisher 1977:51–2; Gough 1984:32–49). This incident, which began when hunters from the T’la’lasikwala division of the Kwakwaka’wakw killed three HBC deserters, escalated into a pitched gun battle between two divisions of the Kwakwaka’wakw and the British Navy. Boas asked Hunt for “the Indian version of that affair” (Boas to Hunt, 23 February 1928, APS-BPC). The resulting account, which Hunt pieced together from several sources, is the only one that tells the story from the Native point of view (APS-KTT:191–7; CU-Hunt xiv:3924–43).²

Hunt’s version, given in English only, generally agrees with those of white historians as to the nature and sequence of events. Both tell of the murders and the concealment of the bodies, the dispatch from Fort Rupert of a Native mediator and fact-finder, the visit of a British warship to the village of P’atlams on Nigei (Galiano) Island, and the refusal of the Indians to hand over the perpetrators. Both versions describe the destruction of P’atlams, a further search for the perpetrators, the destruction of a second Native village on Bull Harbour on Hope Island, the death of an important chief during the fighting, and the settlement of the affair when the Natives handed over dead bodies that were supposed to be those of the murderers.³

Hunt’s version differs from those of white historians, however, in many details. The areas of greatest disagreement are those of time frame and general

setting. HBC and colonial government records place the events entirely within the summer and fall of 1850, in the first year of Fort Rupert’s existence (Gough 1984:32–49; Johnson 1972). The erroneous date of 1848 given by Hunt seems to have originated in the letter Boas wrote requesting the account (Boas to Hunt, 23 February 1928, APS-BPC). Hunt’s account also differs in setting the British reprisals a full year after the murder, but this may be an error in translation from his Kwakwaka’wakw sources: the Kwakwaka’wakw counted each season, summer and winter, as a year (Hunt to Boas, 19 November 1911, APS-BPC). Hunt also states that the murdered men were two whites who deserted a whaling ship named “Bobalits” that had anchored near the northern tip of Vancouver Island (CU-Hunt xiv:3934). Historical records, however, indicated that the deserters were three indentured sailors from the HBC ship *Norman Morison* (Gough 1984:40–1). The reference to a whaling ship may have arisen from a memory of another incident.

The more important differences lie in the perspective and in the nature of the story being told. White historians have been preoccupied with the question of whether colonial and military authorities were justified in punishing the entire tribe for the misdeeds of a few. In their accounts, the Natives appear as a volatile, dangerous, and not particularly comprehensible mass. Hunt, in sharp contrast, focuses on the character and actions of individual Indians, on ambivalence and conflict within the Native community, and on the consequences of the Natives’ unfamiliarity with the white men who would increasingly dominate their lives.

According to one historian, the white deserters had been killed “for refusing to submit to some extravagant demands” (Gough 1984:41). Hunt says nothing of any such demands; he portrays the murderers, who were out that morning hunting for seals, as shooting the white deserters almost for sport, or perhaps to obtain their possessions:

[T]hey [the deserters] arrived among the little Islands about one mile west of p!*ELEMS*

[P'atlams] in the morning and they were laying Between two Islands in a narrow Passage must have thinking about to Haul up their Boat into the wood and Hide there that Day. and as soon as one of the white men Jump ashore . . . three Indian men . . . came round a Point East of them not sixty yard [away] . . . and Right away ts!äḡE yos and yemḡwäs take there guns and ts!äḡE yos told to yemḡwäs. you shoot at the man standing at the left side . . . and I will shoot at the other, said he as they fired. and killed Both of the two white men. and the three Indians went to the Place and took off all the cloths of the two Dead men. and after took Every-thing off them and then they carry the Dead Bodys and Burry them in a Hollow tree. (CU-Hunt xiv:3934)

The murderers, who were said to be “the two great warriors” of their T'łat'łasikwala division (CU-Hunt xiv:3936), told no one, not even their closest associates, what had happened. Soon, other white men arrived, searching for the deserters, whom they believed to be still alive. A chief, Yakudłas (Hunt: YäqoLäs), told the whites that no one at P'atlams had seen the men or their boat. Colonial authorities later judged the Newiti to have been lying (Gough 1984:41), but according to Hunt, Yakudłas began to suspect what had happened only when one of the murderers began wearing a white man's shirt and trousers, which he implausibly claimed to have purchased at Fort Rupert.

In Hunt's account, Yakudłas' role is a crucial one; in a way, he is the hero. Hunt's story also centers on Kwakwaka'wakw ambivalence about warriors. Boas states, “Warriors [*babak'wa*] were generally disliked and feared by the rest of the people. They were taught to be cruel and treacherous and to disregard all the rules of decent social behavior” (Boas 1966:106).⁴ At the same time, in an age of intertribal warfare and slave raiding, warriors were defenders of the community.

Yakudłas feared the two warriors who had done the killings and dared not confront them directly. He tricked one of the murderers into confessing the deed to his lover, but nothing of substance occurred until the HBC dispatched to the scene a Fort Rupert chief named Nenagwas (called “Old Wale” by the HBC men;

Gough 1984:41). Yakudłas confided in Nenagwas, who promptly made a full report of all he had learned to the white authorities. The subsequent arrival of a British man-of-war, demanding that the murderers be handed over, precipitated a crisis. One old T'łat'łasikwala chief said, in some disgust, “[I]f I had Power over these two Bad men. I would send them off to the . . . man-of-war. and let them Do as they like with them” (CU-Hunt xiv:3939). But if the two warriors were unpopular, they were still important members of the community. The warriors argued that they had only done their duty: “the Rules given to us By our forefather is to kill the first foreigner or stranger we meet” in the territory controlled by their division. This argument was accepted by most of the villagers.

lots of the men . . . cried out we will fight against the white men. sooner than let them take ts!äḡE yos and yemḡwäs away. and turn our great warriors into slaves. and we know well that threat to Burn Down our Houses . . . has no meaning. (CU-Hunt xiv:3839)

Yakudłas pondered what the best course of action would be.

“[W]hat can I say my tribes People [?] . . . it is true we Dont know the ways of the white People. about the murder and the only thing I say [is] for you all to take good care in case they carry out thier threat.” (CU-Hunt xiv:3840)

As it happened, the British made good their threat to burn the village to the ground, but the inhabitants had already fled. Still trying to apprehend the murderers, the British man-of-war proceeded northward to another village, at Bull Harbour, where they were met with gunfire. The British assumed this to be a hostile gesture (Gough 1984:44), although, according to Hunt, the Indians were merely attempting to “frightens the Boats away.” The sailors landed anyway.

still the Indians shooting at them and one of the Boats midshipman a Very young man saw a Indian Runing along in Front of the Houses. and the midshipman toke his Rifle up and he fired at the Indian. (CU-Hunt xiv:3942)

This act provoked the Natives to shoot to kill. In response, the British boats bombarded the village with field artillery, driving the residents into the woods. Then the sailors destroyed the second village, breaking up all the canoes and burning down the houses.

Hunt ends his story with a series of outcomes that are made ironic by his treatment of character. The murderers—the two men whose actions had rendered their fellow tribespeople homeless and impoverished at the onset of winter—escaped arrest and punishment. They accomplished this by killing two more men, a pair of “innocent slaves,” as Hunt says, and causing the bodies to be delivered to Fort Rupert. “Of course the white men Believed these two Dead men are the murderer[s],” who had been killed by the T’at’asikwala to end the trouble.

As for Yakudʔas, the conscientious and percipient chief who had worked the hardest to discover the truth and avert disaster—he was the only Native killed during the entire affair.⁵ Yakudʔas was the Native seen running along the beach who was shot by the midshipman Hunt mentioned. His death was the reason “why the Indians Begin to shoot to kill.”

The intermediary Nenagwas, whose role in the affair seems ambiguous at best, did not escape entirely unscathed. As he watched while British sailors destroyed a second village of his friends and neighbors, he

was sitting in one of the Boats. Dressed with Button Blanket. and he was wearing a *gëqeml* or large chief Hat and he Had a Bone with abalone shell Decoration Ear Hanger . . . and while the sailors Breaking up the canoes *wäläs* the chief told *ts!ägē yos*. you are a good shot you take a shot at *nā nagwas* you shoot at his Head and then *ts!ägē yos* take a Rest on a stump of a tree. and he fired and the Ball struck *nā nagwas* Ear and cut the string that go through his Ear to Hold up the Bone Ear Hanger. (CU-Hunt xiv:3943)

Boas’ “Kwakiutl Dictionary”

A second volume that Boas was not able to see to publication was his typescript Kwak’wala dictionary (APS-KWD). This dictionary, located at the APS, is an

important linguistic document, and by far the most comprehensive dictionary of Kwak’wala in existence at this writing.⁶ It is based on a huge corpus of material, including over 2,000 printed pages of Hunt’s text, many more pages of unpublished materials, Boas’ own not insubstantial linguistic fieldwork, and 45 years of epistolary questions-and-answers between Boas and Hunt on many points about the language. The dictionary was constructed in relation to the text corpus; almost every entry gives textual citations, often numerous ones, from the published text volumes.

This unpublished dictionary reflects a phonological and morphological understanding of Kwak’wala that is far more sophisticated than anything Boas published in his lifetime. Together with Boas’ posthumously published grammar (1947), the dictionary shows that Boas, while perhaps lacking the sheer analytical brilliance of his student Edward Sapir, was nevertheless one of the most talented linguists of the first half of the 20th century.

One sees in these works, however, a certain amount of inconsistency, even contradiction, in Boas’ treatment of linguistic structure. Here, as almost nowhere else in his corpus, Boas’ analysis reveals simplicity of pattern within the massive multiplicity of his data. One could cite his presentation of such topics as the phonological structure of roots or the morphophonemic changes undergone by stems in various circumstances.

At the same time, Boas appears at first glance to be unaware of or uninterested in systematic patterning in other areas of the language—patterning that his own data seem to reveal quite clearly. The issue of categories within the very numerous “stem-suffixes” (a morphological class) provides one example. “Stem-suffixes” can express, in Boas’ words, “denominative, predicative or adverbial concepts” (Boas 1947:225). Boas rejected the efforts of Sapir and Swadesh (1939:236) to classify such suffixes in the Wakashan and Salishan languages. The criteria these men used, he argued, were “not based on internal evidence, but rather on our European classifications.” Boas did not

see any “internal evidence” in Kwak’wala either, choosing instead a general semantic categorization for his grammar that, he said, “should be considered merely as a convenience designed to give an impression of the range of ideas expressed” (Boas 1947:237).

There are, however, functional distinctions between various categories of Kwak’wala stem suffix. One such functionally defined category is that of the numeral classifiers (Berman 1990). When counting objects in Kwak’wala, numbers and other quantifier stems nearly always appear with one of a strictly limited set of suffixes. The suffixes are divided between “sortal” and “mensural” classifiers, the first indicating the shape of the object being counted—bulky (round), long, flat, hollow, and so on—and the second indicating a measurement, such as number of days, armspans, or layers (Berman 1990:38; see Lyons 1977).⁷

Nearly all of this information can be found in Boas’ grammar, but it is obscured by his presentation. Boas mentions five of the sortal classifiers in a list of “classifying suffixes” that he states are used in counting objects (Boas 1947:279). His phrasing in that passage, though, suggests that these are the *only* classifying suffixes used with numbers. In a different part of his grammar he again lists the 5 suffixes, together with 14 others, in one of his categories of “convenience” labeled “limitations of form” (1947:240). He makes no explicit statement about the use of these 19 suffixes. In nearly every example he gives, however, the suffixes are used with a numeral. Examination of quantifier phrases in the Kwak’wala texts reveals that all or nearly all of the 19 are in fact numeral classifiers. This would seem to indicate that Boas understood the rule he did not state: these 19 suffixes belong to a category that is clearly and unambiguously defined in terms of function—use with numbers.

Another example is provided by the locative suffixes. The stem suffixes of this category are numerous and highly productive in the 19th-century Kwak’wala of Hunt’s texts. At least three factors define them as a class. First, the plural element (*-am-*) requires a

following locative suffix. Second, four suffixes expressing various kinds of determinate and indeterminate motion are always followed by locative suffixes. Finally, there is a set of stems that require locative suffixes. As it happens, these stems also express the shape gender of the object to which they refer, i.e., whether it is flat, long, hollow, and so on.

Again, this information is not lacking in Boas’ grammar and dictionary; it is merely scattered, and difficult to find if one is looking for information on locative suffixes. Boas does explicitly state the first two of the three rules, but only in his remarks on other, nonlocative suffixes (Boas 1947:302, 349–50). The third rule can be gleaned from the entries for the stems in his unpublished dictionary. For instance, for the stem *makw-*, Boas has the gloss “a round thing is somewhere (sing.),” and he further gives numerous examples of words formed from these stems using locative suffixes, including *maxwt’so* (round things inside, i.e., seeds), *makola* (round thing stationary on water, i.e., island), *magwap’e* (round thing in nape of neck, i.e., occiput) (APSKWD:145–6). Although Boas nowhere discusses locative suffixes as a group—they are split among at least four of his categories of “convenience”—he definitely understood the rules that defined them as a category.

Moreover, it seems clear from Boas’ glosses that he perceived the semantic patterning common to numeral classifiers, the shape-expressing stems just mentioned, and a subset of the locative suffixes, the shape locatives. The latter refer to the location of an object or activity in terms of a feature of its shape—for example, *-ba*, “end of long horizontal object.” Among other things, Boas uses a common vocabulary in glossing classifiers, shape stems, and shape locatives: “round,” “flat,” “long,” “human,” and “hollow.” These are also the five shape classes he lists in his brief comments on the use of numeral classifiers (Boas 1947:279). Other usage indicates that Boas perceived additional common features of the system; for example, the orientation of the object (horizontal, vertical, upside down, etc.).

In contrast to Boas' presentation of these points in his dictionary and grammar, his inquiry into the shape-expressing stems and suffixes, as revealed in miscellaneous linguistic materials in his papers, was systematic and extensive. He sent Hunt numerous lists of forms in English to translate into Kwak'wala, or in Kwak'wala to correct or translate into English (APS-KM i:4529, ii:1358-60, iii:4810; APS-KEM iii:384-5; see also APS-KM iii:1584-97; Boas to Hunt, 10 September 1918, 20 February 1919, APS-BPC). Boas had clearly identified the various categories and patterns. What he did not do was make formal and explicit in his dictionary or grammar what he had learned about the system.⁸

The Boas-Hunt Correspondence

In the context of the entire Boas-Hunt corpus of written records, the most important set of unpublished documents is their correspondence. These letters span the years 1894 to 1933 and number over a thousand pages, split more or less evenly between those from Boas' hand and those from Hunt's.

The correspondence is divided among several locations. The bulk of it can be found in the Hunt file, Boas Professional Correspondence, American Philosophical Society (APC-BPC). Quite a few individual letters are scattered through other document collections at the APS (e.g., Boas-Hunt Kwakiutl Materials, KM). The Anthropology Archives at the American Museum of Natural History (AMNH) holds a significant body, divided between the Hunt correspondence files and accession records (designated, respectively, as AMNH-HCF and AMNH-HAR in this chapter). These letters are from the years 1894 to 1905, and most are connected with the Jesup Expedition. Finally, a few letters are among the Hunt manuscripts at Columbia University (CU-Hunt).

The Boas-Hunt correspondence is important because it documents the two men's published and unpublished ethnographic record so minutely. The Boas-Hunt ethnography of the Kwakwaka'wakw was, as I have said elsewhere, an epistolary ethnography (Berman 1996). Boas sent his requests for information by letter;

Hunt sent a letter in return with just about every shipment of texts, objects, or photographs. Both sides of the correspondence are of great interest.

Just as Boas' unpublished linguistic materials reveal that his thinking on several linguistic subjects was far more organized and insightful than his publications would suggest, his letters reveal much about the nature of his ethnographic research that is invisible elsewhere. I have discussed this issue in some detail elsewhere (Berman 1996); here I would like just to mention the key points.

The correspondence shows that Boas' research proceeded in a logical order that is not obscured by the numerous digressions. In the 1890s Boas was most concerned with collecting material culture for museums. In the first decade of the 20th century he moved to an examination of technology, foodways, ethnozoology, and ethnobotany. By the latter part of that decade he had taken up social organization—a subject he actively pursued until the 1920s, when he began questioning Hunt about “the way the Indians think and feel” (Boas to Hunt, 19 September 1920, APS-BPC). By the late 1920s he had become interested in the socialization of children (Boas to Hunt, 22 May 1928, APS-BPC).

The letters show that on any given topic, Boas pursued information in an orderly and systematic fashion. The kinds of questions he posed to Hunt were often no more than standard anthropological queries on such subjects as the use of a particular plant species or the possibility of parallel-cousin marriage. But other texts were intended by Boas to be case studies to help him sort out areas of social organization or religion that he found difficult to understand. Boas has been criticized for the endlessness, obscurity, and triviality of his texts, yet there is little that is obscure in the questions he posed to Hunt.

Hunt's letters are an equally rich source of information. First, they often provide considerable context for individual Hunt texts. For instance, one narrative text appears in published form with a typically terse

annotation by Boas, "A Koskimo story, recorded by George Hunt" (1943:202). When Hunt transmitted this text, however, he sent along the following comment:

on the 19th I send you Page 2097-2111 the true whole story about *nekwel!ag̃eme* or night-time Hunter. who was towed across the ocean By the rotton wood Hair seal. This old man *negätse* told me this same story few year ago. . . . and now this time I made a special trip to *gosgi mox* to see this old man *negätse*. which told me this story again. and he told the story just the way I write it Down . . . next I will try and find out as you say about the Birds of the upper world. (Hunt to Boas, 25 April 1921, APS-BPC)

Hunt's letters also contain ethnographic and ethnohistorical information not found elsewhere. For instance, they supply otherwise unavailable information on genres of oral literature (Hunt to Boas, 4 July 1916, 28 February, 10 March 1917, APS-BPS). Together with comments Hunt made in texts and in other unpublished materials, these statements allow reconstruction of the taxonomy of 19th-century Kwakwaka'wakw oral literature, despite Boas' near-total silence on the topic (APS-KM iii:4624; Berman 2000; see also Berman 1991:117-34).

A more extended example is to be found in Hunt's detailed comments on the history of "coppers," the highly valued copper plaques of the North Pacific Coast. What started Hunt on this topic was Boas' request that he confirm a story regarding a copper recently purchased by the Peabody Museum in Cambridge, Massachusetts. Hunt wrote to Boas,

I called the oldest men of Fort Rupert into my House. and I Read the . . . story to them about the copper. and the 12 slaves and Blankets paid for it. all the old men laughed loud. and said . . . in all the coppers which was Brought Down to Fort Rupert By the Haida and tsimshians and kelkatla . . . [the highest number of] slaves Paid on the[s] copper[s] . . . is one slave a little saanich [Salish] girl. and the Highest Price Paid for a copper By these People [in those days] is from 40 to 86 blankets. (Hunt to Boas, 4 December 1921, APS-BPC)

Hunt told Boas that the Peabody Museum's copper

had been left in Fort Rupert by a Nisga'a man who had hoped to sell it there but had been called home by a sudden illness in his family. The Fort Rupert acquaintance with whom he had left his copper was unable to sell it because it

have to much Ring in it. what the Indians Dont want to Buy. for it shows that it is white mans sheet copper for the true native copper have no Ringing sound in them for the face of Body . . . is all scale or Rough that shows where its Been Hammerd By Round stone. (Hunt to Boas, 4 December 1921, APS-BPC)

Hunt went on to relate the history of another copper, a story that illustrates some of the changes occurring in Kwakwaka'wakw society during the second half of the 19th century (Fig. 53). The main character in Hunt's story is a man of a certain entrepreneurial bent named K'odi (Hunt's K!âde):

now this man *K!âde* is not a chief son. his one of the first man who takes for his wife a Pretty young women. and take her Down to Victoria. and makes money from her. and By this kind of Badness he cought up to what the true chiefs where Doing in there Potlatches . . . But when he Had a Row with them they soon let him know that he was a common class man. (Hunt to Boas, 4 December 1921, APS-BPC)

K'odi gave lavish potlatches from these dubious proceeds in order to raise his status. Then he secretly hired a Haida man to manufacture a forgery:

and after the Haida man fineshed, he was told By *K!âde*. to Pretend and Put it up for sale. to all the Defferent tribes who use to go to Victoria. so this Haida man show the copper to all the chiefs. in a feast that was given by *K!âde* and all of the Defferent tribes looked at the copper. and of cou[r]se they Each one of them wants to Buy it. But they Had no cash or Blankets with them. so . . . *K!âde* asked the chiefs. now if I Buy this copper. who will Buy it from me. and give me Double the Price I would give for it. and one of the chiefs said I will Buy it from you . . . so *K!âde*. gived the Haida man one Hundred Dollers for the. copper.

K'odi told the chiefs that the copper was an old one that had belonged to the Haida man's grandfather. "[I]t was so well made," Hunt wrote, that "one of

the chiefs Did take it and Paid K!âde two Hundred Dollers worth of Blankets for it."

But K'odi and his Haida accomplice had a falling out when K'odi took back half of what he had paid in public to "purchase" the copper. In retaliation, the Haida man

told the chiefs that K!âde . . . Paid him the other fifty Dollers for makeing the copper. and that K!âde. paid three Dollers and fifty cents for the copper sheet from a store . . . in the year 1873. and this copper Been sold so meney times now the Price is twenty thousand . . . Blankets. (Hunt to Boas, 4 December 1921, APS-BPC)

Materials for the ethnography and ethnohistory of other North Pacific groups occasionally surface in Hunt's letters. One story concerns the fate of the companions of a Haida chief, "Getqon" (or Gêdexân as in Boas 1966:107), who in 1856 made the mistake of visiting Fort Rupert and was killed trying to escape the wrath of the resident Kwakwaka'wakw (Hunt to Boas, 5 December 1921, APS-BPC; Boas 1966:107; Travis 1946:33). Hunt also sheds light on his Tlingit mother's home village of Tongass. For example, in one letter Hunt describes an occasion on which he witnessed the construction and use of "the sweat Bath of the *Alaska* Indians" (Hunt to Boas, 28 September 1918, APS-BPC). The details Hunt supplies of Tongass Tlingit ethnography and history, though few and far between, are valuable because Tongass is so poorly documented elsewhere.

Hunt's letters also document, sometimes in considerable detail, the Kwakwaka'wakw collections he made for Boas. Examples can be seen in the captions for the exhibit catalogue *Chiefly Feasts* (e.g., Marcus 1991). To a lesser degree, the letters document collections that Hunt made for George Heye, but the information that would make the letters useful is still scattered among the Boas-Hunt correspondence, the Hunt myth texts associated with the objects, and the accession records of the National Museum of the American Indian. (See, for instance, Boas to Hunt, 5 June 1906, 20 April 1909; Hunt to Boas 10 March, 20 November 1909, 29 April, 9 December 1910, APS-BPC).

Census and Maps of Fort Rupert

In 1910, Boas wrote to Hunt asking for a

detailed statement about the relationship between all the men and women, and the houses they live in, in Fort Rupert, and . . . the ^εnEMemut [descent group] they belong to, and to what ^εnEMemut their wives and children belong. (Boas to Hunt, 28 February 1910, APS-BPC)

Boas was to repeat this request in various forms over the next nine years (e.g., Boas to Hunt, 4 April 1913, 12 May 1919, APS-BPC). Finally, in late 1919, Hunt complied, leaving out, however, some of the information Boas had asked for and providing instead two censuses, one representing Fort Rupert in 1919 and one, remarkably, as the community had been in 1866 (Hunt to Boas, 18 July, 20 August, 4 October 1919, APS-BPC). Hunt also made two pencil drawings to show the site plan of the community as it had been in each of those years.

Unfortunately, this set of manuscripts does not seem to have survived intact, although the missing pieces may eventually be located among Boas' papers. Enough of it remains in some form, however, to make discussion of it possible. The two drawings are extant, and although both the finished censuses that Hunt mailed to Boas are apparently missing, a draft of the 1866 census has survived in a notebook of Hunt's in a private collection.⁹ A copy of this notebook was kindly made available to me by Bill Holm. Two pages of the draft 1866 census, and information about the notebook, are in Holm and Quimby 1980:48, 127-8.

The census, which covers 20 nonconsecutive pages in the notebook, lists every inhabitant of the Native community at Fort Rupert, known in Kwak'wala as Tsaxis. This community was founded in 1850, when the four divisions (i.e., "tribes") of the Kwakwaka'wakw then living along Clio Channel moved to the recently established HBC fort (Boas 1921:973-7, 1966:46; Johnson 1972:8).¹⁰ These four divisions came to be known collectively as the Kwagwł, from which name arose the "Kwakiutl" of anthropological literature.

Hunt's 1866 census is organized by the traditional "bighouses" and identifies each house by descent group and divisional affiliation. The houses are numbered 1 through 26, and correspond almost exactly to the 26 houses on Hunt's 1866 site plan (Fig. 54).

Within the house, individuals are listed by name, descent group, and divisional affiliation and, often, by their relationships to others in the house. The individuals are numbered; thus, the inhabitants of House 1 are numbered 1 through 27 and those of House 14, 509 through 558. There are gaps, irregularities, and many alterations in the numbering in this rough draft of the census, and Hunt's total of 840 for the population of Tsaxis may not represent a completely accurate count.

Hunt's choice of 1866 for his census raises some questions. In late December 1865 the British naval vessel *Clio* bombarded and burned Tsaxis, completely destroying it, in retaliation for the refusal of the population to surrender three Kwagwł suspected of murder (Gough 1984:82–4). According to Johan Adrian Jacobsen, who visited Tsaxis in 1881, the Kwagwł partially abandoned the site thereafter, and only about 250–300 people remained to rebuild the community (Woldt 1977:32).

Hunt's 1866 site plan of Tsaxis generally agrees with an undated photograph (Fig. 55) that shows a long row of Native houses with early house fronts of broad, horizontal, hand-hewn planks supported by upright poles. This image was taken from the east side of the stream mouth and the fort. It is a composite of two photographs, and a section of the village containing parts of at least two houses is missing where the segments were joined imperfectly near the center. Otherwise, the visible houses correspond well with the western portion of Hunt's "1866" site plan.

In sharp contrast, a sketch of Fort Rupert made in May 1866 shows only a few houses standing on the west side of the site (Fig. 56). These may be either ruins or new construction. The rebuilt village, seen in an image from 1881, shows a rather different distribution of houses (Fig. 57). Moreover, by 1881 upright

planks had replaced the older-style fronts in all but one of the houses. The census and the "1866" site plan, then, most likely represent Tsaxis before its bombardment by the *Clio*.

A further question about the census is that it is apparently based on Hunt's interrupted experience of the community as a teenager, recorded 50 years later. Hunt was born in Fort Rupert in February 1854 and would have been a few months shy of 12 in late 1865 (Hunt to Boas, 7 April 1916, 6 January 1919, APS-BPC; Barbeau 1950:651). Further, his residence in Fort Rupert during the 1860s was not continuous. According to Hunt, in 1863 his mother took him north to her home village in Tongass Tlingit territory (Hunt to Boas, 2 August 1920, APS-BPC).¹¹ It is not known how long they stayed there, but Hunt did witness the "1864" Tongass winter dances (APS-KM v:5552), which could mean those of 1863–64 or, more likely, of 1864–65.¹² Hunt may also have lived in the north during 1868–71, while his father was stationed there (HBCA, Robert Hunt Biography:7–8; Barbeau 1950:651).¹³ By 1872 he had married and settled permanently at Fort Rupert (CU-Hunt xiv:2197, 2238).

Nevertheless, the census should not be dismissed. Hunt's writings demonstrate that he was a man with an extraordinary capacity for remembering detail, and his mother's family, his wife, and most of his friends came from cultures in which vast amounts of genealogical, historical, and mythological information were stored and transmitted without benefit of the written word. It is likely, anyway, that Hunt did not rely solely on his own memory to draw up this census. The Boas-Hunt correspondence makes it clear that Hunt frequently consulted Kwakwaka'wakw elders and friends in his work for Boas.

As one measure of its reliability, the census corresponds at many points with other Hunt documents, such as his unpublished list of descent-group "seats" (ranked positions; APS-KM vi:3144–75) and various published and unpublished family histories (Boas 1921:891–938, 951–1002, 1093–1117, 1925:64–

357; APS-KEM iii:342, 391–403; CU-Hunt xiv:1598–1619, 1660–70). It also corresponds in some details with historical documents such as the 1851 land purchase agreement between the Hudson's Bay Company and the chiefs of Tsaxis (FRP; Curtis 1915:150). Caution must be observed, however, as some Kwakwaka'wakw names occur widely, and some high-ranking Kwakwaka'wakw of the day changed their names frequently or had more than one name. Hunt sometimes refers to an individual by one name, sometimes by another.

Social Information in Hunt's Census

Despite questions that remain about the 1866 census, it is an important document for the study of the four Kwaguł divisions, as well as for the Kwakwaka'wakw in general, and it has implications for the ethnohistory of the larger region. Among other things, the 1866 census and site plan are a rich source of information on 19th-century Kwakiutl social organization and have the potential to clarify issues of descent, succession, residence, and marriage among the 19th-century Kwakwaka'wakw that remain subjects of controversy to this day.

The census shows the houses as being grouped according to division, as another Tsaxis resident, Charley Nowell, also recalled (Ford 1941:13, 49). Thus, the nine houses of the K'umuyo'i division are clustered on the left (east) side of the HBC fort, while the nine houses of the Gwitala division or Kwaguł proper extend along the right (west) side. Beyond them in a line extend the two houses of the K'umk'u'as division (marked "4" or "Y" in Hunt's drawing) and the six belonging to the `Walas Kwaguł. The spatial arrangement of divisions recalls that of the old Kwaguł villages on Clio Channel (see Figs. 52, 60; Boas 1934, map 14). There, the K'umuyo'i town at K'abe' and the Gwitala community at Kalugwis faced each other across a body of water; the `Walas Kwaguł and K'umk'u'as dwelled together at Adap' on the far side of the Gwitala (Boas 1921:138–9, 1966:46).

In contrast, the census reveals no obvious pattern in the distribution of houses within each "quarter" of the village. The order of the houses does not reflect the ranking of descent groups within the division as given by Boas (1966:39). When a single descent group occupies more than one house, the houses seem placed more or less randomly. Thus, in the Gwitala quarter the houses occupied by the highest-ranking (Ma'amtagila) descent group are 1, 7, and 9. This is contrary to Boas' statement that each descent group "occupied its own section of the village" (Boas 1966:48).

The two houses closest to the beachside entrance to the HBC post, 1 and 18, belonged to the Ma'amtagila and Kwakwak'wam, the highest-ranking descent groups of the Gwitala and K'umuyo'i, respectively. These two divisions, in turn, comprised the higher-ranking "side" in the dual organization of Tsaxis (Berman 1991:97–102; Ford 1941:17, 70). House 1, interestingly, is identified in the census not by descent group but only as "âwades House." Owadi, the first person listed in House 1, was a powerful figure in Fort Rupert and is mentioned by Hunt in a number of contexts (e.g., Boas 1966:190, 256). Owadi is the first chief in his division listed in the 1851 land purchase agreement (where the name is spelled "Wawattie"), and he was said to have been the head chief of Tsaxis in 1865.

There is no information about the first man listed in House 18, but the man following him in the census, his brother "Nölis" (Nulis), seems to be mentioned in other documents. Nulis is the second chief listed in the 1851 agreement for the K'umuyo'i/Kwixa division (the name is given there as "Noolish"), and by 1871 he was head chief of the descent group owning House 18 (CU-Hunt xiv:2269). The placement of the two houses calls to mind Boas' observation that in Kwakwaka'wakw myths the chief's house is to be found in the center of the village (Boas 1966:301).

In most cases in the census, each house serves as the residence for a single descent group, although several descent groups are large enough to require two or more houses. The number of residents per house

averaged 31. The number of distinct households—a household, generally speaking, consisting of a nuclear family (Berman 1991:66–8; Ford 1941:11; APS-KEM iii:342, 391–403)—is also rather large. In House 1, for instance, the 27 inhabitants consisted of 7 monogamously married couples and their children, 2 other men with 2 wives each, and 1 slave couple. In House 7 the 36 inhabitants included 8 couples and their children, 2 sets of divorced or widowed women and their unmarried daughters, and a single man apparently living by himself. These are far in excess of Ford’s estimate that the traditional Kwagw̓ house held an average of four family units (Ford 1941:11; compare Boas 1897:369).

Rank, that pervasive feature of North Pacific social organization, is implicit in the order in which Hunt lists the residents of a house. As noted above, the first person listed in House 1, Owadi, was the highest-ranking chief in Fort Rupert. In other cases, the rank of the individual’s name or “seat” (*k’wa’yɪ*) in the descent group can often be discovered by consulting another unpublished Hunt manuscript (APS-KM vi:3144–75). In cases in which at least three seats of house members are known (19 of 26 houses), it can be seen that house members are listed generally in order of rank. The irregularities in rank order probably arise from the frequent practice of making a young heir the ostensible rank holder (Ford 1941:177, 209). For example, in House 8 the first individual listed is a man whose name belongs to the third seat in the La’alaxs’andayu descent group; two of his sons have the first and second seats, while his daughter is in the fourth seat. Given that only two of the men listed first in their houses actually occupied the first official rank in their descent groups (Houses 4 and 5), this situation may have been quite common.

That the man listed first in a house is always head of the house regardless of ostensible rank is supported by the distribution of polygyny in Tsaxis. Hunt wrote,

Everyone of the chiefs of the four tribes . . . Had two wives Each they take their first wife who is a Daughter of a chief of one nememot [descent

group] then again he takes another chief[’s] Daughter who is Belong to another nemimot. (Hunt to Boas, 7 December 1926, APS-BPC)

To judge by the census, there is some exaggeration in this statement, but 8 of the 11 cases of polygyny listed in the census do involve a house chief. Two of the remaining cases are a son and a brother of house chiefs who were also polygynous, and these men are listed *second* in their respective houses.

Another indication that the names given in the census can be a poor guide to the bearer’s real position is demonstrated by the ostensible rank of the house chiefs of the two houses built on either side of the entrance to the HBC post, Houses 1 and 18. These men, as discussed above, were the highest-ranking chiefs of their respective divisions. What is more, each had two wives, and each is followed in the census by a relative (son or brother) who also had two wives; these are the only two houses in Tsaxis where there is more than one plural marriage. Yet the names these men held belonged to seats 13 and 18 in their respective descent groups, and in each case someone else’s child had the name belonging to the first seat of the descent group.

Another point of interest in the census is the considerable evidence for the nonunilineal tendency of Kwakwaka’wakw social organization. Although Hunt always affiliates children with their father’s descent group and there appears a definite tendency in the direction of patrilocal residence, the houses frequently contain residents connected through women members of the descent group. The inhabitants of House 1 included, among others, the house chief Owadi, his two wives, his son, and the son’s two wives, as well as Owadi’s sister and her husband and the latter’s niece and nephew. In House 11 residents included the chief and his two wives, the chief’s son and three daughters, and the husband of one of the daughters. Another lower-ranking man dwelled in that house with his wife, two daughters, the daughters’ husbands, three granddaughters and one grandson (each daughter

having borne two children), the spouses of the four grandchildren, and, finally, five great-grandchildren, who were all a daughter's daughter's daughter's children.

The census further tells us that only about 2 percent of the population of Tsaxis was slaves and that the slave population was divided more or less evenly by sex.¹⁴ Only one chief, in House 26, owned as many as 7 slaves (who are explicitly said to be his). Many houses had none, and most descent-group houses with slave residents possessed only one.

The census also contributes to the understanding of demographic changes on the North Pacific Coast during the 19th century. It is clear that severe depopulation took place, but there is little reliable data. Hunt's census and related documents supply figures for Tsaxis only after demographic decline had probably been under way for at least half a century (Gibson 1992:272–7; Gough 1984:80). Hunt's numbers for the years following 1865 are telling, however: according to him, his two censuses showed how the Native population of Fort Rupert declined from a total of 840 people in "1866" to a community with only 45 adult men, or around 200 people in all, by 1919 (Hunt to Boas, 18 July, 20 August, 4 October 1919, APS-BPC).

Although out-migration, including the partial abandonment of Tsaxis following its destruction by the *Clio*, no doubt contributed to this decline, the census, together with another unpublished Hunt document, shows that, as expected, increased mortality and low fertility also played an important role. Hunt wrote at length about the inhabitants of a single house named Gukustolis ("House That Came out of the Sea") as they were in 1870 (Hunt to Boas, 27 April 1906, APS-BPC; APS-KEM iii:391–403). A comparison of names shows that these are the people of House 8 on the "1866" census. In 1865 House 8, belonging to the La'alaxs'andayu descent group, had 73 inhabitants and 15 households and was the most populous dwelling in Tsaxis. By 1870 the number of households had dropped to 13, and Hunt mentions only 25 inhabitants (although those figures may include only rank

holders and close relatives of the chief, and we do not know whether the former inhabitants had died, left Tsaxis, or split off to form a new house). After 1870, however, misfortune and disease—chiefly tuberculosis—exacted a severe toll on those 25 inhabitants. In 1906 Hunt wrote somberly, "[A]ll of the People lived in that House . . . and now there is only one living in it without a wife" (APS-KEM iii:398).

Boas' Kinship Research

The census is a particularly good demonstration of the synergy of the Boas-Hunt collaboration. The idea was Boas', and he had to ask Hunt several times. When Hunt finally undertook the task, he did not do precisely as Boas asked, as was often the case; he did not, for example, list "the relationship between all the men and women . . . in Fort Rupert" (Boas to Hunt, 28 February 1910, APS-BPC). As was also often the case, he did other things that are of equal interest. He completed a much longer census of Tsaxis as it was in 1866, in addition to the census of the contemporary community that Boas had asked him to make, and he further supplied the two site plans.¹⁵

The census and related documents, together with the list of ranked positions, highlight the ratio between the immense amount of information Boas gathered on Kwakwaka'wakw social organization and the relatively small amount of his writing devoted to that subject—one article (Boas 1940b) and a chapter in each of two different books (Boas 1897:328–41, 1966:37–76). Boas has frequently been criticized for the quantity of detail and the dearth of analysis in his publications. Of course, one of his ethnographic goals was to collect raw ethnographic and linguistic materials for future generations (Berman 1996:218–9). But it is increasingly clear that there was more insight and analysis behind Boas' collection of data than might at first appear.

In the correspondence, Boas frequently refers to the difficult nature of Kwakwaka'wakw descent, affiliation, and succession—topics that have bedeviled

succeeding generations of anthropologists. Boas asked Hunt to collect family histories, for example, to aid in “straightening the matter out.” “[Y]ou cannot be too detailed in getting information,” he wrote (Boas to Hunt, 6 March 1906, APS-BPC; compare Boas to Hunt, 20 May 1911). In Boas’ last comments on the Kwakwaka’wakw descent group, published post-humously, he offered what is probably the clearest insight into the system to be found anywhere.

The structure of the numayma [descent group] is best understood if we disregard the living individuals and rather consider the numayma as consisting of a certain number of positions to each of which belongs a name, a “seat” or “standing place” that means rank, and privileges. . . . These names and seats are the skeleton of the numayma, and individuals, in the course of their lives, may occupy various positions and with these take the names belonging to them . . . The numayma is neither strictly patrilineal nor matrilineal, and within certain limits, a child may be assigned to any one of the lines from which he or she is descended, by bequest even to unrelated lines. (Boas 1966:50-1)

It is easy to miss the fact that this short passage is the summation of years of analysis of a vast body of data—Boas needed the vast body of data in order to arrive here. Comparison of this passage with Boas’ rather primitive analysis in 1897, in which he argued that the Kwakwaka’wakw descent group showed a patrilineal form of organization under the influence of the matrilineal societies to the north (Boas 1897:334-5), shows how far he had traveled in the intervening years.

Another way to look at Boas’ body of data on Kwakwaka’wakw social organization is to focus on its unusual form. Although later generations of North American anthropologists regarded Boas as having little worthwhile to say about social organization, the unpublished documents suggest that he might better be regarded as a pioneer. The lengthy family histories were intended as case studies. The censuses of Fort Rupert are virtually unique for their era. (Gifford 1926 is an exception but was obtained somewhat later.) Boas also obtained from Hunt lengthy biographical accounts,

including one that examined the potlatch and the chiefly role in terms of one man’s life (Boas 1925:112-357) and another, the socialization and education of a Kwakwaka’wakw woman (CU-Hunt xiv:4137-45, 4198-205, 4250-3, 4327-35; APS-KM iii:4145-83, 4189-98, 4206-50, 4283-326). We have already noted the unpublished texts examining the margins of gender among the 19th-century Kwakwaka’wakw. These materials are all the more impressive when we consider what is to be found in other ethnographic publications of the time.

Boas did not stumble on these ethnographic notions blindly; they clearly arose out of a principled and creative thought process. Of course, it was George Hunt who made it possible for Boas to collect such vast amounts of detailed information on such (for the time) unusual topics. But it was Boas who thought of collecting this information, and who pursued it through Hunt in an organized and systematic fashion.

The Social Organization and the Secret Societies of the Kwakiutl Indians

Boas’ 1897 monograph *The Social Organization and the Secret Societies of the Kwakiutl Indians* (designated here as *SOSSKw*) is one of his few major analytical or summarizing works on the Kwakwaka’wakw. The only other major publication covering similar ground is a posthumous volume, *Kwakiutl Ethnography* (1966), which was assembled by Helen Codere from a combination of published sources and manuscripts in preparation at the time of Boas’ death in 1942.

The 1897 monograph remains Boas’ most significant and primary statement on the Winter Ceremonial complex of the 19th-century Kwakwaka’wakw, and particularly on the relation between the ceremonial and its material culture and mythology. Boas’ only other significant discussion of the Winter Ceremonial is in the two chapters in *Kwakiutl Ethnography* (1966:171-298; see also pp. 400-22) that rely heavily on extracts from and summaries of the 1897 volume (especially 1897:544-605).

George Hunt's Contribution to SOSSKwl

The unpublished materials in Boas' papers have steadily expanded our understanding of the magnitude of George Hunt's contribution to Boas' work (Berman 1994, 1996, n.d.; Jacknis 1991, 1992). What they show about his labors in relation to Boas' first important monograph is no exception.

The title page of *SOSSKwl* states, "Based on Personal Observations and on Notes Made by Mr. George Hunt." In the preface to the volume, Boas goes further:

The great body of facts presented here were observed and recorded by Mr. George Hunt, of Fort Rupert, British Columbia, who takes deep interest in everything pertaining to the ethnology of the Kwakiutl Indians and to whom I am under great obligations. I am indebted to him also for explanations of ceremonials witnessed by myself, but the purport of which was difficult to understand, and for finding the Indians who were able to give explanations on certain points. (Boas 1897:315)

These acknowledgments, while generous, do not supply a complete picture of Hunt's contributions to the volume. Hunt played an important role in at least three areas that Boas does not address directly. First, Hunt was a crucial figure in the acquisition of several collections of Kwakwaka'wakw ceremonial objects that are illustrated and discussed in the book, including the collection used most extensively—that of Johan Adrian Jacobsen. Jacobsen made the collection with Hunt's assistance in 1881–82 for the Berlin Museum für Völkerkunde, then the Royal Ethnological Museum (Berman n.d.; Cole 1985:60–7; Jacknis 1991:181).¹⁶

Second, Hunt made possible in every way Boas' "personal observations" of the 1894–95 ceremonial (Berman n.d.; Rohner 1969:177–87). Hunt fed and housed Boas during the ceremonial; he advised Boas how to go about his work; he searched out and purchased objects for the collections Boas was making; he took Boas to feasts that were occurring at all times of the day and night; and he explained and interpreted for Boas constantly.

The third area in which Hunt made a major unacknowledged contribution to this volume is in its actual writing, a role that goes far beyond what we would today understand by the making of "notes" or the recording of "facts." Hunt, for example, provided much if not all of the myth material that explains the origin of the various dances and masks of the Winter Ceremonial (APS-KM i:31–67, 100–10, 180–9, 212–24; Hunt to Boas, 20 March, 23 April, 9 July, 21 October, 15 January 1895, AMNH-HAR). This is not surprising, given Hunt's subsequent labors on behalf of Kwakwaka'wakw oral literature.

What is less expected is that Hunt's English-language manuscripts functioned as the first draft of the chapter of the book that purports to present Boas' personal observations of the 1894–95 Winter Ceremonial (Boas 1897:544–606). Boas, in fact, prefaces this chapter by saying, "I will describe the ceremonial as it actually took place and so far as I witnessed it" (1897:544–5).¹⁷ Now, Boas did indeed witness these events, but the descriptions we have are largely from Hunt. Hunt made use of materials supplied to him by Boas, but he elaborated on and expanded them greatly; in one place he states that he wrote out 15 pages for a single page of Boas' notes (Hunt to Boas, 16 February, 9 March 1896, APS-BPC).

Hunt's first drafts of this chapter of *SOSSKwl* survive only as fragments scattered through one of the manuscript collections under Boas' name at the APS (APS-KM i, xi). The pages, when brought together, consist of two sections of text. The first, 12 pages in length, bears Hunt's page numbers 18–29 (APS-KM i, vi). There is some difficulty with the page numbering of the second section, but it appears to consist of Hunt's pages 35–56, minus pages 51–2, a total of 20 pages.

These materials are not to be found in any published or unpublished list of Hunt manuscripts (cf Boas 1921:1469–73). The handwriting and transcription practices clearly date them to the mid-1890s.¹⁸ The Boas-Hunt correspondence provides more precise clues to the date of the manuscripts. They are most likely

those referred to by Hunt in letters sent during 1896. Hunt writes, “now I am Writing out the Dances you;v seen While you was here” (Hunt to Boas, 4 January 1896, APS-KM vi; see also Hunt to Boas, 16 February, 9 March, 30 April, 9 July 1896, APS-BPC).

The first set (12 pages handwritten) corresponds closely to pages 577–81 of *SOSSKwl*. The first 7 pages of the second set correspond to pages 586–89 of *SOSSKwl*.¹⁵ The remainder includes a passage that was not reproduced in *SOSSKwl* but that appears in a later volume (Boas and Hunt 1905:484–91). The passage describes an episode that is partially summed up in one sentence in the third paragraph on page 589: “In the evening a feast was given, the blankets were distributed, and shortly after the beginning of the feast the *hämats’ a* *Yäqo’is* came in and danced three times.”

The gaps in page numbering indicate that pages are missing from the Hunt manuscripts. Judging from the correspondence, several other sections of manuscript are missing as well. On the last page of extant manuscript (p. 56), Hunt writes, “now after this I will write about what the *Koskimo* Done on the 25th of Nov” (KM vi). In Boas’ published version those events are described in 12 pages of typeset prose (Boas 1897:589–600). Together with another 6 published pages, describing the final events of the 1894–95 Winter Ceremonial, this would equal at least another 40 handwritten pages of Hunt manuscript. All in all, about 100 pages of Hunt’s draft of this chapter are missing.

Comparison of an extended section of Hunt’s manuscript with Boas’ published version shows how closely Boas followed Hunt’s first draft. Hunt wrote:

all the time the new *Hämädga* [*Hamats’ a*] Was Dancing. *lügäxstääq* Halding a copper in his Hand and a woman came out With a strip of calico about 40 yards in length this women name is *äyägä*. she toked the calico all Round the fire. and the *Hämädga* Danced Between the fire and the calico. he Wore the Balsam Pine Branch and Danced the two first song with it on. and after the singers sang the two songs. then he *äläbälä* came foward and asked the singers to wait awhile and not to sing. and he asked *tögümälis* to come and

make a speach and he *tögümälis* came and stand up at the Rear End of the House. and he said. yes you my children yes I for I am your Box your mind for I Keep all the old sayings in my Head and I have seen thing in my youngs Days that you young men never have Heard of and seen. and it is good to have one old man to show you all this things. now I am going to this *Hämädgä* and ondress, the Dress that was Put on him By the *Bäx-bäqälänöxsiwi* for I am he, said he the old man. and he Walked up to the *Hämädga* and toked the Head Ring off first and next he take off the neck Ring off and the arms and legs Ring. then he gived the Rings of Balsam Pine Branch to *lämäälä* and he the old man asked *näwäkälä* to Bring the Blanket and the Red cedar Bark, then he *näwäkälä* Went Back into a Bed Room for about one minut and he Brought all that the old man asked for, and he *näwäkälä* gived the Blue Blanket first to the old man. and he Put the Blanket on to the *Hämädga* and again the old man toked the neck Ring and put it on to the neck of the *Hämädga* and again the old man toked the apron and Put that on and next the arms and legs Ring all of Red Ceder Bark Rings then last of all he toked the Eagles Down and Put it on to the Red ceder Bark Dress of the *Hämädga*. then the old man *tögümälis* step in front of the *Hämädga* and said it is all Done. (APS-KM i:24–5)

For this passage, Boas has:

After this song *LöXuaxstaak*^u arose in the rear of the house, holding a copper, and a woman named *Ayaqa*, brought a strip of calico about 40 yards long, which was unrolled and spread in a circle around the fire. . . . Then the singers began the second song: . . . The *hämats’as* were dancing between the calico and the fire in a squatting position. Their attendants tried to pacify them with cries of “*höip*,” and the women danced for them. Then *Alabala* stepped forward and asked the singers to wait before beginning the third song. He called his speaker, *Töqoamalís*, who took his position in the rear of the house, and addressed the people as follows:

“Yes, my children, I am the storage box of your thoughts, for I remember all the old tales, and in my young days I have seen things which you young people never heard of. It is good that there is one old man who can show you all these things. Now I will go to this *hämats’a* and take off the dress that *BaxbakuälanuXsiwaë* put on him.” He

stepped up to the *hámatsá*, who was standing in the rear of the house, and took off his head ring first, then his neck ring. He cut off the arm rings and anklets and gave them to *LamáLa*. Then he asked *Naúaqala* to bring blankets and ornaments made of red cedar bark. *Naúaqala* went to fetch them from his bedroom, and when he had returned, *Tóqoamalís* proceeded to dress the *hámatsá*. He put the blue blanket over his back and cedar bark ornaments on his head, his neck, his arms, and around his ankles. He also tied a dancing apron around his waist and strewed eagle down on his head. Then he said, "It is done." (Boas 1897:578–79)

There are, of courses, differences in the two passages. Boas made revisions to Hunt's Kwak'wala transcription, and, as is obvious, he divided Hunt's draft into paragraphs, corrected Hunt's spelling and grammar, and, in places, altered Hunt's wording. Boas also changed the sequence of some elements in his version. For example, while Hunt grouped the *Hamats'a's* four songs in an earlier place in the text, Boas scattered the songs throughout, with one occurring in this very passage (omitted at ellipsis).

