Histories of Anthropology Annual

Volume 2

EDITED BY REGNA DARNELL & FREDERIC W. GLEACH

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Editors' Introduction

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As we write these words, the first volume of *Histories of Anthropology Annual* is wending its way from page proofs to print, in time for sales at the American Anthropological Association meetings in November/December. Between that volume and this one lies considerable enrichment of our own understanding of the state of the art in history of anthropology in North America, a process that we expect to be ongoing. We have assembled volume 2 with the aid of a distinguished and enthusiastic international editorial board. Even without the exposure of extant published volumes there has been sufficient critical mass to fill two volumes on an annual basis, and we have a modest backlog toward volume 3. Each volume can certainly stand alone, but we are optimistic that the presence of this regular publication outlet geared to the history of anthropology will contribute to a florescence in this area of specialization. Our task for the coming year will be to further widen the scope of papers presented and to establish a subscription base.

As is appropriate for a journal-like agglomerate of current research in a subdiscipline, there is no single thrust to the papers included here. But several clusters of concerns permeate the papers. First, several anthropologists have contributed historical papers arising from the areas where they have done their ethnographic work. These disciplinary historians combine the methods of archival history with those of ethnographic interpretation and documentation. Their history of anthropology is treated as an anthropological problem. Second, historical research in anthropology often plays around the role of significant figures in the discipline. Professional biography stands alongside the life-history methodology of ethnographers in the field. Third, there is a concern with documenting the particular as well as contrastive features of national traditions; Argentine, U.S., British, Canadian, Swedish, and Russian traditions are explored in this volume. Fourth, there is a belief that institutional infrastructures for anthropological practice provide context for lives and works in the past, with implications for present and future. Finally, interpretative traditions and practices within subdisciplines of anthropology are salient for contributors in very different ways.

These heuristic categories intersect and crosscut, with most papers falling across several. This is, in part, the claim we stake with the title "Histor*ies* of Anthropology." There is a plurality to what we separate out for historicist examination as well as a multiplicity among the audience(s) to which we direct these examinations. Many of the papers are open ended in the sense that their histories raise questions of some urgency for the practice of our discipline.

We encourage all interested parties to participate in these exchanges as readers and writers, and we welcome submissions on any dimension of our discipline's histories.

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1. The Birth of Ciencias Antropológicas at the University of Buenos Aires, 1955–1965

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Sergio Visacovsky, University of Buenos Aires, CONICET-Argentina

> History need not focus the past from the viewpoint of the present, but may rather refocus the present itself, obliging us to see current views in a fresh, often unexpected, even disturbing perspective. History may make the present seem troublingly inconsequential rather than comfortingly inevitable.
>
> Adam Kuper, "Anthropologists and the History of Anthropology"

This paper analyzes the subordination of the academic field to the political domain in twentieth-century Argentina, an issue that has become common sense among scholars and intellectuals who work on the history of the social sciences. In fact the development of academia in Argentina is marked by abrupt political shifts such as right-wing military coups and democratic liberalizations of populistic or even left-wing leanings. However, the effects of the political sphere on academic and intellectual life are far from homogeneous. This aspect still waits for a systematic assessment of the translation of politics into theoretical perspectives, notions, and disciplinary topics. Thus, scientific fields—systems of objective relations constituted by the positions taken by the agents starting from their preceding struggles (Bourdieu 1975)—are crossed by trends and countertrends that affect each discipline and institution in different ways.

This is particularly true in Argentina, where only public universities—that is, universities run by the national state—existed until 1958. Although since then private, namely Roman Catholic, undergraduate *carreras* (degrees) have been offered, public universities have retained their hegemony and prestige in the realm of higher education. However, close dependence upon the state does not entail complete subservience to its powers. Actually, despite the coups of 1930 and 1943, the law setting the University Reform of 1918 initiated a tradition of political and academic autonomy that lasted until 1947. By then Juan Domingo Perón, who had ruled the country as elected president since 1946, demanded closer and more explicit ties among the university system, the executive power, and the Partido Peronista (Peronist Party). State intrusion into university autonomy came about two years after the September 1955 military-civilian coup, the self-described Revolución Libertadora (Liberating Revolution) that ousted Perón. The 1955–57 intervention was meant to "normalize" — actually to purge — the university of Peronist remnants. Autonomy was restored in 1957 and gave way to the so-called golden age of the Argentine university.

In line with these political changes, the new university management attempted to "modernize" and "restore intellectual prestige" to higher education. This meant opening up to the latest scientific developments, using academic knowledge to solve concrete problems, incorporating and developing the latest technology, and filling the university with experts rather than with members of the ruling party (a mandatory requirement during the Peronist years for all those employed as university professors as well as in bureaucratic positions).

After Peronism the university was meant to help, even to lead the "development" and "modernization" of Argentine society at large. Therefore, the creation of the *licenciatura* (a six-year-long undergraduate degree) in psychology, educational sciences, and sociology at the School of Philosophy and Letters (Facultad de Filosofía y Letras) in 1957 was meant to produce experts in empirical research pursuing theoretical work, applied ends, and academic excellence.[1] Clinical psychology, Piagetian education, and Parsonian sociology were thus fostered. The Italian accountant-turned-sociologist Gino Germani became the chairman of the new Department of Sociology and the main protagonist of this academic shift.

One year later another licenciatura was established. The licenciatura in Ciencias Antropológicas (undergraduate degree in Anthropological Sciences) appeared to be part of this larger academic movement, but it was not. Born in the second half of the 19th century concurrent with attempts by the Argentine republican state to build a "modern, European, white" nation in a "mestizo" Latin America, in the early 20th century Ciencias Antropológicas had been taken up by amateurs, paleontologists, archaeologists, historians, geographers, writers, librarians, lawyers, and medical doctors. Museums, scientific research institutions, and university courses could be found beginning in the 1920s at many Argentine universities, such as La Plata, Córdoba, Mendoza, Tucumán, Santa Fe, and Paraná (Arenas 1989–90; Fígoli 1990, 1995). But it was only at the end of the 1950s that a systematic undergraduate degree was established. The licenciatura of Buenos Aires began in 1959.[2]

As we will demonstrate, Ciencias Antropológicas was an anomaly in the context of the modernizing trend that supposedly reached every corner of the main Argentine university, the University of Buenos Aires, in the post-Peronist decade. This case nevertheless helps clarify the myriad ways in which national and academic politics affect each other. It may also illuminate the academic perspectives underlying the Argentine social sciences and the constitution of the field of anthropology in Argentina.

A "Modernizing" Context

The military coup of September 1955 was christened the Revolución Libertadora, since anti-Peronists believed they were saving the nation from "Peronist tyranny," which they likened to German National Socialism and Italian fascism. Names, emblems, icons, literature, and organizations invoking Perón and his wife, Eva, were banned, while the Peronist movement was excluded from this partial democracy.[3] With this ban the de facto government, first led by General Eduardo Lonardi and then by General Pedro E. Aramburu, politically displaced the working classes, who considered Perón to be their main representative.[4] The 1955 coup was supported by a wide alliance that included liberals, Catholics, and radicals as well as socialists and communists, who would soon be banned in the context of the Cold War.[5] The government remained in the hands of the military until national elections took place in 1958.

The Libertadora vowed a return to liberal democracy, with the addition of two new ideals: modernization and development. These concepts remained salient from 1955 to 1973, including periods of restricted democracy (1958–66) and full authoritarian rule (1966–73). Modernization and development underlaid most economic, political, social, and cultural reforms. In the light of developmentalist modernizers such as Raúl Prebisch, Argentina was depicted as structurally "backward," isolated from the "developed" world—the United States and western Europe. From a *desarrollista* (developmentalist) viewpoint, Latin American "underdevelopment" favored the actions of the "enemy within," the local bases of international communism that sought to deepen social conflict. Thus "development" also became a political necessity in order to guarantee national security. Arturo Frondizi, the head of the so-called Intransigent faction of the Partido Radical (Radical Party), won the 1958 general elections with the support of banned Peronists.

Meanwhile, many intellectuals had reorganized against Peronism. Perón had excluded them from the universities and replaced them with nationalists and Catholics. Moreover, political persecution, which was not always paralleled by theoretical dissent, led many intellectuals to outright anti-Peronism. Peronism appeared to them as a "damned fact of life," an anomaly that needed to be explained and hopefully defeated or overcome. Therefore, the attempts to understand the mass movement born in 1945 started to guide the official program to modernize and de-Peronize the working class, which comprised the bulk of its political support (Neiburg 1998).

Some of these intellectuals based their post-Peronist identity on the ideal of "*compromiso*" (commitment) as voiced by French philosopher Jean-Paul Sartre. These early years allowed for the consolidation of an intellectual bloc made up of (political) liberals and left-wing trends. However, this alliance started to dissolve in the early 1960s after the Cuban Revolution, the advent of Latin American and Argentine guerrillas, increasing repression of labor unions, and a new reading of the Peronist experience from the Left. This process contributed to the "Peronization" of the middle classes and of many intellectuals who, simultaneously, adopted Marxism as a bond within their generation.[6] The structuring role of Marxism helped convert the ideal of the "committed intellectual" to that of the "revolutionary intellectual" (Neiburg 1998:22). The main cities of Buenos Aires, La Plata, Rosario, and Córdoba thrived, with books and journals, plays, *vernisages* (presentations of paintings and plastic arts), and meetings (Neiburg and Plotkin 2004). Part of this movement was to reach the *pueblo* (the people, the classic Peronist interpellation), its uses and customs, its knowledge and perspectives. And most of that pueblo was *peronista*. The urban middle-class and the left-wing intelligentsia contacted workers in the cities and in rural areas, first within the setting of modernizing development and its associated disciplines and later as outright political practice.[7]

In fact the notions of "modernization" and "development" in academia not only required private capital investment, but also an impetus to science and academic institutions. The university was reorganized as an agent of social change, but this, it was maintained, could not be achieved by conservative and traditional teachers. Therefore, professors suspected of Peronist leanings were fired, whereas those who had been expelled from 1946 to 1955 were reinstated. The "normalization" of the universities was carried out by opening all teaching positions up to public *concursos* (contests): successful candidates had to demonstrate academic ability as well as a lack of involvement with the outgoing regime. Politics, masked in academic disguise, returned to the stage (Neiburg 1998).

Teaching these reforms included updating the curricula of already existing courses as well as creating new courses presumably crucial to train experts in managing scientific and technical "modernization and development." Therefore, traditional degrees in philosophy and in letters and history and a more recent (1952) degree in geography at the Facultad de Filosofía y Letras of Buenos Aires were joined by sociology, psychology, and educational sciences, each organized as a department in charge of teaching and research.

These new undergraduate degrees were not just *profesorados* (professorships), but licenciaturas, or undergraduate degrees, which would focus on the "scientific study" of social relations and individual behavior. Sociology's self-assigned mission was to depict a scientific — namely quantitative — description of Argentine society and to understand Peronism and its allure to the working classes. To unravel the political phenomenon of Peronism, "irrationality" was not considered just a neutral object, for the object was to "de-Peronize" the working classes envisaged as an "obstacle to development." Their bonds to a charismatic leader prevented them from becoming a "modern and democratic" proletariat (Terán 1991; Sigal 1991; Neiburg 1998).

Sociologist Gino Germani (1911–79) was the cultural hero and founding father of "scientific sociology" in Argentina. He considered the modernization of the universities as part and parcel of national modernization. Argentina needed experts who would analyze the national conjuncture as a transitional stage from traditional to modern and from Peronist to liberal-democratic. Sociology as a profession would carve out its legitimacy through empirical data. As an admirer of Anglo-Saxon sociological and anthropological theory, Germani opposed spiritualist, speculative, and antipositivist German traditions, which reigned in all of Latin America with the exception of Mexico. Other departments and subjects,[8] namely historian José Luis Romero in social history at the History Department and psychologist Enrique Butelman in Social Psychology, later chair of the Psychology Department,[9] took part in this move (Neiburg 1998; Visacovsky 2003). In such a context the Ciencias Antropológicas degree was launched at the University of Buenos Aires, but this process, as we will see, was far from the dominant modernizing trend.[10]

A New Institution for an Old Discipline

A Disputed Fatherhood

Different accounts attribute the paternity of Ciencias Antropológicas either to epistemologist Mario Bunge,[11] or to the already existing faculty, or to anthropologists José Imbelloni, Oswald Menghin, and Fernando Márquez Miranda (Fernández Distel 1985:91),[12] or even to the first student cohort (Lischetti in Colegio de Graduados en Antropología [CGA] 1989: 11). The debates preceding the creation of the degree, which are recorded as part of the ordinary sessions of the governing council of the School of Philosophy and Letters in 1958, highlight the role of Bunge, then a consultant professor. This version ties the birth of Ciencias Antropológicas to the institutions that ruled the university: a rector, the deans' council in charge of the schools (Medicine, Philosophy, Law, Engineering, and so forth), and the government council, or threefold government representatives of the faculty, graduates, and students. During the session of August 18, Bunge set forth a project for the creation of the degree. A student representative, Julio César González, pointed out that a similar project was under consideration by the History Department (where anthropologists taught) and requested that the one presented by Bunge should also be passed over to that department. On September 1 the creation was discussed in the following terms:

> On several occasions members of the distinguished circle of archaeologists, anthropologists, etc. who belong to the School, have turned in several projects and contributions to the Dean, on the creation of the degree of Ciencias Antropológicas. The country needs a suitable group of graduates in Ciencias Antropológicas. At present there are many institutions lacking the adequate technological staff. There are 11 museums, institutes and university departments dedicated to these topics; there are about 10 provincial museums, almost all of them in the charge of "amateurs," and no less than 16 university courses throughout the country. It is also necessary to take into account fieldwork in entire regions waiting to be explored, and private collections which must be classified. Finally, there is an important social problem: the indigenous peoples who are neither assimilated nor keep their primitive condition, to say nothing of the hundreds of thousands awaiting the work of the anthropologist. . . . The Facultad already has an important core of researchers: Professors Menghin, Palavecino, Márquez Miranda, Alberto Rex González[13]—who will be here shortly—, Lafón, Bormida and others who have trained students. . . . Conditions are now ripe for the creation of this degree, which will be the cheapest of all. [Bunge in Actas del Consejo Directivo, Facultad de Filosofía y Letras, Buenos Aires, September 1, 1958]

In his proposal Bunge referred to previous attempts to create the degree, beginning in at least the late 1940s, by the faculty of the Peronist era, namely by José Imbelloni.[14] Bunge also referred to the professionalization of anthropology, meaning that amateurs had to be replaced by experts with scientific aims. Professors of the new anthropological staff would be those prestigious anthropologists who already belonged to the university. This would mean that the enterprise would cost nothing. Unlike its triplet sisters — sociology, education, and psychology — Ciencias Antropológicas needed to carve out a specific métier that had hitherto been auxiliary to history. The text of the resolution of 1958 (Resolution no. 505 del Consejo Superior, Universidad de Buenos Aires 1958), stated that: "Ethnology, Anthropology, Archaeology and Prehistory form a group of similar disciplines which require specific practices, distinct from historiographic practices." Ciencias Antropológicas and History had until then encompassed the teaching of archaeology and anthropology. Now the main distinction of their practice lay in their unique methodology and those techniques aimed at exploiting the Argentine "mine." This metaphor implied that the object of study was conceived as raw material in its "natural" state waiting to be extracted and thus gain value (in this case "scientific" value), which also situated Ciencias Antropológicas in line with nineteenth-century anthropology, devoted to saving cultures under threat of extinction. "Extraction work" was recommended, but this time it was reserved for those with a scientific background.

However, something else was needed. The decree aimed to "encourage research and increase the diffusion of studies of this type, as much for their scientific importance as for their relation to social problems." "Social problems" were, in fact, the hallmark of the times and the requirement for the discipline to be accepted into the legitimate domain of the university and the "modern social sciences" (Madrazo 1985).

Beyond this epistemological framework, neither totally modern nor totally antimodern, the resolution also highlighted the economic side of a new licenciatura. The "initial core of the degree in Ciencias Antropológicas," with already existing courses, would help solve chronic budgetary problems. Interestingly enough, this was never an obstacle to sociology, education, and psychology. It was recommended that the faculty "make full use of its existing [courses] and reduce the number of new topics to an absolutely indispensable minimum." By way of exception, the faculty were entrusted with the responsibility of "including into the project [the new degree] experts to make up the research and teaching body that would be in charge of both the teaching of the courses and additional specialties, and of doing fieldwork." This recommendation had an impact on the new degree. Making "full use of the Faculty's existing courses" meant not only including topics shared with other degrees, but also establishing the basic core of specific (anthropological) topics. These courses would end up being the anthropology courses already taught at the Profesorado de Historia (University School of History Professors). As we will see, most of those professors who had held teaching positions prior to 1958 would now be in charge of teaching the new degree in anthropology. Disciplinary continuity was thus ensured (Vessuri 1992:268–319), although with a novel aspect.

Ciencias Antropológicas had until then been subordinated to history, anthropology, and archaeology, but professors accrued academic power with the new degree, since they now chaired an independent degree program. This gave them full control over teaching, the content of the curricula, and a significant portion carved out of the entire field, including its new graduates. Once in charge of the comprehensive training of a new field of expertise, they assumed a more powerful position within the university concerning financial support for research and publications. Their greatest power lay in the ability to confer a university qualification that would henceforth be superior to that of *profesor* (teacher): the licenciado in Ciencias Antropológicas. Thus the old professors in charge of anthropological courses — physical anthropology, folklore, ethnology, and archaeology of Argentina and the Americas — would now be able to define a new field, its internal structure, and its relations to other disciplines, namely the social sciences and the humanities. But who were these professors?

The First Faculty

To begin with, the professors' permanence at the Facultad contrasted with the academic purge of 1955–56. The Revolución Libertadora had imposed the exclusion of those university professors suspected of Peronist leanings through public academic contests. Archaeologist Eduardo Casanova, director of the Institute of Archaeology, and José Imbelloni, chair of the Institute of Anthropology (which he had founded in 1947), professor of the course in anthropology, and, above all, the strongman of Argentine anthropology from 1947 to 1955, decided to quit their positions and left the university (Guber 2006a).[15] What happened to the rest?

The new faculty was made up of archaeologists Fernando Márquez Miranda (1897–1961), Oswald F. A. Menghin (1888–1973), and Ciro René Lafón (b. 1923); ethnographer Enrique Palavecino (1900–1966); folklore experts Augusto Raúl Cortazar (1910–1974) and Armando Vivante (1910–96); and archaeologist-turned-ethnographer Marcelo Bormida (1925–78). In the eyes of the authorities, Márquez Miranda and Palavecino's prestige resulted from their having been ousted in the Peronist years. However, while Márquez Miranda followed the Culture-Historical School, Palavecino introduced Anglo-Saxon functionalism to Buenos Aires anthropology. Imbelloni was relieved of his post in September 1955, but two men he had introduced into the Ethnographic Museum in 1947–48 remained thereafter: prehistorian Oswald Menghin, who arrived in Argentina fully trained, and the young Italian Marcelo Bormida, who received his Ph.D. just before the Revolución Libertadora at the University of Buenos Aires. Both worked within the Austrian version of the Culture-Historical School. Ciro René Lafón had graduated as Professor of History at the University of Buenos Aires in 1946. In 1948 he entered the Institute of Archaeology, with Imbelloni's assistance, as a technician under Casanova's direction. Lafón admired Imbelloni theoretically, but he differed from him in being closer to popular nationalism. Augusto R. Cortazar, who had graduated as Professor of Letters at the Facultad de Filosofía y Letras, ran a short-lived degree program in folklore, applying Robert Redfield's approach to northwest Argentina.

Lafón, Cortazar, Bormida, and Menghin crossed the Libertadora threshold and remained in the Ethnographic Museum, whereas Márquez Miranda and Salvador Canals Frau, a former professor in Mendoza with some participation in Buenos Aires as well, were reinstated and would henceforth occupy leading positions until their deaths. Márquez Miranda went back to the University of Buenos Aires as Professor of American Archaeology, which he had taught before 1947; Spanish ethnologist Salvador Canals Frau, a follower of Fritz Graebner, became director of the Ethnographic Museum.

Different political trajectories could not be equated to theoretical groupings. Márquez Miranda had taught at the University of La Plata from 1933 until 1947 and had chaired the world famous Museum of Natural Sciences. He had also taught American archaeology at the Ethnographic Museum from 1939 to 1947. He was reinstated in 1955. Lafón had headed the course on archaeology since 1951. Menghin had been appointed "extraordinary professor" since his arrival in 1948; he had received his Ph.D. in philosophy in Vienna and specialized in prehistory. Palavecino, who had worked at the University of La Plata and Tucumán, taught anthropology and general ethnography in Buenos Aires. Cortazar, a lawyer devoted to literature, was also appointed chair of the library of the Facultad. Bormida had been in charge of anthropology since 1954 and since 1957 had held the chair of the department. His academic career had begun in Italy in the natural sciences under Sergi's supervision. He shifted towards physical anthropology and archaeology when he arrived in Argentina and joined Imbelloni's team in 1948. Vivante was a geographer.

In sum, faculty members of the brand new Ciencias Antropológicas had taught anthropology before and after 1947 and could share theoretical views. However, hierarchies expressed a different stand according to political groupings. After the 1955 reform Márquez Miranda became the key political figure, since he had been removed from his positions in Buenos Aires and La Plata by the Peronist regime.[16] In addition he was appointed the first director of the licenciatura in 1958. Palavecino, who had also been reinstated after his displacement from Tucumán in 1947, became the director of the Ethnographic Museum and chair of the department some years later (1959). The institutes were handed over to the youngest faculty members: Lafón was confirmed as the head of the Institute of Archaeology, a position which he had occupied on a temporary basis since 1953, and Bormida was acting director of the Institute of Anthropology from 1958 until 1963.

These changes implied neither a political nor a theoretical reorientation. Although Lafón and Bormida had begun as research assistants during the Peronist administration, their political and theoretical loyalties differed: Bormida had been a member of the Fascist Italian youth "Balilla," whereas Lafón appears to have been closer to the Fuerza de Orientación Radical de la Joven Argentina or FORJA (Radical Orientation Force of Young Argentina), an initially Radical populist group that ultimately supported Perón and populist democratic nationalism. On the academic side, Lafón and Bormida had entered the Ethnographic Museum under Imbelloni's leadership, but Lafón began to work with Casanova while Bormida worked with Imbelloni, a declared fascist (Garbulsky 1987, 1992). Besides, Lafón was born to a middle-class family in the Buenos Aires interior and studied history in Buenos Aires. Bormida not only came from abroad, but from a well-off Roman family. Both adopted the Culture-Historical approach, but they applied it in different directions. Bormida ultimately supported the notion of fixed cultural groups, while Lafón maintained that the *colla* (people of Puna), the *guaraní* (of the northeast), and the criollos were all compatriots who deserved a place in the Argentine nation.

Menghin was an astounding case of academic and political continuity. He was born in Meran (Südtirol) and received his Ph.D. in 1910 at the University of Vienna. The legitimacy of any European anthropologist in the 1930s rested quite firmly on his or her ideas of race, nation, language, and culture. Menghin thought that hybridization had taken place in the Stone Age, so that race, language, and culture were not as overlapping as in earliest times. Nonetheless, in his view difference had persisted to the present; therefore, it was impossible "because of the racial difference, for a Negro ever to become an Englishman" (Kohl and Pérez 2002:564). Thus, the assimilation of Jews was equally unfeasible and even undesirable, since "every people has not only the right but also the moral duty to defend its nationality" (Kohl and Pérez 2002:563). As a Catholic advocate of the Austrian-German unification into a new German Reich, Menghin was highly praised by National Socialists, but he was never affiliated with the party. In 1935 he was appointed rector of the University of Vienna, and "two years later, after greeting Hitler on Austria's 'longest day', he became minister for culture and education in the Seyss-Inquart cabinet." He stayed in this position for just two months (Fontán 2005).[17] Menghin left the University of Vienna in March 1945 and was held captive in the American camps at Ludwigsburg and Darmstadt, West Germany, where he managed to lecture his fellow inmates on prehistory, art, and religion. He entered Argentina with an official ticket and was acknowledged as a distinguished professor at the University of Buenos Aires (Kohl and Pérez 2002:565–66). After being acknowledged as a "world celebrity in Prehistory" by the liberal and anti-Peronist authorities of the University of Buenos Aires in 1957,[18] he was appointed full professor in prehistory at the University of La Plata. He was also honored by the Austrian government for his contributions, which numbered more than 850 titles. It was only after his retirement in 1963 that students of anthropology at Buenos Aires started to denounce his "Nazi past" before the university authorities.[19] He died in 1973 in Argentina and was buried in the city of Chivilcoy, Buenos Aires Province.

It is clear, then, that Bormida's and Menghin's permanence ensured the survival of the Culture-Historical theory advocated by the now excluded Imbelloni. Menghin's Catholic nationalism and Bormida's fascism gave a new slant to theoretical nuances, resulting in a strange confrontation. The initial distribution of the highest positions for the new degree of Ciencias Antropológicas seemed to tie the Culture-Historical theory and liberal anti-Peronism (Márquez Miranda) to the anti-liberal Culture-Historical scholars who had survived the Peronist purge. However, following the death of the first director of the degree, Márquez Miranda, in 1961, Bormida and Menghin gained power. Bormida was appointed acting director of the department from 1962 to 1964 and became the main political and academic figure in the ensuing years (Madrazo 1985:36–40).[20]

Theoretical Orientations

The course plan was divided into five sections: an introductory cycle of 4 mandatory courses; 17 basic courses; 23 complementary courses (of which the students had to choose at least 4); short courses of specialization in one of the offered anthropological branches — ethnology, archaeology, and folklore — and two foreign languages.

The first cycle included the introductions to history, philosophy, sociology, and anthropology. This gave anthropology students contact with students and professors with other degrees at the same Facultad, since courses in anthropology were usually taught at the Museo Etnográfico. Of the 17 mandatory courses, 12 were in anthropology: 1, broadly speaking, in anthropology, 4 in ethnology, 3 in archaeology and prehistory, 3 in folklore, and 1 in physical anthropology.

The 5 remaining mandatory courses were taught by other departments and, therefore, outside of the Museum. In addition to Introduction to Sociology, sociology included Systematic Sociology and Social Anthropology; geography covered Introduction to Geography and Human Geography; and Letras (Literature) provided Linguistics. Complementary courses were grouped into blocks according to their orientation: ethnohistorical, socio-anthropological, ethno-philosophical, biopsychological, and geo-anthropological. Finally, students chose from three anthropological orientations (folklore, ethnology, or archaeology), undertook short fieldwork under the professor's advice, and wrote a short monograph.

Although in the anthropological curricula the Culture-Historical School was clearly dominant, the theoretical landscape was far from monolithic. Born in Austria and Germany in the first decade of the 20th century, the Culture-Historical School was the first reaction against 19th-century linear evolutionism. Culture-Historical intellectuals maintained that cultural materials had spread from primary geographic centers and not by evolution through sequential stages shared by humanity.[21] The Culture-Historical School entered Argentina through Imbelloni, who used it to classify the "cultural patrimony" and the origins of the "American man" (Fígoli 1990, 1995, 2004). This trend prevailed until the beginning of the 1970s as a Kuhnian paradigm, with the support of most established anthropologists in Buenos Aires.[22]

Bormida made his mark on the nascent degree along these lines.[23] He started lecturing in general ethnology in 1959, developing his idea of the birth of the discipline in Universal History (Bormida 1958–59a, b), advocated for Culture-Historical orientations (Bormida 1956), and defined the peoples studied by anthropologists as *bárbaros* (barbarians). In ancient Greek terms this meant "otherness" and "strangeness" (Bormida 1958–59a, b). In his courses he taught different trends from highly idiosyncratic perspectives: evolutionism and historical materialism (in Engels's version) remained within "materialism," and racism was one of its offshoots (Bormida 1958–59a, b). Great attention was paid to the Austrian Culture-Historical School and to Manchester diffusionism and its North American offsprings. Bormida also took up Imbelloni's readings of Gianbattista Vico, existentialism, and irrationalist phenomenology as exemplified by E. Volhardt, R. Otto, and M. Eliade.[24] Following the concerns of the School of Vienna, he pursued theories of primitive religion from the de-generative Christian concept to the developments of A. Lang, W. Schmidt, and R. Pettazoni, together with a critique of "evolutionism," in which he included E. Durkheim and M. Mauss. Italian ethnology was represented by folklore experts such as Cocchiara and Ernesto de Martino.[25] Functionalism, Marxism, social anthropology, and neoevolutionism were marginal. From his European perspective, he cast aside the anthropological developments from the 1930s, particularly those of modern British anthropology. Anthropological theories were depicted according to his philosophical orientation, so empirical evidence was almost ignored in producing theories.

Bormida, however, was not alone. Cortazar took Malinowski's functionalism to the Argentine northwest; in his studies of Chaco Indians, Palavecino closely followed North American culturalism and the British school, his bibliography dealing with the concept of social anthropology as an empirical social science.[26] This concept allowed anthropology students to gain access to Anglo-Saxon traditions—especially the North American ones—even when, for lack of translation, the content of many of those texts was only conveyed orally by the professors. However, Palavecino had also been influenced by the Cultural-Historical school and the concept of "cultural patrimonies" voiced by A. L. Kroeber, C. Wissler, and G. Murdock.[27]

In short, the hegemony of the Culture-Historical trend in Ciencias Antropológicas was not threatened by the professors' theoretical preference, nor was it endangered by the students' search for an "applied," "committed," and "social" anthropology.

Social Anthropology, the Controversial Branch

The degree in Ciencias Antropológicas included a course on *antropología social* (social anthropology), which was taught by the Department of Sociology at the Facultad headquarters. This course was mandatory for future anthropologists and optional for future sociologists. Despite being featured in the original curricula of the Sociology and Anthropology licenciatura, it only began to be taught in 1962. The U.S. anthropologist Ralph Beals was hired by Germani for only one semester.[28] His course focused on fieldwork, the idea of culture, function and structure, the relationship of sociology and anthropology, the history of anthropology, theories of kinship, politics, cultural ecology, evolutionism, adaptive systems in communities "in different stages of evolution," urban studies, ethnographic studies of national cultures, culture contact, and culture and personality. The English and North American traditions were dominant in its reading list.[29] Beals brought texts that were not present in Argentina, but he also made use of the bibliography supplied by the Department of Sociology itself through its system of translations.[30] A large part of these materials were also referenced in Germani's courses.[31]

The fact that the department of Ciencias Antropológicas did not have a course in social anthropology is as notable as the fact that the Sociology Department had created one.[32] Although this issue is the topic of another work (see Guber 2002), it is worth noting here that even before the degree was launched, antropología social had begun to gain adherents as well as foes in the Ethnographic Museum, Ciencias Antropológicas's main home, as well as in the Sociology Department.

Antropología social was practiced in 1957 and 1958 by a peripheral graduate in history, Esther Alvarez de Hermitte, who had taken some courses on social anthropology at Chicago in 1948. Hermitte would receive her MA and Ph.D. in social anthropology from the University of Chicago in 1962 and 1963 respectively. Evidence from her predoctoral phase shows that she conceived of social anthropology as an applied social science to be used in solving social problems. Although this matched the goals of the academic modernizers, she was included neither in the Ciencias Antropológicas faculty nor in the course offered by the Sociology Department. Her exclusion applied both to the 1957–58 and to the post-1965 period, when Hermitte returned to Argentina after serving as research assistant in David Schneider's research on American kinship (Guber 2006a).

Several professors of Ciencias Antropológicas rejected both the focus and the content of antropología social on political and theoretical grounds. Imbelloni certainly was aware of social anthropology, which he dismissed as being overly descriptive and sociological.[33] Echoing the opinions of his teacher, in 1961 Bormida revealed his pessimism concerning the future of this "too sociological" branch. To him it was an extension of functionalism and the study of social and cultural change. The aim of social anthropology was, in fact, applied knowledge. Therefore, it differed from the salvage mission and philosophical perspective that had guided Argentine anthropology up to then:

> [Social anthropology] has gone beyond the field and the traditional objectives of ethnology and has attempted to apply the methods and principles of functionalism to indigenous cultures in a process of western transculturation — and to the same western culture — with the aim of understanding and controlling the processes of change. The practical results of this trend are still very meager due both to the lack of interest on the part of the authorities in applying the advice of the social anthropologists on a large

> scale, and due to their failing training. It is clear that social anthropology is still in its experimental phase and is not anywhere near the day when concrete results will compensate for the enormous amount of effort and the immense amount of material which have been accumulated. [Bormida 1961:486, our translation]

The article containing this paragraph was probably written in 1960 at the latest. Thus Bormida's rejection could have been directed both at the modernizing Department of Sociology that taught the course and at the "native social anthropologist" Hermitte. The latter is unlikely, since Hermitte was by then nominally in the United States (actually she was doing fieldwork in Chiapas).

But to Bormida, anthropology students were very important. To the students the course on antropología social allowed anthropological knowledge to be tied to the "real world."[34] This tie went beyond applied anthropology. Not only the Cuban Revolution (1959), De Gaulle's cession of independence to Algeria (1959), and the postwar African movements of liberation, but also the relative *apertura* (democratic opening) in Argentina in 1957–58 created a climate of political involvement. As we said at the beginning of this paper, commitment was almost mandatory.[35] Consequently, anthropology students looked for a committed anthropology everywhere they could, as they did in the neighboring Department of Sociology and its course in antropología social. Germani had been training anthropologists in the Anglo-Saxon orientations even before Beals's arrival.[36] In his course Introduction to Sociology in 1959, subtitled "Aspects of the Contemporary Crisis," the first unit was termed "Preliminary Concepts of Sociology and Social Anthropology." Here Germani developed the notion of culture and its links to society and personality.[37] In another course under his direction, Systematic Sociology, Germani insisted on anthropological topics and authors and compared Alfred Radcliffe-Brown to the then novel and almost unknown Claude Lévi-Strauss.[38] Although their works were almost unknown in the curricula of Ciencias Antropológicas, Anglo-Saxon and French anthropologists had their own audience beyond and independent of Buenos Aires official anthropology.

Given that the two courses, Social Anthropology and Systematic Sociology, were mandatory to the curriculum of Ciencias Antropológicas, their contents and authors became known throughout the student body, who looked toward sociology as a means of filling what they considered to be an "educational lacunae."[39] And in fact Bormida despised English structural-functionalism, which he grouped with empiricism, a-historicism, and linear forms of explanation (Bormida 1961:486).[40] However, most accounts offered by Argentine anthropologists do not consider the contribution of the course on social anthropology to be decisive to their own definitions of antropología social. Rather, it was a search for a different anthropology that appeared as a generative seed for a "mythical" or "utopic horizon." Students began to call this sort of millenarian academic movement "antropología social" (Ratier in CGA 1989:16). Although the term recaptured the empirical character of a discipline that studied and operated in the present, antropología social moved away from its British origins. Instead of aiming at the study of "social relations" rather than that of "culture," antropología social at Buenos Aires in the mid-1960s referred to a kind of knowledge that could be applied to concrete social reality, solving social problems and thereafter turning into political praxis, that is, committed and even revolutionary anthropology (Ratier in CGA 1989:16).[41] From that point, former students turned scholars or licenciados began to build a kind of knowledge that would be as new as the world to come — or so they believed.

However, when Bormida wrote the piece quoted above, the students of Ciencias Antropológicas still felt comfortable within the "anthro community" that emerged from the Museo Etnográfico (CGA 1989). It was only in 1965 that the controversy was in full flow and social anthropology turned into an academic emblem of a rebellious student sector. In a proposal for a study plan, Lafón supported this view from his own standpoint when he advocated opening up a new branch in Ciencias Antropológicas, antropología social, which should be added to the already existing branches of ethnology, archaeology, and folklore. This would be a sign of both modernization and nationalization:

> A new orientation has to open up, not along the old lines, which is called the 'modern extension of anthropology,' encompassed under the already battered title of Social Anthropology. . . . This does not mean in any way that we disqualify or forget the classic specializations, nor should this be taken to be an extra-academic flag. It is a problem, and it is there, in the very essence of our society and current culture, in its development, conflict and struggle to define itself and set its own stamp. And the Argentine anthropologist should recognize it, with Argentine eyes, at least. [Lafón 1965, our translation]

Lafón was addressing two audiences at once. An external one, the Department of Sociology, had to acknowledge that Ciencias Antropológicas would soon "anthropologize" and nationalize the contents of the course-turned-orientation. However, this should not have concerned the second and internal audience, the regular faculty of Ciencias Antropológicas, since it would never overlap with nor displace the classic orientations.

Moreover, antropología social would not become the Trojan horse of a political (Leftist) takeover. Lafón confirmed, then, that antropología social already marked a pivotal point between academia and politics, closely bound as it was to the intellectual history of Argentina in the 20th century. By means of this academic category, projects were discussed that transcended the university and dealt with the relationships among science, university, and politics; the role of intellectuals; and the place, nature, and boundaries of Ciencias Antropológicas.