Finally, Boas omitted or misconstrued some information in Hunt's text. Hunt, for example, quotes *Tógümälís* as saying, "now I am going to this *Hämädgä* and ondress, the Dress that was Put on him By the *Bäx-bäqälänöxsíwí* for I am he." The old man, for that moment, is assuming the role of *Baxwbakwalanuxsiwe'*, the *Hamats'a's* initiating spirit. Boas leaves this identification out. In another passage, Hunt writes about a man who angrily tears up a blanket, which he deemed an insulting gift, and throws it in the fire. The man says, "now you that set on the fire take that to Keep you warm" (APS-KM i:28–9). The insulted man is referring to *K'waxtłala*, the "One-Sitting-on-the-Fire," a being to whom food and prayers were given at Kwak'waka'wakw feasts (Boas 1921:1332). Boas altered Hunt's words to, "Now you who saw it in the fire take good care to keep it warm" (Boas 1897:580), probably because he did not yet know about this being.

Despite the numerous minor differences, the overall similarity of these passages is clear. More of these

early manuscript pages may yet be found among Boas' papers. To determine the total portion of the volume drafted by Hunt from Boas' notes may no longer be possible at this date, but Hunt's role was clearly much greater than had been thought.

Hunt's Revisions and Corrections to Social Organization

Hunt's involvement with *SOSSKwl* did not end with its publication or even with the appearance several years later of the corrected Kwak'wala texts of a number of the Winter Ceremonial songs, and the Kwak'wala portion of several myth and historical narratives in the volume (Boas and Hunt 1905:247–9, 271–8, 354–5, 418–24, 447–84). Over 20 years later, Hunt wrote to Boas, saying, "now about the Book with the many illustrations [i.e., *SOSSKwl*], there are so many mistakes . . . that I think should Be Put to Rights Befor one of us Die" (Hunt to Boas, 7 June 1920, APS-BPC). Boas replied that he was "very anxious" to have the mistakes corrected and asked Hunt to begin (Boas to Hunt. 22 July 1920, APS-BPC; see also Hunt to Boas, 4 February, 21 May 1920, APS-BPC; APS-RMC).

Hunt began to produce the corrections to *SOSSKwl* in August or September 1920, consulting Kwak'waka'wakw elders in order to do so (Hunt to Boas, 25 September, 14 October 1920, APS-BPC; APS-LKM:2–3). He generated two batches of revisions to the volume, 109 pages during 1920–21 and another 54 pages in 1924, and then laid the task aside for seven years (Hunt to Boas, 14 January, 25 April 1921, 1 January, 31 May 1924, APS-BPC; APS-KM iii:1679; APS-LKM:3–5).

In early 1931, at Boas' request, Hunt took the task up once again (Hunt to Boas, 17 February 1931, APS-BPC). From that point until his death in September 1933, Hunt was entirely occupied with the revisions (J. Cadwallader to Boas, 6 September 1933, APS-BPC). During this period, he produced over 670 pages of additional corrections and comments on the volume (Hunt to Boas, 17 February 1931, APS-BPC; Hunt to

Boas, 27 July 1933, KM v; J. Hunt to Boas, 26 September 1933, APS-BPC). Altogether, Hunt's revisions to *SOSSKwl* amount to over 800 pages of manuscript, which today are to be found in one of the unindexed masses of Boas' Kwakwaka'wakw papers at the APS (APS-KM).

The method by which these revisions were produced differed from what was typical of the Boas-Hunt collaboration. Here, Boas did not prompt Hunt with specific questions. Rather, he told Hunt to "simply mark the page [of the published volume] and then say what you want to say about it" (Boas to Hunt, 22 July 1920, APS-BPC). Hunt corrected and added to the text at his own initiative. As he stated, "I see that I got to go all through the Book. to do it Rightly, some times I got to write some other story that Belong to it, to Explain the meaning of it" (Hunt to Boas, 17 February 1931, APS-BPC). In consequence, Boas' own research agenda has less of an imprint here than elsewhere in Hunt's work. This renders these materials more heterogeneous, but perhaps even more interesting.

Many of the revisions are, in fact, corrections to the Kwak'wala of *SOSSKwl*. As much as a third of the manuscript pages was copied from the original text with the addition only of new transcriptions of Kwak'wala names and other words. Boas discarded those pages, preserving a record of the corrections.

Hunt also revised songs and texts in the volume according to the vastly improved standards that he and Boas had achieved by the later decades of their collaboration. (Hunt produced two or more slightly different corrected versions of some songs and texts.) There remain, however, continuing problems with Hunt's notation of glottalized versus non-glottalized sonorants (see Berman 1994:494). Boas generally corrected these as he compiled Hunt's new transcriptions.

Most of Hunt's revisions, though, are to the ethnographic content of the volume. These revisions, largely in English, include correction, expansion, and addition of numerous points of ethnographic and ethnohistorical

detail. The very first corrections Hunt transmitted were to the identifications of the objects illustrating *SOSSKwl*. As Boas stated in an unpublished article,

The explanations of these specimens given at that time [1894] were based upon information given to me by the Indians from whom I purchased the specimens, in part corroborated by inquiries among others, although these were difficult on account of secrecy involved in the purchase of the masks. The specimens collected by Mr. Jacobsen were explained on the basis of illustrations which Prof. Albert Grünwald of Berlin had the kindness to make for me and which I showed to the Indians. I did not succeed always in finding the owner of the objects in question, so that there remained some uncertainty in regard to the right interpretation of the objects. . . . I requested [George Hunt] particularly to find the owners of the specimens illustrated in my report and to obtain further information in regard to the objects. In some cases his information differs from the explanation previously given, while in other cases it is more specific than what I was able to present in my previous report. (APS-RMC:1-2)

Hunt made a kind of catalogue of the illustrations of the volume giving "the right name of the masks on the Book and who there Belong to," with the page and figure number from *SOSSKwl* and a paragraph or more of English description. The catalogue of nearly 50 manuscript pages covers most of the illustrations in the book of Kwakwaka'wakw material (APS-KM iii:1877-93, 1904-20, 1927-39). "[T]his is all that I know," said Hunt, "and what I Dont Know I pass them" (APS-KM iii:1239).

Hunt's later batches of revisions included more extended commentary on some of the masks and dancers. One example is Hunt's statement about the *nułamał*, a type of dancer. In the original, Boas wrote,

The *nöönLEMala* (pl. of *nułamał*) or "fool dancers" [a particular Winter Ceremonial dance] . . . are initiated by a fabulous people, the *ALAsîmk*, who are believed to live near a lake inland from *LîXsîwaë*. Their village is believed to be on an island floating on the lake. In olden times a man went beaver hunting and fell in with these people. He came back exhausted and "crazy." . . . From him the *nöönLEMala* are said to derive their origin. (Boas 1897:468)

Hunt's comment on this passage was as follows:

the *äl!ESEMk* or *Back of the woods* living tribe . . . use to live eat [at] *xwetes* which are called *xuyäläs*. and the *gōsgemox* tribe use to live eat [at] *gōse* on the south of cape scot. and the *gōsgemox* tribe went to war against the *xuyäläs* tribe. and the *gōsgemox* on the second war Drove the *xuyäläs* in [to] the wood . . . and from that time the *gōsgemox* tribe lived at *xwetes*. so the *xuyäläs* is not a spirit But a common [secular] People who use to come and Halibut fishing at Place called *q!Edes* or *Patch on the Beach* . . . about one Mile and Half East of *L!EX-sewe*. and as soon as the *äl!ESEMk* People sees a strangers canoe comeing then they Paddle ashore and Run away to their Home at a large lake long Ways back of *L!EX-sewe*. which supposed to Have floating Island with their Houses Built on it. these what the *Kwäguł* tribes calls *äl!ESEMk*. are Really the *xuyäläs*. and I was told that they are the first People that the wolves give the *nun!EM* or *all turn craze Dance* to [a Winter Ceremonial of some of the northern Kwakwaka'wakw tribes] . . . and from the *xuyäläs* tribe. the *gōsgemox* got the Dance [ceremonial] and from the *gōsgemox* the *neqamgeliselä* got it. and from them the *L!äl!äseqwälä* got the *nun!EM* Dance [ceremonial]. and from the [time of the] wolves the *nun!EM* [ceremonial]. and *nulEMälä*. or *fool Dance* was always kept together. (APS-KM v:5601-2)

There are a number of notable points in Hunt's commentary. Among other things, it both agrees with and adds to the scanty information available about the Xuyalas division of the Kwakwaka'wakw, a group that was already extinct by the time of Boas' first fieldwork and is not even mentioned in his comprehensive list of Kwakwaka'wakw divisions and descent groups (Boas 1966:38-41, 44). Hunt also gives a fuller account of the historical spread of the *nun!EM* dance ceremonial than is to be found elsewhere (Boas 1966:400-1).

Another point of interest is the nature of the disagreement between Boas' and Hunt's versions of the origin of the *nun!EM* dance. To assume that Hunt's identification of the "äl!ESEMk" as the Xuyalas division is historically correct does not require that we reject Boas' identification of the same as a population of spirits. One of the characteristics of 19th-century Kwakwaka'wakw ceremonialism was the possession by

each descent group of an origin myth that, among other things, specified the origin of the ceremonies owned by that descent group's noble lines. These origin myths, while distinct in many details of content, are formally quite similar, and Boas' brief synopsis of the origin of the fool dancers is in consonance with the general pattern. The relationship between Hunt's account and Boas' version (which Boas may well have obtained through Hunt) might also therefore count as evidence for how historical knowledge both coexisted with and was assimilated into the formal patterns of myth and ceremony.

Another example of the type of information in these revisions is Hunt's commentary on the mask illustrated on page 628 of *SOSSKwl* (Fig. 59). This mask was described by Boas as a "Laö!axa mask representing the killer whale . . . Collected by A. Jacobsen." Hunt stated that the object was, rather, a killer whale mask belonging to the more important *t'set'seka* (Winter Ceremonial) dance complex, and he distinguished between the *t'set'seka* killer whale dance, the *d!a'walaxa* killer whale dance (the one to which Boas refers in the original), and the "Bäxus Hämäxälä! or summer time Keller whale Dance."

Hunt further asserts that the mask illustrated was used at the initiation of his own wife ("L!äle!eläkw or *spouter of the House*. who Belong to the *ya'ex-ağEME* descent group"). This statement seems eminently believable, given the extent of Hunt's involvement in Jacobsen's collecting activities; Hunt could well have been the means by which the mask came into Jacobsen's hands. Hunt goes on to supply an account of the event in which this mask was used:

she [Hunt's wife] Desappeared on the Beach while she was Degging for clams. where they found all her cloths Piled up . . . and she stay away one whole month. (APS-KM v:5613-4)

New songs were composed for Hunt's wife and rehearsed. The people assembled,

and then the killer whale mask or *hemäxälä!EM!* come out of the secret Room

and [went] spouting around the fire. with fin on his Back untill he go up to the Door. then the user Pulled the string and the fin stand up Right. and it Divide apart and the face open out and the tail went up and Down. and it keep that way untill it went into the secret Room. then L!älelelakw came out of the secret Room and Danced with her two Hands Hiden under her Blanket then after her song Ended she went Back in the secret Room again she wear all Pure Red loose [cedar bark] neck and head Ring. (APS-KM v:5614; see also APS-KM iii:1938)

Hunt's revisions to *SOSSKwl* contain not just additions of detail to the ethnographic record but also commentary that, especially in conjunction with other Hunt materials, suggests broader reinterpretations of the Winter Ceremonial and other aspects of 19th-century Kwakwaka'wakw cosmology and culture. For example, on page 418 of *SOSSKwl* Boas discusses the descent-group ancestors' acquisition of winter dances in myth. Hunt's amplifications place those events within a larger framework of Kwakwaka'wakw cosmogony.

Hunt states that the very first winter dance, an event of major cosmogonic implications, was held by Raven and Mink and their party of the "myth People," who "were Birds and anamals yet they can talk to Each other and understand Each other. these are called the *myth People* or *nux^εnemes*" (APS-KM vi:4969). In order to perform the ceremonial, the myth people (or "Historie People," as Hunt more often called them) took off their animal shapes. Some of them dressed in their animal masks afterward, while others remained in human form (Boas and Hunt 1905:489; also Boas 1966:258). This event was the beginning of the separation between the human realm and the spirit realm of the animals.

The first winter dances of humanity were based on the animal natures of the primordial generation:

and what Ever kind of Bird a man Belongs to his Dance will Be as he was Befor he was turned into a man. and [for those who were] the animals [it is] the same. (APS-KM vi:4969)

Hunt lists some of the dances of "the myth people," which include the Wolf Dancer, the Fool Dancer (for

Deer), the Grizzly Bear Dancer, the Raven Dancer, the Thunderbird Dancer, and others. In his cosmology these archaic dances predate those acquired in the age of myth proper (*nuyam*), when the children and grandchildren of the first generation of transformed, secularized beings grew to human adulthood, ventured into the deep forest or out to sea, acquired spiritual wealth, and founded descent groups:

these spirits appears to the first man of each one clan or *nememot* and tells him what to Do. what kind of Dances he will use. [But] that is after the myth People Past. (APS-KM vi:4969)

The Winter Ceremonial of his day, Hunt argues in these pages, evolved through a series of accretions: beginning with the dances passed down from one's ancestor, based on his spirit nature, growing through the addition of dances such as the Tuxw'id and Hamsham'tsas acquired from spirits by the early generations of humanity; and ending in the historic period with acquisition of the Hamat'sa complex through marriage and war from the northern neighbors of the Kwakwaka'wakw.²⁰ In Hunt's view, the dance acquisition stories belong to a range of ethno-literary genres that correspond to these developmental stages of the cosmos. As Hunt states elsewhere, the eponymous "nux^εnemes" (*nuxwni'mis*) are stories told about the primordial beings; following this are "nuyem", stories concerning the first generations after the first winter dance; then come "q!a'yul" (*k'ayul*), "tale[s] about the forefathers" that occurred after the end of the myth age, within the historical memory of latter-day humans; and finally there are "q!a'yâla" (*k'ayola*), a person "telling what he have seen and what he Heard his Friends talking about" (APS-KM iii:4624).

In his discussions of myth and the Winter Ceremonial, Boas did not ignore the varieties of acquisition story. He treated them, though, as story types of equal significance, coexisting, as it were, in ethno-literary time and space. In these late unpublished manuscripts, Hunt places not just the dances but also the stories about

their origins within the framework of a developing, transforming universe.

Hunt's focus on the History People and their transformation suggests that the key to the underlying meaning of the Winter Ceremonial should be sought there, in the story of its origin, and not just in the elaboration of the hereditary prerogatives that are the actual dances. (Boas published several versions of the story under various titles, the first being "Mink and the Wolves"; Boas 1897:538-9; 1930 [1]:57-86, 86-92, 1943:22; Boas and Hunt 1906:103-13; see Berman 1991:698-702; 2000). It is hard to say whether the emergence of this cosmogonic framework is the result of Hunt's greater freedom to set his own agenda in these revisions or whether it is due in some measure to the time he spent in the late 1920s learning what the Winter Ceremonial's hereditary officers had previously kept "strickly secret" (Hunt to Boas 15 June 1926, APS-BPC). Either way, it offers a tantalizing glimpse of a Kwakwaka'wakw cosmological order, an order for which anthropologists have hitherto been able to search only indirectly, through complicated interpretive operations (Berman 1991; Goldman 1975; Walens 1981).

As elsewhere in Boas' unpublished papers, Hunt's revisions to *SOSSKwl* also contain rich nuggets of ethnographic and ethnohistorical information about other peoples on the coast. In several places he discusses the movement and transfer of dances and dance elements from group to group. Hunt was not merely echoing Boas' interest in diffusion; he was clearly fascinated by the topic on his own account. He himself had seen much change in the winter dances since his youth, when he danced for seven chiefs of the old-time Kwagwł (Boas 1966:256). "[S]ince they got mixed in with the [dances of the] Hełdzaqw [Heiltsuk] and the ääwek!enox [Oowekeeno] there lots of change in the way they dance now" (APS-KM vi:4971-2).

In one manuscript, as a comment on Boas' discussion of Nuu-chah-nulth dances in *SOSSKwl* (Boas 1897:632-5), Hunt relates his experience at a Nuu-chah-nulth [Nootka] wolf dance held around 1917:

about fifteen year ago. my son Johny and me went to Nootka or *mōts!adox* or *Deer tribe* . . . and geting Dark that Evening I took notice that all the young men walk together. and late in the night I Heard lots of wolves Howling in the woods long ways off . . . and the wolves Howl Every night. and on the fourth Evening the wolves Howled most. . . . I did not sleep much that night. and Early in the morning I got up and Joh[n]ey and me went out of the House . . . the wolves came at the Right side of the House in a file. wearing wolfs mask as is show on Page 477 Plate 36 [of *SOSSKwl*] and Holding their Hand with their thumbs as they Do on the Picture . . . (APS-KM v:5356-7)

In this manuscript Hunt also discusses the history of that particular Wolf Dance, recounts the myth of its origin, and describes the ceremonial in some detail (APS-KM v:5356-74). He was surprised at some of the elements in the dance, including a song with Kwak'wala words that derived from the Hamat'sa ceremonial of the Kwakwaka'wakw. Hunt was told that the chief's wife, who came from the `Namgis division of the Kwakwaka'wakw, had asked a visiting relative to make her Nuu-chah-nulth husband a Hamat'sa,

and [her relative] said to her jokeingly o you can Have it. But my songs I cant give them away . . . and the women said give me one song if it is a Bāxus [*baxwas*; i.e., profane or ordinary] song for these People Dont know *ts!ets!eqä* from Bāxus. and she Did not aske for a name for the Hāmāts!ä and he sung the *thanking song* or *molxeduyu song* and she was so Pleased that she foget to say more. (APS-KM v:5359)

In several places in his revisions, Hunt comments on the acquisition of Winter Ceremonial dances through warfare (e.g., APS-KM vi:5051-9). This was one of the means by which the dances spread north all the way to the Tsimshian and Tlingit, as Hunt had witnessed in his youth. His discussion of the topic also imparts details about the indigenous slave trade on the North Pacific Coast during the 19th century.

According to Hunt, it was common practice to question war captives in detail about the ceremonials into which they had been initiated when they were free. "[T]he northern People learn about the winter dance

. . . from their slaves” (APS-KM v:5420). Hunt tells the following story about “ä^εmäxs the great warrior of the gedaxaʔ [Kitkatla] tribe” (APS-KM v:5418–20; Hunt to Boas, 14 December 1921, APS-BPC). Some time during the 1850s, “ä^εmäxs” (that is, the Coast Tsimshian person named Haymaas) killed a Kwaguʔ chief and took the chief’s sisters prisoner. Seven or eight years later, one of the women who had been captured returned to Fort Rupert, probably after having been bought and freed by an HBC factor at Fort Simpson. She told the Kwaguʔ how she and her sisters had been interrogated by their captors. First they were asked

if they were chiefs Daughters or sisters. and she said yes I am sister of . . . the head chief of the Kwakwäkw!um clan of the q!umoyEWE . . . said she. and then the man . . . ask what kind of Dance you have in the winter. and she say we *ts!ets!eqä* [the major Kwakwaka’wakw Winter Ceremonial] . . . my Elder sister is *meLä Dancer* . . . and . . . *tamer Dancer*. and lots of other kind of other [dances]. and the man said the slave we took Before you said that also you have the *Hamats!e* [a much higher-ranking dance] and the *lo!EM* [Ghost] Dance also. yes she said true about the *Hamats!a* . . . But the *lo!EM* [or] . . . *non!EM* . . . Dance Dont Belong to the Kwäguʔ. it Belongst to the *L!äl!aseqwalä* [and other northern Kwakwaka’wakw divisions]. . . . so By the slaves they try to learn all they can, about the names and the . . . dances. and Even their . . . son[g]s. and ä^εmäxs never keep lot of slaves. for he sells them. firther up north . . . and when their sold. the new owner aske the same Questions. (APS-KM v:5419–20)

The information from the Fort Rupert woman evidently motivated Haymaas to go to war against the northern divisions of the Kwakwaka’wakw. Once more, he took prisoners and interrogated them about their dances, and this time he learned all about the *nun!am* Dance as well. He eventually sold these latter prisoners to a Tongass Tlingit man, and as a boy Hunt met them in his great-uncle’s town of Daasaxákw.

Not all of Hunt’s ethnohistorical commentary in these revisions concerns dances and ceremonials. For example, he also discusses trade in mundane items:

while I stay with my grandfather [i.e., great-uncle] at *tongäs* . . . I use to aske him about

Defferent thing. where they came from. and how he get them. then he alway say that the *che!gät* [Chilkat division of the Tlingit] People Brought . . . fancy Braided mats and small fancy Braided Baskets with Rattleling covers on them and carvings of wood and *Ivory* and the *copper breslets* . . . and other copper implements are Brought By the *xóneyä* [Heinya division of the Tlingit] People to sell to us, said he. (APS-KM iv:4897)

One illustration of a Tlingit oil dish carved like a seal (Boas 1897:393; see Fig. 59) sparked a train of thought regarding which designs in North Pacific Coast art are merely decorative and which represent the hereditary privileges used by the aristocracy—what in Kwak’wala Hunt refers to as “k!eso^ε.” Hunt writes,

I had two [Tongass Tlingit] uncles who were good carvers. and lots of their People. and the other tribes come and ask them to make a grease Dish for them. and my uncle . . . ask the man what well I carve on it. and the man say to him. you carve on it anything you like on it that will make it look Pretty. now thes I seen for I use to Be [with] my uncles all the time, and from that time. I thought these kind of Dishes is not a k!eso^ε. now another thing. a man come to my uncles. and say to them I come to ask you to carve a totem Pole for me. and now my uncle ask the man How many figure you want me to Put on the Pole . . . and [if] the man said I want *sea Raven* or *näshäk yäʔ* at the Bottom. and above this will be *yan Län* or *great Whale*. and above it will be *yäʔ* or *Raven*. and above it will Be *täne’k!uxu* or the *mink*. and above will Be woman and her. toad. or *säwät*. *gänaow* and on the top of the Pole will Be *yäʔ* or *Raven* sitting now. the carver cant add Anything onto those figures. Because they are true k!eso^εs. and that is the way the other totem Poles. are made. and also Big feasting Dishes they have to Be made By the carver according to what the chief told him to carve onto them or House Post. for these are true k!eso^ε. (APS-KM v:4896–7)²¹

What, if anything, did Boas do with these hundreds of pages of manuscript? In the early 1920s he put together a short article (“Remarks on Masks . . .,” APS-RMC) based closely on Hunt’s first batch of revisions to *SOSSKwl*, the list of corrections to the illustrations. Boas hoped that the National Museum of Natural

History would publish this article as it had the original, but he was unable to excite any interest in that quarter (J. R. Swanton to Boas, 18 June 1924, APS-RMC).

Boas incorporated other revisions into the manuscript that was published posthumously as *Kwakiutl Ethnography*. The revisions appear primarily in the two chapters on the Winter Ceremonial (Boas 1966:171–98). The first of these chapters, as already noted, consists largely of material taken from the original *SOSSKwl*; the corrected Kwak'wala transcriptions are just about the only additions. The second chapter is a compilation of English paraphrase from published Hunt texts (Boas 1930:57–131) that is interpolated with material from both the original text of *SOSSKwl* and Hunt's later commentary on that text. Some, but by no means all, of Hunt's revisions are credited to him.

The revisions that Boas saw to print are only a small portion of the whole. Their scope is such that any evaluation of the original monograph, or any reinterpretation of the Kwakwaka'wakw Winter Ceremonial, for that matter, should not be made without them.

Nuu-chah-nulth Tales

One last set of Hunt documents needs to be mentioned: a manuscript collection of Nuu-chah-nulth myths, tales, and prayers in English, numbering over 500 pages. George Hunt wrote these down during the Jesup Expedition period, and many of them may be connected to the Nuu-chah-nulth objects that Hunt purchased for the AMNH (Boas to Hunt, 4 March 1904, AMNH-HCF; Boas to Hunt, 11 April 1903, AMNH-HAR). One Nuu-chah-nulth myth collected by Hunt (and published by Boas as a Kwak'wala text) refers to a whalers' purification shrine now in the collections of the AMNH (Boas 1930 [1]:257–65).

Most of the written Nuu-chah-nulth materials were apparently related to Hunt by a man named Lewis who returned with Hunt to Fort Rupert after the latter's 1903 collecting expedition on the west coast of Vancouver Island (Hunt to Boas, 25 November 1904, AMNH-HAR; Hunt to Boas, 22 January 1904, AMNH-

HCF). Lewis is perhaps the "âlëwes, a Kayoquath" mentioned several times in the manuscript. Hunt's Nuu-chah-nulth manuscripts seem to have bounced back and forth between Boas and Edward Sapir over the years; they are currently catalogued under Sapir's name at the American Philosophical Society (APS-SHN).²²

Conclusion

The manuscripts discussed here represent only the highlights of the unpublished North Pacific materials in Boas' papers. One could also mention his files on Fort Rupert social organization; linguistic materials on Tsimshian, Aleut, and other North Pacific languages by his students and correspondents; and more Hunt writings on everything from the history of certain Kwakwaka'wakw coppers to Hunt's Tlingit mother's clan myths. This unpublished material, taken as a whole, could fill six or more published volumes that would each add much to our understanding of the Native peoples of the region and of Boas' own work.

Although there is a great deal more to be said on the latter subject, two points are striking. The first is how much Boas' output on the Kwakwaka'wakw was dependent on Hunt's vast knowledge and ceaseless labor. Of the proverbial "five-foot shelf" of Kwakwaka'wakw materials, all but a few inches turn out to originate with Hunt himself. Even those few seem to be shrinking as we learn more.

The magnitude of Hunt's contribution is so great that it makes us uneasy to see Boas' name alone on the cover of most of these volumes. True, Boas always acknowledged Hunt's contribution: Hunt's name is even on the title page of *SOSSKwl* and on the cover of the first two text volumes (Boas and Hunt 1905, 1906). Why Boas chose to relegate mention of Hunt to the prefaces of the later text volumes is unknown, but it was not because Hunt's contribution was any less. The latter practice may have been more in line with scholarly etiquette of the time: Boas also used extensive notes of (white) whaling captains in his later Inuit monographs and, again, acknowledged that fact on

title pages and in his prefaces but not on the covers (1901:4-5, 1907:374-5). Still, from a modern perspective, the suggestion of sole authorship is misleading.

At the same time, Boas' contribution should not be undervalued. Although the fact is often obscured by the way in which Boas published Hunt's materials, Boas was largely responsible for the scope and focus of Hunt's work. With some exceptions, of which the revisions to *SOSSKwI* are the most significant, he set the research agenda. He picked the topics to be investigated, asked Hunt the specific questions, and decided when further details were needed and when it was time to move on to the next topic. And, of course, he provided the money that enabled Hunt to devote so much of his life to ethnography.

The second important insight to emerge from the unpublished materials is how, while anthropologists have underestimated Hunt's contribution to Boas' Kwakwaka'wakw publications, they may also have underestimated Boas. As we have seen, Boas was often reluctant to formalize and make explicit the notions driving his research, and his thinking on a subject cannot always be gauged by his published comments. Some of his most interesting anthropological thinking seems to have taken place out of sight. These unpublished materials allow us glimpses of it.

Acknowledgments

I am indebted to the staffs of the American Philosophical Society, the Anthropology Archives of the American Museum of Natural History, the Columbia University Rare Book and Manuscript Library, and the Hudson's Bay Company Archives at the Provincial Archives of Manitoba for their aid in locating numerous manuscripts and photographs. Igor Krupnik offered helpful comments on the final form of the paper, and Marie-Lucie Tarpent kindly dug up hard-to-find references. I would particularly like to thank Bill Holm for making a copy of Hunt's memorandum book available to me, and Beth Carroll-Horrocks, formerly the manuscript librarian at the APS, for her endless patience and helpfulness.

Notes

1. Two orthographies are used here to transcribe Kwak'wala words. In quotations from Boas and Hunt, words are spelled as closely as possible to the way they wrote them, within the constraints of utilizing those characters represented in the First Nations Courier New Font and First Nations StillMore Font. All other words are transcribed from Boas' or Hunt's original spellings into the standardized orthography of the U'mista Cultural Centre of Alert Bay, B.C., produced with First Nations Courier New font. Both fonts were created by Robert C. Hemphill of Port Hardy, B.C..

2. Hunt's sources included George Blenkinsop, the HBC officer in charge of Fort Rupert at the time, and an Indian man named Hémisilakw. The latter may be Hunt's friend Tom Hemaselakw, one of the Kwakwaka'wakw troupe who accompanied Hunt to the 1893 World's Columbian Exhibition in Chicago. Hemaselakw's father and perhaps mother were from the T'lat'lasikwala division of the Kwakwaka'wakw (APS-KM v:5420).

3. Hunt says the final confrontation occurred at Návídi itself, i.e., "Sutil Point," but Návídi is elsewhere said to be Cape Commerell, at the tip of Vancouver Island (Boas 1934, maps 3, 20). Hunt perhaps means "Newiti" in the sense used by whites, i.e., both the village on Hope Island and the two closely linked divisions that dwelled there at the end of the 19th century, the T'lat'lasikwala and the Nakamgalisala.

4. In 1991 the word *babak'wa* was glossed to me as "vicious man" rather than "warrior."

5. Hunt states that a number of British sailors were killed during the gun battle at Bull Harbour (CU-Hunt xiv:3942), but according to reports of the time, none of the injuries suffered by the sailors were fatal (Gough 1984:45).

6. Neville Lincoln is currently preparing a comprehensive analytical Kwak'wala dictionary.

7. A typical example of a sortal classifier would be *musgami migwat*, "four harbor seals," where *mu-* is a quantifier stem meaning "four," *-sgam* is the classifier used for bulky objects, *-i* is a demonstrative suffix, and *migwat* is the term meaning "harbor seal."

8. Indirect evidence that Boas understood the shape class system comes from Helen Codere,

who noted that Boas' Kwak'wala was only criticized by the Kwakwaka'wakw for being too slow, while an assimilated Kwakwaka'wakw woman was criticized because she could not correctly use shape locatives (Codere 1966:xxiv, xxvii).

9. One of the drawings is presumably the "plan of Fort Rupert as it appeared in 1866" that Boas refers to in the manuscript for the posthumous *Kwakiutl Ethnography* but that could not be found for publication (Boas 1966:48).

10. A fifth division, the Mamalilekala, moved with the others but soon returned to its original location (Boas 1921:973-7).

11. It is uncertain whether Hunt and his mother stayed at Daasaxákw on Village and Cat Islands or at Kadukguká on Tongass Island. Both sites were occupied by Tongass people during the 1860s, and Hunt mentions being at both locations (APS-KM v:5420; Hunt to Boas, 2 August 1920, APS-BPC; HBCA, Fort Simpson Journal, 24 February 1858; Olson 1967:94; Paul 1971:12).

12. Documents indicate that George Hunt was employed by the company in Fort Rupert in January 1864 (HBCA, Robert Hunt Biography; W. Tolmie to P. Compton, 9 January 1864, HBCA, B.226/b/23:304).

13. The relevant HBCA material is as follows: Tolmie to W. Smith, 15 August 1868, B.226/b/34:346; J. Bissett to J. Grahame, 12 October 1870, A.11/85:474; Grahame to R. Hunt 14 September 1871, B.226/b/44:807; Grahame to W. Armit, 3 October 1871, B.226/b/45:204, 206-7.

14. The number of slaves was between 16 and 19; slave status is not clear in several cases.

15. Hunt's father was hired to conduct an official census of Fort Rupert and vicinity in 1881 (HBCA, Robert Hunt Biography: 12; William Charles to R. Hunt, 29 March 1881, B.226/b/23 fo.132, HBCA). Hunt may have aided his father in this effort, and the experience may have influenced the form of the two censuses he carried out for Boas.

16. A second Kwakwaka'wakw collection was made by Jacobsen's younger brother in 1884 (Cole 1985:67), but Boas may not have used it.

17. Boas wrote that the Winter Ceremonial occurred in 1895-96. His letters home from the field (Rohner 1969), his subsequent correspondence with Hunt and others, and his own list of

field expenses (APS-BPC, AMNH-HAR) show that this date is erroneous.

18. The relatively unpracticed handwriting is clearly similar to that of Hunt's letters that date to the mid-1890s. By 1900, he developed very regular penmanship. The features of Hunt's earliest transcription practices include the following: "Q" or "q" as any back labialized stop or fricative; the combination "dg" as either the voiced affricate *dz* or the voiceless glottalized affricate *ts'*; the character "L" as, interchangeably, the voiced, voiceless, or glottalized lateral affricate or, with a bar above it, the lateral fricative; and the frequent use of a length diacritic above every vowels. In Hunt's post-1897 manuscripts, he has abandoned these features except the use of "L" for all lateral affricates. For example, for the word *t'lagakw*, "red-dyed cedar bark [for the Winter Ceremonial]," Hunt wrote *Lägäq* in 1895 but *Lagëkw* by 1898 (Hunt to Boas, 5 November 1895, 25 May 1898). Boas would have rendered this word as *L!agëkw*.

19. This set begins at page 41 but switches several pages later to page 34 and runs from there to page 56.

20. Hunt was aware of variation in and elaboration of this developmental sequence among the Kwakwaka'wakw but was most concerned with the four Kwaguł divisions of Fort Rupert.

21. Hunt is describing a pole that was raised to his maternal grandmother at Kadukguká on Tongass Island and later removed to Pioneer Square in Seattle. While Hunt may have been with his uncles at the time of his grandmother's death in 1870, the pole would not have been raised until some time afterward (Barbeau 1950:651-2; Paul 1971:14). It also seems unlikely, though not impossible, that his uncles would have carved their own mother's memorial pole—a task properly carried out by their moiety opposites.

22. After Hunt's death in 1933, Boas began to work with William Beynon, a part-Tsimshian man, much as he had with Hunt. From 1933 to 1941 Beynon generated thousands of pages of Tsimshian manuscript for Boas, only some of which have been edited and published (Beynon to Boas, 7 October 1935 et seq., Boas to Beynon 14 April 1941; Tsimshian Chiefs 1992; see also Anderson and Halpin 2000). The Beynon manuscripts are currently in the Columbia University Archives.

Appendix A

Location of Major Hunt/Boas Manuscript Collections

The American Philosophical Society (APS) in Philadelphia is the major repository of Boas' papers. The holdings include a number of Hunt manuscripts and related materials, in English and Kwak'wala. There are two major collections of Boas papers at the APS: Boas Professional Correspondence (BPC) and the Boas Linguistic Collection. Each has a finding aid that is close to comprehensive. The two volumes of the *Guide to the Microfilm Collection of the Professional Papers of Franz Boas* (1972) list all of Boas' correspondents alphabetically and then by date. Nearly all the other Boas and Hunt manuscripts at the APS are referenced in John Freeman, *A Guide to Manuscripts Relating to the American Indian in the Library of the American Philosophical Society* (APS, 1966). The Freeman catalogue numbers, given below, are a useful reference tool only; they are not the APS manuscript accession numbers.

Unfortunately, the contents of the massive Hunt manuscript collections referenced in the APS Freeman *Guide* are not indexed. The "List of Kwakiutl Manuscripts by George Hunt in Columbia University Library" (APS-LKM), written by Boas some 50 years ago, is a partial catalogue of Hunt manuscripts, published and unpublished, that are today split between the Columbia University Libraries, the APS, and perhaps other places as yet unknown. It seems likely that most of the still unlocated manuscripts mentioned in Boas' list will eventually be found at the APS.

The Rare Book and Manuscript Library of Columbia University holds 14 volumes of Hunt manuscripts (designated here as CU-Hunt). Volumes i-xiii of CU-Hunt consist almost exclusively of the originals of the published Hunt texts; the final volume (xiv) contains the original Hunt manuscripts for a text volume that is held at the APS, "Kwakiutl Ethnographic Texts and Translation," which never went to press (APS-KTT).

Abbreviations used in the text

For internal consistency and ease of referencing, the following abbreviations have been devised for this paper; they may bear little resemblance to abbreviations used within the holding institutions.

American Philosophical Society (APS)

- APS-BPC Franz Boas Professional Correspondence
APS-KEM Franz Boas [and George Hunt], Kwakiutl Ethnographic Materials [1900–31]. 3 vols. Boas Linguistic Collection [Freeman 1927]
APS-KM Franz Boas [and George Hunt], Kwakiutl Materials [1896–1933]. 6 vols. Boas Linguistic Collection [Freeman 1941]
APS-KTT Kwakiutl Ethnographic Texts, and Translation. 2 vols. [pt. I, Texts; pt. II, Translations]. Boas Linguistic Collection [Freeman 1938]
APS-KWD Kwakiutl Dictionary. Edited by Helene Boas Yampolsky. Boas Linguistic Collection [Freeman 1948]
APS-LKM List of Kwakiutl Manuscripts by George Hunt in Columbia University Library. Boas Linguistic Collection [Freeman 1923]
APS-RMC Franz Boas, Remarks on Masks and Ceremonial Objects of the Kwakiutl [Amplification and correction of specimens in Boas 1897, with information on use]. Boas Linguistic Collection [Freeman 1926]
APS-SHN Edward Sapir and George Hunt, Nootka Tales. 4 vols. [two, the original Hunt ms.; two, a revised typescript] [Freeman 2405]

Anthropology Archives, American Museum of Natural History (AMNH)

- AMNH-HAR George Hunt Accession Records
AMNH-HCF George Hunt Correspondence File
Rare Book and Manuscript Library, Columbia University Libraries (CU)
CU-Hunt George Hunt, Manuscript in the Language of the Kwakiutl Indians of Vancouver Island. Preface by Franz Boas, reviser. 14 vols.

Hudson's Bay Company Archives (HBCA), Provincial Archives of Manitoba

- Fort Simpson Journal 1855–59
Robert Hunt Biography

Other Manuscript Sources

- FRP Register of Fort Rupert Land Purchase, British Columbia Archives, Victoria
Hunt Memorandum Book, Private Collection

Appendix B

Table of Contents for the unpublished "Kwakiutl Texts" volume

[from the copy at the American Philosophical Society, APS-KTT]

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61/ Jesup Expedition Collections displayed at the American Museum of Natural History, 1905 (AMNH 386)



part 3

THE RESOURCES: CRITICAL VIEWS IN THE POST-JESUP ERA

The “Russian Bastian” and Boas

Why Shternberg’s “The Social Organization of the Gilyak” Never Appeared Among the Jesup Expedition Publications

SERGEI KAN

This paper, like the manuscript it deals with, has a rather complicated history. It was originally written for a session devoted to the Jesup North Pacific Expedition (JNPE) at the 1993 meeting of the American Anthropological Association (see Fitzhugh and Krupnik, this volume; Kan 1993).¹ The aim of that original paper was to establish why Shternberg’s “The Social Organization of the Gilyak,” which had been commissioned by Boas in 1904 for the JNPE series, never saw the light of day. At that time, I had done but a limited amount of research on Shternberg’s biography and scholarly activities, using his own and others’ published works as well as his correspondence with Boas, which is preserved in the archives of the American Philosophical Society (APS) and the Department of Anthropology, American Museum of Natural History (AMNH-DA). I had also utilized both the Russian- and English-language versions of his Gilyak manuscript located at the AMNH.

Although my paper did provide a fairly accurate answer to the question it asked, it did not utilize the large collection of Shternberg materials at the Archive of the Russian Academy of Sciences (AAN), St. Petersburg Branch, and consequently did not go far enough in exploring the various intellectual, political, and personal obstacles that prevented Shternberg from completing the monograph.² But the paper nevertheless served an important purpose: at my suggestion, the AMNH decided to finally publish this manuscript, which had been lingering in its archive for over half a century. Bruce Grant, who has done archival research on

and ethnographic fieldwork among the Gilyak [Nivkh] and has published his own book (Grant 1995) on their cultural and sociopolitical history under Soviet rule, edited the AMNH manuscript and wrote the foreword. In preparing Shternberg’s Gilyak study for publication, Grant examined many of the same source materials as I had, as well as my 1993 manuscript (Grant 1999:xliv) and, more important, a number of key documents from the Shternberg archive. The result of Grant’s work—both his substantial foreword and his notes—is a major tour de force that answers many of the questions raised a few years earlier (Shternberg 1999).

My own research on Shternberg’s intellectual biography, which has been going on since 1998, has involved a thorough examination of most of the documents from the Shternberg archive, as well as a careful review of his entire corpus of publications.³ In the course of this work, I have discovered some important additional information on the history of the Gilyak manuscript. I have also come to some conclusions about its content that do not fully agree with or that at least supplement those of Grant (1999). Consequently the focus of the present piece is rather different from that of its 1993 predecessor.

While Grant’s critical evaluation of the contents of the Gilyak manuscript concentrates mainly on Shternberg’s deeply flawed evolutionist reconstruction of Gilyak social organization, I pay more attention to the monograph’s last three chapters, which discuss, in a synchronic perspective, the functioning and the

religious symbolism of the clan—the key unit of the Gilyak sociopolitical and ideational universe. I argue that in this part of his work, in which Shternberg eloquently demonstrates the interrelationship between the Gilyak social structure and the Gilyak religious worldview, he sounds more like Durkheim and Mauss than like Morgan and Tylor. My analysis also shows that his fascination with and very positive evaluation of the role of the clan in Gilyak culture and society had much to do with his own lifelong commitment to Russian populism (*narodnichestvo*), a unique blend of western socialist and home-grown ideas. In fact, I believe that this contradiction between Shternberg's progressivist 19th-century evolutionism and his somewhat romantic admiration for the precapitalist social organization and social life of Siberia's indigenous peoples was central to his entire scholarly worldview and set him somewhat apart from the classical evolutionists.

Boas' correspondence, not only with Shternberg himself but with Shternberg's closest Russian colleagues and friends, Bogoras and Jochelson, sets the saga of the manuscript's preparation and its absence from the Jesup publication series in the context of the larger story of Boas' complex, four-decade-long relationship with his three Russian colleagues. Such contextualization of the Boas-Shternberg relationship gives us a much better understanding of Boas' truly heroic efforts to foster a Russian "ethno-troika" and to encourage its greater concentration on scholarly work than on left-wing political activities and journalism (and in Shternberg's case, on Jewish politics, as well).⁴

Boas first became acquainted with these scholars on the eve of the Russian Revolution of 1904–05, when he recruited them to take part in the JNPE project. His effort to maintain close contact with them throughout the turbulent 1910s, World War I, the February and October Revolutions of 1917, the devastation of Russia in the early 1920s, and the rise of Stalinist totalitarianism in the late 1920s and early 1930s indicates the importance of this relationship for him, both as a scholar and as a human being. Similarly, the relationship was

very important to the three Russian scholars, both professionally and personally. Boas, after all, had always been one of their most important western professional contacts, the main publisher of their scholarly works outside Russia, a source of badly needed additional income, and a close friend. Although the space limitations of this paper do not allow me to explore Boas' relationship with Bogoras and Jochelson in as much detail as that between him and Shternberg, I believe that this topic is crucial for our understanding of the entire Jesup project and requires a great deal of further investigation (see Krupnik 1998). At this point, however, I simply offer an examination of the relationship between Boas and Shternberg, whom Boas once referred to as the "Russian Bastian" (Boas 1934:xli), as well as a preliminary review of Shternberg's scholarly contributions and public life (see Kan 1993, 1999, 2000). The purpose of this paper is also to emphasize that, in many ways, Shternberg was very much a part of the Boasian JNPE project, although the long delay in publishing his contribution has obscured this fact.

Shternberg as a Jewish Populist

Since Shternberg's biography has been recently outlined (Grant 1999), I offer only a brief overview of his political and scholarly activities, focusing in particular on those aspects that either are directly related to his work on the Gilyak manuscript or are not discussed in detail by Grant.⁵ Lev Shternberg was part of a cohort of Russian-Jewish revolutionary populists (*narodniki*) who rose against the tsarist government in the late 1870s–mid-1880s and were sentenced to exile in Siberia. His future friends and colleagues, Jochelson and Bogoras, shared the same ethnic, social, and political background and suffered the same punishment.

Born in 1861 in Zhitomir, a provincial Ukrainian town, Lev (Khaim) Iakovlevich Shternberg attended the local Jewish religious school, where he acquired a deep knowledge of the Hebrew Bible and, in the words of his childhood friend, Moisei Krol' (1929:215), was inspired to begin asking "important questions of

a religious, juridical, and moral nature.” Although later in life Shternberg moved away from the traditional Judaism of his childhood and became a member of the urban *intelligentsia*, he did retain a deep affection for his people and a strong interest in their culture and historical experience. Like many other Jewish populists of his era, he was particularly drawn to the ideology of the biblical prophets, with their emphasis on compassion and social justice (see, for example, Shternberg 1924; Haberer 1995). Unlike Bogoras and Jochelson, he eventually became very active in Jewish political and cultural activities, journalism, and ethnographic research.

Shternberg’s life changed dramatically at the age of 10, when his father sent him to a Russian high school. There, he entered a new world of secular culture. He devoured the classical novels by Russian and western European authors and then the works of Darwin and other materialist natural scientists and philosophers, which were extremely popular with young Russian intellectuals in the 1860s and 1870s (Vucinich 1988). He also began studying the works of the Russian “revolutionary democrats” of the previous generation who attacked Russia’s conservative political regime and backward socioeconomic system.

Soon, a biblical commandment “to love thy neighbor” became an inspiration for him to fight for social justice (Krol’ 1929:218). In 1876–77 populist ideas spread quickly from the urban centers to the provincial towns. Young people, many of them members of the lower middle class and the intelligentsia, organized a movement of “going to the people,” that is, to the Russian peasants, whom the *narodniki* hoped to radicalize through education and political propaganda. Although Shternberg was too young to join this movement, he helped the radicals in various ways (Hardy 1987).

It is not surprising that in 1881, on graduating from high school, Shternberg decided to enroll in St. Petersburg University, one of the most intellectually and politically progressive institutions of higher learning in Russia and a major center of populist activities.

Having chosen the natural sciences division, he attended lectures by the leading scientists of the day, who introduced him to the latest positivist, evolutionist, and materialist theories. Along with Krol’ and Bogoras, Shternberg joined the student branch of the People’s Will, the leading underground populist party (which by this time was in decline), and in 1882 he played an active role in organizing a large student demonstration against increased restrictions on the students’ academic freedom (Naimark 1983). As a result, all three were expelled from the university and banished from the capital (Krol’ 1944:22–46).

Shternberg then became a student in the law division of Novorossiysk University in Odessa, where for four years he studied subjects that were closely related to his future work in comparative ethnology: history, philosophy, sociology, and primitive law. In Odessa he became a leading member of the “Southern Group” of the People’s Will. In 1886, during his graduation examinations, Shternberg was arrested, along with other activists of the Southern Group, including Bogoras. The People’s Will was finished (Naimark 1983; Haberer 1995:242–51). After spending three years in solitary confinement in an Odessa prison, where he studied several foreign languages as well as history, political science, and other subjects (AAN, 282/1/120), Shternberg was exiled to Sakhalin Island, Russia’s infamous penal colony (Grant 1995, 1999).

Like other populists, Shternberg had a strong faith in the power of science (understood in positivist and materialist terms) and in sociopolitical and moral progress. He subscribed to the theory of social evolution and saw the evolution of ideas as the main cause of social progress—like most other late 19th century evolutionists, but unlike Marx, whom the *narodniki* did study and respect a great deal (Malinin 1991; see also Stocking 1987). He shared the populists’ strong interest in and romanticization of the Russian peasant commune, seen as the foundation of a more egalitarian, nonexploitative, and just society of the future that was to be different from the capitalist West. In the 1870s

through the 1890s, interest in rural social institutions and the spiritual culture of the peasants—and, by extension, the “precapitalist” Siberian natives (*inorodtsy*)—stimulated a great deal of sociological, folkloristic, and ethnographic research, carried out mainly by the exiled populists (Tokarev 1966; Slezkine 1994:113–29).

Shternberg as Ethnographer/Social Theorist

After arriving on Sakhalin in May 1889, Shternberg continued reading voraciously and studying European languages, philosophy, and history.⁶ He soon came across the island’s main indigenous people, the Gilyak [Nivkh], who occasionally visited Aleksandrovsk, the main Russian community on Sakhalin, where he had initially settled.⁷ In the spring of 1890 Shternberg was punished for defending a fellow exile from administrative abuse and was sent from Aleksandrovsk to Vyakhtu, a remote military outpost 100 kilometers to the north. There he was able to get a much closer look at the natives who lived nearby and often came to Viakhtu to trade.

While some exiled revolutionaries might have been pushed toward ethnographic research by the sheer boredom of their life (see Vahktin, this volume), this seems not to have been the case with Shternberg. As he wrote two decades later, “My previous scholarly studies, predominantly in the domain of the humanities and the social sciences, naturally pushed me . . . towards the study of the Gilyak social and spiritual culture. My primary interests included the family structure, the clan, and religion, followed by poetry [folklore] and language. At that time I was particularly interested in the first two and with them I began” (Shternberg 1908:viii).

Shternberg’s research methods included some participation in the Natives’ daily activities, such as hunting and trapping (see AAN, 282/1/2, p. 10), as well as working with an informant, an influential and wealthy man who often visited the post and traded information on the Gilyak religion and other subjects for bread, sugar, and tobacco (AAN, 282/1/2, p. 10; Shternberg 1999:5). Even though many of the Gilyak visitors to the post spoke some Russian, Shternberg

soon realized that without learning the Gilyak language and using it to gather ethnographic data, any attempt to understand the Natives’ “true [*podlinny*] life,” and especially its “psychological aspects,” would fail (Shternberg 1908:viii–ix).⁸

In February 1891 the island’s Russian administration found out about Shternberg’s studies, and he was asked to undertake a census of the Gilyak population in the northwestern part of the island. Eventually, he was allowed to visit the rest of Sakhalin and the nearby lower Amur River region, where he continued his census work as well as his ethnographic observations of the Gilyak, Oroki [Uilta], Ainu, Orochi, and Gol’dy [Nanay]. Except for his first ethnographic expedition, undertaken in the winter of 1891, Shternberg normally surveyed the Native settlements in the summer and spent the winters analyzing his data, as well as collecting additional information from visiting Natives and a few young Gilyak who resided with him for substantial periods of time.⁹

The fact that a significant part of Shternberg’s ethnographic research was conducted in the context of rather brief visits to Native settlements for the purpose of census taking had a definite effect on the kind of data he was able to collect.¹⁰ Although, like most other ethnographers of his time, he tried to gather information on every aspect of Native life and even bought objects of material culture and undertook some archeological excavations, much of his data had to do with demography, kinship terminology, and the Natives’ statements about their laws, customs, and beliefs, rather than his own observations of their everyday and ceremonial life.

To Shternberg’s credit, he was a tireless ethnographer who used every opportunity to question his Gilyak hosts and guides about their culture. He even developed a clever method of encouraging the Gilyak to share information with him: he would often show them an illustrated book depicting the various peoples of the Russian Empire and ask them to compare those peoples’ “exotic” customs with their own (Shternberg’s

1891 diary, AAN, 282/1/3, p. 82; Shternberg to Krol', 19 May 1891, AAN, 282/2/363, p. 30). This cast his relationship with them in a more reciprocal light. He also used every opportunity to get at the deeper layers of the Gilyak religious worldview and philosophy. For example, during one of his journeys through northern Sakhalin, Shternberg climbed a mountain that the Gilyak considered very sacred. His Native guides were terrified and were convinced that he would not come back alive. When he did, they volunteered a great deal of valuable information on the mythology and religious beliefs surrounding the sacred site (Shternberg 1908:30).

Shternberg's study of the Gilyak language and his method of recording the various genres of Gilyak folklore were on a par with the work of most other Russian and foreign ethnographers who had not had any previous training in linguistics.¹¹ At the same time, neither his published works nor his field notes contain many really detailed descriptions of Gilyak rituals, despite his interest in "primitive" religion.

As his diaries and journals indicate, Shternberg stayed in a Gilyak village only long enough—usually only for a few days—to conduct an adequate census and record kinship terms, along with some other data, but not long enough to make any systematic, detailed observations of day-to-day activities, social interactions, or rituals. In fact, although he was happy about the research opportunities census taking provided, he complained on occasion that his Native hosts would sometimes become bored with the census-related questions and would give him only perfunctory answers. Hence, while he eventually became a strong advocate of what he called "the [long-term] stationary method" of field research (Bogoras 1928; Ratner-Shternberg 1935), his own ethnographies lack the kind of rich and detailed data, derived from first-hand observation, one finds in Malinowski's writing on the Trobriand Islanders or in Bogoras' on the Chukchi (Bogoras 1904–09).

From the very beginning, Shternberg's ethnographic research had a definite focus on the Gilyak system of

kinship and marriage, which also accounts for a certain one-sidedness of his data. His interest in these topics probably resulted from his previous reading in primitive law and social organization, as well as his populist fascination with the workings of a rather egalitarian social order in which exploitation of the poor by the wealthy was absent. As Shternberg wrote to Krol' on May 19, 1891, just a few months after his first trip through northern and northwestern Sakhalin, the life of the Gilyak was "wholesome and full [*tsel'naia i polnaia*], the individual and the group are linked together by natural bonds . . ." (AAN, 282/2/363, p. 34). The same letter indicates that by this time he had already read Engels' book *Der Ursprung der Familie* (The origin of the family) and that through Engels he had become familiar with Morgan's reconstruction of the evolution of marriage and the family.¹²

Shternberg's letters and diary entries show that soon after initiating his research on the Gilyak he became firmly convinced that he had discovered evidence of group marriage among them. In the same letter to Krol', he wrote:

My main accomplishment is the study of their social organization and marriage system. I discovered among them a system of kinship nomenclature and a system of family and clan law [*semeino-rodovoe pravo*] which are identical to those which exist among the Iroquois and in the case of the famous Punulua. In other words, I found the remnants of that form of marriage upon which Morgan had built his theory and which serves as the starting point of the brochure *Der Ursprung der Familie*. . . . At first I was afraid to believe my discovery. However, during the census-taking, when I tried not to miss a single family or a single dwelling, I asked detailed questions about the terms of address used by the various family and clan members and about their sexual rights and finally became convinced that my discovery had been correct. Despite the fact that quite a few descriptions of the Gilyak exist, none has addressed this issue, at least in the works known to me. I plan to publish a report about those aspects of the Gilyak social life, which I have studied, and hope that it would [be] of interest not only to the specialists. (AAN, 282/2/363, pp. 36–9)

As Shternberg's first ethnographic report on the Gilyak, written in 1891 and published two years later in one of Russia's two major ethnological journals, indicates, he was aware of the fact that by the 1890s the Gilyak had become basically monogamous and that the "sexual/marital rights" he had "discovered" among them were no longer exercised all the time. In fact, their occasional exercise could cause displeasure and even violent protest from the woman's husband. However, in Shternberg's words, "from the legal point of view, so to speak, they [these rights] still exist and their exercise is not considered adultery, is not penalized, and is often carried out with the permission of the man's brothers and his wife's sisters' husbands" (Shternberg 1893:7, 15). As Grant (1999:xl-xlii) points out, what Shternberg found among the Gilyak was not a survival of group marriage but "a loose kind of monogamy" characterized by "discreet but permissible affairs" between certain categories of relatives, especially if one of the participants in the affair was a visiting guest. Shternberg's firm adherence to Morganian evolutionism—and, I believe, a certain feeling of "eureka"—prevented him from ever questioning his "discovery."

This fascination with Gilyak social organization is clearly reflected in Shternberg's first ethnographic publication, two-thirds of which is devoted to discussion of the family, the clan, kinship and marriage, and indigenous law. While this essay contained a fairly detailed account of the Gilyak system of kinship and marriage, as well as an interesting and laudatory description of the Gilyak agnatic clan, including a discussion of the clan's symbolism (see below), his comments on Native religion are fairly brief and are cast in evolutionist terms (Shternberg 1893:22). Another example of his lack of understanding of the depth and complexity of the Gilyak religion is his inadequate treatment of the bear festival as a purely social institution that, in his view, functioned simply to strengthen intraclan bonds and had no religious significance (Shternberg 1893:9). This view of the most important Gilyak ceremony was eventually challenged by some of Shternberg's own published

data and, especially, by the work of later ethnographers (e.g., Kreinovich 1973). Shternberg also argued that despite several centuries of Gilyak interaction with and subordination to the Manchurians, the Chinese, the Japanese, and, most recently, the Russians, their culture had remained largely intact and could thus be used for a comparative study of primitive social organization and religion.¹³

Despite its obvious limitations, Shternberg's 1893 essay on the Gilyak generated considerable interest among Russian ethnographers, both because of its description of a relatively "unknown and exotic" culture and on account of its "discovery" of an interesting form of "primitive marriage." Moreover, his "discovery," summarized briefly in a Russian newspaper, was noted by Engels himself, who praised it in an article in *Die Neue Zeit* entitled "A Newly Discovered Case of Group Marriage" (see Engels 1933 [1892-93]). For Engels, Shternberg's "discovery" represented a powerful proof of the validity of Morgan's evolutionary scheme and his own arguments in *The Origin of the Family* (1972 [1884]; see Grant 1995:55-8; 1999:xli). This recognition by the scholarly community, including one of the leaders of the world socialist movement, was obviously very important for Shternberg, who still occasionally expressed doubts about his research and especially about his lack of training in ethnology and linguistics (see his letters to Krol', AAN, 282/2/363).

Having now become even more convinced of the validity of his evolutionist theorizing, Shternberg went on to "discover" another example of Morgan's classificatory system of kinship relationship and group marriage, this time among the Orochi of the Tatar Strait, a Tungus-speaking group of sedentary hunters and fishers living on the Pacific Coast across from Sakhalin Island. The results of his Orochi research appeared in an 1896 essay published in several installments in a local newspaper (Shternberg 1896). In it, Shternberg spoke with the greater authority of an ethnographer who had already made an important discovery among a neighboring people, as well as a comparativist who

had read a great deal of theoretical literature on the evolution of marriage and social organization.¹⁴

Shternberg's Career in the Early 1900s

In May 1897 Shternberg's exile ended, and he returned to his hometown. However, without a university diploma it was difficult for him to find a satisfying and adequately paying job. While doing some writing for a local newspaper, he also busied himself with organizing his Gilyak data and preparing it for publication. His friends and fellow populists, Krol', Bogoras, and Jochelson, who had finished their exile earlier, had already begun publicizing their ethnographic and linguistics data among several prominent members of the Russian Academy of Sciences (RAS) in St. Petersburg and were looking for money to publish them. They tried to help him follow their path (see Jochelson's letters to Radloff, 24 February, 17 November 1898, AAN, 177/2/120, pp. 1–4).

Of the three, it was Krol' who spoke about Shternberg with Vasily V. Radloff, the head of the Museum of Anthropology and Ethnography (MAE) and a leading specialist on the languages and folklore of the Turkic-speaking peoples of Central Asia and southern Siberia. After describing in glowing terms Shternberg's Gilyak ethnography and the ethnographic community's response to it, Krol' managed to convince Radloff that his friend had to reside in St. Petersburg and work for the MAE (Krol' 1944:274–6; letters from Krol' to Shternberg, 1899–1900, AAN, 282/2/157). Thanks to Radloff's intercession, the police gave Shternberg—who was required by law to reside within the “pale of Jewish settlement”—a three-month permit to live in the capital. Bogoras also spoke to Radloff about his friend's research and sent Shternberg instructions on how to prepare his linguistics work so as to make it more interesting to the MAE, especially to Karl Zaleman, a member of the Academy and a prominent specialist on Central Asian languages (Bogoras' letters to Shternberg, 1899, AAN, 282/2/34, pp. 15–17; Zaleman's letters to Shternberg, 1900–01, AAN, 282/2/107). Shternberg's

friends' efforts paid off: in the spring of 1899 Zaleman agreed to examine his “Obraztsy materialov po izucheniiu giliatskogo iazyka i fol'klora” (Samples of materials for the study of the Gilyak language and folklore) and was very impressed with the work. Later that year, Zaleman and Radloff invited Shternberg to St. Petersburg, where he spent several months interacting with them and several other prominent linguists and ethnologists. With substantial help from Zaleman, Shternberg prepared his “Samples” manuscript for publication, and in 1900 it appeared in the RAS publication series (Shternberg 1900). By that time, Shternberg's permit to reside in the capital had been extended, and he could finally bring his wife, Sarra Ratner, there.

Through Krol', he also met a number of prominent liberal journalists, many of them populist sympathizers or “legal populists” (Malinin 1991), as well as future leaders of the Constitutional Democrats (KD), Russia's leading liberal political party. As a result, he began writing on political subjects for several well-known progressive newspapers and submitted reviews of books on ethnology, sociology, and related subjects to *Russkoe bogatsvo*, an influential literary and political journal of the legal populists. From then on, journalistic writing remained an important avenue for expressing his views on social and political issues, as well as a source of badly needed supplementary income. Most important for Shternberg's scholarly career was an invitation to become the editor of the ethnology section of the remaining unpublished volumes of the famous *Encyclopedic Dictionary of Brockhaus and Efron*, which featured articles by the country's leading liberal intellectuals. In the course of writing a large number of entries for it and editing those written by others, Shternberg familiarized himself with many of the latest Russian and western anthropological publications and reaffirmed his evolutionist position, as well as his view of “ethnography” (anthropology) as a comparative and holistic discipline that had to become the cornerstone of all the humanities and the social sciences. By 1904 the project had been completed, but throughout the

1910s Shternberg wrote and edited entries on anthropological topics for several other Russian encyclopedias and dictionaries. In 1901 Radloff invited him to join the staff of the MAE, where he stayed the rest of his life. By 1904 he had been appointed the museum's senior ethnographer—its second in command.

From Gilyak Ethnography to Evolutionist Ethnology with a "Durkheimian" Twist

In the early-to-mid-1900s, Shternberg also prepared for publication his only two major monograph-length works: an annotated collection of Gilyak folklore (Shternberg 1908), and a rather extensive Gilyak ethnography that elaborated on many topics only briefly mentioned in his 1893 essay and introduced a number of new ones (Shternberg 1904). From the point of view of this paper, his discussion of the Gilyak kinship and marriage system and of the centrality of the clan in Gilyak social life is particularly important, since it formed the core of "Social Organization of the Gilyak." An analysis of the 1904 work also demonstrates the theoretical maturity that Shternberg had achieved before beginning his earnest correspondence with Boas.

Like his 1893 essay, Shternberg's 1904 Gilyak monograph was not a truly comprehensive one in the classic Boasian style, although it was three times as long as the earlier piece. While it did cover a variety of topics, including the origin of the Gilyak and their natural environment, subsistence, material culture, language, and religion, issues related to social organization were, once again, at its core. At the very beginning of his work, Shternberg justified his focus on this topic: "No other aspect of the Gilyak social life differentiates them so sharply from the surrounding peoples as their classificatory system of relationships and the rules regulating sexual relations and marriage" (1933a [1904]:30). Although the new discussion of Gilyak kinship differed from the old one mainly in the amount of detail presented and not in substance, it did contain important new information on "a triangulated system of marital exchange, based on a tri-clan phratry or alliance group

. . . that underwrote a complex web of mutual social and economic obligations" (Grant 1999:xl).

As a comparative ethnologist with a secure position rather than just an ethnographer, Shternberg compared the Gilyak kinship and marriage system with those of the Australian aborigines and other "primitive" peoples and concluded that the former was very similar to the "Punaluan" system documented by Morgan. In fact, he used his own Gilyak data to "solve" a number of puzzling questions raised by the work of several western ethnographers in other parts of the world. It is obvious that Shternberg's evolutionism had become even stronger in the time between the publication of his first and second Gilyak studies. Thus, the 1904 publication omits a passage that appeared in the 1893 article about the displeasure often caused by theoretically permissible sexual liaisons among the Gilyak. In fact, by the early 1900s, Shternberg appears to have become so wedded to evolutionism that he ignored his own data on a widespread Gilyak practice of marrying outside the prescribed clan and even outside the ethnic group (e.g., Shternberg 1933a:45). For him this phenomenon represented a more recent departure from the original "pure" practice that he tried so hard to reconstruct. As Grant (1999:xliv) correctly points out, the clan system that Shternberg so elegantly described "was far less fixed than he first had perceived it. Given the swell of non-Gilyaks into the area, the increasing dislocations through travel and trade, and the demographic havoc wrought by disease," much of what he had presented was only an ideal system.¹⁵

To Shternberg's credit, it should be noted that when describing the "survivals of group marriage" among the Gilyak, he repeatedly stated that the Gilyak were not promiscuous and that they strictly followed their own laws of morality. Unlike most western evolutionists, who saw "primitive" forms of kinship and marriage as something to be overcome by progress, this Russian populist was ambivalent about them. On the one hand, as a firm believer in humankind's inevitable progress, he did express hope that some day the best aspects

of European civilization would be accepted by the Gilyak and other indigenous Siberians. On the other hand, he admired many Gilyak customs, especially their social solidarity and the support an individual found in his or her primary kinship group, the agnatic clan.

In my view, it is Shternberg's detailed and sensitive discussion of the socioeconomic and political functions and religious symbolism of the Gilyak clan, which he convincingly presented as their central institution "regulating all of the other aspects of their life" (1933a:81), that makes his writing on the Gilyak different from most other contemporary evolutionist accounts of the social life and culture of "primitive peoples." Paradoxically, while Shternberg never cites Durkheim and Mauss in his works, his discussion of the Gilyak clan, especially the interconnectedness between its social and ideological symbolic dimensions and the harmonious relationship between the individual and the group in Gilyak society, is strongly reminiscent of *Primitive Classification* (Durkheim and Mauss 1963 [1903]) and other works by these authors. This similarity should not surprise us. Like Shternberg, Durkheim and Mauss were socialists who sought in primitive societies characterized by "simple economic relations and an integrated socioreligious world view" (Shternberg 1933a:113) an alternative to modern capitalist society's "organic solidarity" and anomie.¹⁶ Also like Durkheim, Shternberg was fascinated by the fact that the Gilyak adhered to their laws "despite an almost total absence of authority or compulsion" (Shternberg 1933a:108).

In his concern with the freedom of the individual, Shternberg differed from Marx and Engels and their followers. While he occasionally describes Gilyak economic and social life as a kind of "primitive communism," he also emphasizes that among the Gilyak, "communism and individualism coexist almost without tension" (Shternberg 1933a:83). Like his fellow-populists' descriptions of the Russian peasant commune, Shternberg's account tended to overemphasize egalitarianism and downplay economic and sociopolitical inequality. He appears to have been correct, however,

in stating that in a society like that of the Gilyak, the wealthy leaders had to support their less fortunate clan relatives, and that clan solidarity would thus ameliorate the hierarchical tendencies. More important, unlike most of the classic evolutionists or the Marxists, but like Durkheim, Shternberg was interested in the effect of a "clan-based social order" [*rodovoi stroi*] on an individual's personality. In his view, an average Gilyak had a "holistically developed personality with its integrated world view" (Shternberg 1933a:120).

Finally, like the Durkheimians and their followers among the British structural-functionalists, Shternberg paid a great deal of attention to the role of religious sanctions in encouraging the individual to adhere to the rules and laws of his or her society. His approving discussion of the Gilyak clan ends with a virtual hymn to an institution that he refers to as a "whole school of social upbringing, a school of benevolence, hospitality, compassion, and . . . proper social conduct [*blagovospitannost*]." In this school those social habits and emotions are created, which eventually become too strong to be limited to interclan ties and evolve into sympathy towards one's entire tribe [people] and eventually towards human beings in general" (Shternberg 1933a:127). Here the voices of Shternberg the ethnographer and Shternberg the populist merge into one.¹⁷

Boas, Shternberg, and the Jesup Expedition Publications, 1900s–1917

Shternberg's career and theoretical development are important because of his considerable influence on Russian anthropology. This paper, however, focuses mainly on his relationship with Boas and Boas' efforts to persuade him to produce a monograph on the Gilyak for the Jesup Expedition publication series. The development of Boas' plan for a large-scale expedition aimed at studying the cultural affinities between the inhabitants of the coasts of eastern Siberia and northwestern North America, and his efforts to recruit Bogoras and Jochelson to lead the Russian part of the expedition and then transform their field data into

detailed monographs, have been well documented by scholars and will not be repeated here.¹⁸

Less known is the fact that Shternberg's three friends mentioned above attempted to get him, too, involved in the project. Thus, in a letter sent some time in 1899 to Shternberg, who was still in Zhitomir, Krol' wrote, "Your trip to America did not materialize—they already have their own 'Gilyak'" (AAN, 282/2/157, p. 110). The reference here is obviously to Berthold Laufer, a young German linguist and sinologist whom Boas had recruited in 1897 to undertake research among the Natives of the lower Amur River and Sakhalin Island and who spent 16 months there beginning in the summer of 1898. In another letter to his friend, dated January 31, 1899, Krol' urged Shternberg to waste no time and to send at least one analyzed Gilyak text to the St. Petersburg academicians as soon as possible in order "to beat Laufer" (AAN, 282/2/157, pp. 274–6). Thus it appears that had Shternberg already been living in St. Petersburg when Boas was negotiating with his Russian colleagues about the Siberian part of the expedition, he would have been hired along with Bogoras and Jochelson. Instead, the field research in the Russian Far East was carried out by a much less experienced ethnographer who spoke neither Gilyak nor Russian and who worked only through interpreters, except when he could find a Native who knew Chinese.¹⁹

It must not have been difficult for Boas to realize that the data collected by Laufer were inferior to those of Bogoras and Jochelson, the two seasoned Siberian ethnographers. While the Russians managed to produce enough contributions to fill four volumes of the JNPE publications, including two very substantial and rounded monographs (Bogoras 1904–09; Jochelson 1908), Laufer's contribution to the same series was limited to a slim essay on the decorative art of the Amur River tribes (Laufer 1902).²⁰ During their stay in New York in 1902–04, Bogoras and Jochelson undoubtedly told Boas about Shternberg's extensive research in the same area where Laufer had labored with such limited results. Boas also must have heard a lot about Shternberg

in the course of his negotiations with Radloff about sending to the MAE duplicates of the objects collected by the two Russians for the AMNH.

Shternberg and Boas Meet

The first evidence of Boas' interest in having Shternberg write something for the JNPE series is in Jochelson's March 30, 1903, letter to Shternberg (AAN 282/2/124, p. 4a). Boas had decided that Shternberg had to be brought to New York by the end of the summer of that year to work with the AMNH's Amur and Sakhalin collection and write a monograph on the Gilyak. As Jochelson put it, "Boas wants you to work on the collection . . . but his real goal is to get acquainted with your Amur and Sakhalin materials" (AAN 282/2/124, pp. 6–7). Boas wanted the MAE to send Shternberg on an official business trip to the United States and was willing to commit AMNH funds to cover some of the expenses involved (AAN 282/2/124, pp. 6–7). In a letter to AMNH Director [Hermon C.] Bumpus, Boas described his reasons for bringing Shternberg to New York:

Dr. Sternberg has lived in the Amur River area and on the Island of Sakhalin for ten years and has made very extended studies on the Gilyak and Ainu. The results of his investigations are being published now by the Petersburg Academy of Sciences. Professor Radloff thinks that it would be of advantage to Dr. Sternberg to familiarize himself with the collections of our Museum, and I believe that it would be of very great advantage to us to have Dr. Sternberg go in detail over our Ainu and Gilyak material. I should also very greatly value the opportunity to discuss with him fully the tribes of the southeastern part of Siberia, which are of great importance in relation to the Jesup Expedition. Dr. Sternberg's services would also be valuable in selecting the duplicates which Mr. Jesup intends to present to the Museum of the [Russian] Academy of Sciences. (Boas to Bumpus, 26 October 1903, AMNH)

Unfortunately, because of some bureaucratic problems at the AMNH, Boas was unable to carry out his plan and decided to postpone Shternberg's visit to the

United States until 1905. In the meantime, on April 30, 1904, he sent Shternberg an invitation to attend the 14th International Congress of Americanists, to be held in Stuttgart in August 1904 (AAN/282/2/29, p. 1; see Jochelson to Shternberg, 25 January 1904, AAN, 282/2/124, p. 8; Boas to Radloff, 23 January 1904, AMNH). As Boas wrote to Shternberg: "Your thorough knowledge of the Ainu and Gilyak will be of great value to us, and I believe that the comparative points of view, which the other gentlemen [Bogoras, Jochelson, and Laufer], who partake in the conference, possess, will be of interest to you" (AMNH). On June 6, 1904, Shternberg replied, thanking Boas for his invitation and for "affording" him "the possibility of taking part in the discussion of the great northeastern Siberia and northwestern America problem," which he himself had already been "greatly interested in" (AMNH).

The congress was Boas' first opportunity to meet Shternberg and discuss with him a variety of scholarly issues of mutual interest in the company of Bogoras, Jochelson, and Laufer. While all three of the Russian participants made presentations at the congress, it was Jochelson's (1906) and Shternberg's (1906a) papers that reflected most closely Boas' comparative JNPE agenda. For the purposes of this paper, Shternberg's presentation is particularly important, since it demonstrates that he had been interested in that agenda for some time.

In the wake of this meeting, on March 2, 1905, Boas sent Shternberg an official invitation to visit the AMNH "for the purpose of examining and re-arranging our collections from the Amur River region and also to write out such information on the ethnology of those tribes as may seem best after an examination of our material, and after our discussion of your publications." He also expressed the hope that Shternberg would be able to share his knowledge of the region's ethnology with Laufer, who at that time was working for the AMNH (AAN, 282/2/29: pp. 2-3).

A few weeks later, Shternberg sent Boas a letter. He accepted the invitation and mentioned his plans to study AMNH's Amur and Sakhalin collection, "writing

out all the necessaries for the literary work to be carried out at home" (AMNH). Although we do not know exactly what sort of monograph Boas had asked Shternberg to write for his series, one would suspect that he was hoping for something as comprehensive as Bogoras' and Jochelson's contributions. It is possible, however, that he was willing to make an exception for Shternberg, whose research interests, as we have seen, had a definite focus. In a letter to Jochelson, dated April 22, 1905, Boas wrote, "I hope that he [Shternberg] will contribute to our series of Memoirs a description of the religious life and sociology of the Gilyak" (AMNH).

Shternberg arrived in New York in late April-early May 1905. Although his goal was to engage in museum work, he must have been preoccupied with the dramatic events in his native country. By mid-spring of 1905, Russia had already plunged into tremendous political turmoil. A disastrous war with Japan, begun in 1904, broke the patience of both the ordinary people and the liberal intelligentsia. For several years already, the latter had been gravitating toward the underground *Soiuz osvobodzhenia* (Union for Liberation), to whose newspapers Shternberg occasionally contributed. In their speeches given at the famous "banquets" of late 1904, the liberals advocated political reform and the establishment of basic freedoms. Shternberg must have been involved in these meetings, since many of his friends and fellow journalists were.

Although undoubtedly encouraged by the rising tide of the liberal and radical opposition to the old regime, Shternberg was deeply troubled by a simultaneous increase in anti-Semitic propaganda and especially by the anti-Jewish pogroms that began in the early 1900s and continued throughout the decade. For him, the right-wing attacks on the intelligentsia and the workers that accompanied the rise of the revolutionary movement were similar in essence to the pogroms. Having never lost his interest in the fate of his fellow Jews, and having been galvanized by the pogroms, Shternberg came to believe that the struggle for political liberation and socioeconomic justice in

Russia had to include a concerted effort to emancipate the Jews, who were still the subject of various forms of legal discrimination. Hence, during the same period he began taking part in the activities of the various organizations of the liberal Jewish intelligentsia and wrote a number of eloquent and widely read pieces for several major Russian-Jewish periodicals on the subject of Jewish liberation in the context of the broader revolutionary movement (Gassenschmidt 1995).

For Boas, too, the spring and summer of 1905 were difficult. An increased teaching load at Columbia University and constant disagreements with AMNH Director Bumpus and with President Jesup, the main patron of the expedition, finally led him to resign his position, on May 24, while retaining some of his salary for completing the work on the JNPE publications and several other projects (Cole 1999:242–60). This new development made Boas extremely anxious to complete the JNPE publication series as quickly as possible.

Despite these distractions, the two men quickly developed a warm relationship, with Boas frequently inviting Shternberg to his Columbia lectures and to dinners at both his city and country residences. In the course of their conversations, the two scholars reaffirmed their plans concerning Shternberg's contribution to the JNPE publication series (AAN, 282/5/64, *passim*). In fact, one of Shternberg's tasks was to select those objects from the AMNH collection that he wished to serve as illustrations for his book. Although he did study the AMNH's Amur and Sakhalin materials and discussed them with Laufer, who had brought them there, Shternberg's written comments on them are extremely brief (Roon 2000:141). Unless some of Shternberg's writings on the subject have been lost, he clearly did not have very much to say about the collection. In fact, one of his letters to his wife mentions his not having very much work to do at the museum (AAN, 282/5/64, p. 98a). Although this may have been partly because his knowledge of the material culture of the Sakhalin, and especially of the Amur River Natives, was still somewhat limited, other factors were clearly involved.

As a Jewish socialist and a journalist, Shternberg was fascinated with the United States. His letters to his wife mention his wanting to be able to see more of the country, and even his entertaining a plan of traveling throughout the United States as a correspondent for one of the liberal Russian newspapers and writing a book about the country (AAN, 282/5/64, p. 98a). During his relatively brief stay in the United States, Shternberg found time to attend meetings of various left-wing organizations (including a congress of what he called "The American Workers' Party" in Chicago, which he visited to examine the Field Museum's Siberian collection), as well as Jewish organizations (AAN, 282/5/64, pp. 98–100a). He also socialized intensely with Russian-Jewish émigrés in New York.

A few weeks after Shternberg's arrival in New York, he learned of a terrible pogrom in his hometown, Zhitomir, that had occurred on May 9–10. Even though he soon received a telegram from his parents assuring him that they were all right, it was obviously difficult for him to concentrate on museum work. After two months in the United States, he finally sailed for Europe, where he visited ethnographic museums in Switzerland and Vienna. The large collections from the Russian Far East in Vienna were of special interest to him. However, his stay in Austria was interrupted by the sad news of his mother's death, caused by the emotional suffering she had endured during the pogrom (Shternberg to Boas, 28 August, 1905, AMNH).

Political Upheaval Delays the Gilyak Manuscript

On September 21, a few weeks after his return to St. Petersburg, Shternberg received a letter from Boas inquiring about the title he intended to give his contribution to the JNPE publications (AAN, 282/2/21, p. 5). The fact that Shternberg took an entire month to respond was probably attributable to the intensification of turmoil in Russia. His response was dated October 17, the very day on which Tsar Nicholas II issued his manifesto granting limited freedoms to the country's population and promising to proceed with elections

for its first parliament (*Duma*). Despite these developments, Shternberg's letter sounded somber: "Our public affairs are going very heavily. The unrest is growing every day, the intensity of public feeling is very high, and we are on the eve of terrible things" (AMNH).

Shternberg's mood must have given Boas reason to worry about the future of the JNPE publications, especially since the work of his two other Russian contributors was also being negatively affected by their country's troubles (see Vahktin, this volume). Even Jochelson, the least politically engaged of the three, who lived abroad for long periods of time, was being distracted from his work by events back home (see Cole, this volume). As he wrote to Boas in one of his 1905 letters, "You know, of course, that next to the researcher stands in me a citizen" (AMNH, quoted in Cole 1999:236). Most troublesome of the "ethno-troika" was Bogoras. After a period of silence, which worried Boas a great deal, Bogoras wrote to Boas, on April 6, 1905. He apologized for neglecting his scholarly writing but stated that "an epoch like this happens only once in many centuries for every state and nation and we feel ourselves torn away with the current even against our will." As a European-style progressive liberal, Boas was sympathetic to his Russian colleagues' concerns and watched the unfolding events in Russia with great interest. Still, for him, science came first. As he lectured Bogoras in a letter of April 22, 1905, "If events like the present happen only once in a century, an investigation by Mr. Bogoras of the Chukchee happens only once in eternity, and I think you owe it to science to give us the results of your studies." A November 23, 1905, letter from Bogoras contained more regrets about his lack of progress but expressed the same sentiment: "my mind and soul have no free place to let in science" (all correspondence from AMNH).

The final blow came on November 27, when Bogoras was arrested because of his active involvement with the All-Russian Peasants Union, which came under government attack. He informed Boas of his misfortune in a cable, causing his friend to

contemplate appealing to both Radloff and Jesup for help in securing his release (see Boas' letter to Jochelson, 4 December 1905; Boas' telegram to Radloff, 10 December 1905, AMNH). While concerned about Bogoras' safety (see Boas to Bogoras, 10 January 1906, letter, APS), Boas was also very worried about the fate of the scientific data Bogoras had collected in Siberia. This concern prompted an official letter to Shternberg on January 22, 1906, from the new head of the AMNH, Henry Osborn,

My dear Mr. Shternberg:

You have undoubtedly heard of the arrest of Mr. Bogoras, which we learn took place in Moscow on November 29, but the details concerning which we know nothing.

I have written to The Honorable George von L. Meyer, our Minister to Russia, asking if it would not be possible for him to make an effort to secure any notes, manuscripts, etc., bearing upon the Jesup North Pacific Expedition, that may have been in Mr. Bogoras' possession at the time of his arrest, and I would say that if Mr. Meyer should call upon you, I hope that you will give him such assistance as is within your power, for I feel that it would be a distinct loss both to the Museum and to science if the ethnological records in Mr. Bogoras's possession should be destroyed. (AMNH)

Fortunately, Bogoras was out on bail two weeks later, and by the beginning of 1906 he was safe in Finland, where he resumed his scholarly work (Bogoras to Boas, 10 January 1906, APS). Happy to hear the good news, Boas cautiously suggested to Bogoras that it might be better for him "under the present conditions" to devote his time "to scientific work" (Boas to Bogoras, 24 January 1906, APS).

While his Russian colleagues were causing Boas a lot of grief, so did his AMNH superiors (Cole 1999:223–61). Throughout the spring of 1906, he shared his frustrations with both Jochelson and Shternberg. Finally, on May 24, he sent both of them similar letters explaining the new arrangement he had worked out with the AMNH's director and with Jesup concerning the remaining JNPE publications. The one sent to

Shternberg read:

Presumably I shall make a contract with Mr. Jesup for completing the Jesup Expedition publications. The only manner in which it has been possible to make this arrangement is for me to undertake the whole risk of publishing the material, to pay for contributions and for assistance. . . . Since I am to be paid after the completion of printing, it is of course essential that the contributions come in as promptly as possible, and I rely upon your assistance. In making the estimates, the best I have been able to do is to set aside for your manuscript the sum of \$1250. (AAN, 282/2/21, pp. 14–15; see also Boas to Jochelson, 24 May 1906, APS)

As far as the exact contents of Shternberg's manuscript were concerned, Boas was still in the dark, except that it was supposed to deal with "the tribes of both the Amur River and Saghalin." In his June 8, 1906, letter to Shternberg, he wrote,

Will you kindly let me know . . . the general contents of the paper that you would be willing and ready to write for the amount that I am able to offer you, and also when you will be ready to let me have the manuscript (AAN, 282/2/21, p. 16).

On August 24, 1906, Shternberg finally responded, blaming his long silence on the "political situation in Russia," which had prevented him from doing much serious work. Nonetheless, he promised to return to the Gilyak monograph and complete it in 10 or 12 months. By that time, it must have been easier for him to turn his attention back to scholarship: a month earlier the government had disbanded the First Duma, and the revolutionary movement was on the decline. This letter also contained the first of Shternberg's many requests for an advance payment, which he justified by noting that while working on the manuscript for Boas, he had to set his journalistic writing aside and, consequently, stood to lose a substantial amount of money. It is ironic that while Boas' Russian colleagues (especially Bogoras and Shternberg) often failed to deliver their work to him on time, they also depended on the money he paid them for it and often reminded him of that.

By 1907, Boas was becoming increasingly anxious about the delay in receiving the Gilyak manuscript, especially since the first part of Bogoras' Chukchi monograph had already been typeset and Jochelson's Koryak manuscript was about to go to press (Boas to Shternberg, 15 February 1907, 5 March 1908, APS; Boas to Bogoras, 16 August 1907, 4 May 1908, APS). In another letter, (27 September 1907, APS), Boas suggested that to speed up the process, Shternberg should write in Russian and Boas would arrange to have the work translated into English. We do know that in 1907 Shternberg was spending a fair amount of time working on his monograph. However, various old and new distractions, such as the political upheaval in Russia, his heavy workload at the MAE and at the recently established Russian Division of the International Committee for the Study of Central and Southern Asia, some part-time teaching, his heavy involvement in various Jewish political and cultural activities, and the need to earn money by writing popular articles, continued to interrupt his work.²¹ Except for a short essay on the *in au* cult of the Ainu for a Boas *Festschrift* (Shternberg 1906b) and an important work on Gilyak folklore (Shternberg 1908), he published little during this period. Hence, in his letters to Boas he repeatedly extended the deadline for the manuscript's completion (see Shternberg to Boas, 28 March, 10 September 1907, APS). He was also finding that the preliminary work of extracting the data from his field notebooks and rewriting it for the monograph was taking much longer than he had expected (Shternberg to Boas, 23 December 1907, APS). His letters show that he began his writing by dealing with those topics which were of most interest to him, that is, "social organization and [social] life," including kinship and marriage (Shternberg to Boas, 10 September 1907, APS).

Boas' frustration with his Russian contributors' foot-dragging is very palpable in a letter of March 12, 1908, to Jochelson:

I should like to say once more that I had to take considerable financial obligations in

order to insure the completion of the Publications of the Jesup Expedition and that I can meet these obligations only when the contributors furnish me promptly with material, for the reason that I am paid always after the completion of printed signatures. This is one of the reasons why I am constantly urging you and Mr. Bogoras and Mr. Shternberg to send me material. Otherwise I should be only too glad to be relieved of the necessity of pushing the editorial work so much that I hardly get time for anything else. (APS)

The Manuscript Begins to Come In

In mid-September 1908, on the eve of his departure for the Congress of Americanists in Vienna, Shternberg finally sent Boas the first section of the manuscript (Shternberg to Boas, 30 July, 19 September 1908, APS). Its title, "The Gilyaks and Their Neighbors," suggests that he had finally been persuaded by Boas to compose a more rounded ethnography that extended beyond the one ethnic group he knew best (Shternberg to Boas, 20 October 1908, APS). On his return to Russia, Shternberg became seriously ill and did not recover until the next spring (see Boas to Shternberg, 6 March 1909; Shternberg to Boas, 10 April 1909, APS). This was unfortunate for Shternberg but helped him proceed with the Gilyak manuscript. On October 16, Boas informed Shternberg that he had just received pages 84 through 225 (Boas to Shternberg, APS). Throughout that year, checks from the AMNH were sent to Shternberg regularly. A new problem that arose in 1909 was a cutback in AMNH funding for the JNPE publications, which forced Boas to undertake some "condensation" of the contributors' manuscripts (see letters from Boas to Bogoras and to Shternberg, 5 May 1909, APS).²²

In 1910 Shternberg's work on the manuscript was once again interrupted: an MAE-sponsored expedition to the Russian Far East took him away from his desk for about five months. Shternberg hoped the new data on the Gilyak and other indigenous inhabitants of the lower Amur River and Sakhalin Island, especially the Nanay (Gol'd), that he was planning to collect would enrich his contribution to Boas' series (Shternberg to Boas, 27 May 1910, APS). This did not really occur; because of

the limitations imposed on his work by the demands of the MAE and the limited funding, he spent only short periods of time in each Native community and was rarely able to gather information thoroughly and systematically (see his report on the expedition in AAN, 282/113; see also Shternberg 1933a).

The end of 1910 and the beginning of 1911 brought new distractions and troubles to both Bogoras and Shternberg. Bogoras, who had apparently remained out on bail since his 1905 arrest, was finally given a jail sentence and was suffering from various old ailments.²³ Responding to appeals by Bogoras and Mrs. Bogoras, Boas had the American Anthropological Association pass a resolution on October 12, 1910, requesting that the Russian minister of justice allow Bogoras to have access to all the materials he needed to continue his scholarly work and to correspond freely with his colleagues abroad, as well as his publisher (APS).²⁴ Thanks to Boas' efforts and those of several members of the Russian Academy of Sciences, Bogoras' sentence was reduced, and he was finally released in April 1911.

In the meantime, Shternberg spent much of 1911 fighting accusations, leveled against him and Radloff by one of the MAE's collectors, that they had misappropriated the museum's funds and had secretly sold part of his collection to a foreign dealer (AAN, 282/1/179-180).²⁵ Shternberg was eventually exonerated, but, being a very sensitive and emotional person, he suffered greatly during the investigation and could hardly concentrate on his work. In addition, in the early 1910s he was doing a great deal of writing for a leading Russian-language Jewish newspaper, as well as other periodicals. Finally, he played a major role in advising Semeon (Shlomo) An-sky (Rappaport), the head of the famous Jewish ethnographic expedition of 1912-15 (An-sky's letters to Shternberg, AAN, 282/2/175; Shternberg, ed. 1914), and he participated actively in the work of a special bureau that advised the Duma on Jewish affairs.

Despite these setbacks, in late 1911-early 1912 the Russian scholar returned to his Gilyak writing, and in the winter of 1912 he was able to send Boas "the

continuation of the manuscript containing the last chapters of the construction of the Gilyak marriage" (Shternberg to Boas, 29 February 1912, APS). As he admitted in the same letter, this part of the monograph was the most difficult for him to complete because it required a "great deal of comparatory [comparative] and preparatory work" and rewriting. The new section of the manuscript mailed to Boas contained an ambitious comparative chapter that placed the Gilyak system of kinship and marriage in the context of the various North Asian and North American systems. Shternberg was planning to devote the next few chapters to a discussion of Gilyak daily life and marriage customs and of the clan.

This comparative segment of the manuscript became the subject of the paper Shternberg delivered in London at the 18th Congress of Americanists in June 1912, in which he used his Siberian data to support Morgan's ideas about the "Turano-Ganowanian" kinship system (Shternberg 1912).²⁶ According to letters home, his work was well received by prominent British anthropologists Haddon and Rivers, even though by this time evolutionism was rapidly losing ground in western anthropology (AAN, 282/2/361, pp. 95–103).

While in London, Shternberg and Boas had a long discussion about his manuscript and worked out a plan for the entire publication, which was to be a rounded ethnography akin to the works of Bogoras and Jochelson, rather than Shternberg's topical monograph. Thus, in addition to the discussion of the social organization of the Gilyak, which had been pretty much completed, Shternberg promised to provide information on their natural environment, physical anthropology and demography, archaeology, history, material culture, language, folklore, art, and religion (see Shternberg to Boas, 28 February 1917, APS).

Between the end of 1912 and the beginning of World War I, there was a steady exchange of letters between Shternberg and Boas indicating that the work on the monograph and its preparation for publication were progressing steadily. In fact, Boas' letter to

Shternberg of October 26, 1912 (APS), stated that he was about to send the Gilyak manuscript to the printer but was having some difficulty with the terms used for the various levels of the Gilyak social order. To clarify matters, Boas proposed a series of English terms that to him seemed to be adequate equivalents of the Gilyak ones. On December 1, 1912, Shternberg sent Boas a response in which he accepted many of his suggestions and answered most of his queries (AMNH). Volume 8 of the Jesup Expedition series, published in 1913, carried an announcement that a monograph by Leo Sternberg, *Tribes of the Amur River*, would appear in volume 4, part 2, of the series—presumably replacing Laufer's monograph, which had been advertised in an earlier volume but never written.

Swept Up in World Events

Still, the work had not been fully completed, and that bothered Boas considerably, since the AMNH was clearly getting tired of his JNPE publication project. Shternberg, always a perfectionist, continued to tinker with his manuscript and complained about some inaccuracies in the English translation (Shternberg to Boas, 23 June 1913, APS). To make matters worse, in the spring of 1913 he had experienced another set of professional and political troubles, and he and his wife had suffered a major personal loss, the nature of which I have not been able to establish (see Boas to Shternberg, 29 April 1913, AAN, 282/2/29, p. 51). On October 2, 1913, Boas sent an exasperated letter to his Russian contributor, saying:

Last time you wrote to me you said you were going to send me your manuscript very soon. I am exceedingly anxious to get your material. If I do not finish my work by the last of December 1915, the whole matter will be at an end, and I am simply held up by you. Can you not please finish your part of the work, so that we can at least go ahead with that part that has been translated? (AAM, 282/2/29, p. 54)

On November 18, 1913, Boas acknowledged having just received the ill-fated manuscript and wrote that he was planning to send it to the printer very

soon. He begged Shternberg to read the proofs as soon they reached him. One difficulty remained, however; Boas could not print the table of contents, since he did not know exactly what Shternberg's further plans were. He also continued to press his colleague to "keep up the work, because, as I told you several times, the time is drawing very near when the work must be closed. The whole labor after I receive your manuscript—translation, revision, etc.—means a great deal and consumes much time" (APS).

Unfortunately, in 1914 it was Boas' turn to delay the publication of the Gilyak monograph. As he complained to Shternberg in an April 17 letter of that year:

The delay in printing is due to the very great pressure of work here. It so happens that so much has accumulated this winter, that, although I made a start with your material several times, it had to be put aside again. My present plan is to take it up seriously in May, and it will then go to the printer at once. I do hope that you will go right on with your writing, so that we can get the whole matter under way before my contract expires. Even after I receive your manuscript, it will still take quite a little time before we can get it published. (AAN, 282/29, p. 57)

With the onset of fighting in Europe, the work on the JNPE publications slowed even more. In a letter to Clark Wissler of the AMNH Department of Anthropology (10 October 1915, AMNH), Boas mentioned that he had in hand a "paper" by Shternberg on the Gilyak, "although the actual printing will probably have to wait until the end of the war." On September 28, 1916, he sent a similar message to Shternberg, saying that even though he now had the entire manuscript, he was "quite unable to send it to the printer. I do not receive the proofs that are sent to me from Leiden, and all printing has probably stopped" (AAN, 282/2/29, p. 62).

Both Shternberg and Boas were deeply disturbed by the war in Europe, though for somewhat different reasons. As was the case with many other moderate former populists (who either joined or at least sympathized with the Socialist Revolutionaries, or SRs) and with liberals further to the right, Shternberg, like Bogoras,

became a "defensist" (*oboronets*) patriot during the war and was very upset about Russia's losses (Melancon 1990). In addition, he was deeply troubled by the anti-Jewish propaganda and violence committed by the Russian army in those parts of the country where the fighting took place (Gassenschmidt 1995).²⁷ Boas was upset about the war because it pitted his native country against his adopted one and its allies and demonstrated how brutal the most "civilized" Europeans could become. Along with some other liberal and leftist American intellectuals, he took a pacifist position that made him quite unpopular among his more conservative colleagues (Stocking, ed. 1974: 331–5; Stocking 1992: 102–6). Boas' state of mind during this time is well captured in his September 28, 1916, letter to Shternberg: "I hope that at a later time I may write to you more fully. At present it is hardly possible to write about anything serious" (AAN, 282/2/29, p. 62).

Despite their preoccupation with the war, both scholars continued their administrative and scholarly work throughout this period, with Boas publishing his monumental *Tsimshian Myths* (Boas 1916) and Shternberg delivering several key lectures at the meetings of the Ethnography Division of the Russian Geographic Society and publishing an important essay on comparative religion (Shternberg 1916). Finally, during the war, after years of giving various small and unofficial ethnographic and museological seminars and lectures within the MAE walls, Shternberg received an opportunity to give regular lecture courses in "ethnography" (anthropology) at the "Higher Geography Courses."

The Fate of the Gilyak Manuscript after the Bolshevik Coup

It is surprising that Shternberg's letter to Boas (prior to a six-year-long silence), written during the height of the revolution of February 1917, does not mention that event. After all, like the majority of Russia's intelligentsia, he enthusiastically welcomed the overthrow of the monarchy and the establishment of the

Provisional Government, dominated by the liberals and the moderate socialists. From the time of the February Revolution until the beginning of 1918, he plunged into political activities, including those that would have been illegal before the fall of the emperor. As always, his most important political activity was journalism. He joined the staff of *Volia naroda* (People's Will), a newspaper reflecting the views of the most moderate wing of the SR party, which fully supported the policies of the Provisional Government and was highly critical of the Bolsheviks. Following the October 1917 Bolshevik coup, the *Volia naroda* office was raided several times by the government and was finally closed down in February 1918.