Modernity and Antimodernity: *Barbarie*, Anthropology, and the Nation

In 1958 an independent degree entitled Ciencias Antropológicas emerged from two courses taught at the previous school of history professors at the Facultad de Filosofía y Letras. This split was not the result of a scientific crisis of the dominant paradigms; rather, it was a different orientation toward the kind of empirical "objects" concerned. The genesis of Ciencias Antropológicas as an academic space in which to think about the past dates back to the very formation of the Argentine Republic at the turn of the 19th century. Devised then as part of the natural sciences, such studies were directed toward the origins of the "American man," representing a chapter in the making of the Argentine nation. With the fall of archaeologist Florentino Ameghino's thesis concerning the birth of the American man in the Argentine Pampas, a new perspective took hold. With the help of World War II emigrés from central Europe, the Culture-Historical School was welcomed by Imbelloni, an Italian anthropologist with strong ties to Argentine right-wing nationalism. The goal was clear enough: Ciencias Antropológicas would help clarify the Argentine past, but not as a social process; what mattered was rather the cultural past as an ahistorical essence of the American man. This view lumped together contemporary indigenous peoples, the material remains of the past, and folk cultures of peasant communities all toward the same goal: to bear witness to a past that would be recovered.

Imbelloni turned the science of Americanística (the study of American man's origins) (Fígoli 1990:242) into the foundation of anthropology, whereas the Austrian Culture-Historical School subordinated Americanística to anthropology. The death throes of liberalism, the emergence of popular and elite nationalisms, and the threats posed by the huge immigration to the constitution of a "true nation" were key to this development (Fígoli 1990:338–40). Diffusionism had made the question of migration into a central theoretical issue and allowed an account of the Argentine nation to be part of it (Lazzari 2004). In this program folklore was given a primary role in order to "restore the national past by recovering the cultural patrimony (Fígoli 1990:350).

A science formerly devoted to unraveling the genealogy of the Argentine nation, anthropology became a science of the primeval spirit that could still be found amidst Indian populations and some mestizo people in northern Argentina and in the Pampas and the Patagonian south. This transformation took place between the 1930s and the 1950s, with no serious opponents in Buenos Aires. Thus official Ciencias Antropológicas were not concerned with the present, and even some racist attempts at applied anthropology, such as those undertaken by the short-lived Instituto Etnico Nacional, or IEN (National Ethnic Institute) during the Peronist years (Lazzari 2004) were despised from the ivory tower of the Ethnographic Museum.[42]

This standpoint was not only tolerated but also fostered by the rationalist modern university administration. A second look at the underlying premises held by both sides may cast some light upon the links between the modern social sciences and the presumably traditional Ciencias Antropológicas. Bormida expresses this point fairly well, emerging as the strongman of the new degree. His rise took place precisely between the late 1950s and the early 1960s, when Ciencias Antropológicas was launched.

The idea of setting anthropology as a Kultur science within the Culture-Historical framework (Fígoli 1990:318) paved the way for the marriage of historicist and humanist perspectives. Bormida achieved a greater radicalization by presenting history as a metadiscipline, with ethnology as one of its branches, and by opposing, in a Diltheyan vein, the "sciences of nature" to the "sciences of the spirit" (Fígoli 1990:359–85; 1995). Bormida defined the goals of ethnology as the "study of the barbarians" in an effort to overcome definitions such as the study of the "primitive" or "savage," which owe their debt to evolutionary concepts.[43] Following its Greek meaning, "barbarians" were foreigners who did not speak Greek. Bormida believed that the term had been relieved of its pejorative meaning since the time of the Roman Empire (Bormida 1958–59a).[44] Although Bormida thereby expected to found an epistemology of *extrañamiento* (otherness), the operation had most important consequences when applied to the indigenous populations and to the definition of the Argentine nation. The Ona or the Tehuelche, for example, became "foreigners" within the national territory. Argentina was a dual society made of two essentially different halves.[45] According to this view, the social conditions, the national economic and political context, did not impinge upon the definition of the objects of study.[46] Dualism was almost an ahistorical essence of Argentineness.

Of course this standpoint was not new, since Argentina had long been conceived of by its own intellectuals as a nation split into two poles, one traditional and the other modern. This division was inherited from other dichotomies such as port (Buenos Aires)–interior (provinces), unitarian–federal state, and the more significant and long-lived one of barbarianism–civilization coined by politician, educator, and national organizer Domingo F. Sarmiento (Botana 1984; Gallo and Cortes Conde 1987; Halperín Donghi 1987; Solberg 1970; Shumway 1991). As Federico Neiburg has noticed, dualism permeated the whole spectrum of political and intellectual fields (1998). But, some attempts notwithstanding, such dualism was always held on political, not on ethnic grounds (Hernández Arregui 1957, 1960; Jauretche 1958, 1959; Ramos 1957; Romero 1956). After the 1955 coup the quest for the nation reemerged, so that liberal ideas conflicted with nationalist-cum-Peronist ideals. Any sign of diversity that might resist inclusion was ignored. The only truth was a public lie: after 1955 there were neither victors nor vanquished.

Ciencias Antropológicas adopted the civilization-barbarian dualism in a different vein from Sarmiento's and the liberal democrats' interpretations. In this view the aboriginal populations could not mix with people of European descent, nor could they be instructed at public schools, since they were essentially different and lived within the mythic horizon of their forefathers. Thus, the Bórmidian concept of "bárbaro" confirmed the image of the nation as a white and European one, since those barbarians were excluded from Argentineness as peoples whom Argentines could not understand. Meanwhile, in Germani's perspective the "other" of the traditional national interior—the rural, Catholic Hispano-Indian—was envisaged as likely to be changed by means of political education. Therefore, "scientific sociology" assumed the mission of not only explaining the Peronist phenomenon and its diffusion in the popular sectors, but also of contributing to its transformation.

Where did the Other as coined by the Ciencias Antropológicas and the Other deployed by the social sciences at the University of Buenos Aires meet? In a dualistic perspective of the nation. But since such a dualism differed, sociology and anthropology divided their own realms. While sociology saw itself as the leader of the modernizing project at the university and on the national level, the Culture-Historical orientation in Ciencias Antropológicas hid behind the quintessential and immutable question of a non-national "otherness."

However, this division did not go unquestioned. Some anthropology professors, as we have implied, began to refer to the discipline's subjects as members of national society. Referring to the indigenous peoples of the Chaco region, Palavecino expressed his concern about the impact that acculturation would bring on those groups close to extinction (1958–

59:379–89). He specified that Indians had been forced to change their ways of life abruptly when faced with the advance of industrial society. But unlike Bormida, he did not see the decline of traditional ways of life with the romantic nostalgia requiring the speedy collection of cultural remnants. Palavecino did not reject those changes resulting from industrialization, but he claimed that the state should take responsibility and push forward those positive factors that would allow people to better adapt. Anthropologists had to diagnose social decline and provide explanations of why some cultures could not accept the new patterns and adapt to change; they had to generate projects that would help the state solve such problems (Palavecino 1962). Hence the active role of the state as promoter of indigenous policies and the role of the anthropologist as a technician. To Palavecino Indian peoples were, above all, citizens of the nation.[47] His view approached American culturalism and British social anthropology and thus the "technocratic" view that ruled in the Sociology Department. Therefore, within the Ethnographic Museum Palavecino was considered a progressive teacher who, nonetheless, could not lead the students to defy the full-fledged theoretician Marcelo Bormida.

Lafón's work had similar implications, perhaps with more political overtones. Closer to the students, he took them to do fieldwork to northern sites, and although he was an archaeologist, he also recorded the peasants' economy and their material and religious traditions. He and his students soon linked these peoples' daily lives with poverty and marginality rather than with tradition for its own sake (Lafón 1967, 1969–70).

By 1964 and with Lafón and Palavecino's support, modernization and commitment reached Ciencias Antropológicas through a fourth orientation, that of social anthropology, but its success was cut short. Palavecino died in 1965, and the military coup in June 1966 put an end to university autonomy. After the fierce repression unleashed by the police against faculty and students alike on July 28, 1966, known as Noche de los Bastones Largos (Night of the Long Batons) in reference to the Nazi purge of 1934, many professors and most teaching assistants resigned. Thus most advocates for antropología social, such as Esther Hermitte, Hugo Ratier, Santiago Bilbao, and Eduardo Menéndez, left the University of Buenos Aires. Two days later Bormida was promoted as chairman of the Department of Ciencias Antropológicas.

Conclusions

Even though the new degree was the fruit of a "modernizing" political and intellectual context that had also given rise to sociology, psychology, and educational sciences, its orientation was quite different, even contrary to them. This apparent dissonance illuminates both the logic underlying the creation and institutionalization of academic anthropology and the logic underlying a presumably homogeneous, modern, and progressive era of the golden age of the Argentine university. Our question was: On what grounds could antifascist rationalists who fostered the social sciences as social engineering enforce a new degree dominated by irrational essentialism cultivated by Nazi-fascist experts?

We analyzed the continuities and discontinuities between the Peronist era and the Reformist university at the School of Philosophy and Letters and, in particular, at the Ethnographic Museum, the home of anthropologists and archaeologists in the city of Buenos Aires. There we discovered that continuities outnumbered discontinuities. In fact by observing the official university principles that fostered the creation of the degree and looking at the academic trajectories of the faculty involved as well as the curricula, we saw that, with few exceptions, modern contents were taught outside of the department. We also considered how anthropologists forged their empirical and analytical objects of study and how they differed from those of the "modern" disciplines. However, interestingly enough, modern social scientists and Culture-Historical anthropologists converged at some point. Both believed in a nation split by long-lived traditions that were hard to change. To sociologists the split was cultural in nature but its implications were political, whereas to anthropologists the split, along with its consequences, was cultural. Since anthropologists devoted themselves to unraveling the cultural horizon of the Indian peoples living on Argentine territories, they were mostly uninterested in the political aspects of Indian daily life. This rendered them participants in a dualist conception of opposed halves of national society by means of which the hegemonic concept of the nation as white and European was confirmed — in truth more by having discarded it as a legitimate object of study than by dealing with it explicitly as a problem.

Dualist concepts were not new to Argentine intellectuals, either liberals or nationalists; in fact "liberals versus nationalists" was a pertinent way of describing the Argentine intelligentsia, as Nicolás Shumway argued in the 1990s. Modern social sciences concurred with Ciencias Antropológicas that the Indian and mestizo populations of the rural interior belonged to a "traditional" pole dominated by customs and feelings rather than by reason. Nevertheless, for sociology in the second half of the 1950s and the first half of the 1960s, the traditional–modern polarity was dynamic, not static. Sociology's homogenizing concept of the nation was based on political bonds, not on ethnic, linguistic, or religious traits. In this view the "traditional" pole could be modified and even included in the "modern" one. Moreover, the achievement of that transformation held the possibility of resolving the original sins of Argentina. Disputes concerning the meaning and value of social anthropology present a privileged route by which to analyze the conflicts and tensions in Ciencias Antropológicas, stemming from the harmonies and disharmonies of the discipline, and those of the "modern" social sciences. The category of antropología social allowed some professors and many students to initiate a debate about the assignments, the agents, the recipients, and the temporality of Ciencias Antropológicas.

A consideration of Argentine anthropology at the dawn of the 1960s illuminates the process undergone by the country's weakest social science. It also presents a more complete and realistic view of the academic-intellectual processes in the post-Peronist era that may help break the dualistic standpoint that has permeated and still permeates the social sciences in Argentina. Rather than a singular and unusual case in the "modernizing" context, Ciencias Antropológicas at Buenos Aires must be seen in terms of both its near and its distant past. This reveals a little-known aspect of the emergence and institutionalization of the post-Peronist university that brings to light extraordinary continuities in the apparent shaken earth of political turmoil and bloody university politics.

Notes

1. In Argentina undergraduate degrees are called *licenciaturas* and enable *licenciados* (i.e., professional doctors, economists, dentists, engineers, and anthropologists). The master's degree is a 1990s novelty in Argentine higher education, belonging to the American system rather than to the French-inspired passage from *licenciado* to *doctor* (similar to the Ph.D.).

2. The Argentine degree in anthropology is not recent compared to that of other peripheral anthropologies. In Brazil the Graduate Program in Social Anthropology at the National Museum of Río de Janeiro was officially established in 1968, although specialization courses began in 1960 (Cardoso de Oliveira and Ruben 1995:211). In Australia, a country linked to the core United Kingdom in anthropology, the first doctoral program in anthropology was introduced in 1955 at the University of Sydney (Baines 1995:90, 106). However, such cases are hard to compare since their university systems differ; while in Sydney and Río de Janeiro graduate programs were created, in Buenos Aires the graduate program was designed as an undergraduate program eventually leading to a Ph.D.

3. The revolutionary coalition intended to restore a new "democratic" order that would ensure social and political stability without Perón and his party, Justicialismo. The revolutionary government was meant to be a transition, only needed to ward off the danger of an unexpected return of the deposed "tyrant" and to set up a new civilian president, free from the ties of the recent past. However, the subsequent civilian administrations were put under military surveillance, the armed forces interfering in almost every decision, seizing executive power, and closing the National Congress. Henceforth neither civilian nor military governments could guarantee a "new order" (O'Donnell 1977:157–58).

4. The political system developed a dual character based on a weak parliamentary regime with legal political parties and an extraparliamentary, extraparty system based on lobby and the use of direct force. A game of pressure and threats was played by unions, the armed forces, banned and legal political parties, political groupings that would not enter the electoral system, and entrepreneurs from the rural-export and the industrial sectors. Most important issues were negotiated outside the legal system. In this game Perón was the best gambler (Cavarozzi 1983:20).

5. After having participated in elections for conventional constituents in 1957, Communism was banned by Frondizi due to pressures from the United States and the armed forces. The Cold War was in progress, and the National Security Doctrine (NSD) would ensure the American domains. The NSD, taught at U.S. military-training institutes to Latin American officers, was the ideological cement that rendered a meaningful interpretation of the social order in a world with too many new independent nations, two blocs, and the Communist threat (O'Donnell 1972:537; Schoultz 1981). However, in Argentina the Red threat added up to a native foe-Peronism (López 1987:155–59). In fact an Internal State Disturbance Plan (Plan CONINTES) established under Frondizi's administration in 1960 put many Peronist military men, workers, and activists in jail and even justified the execution and disappearance of many Peronist activists.

6. The fact that French philosopher Louis Althusser, who reread Marx with a structuralist eye, recommended the "return" to Freud helped extend psychoanalysis's legitimacy to a wide range of intellectuals. Masotta's first article on Lacan — "Jacques Lacan or the Unconscious in the Foundations of Philosophy" (1965) — referred to Marxism, Sartre, phenomenology, structuralism, and psychoanalysis (Vezzetti 1992). This article was published in *Pasado y Presente*, the journal of the Argentine Gramscians expelled from the Communist Party.

7. Other analyses see this as a show of autonomy by the intellectuals, for whom politics was only an imaginary protector (Sigal 1991:249–51).

8. Germani organized departmental activities around two research projects, one on stratification and social mobility in Buenos Aires and the other on the impact of mass migration to the Río de la Plata (Neiburg 1998:238–39).

9. Butelman and Jaime Bernstein founded the publishing house Paidós. With Gino Germani, Butelman created the collection "Biblioteca de Psicología Social y Sociología," including books by E. Fromm, R. Aron, K. Popper, K. Lewin, C. Wright Mills, G. H. Mead, and anthropologists B. Malinowski, M. Mead, and L. White.

10. Ciencias Antropológicas had its own past in the River Plate area. While anthropology was established in the Museum of Natural Sciences at La Plata (the capital city of Buenos Aires Province) in 1906, the Ethnographic Museum was created in the University of Buenos Aires (UBA) in 1904. This museum housed anthropological, archaeological, and ethnographic work and was to house the two courses in anthropology and archaeology until 1958 (Fígoli 1990).

11. In a speech given by Ciro René Lafón in 1965 for the fourth anniversary of Márquez Miranda's death in 1961, he called the philosopher Mario Bunge "the official father of our Faculty's degree course" (Lafón 1967:14). Born in Buenos Aires in 1916, Bunge received his doctorate in physics and mathematics at the National University of La Plata in 1952. He taught theoretical physics at UBA (1956–58) and at the University of La Plata (1956–59) and philosophy at UBA (1957–62). In 1966 he left for good, going to McGill University in Canada.

12. Lafón states: "After having been the promoter of the degree of Ciencias Antropológicas and the organizer of its entire study plan at the National University of La Plata, the

following year Márquez Miranda joined the team of professors who, through their efforts, contributed to the birth and consolidation of the degree in Ciencias Antropológicas in our School of Philosophy and Letters at the National University of Buenos Aires" (1967: 13–14, our translation).

13. Rex González, professor at La Plata and the founder of an anthropological branch of history at the University of the Litoral (Northeast) did not participate in the Buenos Aires team of professors. Rex González had received his Ph.D. at Columbia University (United States) and was Julian Steward's student.

14. Imbelloni had also drafted a five-year licenciatura that included archaeology, prehistory, folklore, ethnology, and physical anthropology, together with the study of ancient Greek and Latin.

15. Imbelloni (Italy 1885–Buenos Aires 1967) arrived in Argentina in 1914 and by 1933 had been appointed professor of anthropology and general ethnography at Buenos Aires. A doctor in anthropology at the University of Parma, he returned to Argentina and was named director of the Museum of Natural Sciences Bernadino Rivadavia (Buenos Aires). Imbelloni chaired a book series, Humanior, on anthropology and the humanities. After he was expelled from the University of Buenos Aires in 1955, he went to the Jesuit Universidad del Salvador.

16. In his curriculum vitae he states that "at the beginning of 1947 he was removed from all his courses and university posts on account of his contrary stance towards the Peronist dictatorship. He was reinstated by the government of the Revolución Libertadora in October 1955" (Márquez Miranda 1967a:17, our translation). He explains the reasons for his mandatory retirement: "Between the end of that year and the beginning of 1947 Argentine universities lost more than a thousand professors and teaching assistants. The author was among them. Consequently, the promised works could not be published" (Márquez Miranda 1967b:59).

17. In 1914 Menghin founded the Viennese Prehistory Society. His academic career reached its zenith in the following years. From 1928 to 1929 he acted as dean of the School of Philosophy at the University of Vienna; from 1930 to 1933 he became visiting professor at the Egyptian University of Cairo. He was Rector Magnificus at the University of Vienna from 1935 to 1936, and in 1938 he was appointed education minister in Austria, shortly before the German occupation in March of that year (Revista Runa 1959).

18. The dean of the School of Philosophy and Letters, Marcos Morínigo, signed a letter to Rector Rizieri Frondizi referring to Menghin as "a world celebrity in prehistory." Morinigo also stated that Menghin's arrival in Argentina in 1948 was due to an "invitation from the national government." Morínigo added that hiring Menghin would bring to Buenos Aires a broader and deeper knowledge of those materials piled up by archaeologists and amateurs in the museums and would thus promote specific research in prehistory (Revista Runa 1959).

19. Actas del Consejo Directivo de la Facultad de Filosofía y Letras 1966.

20. Social anthropologist Leopoldo Bartolomé calls him the "brilliant, but contradictory tsar of ethnology of the University of Buenos Aires, until his death in 1978" (1982:7). The late Blas Alberti, the first student to graduate from Ciencias Antropológicas and also a militant of the National Left in the 1970s, spoke warmly of Bormida as being the only professor with an ideological and political project (interview by Estela Gurevich, Buenos Aires, 1989).

21. F. Ratzel and L. Frobenius are forefathers of this school. F. Graebner and B. Ackermann represented the German branch, whereas fathers W. Schmidt and M. Gusinde, W. Koppers, and O. Menghin belonged to the Austrian branch (Boschín and Llamazares 1984). The Culture-Historical School divided into "Völkerkunde," or overseas anthropology founded in antique human geography, and "Volkskunde," ethnology practiced within the national terri-

tory and philology. Völkskunde and Volkerkunde shared a preference for area and geographic studies rather than for studies of social difference. Diffusionism and the notion of the cultural cycle, Kulturkreis, were quite influential in North American anthropology (Schippers 1995:236–37).

22. The situation was not extraordinary. German ethnology enjoyed great prestige in the interwar period, since many European anthropologists received their training in Berlin (Schippers 1995).

23. Some of those who were pupils of Bormida at the beginning of the course remember him as the most influential teacher. Hugo Ratier said: "I think that Bormida had a great influence on the course, to the extent that he is still an object of debate. . . . I think he was, without a doubt, the most important figure in Argentine anthropology. [Eduardo] Menéndez said that once, and they got angry with him. He is very important but not my favorite. Besides, he was always on the side of power, he always knew how to wait for the right opportunity, he was always waiting for authoritarian governments. He seduced many people, he was a very seductive figure. He was young, a good teacher, planned each class, one would look for its underlying message" (interview by Estela Gurevich, Buenos Aires, 1989). Blas Alberti shared this view: "He tried to formulate a universalist theory based on Hegel. And from Hegel it is very possible to jump to the critique of Hegel by means of that idea of historical and cultural totality" (interview by Estela Gurevich, Buenos Aires, 1989). See also note 21 above.

24. Gino Germani also encouraged the reading of *Il Mondo Magico* by E. de Martino (1948) and another text dear to Bormida, *Do Kamo: La personne et le mithe dans le monde mélanesien* by M. Leenhardt (1947). Germani used these texts to develop the topic of the emergence of the self and the social formation of the person and the individual. These authors would also have an important place in Marcelo Bormida's production, but for different reasons, since through De Martino, Bormida could justify an experiential reliving of cultural phenomena.

25. "This author — referring to De Martino — makes use both of the phenomenology of culture and that of existence, deep psychology and historical materialism integrated into the Crocian concept of history as contemporary history." He adds: "In his work *Il mondo magico* and the most recent *Morte e pianto rituale nel mondo antico* he shows how reliving participation of cultural facts, ideally distanced from western civilization implies a deepening of its historical links with that civilization and with the experiences proper to them" (Bormida 1961:489). Interestingly enough, Bormida bequeathed De Martino and his master, the antifascist Italian philosopher Benedetto Croce (1866–1952), to his students, who would take over from De Martino to Antonio Gramsci and his writings on popular culture and folklore (see also Cirese, Lombardi Satriani, etc.).

26. The bibliography included *Antropología Teórica* (Theoretical anthropology) by D. Bidney, 1953; *Hombre y Cultura* (Man and culture), the edited volume on Malinowski by R. Firth, 1956; the ethnography *New Lives for Old* by Margaret Mead, 1956, a comparative study of the changes among the Manus of New Guinea between 1930 and 1956; Audrey Richards's *Hunger and Work in a Savage Tribe*, 1932; *Acculturation: A Study of Cultural Contact* by Melville Herskovits, 1938; and *Anthropology Today* by Alfred Kroeber, 1953.

27. This issue has been neglected in most histories of Argentine anthropology. When the degree course was created, a guide for collecting cultural data by G. Murdock was in general use.

28. Ralph Beals (1901–85), the first Ph.D. in anthropology at the University of California–Los Angeles in 1936, was president of the American Anthropological Association in 1950, during McCarthyism. He was advisor for Latin American issues from 1955 and in 1958

became president of the Southwestern Anthropological Association. Beals wrote on northern Mexico, his main contributions to the field of anthropology dealing with the warfare of Native peoples. He retired in 1969 and died in 1985 (Patterson 2001). His best known book in Argentina was *An Introduction to Anthropology* (1953), a textbook coauthored with Harry Hoijer (Aguilar, 1963).

29. These included Robert Lowie, Bronislaw Malinowski, Sigfried Nadel, A. R. Radcliffe-Brown, Meyer Fortes, E. E. Evans-Pritchard, Ralph Linton, Julian Steward, Leslie White, Robert Manners, Oscar Lewis, Robert Redfield, Ruth Benedict, Margaret Mead, and George Foster. There were also authors in the French tradition, such as E. Durkheim and C. Lévi-Strauss, and even Latin Americans, such as G. Aguirre Beltrán (*Programa de Antropología Social*, 1962).

30. Many translations in Spanish were already available: Mexican publisher Fondo de Cultura Económica published *Culture and Personality* and *The Study of Man* by Ralph Linton (1945); *Anthropology* by Clyde Kluckhohn (1949); *Man and His Works* by Melville Herskovits (1952); and *Fundaments in Social Anthropology* by Sigfried Nadel (1955). Other work was published locally: Ruth Benedict's *Patterns of Culture* (Sudamericana, 1939); and Margaret Mead's *Coming of Age in Samoa* (Abril, 1945) and *Sex and Temperament* (Abril, 1947). Augusto R. Cortazar translated *A Scientific Theory of Culture* by Bronislav Malinowski (Losada, 1952). The list of works available in Spanish also included Franz Boas's *The Mind of Primitive Man*, (Lautaro, 1947); L. Levy-Bruhl's *Les fonctions mentales dans les societés inferieurs* (Lautaro, 1947) and *La mentalité primitive* (Leviatán, 1957); and Ernesto de Martino's *Magia e civiltá: Un'antologia critica fondamentale per lo studio del concetto di magia nella civiltà occidentale* (Claridad, 1948). This sample makes clear that there was a public audience prior to 1958 that exceeded a merely anthropological one.

31. Short translations were: an article by Malinowski, "Culture," from the *Encyclopaedia of the Social Sciences*; chapters from Leenhart's *Do Kamo*; and a summary of *Il Mondo Magico* by De Martino.

32. By way of comparison, social anthropology had been an established discipline since the early 20th century in the United Kingdom, with James Frazer creating the first chair in social anthropology at the University of Liverpool in 1908. However, in a major country such as France a chair in anthropology was established as late as 1958, at the College de France. If in theory A. R. Radcliffe-Brown, following J. Frazer, considered anthropology to be a branch of sociology dealing with primitive societies (Radcliffe-Brown 1986:11), the teaching of anthropology and research in the United Kingdom fell to the institutions and departments of anthropology (Kuper 1973).

33. Alberto Rex González, personal communication (2003).

34. Hugo Ratier pointed out the close relationship between social anthropology and "working on a concrete problem." Ratier defined himself as a "grass roots anthropologist." Accordingly, he worked in a first-aid post and health center as part of a project coordinated by Gino Germani in a squatter settlement called Isla Maciel (interview by Estela Gurevich, Buenos Aires, 1989).

35. Blas Alberti maintained that those students closer to Bormida noted "the disjuncture between that theory and our own. So we started to criticize it and we broke away radically from Bormida's perspective, but I, for example, continued in that tradition of European thought, which I consider to be more rooted in the totalizing perspective" (interview by Estela Gurevich, Buenos Aires, 1989).

36. Germani's interest in socio-cultural anthropology was perhaps due to the influence of authors such as Malinowski, Radcliffe-Brown, and North American culturalism within sociological functionalism in the United States. Germani's concept of a "unified social science" should not be discarded either.

37. The bibliography was composed of authors such as R. Benedict, M. Herskovits, B. Malinowski, R. Linton, M. Mead, C. Kluckhohn, R. Redfield, G. Murdock, and F. Nadel (Programas de Materias, Introducción a la Sociología and Sociología Sistemática 1958).

38. The first edition of *Structural Anthropology* by Lévi-Strauss was published by the Editorial Universitaria de Buenos Aires in 1968; the translation of the work was by Eliseo Verón, with the collaboration of Eduardo L. Menéndez, pupil in the first year of Ciencias Antropológicas.

39. "Faced with the recurring need for theories, we turned to Sociology, which was more scientifically based," Mirtha Lischetti recalls (CGA 1989:12). In an interview with Estela Gurevich in 1989, Edgardo Cordeu (a member of the earliest cohort) recalled that around 1962 or 1963 there was a great deal of disenchantment with Culture-Historical theory, in favor of structural-functionalism.

40. Some of these criticisms were also inherited from sectors connected with Marxism and the national left. Many years later, Madrazo—who did not belong to the Buenos Aires student community—still remembered: "In sociology important aspects of national reality were tackled empirically, with a view to forming strategies for development, with a general orientation which was functional-structural and a-critical and scientifically inclined; in anthropology there was no systematic programme of research of that kind, nor any intention of that kind" (Madrazo 1985:36).

41. "The other project, an anthropology committed to social reality" (Herrán 1990:108).

42. The Instituto Etnico Nacional was a branch of the Dirección General de Migraciones (Direction of Migrations), meant to control migrations while building the Argentine nation and its Argentine people. Canals Frau was its first director (Fígoli, 1990:306; Villalón 1999; Lazzari 2004).

43. The same applies to the concept of "barbarie" applied by Sarmiento in "Facundo," more akin to the paradigm of progress, organization, and social evolution (Oszlak 1985; Mayo and García Molina 1988; Shumway 1991).

44. Bormida defined the object within the tradition of classic studies; he traced anthropological genealogy back to the Greco-Latin-Christian tradition.

45. This position owes its debt to the differentiation that German ethnologists had established between non-Western societies, termed "Naturvölker," and inhabitants of the Western countries, considered "Kulturvölker." To Bormida the opposition applied within the national territory (Schippers 1995).

46. This does not mean that in his works Bormida did not realize the social situation of the indigenous people, just that this was beyond his objective of compiling traditional mythic narratives (i.e., Bormida and Siffredi 1969–70:199–200).

47. "It is to the benefit of the Nation and the State, to the *indio* and to the white man with whom he lives, that we initiate once and for all a rational policy with regard to the aborigines of the Chaco which breaks the cultural walls which separate them. It is intolerable that in our time the Indian and white communities should preserve within the country a racial and social discrimination which the basic law of the Nation repudiates and good sense condemns" (*Resolución* No. 1517, 1963, of the document presented by Enrique Palavecino for the renewal of his post as professor).

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2. *"My Old Friend in a Dead-end of Empiricism and Skepticism"*

Bogoras, Boas, and the Politics of Soviet Anthropology of the Late 1920s–Early 1930s

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In 1933 the journal *Sovetskaia Etnografiia* published a translation of Franz Boas's recent paper "The Aims of Anthropological Research." A commentary accompanying it contained the following passage:

> The empiricist school of ethnography, headed by Boas and other American scholars, is, on the one hand, an apolitical one. . . . On the other hand, it prefers to retain its [political] liberalism. However, it has found itself in a dead end, helplessly lost in its own contradictions. These contradictions arise from class contradictions, even though Boas and his friends do not mention the word "classes."
>
> In the most generous view, this empiricist and skeptical school is a kind of *quantité negligible*, something that plays almost no role in that fierce class struggle, which is beginning to burn very brightly in all spheres of social life, from the real battles on the barricades to the polemics in the calm and hefty scholarly periodicals. [Bogoras 1933:193]

It is quite ironic that the author of this critique was not a die-hard Soviet Marxist but an old colleague and a close friend of Boas, Vladimir Bogoras.

In fact, in his comments, Bogoras tried very hard to find ideas that bore at least some resemblance to the ones that came to dominate Soviet anthropology. Moreover, the introductory editorial statement, most likely written by Nikolai Matorin, a relatively young head of a recently created Institute of Anthropology and Ethnography, a Communist Party member since 1919, and a dedicated Marxist, referred to Boas as "a respected friend of the Soviet Union and a famous American anthropologist" as well as a "courageous opponent of the imperialist war."[1] Yet at the same time, it warned Boas about following "a dangerous route, on which he finds himself probably against his own will." It went on to argue that the "treasures of factual data" collected by American anthropologists required a radically different analysis, that is, the kind advocated by Lewis Henry Morgan and Friedrich Engels. Both Matorin and Bogoras agreed that Boas and his followers were stuck in a "dead end of empiricism and skepticism" (Editorial Introduction to F. Boas's "Aims of Anthropological Research" 1933:176).

From Bogoras's correspondence with Boas, we know that the former was eager to publish what he saw as his American colleague's very important paper (APS, Bogoras to Boas, March 3, 1933).[2] To make this possible, however, he had to accompany it with this rather harsh criticism. Did Bogoras betray his old friend, whose brand of anthropology had had such a strong influence on his own earlier work? To answer this question, I explore the history of the relationship between the two men as well as the politics of Soviet anthropology, particularly during the turbulent era of the late 1920s–early 1930s.

Friendship and Scholarly Cooperation Prior to 1917

The history of Franz Boas's cooperation with Vladimir Bogoras, Vladimir Jochelson, and Lev Shternberg is well documented and hence will only be briefly summarized here.[3] Arrested for radical Populist (Narodnik) activities in 1886, Bogoras spent a year and a half in solitary confinement and was then sentenced to ten years of exile in the Kolyma region of eastern Siberia.[4] Like a number of other exiled Populists, including Jochelson and Shternberg, he began recoding data on the folklore and other aspects of the culture of the local Russian and aboriginal population (particularly the Chukchis). He also began writing fiction, using the life of the exiles and the local people as his subject matter. Instead of his last name, he used the alias "Tan." Later in life he began using a hyphenated name: "Tan-Bogoras." As Elena Mikhailova points out, "For Bogoras this hyphenated name has a special meaning: he saw it as a reflection of the different aspects of his work and, in a broader sense, as his two hypostases, his two different interests and goals in life" (2004:95).

After his first ethnographic publications appeared in the bulletin of the Eastern Siberian Division of the Russian Geographic Society, he became a recognized expert on Chukchi culture and language. In 1898, thanks to the intercession of several prominent members of the Academy of Sciences including Vasilii Radlov, the head of the Museum of Anthropology and Ethnography (MAE), Bogoras was able to obtain official permission to reside in St. Petersburg in order to continue working on his large body of linguistic and ethnographic data. His scholarly publications of the 1898–1901 period were well received by Russian ethnologists and linguists. The former exile also resumed his literary and political activities, publishing articles in left-wing periodicals and speaking at gatherings of the liberal intelligentsia that were legal but carefully monitored by the police.

In 1899, when Boas was planning his international Jesup Expedition, he invited Bogoras and Jochelson to conduct ethnographic research in eastern Siberia. In preparation for field research, the two Russian ethnographers were asked by Boas to read anthropological works on the cultures of the American side of the Bering Straits, including his own. Bogoras arrived in New York in early 1900, sailing to Vladivostok in May and arriving in Anadyr in the summer. In the course of an expedition that lasted for a year and a half, Bogoras covered a huge territory and collected a very large body of museum specimens and ethnographic data on the Chukchis, the Siberian Yupiks (Eskimos), and other ethnic groups. After a brief stay in St. Petersburg, he went back to New York to work on preparing his materials for publication. For this work, which took place at the American Museum of Natural History, he received a salary of $150 a month. During his year-and-a-half stay in the United States, he learned a great deal from Boas and came under the strong influence of Boasian historical particularism while still clinging to certain aspects of classical evolutionism. It should be pointed out that theory was never Bogoras's forte. His theoretical and topical interests often changed, depending on what he happened to be reading at the moment.

During Bogoras's stay in the United States, he and Boas became good friends.[5] The time they spent together changed Boas's view of the Russian ethnographer for the better; in his own words, "Bogoras turned out to be a very likable person" (Cole 2001:41). As before, Bogoras combined scholarly work with extensive travel throughout the United States and Canada as well as literary and political writing. He was anxious to return to Russia, where a revolution was in the making. When Boas failed to obtain a grant for Bogoras from the Carnegie Institution, he had to scale back his Russian colleague's salary to $150 per chapter. For this and other reasons, Bogoras left the United States for Europe in late 1903 without completing his work on the Chukchi and Yupik materials. He spent most of 1904 abroad. In the summer of that year, he took part in the Fourteenth International Congress of Americanists (ICA) in Stuttgart, presenting a paper on Chukchi religion (Bogoras 1906). Thanks to Boas's invitation, the Russian "ethno-troika" became members of this society and regular participants in its meetings.[6]

Upon returning to the Russian capital, Bogoras plunged into radical journalism and leftist politics. In the course of the first Russian Revolution (1905–7), he took an active part in organizing the first All-Russian Congress of Peasants and a left-leaning Labor Group (*Trudovaia Gruppa*) of delegates of the first state Duma (Parliament); in the fall of 1905 he joined the Moscow Central Strike Committee, which played a major role in the revolutionary uprising in that city. In 1906 Bogoras joined the organizing committee of a moderate Party of People's Socialists, composed mainly of intellectuals.

Boas followed the news of the unrest in Russia with great interest. While sympathizing with the revolutionaries, he was worried about his Russian colleagues and especially the passionate Bogoras. Moreover, he was greatly concerned that members of the ethno-troika would be neglecting their work on the Jesup Expedition manuscripts.[7] His fears proved correct, when on December 4, 1905, he received a telegram informing him of Bogoras's recent arrest for taking part in a congress of the Peasant Union. Boas immediately wrote to Radlov, asking him to help secure Bogoras's release. He also sent a letter to the American ambassador to Russia, asking him to try to secure Bogoras's field notes and manuscripts having to do with the Jesup Expedition (AMNH, Boas to Shternberg, January 22, 1906). Fortunately, Bogoras was soon out on bail and, by the beginning of 1906, was safe in Finland, where he resumed his scholarly work (AMNH, Bogoras to Boas, January 23, 1906).