After that, Shternberg must have curtailed his SR activities, since he did not leave St. Petersburg (then called Petrograd) during the Civil War or go underground. Moreover, since he never placed his political involvement above his work at the MAE, he must have felt compelled to devote most of his energy to serving as its chief administrator after Radloff's death in the spring of 1918. The years between 1918 and the early 1920s were the most difficult in his life and in the lives of other Russian intellectuals. This was especially so in the capital, which was located very close to the front lines and where severe food and fuel shortages contributed to a general deterioration of economic and social life. In addition to these physical privations, Shternberg, Bogoras, and Jochelson suffered greatly from a travel and communication blockade that for several years cut them off from any contacts with their colleagues abroad and from receipt of scholarly publications (see Jochelson to Boas, 10 October 1921; Bogoras to Boas, 17 February 1923, APS).

During this period, Petrograd experienced one of the worst manifestations of Bolshevik dictatorship and Red Terror. Many of Shternberg's colleagues and friends, who tended to be affiliated with the KD and SR parties, emigrated or were arrested. Shternberg and Jochelson, who had also been involved in *Volia naroda*, themselves fell victim to this terror on February 25,

1921, when they were placed in the infamous "House of Preliminary Confinement" as part of a large-scale campaign of arrests conducted by the Bolshevik secret police in the city during the "Kronstadt Mutiny."²⁸ Fortunately for the two ethnographers, a prominent Russian writer, Maxim Gorky, intervened on behalf of some of the arrested intellectuals, and this led to their release on March 2 (AAN, 282/1/102, p. 41).

Although by the time of this brief arrest Shternberg had completely withdrawn from any anti-Soviet activity, he remained dedicated to the populist ideology of his youth and to supporting his fellow populists. In the summer of 1922, this courageous man composed an appeal to the Soviet government, which was signed by a number of veteran populists, asking the government to be lenient toward and not shed the blood of the "right-wing SRs," on trial in Moscow at the time (AAN, 282/1/102, p. 42-3; Jansen 1982).

One might ask why Shternberg, who never became an ardent supporter of the Soviet regime, did not leave Soviet Russia, as Jochelson and a number of his other colleagues did.²⁹ My guess is that his dedication to the MAE, whose de facto director he was between 1918 and 1922, was a major reason for his decision to stay. In addition, it was under the new regime that he finally was given an opportunity to establish the teaching of anthropology at the university level, first in the Ethnography Division of the Geography Institute and, beginning in the mid-1920s, in the Ethnography Department of the Geography Division of Petrograd (later, Leningrad) University. Not only did he teach a variety of courses in those institutions; he served as well as the dean of the Ethnography Division and later of the Ethnography Department. He also brought Bogoras into these institutions, and the latter became his closest ally in the work of establishing what became known as the Leningrad ethnographic school (Ratner-Shternberg 1935; Gagen-Torn 1971; Staniukovich 1971; AAN, 282/1/135 and 179).

With the death and departure of a number of prominent Russian ethnographers, Shternberg became one

of the remaining leaders of the discipline, especially in Leningrad. Thanks to his and Bogoras' tireless efforts, the new regime came to recognize the importance of ethnography as a field of knowledge with practical applications and as a major component of the higher-education curriculum and began supporting it financially (Solovei 1998). Shternberg must have understood that his departure would be a major blow to the young discipline to which he had devoted much of his life.

After Dubnov, the long-time president of the Jewish Historical-Ethnographic Society, emigrated, Shternberg took on that job in 1923 and also became the editor of the society's journal, *Evreiskaia Starina*, which he tried to make more anthropology-oriented (Shternberg 1924, 1928). It also appears that, like many other Russian intellectuals, he welcomed the degree of liberalization that occurred in the early-to-mid-1920s, when the New Economic Policy reintroduced some private enterprise, censorship eased a bit, and travel abroad again became possible. Shternberg might have been hoping that the new regime would eventually become softer and more humane. In addition, until the late 1920s old populist revolutionaries who, like him and Bogoras, remained in the country and did not oppose the regime were treated by the regime with considerable respect.

The resumption of scholarly contacts with the west in the early 1920s allowed Boas to renew his ties with Shternberg and his other Russian colleagues. In September 1921 he managed to send his first letter to Jochelson; the latter shared it with Bogoras and Shternberg (see Jochelson to Boas, 10 October 1921). As far as the "ethno-troika" was concerned, Boas had two major worries: their physical survival, and the continuation of their scholarly contributions to the various series of which he was the editor.³⁰ To help support his Russian colleagues, Boas managed to get the AMNH president to commit museum funds to remunerate them for their writing. As Boas' identical letters to Bogoras and Shternberg, dated December 9, 1921, stated,

President Osborn of the American Museum of Natural History has asked me to inquire what

material connected with your research in Siberia you have on hand for immediate publication, the amount of time needed for this work and the financial remuneration expected. When he has received this data, he will consider what plan for publication can be adopted. (APS)

Boas' primary goal was clearly to help his Russian friends, since this time he did not specify which projects he would like them to work on. Even in Shternberg's case, he did not name the Gilyak monograph but only mentioned "some subject on the Amur River tribes" (Boas to Shternberg, 17 May 1922, AAN, 282/2/29, p. 66). The remuneration proposed by Osborn was quite generous, especially for the starving Russian scholars: "\$300 to be divided into equal monthly installments for the rest of the current year from the moment that the agreement goes in effect" (AAM, 282/2/29, p. 66).³¹

While Boas was rather vague about the work Shternberg was expected to do in return for this assistance, Shternberg himself was quite specific. In his June 20, 1922, letter to Osborn, in which he accepted the museum's offer, he wrote about "preparing for you a part of my monograph on the Giljaks, *The Family and the Gens* [Clan]" (AMNH). This suggests that he was planning to continue working on his Gilyak monograph. At the same time, it appears that after some 18 years of working on this book, he was beginning to get tired of it and that new research interests were occupying his mind at that time. Thus, in February 1923, he wrote to Boas that he had recently prepared:

a ready paper on the genesis of the idea of election in primitive religion, especially in the Siberian shamanism, developing entirely new and important facts of the psychology of the shamans, from my own observations and unknown manuscripts and from my correspondents. It is written in Russian and [is] now in the process of translation. It is not exactly the subject proposed by you, but for two reasons I prefer to send it as my firstling, 1) because it concerns the religious ideas of all Siberian tribes including the North-Eastern ones, 2) I am till now uncertain about the fate of my first chapters on the Giljak; under such circumstances I am not sure if the

continuation will not have the same fate as the preceding ones. Please let me know about it. In any case I do not cease to prepare my Giljak materials in attending your answer. (21 February 1923, APS)

It is not clear why Shternberg was uncertain of the fate of the portion of the Giljak manuscript that he had delivered to Boas 10 years earlier, but for some reason he felt that it was not ready for publication. Boas did not respond to this letter for over a year, but we do know from Jochelson's letter to Shternberg, written in March 1923 from New York (AAN, 282/2/124, pp. 37-40), that he was not pleased with Shternberg's change of plans and was expecting him to "continue working on the materials for the Jesup Expedition . . . and not to send any theoretical articles to him." Jochelson also informed Shternberg that his American colleague was not going to help publish his "Divine Election" essay in an American journal. The fact that Boas was clearly losing patience with Shternberg is reflected in his May 1, 1924, letter to him:

"I wonder what you have been doing in regard to the manuscript for the Museum. There has been such a delay in publishing your Giljak material that I do not know just what to do. I should like to know particularly whether the manuscript which I have may be printed as it stands or whether you want to revise it" (AAN, 282/2/129, p. 72).

In August 1924, a reunion of Boas, Bogoras, and Shternberg took place at the 21st Congress of Americanists in the Hague and Göteborg. In addition to attending the congress, the two Russian scholars spent over two months in Europe buying books for the MAE and other Academy of Sciences institutions and libraries, reading the latest anthropological works, and conversing with foreign scholars. For both of them, this first trip abroad since the start of World War I was an exciting experience. They not only met many of their old colleagues and friends but also made important new contacts with prominent scholars from Scandinavia, England, France, Germany, the United States, and other countries. For Shternberg, the most important links of this kind were the collegiate

relationships he established with Erland Nordenskiöld, Charles Seligman, Paul Rivet, and Marcel Mauss (see AAN, 282/2/203 and 162). Conversations with them, combined with a great deal of reading, familiarized him with the new developments in western ethnology from which he had been cut off for almost a decade. Shternberg's essay (1926) reviewing these developments demonstrated that while he was enthusiastic about some of them, such as Malinowski's Trobriand work, the extensive field research by Boas' students among American Indians, and the increased attention to psychological issues demonstrated by Seligman, W. H. Rivers, and others, he remained strongly committed to evolutionism and was unhappy about the antievolutionist position most of his western colleagues had taken.

As always, Shternberg used his foreign trip not only to visit museums and learn about new work in his field but also to observe local political and social life, including left-wing and Jewish activities. His letters to his wife written on this trip (AAN, 282/2/361) indicate that, despite his and Bogoras' ongoing disagreements with Soviet government policies, they thought of themselves and were treated as the official representatives not only of the Academy of Sciences but also of their new state. While the majority of the scholars they met were courteous toward them, Shternberg felt that those with more liberal and leftist views were particularly friendly. Among them were Mauss and Rivet (both socialists) and Boas himself. Shternberg wrote home about his old American colleague,

"Boas spent most of his time with us and did it as a demonstration to others, even though he was the central figure at the congress. Our interaction with him was not only scholarly but personal and political. As far as his soc. [socialist?] views are concerned . . . they are very similar to ours; I might even say he is more radical than I am" (AAN, 282/2/361, pp. 202-3a).

The last sentence suggests that Boas, who in the 1920s became quite sympathetic toward the new Soviet regime, might have been more idealistic about life

in the USSR, which he observed from a distance, than his Russian colleagues, who experienced it first hand (see Jochelson to Shternberg, 12 March 1923, AAN, 282/2/124, p. 24). Although toward the end of the 1920s and in the 1930s, Boas became more critical of the political and ideological climate in the USSR (APS), he remained a strong advocate of Soviet-American scholarly cooperation. In the 1930s, using his ties with Bogoras, he helped several young American ethnologists go to Leningrad to study and do research at the MAE and brought one Soviet ethnography student, Juliia Averkieva, to study with him in New York and accompany him to the field (see Krupnik 1998).³²

As Shternberg wrote to Boas prior to his departure for western Europe, he was anticipating being scolded by him for taking so long to complete the Gilyak manuscript (Shternberg to Boas, 5 July 1924, APS). His expectations proved correct, as his letters to his wife and especially Boas' October 29, 1924, letter to him indicate. Since this was Boas' last detailed communication to Shternberg on the subject, it is worth quoting a large section of it here.

My dear Dr. Sternberg:

Allow me to very briefly repeat the various points that we discussed and partly agreed upon at our meeting this summer. First of all, you agreed to send me the chapter on the social organization, history, and statistics of the Gilyak, which is to be covered by the payment of \$300 that was made to you about two years ago by the Museum. I am retaining one part of your manuscript which forms part of this chapter. Furthermore you made the following proposal: to finish by August 1925 the chapter on mythology and folk-lore of the Gilyak; by March 1926 the chapter on religion and history; by August 1926 the chapter on material culture. You asked that if you were to undertake this, the sum of \$2,000 a year be paid to you for the years 1925 and 1926. Furthermore you estimated that the sum of \$500 would be required for illustrations, translations, and so on. Furthermore you were going to include material on the Gol'd and Ainu in your manuscript, which you were going to deliver in English. (APS)

It appears that by this time Boas had realized that to get his Russian friend to complete this work, he simply had to make him commit to a definite schedule. It is worth noting, however, that Boas left the door open for the possibility that, after 20 years of waiting for the Shternberg manuscript, the AMNH administration might refuse to continue paying him. As Boas put it, "I have, of course, not been in a position to make any arrangements, and it remains to be seen what I can do" (APS).³³ Shternberg's December 24, 1924, response to this letter shows that he was well aware of Boas' impatience. In it, he informed his friend that he was working on the "continuation of the social culture," which was to be "not of a small size." Clearly dissatisfied with Alexander Goldenweiser's translation of his manuscript, he was going to have it translated in Russia. He also wrote about a new obstacle he had to overcome in order to complete the Gilyak book: very poor health (APS).

That was not the only factor hindering his Gilyak work. On their return to Russia, he and Bogoras were forced to engage in a major battle with zealous Marxist education officials to save the curriculum they had developed for the Ethnology Division of the Geography Institute from the introduction of new ideologically driven courses and the reduction of fundamental academic ones (Ratner-Shternberg 1935). Although they did win a partial victory, the ideological climate in the country was clearly beginning to change, and, consequently, the higher education curriculum was becoming increasingly politicized (Solovei 1998; Konecny 1999). In addition, in 1924–25 Shternberg's work at the MAE kept him very busy. On the one hand, he had to deal with periodic confrontations between his own faction and that of the museum director, Academician Karskii (Ratner-Shternberg 1928; Reshetov 1996; AAN, 282/4/9, pp. 165–72).³⁴ On the other hand, in the mid-1920s he was able to use increased government support for the MAE to finally begin work on his pet project—the department for the "evolution and typology of culture" (Staniukovich 1978).

Consequently, by April 1925, Boas still had not received any new installments of the Gilyak manuscript (see Boas to Wissler, 5 April 1926, AMNH; Boas to Shternberg, 9 April 1925, APS). By the end of that year, he finally heard from Shternberg, who hinted at the difficulties he had to deal with “during this troublesome year.” That letter also contained a puzzling postscript saying that he had not received a copy of his manuscript and was anxious to get it. Does this suggest that Shternberg wished to see the material he had sent to Boas before World War I, to make changes in it?

By the end of summer 1926, Boas was clearly fed up with his two Russian colleagues, and on August 14 he sent them rather stern letters which seemed to suggest that he was giving them one last chance to complete their work. His letter to Bogoras said:

I have been hoping for all these many months, or years, to get the promised material from yourself and from Sternberg. I made myself responsible for it at that time to the Museum and I feel in a very awkward position because nothing comes. Can you not find your Eskimo material that you promised me and let me have it? (APS)

The one to Shternberg was similar in tone and even went so far as to remind the addressee that he had failed to repay Boas for the help offered to him in the early 1920s:

May I not hope that you will send me sometime, the material on the Amur tribes that you promised me? I am, of course, in a very awkward position because at the time when I got the Museum to help you out I undertook to promise that you would furnish a certain amount of work, so that the responsibility in a way rests with me. (APS)

Despite its severity, the letter ended on a more amicable note: “But setting aside the point, I should like, of course, very much to have the valuable information on these tribes for publication.”

Little did Boas know that this would be his last letter to Shternberg. The latter took over half a year to respond, prevented from doing so by poor health, his various duties at the university and the museum, and a

long and arduous trip to Japan to attend the Third Pacific Scientific Congress in November 1926. In his own last communication with Boas, dated September 15, 1926 (APS), Shternberg mentioned that he was about to embark on this trip and complained that he would much rather have attended the 22nd Americanist Congress, to be held in Rome during the same month, where he had hoped to see his old friend. However, he did have some good news for Boas concerning the Gilyak manuscript: “a great deal is done, it waits now only to be translated and after my return it will be finished.” The letter indicates that Shternberg felt guilty about his having received money for work that took so long to complete: “I am happy to be able . . . not only to send the Museum my work, but also to pay my debt by cash what I hope to make either from Japan or after my return.”

Unfortunately, after the Japanese trip, which included a visit to the Hokkaido Ainu, Shternberg came home exhausted and unwell. He never fully recovered, and he passed away on August 24, 1927. From his wife’s November 4, 1927, letter to Boas (APS), we learn that only a few days before his death he was still working on the Gilyak manuscript.

Unaware of Shternberg’s illness, and still frustrated with his colleague’s constant promises, Boas no longer wrote to him. Instead, he sent a letter to Bogoras, with whom he was about equally frustrated but who had always been his closest friend in the “ethno-troika”:

Jochelson tells me that you wrote to him that you hoped to send me the Eskimo material this spring. I devoutly hope that this may be the case. I believe you know how embarrassing it is to me that this matter is still hanging; both in your case and that of Mr. Sternberg. It is not the question that Mr. Sternberg has to furnish an enormous amount of material, but if he would only send a little of his Gilyak work; whatever may seem most convenient to him. (Boas to Bogoras, 25 February 1927, APS)

This letter shows that by 1927 Boas had become so frustrated with Shternberg, and so uncomfortable vis-à-vis the AMNH, that he was willing to publish *any* Gilyak manuscript of his, regardless of its content.

The Saga of the Gilyak Manuscript after Shternberg's Death

Shternberg's death was a major blow to Boas. Aside from the stop it put to his decades-long efforts to procure a substantial monograph on the peoples of the Russian Far East, it took away a dear old friend and colleague (see Boas to Bogoras, 31 October 1927, APS). At the 23rd Americanist Congress, held in September 1928, Boas memorialized Shternberg as "the leader of the Russian ethnologists" who was an outstanding specialist on the peoples of the Amur River and Sakhalin Island and whose "influence over the study of ethnology extended over the whole world." He also described him as "a dear, personal friend" whose loss Boas was "feeling keenly" (Boas 1930:xxviii-xxix).

Two years later, at the next congress, Boas offered a more detailed assessment of his friend's scholarly contributions (Boas 1934:xl-xli). Since this was a congress of Americanists, he stressed the importance of Shternberg's ethnographic research for the establishment of cultural links between America and the Old World. He also referred to him as someone who since 1900 "had been our colleague and participant in the publications of the Jesup North Pacific Expedition, whose work represented a major element of that project." As far as Shternberg's theoretical position was concerned, Boas did express his reservations about it. He stated that his colleague had observed "unique forms of group marriage and kinship system which he interpreted in terms of Morgan's theory" and that made him "one of the most zealous recent defenders of the entire Morganian scheme and the general evolutionary theory." He went on to say that "no matter what our attitude towards these theories might be, his important observations must be taken into serious consideration." He also stressed the importance of Shternberg's work on the religion of the Amur River peoples, which he had used as data for his general theory of religion. Finally, Boas praised the decedent's work at the MAE and Leningrad University and concluded that those who called him "the Russian Bastian"

were not incorrect. Closing his remarks on a personal note, he called Shternberg one of his "most modest and amiable comrades whose friendship I consider to be one of the most valuable memories of my life."

After nearly giving up on ever being able to publish his friend's manuscript, Boas must have been pleasantly surprised to receive a letter from Shternberg's widow a couple of months after his death informing him that she had found a manuscript on the "Gilyak family and clan" among her late husband's papers (see Grant 1999:xlvii-liv). Sarra (Ratner-)Shternberg asked whether she should send it to Boas and whether Boas had any unpublished parts of the same work in his possession (S. Shternberg to Boas, 4 November 1927, APS). Two weeks later, Boas replied that he was happy to learn about her discovery, did have "the first part" of Shternberg's work, and was still interested in publishing it after all those years (Boas to S. Shternberg, 19 November 1927, APS). On January 26, 1928, Ratner-Shternberg replied that she was trying to verify her late husband's transliteration of Native words with the help of the Gilyak students from the Northern Section of Leningrad's Oriental Institute but that such work took a while to complete (APS). On March 19-20, 1928, she informed Boas that she had recently mailed him the entire manuscript except for the sections dealing with Gilyak language and folklore, which she wrongly assumed to have been copies of the materials that had been sent to him long ago. Boas did receive the manuscript, but, as he informed Mrs. Shternberg, who was becoming increasingly impatient about the delays in printing it (see S. Shternberg to Boas, 25 October 1928, APS), "we are going to publish the manuscript of Professor Sternberg but conditions here are such that publication always very slow" (Boas to S. Shternberg, 17 January 1929, APS).

Having begun to doubt whether her late husband's monograph would ever be printed in the United States, Sarra Shternberg put her energy into trying to get it published in her own country. Several of her husband's students, particularly E. A. Kreinovich, who had already

undertaken extensive field research among the Sakhalin Gilyak (Kreinovich 1973) and was fluent in their language, were recruited to help in this work. Her efforts finally paid off when large portions of the Gilyak monograph appeared in two collections of Shternberg's published and unpublished works (Shternberg 1933a, 1933b; see also Grant 1999:xlvii-liv). Still, Ratner-Shternberg refused to give up on the English-language publication of her husband's monograph and recruited Averkieva, who had come back to Leningrad after studying and conducting research in the United States, to work on (re)translating it into English (see Grant 1999:xlx).

During the early 1930s, Boas, too, was still hoping to publish the Gilyak monograph. He mentioned that in his speech about Shternberg at the 1930 Americanist Congress in Germany, and he promised Ratner-Shternberg in his September 8, 1931, letter (quoted in Grant 1999:240) that her husband's manuscript was next in line after the JNPE volume dealing with physical anthropology, which had just been published (Oetteking 1930). By this time, Boas' declining health was a new factor in the slowing down of the publication process (see Boas to Bogoras, 2 June 1932, APS). Nevertheless, work continued. Even as late as 1933, an exchange of portions of the manuscript between Boas and Mrs. Shternberg was still taking place and in his last letter to her, dated March 17, 1933 (APS), Boas wrote that he had finally received it from her and was going "to try to get the printing started just as soon as possible." Three days later, he sent a similar letter to Wissler at the AMNH (20 March 1933, AMNH). The latter responded the next day, informing Boas that although the museum's budget was "somewhat disorganized at present," he had asked for an appropriation to cover the printing of the paper. By late May of that year, the entire matter seems to have been settled, and the Brill company was ready to proceed with the publication of Shternberg's work as part 2 of volume 4 in the JNPE series (Wissler to Boas, 27 April 1933; Brill to Boas, 24 May 1933; Boas to Wissler, 25 May 1933, AMNH).

Final Collapse

The manuscript's saga, however, was not to have a happy ending. Having given up on Boas, Ratner-Shternberg sent him her last two angry letters on February 2 and June 10, 1934 (APS). Between that year and 1939, no correspondence related to the Gilyak manuscript seems to have been generated (or, at least, could be found by Grant or myself). We do not know exactly what caused the delays in publishing the unfortunate manuscript, but most likely it was a combination of the financial difficulties Brill was having, due to a worldwide economic depression, and Boas' advancing old age, which prevented him from taking care of his editorial duties promptly. The latter factor seems to have been the main reason for the sad fate of the Gilyak manuscript. A June 1939 letter from the publisher to Boas sheds light on the situation: "A short time ago, I . . . received the request whether a further volume of the publication of the American Museum of Natural History might be expected. I see in the previous letters that you wrote me about 5 years ago, that you had not been able to finish the editorial work of volume IV, part 2 of the Jesup Expedition as the *Memoirs of the Museum*" (17 June 1939, AMNH). Despite this setback, the publisher was still willing to proceed with the publication and was awaiting Boas' quick response. Boas' letter to Brill, written on June 30, 1939 (AMNH), blamed the enormous delay on his having been too busy with other projects in the past few years and on Shternberg's death, which made the final editing difficult. He also mentioned the need to obtain the AMNH's consent for the continuation of the publication. My guess is that Boas had not anticipated the amount of editorial work the manuscript still required. With its author deceased and Boas' communications with Soviet ethnographers having come to an end by the mid-1930s (see Krupnik 1998:208-9), it must have been very difficult for him to deal with the various minor questions and problems, particularly terminological ones, that arose during the publication process. What Boas' letter to Brill did not mention was that,

since his retirement from Columbia University in 1937, he had become increasingly involved in writing essays for nonacademic publications on such burning issues of the day as the Nazi threat and intellectual freedom in the United States (Stocking 1992:106–10). His declining health was also slowing down this energetic and prolific scholar.

Boas' interest in the remaining unpublished manuscripts of the JNPE revived two years later, with the arrival in New York of the great Russian linguist Roman Jakobson. Boas asked Jakobson to compare the English-language version of the Shternberg manuscript with the 1933 Soviet publications on the same subject (Boas to Wissler, 31 July 1941, AMNH). Jakobson must have convinced Boas that the Russian-language publications were essentially the same as or very similar to the manuscript in his possession. Since Wissler's October 1941 letter (AMNH) had informed Boas that the museum's publication budget was at that time "hopelessly deficient," Jakobson's argument must have provided Boas with an excuse to end his four decades of efforts to try to publish the Gilyak manuscript. With World War II raging in Europe and the Pacific, nobody had the energy or the resources to commit to this matter. Boas' last letter to Wissler on the subject, written eight months before his death, sums up his thoughts:

As you remember the report by Sternberg on the Gilyak has been hanging for a long time. I have the whole manuscript ready, but owing to financial conditions of the world and the death of Dr. Sternberg, nothing could be done. I have had the Russian publications by Sternberg relating to the Gilyak investigated and I find that all the material has been published in Russian, so it seems to me there is no sense in trying to publish it now in English. . . . I think it would be best to use this translation as a book in your library. (16 April 1942, AMNH)

After 1942, the Gilyak manuscript remained in the AMNH library, where it was consulted by such luminaries of anthropology as Claude Lévi-Strauss, who referred to it as "a work of exceptional value and insight" (1969:292) and who relied heavily on it in his

discussion of "generalized exchange" systems. On several occasions, the AMNH entertained the idea of publishing it. Thus, in 1950 Harry Shapiro of the AMNH Anthropology Department sent the manuscript for review to Kroeber, who praised the work's data but not its theoretical framework. Encouraged by this review, Shapiro attempted to recruit Shimkin, a Russian-born ethnologist trained in the United States, to undertake the editing needed to get the manuscript published. Despite his interest in the project, Shimkin eventually withdrew because of lack of time (see his correspondence with Shapiro, AMNH).

The next attempt to publish the monograph was made in 1958–62 by Needham (1962, 1971) whose interest in its account of the Gilyak marriage system had been stimulated by Lévi-Strauss's work. Once again, nothing happened. But 40 years later, the saga of the Gilyak manuscript finally ended on a happy note.

Comparison of the Manuscripts

I would like to briefly compare the content of "Social Organization of the Gilyak" with that of Shternberg's 1904 Gilyak monograph. My purpose is to establish what exactly Shternberg had been able to write for Boas between their first encounter and the time of his death. I will also compare "Social Organization of the Gilyak" with the other two major Russian contributions to the JNPE publications, Bogoras' Chukchi and Jochelson's Koryak monographs.

As far as the ethnographic data is concerned, Shternberg did not add a great deal to the material that had already appeared in his 1904 monograph. Having reviewed his field notes, I have concluded that he actually did not have much to add to what he had included in that earlier work. In fact, some portions of "Social Organization of the Gilyak" repeat almost verbatim long passages from "Giliaki" (e.g., the discussion of the clan). However, from the point of view of interpreting that data and theorizing, "Social Organization" is a very different kind of work. For example, whereas the 1904 essay presents a tri-clan model of Gilyak

marriage, the monograph written for Boas describes a more complex one, consisting minimally of four clans and ideally of five (Shternberg 1999:79–83). Most important, in “Social Organization,” Shternberg examines the Gilyak system of kinship and marriage not in isolation but in the context of a number of other indigenous Siberian and even northern North American forms of social organization (Shternberg 1999:31–8). This discussion allows the author to show both the similarities and the differences between the Gilyak cases and the others. “Social Organization” also utilizes the Gilyak, as well as other Siberian data, to demonstrate the fundamental validity of Morgan’s hypothesis while pointing out its shortcomings.

Thus, unlike Bogoras and Jochelson, who worked under Boas’ close supervision and produced the detailed, comprehensive, and largely descriptive monographs that he favored, Shternberg wrote a more modern-style topical and theory-driven work. It was, in this sense, not unlike the monographs that began to appear in the 1920s and 1930s, particularly in England, where, during that time, interest in social organization tended to be stronger than in the United States. In some ways “Social Organization” reads as a much more modern work than *The Chukchee* or *The Koryak* or, for that matter, Swanton’s *Contributions to the Ethnology of the Haida* (1905). However, if we consider these monographs’ lasting significance as rich sources of important ethnographic data for subsequent generations of scholars (and the Native people themselves), the works of Bogoras, Jochelson, Swanton, and Boas himself (e.g., Boas 1909) are more reliable and valuable than Shternberg’s Gilyak monograph. Nonetheless, one cannot dismiss the entire monograph out of hand: those sections that were based on the author’s careful first-hand observations continue to be used and appreciated by scholars (e.g., Black 1973; Kreinovich 1973; Smoliak 1975; Taksami 1975; Ostrovskii 1997). We can only regret that Shternberg did not have the time to include in his monograph other Gilyak materials he had collected. For example, his portrayal of Gilyak

culture would have been much more comprehensive had he included his rich and interesting data on Gilyak religion, language, and folklore or had he tried more systematically to demonstrate the interrelationship between the Gilyak social and ideational orders, as he did in the last three chapters of “Social Organization,” dealing with the clan.³⁵

In closing I would like to sum up the main reasons for Shternberg’s inability to complete his Gilyak monograph and Boas’ failure to get it published. While much of the blame for the former must be laid on the various distractions that kept Shternberg from completing his work, his own personality also played a role. Unlike Boas, who was extremely thorough and systematic in his work and did his best to complete the research he had started, Shternberg preferred to write only on those topics that really interested him and, consequently, left a number of unfinished projects. Several of his colleagues and students (e.g., Bogoras 1928:16) pointed out that Shternberg found the research involved in the initial preparation of a lecture or a paper more interesting than completing an article, let alone a monograph, for publication. In fact, toward the end of his life he would often say that his students were going to be the ones to finish the various projects he had initiated (AAN, 282/1/136, pp. 47–50a).

Furthermore, Shternberg set very high standards for himself and refused to publish an article or a monograph that he did not consider to have been thoroughly researched and flawlessly written. That is why, I believe, he kept tinkering endlessly with “Social Organization.” In that respect, he was closer to Boas than to Bogoras, who sometimes published works which had not been thoroughly researched or thought out.

At the same time, although Shternberg was extremely dedicated to his scholarly work, he was just as passionate about his participation in the Russian and Jewish liberation movements as he was about anthropology. In this respect, he differed from Boas, who, while being a conscientious public intellectual who spoke and wrote more about many of the

burning political issues than most of his fellow anthropologists in the United States, still tended to place scholarship above political involvement (Stocking 1992). Finally, we should not forget that except for the last decade of his life, when he became the head of the Ethnography Division of the Geography Institute and later of the Ethnography Department of the Geography Division of Leningrad University, and also a member of the Academy of Sciences, Shternberg was always struggling to survive on his modest MAE salary and was forced to spend a good deal of time doing journalistic work. Had this not been the case, he would likely have left behind a more substantial body of published scholarly works.

Thus, one cannot blame Boas for failing to obtain a completed monograph from “the Russian Bastian.” On the contrary, Boas’ relentless efforts to make Shternberg and the rest of the Russian “ethno-troika” finish their various scholarly projects are worthy of admiration. I am sure that Boas did not like constantly badgering his Russian friends. In the end, instead of criticizing Boas for failing to complete the editorial work on “Social Organization of the Gilyak,” we should give him credit for encouraging Shternberg to work on the manuscript and for procuring a number of important monographs and essays from Bogoras and Jochelson. Those of us who have ever been engaged in any editorial work ourselves cannot but appreciate what Boas managed to accomplish in this area, despite the odds.

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Notes

1. The 1993 paper was recently published as Kan 2000.

2. Throughout this paper, documents from the AAN archive are cited in the following manner: 282 [fond/collection] (Shternberg archive)/1 [opis’/section]/100 [delo/file], p. 1 [list/page].

3. My research was supported by a fellowship from the National Endowment for the Humanities and by a Dartmouth College Claire Garber Goodman Grant and a Rockefeller Social Science Grant.

4. The expression “ethno-troika” was coined by Bogoras (1927:269).

5. For the main Russian-language publications detailing Shternberg’s life and scholarly contributions, see Bogoras 1927, 1928, 1930; Krol’ 1929, 1944; Ratner-Shternberg 1928, 1935; Ol’denburg and Samoilovich 1930; Gagen-Torn 1971, 1975; Staniukovich 1971, 1986. Sarra Ratner-Shternberg’s unpublished biography of her late husband (AAN, 282/4/9) is the most detailed source of information on this subject.

6. See Shternberg’s extensive correspondence with Krol’ (AAN, 282/2/363). Krol’ was himself arrested, in 1887, and in 1890 he was exiled for five years to Irkutsk Province, where he conducted ethnographic research among the Buryat (Krol’ 1944).

7. Although the standard modern term for this ethnic group is Nivkh (pl. Nivkhi), I use the pre-revolutionary “Gilyak,” which was used by Shternberg himself and was retained by Grant in the title of Shternberg’s monograph (Shternberg 1999).

8. Shternberg’s evolving views on the relationship between language and the “inner” or “psychological” aspects of a people’s culture and the need for the ethnographer to use the local Native language in field research may be compared with those developed by Boas at about the same time (see Stocking, ed. 1974).

9. In the summers of 1892, 1893, and 1894 Shternberg visited the various Native settlements on Sakhalin, and in the summers of 1895 and 1896 he conducted ethnographic work along the Amur River (see Shternberg 1900:387–8). During the rest of the year he made only occasional brief visits to the nearby Gilyak settlements.

10. The limitations of Shternberg’s ethnogra-

phy did not stem from any lack of rapport with the Natives. Most of them—especially those who eventually became his “key informants” and friends—trusted and liked the kind man who they considered to be a “big Russian official” and to whom they brought their complaints against the local administration and even turned for assistance in settling their internal disputes (Shternberg’s 1891 diary, AAN, 282/1/3, p. 100). Students of his who, 30 years later, worked in some of the same places that he had visited reported that many of the older people still remembered him fondly (Kreinovich 1973).

11. One should keep in mind that on Sakhalin, Shternberg had no access to Gilyak dictionaries or to linguistic studies of that language (Shternberg 1900:389). Most scholars of the Gilyak language agree that although he never became fluent, his command of the language was good and his analysis of its structure quite adequate, especially considering the fact that he was a true pioneer in this field (Kreinovich’s 1968 manuscript, AAN, 282/1/205; Ekaterina Gruzdeva, personal communication, 2000).

12. Although for obvious ideological reasons Soviet scholars asserted that Shternberg had read Engels’ book before his exile (Grant 1999:xxiv), I found no evidence of that. According to Shternberg’s letters to Krol’, he was reading the book on Sakhalin in 1889. Two years later, he asked his friend to send him a copy of “Morgan’s book,” which I assume was *Ancient Society* (Morgan 1877) (AAN, 282/2/157, p. 61).

13. Although Shternberg was well aware of the impact—much of which he characterized as negative—of Chinese, Japanese, and Russian cultures on Gilyak culture, he chose not to concentrate on this topic in his ethnographic writing. In fact, his discourse on this issue sounds very Boasian, as, for example, in the following passage:

Despite a long period of submission to the Manchurians and a destructive influence of the vagabond [Russian] population of the Amur region, the Gilyak moral order has retained many virtues of the primitive/prehistoric [*pervobytnyi*] peoples. However, their way of life is totally doomed. In one or maximum two generations, the Gilyaks of the mainland will become completely Russified

and along with the benefits of civilization [*kul'tura*] they will also acquire all of its vices. (Shternberg 1893: 19)

14. Shternberg used his Gilyak and Orochi data to defend Morgan and Lubbock against attacks by such scholars as Starcke and Kautsky.

15. Later ethnographers, particularly Smoliak (1975), who combined extensive ethnographic research among the Gilyak and other Native peoples of the lower Amur River region with systematic archival research, argued that Gilyak intermarriage with other indigenous and exogenous ethnic groups influenced the character of many of their settlements, making close adherence to the marriage rules described by Shternberg very difficult (see Taksami 1975).

16. See Shternberg’s description of the Gilyak clan as being “a striking combination of collective solidarity and individual freedom” (1933a:59).

17. Despite its focus on social organization, the 1904 monograph gives considerable attention to religion. This is a major difference between it and the 1893 piece. With over 30 pages devoted to the discussion of this topic, Shternberg demonstrates his considerable knowledge of Gilyak beliefs and, to a somewhat lesser extent, religious practices. Although he uses evolutionist terminology (especially Tylor’s), Shternberg no longer characterizes the Gilyak religion as very primitive, demonstrating that his evolutionism was far from consistent (see Shternberg 1933a:51).

18. See Freed et al. 1988; Kuz’mina 1994; Krupnik and Vakhtin 1997; Krupnik 1998; Cole 1999:185–260, as well as Vakhtin’s and Cole’s contributions to this volume.

19. See Pilsudskii’s November 4, 1898, letter to Shternberg, describing Laufer’s field research on Sakhalin (Latyshev 1996:161–2); Laufer’s May 10, 1899, letter to Boas (AMNH); and Boas’ report on the JNPE (1903:93–8), which includes Laufer’s account of his adventures on Sakhalin.

20. Laufer also published a 30-page essay of miscellaneous ethnographic data (Laufer 1900).

21. In 1907 Shternberg became one of the founders and chief ideologues of the Jewish People’s Group (*Evreiskaia narodnaia gruppy*), and in 1908 he got actively involved in the work of the newly established Jewish Historical and Ethnographic Society (Gassenschmidt 1995).

22. Boas was planning to publish the “overflow” JNPE materials in a new Columbia University series (see Boas to Bogoras, 22 May 1909, APS).

23. See, for example, Bogoras’ letters to Boas sent between October 7, 1910, and April 6, 1911, and Boas’ letters to Bogoras, dated October 12, 1910 (APS).

24. Boas was also responsible for the AMNH’s director’s appeal to the same minister on behalf of Bogoras (see Osborn to Shcheglovitov, 28 February 1911, AMNH).

25. To make matters worse, these accusations had an anti-Semitic tone that prompted Shternberg to refer to the entire case in a letter to Boas as “The Dreifuss Affair” (Shternberg to Boas, 12 March 1911, APS).

26. Shternberg’s presentation at the congress actually mentioned that his monograph “The Gilyak and Their Neighbors” was “about to appear in the publications of the Jesup North Pacific Expedition” (Shternberg 1912:319).

27. In the summer of 1915 Shternberg learned about these army activities first-hand when he was sent by the Committee for Assisting the Jewish Refugees to the front lines to investigate them (see AAN, 282/2/176).

28. The Kronstadt Mutiny was an uprising by the left-leaning, but anti-Bolshevik, sailors at a naval base near Petrograd. Although most of the former SRs arrested during the unrest had nothing to do with the rebellion, the government used the mutiny as an excuse to isolate and terrorize those members of the city’s intelligentsia who were not sympathetic to the regime.

29. In fall 1921, when communication between Boas and his Russian colleagues was restored, Jochelson began his efforts to leave Russia. Like some of the other Russian intellectuals who had chosen a “wait and see” attitude toward the Soviet regime, he described his plan not as emigration but as an extended “business trip” to the United States on behalf of the Academy of Sciences, for the purpose of “describing comparatively some of the anthropological and ethnographical specimens of the American Museum of Natural History collected by . . . Jesup North Pacific Expedition” (Jochelson to the U.S. ambassador in Berlin, 21 November 1921, APS). On

Jochelson’s request, Boas helped him obtain a visa for Germany, which he had to pass through on his way to the United States (Jochelson to Boas, 23 November 1921, APS).

30. In 1917–18 Boas managed to publish two important works by Bogoras that had been sent to him a few years earlier: *Koryak Texts* (1917) and *Tales of Yukaghir, Lamut, and Russianized Natives of Eastern Siberia* (1918).

31. Since the United States did not have diplomatic relations with Russia at that time, there was no way for the AMNH to send money to Russia. As a solution to the problem, Boas proposed to send food packages to Bogoras and Shternberg (see Boas to Shternberg, 19 July 1922, AAN, 282/2/29, p. 70). In addition to food and money, Boas arranged for American institutions, such as the Smithsonian Institution, to send scholarly books and periodicals to Shternberg and other employees of the Academy of Sciences (Jochelson to Shternberg, 20 March 1923, AAN, 282/2/124, pp. 37–9a).

32. Several of Boas’ letters to Bogoras, written in the second half of the 1920s, indicate that he himself almost made a trip to Russia (APS).

33. The same letter indicates that Boas was also trying to get Shternberg to write a summary entry on the Gilyak language for some sort of a volume on Eastern Siberian languages, which Boas was going to edit. Unlike Shternberg’s work on the Gilyak manuscript, this essay was to be provided free of charge (APS).

34. To Shternberg’s disappointment, after Radloff’s death he could not be appointed director of the MAE because the position had always been occupied by a member of the Academy of Sciences (Reshetov 1995, 1996). Shternberg was finally made a corresponding member of the Academy of Sciences in 1924, but he was either too busy or too unpopular with some of the MAE’s staff members to be made director.

35. Shternberg’s 1908 monograph does not contain any of the material on Gilyak folklore that he had collected himself or had received from Pilsudskii, his friend and fellow ethnographer of the Sakhalin Natives (Latyshev 1996). These valuable data are still in his archive and have only recently begun to be published and used by scholars (Ostrovskii 1997).

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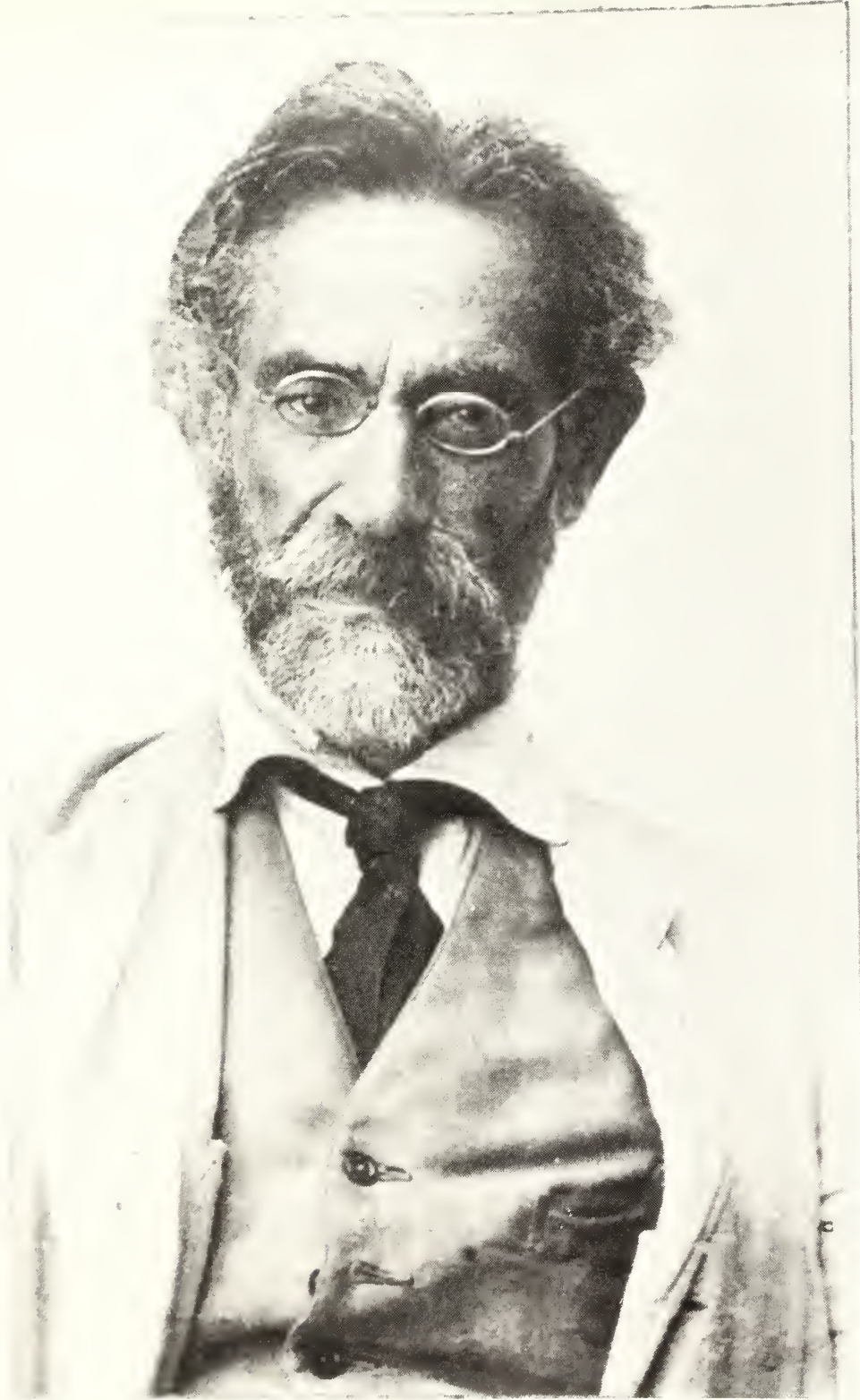
62/ Lev Shternberg conducting a census among the Sakhalin Island Nivkh (Gilyak), ca.1895 (AAN/f. 282/o.1/d. 162/l.118)



63/ Staff of the Peter the Great Museum of Anthropology and Ethnography (MAE) in St. Petersburg, Russia, 1914. Lev Shternberg (first row, fifth from left); MAE director Vasily Radloff (first row, sixth from left); Sarra Ratner-Shternberg (first row, third from left); Waldemar Jochelson (first row, first from right). Reprinted from Staniukovich 1978:137.



64/ Lev Shternberg and Sarra Ratner-Shternberg, ca. 1915
(AAN f. 282/o.1/d.194/l.12)



65/ Portrait of Lev Shternberg taken just prior to his departure for the 1924 International Congress of Americanists (AANf. 282/o.1/d. 194/l.22)

No. 398

MALE.

1. Place of observation. *New Prutz Lake*
2. Date of observation. *July 11-97*
3. Name of individual recorded. *Charlie*
4. Age. *Estimated. 55*
5. Tribe. *Cherokee*
6. Place of birth.
7. Tribe of father.
8. Tribe of mother.
9. Father of No. *396*
 Son of No.
 Brother of No.
10. Mode of life.
11. Beard; color.
12. Beard on upper part of cheeks: full, medium, scanty; short, long, none.
 Beard on lower part of cheeks: full, medium, scanty; short, long, none.
 Beard on chin; full, medium, scanty; short, long, none.
13. Mustache; full, medium, scanty; short, long, none.
14. Hair: black, brown, light brown, blonde, golden, red, gray.
15. Hair: straight, wavy, curly, frizzly.
16. Eyes: black, dark brown, light brown, gray, blue. *well inflamed*
17. Eyes: 1. 2. 3. 4. 5.
18. Nose: form of line drawn between eyes: high, medium, low.
19. Outline of union of forehead and nose: 1. 2. 3. 4.
20. Profile of nose: 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12. 13. 14.
21. Point of nose: short, long; thin, thick.
22. Nostrils: 1. 2. 3. 4. 5. 6.
23. Upper lip: projecting, slightly inclined forward, vertical.
 Nose and lip parallel, converging upward, converging downward.
24. Lips: thin, medium, thick.
25. Ear: round, pointed.
 Standing off, close to head.
26. First section of helix: rolled inward, flat, rolled back; thick, thin.
27. Second section of helix: rolled inward, flat, rolled back; thick, thin.
28. Antihelix: flat, high; wide, narrow.
29. Crura: ridges flat, high.
30. Lobe: large, small; attached, detached; round, triangular, square, divided.
31. Color of skin: covered parts
 uncovered parts
 palms of hands-

cheek bones not high

short

[Repeat number from other side.]

No. 398

L. 7.

MEASUREMENTS.

MALE.

- | | |
|--|--------------------------|
| 1. Height standing. 1635 | 7. Length of head. 184 |
| 2. Height of shoulder. 1353 | 8. Breadth of head. 157 |
| 3. Height of point of second finger. 622 | 9. Height of face. 125 |
| 4. Fingerreach. 1695 | 10. Breadth of face. 145 |
| 5. Height sitting. 845 | 11. Height of nose. 57 |
| 6. Width of shoulders. 323 | 12. Breadth of nose. 40 |

[No attention to be paid to lines below this rule.]

INDICES.

- | | |
|------------------------|--------------------|
| 1. Arm. | 1. Length—breadth. |
| 2. Fingerreach. | 2. Length—height. |
| 3. Height sitting. | 3. Face. |
| 4. Width of shoulders. | 4. Nose. |

[This record when filed to be returned to FRANZ BOAS, Worcester, Mass.]

[Number each record in order used, and write your name after number.]

Jochelson
K 112

No. 223

MALE.

1. Place of observation. *Kual*
2. Date of observation. *2/15 Apr. 1901*
3. Name of individual recorded. *gehijlhiyt*
4. Age Estimated. *20*
5. Tribe. *Koräk*
6. Place of birth. *Mäuätscha*
7. Tribe of father. *Koräk von Koraga*
8. Tribe of mother. *Koräk von Jernotschek*
9. Father of No.
Son of No.
Brother of No.
10. Mode of Life. *Fischer*
11. Beard; color.
12. Beard on upper part of cheeks: full, medium, scanty; short, long, none.
Beard on lower part of cheeks: full, medium, scanty; short, long, none.
Beard on chin: full, medium, scanty; Short, long, none.
13. Mustache: full, medium, scanty; short, long, none.
14. Hair: black, brown, light brown, blonde, golden, red, gray.
15. Hair: straight, wavy, curly, frizzly.
16. Eyes: black, dark brown, light brown, gray, blue.
17. Color of Skin: covered parts. *dunkel braun*
uncovered parts. *rötlich braun*
palms of hands. *hellbraun*

AMERICAN MUSEUM OF NATURAL HISTORY.

68/ Front side of the JNPE Siberian anthropometric data sheet, filled in by Jochelson, 1901 (AMNH)

[Repeat number from other side.]

No. 223

MEASUREMENTS.

MALE.

- | | | | |
|--------------------------------------|--------|---|----------------------------------|
| 1. Height standing. | 164.2 | 13. Breadth of face. | 15.7 |
| 2. Height of shoulder. | 132.9 | 14. Height of ear. | 150.4 (13.8) ^{index} 73 |
| 3. Height of point of second finger. | 119.6 | 15. Height of face. | |
| 4. Finger-reach. | 174.9 | I.—Hair-line—chin. | 20.8 |
| 5. Height sitting. | 84.1 | 16. Height of face. | |
| 6. Width of shoulders. | 41.5 | II.—Nasion—chin. I. | 12.1 |
| 7. Breadth of right hand. | 9.6 | 2. | 13.7 |
| 8. Length of second finger. | 10.3 | 17. Height of nose. I. | 6.1 |
| 9. Length of forearm. | { 26.2 | 2. | 6.9 |
| 10. Length of foot. | 24.3 | 18. Breadth of nose. | 4.1 |
| 11. Length of head. | 19. | 19. Length of right ear. | 6.9 |
| 12. Breadth of head. | 15.4 | 20. Distance between inner corners of eyes. | 3.9 |
| | | 21. Distance between outer corners of eyes. | 9.9 |
| | | 22. Vertical circumference. | 37.5 |
| | | 23. Horizontal circumference. | 57.0 |
| | | 24. Sagittal circumference. | 32.1 |

length of arm 93.3
 " rft. Hand 20.1
 " " U.A. 27.0.

INDICES.

- | | | | |
|------------------------|--------|--------------------|---------------------|
| 1. Arm. | 44.64 | 1. Length—breadth. | 81.5 |
| 2. Finger-reach. | 106.52 | 2. Face. | 85 97.07 |
| 3. Height sitting. | 51.22 | 3. Nose. | 67.21 |
| 4. Width of shoulders. | 25.29 | C.F. | 101.95 |
| F.A. | 15.96 | | |
| U.A. | 16.44 | | |
| HAND | 12.24 | | |



70/ Use of the Edison phonograph for sound recording, Mariinsky Post, Siberia, 1900 (AMNH)

500 Year Old Questions, 100 Year Old Data, Brand New Computers

Biological Data from the Jesup Expedition

STEPHEN OUSLEY
AND RICHARD JANTZ

In the early 1890s, the prominent anthropologist Daniel Garrison Brinton forcefully and repeatedly claimed that American Indian culture and morphology arose in the New World after a migration over a land bridge from Europe (Brinton 1890:38–41, 1891:17–32; 1894). He further stated, “it is time to dismiss as trivial all attempts to connect the American race genealogically with any other, or to trace the typical culture of this continent to the historic forms of the Old World” (Brinton 1890:18).

In many ways, the entire Jesup North Pacific Expedition (JNPE) could be thought of as a direct challenge to Brinton’s ideas. Franz Boas’ overarching goal for the JNPE was to prove the connections across the North Pacific and the superiority of conclusions based on fieldwork and induction rather than the “armchair” deductive approach of Brinton and other contemporary scientists (Ousley 2000).¹ For Boas, “the study of the physical types of the coast of the North Pacific Ocean must form one of the most important subjects of investigation of the Jesup Expedition” (Boas 1897b:537).

While collecting ethnographies, linguistic data, and items of material culture, members of the Jesup Expedition gathered skulls from graves and abandoned villages, made plaster facial casts, and collected anthropometrics (head, face, and body measurements) and morphological observations from over 2,000 Siberian and Northwest Coast Natives on data sheets. The ease of data collection and the sheer numbers made anthropometrics the best biological data available to Boas for assessing population relationships,

and by extension, population histories. He recognized that JNPE data could add to the large database of North American Indian measurements already collected under his direction (Boas 1903).

Boas acknowledged that biological data might not lead to the same conclusion regarding the relationships between these groups as data from ethnology and language (Boas 1899b). Nevertheless, he maintained that anthropometric results supported his conclusions from the extensive ethnographic data collected, which suggested that people from North America had recrossed the land bridge to Siberia (Ousley 2000). This theory for the peopling of the North Pacific and the New World through migrations not only eastward from Siberia but also westward from America became known as the “Americanoid” theory.

The anthropometric data from the JNPE and from many other American Indian groups were recently rediscovered at the American Museum of Natural History (AMNH) in New York. These have been inventoried and computerized and now constitute the most comprehensive database of American Indian and Siberian biological information available (Jantz 1995; Jantz et al. 1992). A modern statistical analysis of the anthropometric data refutes the biological basis of the Americanoid theory.

Franz Boas and Anthropometrics

Boas valued anthropometrics highly, having overseen large-scale collection of anthropometric data for the

British Association for the Advancement of Science (BAAS) and for the World's Columbian Exposition (Boas 1891a, 1895b, 1899c). Whereas museums housed skulls that could be measured at any time, anthropometrics salvaged information from rapidly disappearing peoples (Boas 1891a). While at the AMNH, Boas also oversaw the collection of anthropometric data in Labrador, Ontario, Colombia, and nine U.S. states.

Boas was ahead of his time in believing that measurements were superior to descriptions of physical types. The differences between peoples could be assessed much more reliably if the data were recorded "in exact terms," using numbers rather than subjective categories such as describing the breadth of a person's nose as narrow, medium, or wide (Boas 1894a:313, 1896). Boas believed that groups living close to each other were often too similar to be compared using only observational data. Most contemporary anthropologists of the day, such as Brinton, believed that the cephalic index (head breadth divided by head length) was the only numerical information necessary for parsing humanity into races and types (Brinton 1890).

For Boas, merely using a few measurements or one index was not enough. More measurements ensured more reliable classification (Boas 1899a). A moderate number of measurements from many members of a population was more valuable than many measurements from a few "representative" members of a population (Boas 1894a, 1895b, 1899b, 1912a). Boas was also one of the first to see the potential of measurements in studies of human growth and to apply correlations and other statistics to human biological data (Boas 1892, 1894b, 1895a, 1896, 1897a; Jantz 1995).

By the beginning of the JNPE, however, Boas reached a turning point in his career as a physical anthropologist. He briefly adhered to the contemporary physical-anthropological principle that human "types"—also called characteristic phenotypes, varieties of mankind, or races—were mostly fixed. Admixture between two different human types (as defined by different means for craniometric or anthropometric measurements) was

thought to consistently produce intermediate values; thus, virtually all subpopulations were explained as mixtures of larger populations or races. Metric information, continuous in nature, was to be used to parcel populations more objectively into discrete categories or types (Boas 1899b)

In one of Boas' earliest analyses of anthropometric measurements from the Northwest Coast tribes (and his last using measurement means alone), he remarked on the great number of types (Boas 1891a). Just two months later, in a review of the work of another anthropologist, he published a very different view of the anthropometric results of interactions between populations, based on his own data (Boas 1891b). "Mixed" populations did not show "blending" effects but, instead, tended to show a bimodal distribution of some variables, reflecting elements of both parental types. Boas argued that mixed individuals may show a measurement near the mean of one parent population and another measurement near the mean of the other parent population. Vastly different types could be found within one family. These results were discernible only when one analyzed the distribution of values in a mixed population rather than just the mean values.

In 1895 Boas analyzed massive amounts of data from over 60 North American tribes and summarized data on stature, head length, and head breadth using plots of over 80 measurement distributions. By now, his sample sizes had increased enough for a more thorough investigation of the mixing of types. For face breadth, Boas found evidence for a bimodal distribution in white-admixed individuals—now referred to as a major gene effect in quantitative genetics and recently confirmed by population studies in Nepal (Williams-Blangero and Blangero 1989). Boas also found that the effect did not hold true for all variables; some showed apparent blending or other unpredictable phenomena (Boas 1893, 1895b).

Thus, by 1895 Boas had rejected the assumptions underlying the use of anthropometric data for estimating population relationships. Anthropometrics would

still be the focus of his scientific investigations, but more for empirically testing physical anthropology's assumptions than for inductive investigations of population relationships (Stocking 1968). He continued to collect and publish descriptive summaries of Northwest Coast anthropometric data until 1899, as part of his obligation to the BAAS. In an obituary he wrote on his early mentor Rudolf Virchow, Boas revealed his future course, based on his training as a scientist and his reliance on

the general scientific principle that it is dangerous to classify data that are imperfectly known under the point of view of general theories, and that the sound progress of science requires us to be clear at every moment, what elements in the system of science are hypothetical and what are the limits of that knowledge which is obtained by exact observation. (Boas 1902a:443)

Nearly all of Boas' later work in physical anthropology consisted of empirical tests of the effects of admixture and the environment on anthropometrics using data from families. Boas' interests moved from classification and description to the dynamic causes of human variation (Herskovits 1943) and from studies of variation among populations to studies of variation within subpopulations, groups, or families (Howells 1959). Boas collected family-based samples from West Indian Natives, Spaniards, and Mestizos in Puerto Rico to investigate empirically the effects of mixing populations (Boas 1920). His analyses of data collected from European immigrants to the New World led him to the widely contested conclusion that environmental factors could greatly affect supposedly stable types (Boas 1912a, 1916; Stocking 1968). Boas discovered that the American-born children of recent immigrants showed changes in several head and face measurements and that the longer the children had been in the United States, the greater the effect. In other words, Boas had very strong evidence that human races did not have definite and unchanging traits. This finding called into question the very definitions of biological races and their relationships to each other: A person's

measurements and type could be the result of different environmental and biological factors (Boas 1912a, 1913, 1916). Although Boas had clear evidence that anthropometrics did not always reflect the genetic history of populations, as other anthropologists had assumed, he still believed in the value of anthropometrics:

It seems to me . . . that our investigations, like many other previous ones, have merely demonstrated that results of great value can be obtained by anthropometrical studies, and that the anthropometric method is a most important means of elucidating the early history of mankind and the effect of social and geographical environment upon man. . . . Every result obtained by the use of anthropometric methods should strengthen our confidence in the possibility of putting them to good use for the advancement of anthropological science. (Boas 1912a:562)

Boas also recognized how high correlations (the close relationship of measurements to each other) can confound attempts to distinguish real differences between peoples. For example, taller people generally tend to have wider shoulders and larger heads. In univariate (one variable at a time) analyses, these correlations would not be obvious, and a comparison of mean values between two populations would make the differences appear far greater merely because of size differences. (In multivariate analyses, unavailable in Boas' time, all variables are analyzed simultaneously, and correlations are taken into account, allowing the researcher to investigate differences in both size and shape.) Boas did have hope for future analyses, however. After briefly reviewing these problems and alluding to a need for more comprehensive statistical procedures, he went so far as to write:

I have tried to point out in these remarks a few directions in which it would seem that our anthropometrical material may be made more useful and significant than it is at the present time. . . . I am fully aware of the difficulties and of the vast amount of labor involved in carrying out any of the suggestions here outlined, but I fully believe that any labor devoted to this matter will be repaid by results interesting from a scientific point of view . . . and I hope that our

deliberations may lead to a way of making the vast amount of anthropometric work that we are doing more useful in scientific and practical lines. (Boas 1902b:180)

If the multivariate statistical procedures and electronic computers that enable quantitative genetic analyses had been available during Boas' lifetime, he might have returned to assess American Indian population relationships using anthropometrics.² Only very recently, however, have the quantitative genetics of anthropometrics been revalidated, justifying Boas' initial faith in anthropometric data. Using 12 anthropometric measurements from Boas' data for American Indian families, Konigsberg and Ousley (1995) showed that the phenotypic distances among family members are proportional to their genetic distances. By extension, anthropometric data can be expected to reflect larger-scale genetic relationships among populations, minimally in the same general environment.

Description of Materials

When Boas resigned from the AMNH in 1905 to teach at Columbia University, he took the American Indian anthropometric data sheets with him. They were kept in his office until 1942, when, shortly before his death, he wrote to Harry Shapiro at the AMNH and asked him to take them (Boas to Shapiro, 16 September 1942, AMNH-DA). Thereafter, these data sheets remained untouched at the AMNH for over 40 years. The neglect of the anthropometric data was lamented by Stewart (1973), alerting one of us (Jantz) to their existence. A letter of inquiry to David Thomas, then chairman of the Department of Anthropology at the AMNH, revealed that not only were the JNPE anthropometric sheets there, but so too were nearly all of the other anthropometric data collected for Boas between 1890 and 1911. These records were loaned to the University of Tennessee in 1984. All data from the sheets except the nonmetric



71/ Location of the Groups Measured during the Jesup Expedition, 1897-1902.

The JNPE groups in the analysis are (1) Bella Coola; (2) Carrier; (3) Chilcotin; (4) Chuvan; (5) Maritime Chukchi; (6) Reindeer Chukchi; (7) Siberian Eskimo; (8) Even; (9) Evenk; (10) Haida; (11) Koryak; (12) Reindeer Koryak; (13) Kwakiutl; (14) Lillooet; (15) Makah; (16) Okanagan; (17) Quileute; (18) Tahltan; (19) Tenino; (20) Thompson; (21) Tsimshian; (22) Yukagir. Groups measured at other times are (23) Nivkh; (24) Aleut; (25) MacKenzie Delta Eskimo. Not shown: Labrador Eskimo.

Table 1
Anthropometric Measurements in the Boas Database

Body measurements	Head measurements
Standing height	Head length (maximum)
Shoulder height (acromial height)	Head breadth (maximum)
Sitting height	Face breadth (bizygomatic breadth)
Finger reach (span of arms)	Nose height (nasion–base of nose)
Finger height (height at end of second finger)	Nose breadth (maximum)
Shoulder breadth (biacromial breadth)	Face height (nasion-menton)

observations were entered into a computer database between 1987 and 1990, a task that required roughly 4,000 man-hours (see Jantz 1995 for details).

The anthropometric data sheets, which chronicle the field movements of the various JNPE teams, include the observer, observation place, and date. The demographic information from measured individuals includes tribe, age, sex, occupation, birthplace, tribes of the mother and father, and number of children; many of the children were also measured. Admixture in a subject can be quantified thanks to the meticulous recording of the tribe or admixture of each parent. Anthroposcopies were collected, including hair color, form, and distribution; presence of beard and/or mustache; form of eyes, nose, lips, and ears; and skin color based on color charts. The anthropometric data sheet used by Boas in North America listed demographic information and anthroposcopies on the front (fig. 66) and anthropometrics on the back (fig. 67). Figures 68 and 69 show a Siberian sheet, which has fewer observational data and more anthropometric measurements than the North American sheets.

Twelve basic measurements, chosen because they did not require removal of clothes, were collected from over 18,000 Amerindians and Siberians between 1890 and 1912. Measurements were recorded to the nearest millimeter. Six body and six face measurements (Table 1) were common to all data sheets and have been entered into the database.

JNPE anthropometric data collection began in late May 1897 in southern British Columbia, where Boas personally measured 79 percent (458 out of a total of 582) of the subjects measured in North America. Boas

focused JNPE North American data collection on the southern Northwest Coast to supplement his earlier data from those areas. Two other JNPE team members collected measurements on the Northwest Coast through 1898. Table 2 shows the number of North American individuals measured during the JNPE, by sex and age group. The locations of all groups measured are shown in Figure 71.

Table 2
North American Populations Measured for JNPE

Tribe	Sex	Children	Adults
Carrier	M	0	3
Chilcotin	M	12	27
	F	5	21
Haida	M	3	6
	F	2	4
Hoh	M	0	1
Klamath	M	3	0
	F	6	0
Kwakiutl	M	2	20
	F	2	11
Lillooet	M	16	66
	F	10	52
Okanagan	M	3	0
	F	1	0
Queets	M	0	3
	F	0	2
Quileute	M	0	24
	F	0	6
Quinault	M	0	6
	F	0	2
Shuswap	M	45	60
	F	51	36
Stalo	M	0	6
Tahltan	M	3	21
	F	7	6
Thompson	M	1	1
	F	1	7
Tsimshian	M	0	15
	F	0	3
Total		173	409

Total Amerindians measured for JNPE is 582.

Table 3

Native Siberian Populations Measured during the JNPE

Tribe	Sex	Children	Adults	Locations
Maritime Chukchi	M	15	48	Mariinsky Post, Chechen
	F	7	24	
Reindeer Chukchi	M	12	97	Mariinsky Post, Yeropol
	F	3	25	
Chuvantsy	M	5	38	Yeropol, Markovo
	F	7	9	
Siberian Eskimo	M	43	62	Indian Point (Mys Chaplino), Chechen
	F	25	78	
Even	M	0	20	Yeropol, Markovo, Nelemnoye
	F	1	17	
Reindeer Even	M	1	15	Yeropol, Markovo, Kamenskoye
	F	0	1	
Evenk	M	16	64	Nayakhan, Gizhiga
	F ^a	13	73	
Tundra Evenk	M	0	21	Maniakhtakh
	F	1	3	
Kamchadal	M	23	87	Area of Khayryuzovo, Sedanka, and Napana
	F	26	77	
Maritime Koryak	M	84	193	Penzhina Bay, Northern Kamchatka Peninsula
	F	54	146	
Reindeer Koryak	M	2	24	Kuel, Kamenskoye
	F	0	6	
Russians	M	0	6	Mariinsky Post, Tigil
	F	1	4	
Yakut	M	0	4	Yakutsk, Verkhne Kolymsk
	F ^b	12	55	
Yukagir	M	4	34	Nelemnoye, Omolon, Maniakhtakh
	F	7	21	
Total		362	1,252	

Note: Total number of JNPE Siberians measured: 1,614.

a. Includes 30 Evenk females published in Jochelson-Brodsky 1906.

b. Includes 65 Yakut females published in Jochelson-Brodsky 1906.

On the Siberian side of the North Pacific, Berthold Laufer began work in 1898 along the Amur River and on Sakhalin Island and continued through 1899. Data collection among more northerly Siberian groups began in the late summer of 1900, with Waldemar Bogoras leading one team and Waldemar Jochelson another. JNPE members in Siberia reported collecting data from almost 1,900 subjects (Boas 1903), and we have recovered data sheets from 1,614. About 150 records seem to be missing from the Jochelson and Bogoras teams, but there are large samples for most groups. Alexander Axelrod apparently collected the bulk of anthropometric data in Siberia, measuring over 1,150 people while traveling with Jochelson's and Bogoras'

teams. Table 3 shows the Siberian groups measured and the most common locations. Most of the Koryak are from the region surrounding Penzhina Bay, but there are also 46 from Palana and Karaga—villages on the northern Kamchatka Peninsula. Data from 65 Yakut [Sakha] and 30 Evenk females published by Jochelson-Brodsky (1906) were not recovered from the AMNH but have been added to the database and are noted in Table 3.

The location of Laufer's data sheets—if he actually did measure any Siberians—remains a mystery. They are not among his papers at the AMNH or the Field Museum. In the 1903 summary of the JNPE, Boas quoted from a March 4, 1899, letter from Laufer in which Laufer

said that the Ainu refused to be measured, in contrast to Laufer's letter of September 18, 1898, in which he claimed to have measured over 100 individuals. Laufer's (1902) Jesup volume concerned only interpretations of Amur River people's art, and there is no later mention of his data in Boas' letters. In a July 1, 1906, letter to Boas, however, Laufer wrote of his forthcoming book on the Amur River peoples and informed Boas that it would contain sections on physical anthropology, linguistics, and ethnography. This manuscript was apparently never completed or published and has not been found in Boas' or Laufer's papers (see Krupnik, this volume). We believe that Laufer did not measure a significant number of subjects, if any, and that his letter of September 18 was referring to Natives he intended to measure but did not.³

Boas' team also collected skulls and made facial casts, but the sample size is very small. Bogoras reported that he collected 75 skulls, and at least 55 are in the AMNH, although many of them are incomplete (Boas 1903). The North American teams also collected many skulls, which are especially valuable in combination with data from skulls at other museums. Bogoras made 33 plaster facial casts and Jochelson made 41, according to their reports (Boas 1903). At least 42 casts from Siberia are in the AMNH (Jaymie Brauer, AMNH, personal communication). Unfortunately, the facial casts from Siberia were not cross-referenced to the anthropometric sheets but merely to ethnic group. Apparently, the North American teams also made facial casts; at least 20 anthropometric data sheets from North America make reference to cast numbers.

Despite some limitations, the anthropometric data sheets have the greatest potential of all the JNPE biological information collected because they include a sufficient number of standardized measurements, demographic data, and measurement locations and because of the large number of individuals measured.

The "Americanoid" Theory

The JNPE anthropometrics clearly provided Boas with

enough biological data to assess relationships in the North Pacific area. At the conclusion of the JNPE, Boas stated strongly in several publications that there was overwhelming evidence for strong biological ties across the North Pacific:

It seems clear, however, even at this time, that the isolated tribes of eastern Siberia and those of the northwest coast of America form one race, similar in type, and with many elements of culture in common. (Boas 1903:115)

and

Comparisons of type, language and culture make it at once evident that the Northeast Siberian people are much more closely akin to the Americans than to other Asiatics. (Boas 1905:99)

According to Boas, the "Americanoids" of Siberia and America were also different from the Eskimo, who had migrated from their original home in central Canada (Boas 1910:534–35).

Boas' work in British Columbia before the Jesup Expedition had given him first-hand experience of the great morphological and linguistic variation in Amerindians, which undoubtedly influenced his theory of the peopling of the New World. But even before that time, Boas' view of Northwest Coast Indians was probably influenced by an encounter with a group of Bella Coola [Nuxalk] who were "exhibited" at the Museum für Völkerkunde in Berlin and measured by Virchow. The general public, as well as Virchow, believed that the Bella Coola resembled Asians, especially Japanese, more than "typical" Indians (Cole 1985:71–2; Herskovits 1943). In fact, two years *before* the Siberian data were collected, Boas concluded:

The types of man which we find on the North Pacific coast of America, while distinctly American, show a great affinity to North Asiatic forms; and the question arises, whether this affinity is due to mixture, to migration, or to gradual differentiation. (Boas 1898:6)

Coincidentally, Jochelson also "became convinced that there were cultural and somatological connections

between the Palae-asiatics and the Indians of North America" (Jochelson 1925:2). Boas and Jochelson had therefore come to the same conclusion, probably from general impressions, before encountering peoples or collecting data from the other continent.

Later, the JNPE data suggested to Boas that there were close cultural and physical relationships across the North Pacific. Boas felt that the greater variability in America meant that the peoples of the New World had to have been there longer (Boas 1898, 1903). This pattern was confirmed by Torroni et al. (1993), who found greater diversity in the mitochondrial DNA of American Indian tribes than in Eastern Siberian groups, and by Ousley (1993, 1995), who found greater anthropometric variation in Northwest Coast Indians than in Siberians measured during the JNPE. But Boas also had to account for the culturally and morphologically distinct Eskimo, who separated his "Americanoids" on each side of the North Pacific.

Boas' "Americanoid" theory neatly explained all of his JNPE findings through a series of population movements. First, Asians migrated across a land bridge from Northeast Siberia to North America, where they were later isolated by glaciers, resulting in the greater diversification of Amerindians. When the glaciers retreated, the land bridge was reopened, and some Americanoids migrated back to Northeast Asia, forming an arc of related tribes across the North Pacific coasts of both continents. The arc was later broken by Eskimo, who presumably migrated to the Bering Strait area from Hudson Bay, forming a "wedge" that divided the Americanoids on each side of the Bering Strait. A merit of Boas' theory (outlined in greater detail in Ousley 2000:13–4) was that it explained why the Northeast Siberians were different from typical "Mongoloids" and Eskimo yet similar to Northwest Coast Indians in biology and culture (Boas 1905, 1907, 1910, 1912b, 1929).

The term "Americanoid" actually originated with Brinton, who used it in his *Essays of an Americanist* (1890) to ridicule anthropologists who believed that American Indians were part of the "Mongoloid" race.

Brinton believed that if American Indians were considered Mongoloids, then Asian Mongoloids should be considered a branch of the "Americanoid" race, since American Indians are the "purer" race, their hair being closer to a perfect circle in cross-section (Brinton 1890:62). Boas evidently revived the term in 1904 at the 14th International Congress of Americanists, held in Stuttgart, most likely as a sarcastic allusion to Brinton, who had died in 1899 (Ousley 2000:14).

After the conclusion of the JNPE, Boas had enough data to disprove Brinton's repeated assertion that American Indian culture was autochthonous to the New World, showing no connection to any cultures of the Old World (Boas 1903:73; Brinton 1886, 1890, 1891, 1894). Boas never presented specific anthropometric data that showed similarities between Siberians and American Indians, but much later he referred obscurely to cranial similarities between the Siberians and American Indians found by Jochelson-Brodsky (Boas 1929:112). This is perhaps not surprising: Boas was attempting to use statistics at a time when one had to compute them with pencil and paper. A thorough analysis required many weeks of calculation that would be unappreciated in the typological environment of the day, as was the case with Boas' later studies of heredity (Herskovits 1953). Jochelson-Brodsky alludes to this obstacle: "In spite of the critical attitude of the present days' anthropologists to averages they still form the chief base for somatological considerations" (Jochelson-Brodsky n.d.:104). Also, by this time, Boas was unsure about the use of anthropometrics for assessing population relationships, but for quite different reasons. Thus, Boas used JNPE ethnographic data to prove his theory. Waldemar Jochelson did mention specific resemblances in 1926, citing the cephalic index and nose, eye, lip, and cheek form, but without a reference or data (Jochelson 1926a:93).

Dina Jochelson-Brodsky's (n.d.) manuscript, an analysis of JNPE and Aleutian anthropometric data, was recently rediscovered among the Jesup Expedition materials at the AMNH (Ousley 2000). Her manuscript

conflicts with Boas' and Jochelson's vague references to it. In this unpublished 120-page study, she presented over 60 tables summarizing the means, standard deviations, and distributions of 25 anthropometric measurements and 9 indices from Siberian and American Indian populations. As in all univariate analyses, some groups are more similar in some measurements and different in others, and the choice of which measurements to use is largely subjective. But even her limited conclusions, based on standing height and the cephalic index, clearly show that Boas' "Americanoid" groups were not similar to each other and that the Eskimo were not outliers, but displayed intermediate values. Jochelson-Brodsky avoided an explicit statement that these results contradicted Boas' thesis, but it is likely that Boas recognized that her analysis undermined the biological basis for the Americanoid theory. Jochelson-Brodsky's study was never published by Boas as part of the Jesup Expedition proceedings.

Instead, the only JNPE volume dealing with biological data from the expedition was written by Bruno Oettking, and it was published in 1930 as the last volume of the JNPE series.⁴ Despite samples too small for reliable results, Oettking concluded that the Northwest Coast Amerindians were of the "Mongol" stock and were probably mixed with racially "progressive" and "superior" early Caucasoids (Oettking 1930:376). Although Boas had progressive views on race and did not believe in racial superiority, he appears to have preferred publishing questionable results rather than directly contradict his Americanoid theory.

Analyses

Boas' Americanoid theory never took hold in physical anthropology, but the perception of an Eskimo "wedge" has remained (Freed et al. 1988; Szathmary and Ossenberg 1978). Debets, a Russian physical anthropologist, noted that the Koryak and Chukchi were described by Soviet scholars, who compared them with more typical "Mongoloids" like the Evenk and Yakut [Sakha], while the North American Eskimo were

described by Americans, who were used to comparing them with American Indians. Thus, for Russians, Paleoasiatics (the Chukchi, Chuvan, Koryak, and Yukagir) would seem to have more American Indian features than other Asians, and to Americans, the Eskimo would seem to have more "Mongoloid" features than other American Indians (Debets 1951, cited in Levin 1958 [1963]). This unfortunate tradition has continued (e.g., Laughlin and Harper 1988; Spuhler 1979), illustrating a persistent need for objective data. One exception was Chard (1951, 1954), who recognized that the Americanoid theory was based primarily on cultural data and concluded, along with Russian physical anthropologists, that there was no Eskimo "wedge." Chard's conclusions, however, were based on comparisons of only a few measurements and observations. Until the rediscovery of the JNPE anthropometric data, a reliable test of Boas' Eskimo wedge and Americanoid theories was not possible with the available biological data (Szathmary 1979, 1993).

The debate about the origins of American Indians continues, although it now centers on the timing and number of migrations *from* Asia and whether these can be delineated (Crawford 1998; Greenberg et al. 1986; Merriwether et al. 1995; Ousley 1995; Szathmary 1993; Szathmary and Ossenberg 1978; Torroni et al. 1992, 1993). Genetic analyses have illustrated some general patterns of ancestral relationships, but many questions remain. Traditional blood markers are of limited use because data from at least 20 marker loci, the minimum number necessary for consistent estimations of population relationships, are still scarce for North Pacific groups (Szathmary 1993). Other methods for further analyzing nuclear DNA show some promise but are limited by small sample sizes at present.

Mitochondrial DNA (mtDNA) has been extensively utilized recently for assessing ancestral population relationships across the North Pacific. Different mtDNA analyses have been used to establish one, two, three, four, and more migratory "waves" into the New World. Based on mtDNA diversity, these waves are estimated

to have arrived in the New World between 5,000 and 40,000 years ago. However, the conclusions of many early mtDNA researchers overreached what was supported by the sample sizes. With larger sample sizes, more mtDNA types and subtypes are being discovered, and more complicated patterns of population composition, interactions, and migrations into the New World become evident (Merriwether et al. 1995, 1996; Schurr et al. 1999; Ward et al. 1993). Some groups probably migrated to the New World with a great deal of mtDNA diversity already present, confounding attempts to count or date the “waves” of migration. One study found tremendous mtDNA diversity within the Nuuchahnulth [Nootka] that was estimated to have taken 60,000 years to produce (Ward et al. 1991). In fact, the mtDNA sequence diversity of this one tribe had 62 percent of the diversity present in numerous sampled groups from Sub-Saharan Africa. On the other hand, Northwest Coast groups are known to have engaged in raids on distant villages, sometimes hundreds of miles away, for wife and slave capture (Drucker 1955; Suttles 1987). The incorporation of mtDNA lineages from other tribes would increase the mtDNA diversity and estimated time depth within a tribe (Ousley 1993). In addition, mtDNA sites that show apparent great time depth can be the result of higher mutation rates for certain sites (Gurven 2000). There is also considerable debate about whether American Indians went through a population bottleneck and which American Indian mtDNA lineages originated in the Americas as opposed to Asia. As has been pointed out, however, mtDNA data represent the genetic history of only one maternally inherited locus and may not reflect the history of populations (Szathmary 1993). The mtDNA data may be better suited for detecting and discriminating ancient and recent contributions to genetic variation within populations than for assessing overall population relationships.

The genetic relationships between American Indians and Siberians are not well defined, either, with some research pointing to Mongolian, Central Siberian, and

even early European ancestors who may have migrated to the New World before the peopling of northeastern Siberia (Brown et al. 1998; Crawford 1998; Merriwether et al. 1996; Santos et al. 1999). Investigations of the Y chromosome will provide additional data for researching population origins, but some mtDNA and Y-chromosome results conflict with each other, perhaps reflecting different migration patterns for each sex (Karafet et al. 1997, 1999).

Because of the lack of comprehensive biological data from the North Pacific, many researchers have only presented general physical and cultural impressions, and physical anthropologists have merely chosen the analysis closest to their own results for support (e.g., Harper and Laughlin 1982; Laughlin and Harper 1988; see Levin 1958 [1963] for many more examples). Recent statistical developments allow more informative and objective comparisons among populations based on metric data, which enable reconstructions of population histories (Konigsberg and Blangero 1993; Konigsberg and Ousley 1995; Relethford and Blangero 1990; Williams-Blangero et al. 1992). For example, Relethford and Crawford (1995) analyzed Irish anthropometrics collected in the 1930s and discovered evidence for Viking invasions and gene flow that had occurred over 1,000 years earlier.

The JNPE data therefore offer a unique resource for addressing some of these questions. The first analysis of all JNPE anthropometric data was carried out only recently, almost 100 years after the JNPE (Ousley 1993)—fortunately, at a time when electronic computers are available to perform statistical procedures. Ousley combined JNPE data with data from many other groups measured under Boas and also produced the first multivariate analysis of Siberian anthropometric data in English. Table 4 shows the mean cranial index (CI) values for North Pacific samples, with sexes combined. The Eskimo CIs are generally low but are close to those of many Siberians. A few Northwest Coast groups are near Siberian values, but generally they have the highest CIs. Very similar CIs are found in Jochelson-

Table 4
Cephalic Index (CI) of JNPE Samples⁵

Tribe	Number	Mean CI
EskMak	25	76.6
Evenk-SW	78	79.1
Chuvantsy	27	79.4
EskLab	30	79.4
NPBKory	70	79.5
WPBKory	123	80.0
Yukagir	40	80.0
EskSib	88	80.0
Eve-NE	25	80.1
ReinKory	26	80.8
Okanagan	40	80.9
Kwakiutl	69	81.1
Eve-NW	36	81.1
MariChuk	53	81.5
Tahltan	17	81.7
Yakut	30	81.9
Haida	42	82.1
ReinChuk	89	82.1
Nivkh	16	82.5
Thompson	142	82.7
Tsimshian	57	83.0
Carrier	23	83.3
Bella Coola	19	84.0
Navajo	60	84.4
Aleut	26	85.2
Chilcotin	41	85.8
Lillooet	97	86.1
Makah	47	86.3
Tenino	21	89.5
Quileute	26	90.4

Note: See endnote 5 for abbreviations

Brodsky's work (1906, n.d.). Clearly, Boas and Jochelson overstated similarities in the cephalic index and ignored the position of Eskimo samples (Ousley 2000).

Multivariate Analysis of Siberians

More reliable population comparisons involve *all* variables. Multivariate statistical methods, not practiced in Boas' time, enable comparisons between populations using all available measurements simultaneously; they also express overall similarity between any two groups by one number and permit the graphic representation of overall relationships. These overall relationships can be used to objectively assess the general impressions of a researcher. For example, Hall and

MacNair (1972), in a multivariate analysis of Boas' published Northwest Coast anthropometric data, confirmed Boas' impressions of greater similarity of the Thompson [Nlaka'pamux], Lillooet [Stl'atl'imx], Chilcotin [Tsilhart'in], and Shuswap [Secwepemc] tribes to each other than to other groups (Boas 1899c). Likewise, Boas's impression of three biological types of Northwest Coast Amerindians (Boas 1899c) was supported by an analysis of additional groups (Ousley 1993).

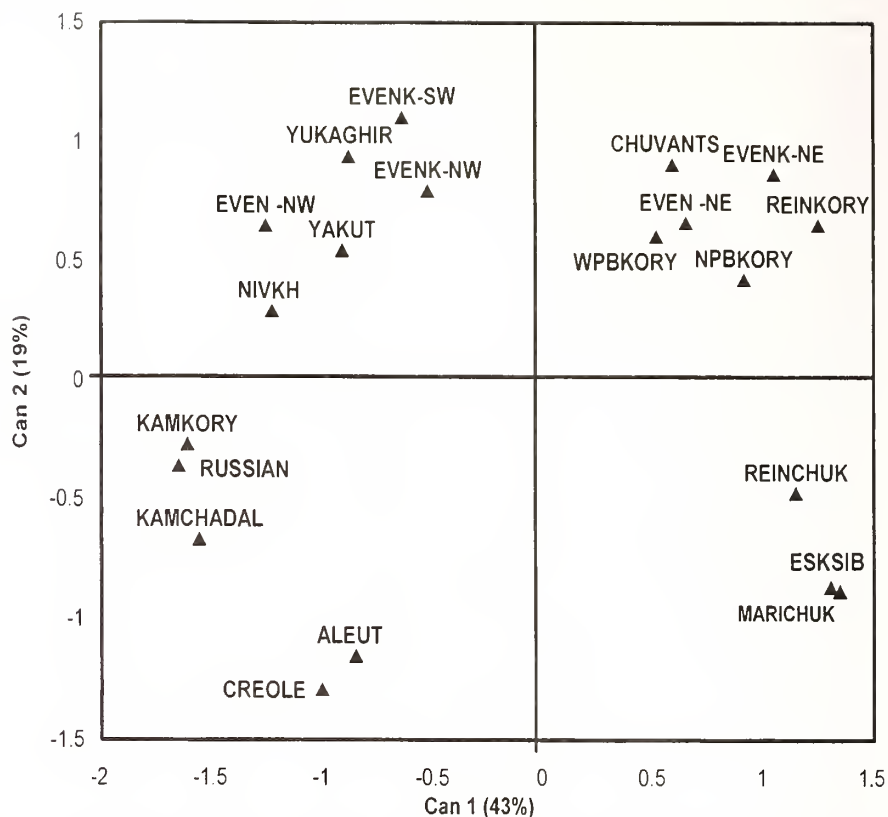
A computer capable of running statistical software such as SAS (SAS Institute 1985) that performs canonical discriminant analysis (CDA) would have also served Boas well. CDA converts the information expressed by many quantitative variables into fewer uncorrelated variables, called the canonical axis scores, which maximize among-group variation and take into account the correlations among variables. Relationships among groups can be illustrated by plotting the group means for two or three canonical axes, with some loss of information.

Anthropometric data from Siberian and Aleutian males and females between ages 20 and 60 were standardized by sex and pooled by ethnic group and location. The results of canonical discriminant analysis of this sample are shown in Figure 72. Groups that score high on the first canonical axis have relatively longer legs, shorter arms, larger faces, larger noses, and wider heads than those on the left of Figure 72. On the second canonical axis, groups in the upper half are shorter, with narrower shoulders, longer heads, and narrower noses than those below. In this case, the two axes represent 52 percent of all information from the measurements. The relationships among groups using all available information are expressed as distances from each group to all others and can be illustrated as a dendrogram, or "tree" diagram. A dendrogram displaying all the information from Siberians was constructed via this method (Fig. 73).

The anthropometric variation among the Siberian groups shows very strong geographic patterning, independent of language and ethnicity as assessed by

Jochelson and Bogoras. The Chukchi groups and the Siberian Eskimo [Yupik] who live on the Chukchi Peninsula cluster at the bottom right of Figure 72. All three trade with each other and intermarry, and Maritime Chukchi and Yupik (Siberian Eskimo) often live adjacently. According to historical accounts, the Maritime Chukchi have been slowly assimilating the Siberian Eskimo (Menovshchikov 1964), a fact well illustrated by the anthropometrics. When all variation is taken into account, using all distances, the Maritime Chukchi are slightly closer to the Reindeer Chukchi than to the Siberian Eskimo, as shown in Figure 73.

The cluster at the bottom left of Figure 72 is from the Kamchatka Peninsula, which is also separated from other Siberians in Figure 73. The Kamchatka cluster includes the Koryak from Kamchatka (Palantsy and Karagintsy), the Kamchadal [Itelmen] who are mixed with Russians, and the Kamchatkan Russians. This cluster reflects documented gene flow between all three. In the 18th century, the Kamchadal were distributed more northward, overlapping with the Koryak (Antropova 1964). At the time of the JNPE, the Kamchadal and Koryak of the Kamchatka Peninsula had been intermarrying with the Russians for nearly 200 years (Jochelson 1908). Nearly all Kamchatkan natives measured had Russian names. The Koryak from other areas are very different from the Kamchatka Koryak, clustering in the upper right of Figure 72. The Aleut and the "Creoles" (Aleut mixed with Europeans, especially Russians) plot near the Kamchatka



72/ Canonical Discriminant Analysis of all JNPE Siberians measured and 27 Aleuts and 16 "Creoles" (Aleut mixtures) measured by Jochelson-Brodsky on the Ribushinski Expedition. Abbreviations are: EskSib, Siberian Eskimo; Even-NE, Even from Yeropol, Markovo; Evenk-NE, Evenk from Kamenskoye, Markovo; Even-NW, Even from the lower Kolyma River and Nelemnoye; Evenk-NW, Evenk from Nayakhan and Gizhiga; KamKory, Koryak from Kamchatka (Palana and Sedanka); MariChuk, Maritime Chukchi; NPBKory, Koryak from northern Penzhina Bay (Kamenskoye and Talovka); ReinChuk, Reindeer Chukchi; ReinKory, Reindeer Koryak; WPBKory, Koryak from the west coast of Penzhina Bay (Kuel, Itkana, Paren River).

cluster, but Figure 73 confirms that they, along with the Nivkh, are very different from other Siberians.

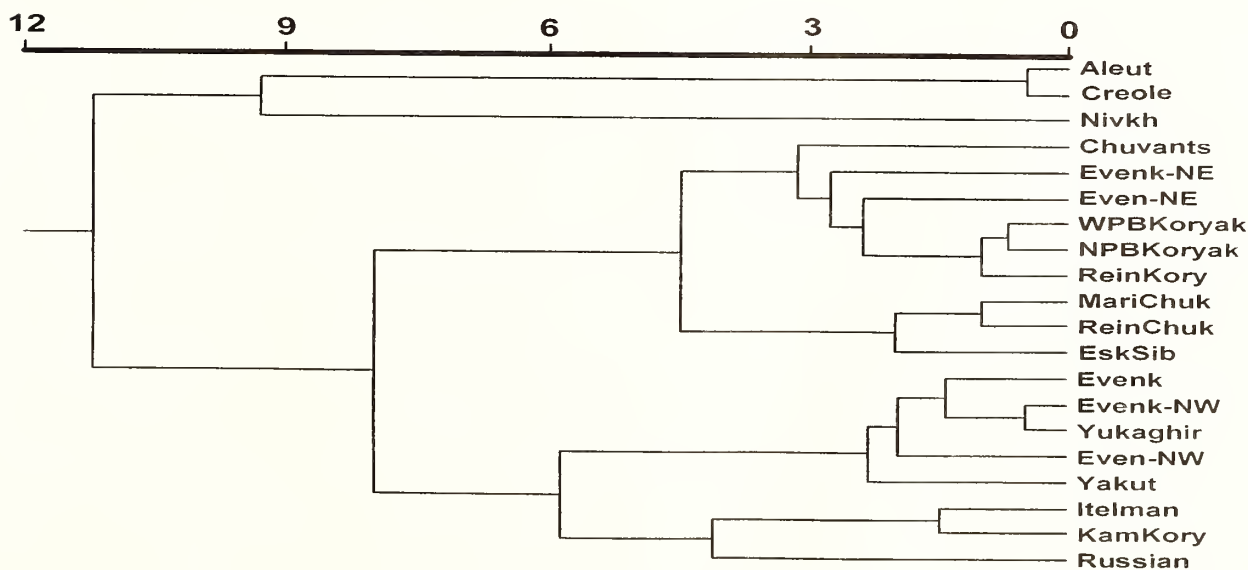
The groups measured in the northwestern area of the JNPE in Siberia similarly cluster with each other in the top left of Figure 72, along with the Evenk measured in the Nayakhan and Korkodon River area. This is probably the result of gene flow between all groups. The Even, believed to stem from the relatively recent assimilation of Yukagir, Koryak, and other elements by northeastern Evenk (Arutiunov 1988a), can be separated into eastern and western subdivisions, each showing affinity to the groups geographically near them. The Even of the northwest area cluster with the Yukagir, Yakut [Sakha], and Evenk in the same area, while the Even of the Markovo area are similar to the Chuvantsy

[Chuvan], Koryak, and Evenk in that area. The two divisions of the Even are also widely separated in the dendrogram (Fig. 73).

The cluster at the top right of Figure 72 represents groups from the Yeropol-Markovo area, as well as all Koryak from the vicinity of Penzhina Bay. The Koryak north of Kamchatka show greater biological cohesiveness than other Siberians. This probably represents a larger range of movement and interaction among Koryak groups north of Kamchatka. As shown in Figure 73, the Penzhina Bay Koryak are most similar to the northern Koryak, followed by the Reindeer Koryak. The Chuvantsy [Chuvan] were described as a Yukagir-speaking tribe (Bogoras 1904–09), but Jochelson remarked that the Chuvantsy in Siberia were either Russianized or were influenced by the Koryak or Chukchi (Jochelson 1926b). At the time of the JNPE, the Chuvantsy were surrounded by Reindeer Chukchi to the north and Reindeer Koryak to the south. The documented ethnographic relationships of the Chuvantsy are reflected anthropometrically, for the Chuvantsy are most similar to the northeastern Even, Evenk, and Koryak groups.

The Siberian anthropometric relationships largely reflect a recurrent pattern seen in Siberia and other parts of the world: groups located close to each other

exchange genes, whether the admixture results from trade, warfare, or migration (Arutiunov 1988a; Bogoras 1904–09; Dikov 1965; Dolgikh 1965; Harding and Sokal 1988; Jochelson 1908; Moss 1992; Townsend 1979). Linguistic barriers are rarely genetic barriers. Geographic barriers are often more formidable, but the strong geographic patterning of the Siberian anthropometric data may also be a product of strong environmental influences. Northern populations, however, have come up with clever cultural adaptations to a severe environment, and gene flow likely affects groups far more than does natural selection in the relatively short term, barring mass extinction. Indeed, all types of biological data (anthropometrics, dermatoglyphics, blood markers, mtDNA, Y chromosome, etc.) should be subjected to Boasian skepticism, and the strengths and weaknesses of each should be acknowledged. This, however, is rarely done. For example, in a recent test of assumptions, Ousley (1997) found that unlike the case with anthropometrics, the phenotypic distances among family members using dermatoglyphic ridge counts are not proportional to the genetic distances, meaning that population relationships estimated directly from dermatoglyphic ridge counts will be inaccurate.



73/ Dendrogram of Siberian and Aleut Samples. For abbreviations, see Fig. 72.

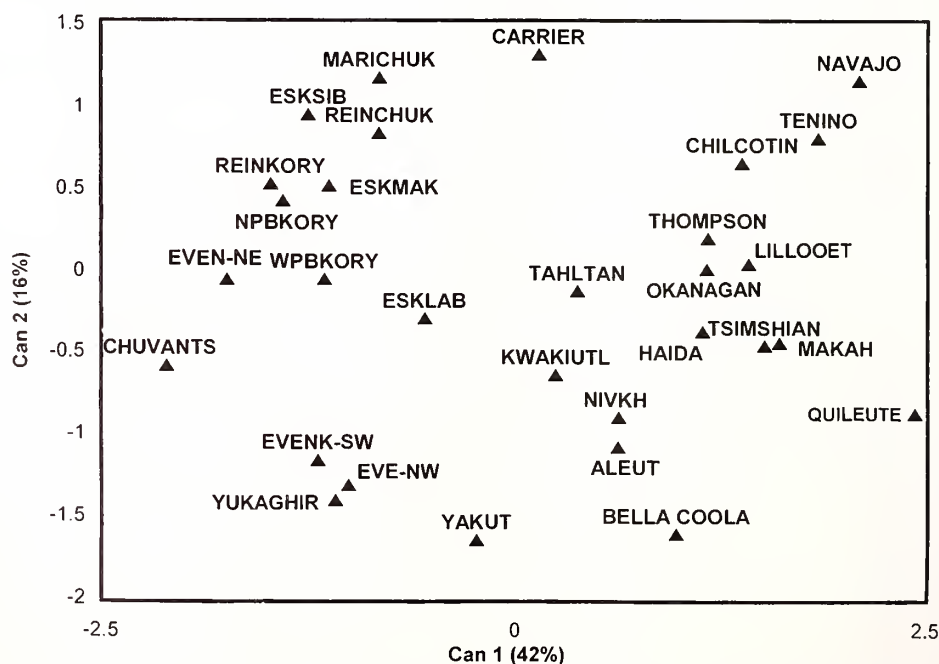
Multivariate Analysis of North Pacific Groups

Figure 74 is a canonical plot of relationships among North Pacific groups for which Boas had data. The results are similar to those from other studies with more groups (Ousley 1993, 1995). Most groups in the left half of Figure 74 are Siberians, with the exception of the Eskimo samples. The upper-left quadrant of the figure shows a clustering of northeasternmost Siberian groups—the Koryak, Chukchi, and Siberian Eskimo—as well as Eskimo from Labrador and the Mackenzie River Delta in northern Canada. The bottom-left quadrant contains the other Siberian groups. American Indians and the Aleut are on the right, as are the Nivkh. This separation of Old and New World populations is also shown in Figure 75, a dendrogram that uses the same population samples as in Figure 74. Most Northwest Coast Amerindians are clustered in the upper half of Figure 74, while the Kwakiutl, Aleut, and Bella Coola are nearer the bottom. The groups are also separated in Figure 75, in which the Tahltan are close to the Kwakiutl, Aleut, and Bella Coola. The division of coastal North Pacific groups into these clusters is supported, as well, when other statistical methods and groups are used (Boas 1899c; Ousley 1993, 1995).