However, his revolutionary zeal had not been dampened. As he wrote to Boas in April 1906, "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" (APS, Bogoras to Boas, April 6, 1906). Despite his own socialist leanings, Boas believed that science came first. As he lectured Bogoras in a letter of April 22, 1906, "If events like the present happen only once in a century, an investigation by Mr. Bogoras of the Chukchi happens only once in eternity, and I think you owe it to science to give us the results of your studies" (APS). His August 23, 1906, letter to his Russian colleague had an even sterner tone:

> Conditions in your country must be very discouraging, and I am under the impression that what is going on now must be so uncongenial to you that I somewhat hope that you may take refuge for the troubles of the day in your scientific work. I am responsible for the publication of the scientific results of your researches, and I rely upon you not to shirk your part of the responsibility. I wish you could go abroad for a couple of months for the purpose of applying yourself to this work. [APS]

A November 23, 1906, letter from Bogoras contained more regrets about his lack of progress but expressed the same sentiment: "right now my mind and soul have no free place to let in science" (APS). Nonetheless, the enormous Chuckchi project was completed and its results appeared in a series of volumes of the Jesup Expedition (Bogoras 1904–9, 1910). His monograph on the Siberian Yupiks also appeared in the Jesup Expedition publication series (Bogoras 1913). During the pre–World War I years he also wrote a great deal of fiction, so that his collected literary works, published in 1911, amounted to ten volumes.

Bogoras's troubles with authorities did not end with the defeat of the first revolution. According to his autobiography, between 1905 and 1917 he was the subject of close to 20 legal cases and investigations (Bogoras 1927:447). His worst experience was an arrest in late 1910–early 1911, when he had to spend several months in solitary confinement. As a result he suffered from a serious liver disease and depression. Once again his American colleague and friend came to the rescue, appealing personally to the Russian minister of justice for clemency and organizing a resolution passed by the American Anthropological Association (AAA), which requested that the same minister permit him to have his field notes delivered to his cell and correspond regularly with the publisher of the Jesup Expedition series (Boas to Shtsheglovitoff, October 12, 1910, APS; Boas to Shtsheglovitoff, February 28, 1911, AMNH). Thanks to this intercession as well as the appeals by Bogoras's colleagues from the Russian Academy of Sciences, his sentence was shortened and he was released in April 1911 (APS, Bogoras to Boas, April 6, 1911).

The Great War interrupted Boas's communication with his Russian colleagues. Publishing academic works became difficult, especially since the publisher of the Jesup Expedition volumes was located in Europe. Thus the Koryak texts, which Bogoras had been working on in the early 1910s, appeared in 1917; a volume of Yukaghir, Lamut, and other folktales in 1918; and a detailed description of Chukchi language, prepared for Boas's *Handbook of American Indian Languages*, only in 1922 (Bogoras 1917, 1918, 1922). Moreover, it was now Boas's turn to experience great difficulties in concentrating on scholarly work. After all, his native country was at war with his new homeland. Being opposed to the American involvement in the war, Boas even joined the Socialist Party for a time because of its similar stand on the issue (see Stocking 1992:102–6). Ironically, his St. Petersburg colleagues, like many other Russian socialists, became ardent patriots and "defensists." Bogoras went as far as volunteering to serve in the army and for three years took part in military campaigns on the eastern front as a medical orderly.

Bogoras in the late 1910s–late 1920s

Like many other non-Bolshevik leftists, Bogoras was opposed to that party's takeover. As he wrote in one of the unpublished versions of his autobiography, "at that time, we — the intelligentsia — were aligned with the bourgeoisie, with the ruling classes. And how much did we mock and curse that revolution" (Mikhailova 2004:115). Along with other members of Petrograd's intelligentsia, he suffered greatly during the Civil War. As he wrote in a published version of his autobiography, "I experienced the entire . . . Golgotha of that era of starvation: lost my family, ended up alone and naturally was angry [at the new regime]" (Bogoras 1989:448).[8] Despite a position at the Museum of Anthropology and Ethnography given to him by Shternberg, Bogoras remained hungry, sick, and very bitter. His hostility toward the new regime was undoubtedly strengthened by its persecution of the remaining active members of his own Party of People's Socialists as well as the Socialist Revolutionaries party (PSR), many of whom had been Bogoras's comrades from the People's Will Party. Although he was never arrested, his friends Shternberg and Jochelson were briefly detained during the 1921 unrest among Petrograd's sailors and workers. A year later, the regime organized the first major show trial in Moscow, which featured a large group of prominent PSR members accused of anti-Soviet activities. Several of them were sentenced to death. Bogoras joined Shternberg and other veterans of the People's Will Party in signing an appeal to the government that asked for leniency towards the accused (Pokrovskii 2002:558–59; RAN, 282/1/102:15–16).

Despite his anti-Soviet stand, like many other members of the Russian intelligentsia, both émigrés and those remaining in the USSR, Bogoras was eventually seduced by a limited improvement in the intellectual and economic life during the era of the so-called New Economic Policy (1921–29) into believing that the Bolshevik regime was undergoing serious liberalization. Along with many other scientists, scholars, and members of the artistic community who had previously refused to cooperate with the authorities, he announced his willingness to cooperate with the regime.[9]

Bogoras's love of public life and his enormous energy found a new outlet when he and Shternberg became the founders of the new Leningrad school of Soviet ethnology centered on the Ethnography Division of the recently established Geography Institute (Ratner-Shternberg 1935; Gagen-Torn 1971; Staniukovich 1971).[10] Starting in 1921, Bogoras taught a variety of courses there, including Evolution of Economy and Technology, Material Culture of the Prehistoric Peoples, Culture of the Palaeoasiatic Circle of Peoples, and Shamanism as a Social Phenomenon. While Shternberg was the intellectual leader of the new school, Bogoras devoted much of his time to the practical tasks involved in organizing and maintaining an extensive program of ethnographic field research by the students. He authored a program for field research (published in 1928) and, taking advantage of the new government's increased concern with bringing the non-Russian peoples under its control, obtained substantial funding for that research.

Unlike Shternberg, who remained more interested in such theoretical issues as the evolution of kinship and social organization and was not eager to shift his attention to the study of contemporary topics that were of greater interest to the new regime, Bogoras was quite willing to combine his old research interests with ethnographic research on the emerging new Soviet society. Thus in the 1920s he edited several collections of ethnographic sketches by his students from the Geography Institute, which bore such titles as *The New and the Old Social Life*, *Revolution in the Countryside*, and *The Jewish Shtetl during the Revolution* (Bogoras 1924, 1925a, 1926a).

Along with Shternberg, Bogoras fought against the efforts of the Ministry of Higher Education bureaucrats and their allies among the leftist faculty and students to politicize the Geography Institute's curriculum. In fact my own archival research revealed that in the early-to-mid-1920s the institute was much more independent from the Soviet ideological pressure than was Leningrad University. Nonetheless, by the mid-1920s Geography Institute students became obligated to take courses on the Soviet constitution and the history of the Communist Party of the USSR. When in 1925 the institute was transformed into the Geography Faculty of the university, the number of its mandatory "ideological" courses was further increased at the expense of anthropology and other specialized courses (Ratner-Shternberg 1935:144–45).[11]

Despite these setbacks, the Ethnography Division of the Geography Faculty remained one of the two main centers of ethnological education in the country, the other being the Ethnology Faculty of Moscow University (Solovei 1998:124–34).[12] After Shternberg's death in 1927, Bogoras became its dean and was able to hold that position until the closing of his division five years later. Determined to continue Shternberg's work, Bogoras spearheaded the campaign for the establishment of the Ethnographic Research Institute at the Ethnography Division of the university. Although such an institute was never created, a group of leading ethnology faculty and graduate students formed a scholarly union in 1928, which organized seminars and lectures (RAN, 250/3/173:114–16).

Despite Bogoras's efforts, an increased ideological pressure on the humanities and especially the social sciences, which began in the late 1920s, further politicized the atmosphere at the Geography Faculty. Bogoras anticipated these troubles as early as 1927 when he wrote to Boas that with Shternberg's departure, "my own position [at the University?] has become difficult and even a little awkward" (APS, Bogoras to Boas, October 2, 1927).[13]

According to the same letter, he was being criticized not only from the left by the young Marxists but also from the right by the conservative members of the Academy of Sciences and the university, who had always viewed him as a "damned socialist." Bogoras believed that it was this opposition of the old guard that prevented him from being elected to the Academy of Sciences and kept Shternberg out until 1924.[14] In 1927 there suddenly arose an opportunity for Bogoras to break through this barrier. Determined to undermine the Academy's relative autonomy from government and party control, the regime decided to orchestrate an election of 42 new members to this distinguished body, with most of them being either Communists or at least Soviet sympathizers. To have a better chance of having persons loyal to the regime elected, the government introduced a new system of nomination: from now on, any scholarly body or institution and even individuals could nominate a person for this election. Anticipating that he would be nominated but would be opposed by the Academy's old guard, Bogoras appealed to Boas for a letter of recommendation to the Academy (APS, Bogoras to Boas, May 17, 1927).[15]

Bogoras was right: upon his own recommendation, the Committee for Assisting the Peoples of the Northern Borderlands ("Committee of the North"), which Bogoras had helped establish in 1924, nominated him for membership in the Academy under the rubric of the study of languages and cultures of the numerically small peoples of the North. Reflecting the new ideological climate in the country, the memo from Petr Smidovich, the head of the committee, stated: "All of the work being done by Bogoras represents constant linkage between scientific research and practical social work among the peoples being studied. We believe that it is imperative to add this kind of work to the broad spectrum of activities carried out by the Academy and give this work an opportunity to become broader and deeper" (RAN, Smidovich to the Soviet Academy of Sciences, 250/3/3:10–11).

Once again, Boas was willing to help his Russian friend. In early June 1928 he mailed his recommendation to Sergei Oldenburg, the Academy's secretary. Boas understood very well what he had to say: his letter not only praises Bogoras's work on the culture and language of the Chukchis and other eastern Siberian peoples, but also concludes with the following statement: "His contributions are, however, of much wider scope. The investigations of the people of the Soviet Union in their bearing upon modern social problems are of the greatest value, both from a scientific and from a practical point of view. The plan of these investigations has been dictated by a thorough mastery of the methods and aims of ethnological science" (RAN, Boas to Oldenburg, June 4, 1928, 250/3/3:10–10a). The same file that contains this letter of recommendation also includes an explanation, written by some Bogoras supporter, of why such a recommendation is highly appropriate. Boas is portrayed in this document not only as a leading foreign anthropologist, but also as a progressive scholar who had the courage to oppose the American involvement in World War I and the use of anthropological research as a cover-up for intelligence gathering in Mexico by Americans. Not surprisingly, Boas's brief affiliation with the Socialist Party is also mentioned. Finally Boas's recommendation is interpreted as an example of a sympathetic attitude toward the new Soviet society that exists among "the best representatives of science and intelligentsia" (RAN, Boas to Oldenburg, June 4, 1928, 250/3/3:2–3).

Despite Boas's and Smidovich's endorsement, Bogoras was not elected to the Academy in 1928.[16] While a negative attitude toward him shared by at least some of the conservative academicians must have played a role here, it is conceivable that his active involvement in "applied anthropology" (via the Committee of the North) and his international research ventures had also worked against him. After all, that same year the academicians did elect another former Populist and PSR supporter, Eduard Pekarskii, to fill the slot vacated by Shternberg. Like Bogoras, Pekarskii had become a linguist and ethnographer while in exile in Siberia. Unlike Bogoras, however, he was a more academic scholar who had stayed away from politics since the early 1920s.[17]

By the early 1930s ethnography had lost its status as a separate discipline and began to be viewed as a subsidiary one, whose subject matter consisted of the history of the prehistoric or preclass societies and the survivals of the culture of those societies within the "more advanced social formations." A large-scale campaign against the "bourgeois" and "right-wing" professoriat, which began in the early 1930s, included a series of reorganizations of the university system. The Ethnography Division of the Geography Faculty fell victim to the latter and was closed down in 1932 (Solovei 1998:210–11).

In the early 1920s, in addition to teaching, Bogoras became deeply involved in the work of the above-mentioned Committee of the North, which was affiliated with the All-Russian Central Executive Committee of the Soviets (the main legislative and executive branch of the government) (Slezkine 1992, 1994:150–83; Vakhtin 1994). Drawing on his own experience in Siberia and inspired by populist ideology, Bogoras advocated government policies that would protect the territories used by the indigenous northerners for subsistence activities from further colonization by the newcomers. He also called for a gradual incorporation of these societies into the new socialist state and emphasized improving educational and medical services for the Natives. Finally he argued for the need for the local representatives of the state to have a good grasp of the language and culture of the people they had to work with. Of course it was the ethnographers' task to provide such information. Throughout the 1920s many of the students of Shternberg and Bogoras combined ethnographic and linguistic research with work aimed at "sovietizing" the Natives. Not surprisingly, by the late 1920s the committee's proposals for a very gradual modernization of the indigenous societies of Siberia were rejected in favor of rapid colonization and industrialization; by the mid-1930s the committee was disbanded altogether.[18]

Another one of Bogoras's pet projects was the creation in the late 1920s of the Institute of the Peoples of the North at Leningrad University. This was the first specialized institution of higher learning for the indigenous Siberians. Bogoras taught and conducted a good deal of linguistic and ethnological work at the institute. Among his major projects was the first primer ever in the Chukchi language, published in 1932. Two years later he produced a Chukchi grammar.

As this brief review demonstrates, by the late 1920s Bogoras was facing a variety of restrictions on his academic, educational, and public-policy ventures. However, the worst was yet to come.

Stalinization Sets In

In the 1920s there was still no Marxist anthropology in Russia, even though some anthropologists were Marxists. Most of them belonged to the younger generation, but some of the older anthropologists also tried to combine Marxist methodology with other theoretical approaches. Thus in his 1928 book *The Spreading of Culture on the Planet: Introduction to Ethnogeography*, Bogoras tried to combine Friedrich Ratzel's anthropogeography with Marxism. However, as T. D. Solovei points out, "most of the time the influence of Marxism manifested itself in the terminology being used as well as materialist dialectic (understood mechanistically)" (2001:107). In the 1920s scholars such as Shternberg and Bogoras viewed Marxism as one of a number of scholarly paradigms rather than as the "only correct one," as the next generation of Soviet anthropologists began to assert in the 1930s.[19] In the late 1920s to early 1930s, most of the older ethnographers continued to view anthropology as a broad discipline, a kind of macroscience that encompassed a variety of social sciences and humanities. This view soon came under attack by the dogmatic Marxists (Solovei 2001:107).

During this time the political climate in the Soviet Union began to change drastically. The era of the New Economic Policy — with its limited ideological pluralism — ended, and Stalin's "revolution from above" began. The latter entailed rapid modernization of the socioeconomic system accompanied by the establishment of a rigid ideological regime. This revolution from above included a large-scale attack on the so-called bourgeois specialists in industry, science, and higher education, with a series of show trials of members of the intelligentsia accused of wrecking Soviet industrial ventures. An atmosphere of suspicion and hostility toward the older generation of "experts" began to be cultivated. In the late 1920s–early 1930s the Academy of Sciences, a bastion of non-Communist scholars that continued to remain semiautonomous, came under vicious attack, which resulted in the arrest of a number of prominent academicians and an engineered election of several Marxists to the academy (Tolz 1997:68–87; Esakov 2000). This ideological revolution inevitably created conditions that favored ambitious young social scientists trained after 1917. Solovei (2001:108) refers to their rise to power as a "revolution from below" that supplemented the one from above. These young and often poorly educated activists translated government decrees and ideological postulates into action.

In ethnology such activists came primarily from the ranks of Society of Marxist Historians. Their leader, Valerian Aptekar', spearheaded the attack on the non-Marxist anthropologists. Because the official party line on the social sciences did not exist prior to 1932, Aptekar' and Co. took it upon themselves to act as the representatives of the party in academia. Starting in 1928, they initiated a debate on the scope and methodology of cultural anthropology. At first these debates took place at public gatherings of scholars and in periodicals. One of the main organizers of such discussions was the sociology section of the Society of Marxist Historians. A number of Moscow and Leningrad ethnologists, including Bogoras, were drawn into the work of this section. According to Solovei (1998: 144–46), Aptekar' accused Bogoras and several other "older specialists," who had begun trying to apply Marxism to their ethnological studies, of distorting the great theory by being eclectic and "mechanistically materialist." His conclusion was extremely radical — to abolish ethnology as a discipline. This position was not supported by the other participants in the debate, except for a few young members of the Society of Marxist Historians. At the same time many of the participants agreed with Aptekar' that ethnology had to be placed on a "Marxist railroad track." A number of the more radical Marxists also advocated narrowing down the scope of ethnology by turning it into a subsidiary/descriptive discipline called "ethnography" (Solovei 1998:147).

Although Aptekar' failed to win wide support at this debate, he continued his attack on the "non-Marxist ethnologists." Thus the main target of Aptekar's criticism at the First All-Union Conference of Marxist Historians, which took place in Moscow in late December 1928–early January 1929, was Bogoras's recently published book *The Spreading of Culture on the Planet*.[20] Aptekar' accused Bogoras of recycling Ratzel's ideas and dismissed his attempt to apply Marxist dialectics as an example of the incompatibility of ethnology and Marxism. A few months later at a debate on the Marxist approach to sociology, Aptekar' spoke even more passionately against Soviet ethnology as a "surrogate bourgeois social science" and attacked "the fathers of modern ethnology," that is, older scholars such as Bogoras (Solovei 2001:112).

The climax of these disputes was the infamous conference of Moscow and Leningrad ethnologists that took place in April 1929 in Leningrad.[21] Although Bogoras was elected to the presidium along with several other older ethnologists and delivered a major presentation reiterating the importance of long-term fieldwork for both ethnographic research and "cultural enlightenment work" among Russia's minorities, several younger Marxist scholars dominated the conference (Bogoras 1929b). The abovementioned Nikolai Matorin and Bogoras's own former student, Ian Al'kor (Koshkin), respectfully but firmly criticized Bogoras's feeble attempts to combine Ratzel's views with Marxist ones (Mikhailova 2004: 123).[22] Aptekar''s keynote presentation dealt with general issues of ethnological theory, reiterating his earlier arguments that ethnology did not have its own distinct subject matter and hence was not a theoretical discipline. Instead it was a "surrogate bourgeois social science" that attempted to replace Marxist sociology and history.[23] Although such a position was too radical even for the two powerful younger Marxist ethnologists Al'kor and Matorin, the conference did reject the notion that ethnology was a separate theoretical discipline and stated that from now on the main subject of ethnographic research should be the "socioeconomic formations in their concrete manifestations." The term "ethnology" was more or less banned from scholarly discourse. Except for one participant, everyone voted for this resolution. Moreover, the resolutions of the conference were considered to be mandatory for all Soviet ethnographers (Soveshchanie etnografov . . . 1929:110–14). As Solovei suggests, "It is unlikely that all of those who voted for these resolutions agreed with them, especially with the elimination of theoretical ethnology. By signing on to the new program, members of the older generation were probably hoping to insure that they could continue their professional activities, even if only within a narrower framework. In the oppressive atmosphere of the late 1920s, it would be difficult to expect a different reaction from scholars" (2001:113).

The April 1929 conference made it impossible to continue a serious debate about the subject matter and methods of ethnological research. Although formally the discussion continued throughout 1930, it had actually become simply an ideological purge aimed at driving the last nail into the coffin of "bourgeois" ethnology, whose representatives were required to "disarm" and "admit their mistakes" (Solovei 2001:113). Thus in January 1930, at a meeting of the former sociology section of the Society of Marxist Historians, which had been renamed "section of the pre-capitalist formations," Bogoras delivered a talk entitled "On the Application of Marxist Methodology to the Study of Ethnographic Phenomena." In it he distanced himself from his own earlier theoretical positions outlined in the *The Spreading of Culture on the Planet*. He also drew a sharp distinction between his own analysis and those of Fritz Gräbner and Wilhelm Schmidt.[24] Bogoras emphasized the struggle that had to be waged within ethnography between "the materialist and the idealist method" (1930a:3). He also argued that ethnography had to concentrate on the "study of the social formations associated with the early forms of production as well the survivals of the earlier modes of production." In addition ethnography, in his words, "had to explore the social superstructures, which are produced by earlier socioeconomic formations but often persist as survivals" (Solovei 1998:158). Thus the old Populist had openly signed on to the new view of ethnology as the history of primitive society. Bogoras realized that his new theoretical arguments and especially his attempts to correlate various socioeconomic and technological systems ("forms") with specific forms of the "psychological and ideological" superstructure were rather weak and proceeded to apologize for his past mistakes; he also admitted that it was not easy for him and other older ethnographers to switch to a new terminology that would correspond better to the new Marxist ideology (Solovei 1998:158).

Bogoras's apologetic tone only stimulated further vicious attacks on him and his generation of ethnologists by the younger radicals, who no longer showed much deference to their own teachers and predecessors. A year later he undertook another attempt to get on the bandwagon of Marxist ethnography by publishing an article in which he attempted to demonstrate the existence of class differentiation among the early 20th-century Chukchis. However, Bogoras's attempt to rethink his own turn-of-the-century data in light of the new politically correct methodology was not particularly convincing (Bogoras 1931). Throughout the early 1930s his status within Soviet ethnography was that of a respected scholar of the old school who was making an effort to master Marxist theory and methodology but was making only limited progress in that area (Matorin

1931). In private conversations with colleagues, Bogoras began referring to himself ironically as a *komsomolets* (a member of the Young Communist League, that is, someone who was still young and learning the basics of Soviet ideology) (Mikhailova 2004:125).

Soviet ethnography by the early 1930s was also felt at the Museum of Anthropology and Ethnography, with which Bogoras continued to be affiliated. Thus in 1930, under the leadership of its new director, Matorin, the old principle of displaying artifacts according to geographic and ethnic criteria was replaced by a socioeconomic one, which was proclaimed to be the only one "that corresponded to the principles of Marxist methodology and the rising demands of the toiling masses" (*Otchiot of deiatel'nosti Akademii Nauk SSSR* 1930:261).[25] The MAE also began presenting exhibits that spoke directly to the political events of the moment and the directives provided by the Communist Party.[26] Another major theme of the museum's exhibits and lectures was antireligious propaganda and atheism.

As part of a major antireligious campaign promoted by the government and spearheaded by the League of Militant Atheists in the late 1920s–1930s, the MAE, under Bogoras's direction, organized a major antireligious exhibit at the Winter Palace. Attended by throngs of visitors, it was so popular that in 1931 it was reorganized into the Museum of the History of Religion and Atheism. Bogoras, a lifelong atheist, served as the new institution's director until his death in 1936. The new museum was supposed to engage in research and exhibiting on the history of religion and atheism as well as "the current state of religion as it relates to the class struggle." It was also charged with antireligious education (Bogoras 1932).

Throughout the 1930s Bogoras wrote several antireligious articles, including "Religion as an Obstacle to the Building of Socialism among the Numerically-Small Peoples of the North" (1932). In this article the old ethnologist proclaimed his radical break with the old "liberal-populist" (that is, somewhat sympathetic) views on shamanism. One of his last works in this genre was "Instruction on the Organizing of the Anti-religious Work among Northern Peoples," which he dictated to Nikitina and published in 1934 (Mikhailova 2004:122).

Bogoras and Boas in the 1920s

In the aftermath of the Bolshevik coup, communication between Boas and his Russian colleagues was interrupted for almost four years, until it was finally resumed in the summer of 1921, when Jochelson managed to send Boas reprints of his articles. Responding to his letter, Boas wrote to him: "It was a great pleasure and relief for me to receive two reprints on the

Aleutian, which you sent to me this summer. I have been wishing for years to get into touch with you again and learn how you and our other Russian friends are faring. I was in Europe this summer, but could not learn anything about your whereabouts. Will you not please send me a line and let me know how you are" (APS, Boas to Jochelson, September 9, 1921). For the three Russian ethnologists, the worst thing about this long break in communication with Boas and other Western scholars was being cut off from scholarly periodicals and news about new research. To satisfy their intellectual hunger, Boas arranged the mailing of anthropological periodicals and books to the Museum of Anthropology and Ethnology. Concerned about their physical well-being as well, he also managed to procure some rather generous remuneration for all three of them for their continuing work on Siberian ethnology and linguistics.[27] Since the United States did not have diplomatic relations with Russia at that time, Boas had to arrange to have food parcels instead of money mailed to his friends in Petrograd. Expressing his and his two colleagues' gratitude to Boas, Shternberg wrote to him:

> I am not versed enough in the English language to duly express how strongly I have been touched by your sympathetic memory of me and my friends, Bogoras and Jochelson. It is not so much the material part — because after all our experience in these years it seems one can outlive sometimes without sufficient food, warmth, and clothing, but without faith in man, without sympathy of our kind, without intercourse, especially scientific intercourse it is too hard. . . . Your answer to our silent call was the more comforting and fortifying. [APS, Shternberg to Boas, June 20, 1922]

Bogoras's letter to Boas echoed Shternberg's sentiment: "We want to have books just as fishes want fresh water, some fresh air from the outer world" (APS, Bogoras to Boas, February 17, 1923).

Since the letters from the ethno-troika were rather cryptic as far as the political situation in the country and their own troubles with the authorities were concerned, Boas was able to learn the true story only in late 1922, when Jochelson arrived in New York. Officially approved as an extended business trip, this was in fact a flight from Communist Russia. Being unable to publish their scholarly works in Russia during the Civil War, Shternberg and Bogoras were anxious to take advantage of the restored communication with Boas and have their articles appear in American journals. Thus in 1925 Bogoras's major paper "Ideas of Space and Time in the Conception of Primitive Religion" appeared in the *American Anthropologist* (Bogoras 1925b).[28] While willing to help his Russian colleagues to publish their work in English, Boas was much more interested in receiving their manuscripts dealing with indigenous Siberian languages and cultures, which they had promised prepare for the Jesup Expedition publication series a long time ago. In fact he was becoming rather irritated with Shternberg and Bogoras (RAN, Jochelson to Shternberg, 282/2/124: 38–39).

According to Jochelson, in the early 1920s Boas was sympathetic to the Soviet Union. His moderate socialist views as well as the hope that things were finally improving in Russia must have played a major role in his viewpoint. In fact in 1923 he was planning to combine his German summer vacation with a side trip to Russia (RAN, Jochelson to Shternberg, 282/2/124:24). However, as Boas explained in a letter to Bogoras sent from Germany, because his stay in Europe turned out to be shorter than he had hoped and because of the length of time needed to obtain a Soviet visa, he decided not to go to Petrograd. He was nonetheless hoping to see Bogoras in Berlin in September and offered to cover the cost of such a trip (APS, Boas to Bogoras, June 13, 1923).

Although Bogoras was unable to come to Germany in 1923, in the summer of 1924 both he and Shternberg were finally permitted to travel to western Europe to attend the International Congress of Americanists in the Hague and Göteborg as well as to spend several weeks visiting museums and purchasing anthropology books and periodicals for the Academy of Sciences in Paris and London. The two of them met Boas in Berlin and then traveled together to the Netherlands. As Shternberg wrote to his wife, "Of all my Berlin impressions, the most pleasant one was my meeting with Boas: it is difficult to convey to you his warmth, the simplicity of his manners, and his noble character" (RAN, 282/2/361:175). In the Hague the three friends stayed in the same hotel and spent a great deal of time together. According to Shternberg, Boas's "socialist views" made him not just a colleague but a "like-minded person" (*edinomyshlennik*).[29] He also observed that Boas deliberately spent a lot of time with the two Soviet delegates to demonstrate his sympathy toward Soviet Russia to the other congress participants, some of whom were undoubtedly less enthusiastic about the new regime in Moscow (RAN, 282/2/361:202). The two Russian delegates attracted a lot of attention, both because of their interesting presentations and as a novelty, that is, as members of the old intelligentsia who chose to remain in Soviet Russia rather than emigrate.

While we do not know in what light the two Russians described Soviet life to Boas, I suspect that they gave the new regime a mixed review. On the one hand they must have told him about a recent purge of "bourgeois" students and professors from the institutions of higher learning (Konecny

1999). On the other hand their enthusiastic account of the great new opportunities for research and teaching that they were taking advantage of had to make a strong impression on Boas.[30] Moreover, this meeting revived Boas's pre–World War I plans to continue international cooperation in the field of circumpolar research initiated by the Jesup Expedition (cf. Krupnik 1998:206–8). His two Russian colleagues were equally enthusiastic about such cooperation. Thus the two papers presented by Bogoras at this congress had clearly been inspired by Boas's Jesup Expedition agenda and provided interesting new Siberian data for it. They also reflected the author's attempt to apply diffusionist and Kulturkreise ideas that were gaining popularity among anthropologists in general and Americanists in particular (Bogoras 1924a, 1925a, 1926b:129). Because part of the 1924 Americanists' congress took place in Sweden, Boas and the two Russians were able to discuss this research with several leading Scandinavian ethnologists who were equally enthusiastic about it.

Bogoras had a great time in Berlin and Paris, visiting his brother, who had immigrated to France after the Bolshevik coup, and meeting old and new friends and colleagues. Despite his own pro-Soviet position, he could not resist seeing his old comrades from the People's Will and the PSR, some of whom continued their anti-Soviet activities. Like Shternberg, he also voraciously read scholarly literature and visited museums. As a result the two of them reestablished ties with a number of prominent senior anthropologists and established new ties with younger ones.

Determined to restore scholarly cooperation between the Western and the Russian circumpolar researchers, Boas, who spent part of 1925 lecturing in Norway, recommended that the Department of Arctic Ethnology of the Norwegian Institute for History of Civilization recruit Bogoras to take part in a large-scale ethnological project in the Arctic that this institution was planning (APS, Boas to Bogoras, November 13, 1925; Boas to Refsdal, December 4, 1925). In the aftermath of his 1924 meeting with the Russians, Boas became even more active in arranging for academic books and periodicals to be mailed to the Russian Academy of Sciences. He also tried to obtain some funding from Jewish organizations to help cover the cost of Bogoras's research as well as to aid the Institute of Higher Jewish Studies, which Shternberg was heavily involved with (APS, Bogoras to Bogen, December 24, 1924).

Two years later Boas and Bogoras saw each other again in Rome at the 22nd International Congress of Americanists. This time Bogoras also became acquainted with several younger American ethnologists and linguists, such as A. Irving Hallowell, Gladys Reichard, Frank Speck, and Leonard Bloomfield. Of the two papers he delivered in Rome, one was very "Jesupian," while the other reflected his continuing interest in religion and its evolution (Bogoras 1928b, 1928c). At that meeting Bogoras was elected to be one of the vice presidents of the society.

While eager to cooperate with Boas and other foreign Americanists, Bogoras did not want to see a repetition of the Jesup Expedition, in which the Russian ethnographers collected data for their American employer. Only three years earlier, desperate for foreign funding, he had still been willing to accept this arrangement, but by 1926 the Soviet government's financial support of ethnographic research had increased, though its control over foreign ventures on its territory had grown as well.

In his memos to the government and the Academy of Sciences officials, Bogoras argued strongly for an allocation of substantial funds for ethnographic and archaeological research on the Russian side of the Bering Strait as the way to insure the USSR's leading role in this field. Among the recent large-scale foreign Arctic ventures that he saw as a model for the Soviet ones was the famous Fifth Thule Expedition organized by the Danes, in which Knud Rasmussen and his team had explored the Inuit cultures of Greenland, Arctic Canada, and northern Alaska. In 1924 Rasmussen had even tried to land near the Asiatic Yupik settlement of Naukan. However, the American captain of the boat the Danish explorer was traveling on was afraid to land on the Russian shore because of a recent confrontation between the Soviet border patrol and American whalers who had allegedly tried to take over Wrangell Island (Bogoras 1926b: 127).[31] Thus appealing to Soviet pride and increasing suspiciousness toward foreigners, Bogoras tried to secure funding for research in eastern Siberia and a role for himself as the organizer and head of that research as well as its main spokesman at international academic meetings (250/3/123:1). Attending those meetings was very important for him both as a way of keeping in touch with his Western colleagues and as a diversion from his busy and stressful professional life.[32]

The next time Boas and Bogoras saw each other was at the 23rd ICA in New York in mid-September 1928.[33] In addition to meeting his old friend and several of his other American and European colleagues, Bogoras had a chance to meet and establish contact with a number of American anthropologists. Among them were Ruth Benedict, Melville Herskowitz, A. L. Kroeber, Speck, Leslie White and others. Bogoras was one of the only two delegates from Russia, and he was clearly the center of attention. He was elected to be one of the four secretaries of the ICA and delivered *six* papers that reflected his various old and new interests and activities (Bogoras 1930a, 1930b; Bogoras and Leonov 1930). In addition he presented several papers written by his and Shternberg's students. His recently developed interest in ethnogeography and diffusionism was reflected in several of these papers as well as in an article he published in *American Anthropologist* in 1929 (Bogoras 1929a). Based on a lecture he delivered while in the United States entitled "Elements of Culture of the Circumpolar Zone," it fit in very well with the American studies of culture element distribution. In addition to spending time with fellow anthropologists, Bogoras met a number of liberal and leftist intellectuals who sympathized with the Soviet Union, including Theodore Dreiser. Boas clearly belonged to that group. In fact he served on the advisory board of a recently organized American Society for Cultural Relations with Russia (USSR).

At the conclusion of the congress, a special meeting of leading scholars of circumpolar ethnology, archaeology, and physical anthropology took place at the American Museum of Natural History. Besides Bogoras, it was attended by two Americans (Clark Wissler and Ales Hrdlicka), one Canadian (Diamond Jenness), and four Scandinavians (William Thalbitzer, Erland Nordenskiold, Kaj Birket-Smith, and Therkel Mathiassen).[34] Whether out of patriotic sentiment or the fear of being accused of pandering to foreigners, Bogoras strongly objected to any proposals made by them to fund and conduct research on Soviet territory. In the end it was agreed that scholars would conduct research in their own countries but could participate in foreign expeditions as invited guests. Another disagreement between Bogoras and his foreign colleagues occurred during the discussion of the preservation of the languages and customs of numerically small peoples of Siberia. While the Westerners bemoaned the impending disappearance of these peoples as distinct cultural and linguistic entities, Bogoras argued that, despite a strong scholarly interest in them, the Soviet Union had no plans to artificially preserve their traditional ways of life, while encouraging the use of indigenous languages and even creating literacy in them (Bogoras 1929b). Thus the old Populist was willing to reject his earlier views on the need to protect indigenous cultures of Siberia from Russification in favor of the more recent and politically correct view. According to Benedict, in his conversations with ICA's participants Bogoras also echoed official Soviet views on other issues, such as literature (Mead 1959:307–8).[35] It is difficult to ascertain how genuine his pro-Soviet stand was, although I suspect that in 1928 he was no longer willing to criticize the regime's policies in a public foreign setting. It is quite possible that he was more open in his private conversations with Boas, whom he fully trusted.

At the end of the congress, Bogoras was able to obtain an extension to his American visa and spent the next two months or so in New York. During that time he saw Boas regularly. In addition to discussing a large-scale cooperation between Russian and Western ethnologists working in the Arctic, the two of them developed a plan for an exchange of anthropology students between the United States and the USSR. On November 19, 1928, identical letters were sent to the Soviet Academy of Sciences, Leningrad University, and the Committee of the North as well as to several American educational and research foundations including the SSRC (Social Science Research Council) and the Guggenheim Foundation. The letters, which were cosigned by Boas, Bogoras, and Stephen P. Duggan, the head of the Institute of International Education (e.g., APS, Boas to Guggenheim Foundation, November 19, 1928), advocated the establishment of fellowships allowing "Russian students to study problems of American ethnology . . . and for participation of American students in the study of Siberian and Arctic European problems. On the American side it would be possible to give the Russian students the opportunity to participate in field work and the same should be done for American students in the Siberian and Arctic European fields" (RAN, Boas to Bogoras, 250/4/35:49–50). In a letter to Bogoras mailed on November 24, 1928, Boas, who must have masterminded the November 19 letters, explained in more detail what he had in mind. Since this document is very interesting and is only available in the Bogoras collection of the Russian Academy of Sciences Archive, I will quote it almost in its entirety. Boas wished for this fellowship to allow American students to spend long enough time in Russia

> to master completely Russian literature and to become familiar with the problems of [the] Siberian field which are absolutely essential for a clear understanding of our North American field. The same holds true for Russian ethnologists and I should like to see a few Russian ethnologists in this country. To begin with I want to try to obtain at least a Fellowship for one young Russian ethnologist. My plan would be that he should stay here at an eastern University long enough to become familiar with our methods and I should want then to send him to Alaska for the purpose of making a thorough study of the Tlingit, which has never been made. This seems particularly necessary because the large and valuable [Tlingit] collection of the Museum of the Academy of Sciences in Leningrad should be worked over thoroughly in the field. And at the same time whoever studies the Leningrad collection ought to be familiar with the New York collection, probably the largest collection from Alaska in existence. The person selected ought to have a very good training in linguistics and be able to record texts accurately. It is particularly necessary that he should be familiar with the study of musical tone, which is a most important element in the language of the Tlingit.