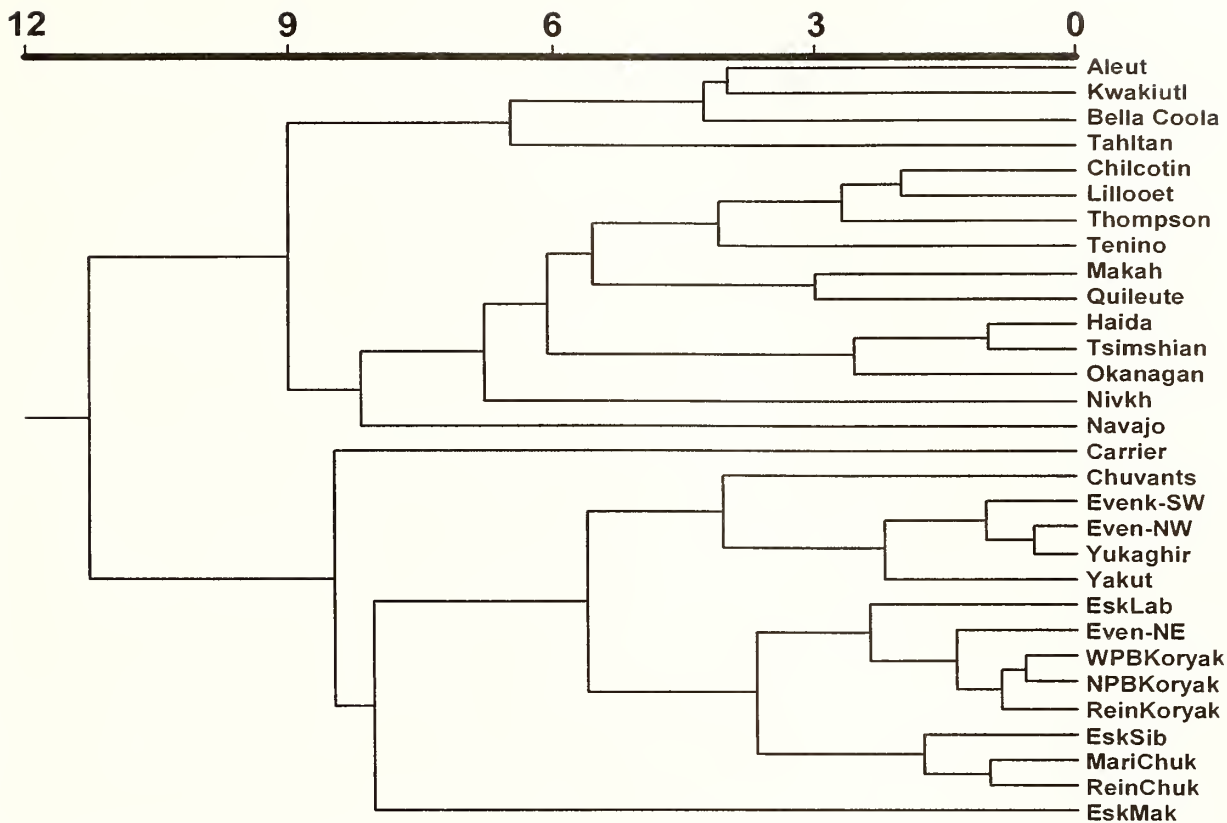
These results call Boas' theories into question, given the absence of Americanoids and the close anthropometric relationship of the Eskimo to other North Pacific populations. Only the Nivkh sample, which Boas apparently never analyzed, shows great affinity to Northwest Coast Amerindians. Both North American Eskimo groups show unquestionable Siberian affinities; in particular, the

Labrador Eskimo sample is most similar to the Maritime Koryak. Thus, there is no Eskimo "wedge." The anthropometric affinities of the Eskimo samples suggest an Asian origin, as have more recent archaeological and ethnographic studies (summarized in Ousley 1995), rather than one in central Canada, as Boas had supposed. Another analysis (Yokota et al. n.d.), using several sets of biological data, finds that most of the Siberian groups from the JNPE are more similar to the Eskimo than to other Asian populations, including the Chinese. On the whole, the data are in agreement with Chard's (1960) suggestion that Eskimo populations, which at one time stretched from Kamchatka to the Bering Strait or beyond, may have been the carriers of Asiatic cultural elements into the New World.

Not all of Boas's impressions were incorrect. Ousley (1993), in a larger-scale analysis, found that Northwest Coast tribes are more similar to Siberians than are other Amerindian tribes. The Eskimo show unquestionable Asian affinities, while the Aleut show strong New World affinities, reflecting ethnohistorical data rather than linguistic relationships. This illustrates Boas' (1911)



74/ North Pacific Canonical Plot. Except for Evenk-SW, the Even and Evenk samples were pooled according to region (Eve-NW, Eve-NE). For other abbreviations, see Fig. 72



75/ Dendrogram of North Pacific Populations. For abbreviations, see Figs. 74 and 72.

assertion that anthropological results based on biology, culture, and language need not agree. Boas' (1912a, 1916) results from immigrants indicating morphological changes in head shape after migration to the New World may temper the results of intercontinental population comparisons. The extent of morphological changes after migration and their effect on estimated population relationships are uncertain. A reanalysis of Boas' immigrant data, however, indicates that age-related variability is a much more significant influence on the morphological changes that Boas observed than is the environment (Corey Sparks, University of Tennessee, Knoxville, personal communication).

In addition, anthropometric data, like other biological data gathered from modern individuals, may reflect historically recent rather than ancient population events. There are major drawbacks in examining modern populations to ascertain what happened 5,000–15,000 years ago. Modern native populations are the

result not only of ancient migrations but also of subsequent and continuous gene drift, gene flow, founder effects, ethnogenesis, in- and out-migrations, warfare, epidemics, extinctions, admixture, assimilation, and perhaps natural selection. The addition of ancient DNA analyses may help in providing data at various points in time, but technical challenges, limited samples, high costs, and repatriation concerns remain formidable.

There are many ways of utilizing the JNPE anthropometric data, some of which do not involve estimating ancestral population relationships. Of course, the similarities among groups from opposite sides of the North Pacific can be explored in greater detail, and the spatial patterning seen in Siberia can be investigated further using more sophisticated methods and additional measurements collected only in Siberia. The stature of Siberian adults and the growth of Siberian children at the time of the JNPE can be compared with these data for modern Siberians. Furthermore, morphological changes among Siberian adults since the JNPE

can be investigated using modern data sets. For example, Comuzzie et al. (1995) reported dramatic reductions in Evenk facial measurements since the JNPE.

Summary

JNPE biological data, collected under Boas' direction, reflected his faith in the analytical value of anthropometrics as part of holistic anthropological fieldwork, in contrast to Brinton's "armchair" anthropology. In geographic range, quality, and extent of data, the JNPE produced an unsurpassed amount of biological information about North Pacific peoples. Initial studies of the data contradicted Boas' Americanoid theory, which was based almost entirely on cultural similarities. Until recently, however, the JNPE anthropometric data had never been adequately analyzed to explore the biological relationships of peoples on both sides of the Bering Strait, as Boas had intended. Paradoxically, although Boas never analyzed the biological data from the JNPE, a rejection of his Americanoid theory is possible only because he insisted that such metric data be collected.

Boas' foresight in amassing quantitative biological data (despite doubts of their immediate utility) has given us extremely valuable biological records. These enable us to perform analyses that shed light on ancient and recent relationships, growth, and morphological changes over time. We should acknowledge the contributions of Franz Boas as we would an expert photographer who captured a moment in time. Under his direction, over 18,000 American Indians and Siberians were measured. He was indeed prescient: many of the populations measured by his teams have disappeared through dispersion and assimilation.

The rediscovery of Boas's anthropometric data has coincided with the availability of much greater statistical and computational capabilities for analyzing them. Much more reliable biological information can be gleaned from all types of biological data, especially in the North Pacific, where important questions linger as to ancient migrations and more recent gene flow. While

there are fewer computational limitations on analyses today, there are greater challenges for data collection. The authors hope that all varieties of biological data will be collected as part of any North Pacific research project in order to assess modern population relationships, to compare the new data with other information collected over the last 100 years, and to investigate changes in growth and body form since the JNPE.

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Notes

1. The contrast between inductive and deductive logic and reasoning does not adequately describe the differences between Boas' and Brinton's research methods. The conclusion of a deductive argument is claimed to follow necessarily from the premises. If the premises of a deductive argument are true and the argument is valid (the conclusion follows from the premises), then the conclusion must be true no matter what other information is added (Copi 1982). Brinton constructed deductive arguments by assembling published observations that supported a foregone conclusion, such as the psychic unity of mankind or the unique nature of American Indian culture, and ignoring any observations and explanations to the contrary. This deductive approach limited what data were relevant, for they were being gathered for a specific purpose.

By contrast, Boas' approach was inductive because he was concerned with gathering as many data as possible; the data were not particularly constrained by conclusions or theory. Inductive reasoning involves probabilistic statements, and the probabilities can change as new information is added. Inductive reasoning generally involves analogies, generalizations, and causal connections (Copi 1982).

As a result of their different approaches, Boas made more numerous and far greater enduring contributions to anthropology. Brinton's legacy is one of pompous and flowery writing, full of conclusions that sound well founded but have overwhelmingly proved false, untestable, or irrelevant. Boas, by contrast, generally avoided theorizing (with the notable exception of the "Americanoid" theory), and some have interpreted this as a weakness. Boas, however, left behind cautious explorations of data in his publications, numerous collected items of material culture, and mountains of archived data that others can use even today to test theories. The JNPE is a microcosm of Boas' career in that great amounts of data were collected but no comprehensive results and conclusions were published.

2. An invoice was found among Boas' professional correspondence for "computers"—of the human variety—to calculate statistics from his biological data.

3. This was Boas' second setback for anthropometric data collection in the Amur River area. In 1893, he had sent D. Scott Moncrieff, an experienced measurer who had worked in British Columbia, to the Amur River to gather data for an exhibit at the World's Columbian Exposition. Shortly after his arrival, Moncrieff drowned while testing a native boat (Johnson 1897).

4. Dina Jochelson-Brodsky's study was advertised on the cover page of the Oettinger volume as Part 2 of that volume, although her contribution was never published—ed.

5. For data in Table 4, order is from lowest to highest SI. Males and females were combined. Except for Evenk-SW, the Even and Evenk samples were pooled according to region. Abbreviations: EskLab, Labrador Eskimo; EskMak, MacKenzie Delta Eskimo; EskSib, Siberian Eskimo; Eve-NE, Even and Evenk from the northeastern area of the JNPE;

Eve-NW, Even and Evenk from the northwestern area of the JNPE; Evenk-SW, Evenk from Nayakhan and Gizhiga; MariChuk, Maritime Chukchi; NPBKory, Koryak from northern Penzhina Bay (Kamenskoye and Talovka); ReinChuk, Reindeer Chukchi; ReinKory, Reindeer Koryak; WPBKory, Koryak from western Penzhina Bay (Kuel, Itkana, Paren River).

6. Although language is not necessarily a barrier to gene flow, dialects can reflect social interactions. The Koryak of northeastern Kamchatka speak Aliutor, a distinctive dialect of Koryak, if not a separate language (Krauss 1988). The Kamchadal samples are from the western linguistic branch, which includes dialects greatly influenced by Koryak and Russian (Antropova 1964a; Arutiunov 1988b).

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Voices from Siberia

Ethnomusicology of the Jesup Expedition

RICHARD KEELING

The stories, folklore texts, and other spoken narratives collected during the Jesup North Pacific Expedition (1897–1902) are fairly well known and are generally available in published editions. By contrast, the musical sound recordings are much less accessible, and their place or purpose in the original expedition design is not adequately understood. The recordings include Northwest Coast and Arctic Siberian collections that have never been reviewed or subjected to comparative analysis in any published study. I come to this subject through my previous research related primarily to North American Indian music of northern California (Keeling 1992a, 1992b). Ideas about music as a vehicle for cultural analysis or historical interpretations have changed immensely over the past 100 years. What intrigued me was the opportunity to subject Boas' data to the light of modern theories.

There is a vast amount of recorded evidence and published research to build on. In order to help others locate some of the more important early recordings and the related writings, I have prepared an inventory and bibliography, which follow (Appendixes A and B). While many of these early collections have been documented quite carefully, the Jesup Expedition musical recordings remain poorly understood, despite their key importance for future research.

Boas' Early Musicological Research

Music was important to Boas. He addressed the subject in more than 20 publications; he corresponded

with virtually all the major figures in Native American music research throughout his career; and his students included not only such distinguished musicologists as George Herzog and Helen Roberts but also Alfred Kroeber and Edward Sapir, whose accomplishments in the area of native music research are less well known.¹

Boas was among the first to recognize that various aspects of culture—"religion and science; music, poetry, and dance; myth and history; fashion and ethics"—were all "intrinsically interwoven" (Boas 1904:243). This concept not only revolutionized current thinking with respect to the nature of culture but also offered the fascinating possibility that music and the arts could be vehicles for comparative research.

In fact, Boas was an early pioneer in ethnomusicology. Systematic research on what was then called "primitive music" began in 1886 when Carl Stumpf published a paper describing songs performed by a group of Bella Coola Indians who visited Germany in 1885. (Myron Eels had published a pioneering paper on American Indian music six years earlier; see Eels 1879.) Boas joined the new field almost immediately by publishing similar, although less detailed, descriptions of music in his classic ethnography of the Central Eskimo (Boas 1888). The musical notations in these and all earlier studies were done by ear. A major advance occurred with the invention of the Edison phonograph, patented in the United States in 1877, which made it easier to collect musical data and also made the process of transcription and analysis much more efficient.

The Edison-type phonograph was first used for ethnographic research in 1890, when Jesse Walter Fewkes created 31 cylinders of songs and spoken texts from the Passamaquoddy Indians of Maine. Here again, Boas was not far behind. In 1893 and 1895 he made almost 150 cylinder recordings among the Kwakiutl [Kwakwaka'wakw] on Vancouver Island and also among various tribes of the Thompson River area in British Columbia (see Appendix A). The Thompson River recordings have particular significance because they later became the subject of an important paper by Otto Abraham and Erich von Hornbostel of the Berlin Phonogram Archive.

With this publication (Abraham and von Hornbostel 1906), the German musicologists Stumpf, Abraham, and von Hornbostel had taken the lead in comparative music theory. Using fairly detailed transcriptions and statistical methods, they developed a style of analysis that basically extended the concept of cultural relativism to music. Previously, it had been thought that "primitive" peoples were incapable of singing in tune. Abraham and von Hornbostel showed, however, that the style of the Thompson River singers was perfectly regular and consistent but was simply guided by different principles of composition.

So when the Jesup Expedition began in 1897, Boas probably had high expectations for musicology as a major component of the project, perhaps hoping to justify his own long-standing commitment to music as a central element in culture. The outlook for comparative musicology as a historical method had never seemed more promising, armed as it was with a new advanced technology for field collection and documentation, the Edison phonograph, and with exciting new developments in theory.

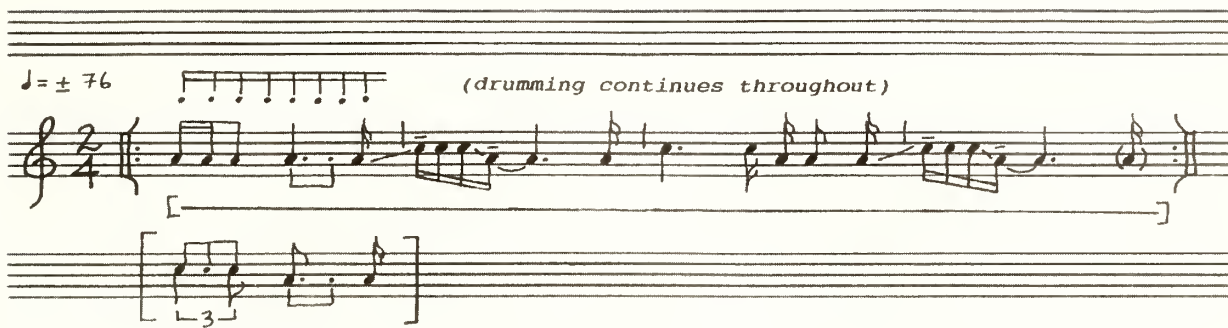
This optimistic spirit lasted well into the 1930s, when comparative research on Native American music reached a peak of sorts in the work of George Herzog (1935a, 1935b, 1936) and Helen Roberts (see Appendix B). There followed a leveling of interest that lasted through the 1950s. Since then, comparative

musicology has continuously declined as a focus within the discipline of ethnomusicology. Indeed, it seems ironic that the recent resurgence of interest in the field is gravitating to such a degree around the same North Pacific region and many of the same comparative issues that had first stimulated Boas' interest a hundred years earlier.

Musical Sound Recordings of the JNPE

Boas commissioned four or five separate musical collections during the Jesup Expedition. This paper focuses mainly on two sets of recordings that Waldemar Jochelson and Waldemar Bogoras collected in Siberia (Fig.70). Other music-related investigations commissioned as part of the Jesup Expedition research were conducted by Livingston Farrand among the Quileute and Quinault in 1898 and by John Reed Swanton among the Haida in 1900–01 (see Appendix A). In addition, Boas' 1905 report on Jesup Expedition activities includes passages from letters in which Berthold Laufer described making sound recordings among the Nivkh [Gilyak] of Sakhalin Island in 1898–99. These cylinders, however, do not seem to have been deposited at the American Museum of Natural History (AMNH) in New York, and I have not been able to locate them in other American collections. Thus Bogoras' and Jochelson's recordings of songs and texts during 1900–02 represent the only cylinder collection from northern Siberia. They are an important component in any discussion of the music of the Native people of the North Pacific region. The ethnic groups represented include the Koryak, Tungus [Even], Yukagir, Yakut [Sakha], Chukchi, and Siberian Eskimo [Yupik]. In all, there are 130 documented Jesup Expedition Siberian cylinders, originally deposited at the AMNH. Today, duplicates of the recordings on tape are most readily available from the Archives of Traditional Music at Indiana University, grouped under catalogue number 54-149-F.²

One type of singing that is described in the published literature but was evidently not recorded (or is not identified properly in the available documentation)



76/ Notation of song performed by Koryak female shaman, recorded by Waldemar Jochelson, 1900.

is that connected with a Circle Dance performed among several Arctic Siberian groups. Jochelson has this to say about the dance, its distribution, and the animal symbolism in the songs:

The circle dance is accompanied with singing, which consists of four notes corresponding to the four steps. The words sung—ho'yoi-he'yui or he'ke-ha'ka—are Tungus. The Yukaghir do not know their meaning, and hold them to be pure interjections. It seems pretty obvious to me that this dance has been borrowed from the Tungus. Possibly the Yakut have also borrowed it from the latter; but among the Yukaghir this dance is at times accompanied by singing and motions which are absent in the circle dance of the Tungus but have become familiar to us in dances of the Chukchi and Koryak. The Tungus singing referred to above was from time to time interrupted by a guttural rattle and by other sounds in imitation of the cries of various animals. Some of the dancers, generally girls, produce very skillfully a guttural rattle resembling the grunting of seals, while the others answer with higher guttural sounds. (Jochelson 1910-26:130)

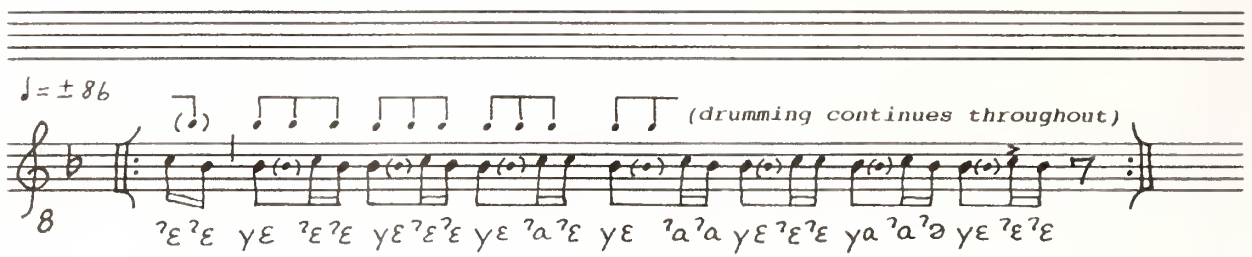
Jochelson made four recordings of a Yakut [Sakha] woman performing what seem to be epic songs but are not identified as such in the documentation provided with the recordings (ATM cylinders 4569-4562). Epic songs called *yukara* are also an important genre among the Ainu of northern Japan. Thus, the lack of epic songs in other Jesup Expedition collections raises the question of the extent to which the Jesup recordings provide a complete picture of Native Siberian musical activities.

Shaman Songs

What is certainly a strength of Jesup Siberian recordings is the significant number of shaman songs. Disregarding the cylinders containing spoken narratives and Russian material, there are 92 songs or other musical items, of which 37 are clearly identified as being shamanistic in character.³ The recordings contain many different types of shamanistic vocalizing, which suggests a possible distinction between Koryak, Chukchi, and Tungus singing. Their importance for research is enhanced by the fact that they correspond to activities that are extensively documented in the published ethnographies by Jochelson (1908, 1910-26) and Bogoras (1904-09).

One very prominent style among the musical recordings is illustrated in a song performed by a female Koryak shaman. It was recorded by Jochelson at the Koryak village of Kuel, on the coast of the Sea of Okhotsk in Northeast Siberia (Fig. 76).⁴ This is a fairly repetitive two-phrase melody (*a* and *b*), and in fact the *a* section has a variant that makes it nearly identical with section *b*. The range is quite narrow, as the scale consists basically of just two tones only a minor third apart. For a woman, the vocal quality is raspy, nasalized, and strongly accentuated. The metrical structure is basically simple, but the rhythm is very complex in detail because it follows the changing syllables of a text and is highly flexible or irregular in character.

A more exaggerated version of the same basic style was performed for Jochelson by a male Koryak shaman from the same village. It has an even more



77/ Notation of song performed by Koryak male shaman, recorded by Waldemar Jochelson, 1900.

repetitive melody and is narrower in range, a major second. In Boas' transcription the same melodic pattern is reiterated seven times, with slight variations, but on the recording itself it is repeated as few as five and as many as eight times between breaks, and there is also variation in the vocable patterns, as shown in a typical rendition (Fig. 77). The tones are not clearly focused in pitch, and there is much glottalization and pulsation (indicated by the parenthesized noteheads). The singing is loud and strongly accentuated. The drumming is mainly in triplets but does not seem to be precisely coordinated with the vocal part. On balance, the Koryak songs notated in Figures 76 and 77 represent the most predominant vocal pattern documented in the Jesup Expedition recordings, since the collection also includes Yukagir and Chukchi songs in a similar style.

A distinctly different style is heard in three songs performed by a Tungus [Even] shaman at Najakhan [Nayakhan], Siberia. In one of them (Fig. 78), what seems to be a lexically meaningful text is intoned to a two-beat melodic pattern that is repeated for as few as five repetitions and as many as nine between breaks. The phrase "bo-bo-bo-bo-bo-bo-bo," not shown in the

notation, is interjected twice in a higher register. As in the previous examples, the melody is quite repetitive, but this is clearly a text-driven form, and the speechlike vocal delivery is also much more relaxed than in the previous examples. I have noticed a softer vocal delivery and similar three-tone scales in other, more recent recordings of Tungus singing collected by the Russian ethnomusicologist Yuri Sheikin.⁵

Several of the Jesup Expedition recordings contain sounds that were made by shamans while conjuring spirits or actually being possessed. These "animal spirit" sounds are virtually impossible to notate, and the recordings must truly be heard to appreciate the variety of phonetics and vocal techniques involved. In his description of the performance of a Yukagir shaman named Tretyakov, Jochelson employed phonetic spellings to indicate the voices the shaman used in conjuring nine different animals, including various types of birds, a wolf, and a bear (Jochelson 1910-26:206).

Bogoras also provided several vivid descriptions of shamanic seances and demonstrations, including this account of what happens when a *kele* (a monstrous evil spirit) enters the body of a Chukchi shaman:



78/ Notation of a song performed by a Tungus male shaman, recorded by Waldemar Jochelson, 1901.

The shaman shakes his head violently, producing with his lips a peculiar chattering noise not unlike a man who is shivering with cold. He shouts hysterically, and in a changed voice utters strange, prolonged shrieks

such as “O to to to,” or “I pi, pi, pi, pi”—all of which are supposed to characterize the voice of the kelet. He often imitates the cries of various animals and birds which are supposed to be his particular assistants. If the shaman is only a “single-bodied” one—that is, has no ventriloquistic power, the kelet will proceed to sing and beat the drum by means of his body. The only difference will be in the timbre of the voice, which will sound harsh and unnatural, as becomes supernatural beings. . . . With other shamans the kelet appear all at once as “separate voices” . . . from all sides of the room, changing their place to the complete illusion of their listeners. Some voices are at first faint, as if coming from afar; as they gradually approach they increase in volume, and at last they rush into the room, pass through it and out, decreasing, and dying away in the remote distance. Other voices come from above, pass through the room and seem to go underground, where they are heard as if from the depths of the earth. Tricks of this kind are played also with the voices of animals and birds, and even with the howling of the tempest, producing a most weird effect. (Bogoras 1904–09:435)

by George Herzog.⁶ Bogoras’ Chukchi recordings also include “vocal games” or “throat games” much like those performed by Eskimo women all across the Arctic region, thus providing a highly significant basis for comparison. According to Bogoras (1904–09:268–9) these sounds imitate animal spirits such as Raven, Fox, and Bear, suggesting that although the songs are ostensibly games, they may have shamanistic implications as well. Similar vocal games are documented among the Ainu of northern Japan and the Amur River Nanay (who belong to the Tungusic language stock), but they are not present among other Siberian recordings in the Jesup Expedition collection.⁷

Yupik Songs

The Jesup Expedition recordings clearly document a different style of vocal music being performed among the Yupik [Siberian Eskimo]. Beyond the obvious differences in vocal quality, the style of a Yupik song is clearly distinguished from those of the other Siberian groups by its wider melodic range, relatively complex strophic form, and six-tone scale. On the recordings by Bogoras, one song is sung three times, first in vocables (as notated) and then twice with words. The text of this song could not be transcribed effectively. It is notated a minor second lower than it sounds on the recording (Fig. 79).

Bogoras theorized that several of the Maritime Chukchi songs he recorded in 1901 were largely imitations of Eskimo songs (Bogoras 1904–09:138). Influences of the more complex Eskimo style are also apparent in unpublished notations of Chukchi songs

The musical notation consists of a single melodic line on a staff with a treble clef and a key signature of one flat (B-flat). The tempo is marked as $J = \pm 88$. The notation includes various time signatures: 4/4, 3/4, 2/4, and 4/4. The lyrics are written below the notes, including: ya (o) wε, 'a ya 'a (a) (a) (a) ya ba ya — ya 'ε, ya ya 'a (a) (a) bey ya nga ya 4 ya, 'a (a) na ya 'a (ə) ya ya (a) nga ya 'a ey, ya 'a mε yε yə mε(ε) 'a (a) yε ya yε ya, 'ε — 'ε ya 'ε ga 'ey ya, yə ya 'ε he (i) ya he ya ha- 'ə hey ya, 'a 'ε ya gε ya ya ya, 'a 'ə yε ya ya he ya —, 'a 'ə ma 'ε yə ya — ə ya yε ya 'a.

79/ Notation of a song performed by a Yupik Eskimo man, recorded by Waldemar Bogoras, 1901.

Comparative Perspectives on Arctic Siberian Singing

Given these general divisions, Tungus [Even], Yakut [Sakha], and Siberian Eskimo [Yupik] songs are clearly related to, but also distinguishable from, a core Arctic Siberian vocal style that could be summarized as follows:⁸

1. Shamans' songs tend to predominate.
2. The singing is loud and raspy, with much glottalization, vocal pulsation, and nasality.
3. Most texts consist of vocables or combinations of words and vocables; the texts are highly repetitive, and vocable patterns seem to be varied rather freely.
4. All of the songs are soloistic (except for vocal games), though this may be because certain genres were not recorded.
5. Simple one- or two-phrase melodies are the rule, and phrases are short.
6. The melodic range is narrow.
7. The melodic contour is flat or undulating.
8. Simple two- or three-note scales predominate, and the intonation is diffuse or imprecise.
9. Tempos are quick; simple meters and one-beat rhythms predominate.
10. There seems to be a great deal of emphasis on vocal "sound effects," some of which require considerable virtuosity, while melodic and rhythmic patterns are highly repetitive.

In comparing this music with New World styles, the differences between Arctic Siberian singing and the more "complex" styles generally associated with Eskimo (Inuit) singing or the Indian music of the Northwest Coast are striking. Utilizing standard methodology, a musical overview of the North Pacific region as a whole would have to include at least six distinct subareas: Ainu, Arctic Siberia, Eskimo-Aleut, Athabascan, Northwest Coast, and Northwestern California. Describing song types and general profiles for the vocal music of these groups would undoubtedly produce interesting evidence of historical contacts and local elaborations. There is a fundamental consistency

through which the shamanistic functions of vocal music are expressed throughout the North Pacific region, despite the variations or differences between musical systems. These patterns of musical symbolism are clearly distinct from those documented elsewhere in North America during the 19th and early 20th centuries.

New Directions in Comparative Music Research

In order to understand the musical traditions of the North Pacific region as a whole, or to appreciate that Arctic Siberian recordings have significance for Native American music, knowledge of advances in music theory over the past 100 years is helpful.

The standard approach for analyzing musical systems, as employed by Nettl (1954), focuses mainly on the stylistic characteristics of the music itself. This approach, however, has several drawbacks. The predominant characteristics are always difficult to identify with certainty in repertoires that are seldom homogeneous, and this way of thinking does not place enough emphasis on the relationships between style and cultural function or significance. Nettl's approach is also synchronic in that it allows no means of documenting change over time. This is a problem that has limited the success of comparative research on Native American music since its beginnings. Nettl basically tries to identify the predominant styles of Indian and Eskimo music in six different culture areas and finds no correspondence with the Siberian style.

A more integrated concept would focus more on the social and ritual contexts of music-making and on musical semiotics or symbolism. Specifically, I believe that connections can be found between the music itself and other elements of what I have called "the northern hunting complex" or the "northern hunting religion" (Keeling 1992b:36–9). This concept basically follows the interpretations in Fitzhugh and Crowell (1988) and in earlier works such as Hallowell's (1926) study of bear ceremonialism. It includes such features as animal understanding of human intentions, refreshing

of animals after the kill, and generalized shamanism. Songs and dances that imitate or evoke animal deities are central to this complex.

As George Herzog pointed out in 1935 (Herzog 1935b), most Indian repertoires also contain simpler songs—older songs, evidently—in which the singer imitates the speech of animals or spirit-persons. Literally hundreds of these “animal-speech songs” were collected among North American Indian tribes between 1890 and 1930. These do correspond to the Arctic Siberian style, and their wide distribution throughout North America strongly suggests that the style is very ancient indeed. This highlights the importance of accounting for the historical dimension in any comparative study, not only across the North Pacific region but all over North America. This type of song is quite possibly the very type of singing that Paleo-Indian peoples brought with them when they first populated the Americas, a type from which other styles of singing gradually developed. In other words, the significance of the Siberian recordings is perhaps best revealed by taking a historical approach to the field of Native American music as a whole.

Although this “musical archaeology” has some scientific value, it has other implications as well. Most important, it tends to validate the songs and dances of modern Native peoples. The older viewpoint implies that modern styles and functions of music are somehow less authentic than those of the 18th and 19th centuries. By contrast, a historical orientation underlines the fact that Native American culture has been changing and adapting to new circumstances for thousands of years.

Future research on the music of the North Pacific region therefore also needs to focus on contemporary musical activities. This is important for promoting cultural survival and increasing public awareness that Native cultures are by no means becoming “extinct.” But it is also necessary because this modern Native music has social, psychological, and even political functions that are historically significant in their own right.

I mentioned toward the beginning how ironic it seems that a recent interest in comparative studies of music has centered on the same region and some of the same questions that first absorbed Boas and his coworkers a century ago. But perhaps even more poignant is the extent to which the prospects for future research depend on documenting and building on what the Jesup team accomplished. Without a doubt, the pathway to future investigations can only begin where the trail of the Jesup Expedition came to an end.

Appendix A

A Preliminary Inventory of Phonographic Cylinder Collections, 1893 to 1933

This appendix provides an overview of early musical recordings, listed in roughly chronological order. Many of the collections listed here also include spoken texts. The following types of information are provided, as available: (a) collector’s name, (b) tribes or ethnic groups represented, (c) approximate dates of the recordings, (d) number of cylinders recorded, (e) area where the recordings were made and name of the institution or program sponsoring the research, (f) the current locations of tape duplicate recordings in (American) archives or libraries, and (g) published sources that provide musical transcriptions, translations of song texts, or other useful information on the recordings. The citations refer to Appendix B, which presents a selected annotated bibliography of these and other relevant sources.

As for cultural and geographic coverage, I have included recordings spanning an arc from the Ainu of northern Japan to the Yurok and other tribes of northwestern California. Eastern Arctic Inuit groups such as the Caribou, Labrador, and Greenland Eskimo are not listed, although I have included materials identified as McKenzie Delta Eskimo, Copper Eskimo, and Central Eskimo. The lack of early cylinder collections from Alaskan Eskimo groups was unexpected, considering that more recent types of recordings are fairly numerous.

In preparing the inventory and bibliography, I relied on several reference works rather than personally

consulting every source or collection. The list is intended to be comprehensive, but there are sure to be omissions and inaccuracies, particularly because I have summarized information that other sources generally provide in more detailed form. This appendix represents a preliminary phase of my own research in the region. The formidable task of listing more recent recordings will have to be addressed later, at which time it may also be possible to provide additions and corrections to the present inventory.

Abbreviations

The institutions and programs that store original recordings or duplicates are as follows:

AFC: American Folklife Center, Library of Congress, Washington, D.C.

AMNH: American Museum of Natural History, New York

ATM: Archives for Traditional Music, Indiana University, Bloomington

MJC: Melville Jacobs Collection, University of Washington, Seattle

1987:25; see also Roberts and Swadesh 1955.

Collector	Ethnic Group	Year(s)	Number of Cylinders	Collection Information	Storage Location	References
Benjamin I. Gilman	Kwakiutl [Kwakwaka'wakw]	1893	18 cylinders	Collected at the World's Columbian Exposition, with support from the Mary Hemenway Expedition.	Tape duplicates: AFC.	Gray 1988: 51.
Franz Boas and/or John Comfort Fillmore	Kwakiutl [Kwakwaka'wakw]	1893 or 1895	37 cylinders	Recorded at Fort Rupert, Vancouver Island, British Columbia, with support from the AMNH.	Tape duplicates: ATM (54-121-F).	Problems c identificati discussed i Seeger and 1987:24.
Franz Boas and George Hunt	Kwakiutl [Kwakwaka'wakw]	1895	91 cylinders	Collected by Boas and Hunt for the AMNH.	Tape duplicates: ATM (54-035-F).	Problems c identificati discussed i Seeger and 1987:24.
Franz Boas	Thompson River Indians	1895	42 cylinders	Recorded among various tribes of the Thompson River area, British Columbia, for the AMNH.	Tape duplicates: ATM (54-139-F).	Seeger and 1987:65; s the notatio other infor in Abrahan von Hornbr 1906.
Livingston Farrand	Quileute and Quinault	1898	44 cylinders	Collected from the Quileute (10 cylinders) and Quinault (34 cylinders) of Washington State for the JNPE.	Tape duplicates: ATM (54-127-F, 54-128-F).	Seeger and 1987:58-9

Collector	Ethnic Group	Year(s)	Number of Cylinders	Collection Information	Storage Location	References
John Reed Swanton	Haida	1900-01	?: May have involved cylinder recording	Research sponsored by the JNPE	No collection has been located	Swanton 1912 contains 106 song texts and translations. See also Swanton 1905
Waldemar Jochelson and Waldemar Bogoras	Northeast Siberia	1900-02	130 cylinders	Collected for the JNPE.	Tape duplicates: ATM (54-149-F).	Bogoras 1904-09, 1910, 1913; Jochelson 1908, 1924.
George A. Dorsey	Kwakiutl [Kwakwaka'wakw]	Ca. 1902	7 cylinders	Collected for the Field Museum, Chicago.		
Alfred Kroeber and others	Yurok, Hupa, Wiyot, Whilkut, Chilula, Karok, and Tolowa (northwestern California)	1902-27	495 cylinders	Collected as part of an ethnological survey of the Department and Museum of Anthropology at University of California. Contains spoken narratives and musical items.	Originals and tape duplicates: Phoebe Hearst Museum of Anthropology, University of California, Berkeley.	Keeling 1991; 1992.
Bronislaw Pilsudski	Ainu (Sakhalin Island)	1903	62 cylinders		National Museum of Ethnology	Description and notations for each recording are listed in National Museum of Ethnology 1987. See also Tanimot 1985.
John Reed Swanton	Tlingit	1903-04	32 cylinders	Collected for the BAE.	Tape duplicates, AFC.	Gray 1988:259-74; also see Swanton 1908, 1909.
Frank Speck	Northern Athapaskan	1908	1 cylinder		Tape duplicate, ATM	Seeger and Spear 1987.
Edward S. Curtis	Yakima (Washington State)	1909	14 cylinders		Tape duplicates: ATM.	Seeger and Spear 1987.

Collector	Ethnic Group	Year(s)	Number of Cylinders	Collection Information	Storage Location	References
Waldemar Jochelson	Aleut	1909-1910	97 cylinders mainly spoken narratives, but also 18 songs	Collected as part of the Riabushinski (Aleut-Kamchatka) Expedition of the Imperial Russian Geographical Society	Originals stored in St. Petersburg, Russia. Copies: Alaska Native Language Center; ATM (80-226-F).	Seeger and Spear 1987; see also Bergslund and Dirks 1990
Edward S. Curtis	Various tribes of British Columbia	1910	25 cylinders	Collected from the Clayoquot (11), Cowichan (3), Hisquiat (3), Kwakiutl (6) and Makah (2).	Tape duplicates: ATM (57-014-F).	Seeger and Spear 1987:80-3; see also Curtis 1907-30.
Edward Sapir	Nootka, Tlingit, and Tsimshian	1910, 1913, 1914	101 cylinders	From the Nottka [Nuu-chah-nulth] (99), Tlingit (1) and Tsimshian (1).	Tape duplicates: ATM (57-041-F).	Seeger and Spear 1987:25; see also Roberts and Swadesh 1955
Diamond Jenness	Copper Eskimo [Inuit]	1914-16	Number of cylinders unknown, 137 songs.		Whereabout unknown.	Songs are transcribed by Helen Roberts in Roberts and Jenness 1925
Leo Joaquim Frachtenberg	Kalapuya Indian (Oregon)	1916	9 4-inch cylinders	Recorded at Grand Ronde Reservation.	Tape duplicates: AFC. 4.	Gray 1988:140-4.
Herman Haeberlin	Puget Sound Salish	1916	11 cylinders	Identified as Snohomish (10) and Snoqualmie (1).		Noted and analyzed by Helen Roberts in Roberts and Haeberlin 1918.
Leo Joaquim Frachtenberg	Quileute	1916-17	82 4-inch cylinders		Tape duplicates: AFC. 54	Gray 1988:223-54
Marius Barbeau	Various tribes of Nass and Skeena River areas	1920-29	?: Number unknown, 300 songs		National Museum of Man Archives (Ottawa).	Barbeau 1933:101, 1934.
Hisao Tanabe	Ainu of Sakhalin Island	1923	?: Number unknown			Tanimoto 1985:78.

Collector	Ethnic Group	Year(s)	Number of Cylinders	Collection Information	Storage Location	References
Frances Densmore	Makah, Clayoquot, and Quileute	1923 and 1926	212 cylinders	From the Makah (153), Clayoquot (48), and Quileute (11).	Tape duplicates: AFC.	Gray 1988:101-19, 120-6, 152-206.
Frances Densmore	Various tribes near Chilliwack (British Columbia)	1926	88 cylinders	From the Halkomelem Coast Salish (21), Nitinat (33), Mainland Comox (24), and Squamish (10).	Tape duplicates: AFC.	Gray 1988:120-128-38, 207-1255-8.
Helen Roberts	Karok (Northwestern California)	1926	377 items, mostly songs, some spoken narratives.		Tape duplicates: AFC.	For information the recordings, see Gray and Schupman 1990:117-63. Songs are also notated and discussed in Keeling 1992.
Melville Jacobs	Klikitat Sahaptin and others (Washington State)	1929	19 cylinders	Recorded at Husum, Washington. Mainly Klikitat Sahaptin. Includes items identified as Molale, Klamath, and "Siletz Reservation."	Tape duplicates: MJC.	Seaburg 1982:4.
Melville Jacobs	Clackamas Chinook (Oregon)	1929-30	27 cylinders containing songs and spoken texts		Duplicate tapes: AFC; MJC.	Gray 1988:85-100; Seaburg 1982:45-50.
Takeshi Kitasato	Ainu of Hokkaido (Saru River area)	1931	?: 22 examples of Ainu music.		Part of a larger collection	Asakura and Tsuchida 1988
Arthur C. Ballard	Lushootseed Snoqualmie (Washington State)	1932	9 cylinders, each with many short items.	Recorded at Auburn, Washington.	Tape duplicates: MJC.	Seaburg 1982:2.

Collector	Ethnic Group	Year(s)	Number of Cylinders	Collection Information	Storage Location	References
Victoria Garfield	Tsimshian (British Columbia)	1932	18 cylinders	Recorded at Port Simpson, British Columbia.	Tape duplicates: MJC.	Seaburg 1982:53-6.
Melville Jacobs	Coos (Oregon)	1933	11 cylinders containing many short items on each	Recorded at Florence (Oregon) and Empire (Oregon). Mainly Coos of Hanis and Miluk dialects.	Tape duplicates: MJC.	Seaburg 1982:56-9.
Morris Swadesh	Nootka [Nuu-chah-nulth]	1933	1 cylinder containing a story with an embedded song		Tape duplicate: AFC.	Gray 1988:217-8.

Appendix B: Selected Annotated Bibliography of Publications on North Pacific Musical Sound Recordings and Existing Phonographic Collections

Abraham, Otto, and Erich M. von Hornbostel

1906 Phonographierte Indianermelodien aus Britisch-Columbia. In *Boas Anniversary Volume: Anthropological Papers Written in Honor of Franz Boas . . . Presented to Him on the Twenty-fifth Anniversary of His Doctorate*. Pp. 447-74. New York: G. E. Stechert. Trans. by Bruno Nettl in *Hornbostel Opera Omnia*, 1, Klaus Wachsmann et al., eds. The Hague: Martinus Nijhoff, 1975.

Contains analyses and transcriptions of 43 Thompson River Indian songs collected by Franz Boas and sent by him to Erich M. von Hornbostel of the Phonogramm Archiv of the University of Berlin. The fine transcriptions, quantitative analyses, and relatively slight information on the cultural contexts of the music make this a prime example of comparative methodology as practiced by the so-called Berlin school.

Asakura, Toshimitsu, and Shigeru Tsuchida

1988 *Kan Shinai-kai/Nihon-kai Shominzoku no Onsei/Eizo Shiryo no Saisei/Kaiseki* (Recreation and analysis of sound and visual sources on Native peoples of the Chinese and Japanese oceanic area). Sapporo: University of Hokkaido, Institute of Applied Engineering.

The recordings were made by Takeshi Kitasato (1870-1960), who sought to explore the origins of the Japanese language through a comparative study of several languages of the Pacific area. He collected 240 recordings in all, many containing songs and other musical items. Kitasato's recordings, including 22 items of Saru Ainu music collected in 1931, are catalogued in Appendix A.

Barbeau, C. Marius

1933 Songs of the Northwest. *Musical Quarterly* 19:101-11.

Contains 8 musical notations and translations of songs from a total collection of 300 songs recorded by Barbeau in 1920 and the years following. The transcriptions (by Barbeau and Ernest MacMillan) seem faithful, but tribal and linguistic identifications are not always clear. The songs were collected along the Nass and Skeena Rivers in British Columbia, and tribal groups are identified as Tahltan [Athapascan], Carrier [Athapascan], Gitskan [Penutian], and Tsimshian [Penutian]. An Asiatic origin for the songs is asserted but is not systematically demonstrated.

1934 Asiatic Survivals in Indian Songs. *Musical Quarterly* 20:107-16.

A continuation of topics touched on in Barbeau (1933), this includes five musical examples from the Nass River and Skeena River regions in northern British Columbia. The relationship of these songs to the musical traditions of Siberia, Japan, and China is argued mainly on the subjective impressions shared by Barbeau and a Chinese scholar.

- 1951 Tsimshian Songs. In *The Tsimshian: Their Arts and Music*. Viola E. Garfield, Paul S. Wingert, and Marius Barbeau, eds. Pp. 94–280. *Publications of the American Ethnological Society*, 18. New ed.: University of Washington Press, Seattle, 1966.
- Contains musical transcriptions, analyses, texts, and translations for 75 songs collected by James Teit in 1915 and by Barbeau, ca. 1920–29. The transcriptions are by Barbeau and Ernest MacMillan; the musical analyses are by Marguerite Béclard d'Harcourt. The analysis and musical examples are edited by George Herzog.
- Bergsland, Knut, and Moses L. Dirks, eds.**
- 1990 *Unangam Ungiikangin kayux Tunusangin/Unangam Uniikangis ama Tunuzangis: Aleut Tales and Narratives, Collected 1909–10 by Waldemar Jochelson*. Alaska Native Language Center. Fairbanks: University of Alaska.
- Mainly contains translations of spoken narratives but also includes a song text (“Blanket-Tossing Song,” pp. 486–7) and translations of 12 Eastern Aleut songs first published in Russian by Ioann Veniaminov in 1840 and 1846. Cylinder recordings of 18 songs collected by Jochelson are among the holdings at the Archives of Traditional Music, Indiana University (catalogue no. 80-226-F).
- Boas, Franz**
- 1887 Poetry and Music of Some American Tribes. *Science* 9:383–5.
- Contains three musical examples (with texts and translations) collected by Boas among the Eskimos of Baffin Island and another song (music, text, and translation) collected among Indians of British Columbia. Also includes general descriptions of the music in these areas.
- 1888a The Central Eskimo. In *Annual Report of the Bureau of American Ethnology (1884–1885)*, 6. Pp. 399–669. Washington, DC: Smithsonian Institution.
- Contains musical transcriptions and analyses for 25 melodies collected by Boas in 1883–84 and 4 notations reprinted from the journal of Captain Parry (1824) and other early sources. Also includes general comments on poetry and music (pp. 648–58) and descriptions of dance houses, drum construction, and drum-playing techniques. The research was done at Cumberland Sound and Davis Strait.
- 1888a On Certain Songs and Dances of the Kwakiutl of British Columbia. *Journal of American Folk-Lore* 1(1):49–64.
- Includes four musical notations (with texts and translations) collected by Boas in 1886 and 1887. Two other song texts are given in translation.
- 1888b Chinook Songs. *Journal of American Folk-Lore* 1(3):220–6.
- Contains 39 song texts and translations, including a Tlingit example. Also includes musical transcriptions for three of the songs. The research was conducted in 1886.
- 1891 Second General Report of the Indians of British Columbia. In *Report of the Meeting of the British Association for the Advancement of Science (in 1890)*, 90. Pp. 562–715.
- Includes 1 melody and text identified as Lku'ngen Songish (p. 581), 15 Nootka [Nuu-chah-nulth] melodies and texts, with translations (pp. 588–603), and 20 Kwakiutl song texts and translations (pp. 625–32). All of the material was collected by Boas in 1889.
- 1894a Chinook Texts. *Bureau of American Ethnology Bulletin* 20.
- Contains 12 song texts (and translations) with rhythmic notations for each (pp. 116–8, 144, 146, 150–1, 192, 234–5).
- 1894b Eskimo Tales and Songs. *Journal of American Folk-Lore* 7:45–50.
- Includes song texts and translations for six songs, five of which are also included in Boas 1888b. Lists and explains certain shamanic words in the songs.
- 1896 Songs of the Kwakiutl Indians. *Internationales Archiv für Ethnographie*, 9 (suppl.):1–9. Leiden: E. J. Brill.
- Contains notations of five melodies and texts (with translations) of songs that Boas transcribed by ear and from phonographic recordings collected by John C. Fillmore. Six other song texts are given without notations.
- 1897 The Social Organization and the Secret Societies of the Kwakiutl Indians. In *Report of the United States National Museum for 1895*. Pp. 562–715. Reprint: Johnson Reprint Corporation, New York, 1975.
- Contains texts and translations for 12 songs (pp. 355 ff.). Provides verbal descriptions of songs and dances used in various ceremonies (pp. 431 ff.). Also includes transcriptions of 36 songs and texts, and texts only for 109 songs (pp. 665 ff.).
- 1898 The Mythology of the Bella Coola Indians. *The Jesup North Pacific Expedition*, vol. 1, pt. 2, pp. 25–127. *Memoirs of the American Museum of Natural History*, 2. New York. Reprint: AMS Press, New York, 1975.
- Includes notations of four songs, three with texts (pp. 71, 82, 93, 94). Many other song texts and translations are also given (passim).