> I can assure you even now, that the money for an expedition to Alaska, probably between $1200 and $1500, will be available. I cannot promise definitely a Fellowship, but I shall try my best to secure one. I hope that you will make every effort to see this plan consummated and to select a young Russian scientist, man or woman, who would be competent to undertake the proposed study.
>
> If this plan is continued I should want to see Russian students taking up the study of the tribes of the interior of Alaska and British Columbia, an inquiry which is very much needed and which also will help to throw much light on the Siberian problems just as Siberian problems will help us understand American problems. A solution of your and our problems is possible only through continued cooperation. [RAN, Boas to Bogoras, 250/4/35:49–50]

Boas kept his word, and when Bogoras returned from New York to Leningrad in early 1929, he brought with him invitations from the United States for five Soviet students. One of them was from Barnard College for a female anthropology graduate student (Nitoburg 2003:401). Considering the fact that there were still no diplomatic relations between the two countries, this was quite a coup. Bogoras, who had several candidates in mind, chose Iuliia Averkieva (1907–80), a young woman from a provincial working-class family. She was a hardworking student who had completed her university education at the Ethnography Division between 1925 and 1929. It is very possible that her working-class background, pro-Soviet views, and membership in the Young Communist League influenced the university authorities' choice (APS, Bogoras to Boas, September 27, 1929).[36] After a year of studying anthropology with Benedict, Reichard, and Boas himself, she was invited by her mentor to join him for a stint of ethnographic fieldwork at Fort Rupert.[37] Even though Averkieva was not exactly the kind of ideal student Boas had described in the above letter to Bogoras, her four-month-long field research was quite successful (Averkieva and Sherman 1992).

Boas and his young Russian student became quite fond of each other. Like other female graduate students of his, Iuliia called him "Papa Franz" and corresponded with him regularly between 1931, when she returned to Russia, and 1937.[38] She was the first young Soviet anthropologist with whom Boas interacted for a long period of time. Conversations and correspondence with Averkieva revealed to him the mind-set of the new generation of Soviet youth. As Boas wrote to Bogoras in April 1931, "she is still a devoted adherent of your new political system" (APS, Boas to Bogoras, April 24, 1931). Several passages from their correspondence reveal strong political disagreements between the young Communist and the old democratic socialist. Thus in her October 9, 1933, letter, Averkieva expressed a strong disagreement with Boas's argument that the Nazi persecution of the opposition was similar to the Soviet persecution that was taking place in the early 1930s (APS). Upon her return home, Averkieva quickly absorbed the new party line, which had come to dominate Soviet ethnography since the 1929 conference and especially the first congress of Soviet archaeologists and ethnographers that took place in 1932. The resolutions adopted by the congress significantly narrowed down the scope of Soviet ethnography. From now on it was defined as an auxiliary discipline that was to help historians understand the preclass societies (Solovei 1998:172–73). Averkieva's first publication was a fairly simplistic summary of the latest anthropological theories advocated by such leading American anthropologists as Boas, Kroeber, and Robert Lowie. However, it also contained the kind of criticism of "reactionary bourgeois" anthropology that became standard in Soviet works in the early 1930s. She accused American anthropologists of antievolutionism and empiricism and criticized diffusionism and the theory of culture-elements distribution. In the words of the young Communist scholar, "American ethnographers serve capitalists, both consciously and unconsciously." The only ones among them who received Averkieva's praise were the young Morganists Bernard Stern and Leslie White. At the same time she was mild in her criticism of Boas himself and praised him for speaking out against the infamous trial of the "Scottsboro boys." Averkieva's 1935 dissertation on slavery among the Kwakiutl was a typical example of a dogmatic Marxist interpretation of ethnographic data (Averkieva 1935, 1941), which must have disappointed her American mentor.[39]

Despite Boas and Bogoras's efforts to arrange for additional Soviet students of anthropology to study in the United States, the changing political climate in the USSR made this impossible. The American side was more fortunate—in the early to mid-1930s several young anthropologists from the United States were able to study in Leningrad. Among them were Emanuel Gonick, a student of Kroeber and Lowie from the University of California; Eugene Golomshtok, a Russian émigré who had earlier studied archaeology with Bruno Adler in Russia, received his MA from the University of California–Berkeley, in 1923, and worked under Speck at the University of Pennsylvania Museum in the late 1920s; and Archie Phinney, a Nez Perce anthropologist and linguist trained by Truman Michelson at George Washington University. Bogoras advised all of them and particularly Phinney, whose work on Nez Perce folklore he officially supervised (see Willard n.d.; RAN, Golomshtock to Bogoras, 250/4/78; Gonick to Bogoras, 250/4/80; Lowie to Bogoras, 250/4/196). These Americans were able to travel throughout the USSR but not conduct any ethnographic or archaeological field research there. The only Western ethnographer who was able to conduct extensive fieldwork in the Soviet Union was a German, Hans Findeisen, who studied the Kets, an indigenous Siberian people, in 1927–28, when the political climate was different and Russia and Germany were on friendly terms (Findeisen 1929).[40] Nonetheless, throughout the late 1920s to mid-1930s a significant number of American anthropologists and other scholars who came to Leningrad bore letters of introduction from Boas and the Boasians to Bogoras. Thus he was clearly the main, if not the only, contact person that they had.[41]

The 1930s: Friendship under Threat

After their lengthy encounter in New York in 1928, Boas and Bogoras saw each other only once more. It was a brief visit in Berlin in the summer of 1930. Both of them were hoping that Bogoras would be able to attend the upcoming 1930 International Congress of Americanists in Germany, but this did not happen. An increased ideological pressure on Bogoras and his brand of ethnology from the Marxist camp was undoubtedly responsible for his failure to obtain permission to go to that scholarly meeting. Boas, who appears to have requested that the Academy of Sciences allow Bogoras to attend the congress, understood why no such permission had been granted; in a 1931 letter to his friend he wrote: "I do hope that the conditions in your institution may develop more satisfactorily than they seem to be at the present time. I do not think it is right to give way to people of less experience" (APS, Boas to Bogoras, January 28, 1931).

The Russian ethnographer understood very well why he was blocked from going to the Berlin congress and became depressed. As he put it in a letter to Boas dated November 5, 1930, "without the Americanists' congress I really do not know whether in the future I will have much chance to go abroad and to see you or any other of my American and French friends" (APS). Another letter sent to Boas in November 1930 shows that Bogoras found himself at a crossroad: on the one hand he speaks of considering joining the Communist Party, but on the other he hints at the possibility of his permanent departure from the USSR (APS). He also apologizes for having to write in generalities; in other words he is hinting at the possibility that his correspondence with foreigners was being read by the authorities (RAN, Bogoras to Boas, November 16, 1930). Bogoras's next letter to Boas is more specific in its description of his current professional (that is, political) difficulties. In his words:

> My position is gradually changing. On the whole I am [going] through a period of eclipse in the same way as it had happened to you from the part of the American Museum of Natural History. My younger friends strive to take all the things into their own hands. Of course, the quality of the work in all the institutions, which were founded by Shternberg and myself is going down. . . . To be fair I must say that I don't think this will go on very long. The younger friends aforesaid are too ardent and they will learn quickly [illegible]. [RAN, Bogoras to Boas, November 26, 1930]

The letter ends with Bogoras predicting that next year he might have to leave one or several of the institutions he was still working for. Despite his pessimism, Bogoras was hoping to continue his foreign travels. Thus in late 1931 he wrote to Boas about an invitation he had recently received from a Canadian anthropologist, Diamond Jenness, to attend the next Pacific congress to be held in that country (APS, Bogoras to Boas, October 16, 1931). However, the authorities had decided otherwise — Bogoras was not to travel abroad from now on.[42]

In 1930–1933 Soviet ethnography underwent a fundamental structural reorganization. A new research institution was established in Leningrad in 1930 under the name Institute for the Study of the Peoples of the USSR (IPIN). It became the leading institution that coordinated ethnographic research on Soviet territory. Among the new institute's main tasks were: a critical examination of the current ethnographic literature; research on the survival of religious practices among various nationalities of the country; and the study of human beings as a force of production. The most ardent Marxists took over the "brigade" charged with critiquing "bourgeois influences" on Soviet ethnography, one of which was "Populist tendencies."[43] In 1932 a major All-Union Conference of archaeologists and ethnographers further redefined the direction of research at the new institute. It was charged with concentrating mainly on the noncapitalist development of the "backward" peoples of the USSR and the "construction of their new culture" as well as on "unmasking the anti-Marxist and anti-Leninist trends in the pre-revolutionary Russia and contemporary western ethnology" (Solovei 1998:196–97).

Hence from 1932 on the "idealist" views of Shternberg, Bogoras, and their followers began to be subjected to rather harsh criticism. In 1933 IPIN was combined with the Museum of Anthropology and Ethnography and given the name Institute of Anthropology and Ethnography of the Academy of Sciences of the USSR. In addition to the previously mentioned research tasks, it was now charged with the study of precapitalist socioeconomic formations and the problem of primitive communism as well as the ways of overcoming precapitalist and capitalist survivals in the culture and society of certain peoples of the Soviet Union. Appointed the head of this new institute, Matorin zealously proceeded to implement the charge given to it by the party and the government. As Solovei (1998:219) points out, as a result of this reorganization, the scope of ethnological research in the USSR was significantly narrowed down. Moreover, by the early 1930s the views of Marx and Engels on precapitalist societies, based in large part on Morgan's evolutionist theory, became the dogma that would dominate Soviet anthropology for decades.

With the atmosphere at the Institute of Anthropology becoming more and more stifling and the Ethnography Division at the university being shut down, Bogoras was forced to concentrate on his other jobs at the Museum of the History of Religion and Atheism, which he headed, and the Institute of the Peoples of the North, which brought students from Siberian minority nationalities to Leningrad. Much of his time was spent working on linguistic materials, including creating alphabets and composing texts in the indigenous Siberian languages. In his 1932–33 letters, Bogoras complained to Boas that he was overworked, tired, and underpaid.

At the same time he was trying hard to keep up with the changing ideological winds blowing in Soviet anthropology, which he referred to as "our incessantly seething cauldron" (APS, Bogoras to Boas, March 9, 1933). As I have mentioned, his efforts to rethink his own Chukchi and Siberian Yupik ethnography in light of Marxism were not very successful from his own point of view or that of the new ideologues. Referring to his 1931 article on "class differentiation" among the Chukchis, he wrote to Boas, "I have worked on this Chukchee paper for [the] past two years and still I cannot say that it pleases me altogether" (APS, Bogoras to Boas, August 12, 1932).

At the same time Bogoras was determined to keep his intellectual ties with Boas and continue to promote Boasian anthropology in the USSR. Thus he encouraged Al'kor (Koshkin), his former student and now the head of research at the Institute of the Peoples of the North, to publish a Russian translation of an updated version of Boas's seminal *Introduction to the Handbook of American Indian Languages* (APS, Koshkin to Boas, January 20, 1933).[44] And he also arranged to have Boas's essay "The Aims of Anthropological research" published in *Sovetskaia Etnografiia* (Boas 1933).[45] As Koshkin wrote to Boas on March 9, 1933: "I have read with great attention your address to the AAAS in 1932 and indeed I have translated it into Russian and I am going to publish it in the magazine *Sovietic Ethnographia* [*sic*] with some commentaries. There are very interesting coincidences between your ideas and those that are being worked out here out of the complex of old ethnographic material and new ideas more or less Marxist" (APS).

Reading Bogoras's critical comments on Boas's 1932 paper, it is difficult to see what these "coincidences" were. Maybe he was simply trying to placate Boas, sine he knew that the latter would not be very pleased with Bogoras's comments. Boas undoubtedly knew about the content of the Soviet publication of his paper. In the 1930s one of his sons-in-law, who was born and educated in Russia before coming to the United States, translated Soviet publications for him. Was Boas offended or at least disturbed by Bogoras's critique? I imagine that he was, even though he never mentioned this subject in his few remaining letters to his Russian friend. This may be one of the reasons that after 1931 his correspondence with Bogoras becomes very sporadic. In fact Boas sent him only a couple of letters in 1932 and one letter in 1933; after that he seems to have remained silent until early 1936, when he wrote him a rather formal business letter dealing exclusively with some archaeological materials he had obtained for the MAE in Mexico prior to the Mexican Revolution (APS, Boas to Bogoras, January 27, 1936). At the same time Boas probably understood why Bogoras had to compromise himself the way he did. After all, Boas's few comments about Soviet anthropology, found in his correspondence of the mid- to late 1930s, make it clear that he did not approve of its having become so dogmatically Morganist and Marxist.[46] Neither was he very pleased with the Soviet politics of the 1930s, even though he continued to sympathize with the ideals of Soviet socialism and saw Nazism as a much greater threat to humanity.[47]

Epilogue

In this paper I have detailed how, in the late 1920s–early 1930s, Bogoras tried hard to maintain close ties with Boas and more broadly between American and Soviet anthropology.[48] As the latter became increasingly politicized and dogmatic, the price of maintaining these ties increased. While I fully agree with Krupnik's (1998:208) characterization of Bogoras's comments on "The Aims of Anthropological Research" as being "surprisingly arrogant and politically motivated," I hope the reader would understand why he felt he had to use the harsh new Soviet rhetoric in describing his friend's views.[49]

We can only speculate about Bogoras's view of his own public conduct in the last few years of his life. Yet we do have some evidence that, like many other members of the Soviet intelligentsia, he learned to engage in doublespeak, publicly proclaiming his allegiance to the regime and his agreement with the views and actions of his Marxist colleagues while comparing Communism to Fascism in private conversations (Tishkov 1993). Given an increasingly stifling atmosphere in the country in the mid-1930s and the arrests of a number of his relatives, students, and colleagues, it is hard to imagine that he remained an optimist. He did not join the party, but we do not know if it was his own decision or he was turned down. He did, however, continue to try to rethink his old ethnographic data by using Marxist-Morganist theory (Bogoras 1934, 1936). As he wrote in his new introduction to the Russian translation of part I of his Chukchi monograph, published a quarter of a century earlier as part of the Jesup Expedition series, "at that time [that is, in the 1900s], I was closer to Franz Boas, who continues to maintain the same exaggeratedly cautious and skeptical position [as I did]. . . . At the present time, I have moved away from this skepticism and have mastered (slowly) the basics of a Marxist worldview, which I have tried to apply in my work in the last five years" (Bogoras 1934:xv).

One could say that, unlike many Soviet ethnographers, Bogoras was fortunate to escape arrest and die in his own bed (or, to be precise, while traveling by train from Leningrad to Rostov-on-Don, where he hoped to have his blocked arteries operated on by his brother, a prominent surgeon). His reputation in Soviet ethnology as the leading senior specialist on the cultures and languages of indigenous Siberians remained high. At the same time, even an article written on the occasion of his seventieth birthday by his student and colleague, Al'kor, characterized him as a member of the Russian populist school of "subjective sociology" who had always been eclectic, did not understand the phenomenon of socioeconomic formations, and had been under the strong influence of the American school of historicist and antievolutionist anthropology founded by Boas (Al'kor 1935:9; cf. Zelenin 1937). This mixed evaluation of Bogoras's scholarly legacy was expressed even in some of the speeches delivered by his colleagues at his very elaborate state funeral in Leningrad in May 1936 as well as in several of his Soviet obituaries (e.g., *Sovetskaia Etnografiia* 1936[3]:3–4).

Stalinist persecutions of the 1930–1940s spared neither Bogoras's students nor his Marxist critics.[50] In fact the latter tended to suffer a harsher punishment. Thus Matorin was executed in 1936, Aptekar' in 1937, and Al'kor in 1938.

Boas found out about the old Populist's death from a telegram sent to him by one of Bogoras's students. In his obituary of Bogoras, Boas paid great tribute to his old friend, albeit this was touched by some gentle criticism:

His work on the Chukchee . . . is proof of his deep insight of the people among whom he was compelled to live. The clarity of his description is due to scientific insight; but no less to his artistic gifts. His work as a novelist . . . is also characterized by remarkable powers of observation and psychological analysis. . . .

During the last years of his life, his interest was centered in what he liked to call the grand generalization of anthropology in which he liked to give freer reign to his imagination than he could do in a narrower field of faithful presentation and careful analysis of observed facts. I think it was the artist rather than the scientist who spoke when he dwelled on these problems. He was filled with these ideas when we saw him here last as Delegate of the Academy of Science in Leningrad at the Congress of Americanists held in 1928 in New York. Those who knew him personally could not help admiring his knowledge as well as his enthusiasm; those nearer to him, like the writer of these lines, valued his staunch friendship, and feel keenly the loss they sustained in his death. [Boas 1937]

Notes

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1. On Matorin see Reshetov (1991, 1994, 2003).

2. See the references for the full titles of the archival collections cited in this paper.

3. Krupnik (1996), Kan (2000, 2001), Cole (2001), Vakhtin (2001), Krupnik and Vakhtin (2003).

4. All of the Russian-language publications about Bogoras's life and his literary and scholarly work appeared prior to 1991 and hence bear the stamp of the Soviet-era ideology. The only exception is a recent article by Mikhalova (2004). The main English-language works on Bogoras and his scholarly legacy are by Krupnik (1996, 1998). See also Krupnik (2001) for the most complete English-language bibliography of Bogoras's publications.

5. The two of them spent most of the summer of 1903 at Boas's country home on Lake George.

6. "Ethno-troika" or "ethno-trio" was the term coined by Bogoras to describe himself, Jochelson, and Shternberg (Bogoras 1934:xiii).

7. By 1905 Boas had recruited Shternberg to work on the ethnography of the Amur River and Sakhalin Natives (Kan 2000, 2001).

8. In the immediate aftermath of the coup, Bogoras published some anti-Bolshevik articles.

9. Some of the intellectuals who changed their attitude toward the Communists at that time were driven by patriotism and simply chose not to focus on the remaining unpleasant aspects of Soviet political life, instead concentrating on their own work, which they saw as their contribution to the well-being of Russia regardless of whether it was a Soviet Russia or

not. Others justified their cooperation with the Bolsheviks by developing a new ideology, which came to be known as "Smena Vekh" ("Changing of the Signposts"). The *smenovekhovtsy* argued that NEP (New Economic Policy) was not just a Bolshevik tactic, but also a sign of a true evolution of the Soviet regime toward a more democratic and free-market type of society. In addition many of the *smenovekhovtsy* were strong Russian patriots and even nationalists, who saw the Communists as the builders of a powerful Russian state. Bogoras, who was one of the Russia-based leaders of this group, announced in 1921 that he was now "betting on the Bolshevik horse" and joined the editorial staff of *Novaia Rossiia*, a Petrograd journal of the Smena Vekh movement (Hardeman 1994:47–48).

10. Throughout this paper I often use the term *ethnography* to refer to ethnology or cultural anthropology, since this was the terminology used in the USSR between the early 1930s and the early 1990s.

11. The new Geography Faculty consisted of two divisions: Geography and Ethnography.

12. In addition some students studied ethnology at the Faculty of the Social Sciences of Leningrad University (Solovei 1998:123–24).

13. Bogoras was hoping that his old friend and colleague, Jochelson, would return to Russia and take Shternberg's place. In fact Jochelson had been seriously entertaining such a plan for several years and with Bogoras's help was able to secure a commitment from the Academy of Sciences to give him a research position at the MAE. However, sensing that the atmosphere in Soviet Russia was beginning to change, the old ethnographer bailed out at the last minute, blaming his poor health. Bogoras was both irritated and greatly disappointed (APS, Bogoras to Boas, October 13, 1927).

14. It is quite conceivable that there were other reasons for Bogoras's not having been elected to the Academy: after all, not all of his published work was of the highest quality, and much of it had been published only in English; moreover, the slot for an expert on indigenous Siberian languages had been filled by Shternberg, while no slot for an ethnologist had yet been created. In addition, as Igor Krupnik pointed out to me (personal communication August 2005), members of the academy must have resented the fact that Bogoras had been nominated for the Academy by the Committee of the North (see note 16).

15. Bogoras claimed that while he was too old to care much for any "new honors," the cause of building up ethnography in Russia that he had dedicated himself to would "proceed with much less friction and difficulty" if he would get elected to the Academy (APS, Bogoras to Boas, May 17, 1927).

16. On a list of candidates approved by the Communist Party's Central Committee, Bogoras's name appears under the rubric "a candidate we do not object to." The other two categories were party members and "candidates close to us [i.e., the party]." Some time before the election, Bogoras's candidacy was removed along with that of several others. Thus the academicians did not have to vote for or against him. It is not clear why the Party eliminated his name, but we could speculate that it either did not fully trust him or was afraid he would be voted down (Esakov 2000:53–54).

17. This intriguing interpretation of Bogoras's debacle was suggested to me in 2000 by my late Russian colleague and friend, Mikhail Fainshtein.

18. See Vakhtin (1994:39–42) for a cogent discussion of the debate within the Committee of the North between Bogoras and his camp (which Vakhtin calls "conservatives") and their opponents (whom he refers to as "radicals").

19. Thus in a manuscript written before 1927, Bogoras rejected the notion that there could be a distinct Marxist ethnology (RAN, 282/1/175:3).

20. This work was based on a lecture course taught by Bogoras at Leningrad University in the late 1920s.

21. This conference is discussed by Slezkine (1991) and Solovei (1998, 2001).

22. As a matter of fact, as early as the spring of 1928 Matorin, who (unlike Aptekar') was genuinely interested in empirical ethnographic research, began pressuring Bogoras to include his own proposed new courses, "Ethnography and Marxism" and "Ethnography and the Soviet State-Building," in the curriculum of the Ethnography Division of the Geography Faculty of Leningrad University (RAN, 250/5/123).

23. Aptekar'"s radical view was echoed by a declaration issued later that year by a small but aggressive group of Marxist students of the Ethnography Division of the Geography Faculty of Leningrad University. Entitled "Our Platform," it proclaimed that ethnography/ethnology "had been slain by Marxism" (RAN, 250/3/178).

24. In the late 1920s and especially the 1930s, after Hitler's seizure of power, Wilhelm Schmidt and his school became the number one enemy of the Soviet ethnographers. The fact that he was a devout Catholic and a German made him an easy target. Bogoras himself wrote about Schmidt in his commentary on Boas, "The Aims of Anthropological Method": "The Catholic school of contemporary ethnography, headed by cardinal Schmidt, represents an active reactionary force and leads the attack against all of the scientific accomplishments of the recent past, just as German Fascism leads the attack against the most elementary conditions of social life" (Bogoras 1933:193).

25. Matorin was the first MAE director who was not a member of the Academy of Sciences. In fact he had not even completed his university education. However, his Communist Party membership and his close ties with Leningrad's party boss, Grigorii Zinov'ev, must have helped make his spectacular career (Reshetov 2003).

26. Thus two of its 1932 exhibits bore the following titles: "Japanese Imperialism and the Annexation of China" and "The Current Status of Negroes in the USA" (*Otchiot of deiatel'nosti Akademii Nauk SSSR* 1932:205). In 1933 Bogoras produced an exhibit on Chukchi society that reflected the new ideological demands.

27. In response to Boas's request, the head of the American Museum of Natural History agreed to pay the Russians $300 during each of the remaining months of 1921.

28. The article was based on his monograph *Einstein and Religion*, published in Russia two years earlier (Bogoras 1923).

29. Shared socialist ideas also contributed to the particularly warm relationships that developed between the two Russian ethnologists and the two French ones, Paul Rivet and Marcel Mauss (RAN, Shternberg to Sarra Shternberg, 282/2/361:199–200). See also Mauss's correspondence with Shternberg and Bogoras (Mauss archive at the College de France).

30. Their enthusiasm must have been further strengthened by the fact that Shternberg was about to be elected to the Russian Academy of Sciences.

31. According to Igor Krupnik (personal communication August 2005), the true story of Rasmussen's landing in Siberia (one well known to Bogoras) was rather different. He did land near East Cape with the intention of doing some fieldwork in Naukan, but he was detained by the Russian border guards because of his lack of a proper "Soviet visa." He was taken to the main Soviet headquarters in Uelen and was officially expelled the next day on one of the American trading boats going back to Nome. Rasmussen described the incident vividly in his book *Across Arctic America* (1927:357–79).

32. Thus in December 1926 Bogoras petitioned the Academy of Science to authorize and fund his business trip to Berlin and Paris for the purpose of taking part in the meetings of European Americanists that was planned as preparation for the 1928 Congress of Americanists to be held in New York (RAN, 2–1(1975)/15:91–92). However, for some reason he did not make that trip.

33. In the wake of Shternberg's death, Bogoras became melancholy and resigned to his own impending death. As he wrote to Boas in the fall of 1927 while waiting for a visa to go to the United States, "I want to see you once more and other friends before I will go the other way, farther than even the Atlantic Ocean" (APS, Bogoras to Boas, November 27, 1927).

34. Bogoras's report on this meeting does not mention Boas among its participants (Bogoras 1929:103).

35. In a letter to Mead dated September 21, 1928, Benedict described her impressions of Bogoras, whom she had spoken to at the ICA meetings in New York: "I've had good talks with Bogoras. He's full of the 'new dawn' and sure as a child" (Mead 1959:307).

36. By this time, all Soviet students and scholars traveling abroad had to go through a stringent approval process.

37. Since she had no Canadian visa, Boas pretended she was his granddaughter (Nitoburg 2003:402).

38. According to Willard (n.d.:18) Averkieva managed to circumvent Soviet censorship by passing her letters to "Papa Franz" via American students from Columbia who visited Leningrad.

39. Ironically, Bogoras's written evaluation of Averkieva's dissertation criticized it from a typical Boasian perspective (RAN, 252/1/153). According to Krupnik, throughout the 1960s–1970s, Averkieva kept a photograph of Boas in her apartment (but not in her office) and reportedly would point to it and say: "What a wonderful person he was! Unfortunately, I had to criticize him all my life" (Krupnik n.d.:10). Of course, before judging Averkieva, one must keep in mind that she spent seven years in the Gulag from late 1947 to the early 1950s and that she did publish a glowing obituary of Boas in 1946 (Averkieva 1946).

40. As far I know, the only other American ethnographers who managed to undertake field research in the Soviet Union were Alfred E. Hudson and Elizabeth Bacon, graduate students of Edward Sapir and Clark Wissler at Yale. However, their 1934 work in Central Asia lasted only for a few weeks (Bacon 1966).

41. While in the 1930s the aging Boas was no longer actively working on Soviet-American scholarly cooperation, the Scandinavians, and particularly Birket-Smith, continued to plan a large-scale international project for circumpolar research. Thus in a 1934 letter to Bogoras he informed his Russian colleague that at the First International Congress of Anthropological and Ethnological Sciences, held in London in 1934, a committee of distinguished international scholars was formed to undertake "an international investigation of the Polar tribes of America and the Old World" (RAN, Birket-Smith to Bogoras, May 5, 1936:10–16). Besides Birket-Smith himself, it included Boas, Jenness, Thalbitzer, and several others. Bogoras, who did not attend the congress, was elected representative of the USSR on the committee. Unfortunately, Birket-Smith took two years to inform Bogoras of this election. By the time his letter reached Leningrad, Bogoras was already dead. His passing, and especially a rapidly increasing isolation of the USSR, made Soviet participation in this venture impossible. The only successful outcome of the project was Alexander Forshtein's six-month fellowship with Thalbitzer and Birket-Smith at the National Museum in Copenhagen. Forshtein, a favorite student of Bogoras and a specialist on Siberian Yupik languages, understood that in 1936 a Soviet scholar returning from a long business trip in the West was very likely to be arrested. Hence his desire to study under Boas for "a year or two," expressed in his letter to the American friend of his recently deceased mentor, must have been motivated not only by scholarly interests. In his response Boas offered no concrete assistance to the Russian scholar, blaming the lack of funds. Forced to return to the USSR, Forshtein was indeed arrested in 1937 and spent 20 years in labor camps (Krupnik 1998:213–14).

42. As late as the fall of 1933, Bogoras was still hoping to be able to travel abroad — this time to the 1934 International Congress of Americanists in Spain.

43. In the 1930s populism came under increasing criticism not only as a philosophical and sociological school of thought but as a political movement. In 1935 the Society of the Former Political Exiles, which had been dominated by populists and which Shternberg and Bogoras had been active in, was closed down.

44. Boas agreed to Koshkin's proposal but wanted to update the 1911 paper. However, he never completed that project, and hence the Russian version of his paper was never published (APS, Boas to Koshkin, February 10, 1933).

45. Two years later, another paper by Boas was published in the same Soviet anthropology journal thanks to Bogoras's efforts. This time it was his "Witchcraft among the Kwakiutl Indians" (Boas 1935). Since the issue it was published in was Bogoras's Festschrift, the publication of his American friend's article was of special importance to him.

46. As he wrote to Walter Rautenstrauch, a Columbia University professor, in 1939, "Soviet anthropology must be Marxian and Lewis Morgan, otherwise it is not allowed" (Stocking 1992:109).

47. Nonetheless, as his letters to Averkieva and Bogoras indicate, he did see similarities between the brutal repression of dissent practiced by Hitler and by Stalin (RAN, Boas to Bogoras, July 16, 1933; 252/4/35:72).

48. According to Krupnik (1998:208), thanks to Bogoras's efforts, throughout the 1930s several American museums continued to exchange ethnographic specimens with the Leningrad Museum of Anthropology and Ethnography.

49. Of course one could say that Bogoras did not have to publish his translation of Boas's 1932 paper at all.

50. Upon his own request Bogoras was placed in a coffin draped in red to signify his old revolutionary activities and was buried at the prestigious Volkovo Cemetery next to prominent pre-Bolshevik revolutionaries Georgii Plekhanov and Vera Zasulich.

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3. *Taking Ethnological Training outside the Classroom*

The 1904 Louisiana Purchase Exposition as Field School

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In 1928 A. L. Kroeber sent a University of California undergraduate anthropology major, Isabel Kelly, to remote Fort Bidwell, California, to do ethnography with the local Northern Paiute people. Years later Kelly complained to Kroeber that he had given her no preparatory training. Kroeber replied that "this field research business is not so much a technique as it is an art, and an art cannot be formally taught" (Kroeber 1955:1). One learns to do ethnography by plunging in and doing it, not by taking courses or being tutored in field methods. Kelly (1932), in fact, had plunged in and produced a quite credible monograph, the only systematic study ever done with the group. Kroeber and Kelly's exchange highlights a debate that has gone on in anthropology for over a century: How should student ethnographers be prepared to "go to the field"? In addition to data and theory courses, should they be tutored in field methods, introduced to ethical issues, perhaps sent through a field school? Or should they simply be taught some data and theory, then be handed a current copy of *Notes and Queries in Anthropology* and sent on their way to either "sink or swim"?

Prior to 1875, interested individuals who collected ethnographic and linguistic information on "Native peoples" were often guided by "heads of inquiry," "circulars," or ethnographic manuals produced, beginning in 1660, by scholarly societies in Europe and North America (Fowler 1975; Holmes and Mason 1902).[1] Most such guides were designed to ensure elicitation of information in a few hours or days from whatever Natives one happened to encounter in the course of other duties. The most thorough was written by Joseph-Marie Degérando (1969), produced in 1800 for the ill-fated French naval expedition to the South Pacific led by Nicholas Baudin. Degérando argued for plunging in and practicing participant observation over an extended time period as the only means of gaining accurate information about another culture.

By 1900 in North America, long-term "participant-observation" ethnographic field studies had been carried out by Franz Boas on Bafflin Island in 1883–84 (Douglas Cole 1999:63–82) and by Frank Hamilton Cushing at Zuni Pueblo between 1879 and 1884. For both, the experiences were life-changing rites of passage (although the term had yet to be coined). Boas (1888) published a solid, straightforward ethnographic account, the first of many he would produce. Cushing was very much in tune with the romantic exoticism of the nineteenth century. He depicted himself as an adventurous, scientific naturalist exploring the unknown at great peril. He published thrilling accounts of his experience at Zuni, intermingled with a corpus of significant scholarly data. He described how he developed methods of participant observation and learned the language, the better to understand Zuni culture and society. But he also described covert means of observation and note taking to obtain sensitive and secret data, often over community objections and threats. He justified his actions in the name of scientific inquiry (Baxter 1882a, 1882b; Cushing 1882, 1883a, 1883b, 1886, 1920; Hinsley 1983). By 1900 there was general agreement among professionals that the crux of good ethnographic fieldwork meant undertaking extended periods of intense interaction with a Native community. If this was not possible, one could conduct systematic surveys for more limited periods using a multiyear research strategy, especially for ethnological data collection (see Rivers 1913). If a new anthropologist was lucky, he could go on a British-style expedition; if not, he simply went and sank or swam. What mattered was that the data collection was systematic and that the fieldworker collected critical, basic information.

What had to be avoided at all costs was the appearance of brief, touristic, "in-and-out" encounters. In 1900 anthropology was just becoming established in universities, struggling to be accepted as a professional scientific discipline. Like other fields, anthropology was in transition from being a "field of study," pursued by self-taught individuals who had other day jobs, to being a "scientific discipline," meant to be pursued only by those properly credentialed through university training and the possession of advanced degrees awarded by universities organized on the German model (Diehl 1978). Franz Boas and Frederic Ward Putnam had introduced anthropological components of that model at Clark, Columbia, and Harvard universities and would help stimulate its introduction elsewhere in the years following (Darnell 1998).

But as anthropologists were struggling to secure their place in academia, they were faced with the specter of Scientism. In the 1870s Lewis Henry Morgan, John Wesley Powell, and Putnam had secured anthropology's admittance to the Science Club when the American Association for the Advancement of Science (AAAS) created an Anthropology Section. Confirmation of status had come with the election of Morgan, and later Powell, as AAAS presidents. But there were those, both within and outside the discipline, who questioned anthropology's status as a "real science." Physical anthropology as then practiced was very scientific, generating a plethora of mind-numbing measurements on skulls and long bones of both human remains and living peoples. But the rest of anthropology, especially ethnography, was scientifically questionable. Each field site was held to be so unique that data-collection techniques could not be replicated. This drew into question ethnology as a comparative endeavor: How could comparable, scientific data be obtained?

In addition to questions about anthropology's place among the sciences, there was also the issue of its place in university undergraduate and graduate curricula. In 1900 thirty-one universities taught courses labeled as anthropology, mostly as electives associated with a variety of other fields, especially sociology and psychology. Museums remained the active sites for archaeological and osteological training (Darnell 1969, 1970; Eggan 1974; Freeman 1965; Lowie 1956; MacCurdy 1902; Stocking 1979). Anthropologists realized that they had to expand this base and develop undergraduate and graduate majors, so that nascent professional anthropologists could be properly trained and credentialed.[2] Section H (Anthropology) of the AAAS established a standing committee on teaching that focused on three tasks: the development of textbooks, basic curricula, and arguments to convince college administrators to establish anthropology departments. Methodological issues, however, were not included in this committee's mission but were given to the anthropometry committee (Fewkes 1900:591).

Members of the committee began taking their case to professional meetings and academic assemblies. Calling the field "the crown and completion of the sciences," Frank Russell, one of the first professionally trained anthropologists and a leader of this initiative, argued for majors and for anthropology as the foundation of more specialized courses of study. Anthropology's "very comprehensiveness is a virtue; for thereby it is rendered suitable to serve as a framework for all other knowledge, a symmetrical framework lacking which the student but too often builds a series of mental watertight compartments that give no unity or harmony to the intellectual edifice" (Russell 1902:2). As an integrating course of study, anthropology would convey overarching theoretical paradigms, the basics of scientific logic, and would develop students' interpretive skills. This was fine rhetoric, but few universities were sufficiently convinced to immediately establish anthropology departments and/or restructure existing college curricula around anthropology as their integrative pedagogical paradigm. The anthropology departments that were established after 1900 were soon as balkanized as were other social and behavioral departments in universities and liberal arts colleges.

Nor did existing anthropology departments try to be integrative. Instead they specialized and claimed intellectual distinctiveness without using methodology or techniques as an educational foundation. In 1901 the Department of Anthropology at the University of California–Berkeley offered a general introduction to anthropology, ethnography courses on the Indians of California and Northwest Coast Athabascans, comparative North American ethnology, advanced work in ethnology, and several classes in linguistics, geological history, and North American and classical archaeology (Department of Anthropology 1904). No attention was paid to methodology in the curriculum. It was assumed that advanced students would learn to do ethnography by informally listening to fieldwork tales of their professors and by plunging into field situations. Students would simply metamorphose into professionals by working independently under a mentor in a pseudo-apprenticeship relationship (Stocking 1976:9). Kroeber was the de facto founder of the department at California, and as we have seen, he saw ethnography as an art. He did not encourage the development of a methodology course.

Science, with chemistry and physics as its models, demanded rigorous, detail oriented, and thorough training—not art. There were those who thought that ethnographic skills and methods, especially minimal requirements that one "record accurately" with "the keenest watchfulness" (Russell 1902:4), could be taught in university laboratories and museums. The idea of field schools was discussed informally, usually in terms of controlled situations in which students could learn to hone their observational and interviewing skills and take proper field notes before being sent out on their own in "real" field situations. The problem was that taking students into the field was expensive unless they paid their own way; furthermore, people were not at all sure it was appropriate for female students (Parezo 1993).

William J. McGee, first president of the new American Anthropological Association (AAA), agreed and promoted this agenda. But he was also concerned about public education and how anthropology should be presented to the public (McGee 1897). In 1903, when he was appointed director of the Anthropology Department at the Louisiana Purchase Exposition, he saw it as a golden opportunity to promote the discipline. McGee thought that international expositions were places where anthropology could develop its educational and professional agenda. Anthropological interpretation, he argued, had not yet been standardized; there were "nearly as many modes of interpreting as there are men to interpret." It was now time for these modes to be evaluated, he argued, as they had been in the older, more established sciences and for quantitative methods to take their place beside subjective qualitative techniques (McGee 1897: 253). McGee also knew that if anthropology was to be a respected synthetic and integrating science, it must demonstrate its professionalization, and that included formal training methods and standardized data-collection techniques. "Exact quantitative work is impossible without careful training, as numberless surveyors and teachers can testify. . . . The firm grasp of analogy and homology, and the clear recognition of energy and sequence, require both native capacity and systematic training" (McGee 1897:257). What better place to demonstrate a new educational technique—a comparative field school—that would advance anthropological pursuit of scientific status than at a universal exposition devoted to education?

Anthropology at the Louisiana Purchase Exposition

The 1904 Louisiana Purchase Exposition (LPE) was held in St. Louis to commemorate the 1803 Louisiana Purchase and American technological, political, and economic prowess. In acreage it was the largest international exposition ever staged. It attracted more than 19 million visitors, fewer than Paris in 1889 and 1900 or Chicago in 1893 but still a very successful gate. One of the reasons for its success, according to exposition president David Francis, was that the LPE contained "the world's first assemblage of the world's peoples" (McGee 1905). Ample evidence of Native peoples and their arts was found in tens of thousands of pieces of material culture exhibited in pavilions scattered across the exposition. Even more visible were the almost 3,000 non-Caucasians who lived on the exposition grounds. These men, women, and children from the Arctic; North, Central, and South America; Africa; the Middle East; Asia; and the South Pacific performed and demonstrated their "traditional ways and customs" in order to educate visitors about their cultures (Parezo and Troutman 2001; Parezo and Fowler in press). The exposition offered as many opportunities to see and actually meet members of what McGee called "the most striking tribes known to science" (Moses 1996:151) as it did opportunities to see the newest advances in electricity or giant sculptures made of butter, to estimate the potential for markets in Brazil or Japan, to eat an ice-cream cone for the first time or ride in a hot-air balloon.

The exposition was billed as the "University of the Future" (Breitbart 1997:30; Rydell 1984). It was the creation of the Louisiana Purchase Exposition Corporation (LPEC), whose stockholders and directors saw the enterprise as a unique event in which to educate the general public in an informal setting that combined entertainment and enlightenment. They also hoped to make a profit and encourage regional and international economic development. Trustees saw the exposition as a place where citizens would learn about advances in science and technology while absorbing and approving the related ideologies of American imperialism and progress. The exposition was grandiosely designed to summarize all existing knowledge in order to help people find their way in America's rapidly changing society. It would give citizens "new standards, new means of comparison, new insights into the condition of life in the world" (Rydell 1984:115). The exposition was to do this by emphasizing "process rather than products"; *how* one learned was as important as *what* one learned.

To highlight advances in knowledge, science, and scholarship, the LPEC sponsored scores of lecture series, classes, and demonstrations. And, following the lead of previous expositions, over 400 international congresses and conventions were held, many of them featuring nationally and internationally known scholars such as sociologist Max Weber. The congresses were open to all exposition goers, who could hear the lectures and speeches and thus expand their minds. Each department was also encouraged to organize practical education seminars and lecture series during the summer in collaboration with noted universities.

Anthropology had a conceptually (if not physically) prominent place at the exposition. Frederick J. V. Skiff, director of the Field Museum of Natural History and head of the exposition's section of exhibits, thought that a good universal exposition should be "a vast museum of anthropology and ethnology, of man and his works" (Skiff 1903:2). McGee agreed, although he knew that "the pictures brought up in most minds by the term Anthropology are those of alien and inferior peoples, or of human curiosities and monstrosities exhibited in midway plaisances if not in circuses and dime museums" (McGee 1897:249). Like Skiff, McGee felt that anthropology, the "newest of the sciences," was a system of thought that would integrate all other forms of knowledge because it searched to understand "that broader and nobler side which distinguishes mankind from all other things" and demonstrates the processes by which evolution had taken place. As McGee had written in "The Science of Humanity," "Knowledge is ever passing from the individual to the common and from the special to the general, and thereby its quantity is constantly increased and its utility extended; during recent times it is passing also from the empiric to the scientific, and thereby its quality is improved and its beneficence multiplied" (1897:248).

The unparalleled chance to exhibit, produce new knowledge, and teach anthropology in action, as it were, was a golden opportunity not to be missed, McGee thought. As he wrote in *Science*, now that the Department of Anthropology and cognate branches of the exposition were well under way, it had become clear that the Louisiana Purchase Exposition afforded unequaled opportunities for ethnologic study (McGee 1904i:253). In *The World's Work* he wrote: "The studious visitor will enjoy, on the Exposition grounds, opportunities for accurate study hardly less useful than those hitherto available only through weeks or months of life in Indian settlements. The industries, too, will be normal, and visitors will be enabled to obtain as souvenirs or as specimens for scientific study, objects of Indian handiwork produced by native methods under their own inspection" (McGee 1904j:5187).

Educational leaders could lead students in fieldwork that utilized those 3,000 Native peoples, a never-before-proposed and probably never-to-be-repeated opportunity to study the physical and cultural differences of all races at one time and in a single place. Students, with proper guidance, could systematically observe, intensely interview, and compare the everyday lives and the physical and social development of the peoples housed in the Anthropology Colonies, Indian Villages, and Philippine Reservation. These peoples included Ainus, Africans, Tehuelches, Cocopas, Kwakiutls, Nootkas, Mbutis, Igorots, Negritos, and hundreds of American Indians from dozens of tribes. Here, the aims of ethnology, which McGee defined as the "the science of races," could be furthered.[3]

Thus, McGee conceptualized the exposition as an ideal place for professional anthropologists to conduct summer research in systematic comparative ethnology and anthropometry on a globally diverse population. It was cost effective and safe (especially for women), a condensed lifetime of study in a single place. Potential research subjects were somewhat segregated from the rest of the exposition but close enough to the "impressive display of the achievements of modern man in the large exhibit palaces" that students would not miss the other attractions. Fun and work could be easily combined (McGee 1904i:253–54; 1904a:5–6).

Newspapers had picked up on McGee's research and educational dreams before the exposition opened and told their readers, "to the ethnologist who looks below the picturesque surface, which has a perennial charm for all, the exhibit will have a great interest" (*St. Louis Post-Dispatch* 1904a). Other newspapers periodically reiterated this contention during the course of the exposition, usually after an interview with McGee. A few visitors discussed the possibility of informal and individual study. A Major General H. C. Corbin was quoted in the *Indian School Journal* as recommending that all parents send their children to St. Louis in groups, escorted by competent guides, to study the ethnic exhibits and question the "primitive" people (*Indian School Journal* 1904). While McGee thought docent-type tours were commendable, he was promoting professional-level research and formal education for university credit. He wanted to train teachers and also potential anthropologists.

McGee wrote to about 50 university and museum officials on June 1, 1904, stating that the "largest assemblage of the world's peoples the world has ever seen" provided students and professors "opportunities for instructional work such as could not be enjoyed otherwise except at the cost in time and money of extensive journeys with attendant hardships." After describing the sections of the exposition that included ethnic and racial groups, McGee reiterated what he had told professionals—that theoretical advances in general evolutionary anthropology, which he defined as the study of man and his creations as exemplified in culture grades and stages of psychic development, could be made through this unparalleled comparative-research opportunity. Special prospects existed for student projects and instruction in Somatology, Psychology, Arts and Industries, Languages, Law and Socialtry, Faiths and Philosophies, General Anthropology, and General Ethnology. McGee also proposed a new course, Record Work—the making and preserving of lists and tables of measurements, sketches and diagrams, photographs, life casts, life models, painting, and sculptures. He was particularly optimistic about the value of this methodology course, since he assumed instructors could utilize the "services" of some 100 distinct tribes. He assured professors that they would receive all possible assistance in terms of facilities and access, because "the sole purpose of the department [is] to educate" (McGee 1904c).

To help students conceptualize appropriate projects, McGee listed several dozen concrete projects for class or independent study. Students could compare physical types, study actual behavior, conduct experiments in psychic character, document how tools were made, compare how artists in different cultures used their hands, record languages, study how different culture grades defined their laws and social organizations, compare primitive and advanced peoples, help with the anthropometry experiments, or make life casts. As far as we can tell, no professor or instructor took him up on the offer with the exception of Frances Densmore, an independent ethnomusicologist, who studied the music of the Filipino Natives, concluding that it belonged to a developmental period preceding that of the American Indians (Densmore 1906). No master's theses or dissertations appear to have been written by students attending the exposition, nor do there appear to have been any official university expeditions to study the indigenous people. The main site of research remained the department's anthropometry and psychometry laboratories housed in the basement of the anthropology building, which drew much visitor interest but produced little in terms of scientific advancement (Parezo and Fowler in press).

Nonanthropologists, however, often approached McGee for permission to observe Native behavior or conduct research in comparative anatomy and physiology; a few proposed experiments that McGee did not approve. McGee did give permission to Gus V. R. Mechin of St. Louis to see if Indians would be interested in studying French as a test of their mental abilities. He told Mechin that if he broached the subject tactfully a few might respond positively, but he doubted that the Indians would be interested (McGee 1904f). Unfortunately we have found no information as to how the Indians reacted to the proposal, although we can speculate that there were no willing research subjects. There is no record of the project's being carried through. The results were never published.

McGee wanted some university to hold a special biological anthropology (anthropometry) or ethnology field school at the exposition. He assured university administrators and professors that they would be afforded every assistance in facilities (including housing for students), publicity, and access to research subjects. The only individual to respond was Frederick Starr, an associate professor of anthropology at the University of Chicago whom McGee had engaged to lead an expedition to Hokkaido, Japan, to bring a group of Ainus to St. Louis. Starr agreed to establish a field school—the Louisiana Purchase Exposition Class in Ethnology—as a University of Chicago extension course. During the 1893 World's Columbian Exposition in Chicago, Starr had presented a special lecture series, "Native Races of North America," which he subsequently repeated for the Chautauqua circuit in 1894–95. He thought he could utilize this model, modify the subject matter to correspond with the LPE's anthropological emphasis, and add appropriate tests and assignments to construct a credit-carrying lecture and fieldwork course. It would be a systematic class in Practical Ethnology. The St. Louis venture would be the first field school in ethnology in the United States. Starr hoped to turn a tidy personal profit from the undertaking as well as extending his influence in the Midwest. McGee promptly agreed and promised to help with the planning and to send out an announcement. The resulting field school reflected Starr, his research interests, and his views on academic and popular education.

Frederick Starr: Populist Educator

Frederick Starr (1858–1933) was a strange man, with a reputation among his contemporaries for espousing outlandish theories and supporting unpopular causes. The son of an abolitionist Presbyterian minister who had been run out of Missouri, he had received a doctorate in geology from Lafayette College in 1885. While serving as curator of geology at the American Museum of Natural History, Starr became interested in anthropology. After he extensively read ethnographic and physical anthropological theories and learned about data-collection techniques from circulars, Putnam placed him in charge of the museum's ethnographic collections. In 1891 Starr became a professor of geology and anthropology at Pomona College in California. The next year he moved to Chicago when Putnam and Boas hired him to conduct anthropometric work among the Eastern Cherokee for the 1893 World's Columbian Exposition in Chicago (Miller 1978). Throughout this period Starr remained active on the Chautauqua circuit, with the rank of professor in New York's Chautauqua Institute. When William Rainey Harper, head of the institute, became president of the University of Chicago, he appointed Starr assistant professor of anthropology. Starr earned a promotion to associate professor with tenure in 1895, a rank he held in 1904.

Starr was a prominent public lecturer on anthropological topics and, indeed, something of a celebrity. In this regard he was an international leader, one who helped convey the essence of the new discipline to the general public, thereby increasing the discipline's status. Newspapers often reported on his lectures; even when he spoke at small colleges in rural America, the *New York Times* would discuss his views (e.g., 1889). His lectures were always entertaining. During a trip to England in 1900 to present the Huxley Lectures at the Royal Anthropological Institute and donate his Mexican ethnographic artifacts to Cambridge University, he lectured to the British Folklore Society. Society president C. H. Read concluded that "an evening could scarcely be passed in a more interesting and entertaining fashion." (1900).

Starr apparently had few equals as a provocative evolutionist who could convey complex information in an understandable and compelling manner. His public reputation as an immensely entertaining speaker who loved the limelight carried over into the classroom. Fay-Cooper Cole said that Starr was the most popular instructor in sociology and anthropology at the University of Chicago until his retirement in 1923. Called "Lone Star" on the campus, he worked by himself, rarely socialized, and refused to take students into the field on his research trips (Fay-Cooper Cole 1935). He was remembered for his personal idiosyncrasies — never wearing an overcoat and refusing to use a telephone — and his lecture style rather than for building a departmental, professional, or intellectual legacy. During the 1910s to 1930s Albion Small, Robert Park, George Herbert Mead, and their colleagues, not Starr, built Chicago sociology into the premier department in the United States (Bulmer 1984:33–40). Starr was simply a deadwood anthropological appendage to the department.

Starr had strong theoretical views and moral convictions, and "his frankness and fearlessness in the expression of opinion often made him enemies" (Fay-Cooper Cole 1935:533). He often overgeneralized when a piece of information fit his paradigm, steadfastly held onto unilinear evolution as his central interpretive framework even when it went out of style, and expressed many ideas that quickly became notorious. For example, he held that American Indians had not migrated from Asia, Israel, or Ireland but originated in North America as an amalgamation of peoples from all over the world who were transformed by the environment into the "Red Race," except for the Algonquians, who were the descendants of Norsemen. He predicted that white European immigrants would soon turn into "Red Men" in America (*Assembly Daily* 1893; *Chicago Inter Ocean* 1895; Starr 1894). He spoke against U.S. imperialism in Cuba and the Philippines in 1898; in 1902 he stated that *Pithecanthropus erectus* spoke baby talk; in 1903 and 1904 he repeatedly stated that Japan would win the war with Russia because they were genetically superior (Darnell 1969:157–58; *Rochester Chronicle* 1904; Starr scrapbooks). While Starr was a believer in the natural superiority of Anglo-Saxons, at every opportunity he expounded against colonial assimilationist policies as violations of sovereignty. Like McGee, Starr considered anthropology to be the height of scientific development. Throughout his career he focused on comparative racial and cultural development, that is, the "origin, position, structure, reaction, appearance, movement, varieties, achievement, and progress" of mankind (Starr 1895:283). His theoretical reasoning and conceptualization of anthropology complemented McGee's but most closely resembled that of Daniel Brinton (1892; Starr 1895; Darnell 1970). A man of boundless but often diffuse energy, Starr's research interests were eclectic and often not in sync with those of his contemporaries. He studied cross-cultural deformity and albinism, photography, religious amulets and charms, anthropometry and racial taxonomy, teeth, head shapes, racial variation, tattooing, hairdressing styles, riddles, and bookplates. Following the tradition of the gentleman naturalist, his methods, by his own accounts, were superficial and, based on our contemporary standards, sometimes unethical (*Christian Union* 1892). Most of his publications were marginal, outdated, racist, and fragmentary — little more than field notes and photographs strung together as a travelogue (Starr 1899, 1902). Starr was adept at the ethnographic-anthropometric travel sweep using natural-history methods of taxonomy to prove the correctness of his evolutionary assumptions. A bachelor with no family obligations, he traveled widely, especially in Mexico, where he sojourned yearly between 1894 and 1903. Historian R. Berkeley Miller summed up Starr's career: "Insofar as anthropological theory and practice are concerned, he left little of insight or originality" (Miller 1978:52). To George Stocking, Starr was a person "rooted permanently in late nineteenth-century evolution" (Stocking 1979:12).

Starr loved international expositions and saw them as effective outlets for his work (Rydell 1984:166). Between 1885 and 1901 he erected personal displays based on his Mexican fieldwork at expositions in Nashville, Atlanta, Amsterdam, Madrid, New Orleans, Chicago, and Buffalo. He requested space to exhibit his Mexican photographs and collections in the LPEC's static anthropology exhibits as early as July 1903, when he brought McGee to the University of Chicago to give a series of lectures on the exposition and his theories (Starr 1903a, 1903b, 1903c). He also agreed to help McGee secure the services of the Ainus as a living exhibit and think about a field school.

Starr was especially interested in the Ainus because he wanted to test an idea espoused by German scholars that there was an ancient Ainu-Caucasoid connection. According to Kotani, "this idea was attractive to Europeans because it posited the continuing existence of an early European hunting-and-gathering people and because of the romantic notion of a 'Caucasoid' ethnic group surrounded by a sea of Mongolians" (1999: 137). Starr and McGee saw the expedition as an excellent opportunity to generate interest in the discipline and to "solve" this taxonomic problem, which was, in fact, a nonproblem. Contemporary anthropologists hold that the group is a genetic isolate and that their cultural creativity was not due to ancient contact with "superior" Caucasians. But in 1900 it was the Ainus who did not fit Eurocentric evolutionary taxonomies (Parezo and Fowler in press).

To Starr the expedition to Japan was an opportunity to see a new part of the world, meet people who were anthropologically interesting, and write a basic ethnographic description (including a biological and racial assessment) of an unknown culture as well as to address the question of the Ainus' taxonomic placement in the evolutionary tree of life. Starr's travels, as related in his book *The Ainu Group at the Saint Louis Exposition* and his field notes (Starr 1904a, 1904d; see also Van Stone 1993; Parezo and Fowler in press), constitute an exciting adventure story full of hard-ship, war, diplomatic uncertainty, bureaucratic barriers, discovery, train wrecks, barely met steamships, hikes over mountainous trails in blizzards, doubt, ethnographic information, racial assessments, perseverance, and successful chases for artifacts, houses, and demonstrators. It required all of Starr's persuasive skills to convince multiple levels of government officials — from local police to the emperor's representatives — to allow the Ainus, wards of the state, to leave their homeland and travel to America (Starr 1904d). Finally, it took him many days to negotiate contracts with the shrewd Ainus who agreed to come and serve as a living ethnographic exhibit: Bete Goro, Ozara Jukataro (Yazo), Shirake, Kutoroge Himaruma, Shutratek, Sangyea Hiramura, and Santukno. (Two babies, Kiku and Kehttle, accompanied their parents, and Yoichiri Inagaki served as interpreter.) The Ainus negotiated better contracts than any other Native group demonstrating in St. Louis (Starr 1904b:75–76, 1904c, 1904e; McGee 1904e).

The expedition also meant dealing with McGee and the LPEC bureaucracy. Because the trip was so late in starting due to continued negotiations among Starr, McGee, the LPEC, the U.S. State Department, and the Japanese government and a lack of a contract or travel funds until two days before he left, Starr had to take a leave of absence without pay from the University of Chicago for the semester. This left him strapped for funds, so he decided to offer the proposed field school as a way to make up for his monetary shortfall. But he was apprehensive, and with good reason. He was no longer certain that he could trust McGee, and he was not enthusiastic about the field school, despite his later press releases. Starr's research techniques did not lend themselves well to classroom teaching nor to classroom exercises. In fact he was noted for disliking laboratory courses and had previously refused to teach methodology. He was more interested in theory and subject-matter courses. Nevertheless he thought he could do it, and he outlined lecture topics during the trip back from Japan.

Starr also wrote McGee that he was concerned the LPEC and McGee might not generate enough publicity in a timely manner to attract a large class, which he needed financially since he would be paid on a per student basis (Starr 1904e). He was also concerned because his LPEC contract stated that he had to give the corporation 25 percent of his proceeds. The school — like the journey to Japan — could be a very expensive venture for Starr, especially considering that in order to undertake the St. Louis class, he had to cancel his summer university classes and several Chautauqua lectures. This meant that he would have no income for the summer, not to mention no reimbursement for all the time he would spend preparing lectures and organizing the curriculum. This was a situation that many adjunct professors today understand only too well.

Anthropology's Practical Ethnology Field School

When Starr returned to his duties at the University of Chicago after settling the Ainus in St. Louis, he waited several weeks for McGee to arrange and publicize the field school, but nothing happened. Starr knew how important planning and publicity were. He had been teaching extension courses since 1893 in cities throughout Illinois, generally a series of four lectures on topics such as the Mayas, origins of writing, prehistoric races and mounds, the development of language, and human physical characteristics. All had filled because Starr had generated lots of publicity.

Increasingly concerned that the class would never happen and, even if it did, that students would not come because they would not know about it or would have no time to prepare, Starr began contacting newspapers in early June (*Marion Tribune* 1904; *Chicago Recorder* 1904). He needed at least 25 students to break even, and he preferred male anthropology students. Starr's scrapbooks contain dozens of newspaper announcements; he must have spent a good deal of time contacting reporters throughout the Midwest. These pieces discussed the educational value of the undertaking and what students would learn as well as the uniqueness of the undertaking, stating that "such an opportunity for definite, practiced study of so many interesting peoples has probably never before been offered" (Starr scrapbooks).

Starr also conveyed his optimism about the rare opportunities for comparative ethnography, which would focus on describing the life, customs, languages, arts, and religious beliefs of each group, and physical anthropology (somatology), which would analyze the physical attributes and variations of the different races. "Live Igorrotes and Patagonian giants are better than textbooks and pictures in teaching anthropology classes," he told one reporter. He would conduct a class in which students would actually see Native peoples with "their dances and other antics, their habitat, their modes of life, their idiosyncrasies, and other matters of interest concerning them" (*Chicago Recorder* 1904).

Starr was not above using exoticism to generate interest. Most of the headlines announced that the class would be on "The Pike [the Midway] so that students would see odd people" (*Chicago News* 1904). He stressed that students would be given authenticity, that is, "true nativity" as well as a chance to interact with "wild men" and that he would elucidate "the mystery of their dances, the symbolism of oriental fascinations and the religious foundations of their rites" (*Chicago Journal* 1904). To entice people with the timeliness of the undertaking he stressed that this would be the last exposition of its kind, since Native peoples were fast disappearing from the world, altered by their contacts with civilization. Some reporters did not get his pitch. The *Chicago Journal*, not understanding anthropology, stated that Starr would conduct a class on psychology so that "seekers after knowledge" could analyze the "wicked place," that is, The Pike. The reporter was very concerned that the topics and locale were not suitable for women — Starr's target audience. But Starr was reassuring: "When the professor reduces the strange customs of The Pike to an ethnological, historical and psychological basis, he believes the co-eds who are going with him will not have to blush, nor the other students to grin at what they see and hear" (*Chicago Journal* 1904).

Starr decided to target potential students in the western United States. In early July *The Oregonian* announced that "no doubt there will be a rush of students to join such a fascinating class, and Professor Starr is to be congratulated on the work he is doing in making the road to knowledge a meandering pathway through a flowery meadow" (1904). Unfortunately, despite these efforts no one had signed up by mid-July, because no one knew where to apply. Starr needed at least 25 students to break even. He hired Frank Adams to serve as general secretary for the course, paying him with his own funds. They decided not to wait for the LPEC or McGee and sent their own announcement to midwestern universities — as a personal letter — as well as placing a notice in the *American Anthropologist* (Department of Anthropology 1904:582). Next they specifically targeted women who were in elementary education in the hope that theories of cultural evolution would be added to their basic courses in pedagogy or science. A special announcement was also sent to the National Society for the Scientific Study of Education describing the proposed field school as a place where future teachers could learn how to assess the scientific accuracy of the information they would teach (Hewett 1904). While many individuals expressed interest, most students wisely decided not to sign up until the arrangements for room and board had been finalized by the LPEC. As the time for the class approached, Adams took it upon himself to find potential lodging, calling on friends to make inquiries, and established a student housing network with St. Louis residents. Another issue arose: some students wanted to take only half the course or receive only one hour of credit rather than the three required by the University of Chicago. While the dean of the extension service declined to offer partial credit, the University of Wisconsin agreed. Adams worked diligently on these details, as did an unnamed individual at the University of Wisconsin who served as unofficial secretary for the undertaking (Adams 1904a).

Even with Adams's labors, Starr was not sure the field school would work. He sent McGee several letters expressing his concern that the LPEC had not fulfilled its share of the agreement. "I have begun preparation for this course and am doing what I can to make it known. A few students are sure to take the work, [but] I fear not enough to make it a success financially as we are so late in getting it before the public. I shall however go on with it" (Starr 1904f). Starr realized he needed a patron to underwrite the school's expenses and offer scholarships. To begin his search and generate interest, he again turned to his tested mechanism for self-promotion — the press.

Periodically McGee told Starr not to worry, but Starr no longer listened to his assurances. Starr wrote his mother that he hastened to St. Louis in mid-August after learning of the mass exodus of American Indians due to the fairground's deplorable conditions (see Parezo and Fowler in press). No Natives negated his entire course structure. What he found was disheartening.

> I am trying to straighten out things with the Exposition relative to my class and my book. They are very hard to deal with, having no business ideas or methods. It was lucky that I went, as I had to arrange various things. By their mismanagement my study material is dropping away! More than half of the Indians have gone in disgust and whole lectures upon which I have labored have been prepared for naught. Found no arrangement for rooms and none of the promised "advertising": no arrangement for sale of my book — in other words everything in disorder. I have reorganized the work and shall draw on The Pike for part of my material; am pushing the matter of a classroom and shall have to continue to do so. [Starr 1904h]

Starr was clearly angry. Nothing was ready; no arrangements had been made, and the LPEC's printed announcements would probably never have been distributed if he had not gone to St. Louis.

When Starr came back to St. Louis the week before the class was to begin in mid-September, he discovered that apparently none of the Native educational subjects were prepared for the classes. None of them, even the Ainus, knew anything about it. Starr had to insist that McGee write all concessionaires and special agents who were looking after Native peoples and request permission for his students to visit the compounds and housing interiors — activities in his lesson plan that were critical to the success of the endeavor (McGee 1904b). McGee finally sent a number of letters, which can be found in his personal papers along with two affirmative responses. Two days before the first class, Starr knew neither where he would lecture nor which groups had agreed and which had not. He decided to simply show up at an encampment and begin lecturing. Starr wrote his mother several times about the mismanagement he found at the Exposition in general and McGee's ineptitude in particular. By the time the classes began, Starr had little respect for McGee's administrative skills.

The field school was plagued with logistical problems, including a lack of dedicated classroom space. McGee had neglected to arrange for a lecture room or study rooms even though he had written to Starr in early September stating that Starr could use the auditorium in the Model Industrial Indian School. When Starr showed up on the first day of class at 7 A.M. for a 9 A.M. lecture, however, Samuel McCowan, the director of the Indian School, informed him that it was not available since it was continuously booked for student demonstrations, recitals, and concerts. McGee had failed to inform McCowan of his commitment, nor had he told Starr that the space was not his to assign. Instead, the class met on the steps of Cupples Hall that day. Students were evidently discouraged by the lack of on-site arrangements, but at least it did not rain. Starr spent the afternoon looking for a classroom and received permission to use the upper-floor lecture hall in the Alaska Territorial Building after basically standing over McGee's shoulder and insisting he locate a classroom. Starr found the space ideal because of the extensive ethnographic collections lining the walls (McGee 1904d, 1904g). McGee also received permission from Washington University in St. Louis to use university dormitory common rooms for afternoon tutorials.

Starr evidently had a student body of 2 men and 27 women from the universities of Wisconsin and Chicago who took the entire course for three hours of credit in ethnology as well as several St. Louis schoolteachers. Few names have survived: Louise Murray, Marion Kellogg, Grace Williamson, Bernice Benson, August Scott, and Marie Ortymayer were education and liberal arts students enrolled at the University of Chicago (Adams 1904b; *St. Louis Globe Democrat* 1904; *The World Today* 1904). Two students from St. Louis, Laura E. W. Benedict and Jeannette Obenchain, opted for one hour of credit. Official students were given passing grades and certificates upon completion of all lectures and field trips, the requisite term paper, and a satisfactory review of their field notes. Apparently none of the students intended to concentrate in anthropology or sociology. They were not, evidently, what Boas (1919:46) later called professional students, that is, those who planned a career in the field.

The course, which ran from September 1 to September 21, was officially a University of Chicago extension service laboratory class rather than an

LPEC event, unlike the August university lecture course in physical education, sport, physiology, and anthropometry sponsored by the Department of Physical Culture (Sullivan 1905:185–86; Bennitt 1997:565). It was the University of Chicago, therefore, that was awarded a department grand prize for the effort. The LPEC had the contract with the university as well as Starr, and it was the university that sent the LPEC its commission. Tuition fees were $12 for the full three-week course or $5 for one week. Individuals who wanted to spend only one day were charged $1. Expenses for students were underwritten by a gift of $5,000 from an anonymous donor at Columbia University. (Starr had been successful in his fund-raising effort.) To help offset his own expenses and generate a profit, Starr devised a new category of "student" after the classes started. For those wanting to come only to a single one-hour lecture, he charged 35¢. Watching a one-hour demonstration cost 50¢. Students paid Starr directly (not McGee or the LPEC), and he gave the University of Chicago their cut. Most of the part-time students were St. Louis schoolteachers (Starr 1904f, 1905:40–42; McGee 1904a; Rydell 1984:273–74).

The course consisted of a systematic series of class lectures, practical talks, demonstrations, observational exercises, directed independent research, and tutorials. The schedule included three hours a day of formal lectures, six days a week, for three weeks. At 9 A.M. Starr gave a general lecture about one or two of the Native groups, describing their cultures by emphasizing their distinctiveness as cultural types. In these lectures Starr also talked about the physical and mental characteristics of each group and how they were racially distinctive. At 10 A.M. he lectured on a synthetic subject or special problem: art, industry, customs, practices, or beliefs among "savage" or "barbaric" peoples. These lectures also included demonstrations of manufacturing processes whenever possible, and students were expected to look at the prehistoric and historical artifacts on display in the Anthropology Department, Philippine Reservation Ethnology Museum, and Smithsonian Institution exhibits to gain a more in-depth understanding of manufacturing processes and changes in material culture through time. At 11 A.M. the class visited the group being discussed and inspected their homes. Students then worked individually in the afternoon, interviewing and observing behavior. The following table summarizes the daily lecture schedule.

Many of the comparative lectures were ones Starr used in his university classes and his summer Chautauqua series. The lecture on bodily modifications dealt with the origins of clothing and the ways in which people had made the body more aesthetically pleasing though piercing, tattooing, and binding. His conclusion was that modesty, aesthetics, and protection

Calendar of 9:00 and 10:00 o'clock Lectures and 11:00 o'clock Visits and Demonstrations

Sept.	9:00 A.M. Lecture	10:00 A.M. Lecture	11:00 A.M. Lecture
1	Northwest Coast Tribes	Social Organization: Totem Poles	Kwakiutl and Nootka
2	Southern Athabascans	The Study of Games	Navajo and Apache
3	The Pueblos of Today	Religion of the Pueblos	Pueblos, Pimas, Maricopa
5	The Cliff Dwellers	Archaeological Theories	The Cliff Dwellers (Pike)
6	The Sioux	Sign Language and Gesturing	The Indian Congress (Pike)
7	Cocopa and Desert Tribes	Bodily Modifications	Cocopa Settlement
8	South American Indians	Origin of the American Indians	The Patagonians
9	The Eskimo	Adaptation to Environment	Eskimo Village (Pike)
10	Pygmy Problems	Cannibalism	Batwa and other Africans
12	Ainu of Japan	Physical Characters of Race	Ainu Compound
13	The Negritos	Fire-making	The Negrito Village
14	The Igorot	Head-hunting and Kindred Customs	The Igorot Village
15	The Visayans and Moro	The People of the Philippines	The Visayan Village
16	The Moros	Music and Musical Instruments	The Moro Villages
17	The Japanese	Art Industries	Japanese Pavilion; Varied Industries Exhibit
19	The Chinese	The Evolution of Writing	Varied Industries Building
20	The Aztecs of Ancient Mexico	Native American Sculpture and Architecture	Government Building
21	The Indians of Southern Mexico	The Exposition's Department of Anthropology	Anthropology Building

Source: "Department of Anthropology of the University of California." 1904. *American Anthropologist* 6(4):583.

against harsh climates were intertwined (*Assembly Herald* 1904). In all the lectures, including those on specific cultures, Starr stressed evolutionary origins. The press came to many lectures, possibly in the hopes that Starr would say something provocative. They were not disappointed. Controversy had swirled around the Igorots and their "nakedness" (i.e., wearing only loincloths) since the beginning of the exposition in May, and Starr commented on the foolishness of the controversy in his lectures while visiting their compound, using Native peoples to critique American society: "The attempt of certain persons to put clothing on the Igorottes is evidence not of modesty, but of the immodesty of Americans. There is nothing wrong in the appearance of these people clad in the simple but perfectly decent garb of their country" (*Chicago Tribune* 1904). Starr interspersed a good deal of cultural relativity in his evolutionary rhetoric as he tried to debunk popularly held preconceptions and theories, especially those that interfered with Native people's rights to self-determination in their cultures and values.

The first day of class was quite spectacular despite the problems. The Kwakiutls performed a rendition of a Hamatsa initiation ceremony (termed by the press the "infamous cannibal dance"), while Starr explained its cultural significance. According to a reporter, the class found the performance very realistic:

> No human flesh was actually eaten on this occasion, but the dancers went through the motions so vividly that several of the ladies in the class were obliged to turn their heads away. Savage and blood curdling yells, horrible expressions of fear on the faces of the dancers, fastening of the teeth in the flesh and smacking of lips, were the climax of the wild dance. When they finished, the dancers, worn out by the tremendous emotional strain, sank to the floor. A burst of applause from the cultured audience of well-dressed women greeted the thespians of primitive origin at the conclusion of the dance. [*St. Louis Examiner* 1904]

Starr then lectured on matrilineal societies and their supposed place in the evolutionary scale. He combined this with his opinions on why the Northwest Coast peoples had to advance culturally but said the road they had to take to accomplish this was questionable. "'The rule of woman in the family is the most primitive social state. It retards progress and civilization cannot come about among any people until the headship of women disappears,' says Starr" (*St. Louis Examiner* 1904).

Starr had very strong views on most subjects, including whether women could be anthropologists. He held that there was and should be a strict sexual division of labor, with women working in the domestic sphere and men in the commercial and destructive spheres (i.e., war). He felt that families ruled by women were barbaric and informed his students that some of the groups understood this while others did not and were corrupted (*Talks of Moros and Bagabos* 1904). The latter were not good candidates for cultural evolution, he said, and the U.S. government was wasting funds trying to assimilate them.

On another occasion Starr lectured about the development of manners and politeness, the effects of civilization, art, clothing, and the nature of women's work. On September 7 the group saw a supposed "snake dance" in Tobin's Cliff Dweller concession while Starr lectured on Hopi culture — but there were no Hopis there. The Cliff Dweller concession was staffed by men and women from San Ildefonso, Laguna, San Juan, and Santa Clara pueblos who gave parodies of Hopi and Zuni dances and ceremonial performances based on the concessionaire's interpretations of Frederick Hodge's accounts of the Hopi snake ceremonies. Starr never acknowledged that he was describing a staged performance, probably because he had not prepared lectures on Rio Grande Pueblos, but only the Hopis.

Starr included his opinions on everything he discussed in his lectures and cared little for alternative interpretations. Nor did he care whose feelings he hurt or how accurate his portrayals were. He expected Native demonstrators to accept his theories and rhetoric and stand passively by until they were asked to demonstrate some skill. While discussing the Eskimos and the Northwest Coast Indians during a tour of the Alaskan building exhibits and while standing in front of Haida men and women who were demonstrating, he noted that Alaskan Indians of long ago wore beautiful garments but that "those of today masquerade in overalls and other ugly castoff clothing of the white man." The same was true for art, he claimed. Their beautiful totem poles and baskets are gone. "Today they do these things not for themselves or their love of beauty but for the fee the chance traveler may have give" (Talks of Moros and Bagobos 1904). Tourism and commercialism were ruining "the pure Indian," but there was still hope for the peoples of the Philippines. Starr declared that the Moros and Bagobos were superior to other groups in part because of their exclusivity and aversion to being physically or culturally touched by strangers (Student Notes 1904). While noncivilized peoples benefited from civilization, Starr said, American imperialism corrupted. Native peoples needed to build barriers and limit their contact if they were able to do so.