1901 Kathlamet Texts. *Bureau of American Ethnology Bulletin* 26.

Contains texts and translations of four songs, one also transcribed in staff notation and two with rhythmic notation only (pp. 21, 24, 65, 154).

1902 Tsimshian Texts. *Bureau of American Ethnology Bulletin* 27. Washington, DC.

Contains texts and translations of eight songs, three also transcribed in staff notation and two with rhythmic notation only (pp. 11, 63, 222, 224, 228, 231, 232, 233).

Boas, Franz, and George Hunt

1905 Kwakiutl Texts. *The Jesup North Pacific Expedition*, vol. 3. *Memoirs of the American Museum of Natural History*, 5. Leiden: E. J. Brill; New York: G. E. Stechert.

Includes a section on "Songs" (pp. 475–91).

Boas, Franz, and Henry Rink

1889 Eskimo Tales and Songs. *Journal of American Folk Lore* 2:123–31.

Provides musical notations, song texts, and translations for two songs. Also includes translations of origin myths and discusses language dialect relationships. Based on fieldwork done at Cumberland Sound in 1885.

Bogoras, Waldemar

1904–09 The Chukchee. *The Jesup North Pacific Expedition*, vol. 7, pts. 1–3. *Memoirs of the American Museum of Natural History*, 11. Leiden: E. J. Brill; New York: G. E. Stechert. Reprint: AMS Press, New York, 1975.

Contains detailed descriptions of shamanistic practices (passim). Of particular interest is the account of how Chukchi shamans were able to throw their spirit-voices like ventriloquists in shaman séances that Bogoras witnessed (pp. 435–9). Bogoras states that he captured these effects in cylinder recordings he made (p. 436).

1910 Chukchi Mythology. *The Jesup North Pacific Expedition*, vol. 8, pt. 1. *Memoirs of the American Museum of Natural History*, 12. Leiden: E. J. Brill; New York: G. E. Stechert. Reprint: AMS Press, New York, 1975.

The section on "Songs" (pp. 138–45) contains interlinear and free translations for 16 song texts. Two (pp. 142–4) are identified as shaman songs and are described in greater detail than others.

1913 The Eskimo of Siberia. *The Jesup North Pacific Expedition*, vol. 8, pt. 3. *Memoirs of the American Museum of Natural History*, 12. Leiden: E. J. Brill; New York: G. E. Stechert. Reprint: AMS Press, New York, 1975.

The section on "Songs" (pp. 437–52) contains interlinear and free translations for 43 song texts. Various types of songs are represented, but shamanistic texts are particularly numerous (12 examples). One set of six shamans' songs (pp. 445–7) presents incantations connected with the walrus hunt; the other six are sung at Winter Ceremonials.

Burlin, Natalie (Curtis)

1907 *The Indians' Book: An Offering by the American Indians of Indian Lore, Musical and Narrative, to Form a Record of the Songs and Legends of Their Race*. New York: Harper. Expanded 2d ed., 1923; reprints of 2d ed.: Dover, New York, 1950, 1968.

Contains 149 melodies and texts from various tribes, mostly with translations or brief explanations of content. The author notated the songs by ear, without use of a recording device. Includes two Kwakiutl [Kwakwaka'wakw] examples.

Curtis, Edward S.

1907–30 *The North American Indian, Being A Series of Volumes Picturing and Describing the Indians of the United States and Alaska*. Frederick Webb Hodge, ed. 20 vols. Cambridge, MA: University Press. Reprint: Landmarks in Anthropology series, Johnson Reprint Corporation, New York, 1970.

A vast storehouse of information with notations of songs from various tribes or cultures. Vol. 8 contains two Chinook melodies (pp. 96–98, 100). Vol. 9 contains five Cowichan melodies, one with text and translation, one with English translation only (pp. 73, 176–8); two Twana melodies (pp. 98, 111); and four Clallam melodies (pp. 179–80). Vol. 10 contains 23 Kwakiutl melodies, 22 with texts and translations, 1 with translation only (pp. 187–91, 195–6, 200, 223–4, 244–5, 311–26). Vol. 11 contains 9 Nootka melodies, 3 with texts and translations, 5 with translations only (pp. 13, 37–8, 41, 48, 52–3, 61, 66–7, 81–2, 92–3), and 5 Haida melodies, 1 with text and translation, 4 with translations only (pp. 123–4, 140–1, 147, 191–3). The songs were collected by Curtis and later transcribed by various other persons.

Densmore, Frances

1939 Nootka and Quileute Music. *Bureau of American Ethnology Bulletin* 74.

Contains musical notations for 211 songs collected by Densmore in 1923 and 1926. Some texts are given in English, but native texts are lacking. The following groups are identified: Makah (138 songs), Clayoquot (52), Quileute (11), unspecified of Vancouver Island (7), Nootka (1), Quinault (1), and Yakima (1).

1943 Music of the Indians of British Columbia. *Bureau of American Ethnology Bulletin* 136, *Anthropological Papers*, 27.

Contains musical notations of 98 songs from various tribes. Each song is analyzed and described. The collection is compared with others the author has made using a (statistical) tabular approach.

Eels, Myron

1879 Indian Music. *American Antiquarian* 1:249–53.
Describes music and instruments observed by the author in 1875. Includes 24 melodies transcribed by ear and identified as follows: Clallam (10), Twana (12), and unspecified (2).

Gillis, Frank J.

1984 The Incunabula of Instantaneous Ethnomusicological Sound Recordings, 1890–1910: A Preliminary List. In *Problems and Solutions: Occasional Essays in Ethnomusicology Presented to Alice M. Moyle*. J. Kassler and J. Stubbington, eds. Pp. 323–55. Sydney: Hale and Iremonger.

A useful guide to the location of early cylinder recordings in archives. The focus is worldwide, but American Indian recordings predominate and are listed by area and tribe (pp. 327–39).

Gray, Judith A., ed.

1988 Northwest Coast/Arctic Indian Catalog. In *The Federal Cylinder Project: A Guide to Field Cylinder Collections in Federal Agencies*, vol. 3. *Great Basin/Plateau Indian Catalog and Northwest Coast/Arctic Indian Catalog*. Pp. 79–288. Washington, DC: American Folklife Center, Library of Congress.

Lists and describes contents of cylinder recordings in 20 collections. The annotated listing for each is preceded by an introduction providing background information on the recordings themselves and on sources of transcriptions, translations, and other documentation. Tribes represented are identified as Carrier Indian, Clackamas Chinook, Clayoquot, Comox (Mainland), Eskimo (Polar), Halkomelen, Ingalik Indian, Kalapuya, Kwakiutl, Makah, Nitinat, Nootka, Quileute, Shasta, Squamish, Tlingit, Tsimshian, Tututni, and Upper Umpqua.

Gray, Judith, and Edwin Schupman, eds.

1990 California Indian Catalogue. In *The Federal Cylinder Project*, vol. 5. *California Indian Catalogue, Middle and South American Catalogue, Southwestern Catalogue*. Pp. 1–328. Washington, DC: American Folklife Center, Library of Congress.

Lists and describes the contents of early cylinder recordings in 34 collections, most notably those of John Peabody Harrington and Helen Heffron Roberts.

Herzog, George

1933 The Collections of Phonograph Records in North America and Hawaii. *Zeitschrift für vergleichende Musikwissenschaft* 1:58–62.

Indicates the locations of about 12,428 cylinder recordings in collections in the 1930s. For decades, this was the only such guide in existence, and it still remains useful because of its organization (by culture area and tribe) and its bibliography.

Jochelson, Waldemar

1908 The Koryak. *The Jesup North Pacific Expedition*, vol. 6, pts. 1–2. *Memoirs of the American Museum of Natural History*, 10. Leiden: E. J. Brill; New York: G. E. Stechert.

1924 The Yukaghir and the Yukaghirized Tungus. *The Jesup North Pacific Expedition*, vol. 9, pt. 2 [Religion, Folklore, Language]. *Memoirs of the American Museum of Natural History*, 13. Leiden: E. J. Brill; New York: G. E. Stechert. Reprint: AMS Press, New York, 1975.

Contains detailed discussions of beliefs and practices related to shamanism (pp. 162–95, 196–218, 234–8). Also includes a section on “Songs” (pp. 310–3), with interlinear translations of five song texts.

Keeling, Richard

1991 *A Guide to Early Field Recordings (1900–1949) at the Lowie Museum of Anthropology*. Berkeley and Los Angeles: University of California Press.

An annotated catalogue of recordings collected on 2,713 cylinders between 1900 and 1938. The collection focuses primarily on tribes of California and includes recordings of northwestern tribes (Yurok, Hupa, Karok, Tolowa, Wiyot) that clearly belong to the North Pacific culture area.

1992 *Cry for Luck: Sacred Song and Speech among the Yurok, Hupa, and Karok Indians of Northwestern California*. Berkeley and Los Angeles: University of California Press.

Contains notations, analyses, and ethnographic information relating to early cylinder recordings collected by Kroeber and others circa 1901–08.

Kroeber, Alfred

1925 Handbook of the Indians of California. *Bureau of American Ethnology Bulletin* 78. Washington, DC. Reprint: Dover Publications, New York, 1976.

A comprehensive overview of California Indian cultures, mainly in their precontact forms. Does not contain notations, but provides much information on the ritual contexts and cultural background of music-making. Also includes translations of song texts collected among many groups.

National Museum of Ethnology

1987 B. Piusuzuki Roka no Rokuon Naiyo (Catalogue of recordings by B. Pilsudski). *Kokuritsu Minzoku Hakubutsukan Kenkyu Hokoku Bessatsu*, 5 (Research Report of the National Museum of Ethnology, 5). Osaka, Japan.

A detailed documentation, in Japanese, of cylinder recordings collected by Pilsudski among the Ainu in 1903. Includes musical notations, translations, and other information on each item recorded. The musical notations are by Kazuyuki Tanimoto.

Nelson, Edward William

1899 The Eskimo about Bering Strait. *Annual Report of the Bureau of American Ethnology (1896-1897)*, 18, pt. 1. Washington, DC: Smithsonian Institution.

Contains descriptions of songs, dances, and instruments (pp. 347-57). Includes one musical example (notated by Bishop Seghers in 1879) and three song texts and translations.

Pilsudski, Bronislaw

1912 *Materials for the Study of Ainu Language and Folklore*. J. Rozwadowski, ed. Krakow.

Pilsudski (1866-1918) was sentenced to 15 years of hard labor and exile on Sakhalin Island for his political activities. While there, he became involved in ethnographic research on the Ainu and other Northeast Asian groups. A museum ethnographer by orientation, he not only collected artifacts but also gathered an enormous amount of folkloric data, including recordings on wax cylinders.

Roberts, Helen Heffron, and Herman K. Haeberlin

1918 Some Songs of the Puget Sound Salish. *Journal of American Folklore* 331:496-520.

Contains notations, texts, translations, and analyses for 11 songs collected by Haeberlin in 1916; 10 are from the Snohomish and 1 from the Snoqualmu [Snoqualmie]. The transcriptions and analyses are by Roberts, who also discusses general characteristics of the music.

Roberts, Helen Heffron, and Diamond Jenness

1925 Songs of the Copper Eskimo. *Report of the Canadian Arctic Expedition (1913-1918)*, 14. Ottawa: F. A. Ackland.

Contains musical notations, texts, translations, and detailed analyses for 137 songs collected on cylinders by Jenness between 1914 and 1916. Groups represented are Copper Eskimo (113 songs), Mackenzie River Eskimo (12), Inland Hudson Bay Eskimo (7), and Iñupiat Eskimo of Point Hope, Alaska (5). Each song is analyzed separately, and various

types of songs are described or defined. The first chapter contains a musical comparison of dance song styles and compares the style of dance songs with that of weather incantations.

Roberts, Helen Heffron, and Morris Swadesh

1955 Songs of the Nootka Indians of Western Vancouver Island. *Transactions of the American Philosophical Society* 45(3):199-327.

Contains notations and detailed analyses.

1912 Haida Songs. *Publications of the American Ethnological Society*, 3. Pp. 1-63.

Contains 106 song texts and translations collected by Swanton in 1900 and 1901.

Tanimoto, Kazuyuki

1985 A Study on the Process of Chronological Changes in the Music of the Sakhalin Ainu Recorded by B. Pilsudski. In *International Symposium on B. Pilsudski's Phonographic Records and the Ainu Culture*. Pp. 78-85. Sapporo: Hokkaido University of Education.

Discusses transformations in Ainu music and the difficulty of identifying certain items among the Pilsudski recordings.

Teit, James Alexander

1900 The Thompson Indians of British Columbia. *The Jesup North Pacific Expedition*, vol. 1, pt. 4, pp. 163-392. *Memoirs of the American Museum of Natural History*, 2. New York. Reprint: AMS Press, New York, 1975).

Chapter 4, "Art, by Franz Boas, contains a section on "Music" (pp. 383-5) that discusses types of songs and instruments.

Notes

1. George Herzog (1901-84) and Helen Roberts (1888-1985) established themselves as leading theorists in comparative research on North American Indian music through several important publications in the 1930s. Herzog entered the field as a student of Erich von Hornbostel in Berlin but later completed his doctorate under Boas in 1931. Roberts claimed that she entered the field of "primitive music" at the suggestion of Boas in 1918 (Frisbie 1989:99). The contributions of Kroeber and Sapir to ethnomusicology are less well understood. Kroeber initiated the study of music among the Indians of California and assembled a vast collection of wax-cylinder recordings from all over the region between 1900 and 1938. His "Handbook of the Indians of California" (1925) provides trans-

lations of many song texts and much information on the cultural contexts of the music (although it does not include musical notations or analyses as such). He also mapped the musical areas of California in his publication on the distribution of culture elements (Kroeber 1936). Sapir's technical abilities as a musicologist are clearly demonstrated by his skilled notations in the essay "Song Recitative in Paiute Mythology" (Sapir 1910). This paper—only one of several articles in which Sapir dealt with songs or song texts—was vastly ahead of its time as a study in musical semiotics or symbolism.

2. The documentation that is currently available from the Archives of Traditional Music, Indiana University, provides a listing of the recordings but does not include other types of information that would greatly enhance their value as ethnological documents. What we urgently need now is a published guide to the Jesup Expedition musical collection that would not only list the recordings but would also provide references to translations and descriptions of related activities in published writings and manuscripts. The excellent transcriptions by Herzog should also be included, and there should be introductory essays discussing the history of the research and providing general information on Native cultures of the North Pacific region.

3. The Russian recordings seem to have been collected by Waldemar Bogoras [or his wife, Sofia Bogoras—ed.] in the village of Markovo on the Anadyr River and at Mariinsky Post, at the mouth of the river, near the Gulf of Anadyr. They include various genres such as epic songs, Christmas carols, love songs, and instrumental pieces.

4. ATM cylinder 4540. The text (which seems to be at least partially composed of vocables) is not transcribed because it contains many slight changes. The melody is simplified for clarity, although one major variant is indicated in parentheses and other variations by the use of smaller note heads. The melody is written a major sixth higher than what is heard on the recording. The rapid drum accompaniment does not seem to be precisely coordinated with the vocal part.

5. Professor Sheikin presented a paper entitled "Sound Culture of the Tungusic Groups" at the International Symposium on Comparative

Studies of the Music, Dance, and Games of Northern Peoples, Sapporo, Japan, January 20–25, 1992. He kindly shared with me a tape containing 17 items that he had recorded among various Tungus groups in Siberia since the 1970s.

6. There are 52 pages of notations in this important manuscript. They mainly focus on the Chukchi recordings, but Herzog also transcribed some songs of other ethnic groups. The manuscript is available at the Department of Anthropology, AMNH.

7. The Ainu vocal games (*rekukkara*) are amply documented in many sources (Fitzhugh and Dubreuil 1999). An example from the Nanay of the Amur River area was given in the lecture by Yuri Sheikin in 1992 (see note 5).

8. This profile follows an outline that I have found useful in previous comparative research. The following aspects are considered: (1) genre, function, or symbolism; (2) vocal quality or timbre, including loudness; (3) presence of words or vocables, text-setting, and repetition of text; (4) musical organization or texture; (5) musical form or structure, including phrase length; (6) melodic range; (7) melodic contour or direction; (8) scale, particularly number of tones in scale; (9) rhythm, especially meter and tempo; and (10) other notable tendencies.

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1936 *Research in Primitive and Folk Music in the United States: A Survey*. Washington, DC: American Council of Learned Societies.
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1987 Early Field Recordings: A Catalogue of Cylinder Collections at the Indiana University Archives of Traditional Music. Bloomington: Indiana University Press.
- Stumpf, Carl**
1886 Lieder der Bellakula-Indianer. *Vierteljahrschrift für Musikwissenschaft* 2:405–26. Reprint: *Sammelbande für Vergleichende Musikwissenschaft*, vol. 1, *Abhandlungen zur Vergleichenden Musikwissenschaft*, Carl Stumpf and Erich M. von Hornbostel, eds., pp. 87–103 (Munich, 1922).

A Jesup Bibliography

Tracking the Published and Archival Legacy of the Jesup Expedition

IGOR KRUPNIK

This Jesup Bibliography was started in 1992 as a special component of the *Jesup 2* activities at the Arctic Studies Center, Smithsonian Institution (see Fitzhugh and Krupnik, this volume). Originally, it was wanted merely as a technical resource, a shared database for listing and checking references for the various *Jesup 2* statements, flyers, memos, symposium papers, and publications. As its size expanded through years of editing and library research, the bibliography eventually took on a special value of its own. It emerged as a valuable chronicle of the many efforts related to the Jesup North Pacific Expedition (JNPE), as well as of the numerous later publications. We accordingly decided to add the bibliography to this review of the diverse legacies of the monumental JNPE project.

The initial practical purpose of the bibliography is still very much reflected in its present structure. Instead of being a single alphabetically or chronologically arranged list of publications and documentary sources related to Jesup Expedition activities, the bibliography is organized into 13 thematic sections:

1. Original volumes in the *Jesup North Pacific Expedition* series, 1–11 (*Memoirs of the American Museum of Natural History*, 2–15), 1898–1930
2. Translations or modified versions of the original JNPE volumes
3. Manuscripts submitted to the JNPE series but not published within that series
4. Contributions to the JNPE series advertised but never produced
5. Contemporary accounts and reports of JNPE activities

6. Reports on and reviews of JNPE publications and collections

7. JNPE-based or JNPE-related publications other than those published in the main JNPE series, 1897–1902 to present

8. Major post-JNPE publications that were regarded as “extensions” of the main JNPE venture, 1897–1902

9. Selected comparative publications by JNPE members based on data collected during and outside the JNPE surveys

10. Unpublished manuscripts related to the JNPE

11. Bibliographies; reviews of manuscript, museum, and archival collections related to JNPE activities

12. Selected post-1960 publications related to the JNPE and its participants

13. Biographies, obituaries, and major personal essays on JNPE participants.

My work in compiling the Jesup Bibliography was greatly facilitated by the availability of several extensive bibliographical guides focused on the Arctic, Siberia, or the Northwest Coast. Among them are Marie Tremaine, ed., *Arctic Bibliography*, vols. 1–12, 1953–65; Jakobson et al., *Paleosiberian Peoples and Languages*, 1957; and Wayne Suttles, ed., *Handbook of North American Indians*, vol. 7: *Northwest Coast*, 1990. Personal bibliographies are also available for most of the JNPE members (see, in section 11, Vinnikov 1935 on Waldemar Bogoras and, in section 13, Andrews et al. 1943 on Boas, Leechman 1949 on Harlan I. Smith, and Nichols 1940 on John R. Swanton). Still, many of the early contributions on JNPE activities are rather hard to trace. Some were published anonymously, and many others were written (or at least signed) by people who

were not directly involved in the JNPE project. This group of references will obviously expand with further searches.

A special aim of this Jesup Bibliography was to compile, as a single common legacy, the many contributions derived from or based on the JNPE's North American and Siberian surveys. This pattern was pioneered by the original JNPE series, but the format of shared Siberian–North American contributions was neither extended nor reproduced in further publications under the JNPE agenda, and no common bibliography of JNPE-based printed contributions was ever assembled. In fact, the format of shared publications was reestablished only 70 years after the expedition ended, through several fairly recent Soviet–North American symposia and through exhibit projects in the Arctic–North Pacific field. Examples (listed in section 12) include Fitzhugh and Chaussonnet 1994; Fitzhugh and Crowell 1988; Gurvich 1981; Michael 1979; and Michael and VanStone 1983; see also Krupnik 1998.

It comes as no surprise that several relevant Russian papers from about 1910 through the 1930s, scattered through various Russian periodicals, remain unknown to or unused by the many American students of Boasian ethnography. The same is even truer with regard to the numerous unpublished or archival JNPE resources, North American and Russian alike. The few recent historical reviews of JNPE efforts, whether by western or by Russian scholars, still tell basically only one side of the trans-Pacific story and rely on either North American or Russian resources.

Despite years of effort, the Jesup Bibliography in its current version is neither a complete nor a finished product. At present, its Siberian material is far more comprehensive than that for North America. I believe that this “Siberian bias” is a short-lived phenomenon, but it may be an additional asset for North American readers, who usually have better knowledge of and easier access to the North American JNPE resources than to the Siberian materials.

Certain gaps in the present format of the Jesup Bibliography were deliberately left to avoid interfering with

individual research in progress. This is particularly true for the many manuscript collections of Franz Boas and his local North American collaborators (Hunt, Teit, Tate, Edenshaw, etc.). The Boas-Hunt archival legacy is a subject of special study by Judith Berman, and it is covered extensively in her paper in this volume. In the same category is Sergei Kan's ongoing project on the intellectual biography of Leo Shternberg, including interactions with Boas and with Shternberg's Russian friends, Bogoras and Jochelson (see Kan, this volume). As a result, section 10, Unpublished Manuscripts, is basically limited to the archival collections of the JNPE Russian participants, Waldemar Bogoras, Waldemar Jochelson, and Dina Jochelson-Brodsky. It will have to be expanded substantially, to include the unpublished records of several other JNPE team members, including Franz Boas himself.

I also made a deliberate effort to keep section 12, Selected Post-1960 Publications Related to the JNPE and Its Participants, under a very tight limit. This section could be easily expanded into a much larger bibliographical summary of its own. It is also a major work in progress that is currently being advanced by many individual researchers, both under and outside the main *Jesup 2* effort. As time goes on, more old and new references will be added to the current list. The result may be an expanded and updated version of the Jesup Bibliography, but never a “final” one. Eventually, it will serve as an appropriate summary of the *Jesup 2* efforts for a new generation of “Jesup” researchers.

1. *The Jesup North Pacific Expedition (JNPE) Series/Memoirs of the American Museum of Natural History (AMNH), 1898–1930*

The JNPE proceedings were initially produced as separate issues (“parts”) organized into “volumes.” They were later bound into numbered volumes, preserved in today's major library collections. Some of the original volume covers still show the series structure, as well as prices for individual issues. All of the original JNPE volumes were reprinted by AMS Press in 1975.

JNPE, VOL. 1: 1898–1900 (*AMNH MEMOIRS*, 2)

Boas, Franz. The Jesup North Pacific Expedition. Pt. 1 (1898), pp. 1–12.

Boas, Franz. Facial Paintings of the Indians of Northern British Columbia. Pt. 1 (1898), pp. 13–24.

Boas, Franz. The Mythology of the Bella Coola Indians. Pt. 2 (1898), pp. 25–127.

Smith, Harlan I. The Archaeology of Lytton, British Columbia. Pt. 3 (1899), pp. 129–61.

Teit, James A. The Thompson Indians of British Columbia. Pt. 4 (1900), pp. 163–392; with conclusions by Franz Boas.

Farrand, Livingston. Basketry Design of the Salish Indians. Pt. 5 (1900), pp. 393–9.

Smith, Harlan I. Archaeology of the Thompson River Region, British Columbia. Pt. 6 (1900), pp. 401–42.

JNPE, VOL. 2: 1900–07 (*AMNH MEMOIRS*, 4)

Farrand, Livingston. Traditions of the Chilcotin Indians. Pt. 1 (1900), pp. 1–54.

Smith, Harlan I., and Gerard Fowke. Cairns of British Columbia and Washington. Pt. 2 (1901), pp. 55–75.

Farrand, Livingston, and W. S. Kahnweiler. Traditions of the Quinault Indians. Pt. 3 (1902), pp. 77–132.

Smith, Harlan I. Shell-Heaps of the Lower Fraser River, British Columbia. Pt. 4 (1903), pp. 133–91; with a contribution by Franz Boas, On Crania of Lower Fraser River Indians (pp. 188–90).

Teit, James A. The Lillooet Indians. Pt. 5 (1906), pp. 192–300; with a contribution by Franz Boas, Notes on the Lillooet Indians (pp. 292–300).

Smith, Harlan I. Archaeology of the Gulf of Georgia and Puget Sound. Pt. 6 (1907), pp. 301–441; with contributions by Franz Boas, On Petroglyphs of British Columbia (pp. 324–6, 329, 330); Clubs Made of Bone of Whale, from Washington and British Columbia (pp. 403–12).

Teit, James A. The Shuswapp. Pt. 7 (1909), pp. 443–813; with contributions by Franz Boas, On the Basketry of the Shuswap Indians (pp. 477–88); On the Basketry of the Chilkotin Indians (pp. 767–73).

JNPE, VOL. 3: 1905 (*AMNH MEMOIRS*, 5)

Boas, Franz, and George Hunt. Kwakiutl Texts. 532 pp.

JNPE, VOL. 4: 1902 (*AMNH MEMOIRS*, 6)

Laufer, Berthold. The Decorative Art of the Amur Tribes. Pt. 1 (1902), pp. 1–79.

JNPE, VOL. 5: 1905–09 (*AMNH MEMOIRS*, 8)

Swanton, John R. Contributions to the Ethnology of the Haida. Pt. 1 (1905), pp. 1–300.

Boas, Franz. The Kwakiutl of Vancouver Island. Pt. 2 (1909), pp. 301–522.

JNPE, VOL. 6: 1908 (*AMNH MEMOIRS*, 10)

Jochelson, Waldemar. The Koryak (1908). Pt. 1: Religion and Myths, pp. 13–382; pt. 2: Material Culture and Social Organization, pp. 383–842; with a contribution by Franz Boas [not acknowledged in the text], Ornamentation of Dress, Bags and Baskets, Rugs, Drawings and Writing (pp. 679–723).

JNPE, VOL. 7: 1904–09 (*AMNH MEMOIRS*, 11)

Bogoras Waldemar. The Chukchee. Pt. 1 (1904): Material Culture, pp. 1–276; pt. 2 (1907): Religion, pp. 277–536; pt. 3 (1909): Social Organization, pp. 537–733.

JNPE, VOL. 8: 1910–13 (*AMNH MEMOIRS*, 12)

Bogoras, Waldemar. Chukchee Mythology. Pt. 1 (1910), pp. 1–197.

Teit, James A. Mythology of the Thompson Indians. Pt. 2 (1912), pp. 199–416.

Bogoras, Waldemar. The Eskimo of Siberia. Pt. 3 (1913), pp. 417–56.

JNPE, VOL. 9: 1910–26 (*AMNH MEMOIRS*, 13)

Jochelson, Waldemar. The Yukaghir and the Yukaghirized Tungus. Pts. 1–3 (untitled): pt. 1 [The Land, the Tribe, and Social Life] (1910), pp. 1–133; pt. 2 [Religion, Folklore, Language] (1924), pp. 135–342; pt. 3 [Material Culture] (1926), pp. 343–469.

JNPE, VOL. 10: 1906–08 (*AMNH MEMOIRS*, 14)

Boas, Franz, and George Hunt. Kwakiutl Texts (Second Series). Pt. 1 (1906), pp. 1–269.

Swanton, John R. Haida Texts, Masset Dialect. Pt. 2 (1908), pp. 273–812.

JNPE, VOL. 11: 1930 (*AMNH MEMOIRS*, 15)

Oettking, Bruno. Craniology of the North Pacific Coast. Pt. 1 (1930). 391 pp. text, 93 pp. tables, xii pp. plates.

2. Translations or Modified Versions of the Original *JNPE* Volumes

BOGORAZ-TAN, VLADIMIR G. [BOGORAS, WALDEMAR]

1934 *Chukchi* (The Chukchee). Pt. 1. Leningrad: Institut narodov Severa [Russian trans. of *JNPE*, vol. 7, Introduction and pt. 3, 1909; rev.].

1939 *Chukchi. Religiiia* (The Chukchee. Religion). Leningrad: Institut narodov Severa [Russian trans. of *JNPE*, vol. 7, pt. 2, 1907; rev.].

1991 *Material'naia kul'tura chukchei* (The Chukchee. Material culture). Moscow: Nauka [Russian trans. of *JNPE*, vol. 7, pt. 1, 1904; rev.].

IOCHEL'SON, VLADIMIR I. [JOHELSON, WALDEMAR]

1997 *Koriaki. Material'naia kul'tura i sotsial'naia organizatsiia* (The Koryak. Material culture and social

organization). St. Petersburg: Nauka [Russian trans. of *JNPE*, vol. 6, pt. 2, 1908].

In press *lukagiry i iukagirizirovannye tungusy* (The Yukaghir and the yukaghirized Tungus). Vladimir Kh. Ivanov-Unarov and Zinaida Ivanova, eds. Yakutsk: Sapipolis [Russian trans. of *JNPE*, vol. 9, pts. 1–3, 1910–26].

3. Manuscripts Submitted to the *JNPE* Series but Not Published within That Series

JOCHELSON-BRODSKY, DINA

n.d. [On the Anthropometry of the Peoples of North-east Siberia] (s.a.). 118 pp., with tables. English manuscript prepared for publication as *JNPE*, vol. 11, pt. 2; on file at the Department of Anthropology, AMNH; copies at the Arctic Studies Center, Smithsonian Institution; Department of Anthropology, University of Tennessee, Knoxville. See announcement: *JNPE*, vol. 11, pt. 1, cover page (listed as Dina B. Jochelson. *Anthropometry of Siberia*); Waldemar Jochelson. *American Anthropologist* 32(2): 377 (1930).

SHTERNBERG, LEO

n.d. The Social Organization of the Gilyak. Manuscript submitted for publication as *JNPE*, vol. 4, pt. 2. 343 pp. Original copy on file at the Department of Anthropology, AMNH; Russian version published in individual chapters in Lev Shternberg. *Giliaki, orochoi, gol'dy, negidal'tsy, ainy* (The Gilyak, Oroch, Gol'd, Negidal, and Ainu). Ian P. Al'kor (Koshkin), ed. Khabarovsk: Dal'giz, 1933. First advertised as: Leo Sternberg, Tribes of the Amur River, *JNPE*, vol.4, pt. 1, 1913 (cover announcement for vol. 4, pt.2 published in *JNPE*, vol. 8, 1913). Recent publication: Lev Shternberg, The Social Organization of the Gilyak, Bruce Grant, ed., *Anthropological Papers of the American Museum of Natural History*, 82 (New York, 1999) (see Kan, this volume).

4. Contributions to the *JNPE* Series Advertised but Never Produced

BOAS, FRANZ

n.d. Summary and Final Results [of the Jesup North Pacific Expedition]. Advertised as *JNPE*, vol. 12 (see Boas 1905:94. section 5, this chapter; *JNPE*, vol. 5, pt. 1, cover), or as *JNPE*, vol. 11, pt. 3 (see *JNPE*, vol. 11, pt. 1).

BOGORAS, WALDEMAR

n.d. The Kamchadal. Advertised as *JNPE*, vol. 6, pt. 3 (see Boas 1905:94; *JNPE*, vol. 5, pt. 1, cover).

LAUFER, BERTHOLD

n.d. The Gold. Advertised as *JNPE*, vol. 4, pt. 2 (see Boas 1905:94; *JNPE*, vol. 5, pt. 1, cover).

5. Contemporary Accounts and Reports of *JNPE* activities

AMNH (AMERICAN MUSEUM OF NATURAL HISTORY)

1900 The Jesup North Pacific Expedition. Departure of Two of Its Members for Northeastern Asia. *American Museum Journal* 1(2):30–1.

1901 Recent Work of the Department of Anthropology. *American Museum Journal* 1(12):164–6.

[BOAS, FRANZ]

1897 Proposed Explorations on the Coasts of the North Pacific Ocean. *Science*, n.s. 5(116):455–7 [anonymous; presumably written by Boas].

1902 Recent Ethnological Work of the Museum. *American Museum Journal* 2(7):63–8 [presumably written by Boas; pp. 66–8 on the *JNPE*].

BOAS, FRANZ

1897 The Jesup Expedition to the North Pacific Coast. *Science*, n.s. 6(145):535–8.

1897 Die Jesup-Boas-Expedition nach Nordwest-Amerika. *Globus* 21: 342.

1898 Jesup Expedition nach der nordpazifischen Küste [letter, May 27]. In *Verhandlungen der Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte*, 30. Pp. 257–8.

1900 The Jesup North Pacific Expedition. In *Verhandlungen des 7. Internationalen Geographen-Kongresses in Berlin, 1899*. Pp. 678–85.

1900 Ethnographical Album of the North Pacific Coasts of America and Asia. *Jesup North Pacific Expedition*, pt. 1. 5 pp., 28 plates. New York: American Museum of Natural History.

1900 Progress of the Jesup North Pacific Expedition. *American Museum Journal* 1(4):60–2.

1901 Die Jesup Nordpazifische Expedition. In *Verhandlungen der Gesellschaft für die Erdkunde zu Berlin*, 28. Pp. 356–9.

1902 [The Development of the American Museum of Natural History]. Department of Anthropology. *American Museum Journal* 2(6):47–53 [*JNPE* activities, 1897–1902, p. 52].

1903 The Jesup North Pacific Expedition. *American Museum Journal* 3(5):73–119.

1905 The Jesup North Pacific Expedition. In *International Congress of Americanists, 13th Session, Held in New York in 1902*. Pp. 91–100. Easton, PA: Eschenbach.

1908 Die Nordpazifische Jesup-Expedition. *Inter-*

- nationale Wochenschrift für Wissenschaft, Kunst and Technik* 2(41):1291–306.
- 1910 Die Resultate der Jesup-Expedition. In *Internationaler Amerikanisten-Kongress, 16. Tagung, Wien 1908. Erste Hälfte*. Pp. 3–18. Vienna and Leipzig: A. Hartleben's Verlag [for translation, see the appendix to Fitzhugh and Krupnik, this volume].
- BOGORAZ, V. G., AND V. I. JOCHELSON
 1900 O Sibirskom Poliarnom Otdel Severo-Tikhookeanskoi Ekspeditsii (On the Siberian polar section of the North Pacific Expedition). *Zhivaia starina* 10(1–2):295–6 [letter from San Francisco, April 16/3, 1900]. St. Petersburg.
- CHAMBERLAIN, A. F.
 1897 Anthropology at the Toronto Meeting of the British Association (for the Advancement of Science). A Brief Summary of Prof. F. W. Putnam's Paper "The Jesup Expedition to the North Pacific Coast." *Science*, n.s. 6(146):580.
- FARRAND, LIVINGSTON
 1899 The Indians of Western Washington. *Science*, n.s. 9(224):533–5.
- FOWKE, GERARD
 1899 Archaeological Investigations on the Amoor River. *Science*, n.s. 9(224):539–41.
 1906 Exploration of the Lower Amur Valley. *American Anthropologist*, n.s. 8(2):276–97.
- GREGORY, W. K.
 1900 The Jesup North Pacific Expedition. *American Museum Journal* 1(1):9–10.
- [JESUP, MORRIS K.]
 1898 Annual Report of the President for the Year 1897. The American Museum of Natural History, New York [JNPE activities, pp. 15–16, with a map of "Field of Proposed Operations"; see Fig. 3, this volume].
 1899 Annual Report of the President for the Year 1898 [JNPE activities, pp. 15–16].
 1900 Annual Report of the President for the Year 1899 [JNPE activities, p. 13].
 1901 Annual Report of the President for the Year 1900 [JNPE activities, p. 13].
 1902 Annual Report of the President for the Year 1901 [JNPE activities, pp. 19–20].
 1903 Annual Report of the President for the Year 1902 [JNPE activities: pp. 19–20].
- LAUFER, BERTHOLD
 1899 Petroglyphs on the Amoor. *American Anthropologist*, n.s. 1 (October):746–50.
 1899 Ethnological Work on the Island of Saghalin. *Science*, n.s. 9(230):732–4.
 1900 Preliminary Notes on Explorations among the Amoor Tribes. *American Anthropologist*, n.s. 2 (2):297–338.
- 1900 Die angeblichen Urvölker von Jezo und Sachalin. *Centralblatt für Anthropologie, Ethnologie und Urgeschichte* 5(6) [Jena].
- NEW YORK TIMES
 1897 [Report on the Expedition] (March 13):2:5.
- PUTNAM, FREDERIC W.
 1905 Synopsis of Peabody and American Museum of Natural History Anthropology Departments. In *International Congress of Americanists, 13th Session, Held in New York in 1902*. P. xliii. Easton, PA: Eschenbach.
- SCIENCE
 1897 Scientific Notes and News [On the Departure of Boas and Other Members of the Expedition Team to the Northwest Coast]. *Science*, n.s. 5(127):874.
 1899 Field-Work of the Jesup North Pacific Expedition in 1898. *Science*, n.s. 9(224):532–41 [includes introduction and thematic sections written by Farrand, Fowke, and Smith; see entries in this section].
 1900 Jesup North Pacific Expedition. *Science* 12(293):235–6.
- SMITH, HARLAN I.
 1898 The Jesup Expedition Collection. *American Antiquarian and Oriental Journal* 20:101–4.
 1899 Archaeological Investigations on the North Pacific Coast of America [in 1898]. *Science*, n.s. 9(224):535–9.
 1900 Archaeological Investigations on the North Pacific Coast in 1899. *American Anthropologist*, n.s. 2 (3):563–7.

6. Reports on and Reviews of JNPE Publications and Collections

- AMNH (AMERICAN MUSEUM OF NATURAL HISTORY)
 1900 Customs of the Ancient Thompson River Valley Tribes. *American Museum Journal* 1(3):46.
 1903 The Forthcoming Report on the Siberian Mammals Collected by the Jesup North Pacific Expedition. *American Museum Journal* 3(3):35.
 1904 A General Guide to the American Museum of Natural History. *American Museum Journal, Supplement to Vol. 4(1)* [pp. 41–5, Northwest Coast; pp. 50–4, Siberia].
 1904 Primitive Art. A Guide Leaflet to Collections in the American Museum of Natural History. *American Museum Journal, Supplement to Vol. 4(3)* [pp. 7–17, Hall 108, Northwest Coast; pp. 29–31, Tribes of the Amur River].

1906 Scientific Publications of the Jesup North Pacific Expedition. *American Museum Journal* 6(2):58–61.

BOAS, FRANZ

1900 *Ethnological Collections from the North Pacific Coast of America: Being a Guide to Hall 108 in the American Museum of Natural History*. New York: American Museum of Natural History. 13 pp., with map.

1906 Publications of the Jesup North Pacific Expedition. *Science* 23:102–7 [résumé of Boas' and Hunt's Kwakiutl Texts; Swanton's Haida Ethnology; Jochelson's Koryak; Bogoras' Chukchee].

1908 Publications of the Jesup North Pacific Expedition. *Science* 28(710):176–8 [résumé of Teit's The Lillooet Indians; Smith's Archaeology of the Gulf of Georgia; Boas and Hunt's Kwakiutl Texts].

GREGORY, W. K.

1900 Memoirs of the American Museum of Natural History, 1. Anthropological Series. *American Museum Journal* 1(7–8):115–25.

1901 Memoirs of the American Museum of Natural History, 2. Anthropological Series. *American Museum Journal* 1(9–10):145–52.

[JESUP, MORRIS K.]

1904 Annual Report of the President for the Year 1903. New York. 23 pp.

MASON, OTIS T.

1900 Anthropological Publications of the American Museum of Natural History, New York, in 1900. *Science* 12(308):804–6 [reviews of JNPE vols. 1 and 2].

SHTERNBERG, LEV YA.

1901 Novye izdaniia Akademii Nauk v oblasti fol'klora i lingvistiki, po izucheniiu chukotskogo i koriakskogo iazykov i fol'klora, sobrannye V. G. Bogorazom i V. I. Iokhelsonom (New publications of the Academy of Sciences in the field of folklore and linguistics: Studies of the Chukchi and Koryak languages and folklore collected by W. Bogoras and W. Jochelson). *Zhurnal Ministerstva narodnogo prosveshcheniia* 4:189–202. St. Petersburg.

1905 The Chukchee, by W. Bogoras. *American Anthropologist*, n.s. 7(2):320–4.

7. JNPE-Based or JNPE-Related Publications Other than Those Published in the Main JNPE Series, 1897 to Present

Three JNPE members—Boas, Bogoras, and Jochelson—had conducted studies and collected extensive data

in the areas they later visited during the JNPE years. Boas' JNPE fieldwork was focused on four Northwest Coast nations: the Kwakwaka'wakw [Kwakiutl], Coastal Salish, Nuxalk [Bella Coola], and Tsimshian. This section lists Boas' main post-Jesup publications, and his publications and articles outside the Jesup series, on these groups only, whether based exclusively on the JNPE field data or on his pre-1897 research. The same rule applies to Bogoras' post-JNPE publications on the Chukchi, the Even (Lamut), the Yupik (Siberian Eskimo) and the local Siberian Creoles and to Jochelson's contributions on the Yukagir.

AMNH (AMERICAN MUSEUM OF NATURAL HISTORY)

1904 The Exhibit of Chukchee Clothing. *American Museum Journal* 4(1):22–4.

1904 The House-Life of the Chukchee in Siberia. *American Museum Journal* 4(2):35–7.

BOAS, FRANZ

1900 Sketch of the Kwakiutl Language. *American Anthropologist*, n.s. 2:708–21.

1902 Tsimshian Texts [Nass River Dialect]. *Bureau of American Ethnology Bulletin* 27. Washington, DC.

1906 Der Einfluss der sozialen Gliederung der Kwakiutl auf deren Kultur. In *Internationaler Amerikanisten-Kongress, 14. Tagung, Stuttgart 1904. Erste Hälfte*. Pp. 141–8. Berlin: W. Kohlhammer.

1906 The Salish Tribes of the Interior of British Columbia. In *Canada. Report of the Ministry of Education. Appendix—Annual Archaeological Report*. Pp. 219–25. Ontario.

1906 The Tribes of the North Pacific Coast. In *Canada. Report of the Ministry of Education. Appendix—Annual Archaeological Report*. Pp. 235–49. Ontario.

1908 Eine Sonnesage der Tsimshian. *Zeitschrift für Ethnologie* 5:776–97.

1910 Kwakiutl Tales. *Columbia University Contributions to Anthropology*, 2. New York: Columbia University Press. 495 pp.

1911 Tsimshian. In *Handbook of American Indian Languages*. Franz Boas, ed. *Bureau of American Ethnology Bulletin* 40(1):283–422. Washington, DC.

1911 Kwakiutl. In *Handbook of American Indian Languages*. Franz Boas, ed. *Bureau of American Ethnology Bulletin* 40(1):423–558. Washington, DC.

1912 Tsimshian Texts (New Series). *Publications of the American Ethnological Society*, 3. Pp. 65–284. New York.

1916 Tsimshian Mythology: Based on Texts Recorded

- by Henry W. Tate. In *Bureau of American Ethnology Annual Report for the Years 1909–1910*, 31. Pp. 29–1037. Washington, DC: Government Printing Office.
- 1920 The Social Organization of the Kwakiutl. *American Anthropologist* 22(1):111–26.
- 1921 Ethnology of the Kwakiutl. *Bureau of American Ethnology Annual Report*, 35, pts. 1–2. Washington, DC: Government Printing Office.
- 1925 Contributions to the Ethnology of the Kwakiutl. *Columbia University Contributions to Anthropology*, 3. New York: Columbia University Press.
- 1930 Religion of the Kwakiutl. *Columbia University Contributions to Anthropology*, 10, pts. 1–2. New York: Columbia University Press.
- 1931 Notes on the Kwakiutl Vocabulary. *International Journal of American Linguistics* 4(3–4):163–78.
- 1932 Current Beliefs of the Kwakiutl Indians. *Journal of American Folk-Lore* 45(176):177–260.
- 1934 Geographical Names of the Kwakiutl Indians. *Columbia University Contributions to Anthropology*, 20. New York: Columbia University Press [includes 22 maps].
- 1935 Kwakiutl Culture as Reflected in Mythology. *Memoirs of the American Folk-Lore Society*, 28. New York.
- 1935 Kwakiutl Tales (New Series). *Columbia University Contributions to Anthropology*, 26. Pt. 1, Texts. New York: Columbia University Press.
- 1943 Kwakiutl Tales (New Series). *Columbia University Contributions to Anthropology*, 26. Pt. 2, Translations. New York: Columbia University Press.
- 1947 Kwakiutl Grammar, with a Glossary of the Suffixes. *Transactions of the American Philosophical Society* 37(3):201–377.
- 1966 *Kwakiutl Ethnography*. Helen Codere, ed. Chicago: University of Chicago Press.
- BOAS, FRANZ, AND LIVINGSTON FARRAND
- 1899 Physical Characteristics of the Tribes of British Columbia. In *68th Report for the British Association for the Advancement of Science for 1898*. Pp. 628–44. London.
- BOGORAS, WALDEMAR
- 1901 The Chukchi of Northeastern Asia. *American Anthropologist*, n.s. 3(1):80–108.
- 1902 The Folklore of Northeastern Asia, as Compared with That of Northwestern America. *American Anthropologist*, n.s. 4(4):577–683.
- 1904 Idées religieuses des Tchouktchis. *Bulletins et Mémoires de la Société d'Anthropologie de Paris*, 5(V Série):129–35.
- 1906 Religious Ideas of Primitive Man, from Chukchee Material. In *Internationaler Amerikanisten-Kongress*, 14. *Tagung, Stuttgart 1904. Erste Hälfte*. Pp. 129–35. Berlin: W. Kohlhammer.
- 1909 Materialy dlia izucheniia iazyka aziatskikh eskimosov (Data for a study of the Asiatic Eskimo language). *Zhivaia starina* 2–3:178–90. St. Petersburg.
- 1913 Chukotskie risunki (Chukchi drawings). In *Sbornik v chest' 70-letia D. N. Anuchina*. Pp. 397–420. Moscow.
- 1917 Koryak Texts. *American Ethnological Society Publications*, 5. Leiden and New York. 153 pp.
- 1918 Tales of Yukaghir, Lamut, and Russified Natives of Eastern Siberia. *Anthropological Papers of the American Museum of Natural History*, 20(1). Pp. 3–148. New York.
- 1922 Chukchee. In *Handbook of American Indian Languages*, pt. 2. *Bureau of American Ethnology Bulletin* 40, pt. 2:631–903. Washington, DC.
- 1928 Chukchee Tales. *Journal of American Folk-Lore* 41(161):297–452.
- 1930 Chukotskii obshchestvennyi stroi po dannym fol'klora (Chukchi social structure as seen from the folklore data). *Sovetskii sever* 6:63–79. Moscow.
- 1931 Klassovoe rassloenie u chukoch-olenevodov (Class stratification among the Reindeer Chukchi). *Sovetskaia etnografiia* 1–2:93–116. Leningrad.
- 1931 Materialy po lamutskomu iazyku (Materials relating to the Lamut [Even] language). In *Tungusskii sbornik*, 1. V. G. Bogoraz-Tan, ed. Pp. 1–106. Leningrad: Akademiia Nauk SSSR.
- 1934 Luoravetlanskii (chukotskii) iazyk (The Luoravtlan [Chukchee] language). In *Iazyki i pis'mennost' narodov Severa*, 3. Pp. 5–46. Leningrad: Institut narodov Severa.
- 1934 Yuitskii (aziatsko-eskimoskii) iazyk (The Yuit [Asiatic Eskimo] language). In *Iazyki i pis'mennost' narodov Severa*. Pp. 105–28. Leningrad: Institut narodov Severa.
- 1934 *Chukchi*. (The Chukchi). Pt. 1. Leningrad: Institut narodov Severa [trans. of *JNPE*, vol. 7, Introduction and pt. 3; rev.].
- 1937 *Luoravetlansko-russkii (chukotsko-russkii) slovar'* (Chukchi-Russian dictionary). Leningrad: Institut narodov Severa. 164 + xlvii pp.
- 1939 *Chukchi. Religiiia* (The Chukchee. Religion). Leningrad: Institut narodov Severa [trans. of *JNPE*, vol. 7, pt. 2, 1907; rev.].
- 1949 *Materialy po iazyku aziatskikh eskimosov* (Materials relating to the language of the Asiatic Eskimo). Leningrad: Gosuchpedgiz. 255 pp.
- JOHELSON, WALDEMAR
- 1904 The Mythology of the Koryak. *American Anthropologist*, n.s. 6(4):413–25.