While ethics and research sensitivities were not part of his curriculum, Starr interspersed his critiques of colonialism, foreign policy, assimilation, and the inevitability of evolution throughout his lectures, much to the delight of the press and his students. He was particularly sarcastic about the U.S. Indian Service and the baseness of American colonialism: "So far as the future of the Red Man is concerned he is safe. As soon as he is denuded of such treasures in the way of lands, cattle and expectations as he may now be possessed of, he will go to work and take care of himself." Indians had to do this, Starr said, because the U.S. Indian Service taught them nothing and there was now no alternative. When discussing the Filipinos, he was recorded as saying: "We have not done one thing to uplift the Indian. We can never help the Filipino. All history teaches that the stronger minded races override the weaker ones" (What Prof. Starr . . . Thinks 1904; *St. Paul Globe* 1904). He also railed against Christian proselytizing: "Religion of the missionaries is absurd because it condemns anyone who is [*sic*] not heard of it to eternal damnation. A primitive man living in his traditional way is a better man" (*New York Evening Journal* 1904).

Whenever Starr focused on gender and the natural and proper roles for men and women, the press noted his remarks extensively. In fact their reports on the field school incorporated commentaries on what constituted proper education for middle-class white women, including those in the anthropology field school. The press was fascinated yet appalled by the idea of young women watching seminaked men "with broad bronzed shoulders" and live, wiggling snakes in their mouths (*St. Louis Examiner* 1904). They were even more upset with the graphic and realistic nature of other demonstrations, especially when a Negrito woman in the Philippine Reservation showed how tattoos were made by carving one on her husband's back while Starr lectured (Chicago Co-eds See 1904).

Starr used Native cultures as interpreted through his conceptualization of "primitiveness" in order to criticize what he thought was wrong with American society, never missing an opportunity to discuss changing gender roles. He pointed out special features of Native cultures that he thought the women in his class should emulate once they became mothers, telling students that their babies would be healthier, happier, and better looking if they used a cradle board for them as the Navajo weavers did. American mothers were "deforming their children by permitting them to crawl and toddle about. Their legs are too weak. Strap the legs together, straight and firm, and strap his shoulders so that he will develop like an Indian" (*Milwaukee Press* 1904). The result would be excellent posture.

Most demonstrations were evidently benign enough for women to see. While they were of questionable scientific or educational value, they were entertaining. Starr held a fire-making competition in which a Negrito man "beat" Ainu and Mbuti men, which, he concluded, told students much about race, racial abilities, and race's potential for advancement (Negritos Defeat 1904). In fact race was Starr's constant theme, as was evolutionary progress. When the Ainus were the subject of Starr's ethnographic lecture on September 12, it was followed by a lecture entitled "The Physical Characteristics of Race" that utilized the anthropometry laboratory. Starr felt this central message should be self-evident to his students, reinforced by their independent afternoon studies.

Starr originally envisioned working with about 40 groups that he felt well represented different races. Of course no one had asked the Native peoples if they would like to be research subjects or participate in a university class. As noted above, until the last possible moment McGee had neglected to inform even his own departmental special agents that there would be daily lectures and also a set of special Saturday afternoon lectures during September for high-school teachers—meaning that Native participants would lose one of their free time periods—and then he mentioned it only at Starr's insistence. Nor did either McGee or Starr offer financial compensation for the work the Natives had to perform for the classes. In cases where there was a bit more preparation and when Natives were paid, things apparently went smoothly. On September 2 McGee wrote to A. J. Smith, the manager of Tobin's Cliff Dweller concession, asking him to help Starr by giving him and his students free admission to the exhibit. McGee suggested that in return, Smith would receive free publicity. Some groups were used to intrusions, because they occurred on a daily basis. According to one Fair official, the Igorots made excellent teaching subjects:

> The communities of uncivilized peoples at the Universal Exposition served purposes other than the satisfaction of random curiosity. They were measured and photographed and cross questioned by scientists and studied by many visitors interested in ethnology. They furnished the live object lessons for lecture courses. The Igorot in their turn faced a class in Ethnology while the professor dwelt upon their customs, vocations, religion and ceremonies. They took kindly to this service in the study of man. They answered questions, pertinent and impertinent, about themselves. [*St. Louis Post-Dispatch* 1904b]

When the students descended on their homes unannounced, several Native groups refused to assist Starr, and he had to change his lectures. The Islamic Moros asked them to leave. In addition about half of the American Indian participants had left by September, and two groups went home during the class session. Many who still worked in the Anthropology Department or the Indian School refused to assist him, because they were not to be compensated for their time and demonstrations. Other groups agreed, because they apparently found the young women students quite entertaining themselves. According to one newspaper source, Geronimo, the Chiricahua Apache leader who was demonstrating in the Indian School, befriended one woman who was able to talk to him in Spanish. As always, Geronimo controlled his interactions with visitors. He would only help Starr late in the afternoon after he had finished his regular duties, not according to Starr's preplanned schedule, which had to be changed. Geronimo helped several women with their independent projects, telling them stories and being interviewed almost every afternoon for three weeks (Geronimo Thrilled 1904; Pretty U of C Co-Ed 1904). Some Native demonstrators apparently enjoyed the students' visits. Richard Spamer, who worked for Tobin's Cliff Dweller concession, wrote Starr following his lecture to thank him for his visit: "Your visit and that of your class will be remembered by all the Cliff Dwellers as one of the brightest particular things that happened to us during the progress of the Louisiana Purchase Exposition" (Spamer 1904). Several students spent hours interviewing these men and women, too.

To further make ends meet Starr held special tours of select Native groups, especially those on The Pike and in the foreign pavilions, in the afternoons, charging $2 per tour. Sometimes students would come with him rather than pursue their independent studies; in this case they were to take extra notes. These tours generated a good deal of cash and also press coverage, including a front-page article in the *St. Louis Post-Dispatch* (1904a). Starr also gave impromptu free lectures at the Ainu village that were technically part of the college series but open to everyone and as many paid lectures and tours as the market would bear.

Students worked hard during the field school. They were expected to acquire information and describe it but not to formulate research questions or interpret the results of their inquiries. They were to record what people believed and did but not formulate their own theories as to why. Starr expected them to keep detailed records during their individual afternoon sessions with Natives as well as meticulous notes on his lectures. The students were to be systematic; if they focused on religion in their interviews with one group, they were required to interview all groups about religion. Students were to visit all groups though they could spend more time with one or two, hence the intensive late-afternoon sessions. But the emphasis was on comprehensive comparisons, very much in keeping with the nineteenth-century museum curator's methodological approach to cultural diversity (see Goldenweiser 1940; Hallowell 1960). While Starr had originally wanted the students to learn how to take anthropometric measurements and photographs, he decided not to make this part of the curriculum so as to avoid interfering with the experiments being conducted by the Department of Anthropology's anthropometry and psychometry laboratories (Parezo and Fowler in press). Starr also decided it might be inappropriate for young women to measure adult Native men but it was acceptable for young men to measure adult Native women.

At the end of three intensive weeks, the students turned in their class work and left. Two women, Laura E. W. Benedict and Jeannette Obenchain (1904), wrote a newspaper article about what they had seen during their classes. What impressed them most were the tipis, the Acoma potters, and the fact that Starr had told them not to ask the Indians their names when they interviewed them because it was disrespectful. Obviously Starr had given his students some information on protocol and how to act with the different groups of Natives. What the others learned is unknown, for Starr did not keep their journals or essays, just snippets of notes from his lectures.

When the class ended on September 21, however, Starr graded the papers, turned in his grades, and left for Mexico two days later. He did not return to St. Louis, a city he did not like. He had had enough of the exposition as well. The LPEC did recognize his efforts. He received his own grand prize for his work with the Ainus and a gold medal for his exhibit, "Photographs of Ethnic Types"; however, the University of Chicago, not Starr, received the award for the field school (Louisiana Purchase 1905; McGee 1905).

Working for McGee and the LPEC turned out to be a very expensive venture for Starr because, due to the lateness of the Ainu expedition, he missed the university's spring semester and had to take an unpaid leave of absence. Since the number of students had been low, he barely broke even. Starr even requested that McGee pay him for some of his extraordinary expenses during the voyage to Japan out of his own pocket, which McGee did at the end of the exposition.

Ethnographic Field Schools after St. Louis

Starr went his own way and did not repeat the field-school experience, nor did he teach any other "methods" courses. He continued to go to the field by himself, without undergraduate or graduate students, and had little interaction with students outside the classroom. He lost track of McGee as McGee moved into environmental management. While Starr continued to give public lectures and teach at the University of Chicago, he was ever the loner, increasingly marginalized from the rest of the anthropological establishment. He became more dogmatic about his evolutionary paradigm as anthropology moved beyond it and his teaching topics. He would teach anthropology his way and did so until his retirement in 1923.

Anthropology, however, was still concerned with proving its scientific validity, and that meant the way in which students became professionals and the knowledge and skills they had to master as part of the process. Neither Starr nor McGee (who died in 1909) was involved with a group of anthropologists, under the direction of Franz Boas, who discussed teaching methods in 1916 (Boas 1919:41; Miller 1978:57). The results of this meeting were interesting. College teaching in anthropology should train "the mind to think clearly in relation to the forms of our cultural life," to broaden outlooks and "increase the power of objective interpretation of our own cultural attitudes." As teachers of progressive, humanistic, and philosophical courses of study, not only scientific endeavor, professors should convey the basics of an enlightened anthropological viewpoint that bridged phenomena and demonstrated its perspective. These values were as important as subject matter. For introductory courses, "little attention can and should be given to the details of methods of research. Only the most general principles of procedures can be outlined." For undergraduates, learning about individual cultures and the comparative study of traits, followed by advanced work on specialized topics, should guide the curriculum. To prepare advanced undergraduate and graduate students to carry out independent research, departments needed a biological laboratory with anatomical collections and biometric equipment, a connection to a museum with archaeological and ethnographic collections, and the ability to provide "opportunities for research work in social groups of varying types," especially "for observation of children, among various social groups of our own communities, and in primitive society" (Boas 1919:48). No one suggested a gathering or even a partial gathering of all races in one place or an attempt by students to understand multiple cultures and multiple peoples in a single three-week period. The model of the 1904 Louisiana Purchase Exposition was never seriously considered again. The note taking and systematic record keeping that Starr taught were picked up in the laboratory setting. What was not replicated was interviewing and observational skills.

Unfortunately for ethnography, the sink-or-swim approach became institutionalized at this meeting. Reminiscences of Americanist anthropologists describe having to figure out how to undertake ethnographic work. Ruth Bunzel, for example, stated that she "had about four or five weeks to become an anthropologist and plan a project" when she first went to Zuni at Boas and Elsie Clews Parson's urging (Bunzel 1985). Bunzel recalled: "I was really alone in a big sea and I had to swim. I assumed that the Zuni artists were not going to be any more articulate about what they were trying to do than the poets and painters I had met in Greenwich Village, and that direct questioning would get me nowhere" (Mead 1959a:34). With the encouragement of Boas and Ruth Benedict, Bunzel developed innovative methods, perspectives, and techniques, using photographs to elicit aesthetic criticism along with personal instruction. She had potters train her and used gendered participant observation and intentional problem solving (making mistakes in constructing and decorating problems to see how they were corrected). Bunzel was apprehensive during the train ride to Zuni because her methods were untested. Luckily, they were appropriate and effective.

Archaeology had developed a well-established system of field schools by the 1930s. Every issue of the *American Anthropologist* and later the *Anthropology Newsletter* and the *SAA Bulletin* listed training opportunities for students in which they could gain firsthand experience in survey, excavation, and analytic techniques around the world. Many universities even had training requirements for their students, and having attended a field school became a prerequisite for admittance to graduate schools. It is difficult to find evidence of undergraduate methodology training courses in ethnography, ethnology, or cultural anthropology in the 1930s through the 1950s, although it was common for individual instructors to offer some discussion of methods. Instead, ethnographic methodology techniques were taught—if they were taught at all—in a few field schools such as that of the Laboratory of Anthropology in the 1930s or those of Harvard in the 1950s and 1960s and Northwestern in the 1970s and 1980s at the graduate level (Fowler and Hardesty 1994:12). More recently, in the early 1990s, the National Science Foundation–supported training workshops that specialized in the basics of specific qualitative and quantitative techniques. The earlier field schools were ethnographically specific and did not attempt comparative ethnology among several cultures, certainly nothing like that undertaken by Starr. There are today few methodology training classes even at major research universities, with the exception of medical anthropology and applied or developmental anthropology programs. Students may be given an observational, participatory, or interviewing exercise in a class, but generally the emphasis is on transmitting theoretical and substantive information based on earlier ethnographers' research. Even cultural anthropology graduate students often need to go outside their departments for formal training in qualitative or quantitative methods. Most methodological advice in cultural anthropology is channeled through informal storytelling sessions.

The perennial question of course still remains today: How much actual training does a student need for general knowledge and understanding of anthropology and how should advanced students who intend careers obtain firsthand experience? While cultural anthropology (ethnography and ethnology) is an eclectic, situationally specific, and, almost by definition, multidisciplinary undertaking, there are still commonalities that apply cross-culturally and a vast array of methods and techniques that neophytes do not need to reinvent through trial and error. But few departments with large graduate programs seem to require their ethnography students to attend such a field school, and the situation is even worse in cultural studies departments. In many instances students are told to learn the language, ask questions, become part of the community, observe, obtain insights into how the culture works, come home, and write up the results. With a bit of luck a novice will receive sage advice from an advisor in the form of tales of his/her fieldwork adventures or be sent to sociology, psychology, or economics departments to learn statistics or survey methods.

At the outset we noted that Kroeber (1955:1) told Isabel Kelly that doing ethnography is an art and that it was hard to be told simply to go and do research alone, without guidance. He also said that when he sent Kelly to Surprise Valley, "I may have been rough on you at first, but so was Boas [in 1900] rough on me." After writing that ethnography is "an art that cannot be formally taught," Kroeber went on to qualify this, saying that "the younger person can learn a good deal by observing the more experienced one in practice." As in 1900 and 1928, so too is this the case in 2006.

Notes

Archival materials for this paper come from the Missouri Historical Society; the Field Museum, Department of Anthropology; the W. J. McGee Papers, Library of Congress; and the Frederick Starr Papers, University of Chicago Library, Special Collections. We would especially like to thank the staff of the University of California Library for their biographical sketch of Starr and their meticulous finding aid. We would also like to thank Regna Darnell and Sydel Silverman for the many hours they have spent reading this and other manuscripts on the exposition.

1. See Holmes and Mason (1902) for an example of museum anthropology's early solution to this problem. Field manuals and collection guides informed nonprofessionals how to ensure that all important information was collected about material culture to aid in artifact identification and taxonomy construction. These field manuals also contained advice and guidelines on how to collect critical ethnographic information on a society.

2. Ethnography could potentially undermine proponents' arguments that the discipline had professional authority, measures of competence that could be met only through professional training (not simply by apprenticeships like those in trades), associations and journals, and esoteric knowledge as well as arguments that its subject matter and paradigm were critical components of a liberal arts education.

3. In contemporary terminology this included anthropometry and the systematic observation and classification of somatic characteristics as well as the documentation of social and cultural activities such as the production of tools or art. McGee considered adding two sections to the department, Research and Conservation and University Instruction, which would be run by a research or educational institution and would not draw on exposition funds, because "neither research (with attendant conservation) nor regular instruction are germane to the work of expositions" (McGee no date). However, McGee firmly believed that the assemblage of Native peoples provided a rare opportunity for research and education beyond the questions that casual visitors posed to demonstrators. Anthropology could not and must not miss this golden opportunity.

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4. *Presentist History as a Means to Overturn Qualified Authority*

A (False) Warrant for a New Archaeology in the 1960s and 1970s

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Studies of the history of archaeology are now fairly common in the literature. These range in length and coverage from books considering the entire world (e.g., Trigger 1989) to brief articles covering a limited topic (e.g., Lyman et al. 1998). These pieces have diverse aims, but they all tend to be studies aimed minimally at revealing aspects of disciplinary history, or historicism (Stocking 1965). With increased scholarly effort and attention devoted to the history of archaeology, it is perhaps not surprising that instances of "presentist" history are identified (e.g., Trigger 1998). Anthropological historian George W. Stocking (1965), inspired by historian Herbert Butterfield, described presentism as analyzing historical events and phenomena in terms of the present, typically for purposes of ratifying the goals of the present and showing more or less continuous disciplinary progress toward attaining those goals. Although not without value (Hull 1979), the standard implication of presentist history is that it is biased toward favoring, even justifying, the present. But presentist history can also be used in a rather different way than arguing for continuity between past and present yet still serve the ends of the present. Presentist history can be used to vilify the past as a service to modern ends.

In this paper I describe an example of writing history that well illustrates a kind of disjunctive presentist history written as a warrant for (1) abandoning an old approach to an old set of questions and (2) adopting a new approach to a new set of problems and a new set of analytical goals. The example derives from the North American emergence during the 1960s of what is known variously as the "new" or "processual" archaeology (Willey and Sabloff 1993). The portion of this historical emergence focused on here concerns the notion—now accepted as a myth in many sciences—that scientists are sufficiently objective that they "never permit their judgments to be affected by authority [such that] the reputation of a scientist making a given claim is irrelevant to the validity of the claim" (Woodward and Goodstein 1996:480).

In his now classic book chapter "Archeological Perspectives," Lewis Binford (1968a) argued for a new means of evaluating the correctness of archaeological knowledge that would be better than what he alleged was the old means of evaluation. According to Binford, the latter involved "passing personal judgment on the personal qualifications of the person putting forth the propositions" (1968a:17). Binford characterized this supposedly traditional evaluative criterion as follows: "the degree to which we might have confidence in the professional competence and intellectual honesty of the archeologist advancing interpretations (see Thompson 1956:33)" (1968a:16). There is no page 33 in Raymond Thompson's 1956 paper; that article spans pages 327 through 332. Given what Thompson (1956) says on those pages, I suspect the page to which Binford (1968a) referred is actually page 331, where Thompson (1956) introduced what he termed "the subjective element in archaeological inference," described by him as an archaeologist's "unique combination of interpretive skills [and his/her] professional competence [and] intellectual honesty." Thompson (1958:8) repeated this definition of the subjective element verbatim two years later. Binford (1987:394) cited Thompson's (1956) page 331 in a later publication, where he repeated Thompson's subjective criterion.

Thompson's (1956, 1958) characterization of how archaeological knowledge should be evaluated became a warrant for the processual archaeologists of the late 1960s and 1970s, especially their advocation of a deductive approach to hypothesis testing (Binford 1968b) and the use of nomothetic laws of culture processes (e.g., Watson et al. 1971) as the bases of knowledge claims. Use of the new warrant demanded that the old warrant for, or means of evaluating, knowledge claims be shown as significantly flawed. Binford therefore repeated that, following Thompson and evaluating "reconstructions or interpretations by evaluating the competence of the person who is proposing the reconstruction [was] scarcely sound scientific procedure" (1968b:270).

In the 1960s the word *science* was thought to denote an objective, impartial field of inquiry—the facts or data would indicate the correct answer to a research question, if both the data and the question were handled in particular ways—and processual archaeologists in the 1960s and 1970s desperately wanted to be scientists. Thus, processualist James Hill stated that the testing of hypotheses "obviates" the necessity of relying "on the expertise of the archaeologist in evaluating the validity of an inference" (1970:51). Two years later Hill was not so direct, but the implication was still clear, when he wrote: "If we are well trained, experienced and perceptive, we should, it is said, be able to recognize the inferential possibilities that the artifacts and associations of artifacts have (Thompson 1958)" (1972:64–65). Like Binford, Hill found that warrant for knowledge claims to be flawed.

Looking back in time, processual archaeologist Charles Redman stated that the new archaeology's "formation was to combat the acceptance of plausible stories as the truth, so long as they were put forward by distinguished scholars" (1991:301). According to Redman, the new warrant for knowledge claims "rejected arguments based on authority alone, thereby providing a means for junior people to assail the establishment on an equal footing" (1991:296). This sounds like an excellent reason for a new, processual archaeology, especially if one was a new, young professional as yet without a reputation. Rejection of authority made for a level playing field; there was no home-field advantage for those who had simply been around longer and who had years of experience (of whatever kind) and various qualifications (whatever they might be).

Processual archaeologist Fred Plog characterized the archaeology of "the 1950s and early 1960s" as follows: "The perceived view [in the 1950s was] that 'truth' was measured in direct proportion to the consensus of professionals, that good archaeology was what made archaeologists happy. That a few individuals chose to state this conclusion in print poorly reflects the extent to which the attitude was pervasive. . . . Underlying this belief is, of course, a basic commitment to ad hominem argument—that whoever makes the argument is more important than the logical and empirical justification for it—that politics supersedes reason" (1982:26). The reference to "what made archaeologists happy" came from Albert Spaulding's statement "[that] truth is to be determined by some sort of polling of archaeologists, that productivity is doing what other archaeologists do, and that the only purpose of archaeology is to make archaeologists happy" (1953a:590). Binford (1968a:27) had quoted this statement at the end of his conclusions to the chapter in which he introduced Thompson's subjective evaluation criterion to processual archaeologists. No processualist of whom I am aware ever compiled evidence that Thompson's criterion was indeed used. Instead, once Binford highlighted it and implied that it had been used, other processualists accepted it as the way things had been. The only individual they identified as having explicitly stated the qualified authority criterion is Thompson.

In this paper I seek answers to two historical questions. First, did Thompson (1956) empirically demonstrate that what he said was true; did he in fact show that what I will call the "qualified authority criterion" was used? And second, did archaeologists prior to about 1960 actually evaluate results or knowledge claims concerning prehistory on the basis of the qualifications and authority of the individual(s) who produced them? I begin with a brief review of what Thompson (1956, 1958) said and did, seeking not universal empirical support for his assertion regarding the role of the qualifications of an archaeologist when it came to evaluating the work of that individual, but support of any kind. I then turn to a consideration of two kinds of evidence reflecting how archaeologists actually evaluated one another's analytical results and knowledge claims. The second of these could easily involve writing a book, but that is unnecessary to show that in fact archaeologists prior to the late 1960s did not use the qualified authority criterion when evaluating the archaeological knowledge claims of someone else. I show first that young newcomers to the profession actually took on qualified authorities in the 1950s, contrary to the implications of the statements made by new archaeologists of the 1960s that the playing field was unlevel and gave experience a decided advantage. Second, I describe a sample of book reviews published in *American Antiquity* during the 1950s and 1960s and show that no one said something like "Professor X is unqualified to make the interpretations that she or he does. The professor cannot be trusted given her or his lack of training and inexperience." Instead, criticisms were of the data used, the methods employed, or the conclusions reached in the books that were reviewed rather than of the author or the author's qualifications or lack thereof.

What Did Raymond Thompson (1956) Say?

Reid and Whittlesey recently stated that Thompson (1956, 1958) "codified how most archaeologists [in the 1950s] evaluated statements made about the past—they evaluated the archaeologist. The evaluation criteria emphasized academic training, background, and experience—essentially a scholar's pedigree and reputation. This seemingly elitist model held no appeal for the [new] archaeological scientists and intellectual populists of the 1960s and 1970s, who used it as a principal foil for promoting objective means of evaluating knowledge claims" (2005:62). Did Thompson indeed codify procedures for evaluating knowledge claims?

There is no disputing that Thompson said knowledge of the past based on archaeological evidence should be evaluated on the basis of the archaeologist's "combination of interpretive skills and his [*sic*] professional competence and intellectual honesty." But interestingly, Thompson did not list any examples of this procedure to help make his case. Further, and overlooked by the processualists, he also noted that "intellectual ability and integrity are not the only variables in an investigator's approach to a problem of archaeological reconstruction. These native qualities cannot be properly exploited without a rich background in anthropological theory and fact and a reasonable amount of familiarity with archaeological materials" (Thompson 1956:332). This is not advocacy of the qualified authority criterion; it is common sense, and it is good sense. For example, it is doubtful that the editor of, say, *American Antiquity* and a set of reviewers would not raise an eyebrow or two when reviewing a manuscript on an archaeological topic authored by a professional chef, a professional football player, a professional engineer, or a professional taxi driver, particularly if they had no professional training in archaeology. Some training in modern anthropology and archaeology is a necessity if one is to do archaeological research, particularly that which is thought to be "modern" at the time that it is done (Woodward and Goodstein 1996). The same can be said for virtually any profession, academic or not. I am a professional archaeologist, not a chef or engineer; I would not attempt to build either a soufflé or a bridge.

Accepting the preceding argument, it is at least somewhat ironic that all those processual archaeologists cited above who find fault with the qualified authority criterion had, at the time they were writing, doctorates in anthropology and were known professionally as archaeologists. Thus the case could be made that there is at least some degree of truth to what Thompson said. His claim was being evaluated by professionals in his own field, though many had fewer qualifications and less experience than Thompson. I suspect that all of us would be at least a bit more skeptical of the archaeological knowledge claims of someone who has no professional training in anthropology and archaeology than we would be of someone who has a college degree in either or both of those disciplines were that individual to submit an article to *American Antiquity*. Otherwise, why do we demand that our students go through a decade or more of college-level course work before we turn them loose in the professional job market with an advanced college degree?

How Did Archaeologists Evaluate Results Prior to 1960?

The year before Thompson (1956) published his paper, Betty Meggers explicitly stated that to dismiss an archaeological interpretation as "merely prejudice on the part of the writer [of the interpretation] would indeed be unscientific," and she urged that we "accept or reject the interpretations on their own merits" (1955:116). Here was precisely the argument that processualists would use to characterize their preferred evaluation criterion and to highlight the alleged flaws of Thompson's qualified authority criterion (Binford 1968b; Hill 1970; Redman 1991). Meggers used physics as a model of an archetypical science and argued that interpretations (knowledge claims) had to be tested and laws established if archaeology was to measure up to the (or, at least her) standards of science. Between about 1915 and 1955, the most frequent, but not the only, kind of result that archaeologists produced concerned the chronological ordering of archaeological materials. Individuals with varying qualifications (amounts of professional training) and experience (years of field and laboratory work) within the discipline argued, sometimes at great length, about whether a chronological interpretation was correct or incorrect. These arguments did not concern the qualifications of the individual building the chronology; they concerned the quality or amount of data, the chronometric methods used, and the results obtained. In this section I present two kinds of data to show that these were precisely the sorts of criteria archaeologists used to evaluate each other's scholarly efforts rather than the criterion of each other's qualifications and expertise. First, selected evaluations of one archaeologist's work by another archaeologist with similar qualifications, or with dissimilar qualifications, are summarized. Second, the contents of 454 book reviews are summarized.

Evaluations of Archaeological Work by Archaeologists

Evaluations of archaeological research were common in the early days of culture history. These were not always book reviews; sometimes they were articles. Other times they were extended reviews of articles thought to be of particular significance. In those early days even short articles received extended comment, as when Carl Guthe (1936) reviewed Thorne Deuel's (1935) article in *American Anthropologist*, "Basic Cultures of the Mississippi Valley." When he penned his review, Guthe was chairman of the Committee on State Archaeological Surveys of the National Research Council. He had earned his doctorate in 1917 from Harvard, had worked in several geographic areas, and by 1936 had compiled an impressive list of publications (Griffin and Jones 1976). Deuel received his doctorate in 1935 from the University of Michigan; his *American Anthropologist* article was an extract from his dissertation, was one of his few total professional publications, and was based on the Pictorial Survey of Mississippi Valley Archaeology initiated by the University of Chicago's Fay-Cooper Cole (Lyman and O'Brien 2003). Guthe's review could have been an example of a qualified authority shooting down a youngster with minimal expertise to speak of, but that is not at all what Guthe did. In fact Guthe noted that Deuel's experience with the Pictorial Survey "made [Deuel] especially well qualified to set up [the] hypothesis [that he does]" (1926: 249). This last statement might lend weak support to the qualifications aspect of Thompson's subjective criterion of evaluation, but it is a stretch to argue that Guthe was endorsing the view that only those with similar qualifications could evaluate what Deuel had said. What Guthe did question was the method that Deuel used to "set up" his hypothesis. And note as well that the word *hypothesis* is found in Guthe's review; that word is fairly common but not ubiquitous in the literature of the 1950s, though its meaning is not the narrow, rigid one associated with it by the processual archaeologists during the 1960s and 1970s (Binford 1968b; Watson et al. 1971).

An example of a young professional with a few qualifications and some experience but who was still in graduate school, yet who took on an establishment figure, is James Ford (1940), a relatively new recipient of his master's degree (Ford 1938) from the University of Michigan when he reviewed Harold Colton and Lyndon Hargrave's (1937) *Handbook of Northern Arizona Pottery Wares*. Colton was a professional biologist and had been a professor of biology when he retired from academia to pursue archaeology. Hargrave was an avocational archaeologist and ornithologist. Together these two men basically established the archaeology program at the Museum of Northern Arizona in Flagstaff. They had been working about the same length of time that Ford had—since the early 1930s—and all three had published various articles, monographs, and the like. Ford (1938) made it clear that he did not accept Colton and Hargrave's conclusions, because the latter had not convinced Ford that their artifact types actually indicated genetic-like continuity between types within a chronological series.

The middle of the twentieth century was a time when many individuals had similar educational backgrounds and qualifications, and so it was not unusual to find them critiquing and evaluating each other's efforts. An example of two individuals with similar qualifications and major professional stature disagreeing is found in the well-known debate between Ford and Albert Spaulding over how to classify artifacts (Ford 1954a, 1954b, 1954c; Spaulding 1953a, 1953b, 1954a, 1954b). This example, plus Ford's review of Colton and Hargrave (1937), illustrates the fact that archaeologists did not just lie back and accept without question what anyone, qualified authority or not, had to say. The archaeologists of the 1940s and 1950s were as argumentative as those of the 1970s. And, like archaeologists today, those working in the 1940s and 1950s were unafraid to criticize others of higher status or with more qualifications and experience.

An excellent example of a qualified authority being criticized by someone who was not a qualified authority but instead a professionally young student is found in James Bennyhoff's (1952) critique of Ford's (1949) proposed chronological sequence of pottery types of the Viru Valley in Peru. Bennyhoff was criticizing the published version of Ford's dissertation, but by 1949 Ford had at least 17 distinct publications (O'Brien and Lyman 1998). He had published articles and book reviews in *American Antiquity*, articles in *American Anthropologist*, and monographs in *Memoirs of the Society for American Archaeology*, in *Yale Publications in Anthropology*, and in the American Museum of Natural History's *Anthropological Papers*. Ford had worked in South America, the Southeast of North America, and the Arctic. His first fieldwork had been done in the early 1930s; he then organized and directed a large WPA project as well as collaborating with Phil Phillips and James Griffin on a major project in the lower Mississippi River valley. By 1952 Ford was unquestionably a "qualified authority." Bennyhoff would not earn his doctorate until 1961; his critique of Ford was only his fifth professional publication (Hughes 1994). He had worked nearly exclusively in California; his first field experience was in 1946. Bennyhoff would go on to become a well-known archaeologist, but in 1952 he was hardly a qualified authority, especially when compared to Ford at that time. Even so, Bennyhoff's criticisms were published in *American Antiquity*. Importantly, those criticisms were not of Jim Ford the man or his qualifications but of his method and his results. At the time manuscripts submitted to *American Antiquity* were not subjected to peer review; the editor alone either accepted or rejected them. In 1951 and 1952 the editor was Jesse Jennings, who had received his doctorate in anthropology from the University of Chicago in 1943 (Aikens 1997). Jennings (1994:185–86) mentions his term as editor in his autobiography, but he does not reveal the criteria he used to accept or reject manuscripts.

Finally, I note that Reid and Whittlesey (2005) recently reported that fieldwork summarized in a dissertation published in two parts in 1954 and 1955 resolved a debate that had been going on for 20 years when it was published. Were Thompson's assessment correct, how could an archaeologist with a brand new doctoral degree—and the limited experience, qualifications, and reputation this typically entails—resolve a controversy between the likes of Paul S. Martin, Alfred V. Kidder, Eric Reed, and other then-icons of Southwest archaeology? I suggest that any holder of a new doctorate of archaeology could not resolve such a controversy were Thompson's description literally accurate.

There is no evidence in the articles I have examined to suggest that archaeologists of the 1930s, 1940s, and 1950s evaluated knowledge claims of their colleagues based on experience, qualifications, or intellectual honesty. But for the sake of thoroughness, it is worth noting that Paul

Martin, who was so open to and so fervently endorsed the New Archaeology (Martin 1971; see Longacre 2000), was against allowing students or, apparently, even newly minted Ph.D.'s to attend the early Pecos Conferences (Woodbury 1993:174, 241, 433). This might have been a manifestation of the qualified authority criterion, but I doubt it. I suspect that he did not want to be bothered by neophytes with unending questions while visiting with his friends and colleagues and getting some new idea forming and sharing done.

Some might argue that a particular form of the qualified authority criterion was implied by archaeologist Walter Taylor (1948:155), who remarked that after the truth or falsity of a statement had been judged on the basis of facts or data not used to make the original statement, one could then examine the "background" of the author of the original statement to help determine why that statement was true or false. But note that this characterization of a criterion by which knowledge claims are evaluated places priority on the evidence for knowledge claims. The claimant's qualifications — interpreting Taylor's remarks broadly — are of secondary importance.

Book Reviews

I was unable to find any evidence of the qualified authority criterion being used in evaluations of particular pieces of archaeological research. My search of the literature was not exhaustive, so perhaps an example or two was missed. But that seems unlikely if not improbable if the qualified authority criterion was indeed as frequently used as implied by the processual archaeologists or was as "pervasive" as Plog suggests it was. Plog (1982) states that "a few individuals" published statements on the qualified authority criterion, but I was unable to find any such statements other than Thompson's (1956, 1958). Was it perhaps used in another context?

One suspects that if that criterion was used much at all, it should show up with some regularity in the book reviews published in *American Antiquity*. That seems to be a perfect venue for using the qualified authority criterion. Given the lack of use of the criterion in simple evaluations of the work of others, did archaeologists say in printed book reviews that they did not believe a particular archaeological conclusion because the author of the conclusion was unqualified, inexperienced, or untrustworthy? To answer this question, I examined all book reviews published in eight volumes of *American Antiquity*. The examined volumes (19, 20, 22, 23, 24, 25, 28, and 32) were published between 1953 and 1967; six volumes were published in the 1950s and two in the 1960s. I chose the volumes based on hard copies I could access easily rather than probabilistically, with the only restriction being that they were published between 1950 and 1967 inclusively.

There are 454 distinct reviews in the eight *American Antiquity* volumes; the number of books, monographs, and articles reviewed is more difficult to determine, because some items were reviewed more than once and chapters in books sometimes received individual reviews. What is more important is that the reviews were authored by 250 individuals, and of those, 106 individuals wrote more than one review. Interestingly, 106 individuals wrote 310 of the total 454 reviews. In other words about 52 percent of the reviewers wrote about 68 percent of the reviews. This suggests that certain individuals might have been chosen because they were individuals deemed by the book-review editor to be qualified to review a particular volume. Several years ago, I examined all book reviews published between 1974 and 1991 in *American Antiquity* and in *American Anthropologist* and found that "over half the book reviews [N = 1698] have been written by about one-fourth of the reviewers" and on that basis concluded that there were "shamans of the book review" (Lyman 1994:16). These were individuals whose "skills and knowledge" made them especially qualified to review books on particular topics. I suggested that having such shamans made sense, because it lent efficiency to the review process and provided readers of book reviews with trustworthy (because of their qualifications and experience) opinions on the worth (financial and scholastic) of books. We may be seeing the qualified authority criterion at work in both the 1950-1967 sample of reviews and the 1974–1991 sample. Even if this is what we are indeed seeing in these data, it does not follow that the authors of the reviews used the qualified authority criterion in their reviews. Indeed, they used other criteria, as many quotations from the 1950–67 sample of reviews illustrate.

There are numerous allusions to the expertise or knowledge of an individual author in the book reviews. For example, because of its "importance," James Griffin's (1952) edited volume *Archeology of Eastern United States* received "more than the usual casual review" (Ford 1953: 172). Ford had "qualified specialists" review particular chapters, such that chapters on biological ethnicity were reviewed by a biological anthropologist, the chapter on the Northeast was reviewed by an individual who had worked there, and so on. This was a reasonable approach given that the chapters in the book were all very data rich; who better to evaluate such contributions than someone familiar with the "local" data? Such an individual would know if an important site had been overlooked. Another allusion to individual qualifications is found in Spaulding's indication that the chapter in Griffin's volume that he reviewed "appears to be authoritative" (1955:289). John Cotter (1954:182) began his review of a volume by William Webb with the comment that Webb "is especially qualified by his personal observations of Indian life . . . and his . . . extensive archaeological investigations" to employ the direct historic approach as his interpretive protocol. John Longyear (1955:296) prefaced his thoughts about a contribution by A. V. Kidder with the comment that Kidder is "outstanding in his field." Irving Rouse (1955:297) referred to the "authoritative" review of literature provided by a particular archaeologist. Robert Heizer (1958:201, 203) characterized a book as a "scholarly piece of work" and also indicated that "only [this particular author] could have written this book" given his personality and his knowledge.

In spite of such accolades, most reviewers did not hesitate to question at least something written by the author whose work they were reviewing. For example, Robert Ascher (1963) was not afraid to point out many flaws in Ford's (1962) manual explaining the history and workings of seriation. Ascher stated that various assertions made by Ford regarding culture change were used as conclusions that informed the seriation procedure but might or might not in fact be true; Ford's history of anthropology was "distorted"; and the "manual may challenge a potential user" (Ascher 1963:571). Jesse Jennings reported that one must take "on faith" certain conclusions presented in a volume he reviewed because "photographs did not reproduce well" and because of "deficiencies in reporting and the fact that many curious phenomena observed and mentioned were too incomplete for full interpretation" (Jennings 1962:105). Finally, in his review of Joseph Caldwell's (1958) important *Trend and Tradition in the Prehistory of the Eastern United States*, William Sears (1959:276) said: "As a qualified archaeologist, Caldwell's opinions deserve respect, but as far as this monograph is concerned they remain opinions." This is an explicit statement regarding an author's qualifications, but note that it does not mean the reviewer does not disagree with the author on other bases, particularly with respect to how the reported knowledge claims were derived.

Not all authors of reviewed books were qualified authorities, but they were not subjected to any harsher comments than anyone else. Frederick Johnson and John Miller, for example, do not question George Carter's qualifications, trustworthiness, or personality when evaluating the latter's book on the controversial Texas Street site; instead they note that such things as "the simple, well known, and rather fully developed and reasonably precise archaeological methods for identifying human occupation and industry seem to have been cynically ignored" (1958:209). John Goggin (1959) criticized an individual for making broad generalizations about pottery technology with obviously limited knowledge of the New World, including apparent ignorance of Shepard's (1956) important volume. When Robert Bell (1953:95) noted the value of a published "professional opinion" regarding the Spiro mound in Oklahoma, it was because the other papers in the volume he was reviewing were authored by avocational or amateur archaeologists. Similar remarks were made by others reviewing work by individuals who analyzed and interpreted data that were not collected using modern (1950s or 1960s) excavation procedures (e.g., Wray 1953). Finally, in his lengthy review of Walter Taylor's (1948) *A Study of Archeology*, Richard Woodbury never once said that Taylor had no business criticizing the likes of A. V. Kidder, Emil Haury, William Ritchie, James Griffin, or other such qualified authorities. Rather, Woodbury (1954:293) noted that Taylor says "much that is worth saying" but he says it in a "strangely patronizing" way.

In short, Volney Jones (1953:92) said it well when he suggested that in lieu of adequate data one could defend a conclusion by "citing selected 'authoritative sources,' " but he recommended against it and instead suggested that "the best scholarly efforts which can be expended" should be directed toward defending a conclusion. Better science makes for better conclusions. The 250 reviewers who authored the more than 450 reviews examined during the course of this study seem to have followed a similar guideline when writing their reviews. They seldom mentioned the qualifications of the author, but even when they did, and even when they noted that the author's qualifications demanded respect, the reviewers did not hesitate to point out what they took to be errors in data, analysis, or reasoning. I found virtually no hard evidence of the qualified authority criterion in action in the book reviews published in *American Antiquity* between 1950 and 1967.

Discussion

To this point, I have examined evidence concerning the qualified authority criterion. Thompson also mentioned "intellectual honesty," an item that Binford and other processual archaeologists mentioned but did not elaborate on. What did Thompson mean by intellectual honesty? Scientific ethics indicate that one scientist should not use another's ideas or data without due credit being given to the individual who first thought of the idea or generated the data. Failure to acknowledge the contributions of someone else can result in accusations of theft, plagiarism, or at least scholarly dishonesty. Ethical behavior in the sciences also requires that one not modify or edit data to fit preconceived notions. Data that have obviously been modified, or even those that simply seem too perfect for whatever reason, can also result in accusations, or at least suspicions, of dishonesty. The avoidance of such things is likely what Thompson meant when he referred to "intellectual honesty." This attribute of an individual's qualifications is also not mentioned in the literature or book reviews I have examined. What of the qualifications, experience, and expertise of individuals?

On the one hand, none of the data presented above support the hypothesis that the qualified authority criterion was used to evaluate archaeological knowledge claims prior to Binford's 1968 implication that it was used. But on the other hand, none of that data really falsify the hypothesis either. The latter is so in light of Plog's (1982) observation that few archaeologists mentioned the criterion even though most of them believed in it and may have even used it regularly (exactly where they used it is unclear). This is a rather strange position for a processual archaeologist to take, because processualists are positivists and empiricists. They have to have data to confirm or refute their hypotheses, and their hypotheses have to be testable (Binford 1968a, 1968b; Hill 1972; Plog 1982; Watson et al. 1971). By arguing that few archaeologists mentioned the qualified authority criterion but they all used it — to repeat Plog's words, "the attitude was pervasive" — Plog set up a hypothesis that is untestable. Are there, then, any data that might serve to justify or to refute the hypothesis? Indeed, there are some interesting data that might well reflect on the validity, or lack thereof, of Plog's hypothesis.

Irving Rouse indicated that Thompson's discussion of the subjective element "might well be made required reading for the next generation of archaeologists" (1959:286). As it turns out, at least two people apparently agreed with Rouse. Thompson's (1956) article was reprinted in two edited volumes of reprinted articles published in the early 1970s (Deetz 1971; Fagan 1970). Both volumes were meant to be used in the classroom as texts, suggesting that at least these two editors thought Thompson's discussion of how to evaluate archaeological knowledge claims was useful. Indeed, in one of those volumes the editor provided a rare statement of support for Thompson's view. Deetz noted that the "subjectivity of the archaeologists intrudes in both [inductive and deductive approaches]" (1971:148). Further, "while Thompson suggests that in the case of inductive archaeology, the work may be evaluated by our estimation of the integrity and skill of the archaeologist responsible, the results of an archaeologist working deductively also must be judged, at least in part, by the integrity of his hypothesis" (Deetz 1971:148–49). Thus, processualists discussed such things as "arguments of relevance" (e.g., Fritz 1972) and debated the exact protocol of deductively structured arguments and research designs (e.g., Hill 1972). Many processualists advocated use of the deductive protocol of science as described by Carl Hempel (1965, 1966), the favored philosopher of science of the new archaeologists of the 1960s and 1970s (O'Brien et al. 2005). Hempel's was the correct procedure, and one had to learn it to use it properly.

This is yet another strange position for those finding fault with the qualified authority criterion to take. The position simply means that one has to know how to use the procedure for it to work. The position is strange because the numerous articles and books produced by processual archaeologists lecturing each other on how to correctly use the procedure (and this was unclear) suggest (ironically) that they feared an unqualified novice would likely not use it properly. Even though this literature is critical of them, it comprises a thinly veiled acknowledgment of the importance of individual qualifications and expertise.

There is very little empirical evidence in the American archaeology literature for use of, or even subscription to, the qualified authority criterion as characterized by processual archaeologists. To be sure, Thompson (1956, 1958) described that criterion, but what seems to have escaped the notice of the processualists who picked up Thompson's banner merely in order to symbolically burn it is the fact that Thompson was suggesting a particular way to evaluate knowledge claims. He did not say that his qualified authority criterion was a statement of past practice; instead, he said that what he was offering was "a statement of the present aims of the discipline and the role which inference is expected to play in achieving those aims" (Thompson 1958:1). It is clear that he believed all archaeologists, past, present, and future, had to have a combination of training in the facts and theories of anthropology and archaeology, a familiarity with the material under study, and some intellectual capacity (Thompson 1956). This combination of attributes is, in Thompson's view, what makes knowledge claims individualistic and subjective. For example, no one will dispute that archaeologists study the past by analysis of artifacts. What, then, is an artifact? Definitions of this most basic subject phenomenon of archaeology vary remarkably; one need only examine the glossaries of the multiple introductory archaeology textbooks on the market. More impressively, perhaps, it is not unusual to find examples in which two or three archaeologists disagree on whether certain particular items are indeed artifacts. Part of the underlying cause of these differences resides in unique training and individualistic experiences. There are strengths and weaknesses to this fact, but they are beyond the scope of this chapter. What is important to realize here is that qualifications (formal training) and experience (fieldwork) can cause one to perfect a verbal definition of *artifact* and also the way in which one sorts, say, mere broken rocks from lithic artifacts.

Significantly, Thompson did not say that the qualified authority criterion was completely without weaknesses; he explicitly stated that the criterion was "certainly inadequate," and while he did not at that time know of a way to make it better, he did "hope for improvements in the methods of measuring the amount of faith in an individual's work" (1956: 331, 332). His 1956 discussion included a description of ways to strengthen analogical arguments, and in his 1958 discussion he outlined the importance of testing suspected relationships between a set of data and inferences derived from those data. Reading Thompson's discussion of these suggested procedures reminds me of an analytical protocol that 20 years later was used to construct what is called middle range theory (Binford 1977). I think it doubtful that any modern archaeologist would dispute that the modern construction of middle range theory requires an incredible depth and breadth of anthropological and archaeological knowledge (Arnold 2003; Binford 2001). That is, it requires experienced, qualified authorities. This point has been recognized in archaeology since Thompson first wrote about the subjective element.

Reid and Whittlesey recently suggested that all anthropologists should evaluate knowledge claims based on the claimant's experience and qualifications when they indicated that the nature of the discipline — "nurtured and sustained on the exotica of distant lands and peoples — justly accords a special authority to 'those who have been there' " (2005:212). More pertinent to archaeology in particular, Reid said it rather well when he observed that "the degree of perceptual distortion is inversely related to experience, to one's knowledge, implicit or explicit, of the [archaeological] record's formation. To the neophyte, much, if not all, of the [archaeological] record is a blur" (1985:16). I agree.

The preceding statements should not be taken as a sign of weakness of archaeology or even of an unlevel playing field. Qualified authorities are necessary in many fields of research. The reasons are that (1) the field's methods, theories, and data are too extensive to be mastered by all practitioners; (2) some methods, theories, or data may be so specialized as to preclude complete mastery by all workers in the field; and (3) general rules of method have thus far proved elusive. Further, actual cases show that even the most detailed written rules for carrying out a procedure — whether fieldwork, analysis, or interpretation — may not result in success; rather, someone with appropriate experiences and skills is often required in order to be successful (Woodward and Goodstein 1996:486). With respect to the data I summarize above regarding book reviews, it has been suggested that a reviewer may be professionally negligent for deferring to an author's reputation rather than considering in detail a manuscript's content. This is so because such deference may impede research creativity and originality (Chubin 1985). Fortunately for archaeology, none of the reviewers I studied were professionally negligent.

Conclusion

Interestingly, at about the same time that Binford (1968a, 1968b) was introducing the argument that the qualified authority criterion for evaluating archaeological knowledge claims was not scientific, K. C. Chang suggested that a " 'general anthropologist,' equally at home in all areas [of anthropology], is now generally regarded as a mythological hero" (1967: 227). Chang was arguing that it was virtually impossible to receive sufficient training in all four traditional subfields of anthropology to practice in each knowledgably, because of the previous several decades of rapid expansion in the number of methods and theories and growth in the amount of substantive data in each. When Chang was writing, an anthropologist with the depth and breadth of a Franz Boas or an Alfred Kroeber was unimaginable; specialization in some aspect of anthropology (or within archaeology) at that time included not just the geographic area where one worked, but such things as ceramic technology or ethnobotany. Multidisciplinary research teams were becoming *the* way to do archaeology (e.g., Taylor 1957). Such is explicit recognition of the scientific necessity of various kinds of qualified authorities. Why, then, would the processual archaeologists argue that the qualified authority criterion for evaluating archaeological knowledge was scientifically invalid?

If one wishes to cause a disciplinary revolution, to overthrow and replace a traditional paradigm, approach, or set of analytical goals, one strategy is to swamp the literature with exemplary applications of the new paradigm. Another, not necessarily mutually exclusive, strategy is to point out every real and also every possible or imaginable flaw in the traditional paradigm and to scrutinize each one, highlighting and perhaps elaborating at length on every nuance. Or one can simply identify a major, easily recognized and commonsensical flaw at every opportunity. With sufficient repetition of the identification, the traditional approach soon becomes unclean, untrustworthy, and little practiced (or at least less visible), regardless of the accuracy or validity of its characterization as flawed. Identification and repetition of the qualified authority criterion by processual archaeologists of the 1960s and 1970s comprises an example of just such an effort to overthrow and replace an alleged traditional archaeological practice with a new one. Both the old and the new practices involved a criterion by which the validity of a bit of claimed archaeological knowledge could be evaluated. The processual archaeologists—all with professional training but relatively limited experience—wanted a level playing field, one on which years of training and experience counted for nothing. They argued that the use of a particular procedure for gaining archaeological knowledge—deductive reasoning, particularly hypothesis testing and law building as described by Hempel—made all archaeologists equal and all knowledge claims subject to the same measure of validity, regardless of one's qualifications and experience.

The processualists used the supposed unscientific nature of the qualified authority criterion as a warrant for a new, allegedly more scientific criterion. This is a case of presentist history for one simple reason. Thompson (1956, 1958) is the only individual the new archaeologists cited as advocating the qualified authority criterion, despite the claim that the criterion was "pervasive." Thompson is also the only individual I am aware of who suggested that it be used (excluding Deetz), but even he indicated it was flawed and better criteria were desirable. No one among the 250 authors of the more than 450 book reviews and articles examined used the qualified authority criterion as a reason to accept without question what another author had written, nor was a lack of expertise or qualifications ever used to reject someone else's knowledge claim. The qualified authority criterion was cited time and again by processual archaeologists as a reason for abandoning an old approach and adopting a new one. The history described by the processualists was presentist because it was used in the service of the present; that history described a (false) warrant for a historical intradisciplinary conceptual disjunction.

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5. *"Pigs for Dance Songs"*

Reo Fortune's Empathetic Ethnography of the Arapesh Roads

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After Reo Fortune died in 1979, the ethnographic materials that remained in his possession were deposited by his niece and literary executor, Ann McLean, in the Alexander Turnbull Library in Wellington, New Zealand. Nearly 600 pages of these materials are directly concerned with the Mountain Arapesh people of Papua New Guinea, whom Fortune studied during his famous joint fieldwork with Margaret Mead in the early 1930s. Among these materials are some real treasures, including numerous contextualized translations of Arapesh *sakihas*, a now all but lost genre of richly allusive traditional speeches that, Fortune shows, were an important means of expressing and transmitting Arapesh morality at that time. But by far the largest part of Fortune's surviving Arapesh materials are notes and fragments toward what was apparently to be an ethnographic monograph on Arapesh society. Without a doubt, this manuscript was intended to stand in opposition to Mead's depiction of Arapesh culture in *Sex and Temperament*, with which Fortune vehemently disagreed (Fortune 1939, 1943; Roscoe 2003; Dobrin and Bashkow in prep), and possibly also in opposition to aspects of Mead's multivolume *The Mountain Arapesh* (1938, 1940, 1947, 1949). The manuscript includes sections with titles such as "Arapesh Religion," "Arapesh Tribal Character," and "Arapesh Ritual Idiom" as well as unlabeled fragments dealing with the interwoven topics of sorcery, warfare, and the system of "roads" (Arapesh sg. *yah*, pl. *yeh* or *yegwih*) along which people and items of value moved through Arapesh territory on its north-south axis (RFFP).

In this paper we focus on one reasonably coherent section of Fortune's archived Arapesh manuscript that deals in detail with the purchase of a dance complex along these roads, an event Mead referred to in her *Diary of Events* as the "Kobelen feast" and which is sometimes mentioned in Mead and Fortune's correspondence as the "(Dogur-)Kobelen show" (PDS; Mead 1947:337, 351, 359, 360; MMP, RF/MM, February 23, 1936

[S2:2]). We also consider the nearly 40 pages of transcribed and meticulously annotated abstracts of the speeches given at this event that are included in Fortune's field notes (KFS). The roads were both real physical paths that permitted travel beyond one's own locality and social pathways for interaction and exchange. The subject of the roads is of particular interest, because it is central to the exception Fortune took to Mead's interpretation of Arapesh culture as expressed in his 1939 paper "Arapesh Warfare," inasmuch as Fortune saw that the roads historically served to construct and organize interlocality competition and war, whereas Mead's interpretation emphasized their function as routes by which sorcerers traveled and culture diffused.[1] The significance of the roads in their disagreement is underscored by the attention Fortune gave in his unpublished manuscript materials to phenomena that depended on them in the stronger sense of being structured in terms of them, the main examples being sorcery, wife abduction, and warfare. Fortune repeatedly asserted in his copious letters to Mead—and subtextually implied in "Arapesh Warfare"—that Mead had insufficient experience with the roads to write about them with authority (e.g., MMP, S2:2; Dobrin and Bashkow in prep).

It is in a section of manuscript entitled "Pigs for Dance Songs" (PDS) that Fortune's understanding of the Arapesh roads is expressed in its most insightful, artful, and explicit form. Although the manuscript is unfinished and only portions of it survive in the archive, it has all the markings of a text Fortune composed for publication, and its extant segments provide a remarkably clear picture of the functioning of the Arapesh roads at the time of Fortune's fieldwork in 1931–32, contributing an important source of evidence for understanding the Arapesh roads as a social and cultural institution. Moreover, in its style, perspective, and voice "Pigs for Dance Songs" is highly revealing of Fortune's ethnographic approach. The manuscript describes a journey Fortune took along the roads, accompanying a formal party of Mountain Arapesh villagers who were gathering in Kobelen to buy the rights to a new dance complex called "Shenei."[2] In addition to showing the roads in action and bringing to light new features of the roads—such as their "telescoping" quality, which iconically realizes the intermediate social relationships linking distant localities—Fortune's text is remarkable for the way it narrates the journey, seamlessly interweaving his point of view as an outside ethnographic observer with a perspective empathetically aligned with a group of participants originating from a particular Arapesh locality. "Pigs for Dance Songs" illustrates vividly that Mead and Fortune's disagreement over the interpretation of Arapesh culture was not only, or even primarily, a matter of substance; there was a great gulf between them in terms of their experiences during their fieldwork and, above all, in terms of their respective intellectual temperaments.

The Arapesh Roads in Mead's Ethnography

The story of Mead and Fortune's last "professional partnership of field-work" is well known, from Mead's perspective at any rate, and we will summarize only the most pertinent points here (Mead 1972:189).[3] At the end of 1931 the two anthropologists took their already strained marriage to New Guinea to study the region's cultures and collect art and other artifacts, a project for which Mead had funding from the American Museum of Natural History. The couple soon settled in the mountaintop Arapesh village of Alitoa, near the New Guinea north coast. Because of the rough terrain and her weak ankle, Mead had to be carried up to the field site, and she was unable to travel beyond the village perimeter until she was finally carried out again eight months later (Mead 1940:337). Given that Mead was the fieldworker whose responsibility it was to study the culture, while Fortune studied the language (Mead 1972:226), this was certainly a less-than-ideal research arrangement, since the Arapesh were "on the roads for time equivalent to one year in three, or two years in five" making the "transition up and down roads . . . a very large part of native life" (PDS 84).[4] It thus fell to Fortune to do a considerable amount of traveling during this period: "The ethnologist domiciled amongst the mountain Arapesh . . . , in order to study the culture from all angles, similarly spent a large part of his time on the roads" (PDS 84). Fortune accompanied villagers to their distant gardens, attended intervillage gatherings throughout the region, managed his and Mead's supply stores on the coast, and made visits to neighboring areas to collect artifacts, survey the extent of the culture, and explore possible sites for subsequent research. Ensconced in their well-appointed village home, Mead could only receive Fortune's reports of his travels and incorporate them into her developing understanding of how the culture she was studying fit into the regional world outside the village.

Drawing on these reports, on the artifacts Fortune brought back from his travels, and on her own fieldwork among the Arapesh and subsequently among the Mundugumor and Tchambuli in the Sepik, Mead's major publications on the Mountain Arapesh (1935, 1938, 1940, 1947) describe them as a people situated within a larger "culture area" through which "material and non-material culture traits" were spread by diffusion (1938:151–52). The roads were of importance in Mead's account as the main avenues connecting Mountain Arapesh villages to the other parts of this area. They gave people access to imported necessities, items of value, and ritual complexes, and they provided a pathway for the transmission of objects such as stolen exuviae (body dirt, food leavings, and so

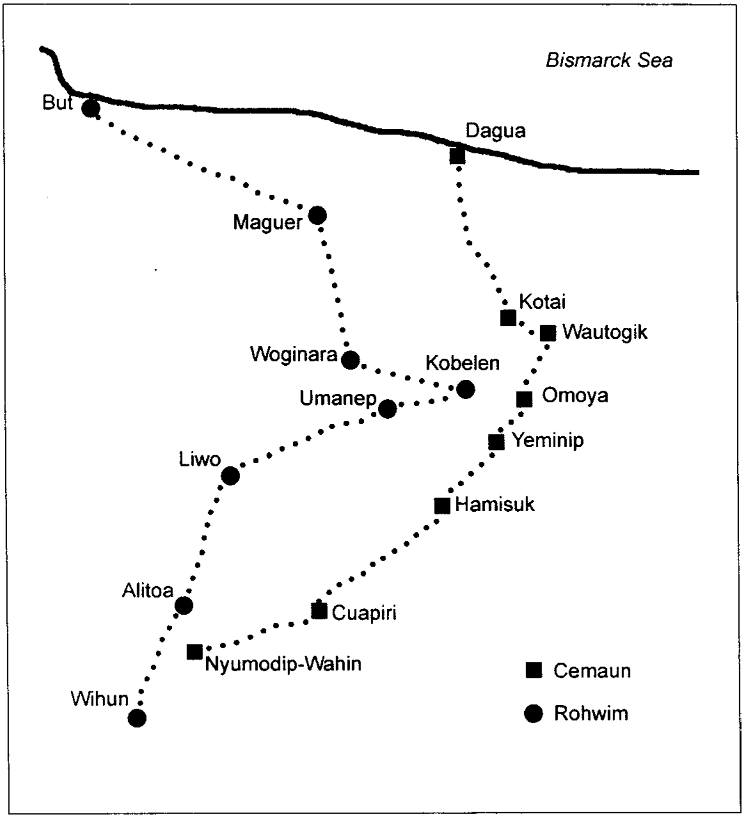

Fig. 1. Schematic diagram of the Cemaun (Shemaun) and Rohwim (Lahowhin) roads running across the Arapesh territory on its north-south axis. Note that not every historically attested locality is represented.

forth) and blackmail payments associated with sorcery. Two main roads traversed the region,[5] each running roughly north-south from the coast across a large swath of precipitous mountain lands and over the watershed of the Torricellis (Prince Alexander Mountains) to an inland piedmont, a region that Mead and Fortune referred to as "the Plains" (calling the people who lived there the "Plains Arapesh").[6] Alitoa is on the western or Rohwim Road; the eastern road is called Cemaun.[7]

In the model developed by Mead, the Mountain Arapesh were sandwiched between the Plains Arapesh to the south and the Beach Arapesh on the coast. The Plains Arapesh played a role in the regional economy as the source of shell rings and other culturally distinctive items and, above all, as death-dealing sorcerers who in their villages maintained inventories of their neighbors' exuviae that could be used to ensorcell them on others' demand. The Beach Arapesh, by contrast, were the region's link to the local coastal maritime trade, which provided access to the highly valued fashion and sophistication associated with distant others, including riverine people living inland along the Sepik River. The roads thus functioned as an arc connecting these three economically interdependent Arapesh groups.[8]

While the general cultural significance of the roads was as thoroughfares of "trade and diffusion" (Mead 1938:330), Mead recognized that they existed for individuals as exchange partnerships that were strung together, forming particular paths for interlocality relationships. (For a detailed discussion of Arapesh topographical, residential, and political units, see Roscoe 1994.) Each man thus had his own personal version of a road, consisting of the series of dwelling places belonging to his individual exchange partners, or *buanyim* (singular *buanyin*). In Mead's account these buanyim, which she glosses alternately as "trade friends" and "gift friends" (see, e.g., 1938:321–28), were "hereditary in the patrilineal line" (321). So a man could say, "This is my path. Along this path I *always* travel" (Mead 1938:322; cf. 1947:363). But in Mead's ethnography the personal mediation of these exchange pathways is not central for understanding the roads' functioning; indeed, it has very little consequence beyond determining the precise pathway that a boy will be shown ceremonially upon his initiation (Mead 1935:75–76, 1938:322). As we will see, a very different view of the roads emerges from Fortune's materials, which emphasize their competitive political dimension and the mobilization of chains of personal exchange relationships.

Fortune Parts Ways with Mead on the Arapesh Roads

When Mead sent Fortune two draft chapters of her *Mountain Arapesh* monograph for comment, he wrote back that he had no criticism except for the section "On the Roads and on Diffusion." Of this he disapproved in no uncertain terms, telling her he thought she should burn it: "You did no substantial work on the roads, but were carried over one road twice under European conditions — and the whole chapter betrays it. [It] is largely garbled from my gossip to you and largely incorrect in consequence" (MMP, RF/MM, February 23, 1936 [S2:2]; see also Dobrin and Bashkow in prep). Mead appears to have at first been taken aback by the strength of Fortune's objection and penciled in "very little of this can be in" on the copies of the manuscript she retained (MMP: Mountain Arapesh manuscripts [I13:5]). But she must have later reconsidered deferring to Fortune in this way, since she eventually published a revised and, indeed, expanded description of the roads, albeit moved "from a statement introductory to the details of the local material culture, to a position as a brief conclusion" (1938:147). Thus, in the Arapesh ethnography Mead ultimately published to lend scientific legitimacy to her theoretical interpretation of Arapesh culture in the *Sex and Temperament* framework, the account of the larger "culture area" in which Arapesh culture participates no longer depended upon sources to which Fortune could lay proprietary claim (1938:151).[9]

The primary point on which Fortune brought his disagreement with Mead over the Arapesh into print was the cultural importance of warfare. In his 1939 article "Arapesh Warfare," published in the *American Anthropologist*, he opposed Mead's characterization of Arapesh men as placid, nurturing, and lacking in cultural resources for systematized aggression or violence. As Fortune illustrated in that article, before pacification Arapesh culture had had "a highly organized social pattern" of male competition and rivalry in which men schemed to lure away women from other localities, thus provoking interlocality violence and warfare (1939:24). While the topic might well have warranted some account of the roads inasmuch as they served to structure interlocality competition and alliances, Fortune's arcane article was narrowly focused on its more immediate aim of casting doubt on Mead's portrayal of Arapesh men as peace loving and maternal in temperament; it also cryptically attacked Mead's ability to speak about such matters with authority (Dobrin and Bashkow in prep). But while "Arapesh Warfare" reveals little about the functioning of the roads as such, it presents the only examples ever published of the richly allusive rhetorical art associated with male competitive politics in traditional Arapesh society — a matter that turns out to be central in Fortune's documentation of the Kobelen feast.

The Roads Fortune Walked to the Kobelen Feast

Fortune's unpublished materials describe the roads' actual functioning in service of the purchase of a dance, the Shenei, from the coast to a more inland locality. For the Arapesh at that time, dances were highly valued ritual complexes that served as prestigious objects of exchange. For a locality to acquire a dance was not only to enjoy "the baubles of a few days' gaiety" (Mead 1935:13); it was to publicly manifest its political sophistication and demonstrate its influence in the region. The performance of dances was associated with the competitive realm of feasting and with the ability to attract and impress guests, who would afterward spread talk of the dance, thus increasing the host locality's renown. The story of a dance's pathway of ownership testified to the power and skill of those who had acquired the dance, since in order to do so they had to have exceeded their rivals in complex negotiations and successfully orchestrated the participation of large numbers of people who contributed wealth and work toward the purchase.

The dance transaction witnessed by Fortune took place in Kobelen over a three-day period in May 1932. Fifteen years earlier, the coastal village of Dogur had purchased the Shenei dance from Murik traders traveling by sea from their home at the mouth of the Sepik River. Dogur thus acquired the Shenei masks, paraphernalia, and styles of body adornment as well as the right to perform the dance, to make copies of the paraphernalia, and to sell the dance with its masks to others in turn. For many years Kobelen had been politicking to get the dance, earmarking pigs for this purpose. Finally, Dogur had agreed to send a contingent (a trading "canoe") inland to Kobelen to formally present the dance to them at a feast, whereupon the men of Kobelen, led by Kabiam of Korugen, a sublocality of Kobelen, called on their allies throughout the region for contributions of shell rings, dogs' teeth, feathers, tobacco, sago, and pigs in the hopes of concentrating sufficient wealth to persuade Dogur to grant them ownership of the dance. Fortune, eager to witness the event, "decided to organize a party and go" (Mead 1947:360).[10]

In "Pigs for Dance Songs" Fortune's narration concentrates on the journey to Kobelen that he took with his Alitoan traveling party along the Rohwim Road, the men shouldering poles from which the gift pigs were hung. Such a party could not go by any route it pleased but was obliged to follow a route that passed through the hamlets along the way: "The wider track made under Government supervision runs down the main hill crest, away from the deep cut valleys where water runs and where hamlets are built by the water. We do not go near the wider track, which is new and for non-traditional business only; but we follow down [the] waterways and over [the] steep divides between them" (PDS 59). But the formality of such an occasion determined not only the proper "path that a pig must take" (Mead 1947:360). It also determined the manner in which the journey took place:

> The carrying of pigs is a ritual business, and it is the gravest insult to carry pigs ourselves over neighbours' territory. We call on our neighbours and they carry our pigs on over their own territory. But first we sit down in the hamlet and our hosts give us coconuts to drink and food to eat. They talk a little with everyday enquiries and answers, and after the food there is some brief orating by the hosts. This is usually talk of the antiquity of the road, for the road that is open to the carrying of pigs today is the road that was open also in the old days of war. [PDS 60–61]
>
> Once the oratory about the antiquity of the open road is done, our hosts take up the pigs and go off at a trot, while we follow behind. So we go up and down to the next hamlet on the coastwards road. Here again we are fed. . . . [PDS 62–63]
>
> Then shortly after, the oratory of the hosts begins, addressed to their next inland neighbors, who have just been carrying the pigs, more than to us. We are the guests of guests of our hosts only. The oratory done, the orators and their men take up the pigs and trot off, we all following behind. The procedure occurs again in the next hamlet, where we are by now the guests of guests of guests of our hosts — and we are all present. The last hosts take up the pigs and run them into Kobelen village,[11] we all streaming behind. [PDS 66]
>
> This is the manner of the open road. We A go to our friends B, who escort us to their friends C; then C escort us all to their friends D, who then take upon themselves the escorting of all us to their friends E — and before escorting, feeding in each case. At least this is the manner of the open road when gifts of pigs are carried upon it. All the people of the road swell the carriage upon the road, and we come into our destination half way down to the coast as if our pigs have rolled up the men of the roadway and carried them with them. Indeed they had. Pigs of other inland villages converged also upon Kobelen by the same general road, but through other hamlets in many cases. [PDS 66–67]

As we see from Fortune's description, this remarkable "convention of the 'telescoping' safe road by repeated escort" (PDS 69) meant that when pigs were carried on the roads to feasts, the exchange relationships linking localities were brought out into the open in the form of a growing assembly of persons. Such an assembly concretely manifested the road as a system of interlocality relationships, making visible the intermediate ties that defined the sections of the extended "telescope" a road represented. In addition, since road friendships had historical depth and were often said to reflect a shared ancestry, the assembly of road friends arriving at a feast could be seen as a living tableau depicting the history of a sequence of places as a chain of genealogies and step-wise migrations, which perhaps sheds some light on why the Mountain Arapesh were able to "count their genealogies in the direct paternal line for twenty to thirty generations back. The open road is maintained by memory of a migration that may have occurred five hundred years ago or more. Friends in the road may be descendants of a collateral line that split off and migrated twenty five or only four or five generations ago. Or again the friendship may be traditional without origin in any known migration" (PDS 62).

Private travel and exchange by road friends did not require the same formal hospitality or escort as carrying pigs to a feast, but it was sanctioned by the same principle of commutative relationships:

> The manner of the open road is somewhat different for the party of one or two men bound on private business. They may call upon friends in various hamlets by the way without escort. But a man has not scattered friends in various hamlets. He is one of a line of friends, inland to seacoast, who are friends of one another. If a man goes unescorted he does not go to other friends than if he goes escorted. Escort is behind the fixed line of friendship, as its sanction and principle. The natural friendships, following migration in former generations and the like, are usually between neighbouring places, and friends in more distant places are the friends of friends, or friends two or three or four times removed. The escort in the manner of the road when pigs are carried is naturally enough formulated with food gifts and oratory, for this extended road served a man well in keeping communication [open] from the hills to the sea and back again, even when war made travelling in other directions a tenth of the same distance impossible or dangerous. [PDS 67–69]

The "open road" was thus a guarantee of safe passage, allowing people the possibility of travel in areas controlled by other localities without fear of ambush. But the notion of a road's "openness" derived not only from the possibility of travel it afforded as such; it also implied a particular *manner* of travel that was aboveboard rather than secretive.[12] The good and proper route for travel and exchange was along the open roads as opposed to "the road of pig and cassowary," which, when used for social purposes instead of ordinary hunting and gathering in the forest, connoted business that was hidden, shameful, or illegitimate. In several of the Kobelen speeches, for example, men expressed their concern that an untoward event such as a death might cause the transaction to unravel, leading everyone to retreat to their homes by road of pig and cassowary, carrying with them news only of failure and fear of further death in revenge (KFS 270, 279, 295, 297, 301, 302, 306).

Empathy in Fortune's Ethnography of the Roads

The Authorial Style of "Pigs for Dance Songs"

In addition to providing us with new information about the ethnography of the Arapesh roads, "Pigs for Dance Songs" is interesting for what it reveals about Fortune's ethnographic approach to studying the Arapesh. Although the handwritten manuscript (on notepaper from the trans-Pacific vessel MV *Rabaul*) is unpolished and digressive in the manner of a first draft written while aboard ship, it nevertheless exhibits certain consistent stylistic features that contrast with many of Fortune's published writings, with Mead's texts, and with the conventional anthropological writing of their time. The most striking of these features is a seamless switching back and forth between the impersonal voice of an objective, outside observer and the empathetic voice adopting the perspective of an interested participant. The scholar's voice is heard in Fortune's comparative observations, linguistic identifications, and historical commentaries and generalizations. So, for example, he informs his readers in scholarly fashion that "the 'telescoping' safe road . . . does not occur in the Arapesh form very generally in New Guinea" (PDS 69), that "the people of Murik speak a Papuan language, not a Melanesian [i.e., Austronesian language]" (PDS 52), or that "fashion becomes old on the Arapesh beach before it is released into the hills" (PDS 53). But in the greater part of the text, Fortune narrates events from the standpoint of an individual traveling the road from a specific Arapesh locality. In many passages his use of pronouns places him as a member of the traveling party his readers "follow" to the Kobelen feast:

> *We* come first to a wide stream with no habitation near it. Here *we* bathe, men and women, the women doffing their grass skirts and slipping leaves into their belts instead. Then *we* are off up a hill again, men puffing and blowing, and the women with jaws tensely thrust forward from the weight of the loads suspended

> from the forehead. *We* go up and up, then down and down, to the first hamlet on the way. [PDS 59–60]
>
> Five or six of *us* take up each carrying pole [for carrying pigs] and up and down *we* go, panting over the tracks. [PDS 58–59, emphasis added]

Or recall the wonderfully rich passage quoted earlier, repeated here with new emphasis:

> It is the gravest insult to carry pigs *ourselves* over neighbours' territory. *We* call on *our* neighbours and they carry *our* pigs on over their own territory. But first *we* sit down in the hamlet and *our* hosts give *us* coconuts to drink and food to eat. [PDS 60–61, emphasis added]

Such use of pronouns communicates Fortune's firsthand involvement in the events he describes, confirming his ability to speak of them with authority even as he does so. In conveying a general feeling of intimacy and identification with his Alitoa traveling companions, Fortune's account of the journey moreover provides for his readers a locally situated perspective on how the roads were constituted by a systematic shifting of groups among different roles (except for the Alitoans, who set off alone with their pigs, thereby activating the road), each group participating first as hosts and orators, then as escorts and carriers, and finally as followers, guests of guests, and so on.[13] So thorough was Fortune's identification with his traveling party that it is not until 30 pages into the text—at the point where the party's contribution of pigs is assessed and officially recognized by Kobelen—that Fortune is led to view his group with any objectivity and specify who "we" are more precisely than "we of the remoter, more inland higher hills" or "we of the middle hills"; it is only here that we learn that "we who carry a pig" hail "from Totoa'laibys clan of Alitoa village" (PDS 54, 79, 81). Even a humorous dig at the Catholic clergy is made from the perspective of Fortune's Arapesh companions, for whom it makes sense to wonder "how Catholics continue to exist, [inasmuch as] all we have seen, men and women, are missionaries and celibate" (PDS 58).