- 1905 Essay on the Grammar of the Yukaghir Language. *Supplement to the American Anthropologist*, n.s. 7(2):369–424.
- 1907 Etnologicheskie problemy na severnykh beregakh Tikhogo okeana (Ethnological problems on the northern shores of the Pacific). *Izvestiia Russkogo Geograficheskogo obshchestva* 43:63–92. St. Petersburg.
- 1933 The Yakut. *Anthropological Papers of the American Museum of Natural History*, 32(2). New York.
- 1934 Odul'skii (yukagirskii) iazyk (The Odul [Yukagir] language). In *Iazyki i pis'mennost' narodov Severa*, vol. 3. *Iazyki i pis'mennost' paleoaziatskikh narodov*. E. A. Kreinovich, ed. Pp. 149–80. Leningrad: Institut narodov Severa.
- JOHELSON-BRODSKY, DINA
- 1906 Zur Topographie des weiblichen Körpers nordost-sibirischer Völker. *Archiv für Anthropologie* 33:1–58.
- 1907 K antropologii zhenshchin plemion krainego severo-vostoka Sibiri (Contribution to the anthropology of the women of the tribes of northeastern Siberia). *Russkii antropologicheskii zhurnal* 1–2:1–87. Moscow.
- OETTEKING, BRUNO
- 1917 Preliminary Remarks on the Skeletal Material Collected by the Jesup Expedition. In *Proceedings of the 19th International Congress of Americanists, Washington, D.C., 1915*. Pp. 621–4. Washington, DC.
- 1928 Craniology of the Northwest Coast of North America. In *Atti del 22. Congresso Internazionale degli Americanisti, Roma—Settembre 1926*, vol. 1. Pp. 421–5. Rome: Riccardo Carroni.
- SMITH, HARLAN I.
- 1900 Archaeology of Lytton, British Columbia. *Monumental Records* 1:76–88.
- 1901 The Prehistoric Ethnology of the Thompson River Region. In *Report of the Michigan Academy of Sciences*, 2. Pp. 8–10. Ann Arbor.
- 1901 The Archaeology of the Southern Interior of British Columbia. *American Antiquarian and Oriental Journal* 23:25–31.
- 1902 Archaeology of Lytton, British Columbia. *Records of the Past* 1:205–18.
- 1903 Shell-Heaps of the Lower Fraser River, British Columbia. *Records of the Past* 3:79–90.
- 1904 The Cairns or Stone Sepulchres of British Columbia. *Records of the Past* 3: 243–54.
- SWANTON, JOHN R.
- 1902 Notes on the Haida Language. *American Anthropologist*, n.s. (3):392–403.
- 1903 The Haida Calendar. *American Anthropologist*, n.s. 5:331–5.
- 1905 Social Organization of the Haida. In *International Congress of Americanists, 13th Session, Held in New York in 1902*. Pp. 327–34. Easton, PA: Eschenbach.
- 1905 Haida Texts and Myths: Skidegate Dialect. *Bureau of American Ethnology Bulletin* 29. Washington, DC. 450 pp.
- 1905 Types of Haida and Tlingit Myths. *American Anthropologist*, n.s. 7(1):94–103.
- 1907 Haida. In *Handbook of American Indians North of Mexico*. F. W. Hodge, ed. *Bureau of American Ethnology Bulletin* 30(1):520–3. Washington, DC.
- 1911 Haida. In *Handbook of American Indian Languages*. F. Boas, ed. *Bureau of American Ethnology Bulletin* 40(1):205–82. Washington, DC.
- 1912 Haida Songs. *Publications of the American Ethnological Society*, 3. Pp. 1–63. New York.
- 1913 A Haida Food Plant. *American Anthropologist* 15:543–4.
- TEIT, JAMES
- 1898 Traditions of the Thompson River Indians of British Columbia. *Memoirs of the American Folk-Lore Society*, 6. New York.
- 1928 The Middle Columbia Salish. Franz Boas, ed. In *University of Washington Publications in Anthropology*, 2(4). Pp. 83–128. Seattle.
- 1930 The Salishan Tribes of the Western Plateau. In *Annual Report of the Bureau of American Ethnology for the Years 1927–1928*. Franz Boas, ed. Pp. 23–396. Washington, DC: Government Printing Office.

8. Major Post-Jesup Extensions

By 1900–02, most Jesup team members (except for “locals” such as George Hunt and James Teit) had completed their JNPE field research. Several later trips, however, were explicitly acknowledged by former JNPE participants as “extensions” of their work under the JNPE agenda. In this category were John Swanton’s trip to the Tlingit (1904), Farrand’s and Dixon’s research in Oregon and California with the Huntington Expedition (1899 and 1900), Harlan Smith’s archaeological survey in the Columbia River Valley (1903), and the Jochelsons’ trip to the Aleutian Islands and the Kamchatka Peninsula with the Riabushinski Expedition (1909–11). Publications resulting from these surveys are therefore included in this comprehensive JNPE bibliography.

- BOAS, FRANZ, ED.
1917 Folktales of Salishan and Sahaptin Tribes, Collected by James Teit and Others. *Memoirs of the American Folk-Lore Society*, 11. New York.
- DIXON, ROLAND B.
1911 Maidu. In Handbook of American Indian Languages. Franz Boas, ed. *Bureau of American Ethnology Bulletin* 40(1):679–734. Washington, DC,
1911 Shasta Myths. *Journal of American Folk-Lore* 33(88):3–37, 363–70.
- FARRAND, LIVINGSTON
1901 Notes on the Alsea Indians of Oregon. *American Anthropologist*, n.s. 3(2):239–47.
1915 Shasta and Athabascan Myths from Oregon. Leo J. Frachtenberg, ed. *Journal of American Folk-Lore* 28(109):207–42.
- JOCHELSON, WALDEMAR
1913 The Aleut Language and Its Relation to the Eskimo Dialects. In *International Congress of Americanists, Proceedings of the 18th Session, London, 1912. Part 1*. Pp. 96–104. London: Harrison and Sons.
1912 Scientific Results of the Ethnological Section of the Riabouschinsky Expedition of the Imperial Russian Geographical Society to the Aleutian Islands and Kamchatka. In *International Congress of Americanists, Proceedings of the 18th Session, London, 1912. Part 2*. Pp. 334–43. London: Harrison and Sons.
1919 Aleutskii iazyk v osveshchenii grammatiki Veniaminova (The Aleut language in the light of Veniaminov's grammar). *Izvestiia Akademii Nauk* 13:133–54, 287–315. St. Petersburg.
1923 *Materialy dlia izucheniia aleutskogo iazyka i folklor* (Data for the study of Aleut language and folklore), vol. 1. Petrograd.
1925 Archaeological Investigations in the Aleutian Islands. *Carnegie Institution of Washington Publication*, 367. Washington, DC. 145 pp.
1927 The Instrumental and the Comitative in the Aleut Language. *Language* 3:9–12.
1928 Archaeological Investigations in Kamchatka. *Carnegie Institute of Washington Publication*, 388. Washington, DC. viii + 88 pp.
1930 Arkheologicheskie issledovaniia na Kamchatke (Archaeological surveys in Kamchatka). *Izvestiia Russkogo Geograficheskogo obshchestva* 63(3):199–242; (4):351–85. Leningrad.
1933 History, Ethnology and Anthropology of the Aleuts. *Carnegie Institute of Washington Publication*, 432. Washington, DC.
- 1961 *Kamchadal Texts Collected by Waldemar Jochelson*. The Hague: Mouton.
1990 *Unangam Ungiikangin kayux Tunusangin. Unangam Uniikangis ama Tunuzangis. Aleut Tales and Narratives Collected 1909–1910 by Waldemar Jochelson*. Knud Bergsland and Moses L. Dirks, eds. Alaska Native Language Center. Fairbanks: University of Alaska.
- SMITH, HARLAN I.
1905 An Archaeological Expedition to the Columbia Valley. *Records of the Past* 4:119–27.
1906 Preliminary Notes on the Archaeology of the Yakima Valley, Washington. *Science* 23(558):551–5.
1909 Archaeological Remains on the Coast of Northern British Columbia and Southern Alaska. *American Anthropologist* 11:595–600.
1910 The Archaeology of the Yakima Valley. *Anthropological Papers of the American Museum of Natural History*, 6. Pp. 1–171. New York.
1910 Canoes of the Northern Pacific Coast Indians. *American Museum Journal* 10(8):243–5.
1911 Totem Poles of the North Pacific Coast. *American Museum Journal* 11(3):77–82.
- SWANTON, JOHN R.
1905a Types of Haida and Tlingit Myths. *American Anthropologist*, n.s. 7(1):94–103.
1905b Tlingit Method of Collecting Herring-Eggs. *American Anthropologist*, n.s. 7(1):172.
1908 Social Conditions, Beliefs, and Linguistic Relationships of the Tlingit Indians. In *26th Annual Report of the Bureau of American Ethnology*. Pp. 391–485. Washington, DC: Government Printing Office.
1909 Tlingit Myths and Texts. *Bureau of American Ethnology Bulletin* 39. Washington, DC.
1911 Tlingit. In Handbook of American Indian Languages. Franz Boas, ed. *Bureau of American Ethnology Bulletin* 40(1):283–422. Washington, DC: Government Printing Office.

9. Selected Post-Jesup Comparative Publications

- BOAS, FRANZ
1902 Some Problems in North American Archaeology. *American Journal of Archaeology*, Second Series, 6: 1–6. Reprinted in Boas 1940:525–9.
1904 The Folk-Lore of the Eskimo. *Journal of American Folk-Lore* 17(64):1–13. Reprinted in Boas 1940:503–16.
1907 Ethnological Problems in Canada. In *Congrès International des Américanistes, 15e Session, Tenue à*

- Québec en 1906. Tome 1. Pp. 151–60. Québec: Dussault & Proulx. Reprinted in Boas 1940:331–43.
- 1910 Ethnological Problems in Canada. *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 40:529–39. Reprinted in Boas 1940:331–43.
- 1912 The History of the American Race. *Annals of the New York Academy of Sciences* 21:177–83. Reprinted in Boas 1940:324–30.
- 1924 The Social Organization of the Tribes of the North Pacific Coast. *American Anthropologist* 26(3):323–32. Reprinted in Boas 1940:370–8.
- 1925 America and the Old World. In *Congrès International des Américanistes, Compte-Rendue de la 21e Session, Deuxième Partie. Tenue à Göteborg en 1924*. Pp. 21–8. Göteborg Museum.
- 1928 Migrations of Asiatic Races and Cultures to North America. *Scientific Monthly* 28:110–7.
- 1933 Relationships between North-West America and North-East Asia. In *The American Aborigines: Their Origin and Antiquity*. D. Jenness, ed. Pp. 357–70. Toronto: University of Toronto Press. Reprinted in Boas 1940:344–55.
- 1940 *Race, Language and Culture*. New York: Macmillan. Reprint: Free Press, 1966.
- BOGORAS, WALDEMAR
- 1902 The Folklore of Northeastern Asia, as Compared with That of Northwestern America. *American Anthropologist*, n.s. 4(4):577–683.
- 1906 Religious Ideas of Primitive Man, from Chukchee Material. In *Internationaler Amerikanisten-Kongress, 14. Tagung, Stuttgart 1904. Zweite Hälfte*. Pp. 129–35. Berlin: W. Kohlhammer.
- 1908 Religioznye idei pervobytnogo cheloveka (Po materialam, sobrannym sredi plemen severo-vostochnoi Azii, glavnym obrazom sredi chukoch) (Religious ideas of primitive man [based upon data collected among the Native groups of Northeast Asia, primarily among the Chukchi]). *Zemlevedenie* 15(1):60–80. Moscow.
- 1919 O tak nazyvaemom iazyke dukhov (shaman-skom) u raznykh vetvei eskimoskogo plemeni (On the so-called “spirit” [shaman] language among various branches of the Eskimo tribe). *Izvestiia Akademii Nauk*:489–95. Petrograd.
- 1924 New Problems of Ethnographical Research in Polar Countries. In *Proceedings of the 21st International Congress of Americanists, First Part. Held at The Hague, August 12–16, 1924*. Pp. 226–46. Leiden: E.J. Brill.
- 1925 Early Migrations of the Eskimo between Asia and America. In *Congrès International des Américanistes, Compte-Rendue de la 21e Session, Deuxième Partie. Tenue à Göteborg en 1924*. Pp. 216–35. Göteborg Museum.
- 1925 Ideas of Space and Time in the Conception of Primitive Religion. *American Anthropologist* 27(2):205–66.
- 1927 Drevnie pereseleniia narodov v Severnoi Azii i v Amerike (Ancient human migrations in Northern Asia and in America). *Sbornik Muzeia antropologii i etnografii* 6:37–62. Leningrad.
- 1928 Ethnographic Problems of the Eurasian Arctic. In *Problems of Polar Research. American Geographical Society Special Publication 7*. Pp. 189–207. New York.
- 1929 Elements of the Culture of the Circumpolar Zone. *American Anthropologist* 31(4):579–601.
- 1935 Drevneishie elementy v iazyke aziatskikh eskimov (The most ancient elements in the Asiatic Eskimo language). In *AN SSSR akademiku N. Ia. Marru*. Pp. 353–66. Leningrad.
- 1936 Osnovnye tipy fol'klora severnoi Evrazii i severnoi Ameriki (Major types of folklore of Northern Asia and North America). *Sovetskii fol'klor* (4–5):29–50. Moscow.
- DIXON, ROLAND B.
- 1933 Tobacco Chewing on the Northwest Coast. *American Anthropologist* 35(1):146–50.
- JOCHELSON, WALDEMAR
- 1904 Ob aziatskikh i amerikanskikh elementakh v mifakh koriakov (On Asiatic and American elements in Koryak myths). *Zemlevedenie* 11(3):33–41. Moscow.
- 1906 Über asiatische and amerikanische Elemente in den Mythen der Koriaken. In *Internationaler Amerikanisten-Kongress, 14. Tagung, Stuttgart 1904. Erste Hälfte*. Pp. 119–27. Berlin: W. Kohlhammer. First published in 1904 in Russian, *Zemlevedenie* 11(3):33–41.
- 1907 Past and Present Subterranean Dwellings of the Tribes of North Eastern Asia and North Western America. In *Congrès International des Américanistes, 15e Session, Tenue à Québec en 1906. Tome 2*. Pp. 115–28. Québec: Dussault & Proulx.
- 1908 Drevnie i sovremennye podzemnye zhilishcha plemen Severo-Vostochnoi Azii i Severo-Zapadnoi Ameriki (Past and present subterranean dwellings of the tribes of northeastern Asia and northwestern America). *Ezhegodnik Russkogo Antropologicheskogo obshchestva*, 2. St. Petersburg.
- 1926 The Ethnological Problems of Bering Sea. *Natural History* 26(1):90–5.
- 1928 *Peoples of Asiatic Russia*. New York: American

- Museum of Natural History [see especially ch. 2, The Americanoids of Siberia, Introduction, pp. 43–5].
- 1930 The Ancient and Present Kamchadal and the Similarity of Their Culture to That of the Northwestern American Indians. In *International Congress of Americanists, 33rd Session, New York, 1928*. Pp. 451–4. New York.
- LAUFER, BERTHOLD
- 1917 Reindeer and Its Domestication. In *Memoirs of the American Anthropological Association*, 4(2). Pp. 91–147. Lancaster, PA.
- SMITH, HARLAN I.
- 1899 Stone Hammers or Pestles of the Northwest Coast of America. *American Anthropologist*, n.s. 1:363–8.
- STERNBERG, LEO
- 1906 Bemerkungen über Beziehungen zwischen der Morphologie der giljakischen und amerikanischen Sprachen. In *Internationaler Amerikanisten-Kongress, 14. Tagung, Stuttgart 1904. Erster Hälfte*. Pp. 137–40. Berlin: W. Kohlhammer.
- 1913 The Turano-Ganowanian System and the Nations of North-East Asia. In *International Congress of Americanists, Proceedings of the 18th Session, London, 1912. Part 1*. Pp. 319–33. London: Harrison and Sons.
- 1925 Divine Election in Primitive Religion (Including Material on Different Tribes of N.E. Asia and America). In *Congrès International des Américanistes, Compte-Rendu de la 21e Session, Deuxième Partie. Tenue à Göteborg en 1924*. Pp. 472–512. Göteborg Museum.
- SWANTON, JOHN R.
- 1904 The Development of the Clan System and of Secret Societies among the Northwestern Tribes. *American Anthropologist*, n.s. 6:477–85.
- 1905 The Social Organization of American Tribes. *American Anthropologist*, n.s. 7:663–73.
- 1906 A Reconstruction of the Theory of Social Organization. In *Boas Anniversary Volume: Anthropological Papers Written in Honor of Franz Boas . . . Presented to Him on the Twenty-fifth Anniversary of His Doctorate*. Pp. 166–78. New York: G. E. Stechert.

10. Unpublished Manuscripts Related to the JNPE

As noted in the introduction to this bibliography, this section is currently confined to unpublished work of Waldemar Bogoras, Waldemar Jochelson, and Dina Jochelson-Brodsky.

Bogoras, Waldemar

- AMERICAN PHILOSOPHICAL SOCIETY (PHILADELPHIA, BOAS COLLECTION), APS-BC
- n.d. Chukchee Lexicon. APS-BC. Ms. 366. 2,000 cards with Chukchee words and English equivalents.
- n.d. Chukchee Suffix List. APS-BC. Ms. 26. 66 pp. (handwritten).
- n.d. Chukchee Word List. APS-BC. Ms. 29. 267 pp. (handwritten).
- n.d. Chukchee Word List and Interlinear Texts with Notes. APS-BC. Ms. 28. 50 pp. (handwritten).
- n.d. Comparative Word List of Alaskan Eskimo, Siberian Eskimo, and Chukchee. APS-BC. Ms. 135. 31 pp. (handwritten).
- ARCHIVES OF THE RUSSIAN ACADEMY OF SCIENCES (ST. PETERSBURG, BOGORAS COLLECTION/FOND 250), RAS-B
- 1901 Dnevnik vo vremia puteshestviia i prebyvaniia v Unyine (Diary during the trip and stay at Unyin). RAS-B. Fond 250, op. 1, no. 116.
- n.d. Diary 1900–1901. RAS-B. Fond 250, no. 119.
- n.d. Materialy o chukchakh (Chukchee materials: The kinship system). RAS-B. Fond 250, op. 1, no. 122. 250 pp.
- n.d. Materialy po chukotskomy folkloru (Chukchee folklore materials). RAS-B. Fond 250, op. 1, no. 124–5.
- n.d. Koriakskii slovar'. Kamenskii dialect. (Koryak dictionary. The dialect of Kamenskoe). RAS-B. Fond 250, op. 1, no. 130, 199 pp.
- n.d. Zapisi po grammatike koriakskogo iazyka (Notes on Koryak grammar). RAS-B. Fond 250, op. 1, no. 132.
- n.d. Kamchadal'skii iazyk (The Kamchadal language). RAS-B. Fond 250, op. 1, no. 134. 4 notebooks, 84 pp. (grammar and vocabulary).
- n.d. List of Chukchee Roots. RAS-B. Fond 250, op. 1, no. 11. 25 pp.
- 1900 Koriakso-kamchadal'skie teksty, 1900–1901 (Koryak and Kamchadal texts, 1900–1901). RAS-B. Fond 250, op. 1, no. 133.
- 1900 Materialy po eskimoskomy iazyku (Data on the Eskimo/Siberian Yupik language). RAS-B. Fond 250, op. 1, no. 134.
- n.d. [Sketch of the Grammar of the Asiatic Eskimo Language/Ocherk grammatiki iazyka aziatskikh eskimosov]. Unfinished English translation of the original Russian manuscript. RAS-B. Fond 250, op. 1, no. 52. 97 pp.
- n.d. [Folklore texts]. RAS-B. Fond 250, op. 1, no. 124.
- n.d. [Folklore texts] (in Russian). RAS-B. Fond 250, op. 1, no. 125.

- INSTITUT VOSTOKOVEDENIIA (ST. PETERSBURG, INSTITUTE OF ORIENTAL STUDIES, OTDEL VOSTOCHNYKH RUKOPISEI/ DEPARTMENT OF ORIENTAL MANUSCRIPTS, FOND 23—W. JOCHELSON COLLECTION), INV-J
- n.d. Chukotskie teksty i peresказы na russkom iazyke (Chukchee texts with Russian paraphrases). InV-J. Fond 23, no. 7.
- n.d. Chukotskaia grammatika (A Chukchee grammar). InV-J. Fond 23, no. 8
- n.d. Materialy po chukotskoi grammatike (Materials on Chukchee grammar). InV-J. Fond 23, no. 9.
- n.d. Russko-chukotskii slovar' (Russian-Chukchee dictionary). InV-J. Fond 23, no. 10.
- n.d. Materialy po koriakskomu iazyku (Koryak materials). InV-J. Fond 23, no. 11.

NEW YORK PUBLIC LIBRARY (MANUSCRIPT DIVISION)

See Yarmolinsky 1947 in section 11 below.

- n.d. Yupik (Asiatic Eskimo) Language and Folklore Materials. (1) Manuscript with ethnographic introduction by Ernest W. Hawkes and preface by Bogoras; (2) outlines of Yupik grammar, with the thematic word list, 98 handwritten pages; (3) short glossary of the shaman "spirit language"; (4) 17 folklore tales—Yupik original with Chukchi, Russian, and English (?) translation; see Bogoraz 1949 (section 7)—all completed in 1918, with some later updates.

Jochelson, Waldemar

AMERICAN PHILOSOPHICAL SOCIETY (PHILADELPHIA, BOAS COLLECTION), APS-BC

- n.d. Chukchee Life History. APS-BC. Ms. 27. 9 pp. (handwritten).
- n.d. The Study of Paleoasiatic and Tungus Languages in the USSR. 1918–1928. Manuscript. APS-BC. Ms. 33. 16 pp. (handwritten).
- [?] Concordance to Koryak Mythology. Motifs, Characters, and Material Culture Items. APS-BC. Ms. 330. 400 cards.

NEW YORK PUBLIC LIBRARY (MANUSCRIPT DIVISION, NEW YORK), NYPL;

See Yarmolinsky 1947 and Jakobson et al. 1957 in section 11 below.

- n.d. The Kamchadal. Manuscript on Kamchadal history, ethnography, material culture, and geography of the area. 129 pp. (in English), with 50 photographs from the Riabushinski Expedition of 1910–11.
- n.d. Kamchadal linguistic materials: observations on the Russian dialect spoken by the natives; list of Itelmen terms of kinship; paradigms of Itelmen nouns,

pronouns, and verbs; and a sketch of the phonology of Itelmen language. .

- n.d. The Kamchadal texts (see Jochelson 1961, section 8). 41 texts in the original Kamchadal with Jochelson's literal and abridged English translation.
- n.d. Kamchadal-Russian and Russian-Kamchadal Dictionary (on cards).

INSTITUT VOSTOKOVEDENIIA (ST. PETERSBURG, INSTITUTE OF ORIENTAL STUDIES, OTDEL VOSTOCHNYKH RUKOPISEI/ DEPARTMENT OF ORIENTAL MANUSCRIPTS, FOND 23—W. JOCHELSON COLLECTION), INV-JI;

See Jochelson 1919 in section 8 above.

- n.d. Zapisi skazok na yukagirskom iazyke, poverii, a takzhe slova, vstrechaiushchiesia v skazkakh (Records of fairy tales in Yukaghir, of traditional beliefs; and also words occurring in the fairy tales). InV-J. Fond 23, no. 2.
- n.d. Slovar' russko-koriakskii (na dialekte koriakovo-olenevodov) (Russian-Koryak dictionary [in the Reindeer Koryak dialect]). InV-J. Fond 23, no. 6.
- n.d. Khangaiskii slovar' (Tundra Yukagir dictionary). InV-J. Fond 23, nos. 13–16.
- n.d. Yukagirsko-russkii slovar' (Yukaghir-Russian dictionary). On cards. InV-J. Fond 23, nos. 17–22.
- n.d. Itelmen Linguistic notebooks, 1910–11. InV-J. Fond 631, nos. 30–32, 34.
- n.d. Notes for the grammar of the Aleut Language. InV-J. 6 notebooks.
- n.d. Aleut Mythology (texts and translations). InV-J.
- n.d. Grammar of the Kamchadal Language.
- ARCHIVES OF THE RUSSIAN ACADEMY OF SCIENCES (ST. PETERSBURG). FONOTEKA (PHONOGRAPHIC COLLECTION), RAS-F
- n.d. Songs, Tales, and Speeches of the Kamchadals and Aleuts. Recorded in 1908(–1910). 142 cylinders.

Jochelson, Waldemar, and Waldemar Bogoras

- n.d. Kamchatskii krai [Kamchatka Province]. Archive of the Peter the Great Museum of Anthropology and Ethnography, St. Petersburg. Fond K-II, no. 164.
- n.d. Catalog of Phonograph Records of Paleosiberian Languages. APS-BC. Ms. 60. 7 pp. (handwritten).

Jochelson-Brodsky, Dina

- 1900 Diary Kept during the Jesup North Pacific Expedition. InV-J. Fond 23 (W. Jochelson File), no. 18, pp. 1–23.
- 1901 Diaries Kept during the Jesup North Pacific Expedition (1901–1902). InV. Fond 631, nos. 127, 128, 129, 130, 131, 132. Notebooks in Russian.

11. Bibliographies; Reviews of Manuscript, Museum, and Archival Collections Related to JNPE Activities

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13. Biographies, Obituaries, and Major Personal Essays on JNPE Participants

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Franz Boas, 1852–1942

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OHIO ARCHEOLOGICAL AND HISTORICAL SOCIETY

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Photographic Records of the Jesup Expedition

A Review of the AMNH Photo Collection

PAULA WILLEY

with afterword by Barbara Mathé

About 3,414 photographs taken by members of the Jesup North Pacific Expedition (JNPE) during the years 1897 to 1902 exist as prints or negatives on file at the American Museum of Natural History (AMNH) in New York. The following photographers are associated with the AMNH Jesup Expedition Collection:

[Axelrod, Alexander]
[Bogoras, Sophia]
Bogoras, Waldemar, 1865–1936
Buxton, N. G.
Dixon, Roland Burrage, 1875–1934
Fowke, Gerard, 1855–1933
French of Tacoma (?)
Hastings, Oregon Columbus
Hunt, George, 1854–1933
Jochelson, Waldemar, 1855–1937
[Jochelson-Brodsky, Dina, 1864–1941]
Laufer, Berthold, 1874–1934
Ninaud, Emile
Orchard (?)
Savannah (?)
Smith, Harlan Ingersoll, 1872–1940

Names in square brackets are not directly noted on the photographic documentation; their inclusion is based on references in letters or field notes.

Photographs credited to “Bogoras” may have been taken by either Waldemar Bogoras or his wife, Sophia Bogoras [or by Alexander Axelrod, Bogoras’ field assistant, particularly in the case of photos depicting Bogoras himself—ed.]. Similarly, it is documented that Dina Jochelson-Brodsky, Waldemar Jochelson’s wife, took many of the photographs credited to “Jochelson.”

Emile Ninaud was hired by Berthold Laufer to take photographs of the Native people in the Amur River valley. One of his images has been identified in the AMNH collection; others can be found in the Louis Marin Collection at the Musée National des Arts asiatiques–Guimet in Paris.¹ “French of Tacoma” and “Savannah” were the names or nicknames of the local photographers in British Columbia hired to make photos on behalf of JNPE members. Oregon C. Hastings, a resident of Victoria, B.C., worked with Boas on the Northwest Coast prior to the Jesup Expedition.² In 1897 and 1898 he was contracted by Harlan Smith to assist with site excavations, and he also did some photography at that time (see Thom, this volume). Orchard was an AMNH employee, either a photographer or an Anthropology Department technician. Images credited to him are either copy negatives of field photographs or pictures of objects taken at the AMNH in New York. [He did not participate first-hand in fieldwork—ed.]

Of the approximately 100 Jesup Expedition photographs that depict aspects of the expedition itself, most are credited to Bogoras or Jochelson. Those primarily depict camp life or expedition transport. The JNPE collection contains confirmed field photographs of Sophia and Waldemar Bogoras, N. G. Buxton, R. B. Dixon, O. C. Hastings, George Hunt and his family, Dina Jochelson-Brodsky, Waldemar Jochelson, Harlan Smith, James Teit and his wife, and a few local officials and interpreters. The number of such “personal” images from the field is remarkably small (about 30) in comparison

with the overall JNPE photo file of more than 3,000 images, but the absence of images of JNPE members in the field is typical of the time.³

Negatives from the Field

After a negative was exposed, it was sometimes given a number (referred to as the field number) and when possible was actually processed in the field. Negatives were then sent to the Department of Anthropology at the AMNH.

On arrival in New York, the negatives were processed (if this had not already been done) and printed. The images were given a unique sequential number in the Anthropology Department files, referred to as the Anthro number. Negative and print numbers correspond. The images were listed in a four-volume handwritten catalogue, "The Catalogue of Photographs, Negatives and Memoranda of Prints from Them," with notations of the original field number and the Anthro number. The field number, negative size, date, photographer's name, subject, and location (site at which the photograph was taken or, for studio photographs, where the object was found) were routinely recorded in the Anthropology Department logs. Some portions of these logs appear to have been recorded in the field; an example is the notes made for the photographs taken on the *S.S. Danube* on the Skeena River in 1897. In a letter from that period, Boas complains that the chopiness of the river made it difficult to write, and indeed, the handwriting in the portion of the negative list that records the photographs taken on that trip is barely legible.

In many cases the AMNH image number still used today was added to the margin of the catalogue.⁴ In addition, marginal references were made to Anthropology Department accession numbers. Catalogue numbers for objects and plaster casts were added to the list, creating a somewhat complicated but valuable record of all related information. The negative envelopes were handwritten, with the AMNH number prominent, indicating that the negatives

were probably placed in the envelopes some time after their entry into the logbook. To add to the labor of comprehensive capture of all the data for each image, the information on the negative envelopes does not always match that found in the log, with one source or the other containing more complete information.

Overall, the four Anthropology catalogues include 7,369 images. Of these, about 2,720 are from the Jesup Expedition, although only the first 432 are identified as such in the lists. Occasionally, the accession records in the AMNH Department of Anthropology hold images that are not duplicated anywhere else in the museum, but can be found in the accession files. Another 694 Jesup Expedition photos that were not recorded on the negative lists can be found as prints in scrapbooks and in files now at the AMNH Library's Special Collections.

Scrapbooks and Prints in the AMNH

As prints were made at the AMNH, many of them were pasted into scrapbooks—large bound volumes—organized in the Department of Anthropology. There are now six scrapbooks (nos. 2, 3, 20, 30, 31, and 32) and four boxes of conserved pages from scrapbooks (JNPE nos. 2, 3, 4, and 5) that include vintage prints of photographs taken on the Jesup Expedition. The captions in the scrapbooks range from a mere identifying number to, in the case of some of Harlan Smith's later photos, page-long typed notes.

Images pasted into the scrapbooks were not arranged in any particular order. Although the order is not completely random, images photographed at different times were arranged in the books, without any substantial identification. For example, Hastings' photographs taken on Boas's 1894 trip to the Northwest Coast begin scrapbook 30, which then continues seamlessly with the 1897 images made by Smith for the Jesup Expedition. Smith's later work through 1909 is also included in the same volume. While some temporal organization can be discerned, only by matching the images with the data in the catalogue can one

distinguish the different provenances of the images. On the whole, the scrapbooks seem to have been viewed as a storage medium for prints rather than as a means of organizing collections by categories.

In addition to the images in the large bound volumes, many of the photographs in the Anthropology Department, including some prints from the Jesup Expedition, were transferred to the AMNH Department of Education, where they were merged with other photographic collections. Photographs were mounted on 10"x14" cards that were originally placed in peg binders. Ultimately, the cards were removed from the binders and filed in drawers. Each image was marked with the AMNH number. Since the pictures were used for subject-based educational purposes, they were not arranged according to the archival principles of provenance and original order. As a result, the Jesup photographs were dispersed throughout the collections. For example, many of Harlan Smith's images of shell heaps found their way into a file drawer marked "Archaeology." George Hunt's photograph of a woman cleaning fish was filed in the "Ichthyology" drawer, under the heading "Halibut—*Hippoglossus Linneaus*."

Again, copies of photographs that Franz Boas took on his trip to the Northwest Coast in 1894 were interfiled with the Jesup Expedition materials, as were some later photographs from the same area. Pre-Jesup images taken by Waldemar Bogoras and Waldemar Jochelson in Siberia [in 1895–98; later donated to the AMNH—ed.] were also mixed with the material created with JNPE funding. In the end, the source of funding and the year of the work become meaningless when one is analyzing the information collected in the images—a fact tacitly acknowledged by the original arrangement of the images in the collection.

For purposes of historical research, however—for determining whether a photograph was, in fact, taken on the Jesup Expedition—it is necessary to cross-reference the name of the photographer, the place where the photograph was taken, and the date, since few of the images are labeled as being from the JNPE. Further-

more, because a great deal of documentation was either lost or never recorded, the researcher often must extrapolate missing data, using whatever information is available to fill the gaps. Corrections, additions, and annotations by AMNH staff members and visiting researchers have been noted on the versos of many of the cards. Visitors to the collection who have personal or family memories of the people or objects in the photographs may also make amendments to the information. Comments are always signed and dated.

Lydia Dohmerr Collection

In addition to the Jesup Expedition photographs sent back to the AMNH from the field, the AMNH possesses another small collection of images relating to Waldemar Jochelson and Dina Jochelson-Brodsky. In the early 1990s, Brodsky's niece, Lydia Dohmerr, donated to the AMNH a collection of artifacts, correspondence, and personal photographs. The inventory of the collection includes 86 photographs: personal photos, snapshots, portraits, and copies of images from the field. Some of the field photographs appear to be unique to this personal collection and cannot be located within the other AMNH Jesup material. Dohmerr's donation of the Jochelsons' personal photographs to the AMNH was made possible thanks to Cynthia Wilder, presently with the Department of Ancient Near East at the Metropolitan Museum of Art in New York.

Computer Database

Today, most of the AMNH photographic collections (including the JNPE files) are preserved in the AMNH Library's Special Collections. An ongoing effort is being made to reconstruct the provenance of these materials in order to facilitate historical research and to restore the integrity of the collection according to classic archival principles.

All the information about each of the recorded JNPE photos was recently transcribed into a computer database. Information from the AMNH Anthropology Department negative lists, photo scrapbooks, and file card

captions are collated, giving a clearer picture of the JNPE collection as a whole. Negative number, field number, date, photographer, content theme, description, source of information, and site of photograph were recorded for each image. The database also includes several photographs that Jochelson and Bogoras took in Siberia prior to their work with the Jesup Expedition.

The site of each photograph is recorded in the database in LCSH (Library of Congress Subject Headings) format, e.g., Russia (Federation)—Siberia—Kamenskayo Village. Several place names, such as the “Kamenskayo Village” cited above are spelled in a variety of ways in the source materials.⁵ Although the original spellings recorded with each photo or negative have been preserved in the database’s Subject field, the Geographic Area field has been populated with normalized data. This consistency gives more accurate results when searching or analyzing data. In some instances, the site of the photograph was not recorded, or the spelling of a place name was so garbled that no match could be found. In that case, the narrowest geographic place that could be established with certainty was recorded in the Geographic Area field. For this reason, there are many records in the database with place names as nonspecific as “Russia,” “Siberia,” or “British Columbia.”

Similarly, data in the Culture field has been normalized and is recorded in LCSH format. For example, Chukchi is often spelled “Chukchee” in the source materials. The original spelling was transcribed verbatim in the Subject field but appears only as “Chukchi” in the Culture field.

Each image was assigned one of six content themes by the database compiler, primarily for statistical purposes. The categories are as follows:

Archaeology: photographs of excavations, survey photographs of excavation sites, photographs of shell heaps and petroglyphs

Architecture: photographs of dwellings, villages, camps, storehouses, etc.

Ethnography: photographs of activities, objects

Landscape: photographs of terrain

Expedition: photographs documenting some aspect of the Jesup North Pacific Expedition, such as transport of collections, expedition campsites, portraits of expedition personnel

Physical type: portrait-like photographs of individuals, often taken from multiple angles. Northwest Coast physical-type photographs tend to be from the waist or chest up; the physical-type photos taken in Siberia are more frequently full length.

It should be noted that in most cases these photographs could be assigned to more than one of these six themes. For example, many Siberian photographs classified as “physical type” are full-length portraits of people in traditional dress taken from the front and from the rear. Not only do these photographs document the proportions of the human beings depicted; they also fully document their clothing. For the purposes of the AMNH computer database, themes were assigned on the basis of the photographer’s apparent intent when taking the photograph. Quite often, this can be difficult to define, as in the case of a physical-type photograph, with the subject holding his shirt open to reveal a tattoo, suggesting “ethnography” as a theme.

This database allows the image records to be sorted, counted, and tabulated in infinite ways. The following tables in this chapter (Tables 1–5) analyze each photographer’s work by date, subject theme, ethnic group or tribe, and location. Unfortunately, even after extensive research, 332 images (300 of which are from Siberia) could not be attributed to a particular photographer, although all but four could be identified as having been taken in Siberia or on the Northwest Coast. These unaccredited photographs are listed as “Unknown, Siberia,” “Unknown, NW,” and “Unknown, no location.”

Any errors in the source information have been inherited by the database, and the same caveats apply. Information was transcribed verbatim—preserving spelling errors—and, since most of the information sources are handwritten and sometimes difficult to read, transcription errors do occur. These and other

shortcomings notwithstanding, the computerized database of the AMNH photo collection offers an invaluable new resource for any research on the Jesup Expedition fieldwork, outcomes, and history.

Notes

1. The single known image of the Nanay people from the Amur River area in the JPNE files (negative 41614) is reproduced in Kendall et al. 1997 (no. 34) [and in Fitzhugh and Crowell 1988:25—ed.]. On other images from the same collections, see White 1993.

2. For example, Hastings was with Boas in 1894. Several of Hastings' excellent photographs of the Kwakwaka'wakw from the Fort Rupert area are reproduced in Jonaitis 1988:134–74.

3. Largely lacking the self-reflexive instincts of today's ethnographers, the members of the Jesup Expedition spent more time, energy, and film documenting their research subjects than themselves. This is particularly understandable when one considers the relative difficulty of creating photographs in the field with large-format cameras, using glass plates (see Mathé and Miller, this volume).

4. The first position in the negative number indicates the size of the negative:

Prefix	Size
1	5" x 7"
2	4" x 5"
3	8" x 10"
4	6" x 8"

5. All Jesup Expedition photographs in the AMNH collection are identified according to the spellings or names of the sites as they existed during the Jesup Expedition years. In addition, there are some misspellings, particularly for the Siberian names (see Table 5). "Markova" is the modern town of Markovo on the Anadyr River; "Indian Point" is the former Yupik village of Ungaziq, or Chaplino, at Cape Chaplin; and "Mariinski Post" is today's city of Anadyr, at the mouth of the Anadyr River. "Kamenskayo" is the Koryak village at Penzhina Bay, now known as the town of Kamenskoye; "Khodarindsha River" cannot be identified—ed.

Afterword by Barbara Mathé

Paula Willey compiled the Jesup database while working as an intern for the Department of Anthropology in 1996 and refined it during her tenure as Special Collections manager, 1998–99. The database proved invaluable in the preparation of the Jesup Centenary Exhibition, *Drawing Shadows to Stone. Photographing North Pacific Peoples, 1897–1902*, shown at the AMNH in 1997, and for the exhibit catalog (Kendall et al. 1997)—the first extensive presentation of JNPE photography in 100 years. The database continues to be a useful resource for researching the unique JNPE photo collection. The information in the database will be reviewed and will soon be integrated into the AMNH Digital Library as part of a larger overall effort to make both data and images from the AMNH's photographic collections available online. The technology now exists to raise the possibility of a future collaborative effort to combine all the resources pertaining to the Jesup Expedition, along with the ongoing work of the *Jesup 2* scientists, in an integrated Web-based resource. In fact, we see our mission now as being to *re-collect* the collections, according to the standards of the present time and for a much broader audience of potential users than Boas and his partners ever envisioned.

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Table 1/ Jesup Expedition Photographs by Photographer and Year

Photographer	1888	1895	1896	1897	1898	1899	1900	1900-2	1901	1902	None
Unknown, NW Coast	1								5		22
Unknown, Siberia		1									299
Unknown, No location											4
Bogoras		184			3		105	115	605		11
Dixon					178						11
Fowke					45		49				4
French											7
Hastings					71						1
Hunt											7
Jochelson		3	1	1			138		634	246	31
Laufer						18					
Ninaud											1
Orchard					8						3
Smith				401	113	38					7
Savannah				24							0
Hastings and Smith					19						
Total	1	188	1	426	437	56	292	115	1244	246	408

Table 2/ Jesup Expedition Photographs by Photographer and Theme

Photographer	Archaeology	Landscape	Ethnography	Architecture	Expedition	Physical type	Misc.	Total
Unknown, Siberia	0	41	97	37	13	104	8	300
Fowke	0	23	2	10	0	0	7	42
Laufer	0	0	0	0	0	18	0	18
Jochelson	0	67	271	37	46	611	22	1,054
Ninaud	0	0	1	0	0	0	0	1
Bogoras	0	72	406	62	19	431	33	1,023
Total: Siberia	0	203	777	146	78	1,164	70	2,438
Unknown, NW Coast	1	2	12	0	2	6	5	28
Savannah	0	0	0	0	0	24	0	24
Dixon	3	0	6	2	0	178	0	189
Hastings and Smith	0	0	0	0	0	19	0	19
Smith	87	24	58	29	9	352	0	559
Fowke	45	0	0	0	0	0	0	45
Hastings	13	0	34	0	1	23	1	72
Hunt	0	0	3	1	3	0	0	7
French	0	0	0	0	0	7	0	7
Orchard	3	0	5	0	0	3	0	11
Total: NW Coast	152	26	118	32	15	612	6	961
Unknown, no location	0	0	1	0	0	3	0	4
Fowke, no location	0	7	1	3	0	0	0	11
Total: no location	0	7	2	3	0	3	0	15
Grand total	152	236	897	181	93	1,779	76	3,414

Table 3/ Jesup Expedition Photographs by Photographer and Location, Asia

Location	Bogoras	Buxton	Fowke	Hunt	Jochelson	Laufer	Ninaud	Smith	Unknown	Total
Northeast, Pacific	0	0	0	4	0	0	0	10	20	34
Russia, (unspecified)	24	0	0	0	894	0	0	1	6	925
Siberia (Sib)	53	17	23	0	129	16	1	0	131	370
Indian Point, Sib	489	11	18	0	129	0	0	0	34	681
Kamenskaya Village, Sib	0	0	0	0	4	0	0	0	0	4
Khodarindsha River, Sib	0	0	0	0	5	0	0	0	0	5
Mariinsky Post, Sib	220	0	0	0	1	0	0	0	1	222
Markova, Sib	230	0	0	0	0	0	0	0	0	230
St. Lawrence Island, Sib	9	0	0	0	0	0	0	0	0	9
Stanovoi Mountain, Sib	0	0	0	0	4	0	0	0	0	4
Unspecified Japan (Jp)	1	0	0	0	1	0	0	0	0	2
Nugata City, Jp	8	0	0	0	0	0	0	0	0	8
Composite or unknown	0	0	5	0	0	0	0	0	0	8
Total	1,034	28	112	14	1,167	16	1	558	199	3,447

Note: This table includes photographs taken by Bogoras and Jochelson prior to their involvement with the Jesup North Pacific Expedition.

Table 4/ Jesup Expedition Photographs by Photographer and Location, North America

Location	Dixon	Fowke	French	Hastings	Hunt	Orchard	Savannah	Smith	Unknown	Total
Unspecified, British Columbia (BC)	6	6	7	1	1	8	0	28	0	57
Alert Bay, BC	0	0	0	0	0	0	0	3	0	3
Bella Bella, BC	0	0	0	0	0	0	0	42	1	43
Comox, BC	0	0	0	3	0	0	0	16	0	19
Douglas, BC	47	0	0	0	5	0	0	0	0	52
Eburne, BC	0	0	0	0	0	0	0	11	0	11
Fort Rupert, BC	0	0	0	44	0	0	0	53	4	101
Hammond, BC	0	0	0	0	0	0	0	26	0	26
Harrison River, BC	6	0	0	0	0	0	0	0	0	6
Kamloops, BC	0	0	0	0	0	0	0	42	0	42
Lillooet River, BC	0	0	0	0	0	0	0	2	0	2
Lytton, BC	0	0	0	0	0	0	0	25	0	25
Musquiam Reserve, BC	0	0	0	0	0	0	0	2	1	3
Nicola Lake, BC	0	0	0	0	0	0	0	7	0	7
River Inlet, BC	0	0	0	0	1	0	0	32	0	33
Skeena River, BC	0	0	0	0	0	0	0	114	0	114
Spences Bridge, BC	0	0	0	0	0	0	0	128	0	128
Steveston, BC	68	0	0	0	1	0	0	0	0	69
Thompson River, BC	0	0	0	0	0	0	0	2	0	2
Vancouver Island, BC	0	0	0	0	2	0	0	0	1	3
Victoria, BC	0	60	0	39	0	0	25	9	0	133
Unspecified, Washington State (WA)	0	0	0	0	0	0	0	4	0	4
Granville, WA	61	0	0	0	0	0	0	0	0	61
Grays Harbor, WA	0	0	0	0	0	0	0	1	0	1

Table 5/ Jesup Expedition Photographs by Photographer and Culture

Culture	Bogoras	Buxton	Dixon	Fowke	French	Hastings	Hunt	Jochelson	Lauffer	Ninaud	Orchard	Savannah	Smith	Unknown	Total
Chukchi (Chukchee)	287	0	0	0	0	0	0	12	0	0	0	0	0	7	306
Chuvan (Chuvantzy)	27	0	0	0	0	0	0	0	0	0	0	0	0	0	27
Clayoquot	0	0	0	0	0	0	0	0	0	0	0	0	0	11	11
Composite or unidentified	411	17	21	108	0	58	5	187	0	0	8	0	330	61	1,206
Cossack (Russian)	16	3	0	0	0	0	0	3	0	0	0	0	0	3	25
Yupik (Siberian Eskimo)	114	0	0	0	0	0	0	0	0	0	0	0	3	0	117
Even (Lamut)	76	0	0	0	0	0	0	1	0	0	0	0	0	1	78
Evenk (Tungus)	6	1	0	0	0	0	0	144	0	0	0	0	0	8	159
Nivkh (Gilyak)	0	0	0	0	0	0	0	0	16	0	0	0	0	0	16
Haida	0	0	0	0	0	0	0	0	0	0	3	0	42	2	47
Heiltsuk (Bella Bella)	0	0	0	0	0	0	0	0	0	0	0	0	7	0	7
Itel'men (Kamchadal)	27	0	0	0	0	0	0	0	0	0	0	0	0	0	27
Koryak	16	1	0	0	0	0	0	383	0	0	0	0	0	3	403
Kwakwaka'wakw (Kwakiutl)	0	0	0	0	0	12	1	0	0	0	0	0	23	5	41
Nanay (Gold)	0	0	0	0	0	0	0	0	0	1	0	0	0	0	1
Nesquaille (Nisquallie)	0	0	0	0	7	0	0	0	0	0	0	0	0	0	7
Nuu-chah-Nulth (Nootka)	0	0	0	0	0	0	1	0	0	0	0	25	0	1	27
Ntlakyapamuk (Thompson)	0	0	0	0	0	0	0	0	0	0	0	0	107	0	107
Quileute	0	0	81	0	0	0	0	0	0	0	0	0	0	0	81
Quinault	0	0	44	0	0	0	0	0	0	0	0	0	0	0	44
"Russianized natives"	3	0	0	0	0	0	0	1	0	0	0	0	0	0	4
Russian	40	6	0	4	0	0	0	10	0	0	0	0	0	3	63
Salish	0	0	42	0	0	17	0	0	0	0	0	0	30	0	89
Shuswap	0	0	0	0	0	0	0	0	0	0	0	0	3	0	3
Tlingit	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
Tsimshian	0	0	0	0	0	0	0	0	0	0	0	0	8	0	8
Sakha (Yakut)	4	0	0	0	0	0	0	174	0	0	0	0	0	34	212
Yukagir (Yukaghir)	7	0	0	0	0	0	0	253	0	0	0	0	0	60	320
Total	1,034	28	188	112	7	68	7	1,168	16	1	11	25	553	200	3,437

Note: This table includes photographs taken by Bogoras and Jochelson prior to their involvement with the Jesup North Pacific Expedition. The nineteen photographs credited to Hastings and Smith are listed under Hastings.

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