Heightening this sense of empathetic identification with the people he has studied, at various points throughout "Pigs for Dance Songs" Fortune overtly adopts Alitoa's low position in the regional social hierarchy in order to convey the limits of the roads' potential for diffusion:

> We of the middle hills are poor. Our land is mountainous, poor and subject to landslides [that destroy our gardens]. [PDS 79]
>
> [We] will probably never be able to purchase the Shenei for ourselves from Kobelen later, even after fifteen more years. For

> Kobelen is richer than we, as Dogur is richer than Kobelen, but we purchase the cheaper dance rights, in time, and the two pigs we carry now will go toward something else later on from Kobelen. Other villages of our inland hills will be doing likewise. . . . We inlanders all pull together to bring fashion inland the one stage only. Kobelen will later hold the Shenei as grudgingly from us as Dogur has from Kobelen, and we have less interior hinterland to help us pull one stage more. But we do our best. [PDS 55–56]

Fortune also conveys empathy by resorting only minimally to the aloof empiricism of the "anthropological gaze," or behavioral observation in the tradition of Malinowskian "I-witnessing" (Geertz 1988:73). Rather, his descriptions serve to humanize his ethnographic subjects by expressing a solidarity of feeling with them. So, for example, Fortune joins with the Alitoans, again using "we," in feeling "excited and keen at . . . the prospect of the new dance" (PDS 57). He also shares in their embarrassment when their poverty is objectified in the contrast between "our two miserable pigs" and Kobelen's "huge fattened pigs" (PDS 55–56). Indeed, so thoroughly does Fortune identify with his Alitoan companions that we cannot always tell for certain whether a feeling he expresses is theirs or his, though at times he also makes it quite explicit that his and their views converge. Certainly this is the case when Fortune discusses the exceptional requirement that the costs of acquiring the Shenei include a night of wife lending. This topic gives Fortune the opportunity to comment disparagingly on the institution of "open marriage," which he does on the basis of both Arapesh and Western moral norms simultaneously: "We [this is his empathetic *we* referring to the Alitoans] do not habitually practice wife lending any more than is European and good Christian custom" (PDS 57). Here, of all places, Fortune came by his empathy honestly, given the circumstances surrounding his turbulent divorce from Mead (see Dobrin and Bashkow in prep).

Fortune's Locally Situated View of the Scope of Arapesh Culture

Fortune's Arapesh-centered perspective has consequences for more than just his writing style. It is also reflected in his substantive interpretation of the scope of Arapesh culture. Fortune's account of the pathways of diffusion through the region parallels Mead's in many respects, for example in recognizing that the dance complexes purchased by the Mountain Arapesh originated on the Sepik river "before Murik village peddled them to the Arapesh" (PDS 71). Fortune's text also includes a lengthy discussion of sorcery that agrees with Mead's writings on the role of the Plains villagers, or *warybim*, in the regional economy as professional sorcerers who received payments both to commit sorcery and to avert it. But Fortune had a different view of what constituted the relevant culture area, implicitly contradicting Mead's adoption of the entire Sepik region for this purpose "without confining [her]self to a too-narrow or systematic use of the term [culture area]" (Mead 1938:157). Whereas Mead gave all cultural similarities and evidence of trade connection equal weight in defining the area, Fortune emphasized the cultural elements that had special significance as imports for the Arapesh people themselves as well as to Arapesh understandings of the limits of their culture based on the way these limits were substantiated in the working of the roads:

> The system [of diffusion] is covered by the native phrase "*urai ani mbuluguh sharupok*," "dance songs contest against pigs." [Through this saying,] it is indicated that dances are the principal merchandise peddled up the roads; and on this basis rights to wear clothing, rights to important *sacra* such as initiatory flutes, and other important cultural rights are also sold. [PDS 72]
>
> Dances and new cultural forms go up the roads to the watershed [of the Prince Alexander Mountains], and pigs go down to buy the rights and the paraphernalia—as far as the watershed. [PDS 79]
>
> The limit to the purchase of dances and cultural rights is the watershed. The Arapesh villages that debouch onto the Sepik plain on the inland side of the watershed do not purchase dances and rights that come inland from the coast. . . . [T]he men of these trans-Prince Alexander Mountains Arapesh villages . . . , called *warybim* . . . by the Arapesh who live seawards from the watershed . . . are modified in culture by influences accepted from their neighbours of the wide plains who speak a language allied to that of the Middle Sepik River [the Abelam], and not by influences accepted from their own people across the watershed. The watershed is a cultural barrier, because at it the system of "*urai ani mbuluguh sharupok*" ends. [PDS 72, 77, 74]
>
> They of the Sepik side of the watershed look to the inland Sepik River instead [of to the coast], and the watershed is the boundary of two varying cultures in the one tribe in consequence—although, of course, much culture is held in common. [PDS 64–65]

While Fortune obviously draws the boundaries around Arapesh culture differently than does Mead, he does not make his disagreement with her explicit, and it must be recognized that Mead's account is not wrong; it is merely different and ultimately complementary. As discussed below, Mead's intellectual temperament inclined her to an etic, generalizing, bird's-eye perspective, and in her Arapesh fieldwork this perspective would have been reinforced by her work collecting Native art and artifacts for the American Museum. While staying in Alitoa, Mead catalogued the artifacts that she purchased from natives visiting from neighboring areas as well as those obtained by Fortune during the strenuous collecting trips he made on her behalf. In so doing she must have formed a mental map of each type of artifact's local "center of . . . manufacture" along with the sources of the most highly decorated and "elaborate specimens" (1938:310, 314). These sources tended to be far away, and when not on the coast they were invariably inland, in Plains Arapesh villages close to the Abelam, a people whose flamboyant artistry and imposing architecture were so attractive to Mead and Fortune that it was the Abelam they had wanted to study when they set out for their fieldwork initially, though they were unable to continue past Alitoa to reach them. Indeed, several important classes of objects Mead described, including cassowary bone daggers, wooden spears, and net bags, were "much more highly developed . . . among the Abelam, and secondarily among the Plains Arapesh" than the "cruder forms" of the Mountain Arapesh, leading Mead to infer that the Mountain Arapesh were "look[ing] to the Plains . . . for inspiration" (Mead 1938:308–9). Moreover, many of the net bags, shell rings, and clay pots present in Alitoa were known to have been imported from the inland Plains region directly (1938:308–19, 327–28). Thus, Mead was hardly unjustified in including the inland villages of the warybim within the Mountain Arapesh culture area.[14]

But Fortune's perspective was enriched by the immediacy of his experiences among the warybim and Abelam, and, as we will discuss further below, it reflects an ethnographic approach very different from Mead's, one that inclined him to take on "the chief interests of [the] people" with whom he traveled throughout the region (Boas 1966[1911]:23). The truth in Fortune's account is very much an emic one that reveals the cultural boundaries the Alitoans themselves perceived, a matter bearing crucially on their sense of their own identity.[15] Again, his adoption of an Arapesh point of view brings to light the extent to which a relativity of perspective was intrinsic to the roads' functioning. So, for example, the warybim did travel along the roads and were incorporated into them as road friends and affines such that the roads appeared *to them* to extend across the watershed from the coastal villages to their own (PDS 74; Mead 1938, 1947).[16] However, as Fortune points out elsewhere in his manuscript materials, while "Arapesh plainsmen . . . came constantly to the coast . . . , coastal men never visited the inland plains before the country came under control by a metropolitan power" (RFFP, 80–323–15/2 ["New Guinea Fieldnotes (iii)"], 348). And when Mead and Fortune's Mountain Arapesh informants spoke of the roads by naming them along with the localities through which they passed, the warybim villages were never included in their overlapping descriptions—and they were similarly not included in the (again overlapping) description of the roads volunteered to us in 1998 from the perspective of Wautogik village, on the same (coastal) side of the watershed as Alitoa (KFS 285; Mead 1938:331).[17] The Mountain Arapesh feared the warybim as sorcerers and cultural others, and the immense importance of their sorcery and shell rings in the local political economy notwithstanding, they were not felt to properly belong to the structure of alliances that the roads represented. In effect, whether or not one saw the warybim as included in the roads or not depended on one's perspective. The more general point is that Arapesh constructed political and cultural boundaries along the roads from the standpoint of their own localities.[18]

Of course such relativity of perspective arises whenever identity is at stake, and the importance of the roads as a vernacular framework for establishing identity is still very much alive in the Arapesh region today. When we visited the coastal village of Dogur, for example, we were told of Dogur's traditional importance as the "mother village" of the Cemaun Road: "Everyone everywhere called Dogur 'mother'—in the bush and along the seacoast, where the sun rises and where it sets." Though clearly hyperbolic, the story of Dogur's road status serves even today as the basis for Dogur villagers' distinctive self-conception. And its importance for people was underscored by the fact that this was the very first thing Dogur villagers "officially" told us about themselves, even before the story of the village's founding, the theme that ordinarily takes pride of place when New Guinea villagers are visited by outsiders and have the opportunity to present the story that tells who they are. The significance of the roads as a framework for identity at the time of Mead and Fortune's fieldwork is attested by quotations in both ethnographers' writings. In another of his draft manuscripts, for example, Fortune quotes his Cemaun informants as saying that they differed from the people of Rohwim in that "[w]e [Cemaun], we weep over friends," likening the depth of their grief to certain species of trees that, "when cut their sap bleeds and stays bleeding." They used their own capacity for great feeling as a basis for contrasting themselves with the bitter Rohwim, who they said "weep not for friends" (RFFP, 80–323–15/2 ["New Guinea Fieldnotes (iii)"], 348). Mead's writings, too, contain evidence that the roads served for the Arapesh as a framework of identity, though she did not seem to take this very seriously. In one of the "bulletin letters" she wrote from the field, she remarked that "this mountain people . . . have no name for themselves, just friendly little nicknames or names for sections of a community, like . . . 'poisonous snakes' " (Mead 1977:117). This term "poisonous snakes" is none other than a literal gloss of *Rohwim*, the name of Alitoa's road (Mead and Fortune more frequently gloss it as "death adder"). Perhaps Mead felt the roads were not terribly important because the groups they defined failed to coincide with the cultural boundaries she herself discerned. But whatever the reason, Mead's failure to recognize the roads as important identity categories was reflected in her analysis, which treats the roads as avenues of diffusion, travel, and trade connecting people across a very broad region while overlooking the roads' simultaneous role in distinguishing political and cultural groupings of narrower scope. Mead was undoubtedly right that the roads served as pathways of diffusion. But for the Mountain Arapesh themselves, this function was eclipsed by the roads' more culturally salient meaning as categories of identity, categories whose expression was nowhere more evident than in the key domains of feasting and war.

The Chief Interests of the Arapesh People

The topic of war brings us to what is perhaps the most important manifestation of Fortune's embrace of the Arapesh perspective, namely, his emphasis on understanding Arapesh public discourse. To be sure Fortune's focus on discourse reflects in part his arrangement with Mead to divide their fieldwork labor, since he got the language (we discuss this further in Bashkow and Dobrin in prep). It also reflects his considerable linguistic talent (while Mead is reputed to have had little ear for languages) and the greater methodological importance he ascribed to learning people's language in depth — "half learning" being often sufficient, in Mead's view, while "virtuosity" was an inefficient excess (Mead 1939: 200, 203). Nevertheless, the fact remains that Fortune, unlike Mead, worked extensively in the native Arapesh language and that he placed particular emphasis on understanding and documenting formal oratory. In his fieldnotes and manuscript materials, significant attention is given to translated texts of high-status rhetorical forms, which, since they make heavy use of elaborate metaphors and arcane allusions, are all but impossible for an outsider to interpret. We know from Mead's letters and memoirs — not to mention from Fortune's 1942 Arapesh grammar and texts — that Fortune devoted great time and effort during their fieldwork to documenting and analyzing Arapesh speech, and his fieldnotes from the Kobelen expedition consist primarily of some 40 closely written pages of glosses of feast speeches, (thankfully) annotated with meticulous detail so as to make their meaning more decipherable. These speech texts represent a sort of precis of the main spoken parts of the public proceedings of the Kobelen feast.[19] Such speeches are intrinsically political; each of the Kobelen feast speeches was intended to advance the speaker's interests and the standing of his locality and clan. They are therefore powerfully revealing of the situation of political competition that obtained at that historical moment. By the same token the speeches are saturated with the speakers' sense of the recent history leading up to that moment, a major aspect of which was the role of the roads in times of warfare.

We fully agree with Roscoe (2003) that Mead was wrong in her repeated contention that there was "a virtual absence of any war pattern . . . among the mountain-dwelling Arapesh" before pacification, a point which was central to her theoretical interpretation of the Arapesh cultural temperament (Mead 1938:161; see also Mead 1935, 1937, 1940, 1947, 1950; Dobrin and Bashkow in prep; Bashkow and Dobrin in prep). Such a contention could only have been sustained by seriously underestimating the extent of historical change in the colonial period, a tendency for which we find evidence elsewhere in her Arapesh ethnography (see also Roscoe 2003:585). In a brief summary of "how white contact has affected the functioning of Alitoa," Mead conceded that "a certain lightening of tension" had followed from the "Pax Brittanica," as the "removal of the threat of violence always alters the life of a people, but it would be hard to find a group to which it made less difference than to the prevailingly peaceful Arapesh." The "greater immunity of the traveling Plainsmen" had perhaps resulted in "a greater tyranny on the part of [these] sorcerers, who now walked unarmed among an unarmed people, where the power was all on their side." But the idea that the roads might be "losing their sanctions as the idea of the King's Highway developed" was a matter that could not be further explored without conceding the importance of warfare, something Mead did not do (Mead 1947:269–70; see also Mead 1938:322).

Yet as the speeches at the Kobelen feast outlined by Fortune amply attest, precisely this issue of a transformation in the roads' functioning was at the forefront of Arapesh interlocality politics at the time of Mead and Fortune's fieldwork. Only two decades earlier, before colonial authorities had succeeded in bringing warfare in the region to an end, open battles as well as isolated surprise attacks were part of the customary background of Arapesh life, and the ever-present possibility of warfare was the basis for the roads' important functions of providing conduits for safe passage and organizing villages into idealized war alliances. However the roads actually worked as war alliances in particular instances, it is clear that the orators at the Kobelen feast believed the roads *should* function in this way. Road friends were felt to be natural allies, and localities on other roads were the homes of one's presumed enemies. This idea remains well entrenched today, nearly a century after pacification. The roads continue to serve as a generalized framework for intervillage competition, and in 1998 the roads were explained to us using the ready phrase "fighting group" (*Tok Pisin: lain bilong pait*). The significance of such roads for warfare and exchange is not restricted in the area to Arapesh-speaking peoples. Among the Yangoru Boiken the distinction is made between two great traditional war confederacies called "Lebuging" or "Labuhnina" and "Samawung" or "Samoun"; these terms are also used to refer to the two exchange moieties into which most of the region's villages are divided (Gesch 1985:170). The phonetic resemblance between these and the Arapesh terms is striking (Jim Roscoe, personal communication, March 20, 2005).

That the roads had traditionally served as warfare alliances was mentioned by many orators at Kobelen as a background against which the current dance transaction was an exceptional political achievement, because in fact the Shenei dance was being imported not up a road, but rather across two roads, from the Cemaun Road to the Rohwim Road. The road-based alliances thus represented a kind of idealized status quo from which contemporary social alignments diverged. So in one instance a speaker from Kobelen, a locality on the Rohwim Road, addressed the contingent from Kotai, of the Cemaun Road, as follows: "You of Kotai, before in time of war I did not know you—you did not know us all here. Your brothers who knew us are dead [i.e., if we met them, we killed them]. Before you killed our father" (KFS 180; see also 296).

Similarly, a speaker from Dogur reminded Kobelen that in former times their two roads had been enemies. Dogur had a special position as the coastal "mother village" of the Cemaun Road, a position in which it rivaled But and Semain, the mother villages of the Rohwim Road. It was from But and Semain that Kobelen formerly would have acquired its dances: "You of Kobelen tell But and Semain you had this dance from me. Before if I met them by river I fought them; if I met them by road I fought them" (KFS 181, 297). It was understood that such a cross-road transaction was facilitated by the newly enforced peace of the colonial administration: "Before we had no Government. . . . Before all men fought, broke head, hand, jaw. Now we sit down well together" (KFS 289–90). "Before we had war. Now Government has finished war. A good [i.e., young, strong, healthy] man will not be killed. Before it was not so" (KFS 271).

At the same time, pacification was diminishing the political power associated with the roads by undermining their sanction. No longer did travelers need to fear ambush when moving along roads other than their own, and there was now, too, the new "wider track," the bridle path that Mead calls the "King's Highway" (no doubt a name then current among whites), which was used by colonial patrol officers and maintained by villagers at their direction (PDS 59; Mead 1947:269). The King's Highway provided everyone a path on which to travel in safety without being dependent for safety on local intermediaries, thus weakening a locality's power to block cultural transmission along its road. Formerly "each village on the way acted as a toll gate, preventing a valuable acquisition from going inland to the next village . . . until they were ready to release it" (PDS 71–72; see also Terrell 1986 for this pattern more generally in the Pacific). Now, however, the new possibility of bypassing traditional road friendships left people weakened in their ability to mediate transactions between their neighbors to either side, making them politically insecure. Thus, in apparent hopes of forestalling the likely event that he be shut out of his rightful place as a recipient of the Shenei dance in the future, a speaker from Liwo pointed out to his traditional road allies in Kobelen that the cross-road pathway of the Shenei dance they were buying was a violation of custom: "Before when your forefathers went sorcery hunting they slept with my forefathers and talked. Now you go past, going altogether to the [warybim]. The talk of friends you should hear first before going to the [warybim]. Our mother is one [i.e., we belong to the same road]. But before all the fathers of Dogur were the enemies of our fathers. Our enemies were they, their enemies were we" (KFS 284).

The snapshot of contemporary local politics afforded by the Kobelen speech texts differs markedly from Mead's brief account, in which the cross-road pathway of the Shenei dance seems to exemplify nothing more significant than the timeless and universal phenomenon of actual practice diverging from a culture's ideals: "Theoretically, each of these dance complexes should pass up the road, from one locality to another, inland, without skipping any one of them. In practice, now one locality, now another, will display the initiative necessary to inaugurate the payments" (Mead 1938:334). To be sure it would be naive to think that even in precolonial times each dance passed neatly up its road from one locality to the next, in apolitical stages.[20] Nevertheless, for the orators who spoke at Kobelen over those three days in 1932, it was precisely because "Kobelen and Dogur [were] enemies by native custom" (KFS 305) that Kobelen's attempt—and ultimate success—at wresting the dance from its Cemaun rivals had particular interest and prestige. The Kobelen feast speeches thus bear eloquent testimony to the circumstances of change that increased the viability of nontraditional pathways for cultural transmission.

Pacification and the innovation of the King's Highway were perhaps the most important changes facilitating such cross-road alliances. But there was also the institution of plantation labor, which brought individuals from diverse, even formerly warring, localities together on neutral ground. Indeed, it was through "an alliance formed on Karawap plantation" on historically Boikin territory nearby on the coast that "negotiations for [the Shenei] dance" had begun. As one Kobelen orator emphasized: "This was not done as a friendship of old, of these places. It was a friendship of the white man" (KFS 292, 305).

Finally, the colonial situation afforded people a powerful new resource for political maneuvering, the force of the administration itself. Thus, in the fierce competitive politics surrounding the Shenei dance transaction, the mother villages of Kobelen's own Rohwim Road, But and Semain, had been so eager "to prevent [Kabiam of Kobelen from] buying the Shenei dance" that they had gone so far as to attempt to get him "imprisoned [by the administration] for alleged sorcery" (KFS 285). Like other native people throughout New Guinea, the Arapesh in this period were learning that white officials could often be manipulated into imprisoning (or even launching punitive raids against) their rivals for offenses of the whites' laws. It is surely revealing of Arapesh political priorities at this time that the end to which Kobelen's traditional road allies sought to harness this colonial power was the prevention of a cross-road dance transaction. Indeed, it might be taken as evidence not only of the high political stakes associated with the dances, but also of the changes in the roads as a force in Arapesh interlocality politics.

Why Mead and Fortune Portrayed the Arapesh Roads So Differently

In part the difference between Mead and Fortune's accounts of the roads reflects the divergent nature of the two ethnographers' experience of Arapesh culture, notwithstanding that they overtly studied it together as a husband-and-wife team. While Mead was confined to Alitoa, giving her a severely restricted view of the competitive realm of interlocality relations, Fortune traveled widely and frequently. And while Mead devoted herself to observing children and the nurture of caregivers and to assembling artifacts for shipment to the American Museum of Natural History, Fortune worked intensively to document the language, including the speeches that occupied a central place in formal exchange and politics. It is certainly understandable that Mead's analysis of her collection might have led her to conceptualize the Arapesh roads relatively apolitically, as conduits of diffusion through a large culture area in which it was the different localities' specialized productions (wooden plates, spears, net bags, and so forth) that gave the roads their primary meaning. It is similarly understandable that Fortune's extensive travels, including the trip he took along the roads to the Kobelen feast, should have led him to give far greater prominence to interlocality competition and conflict and so to such topics as marriage and remarriage, war, feasting, and exchange.[21] People are often prompted to recall events of historical significance while traveling past the sites with which they are associated. It is no coincidence that the topic of warfare was raised in 1991 by one of Roscoe's Mountain Arapesh traveling companions when the group stopped to rest at the edge of an overgrown former battleground (Roscoe 2003:589), or that the lists of villages belonging to the Cemaun and Rohwim Roads recorded in our own fieldnotes were offered to us by our Arapesh friends while walking with them along the roads of today. Mead's strict confinement to a single hamlet for the entire course of her Arapesh fieldwork meant that she simply did not find herself in the kinds of situations that triggered such spontaneous recountings of interlocality history.

Then, too, there is the effect of the two anthropologists' different theoretical inclinations and prior interests. Let us consider Mead's first. Mead's interest in her Arapesh ethnography revolved around the dominant concerns of gender psychology and economics, and she brought to her work a wide and eclectic range of theoretical idioms from the American diffusionist anthropology in which she was raised, the British functionalist anthropology to which she was attracted, various strands of psychology and psychoanalysis, and American capitalism and popular culture. While Mead's concern with the cultural construction of gender is explicit in her best-known writings and has been much discussed (see, e.g., Banner 2003; Lapsley 1999; Howard 1984), the topic of the roads points to the idiom of Western economism that is also present, if less self-consciously, in her Arapesh work. For Mead the roads were seen as connecting not political entities, but rather individual exchange partners, or buanyim. Mead describes these interlocality partnerships as "institutionalized," "hereditary," and "definitely patterned" like fixed "kinship relationships" (1947:204). Although she recognized that they were changeable (1937:32, 1938:322, 1947:207), the model she used to explain them is reminiscent of Bronislaw Malinowski's portrayal of Massim *kula* partnerships as predetermined and permanent, as if some framework apart from the actual exchanges was needed in order to "bind" the parties in their relationship (Malinowski 1922:83, 85, 91; Mead 1938:321–31). Like Malinowski, Mead did not see exchange itself as sufficient to constitute such a relationship. But unlike in kula, where Mead saw exchange as "linked with [a] great ceremonial superstructure" and celebrated as a prestigious form of sociality in its own right, the Arapesh buanyin relationship was fundamentally a means by which items of value were redistributed throughout the area. Since "each community is poor in many things and must look outside its borders for them . . . , exchange becomes not . . . the object of life, but the basis of life" (Mead 1938:164). An economically sophisticated American (indeed, an economist's daughter), Mead readily grasped that exchange along the Arapesh roads was "often a most uneconomic procedure," and given her collector's perspective on where goods were made and where they were scarce, she found it remarkable that individuals often carried items such as wooden plates or shell rings in "the wrong direction for profit" (1937:22, 1938:329). But she did not seriously consider the possibility that exchange along the roads was motivated by economics only secondarily, and her account of the roads makes frequent appeal to terms drawn from the vocabulary of Western capitalism, such as "profit and loss," "scarcity value," "demand," "currency," "purchase," "price," "fee," "debt," "haggling," "vending," "bartering," "banking," "economic crime," and "entrepreneur" (1937:22, 1938:176, 219, 324, 327–30, 333, 1947:206). That the Arapesh themselves mostly spoke about their exchange transactions "in terms of affection," friendship, and gratitude was seen by Mead as a "disguise": the real business was a "vital economic exchange" being conducted "under the guise of free giving" (1938:327–28, 1935:28). Mead of course recognized that people's motivations might be more or less economic and that feasting and exchange could also be used for such noneconomic purposes as "bolster[ing] prestige, establish[ing] ties between families or clans, or validat[ing] claims to position" (1947:223). Nevertheless, such functions were regarded as secondary, pursued within economic constraints, rather than regarded as themselves establishing the constraints and aims of exchange.[22]

Coupled with Mead's interpretation of the roads as primarily economic was her tendency to minimize in her analysis the political significance of exchange relationships and material transactions. Although she noted that buanyin exchange partners behaved assertively and competitively toward one another, she took pains in her texts to square this observation with her central claim that Arapesh men were culturally "schooled in gentleness and non-aggression" by consistently describing "the institutionalized exchange relationship" between buanyim as a patterned exception to the cultural norm (Mead 1940:354, 1947:204). According to Mead, Arapesh "society assumes, usually correctly," that for the ordinary Arapesh man "the active, competitive life" of a buanyin is "eminently uncongenial and distasteful" (1935:30). Nevertheless, since "the organization of large-scale feasts" requires careful accounting and assertiveness, certain individuals in every community were selected to receive "a definite training for the special, contrasting behavior that 'big men' must display." Eventually "a few of them [would] yield to all this pressure, learn to stamp their feet and count their pigs, to plant special gardens and organize hunting-parties, and to maintain the long-time planning over several years that is necessary in order to give a ceremony which lasts no longer than a day or so" (1935:30, 1937:32). In *The Mountain Arapesh*, Mead wrote that "the greatest function of the [buanyin] relationship" was that "it channels . . . feelings of aggressiveness and competitiveness into narrow, socially guarded grooves, and so permits their exercise for the benefit of the society, without the disruption of the mild helpfulness characteristic of the bulk of Arapesh social life" (1947:204–7). The buanyin relationship also served as an outlet for what Mead portrayed as men's natural frustration at the vague terms and open-endedness of so many economic transactions "in a society where the norm for men is to be gentle, unacquisitive, and co-operative, where no man reckons up the debt that another owes him, and each man hunts that others may eat" (1947:205, 1935:30): "whereas [ordinarily] cost accounting, dunning, [and] reproaching in economic terms are regarded [by Arapesh] as disgraceful, between buanyins there is a frank accounting system" (1937:33). (Mead comments, "What a relief to be able turn to one's *buanyin* and openly dun him" [1947:206].) In short Mead portrays the buanyin relationship as "a social institution that develops aggressiveness and encourages the rare competitive spirit," even while insisting that it does not really count as a culturally significant pattern of masculine competitiveness and aggression (1935:28).

Where Mead sees the buanyin relationship as motivated by something other than the need to release or channel primal competitive impulses, its motivation is described as economic and practical. Hence her suggestion that even in their roles as competitors, buanyim actually cooperate in the service of the common good. Mead writes in *Cooperation and Competition* that they are "expected . . . to goad one another on to economic activity. . . . Buanyins do not compete with each other, rather they keep each other up to the mark. They cooperate in maintaining a more rapid large-scale turnover of food than would otherwise occur in the community" (1937:33). It is as if buanyim, then, were rival producers in an idealized model of the capitalist market: their competition increases efficiency and lowers prices, thereby raising the standard of living for all.

Whereas Mead's analysis of the roads may draw too heavily on culture-external models, Fortune's "Pigs for Dance Songs" is hyper-particularistic: it is a document in which no model is imposed on the material at all. Even more than Fortune's other ethnographic writings, which have at least some anchoring in a disciplinary discourse if only in that "virtually everything is at variance with what others have found or assumed" (Rice 1979: 108), "Pigs for Dance Songs" is marked by an extreme paucity of exogenous perspective, a striking absence of the kind of objectivity we expect from a description by a professional outside observer. The extant fragment lacks so much as a single reference to the anthropological literature, and the sole ethnographic comparison in the text is drawn only to assert that the roads' system of "telescoping" escort was unique in the region. We know from comments Fortune made in his correspondence and publications that he disapproved of subordinating ethnographic material to theory,[23] and when we look at the use of analytic categories in his other writings, we find that they serve primarily as loose organizing devices, minimal connectors for what was the real stuff of his ethnographies: detailed descriptions of particular activities or events he participated in, observed, and heard reported. Given that the other manuscript fragments surviving in his papers are broadly divided into sections on such topics as religion, ritual, and (somewhat unsystematically) social organization, Fortune was apparently trying to organize his Arapesh materials according to the conventional rubrics. But the drafts tend to veer off topic and then break off, and they show evidence of repeated reediting, suggesting that he found it difficult to abide by the structure he imposed (and giving us some clue as to why the monograph was never completed).[24] In this sense "Pigs for Dance Songs" is an extremely limited document. It is so particularistic that it would be quite useless for a reader not already acquainted with Arapesh culture from some other source, ideally firsthand fieldwork.

But with all that said, what we find most striking about Fortune's work in the light of our own field experience among the Arapesh is its verisimilitude, the core of ethnographic truth in it that stands out across the intervening seventy years. Many of the specific institutions Mead and Fortune described — the *tamberan* cult, initiation ceremonies, the elaborate system of taboos, and the convention of telescoping escort when carrying pigs along the roads — have since fallen by the wayside. But the larger themes of Fortune's writings — sorcery, morality, formal exchange, and male competitive politics — remain important concerns for Arapesh villagers today and are prominent in their own discourse. We recognize that these are also themes Fortune developed in his earlier major works, *Sorcerers of Dobu* (1932), *Omaha Secret Societies* (1932), and *Manus Religion* (1935), so that he began his Arapesh fieldwork already attuned to them. But this does not diminish our appreciation for how honestly his writings seem to reflect the idioms and concerns of the Arapesh people with whom he lived; hence our repeated reference to a quality of empathy throughout this paper.

Fortune achieved his empathetic understanding by placing primary methodological emphasis on listening, on trying to understand what people were saying, not only when speaking to him, but also when they spoke to one another. Fortune's emphasis on listening is evident in the attention he gave to how Arapesh people construed things, in the care he took to establish what they thought was important, and in the enormous effort he expended on recording and interpreting their words. As we have noted, Fortune's field notes contain numerous texts that are transcriptions or summaries of speeches, primarily the allusive politico-moral disquisitions called *sakihas* that sought to persuade listeners to adopt or eschew specific courses of action by framing moral precepts and explicating their consequences in terms of typified acts. That Fortune wrote down and struggled to translate this speech from a difficult vernacular substantiates the importance he attached to understanding the culture through listening.

And thus we are brought to the difference between Mead and Fortune's ethnographic approaches. The two anthropologists' views of the Arapesh roads differ in part because of their divergent fieldwork experiences and their division of labor, and in part because of the prior interests and theoretical idioms each brought to the research. But Mead and Fortune also had incommensurate explanatory ideals and analytical values (Putnam 1981). Mead aimed to achieve a view of the "culture as a whole," writing "as if the observer stood outside and looked down upon" it (Mead 1938:151). Fortune, by contrast, did not aspire to this kind of universal frame of reference. So whereas Mead applied the objective, distributional concept of the "culture area," an established analytical concept taken from her own scientific discipline, it was the people's own concept of the watershed along the roads that Fortune adopted as the relevant boundary of Arapesh culture. So closely did Mead's work respond to Western scientific questions and American cultural concerns that it has been widely appreciated even outside anthropological circles. Fortune's, in contrast, grew increasingly remote from these. His writings on the Arapesh adopt a local vernacular perspective to such an extent that they suffer in comprehensibility (they did, after all, have to be read by a Western audience), and they have thus been little valued even within Fortune's own discipline of anthropology.

Not only were Mead and Fortune's different analytical values reflected in their writings; these values had consequences for the methods they used. Mead administered projective psychological "Rorschach inkblot" tests and recorded detailed observations on children's lip play, breastfeeding, and people's behavior, for example, during "fifty minutes of village life in Alitoa" (1947:414–15). Fortune's form of empiricism, on the other hand, led him to write down long stretches of discourse, as he did at the Kobelen feast; this kind of patient, nondirective listening was something Mead, it seems, did much less of. Less of a watcher and more of a listener than Mead, Fortune arrived at his formulations of Arapesh morality on the basis of Arapesh public discourse in genres like sakihas that he heard people use with one another, whereas Mead derived hers largely from informants' responses to her inquiries and from her own observations. While Mead strove to make etic generalizations about behavioral patterns and psychological character, Fortune strove to gain an emic comprehension of the subtleties of vernacular idiom and verbal art. The methods Mead adopted on the basis of her scientific ideals distanced her from the Arapesh people she studied, whereas Fortune's attempt to understand this foreign people through careful listening led him to empathetically adopt the Arapesh perspective as his own.

It is one of the great legacies of the German Counter Enlightenment that we get through the work of Franz Boas and his students that empathetic insight is a legitimate and productive way of knowing in the human sciences. In conventional naturalist Enlightenment science, knowing other humans is really no different from knowing natural physical entities: we know them objectively by formulating a theory or abstract model that generalizes about their causes or regularities. But romantic Counter Enlightenment interpretivism recognizes the special power we have to understand human others by virtue of our fundamental similarities, our capacity to understand others subjectively "by empathizing with them and . . . putting ourselves in their situation" (Kögler and Stueber 2000:1). Because we are like others in our humanity, we can imaginatively project ourselves into their lives and even learn to simulate important aspects of their experience by actually taking their places, adopting their practices, listening to and speaking their words, and opening ourselves to their feelings. In attempting to identify in this way with culturally different others, we are often confronted with the limitations imposed by our own cultural perspective. Empathetic understanding is not — indeed, cannot be — absolute; it is instead hermeneutic, achieving only successive approximations to the other's point of view. For all their shortcomings, "Pigs for Dance Songs" and Fortune's outlines of the Kobelen feast speeches are redeemed by their empathetic insight. Though Fortune was an outsider to American anthropology, he was a true Boasian in his empathetic ethnographic approach.

Fortune's excessive particularism might even be seen as a characteristic Boasian shortcoming, albeit in his case one that was so severe that it made his ethnographic materials all but unusable, except through a major interpretive effort such as we have undertaken here. Fortune's ethnography may accurately reflect the values, ideas, and concerns of the Arapesh people he knew, but there is no getting around the regrettable fact that he has left us but little by way of such reflection, just a tiny window only few of us can peek through. In this respect Mead's self-conscious ethnographic thoroughness and consideration of her readers, both those of her day and those she presciently envisioned revisiting her fieldwork far into the future, are virtues not to be regarded lightly. And yet, much as we rely on Mead, we find her Arapesh writings to be rather distorted by her "well-known penchant for excessive generalization" (Lohmann 2004:112). For all their polish and detail, we often need to second-guess Mead's writings in light of their discrepancies from the other sources of evidence. In some important sense, then, Fortune's Arapesh writings—iconoclastic, fragmentary, and mostly unpublished though they unfortunately are—are more trustworthy and reveal greater ethnographic insight than Mead's ably compiled oeuvre.

At the very least we would insist that Fortune's materials are an invaluable resource for getting at "the truth" about Arapesh culture and history, because the work of each ethnographer has strengths that help compensate for faults in the work of the other. Our understanding of the roads pieces together our own field experience, Mead's overview of diffusion, and the voice Fortune gives to the Arapesh people of the time. What emerge are the outlines of a remarkable and previously obscure regional institution, the Arapesh roads, whose central importance to Arapesh social organization, identity, and political rivalry—even to the point of warfare—Fortune fully reflected in his work. Even so, there is still much we do not know.

Notes

We thank Ann McLean for permitting us to use Fortune's unpublished materials and for her gracious encouragement of our work. We are also greatly indebted to Paul ("Jim") Roscoe, who generally made available to us the fruits of his own labors in the Reo Franklin Fortune Papers, thus sparing us the enormous expense and effort of a trans-Pacific journey. Quoted material from the Margaret Mead Papers in the Library of Congress appears courtesy of Mary Catherine Bateson and the Institute for Intercultural Studies, Inc., New York. Our work in the Margaret Mead Papers was carried out with support from the Dean of the College of Arts and Sciences and the Vice President for Research and Graduate Studies at the

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1. For example, in the glossary at the end of Mead's first Mountain Arapesh volume, she defines *road* as "the traditional route from hamlet to hamlet along which inter-group diffusion of complex forms of ceremonial behavior takes place" (1938:345).

2. As is the case with other proper names and vernacular terms, the name of this dance is spelled variously in Mead's and Fortune's writings. We have edited the spelling here in a way that we feel most adequately renders the Arapesh pronunciation (in the Cemaun dialect we know best) in English orthography. We take this approach with all vernacular proper names and quotations cited here.

3. For a more detailed account of Mead and Fortune's New Guinea fieldwork, their marriage's breakup, and their intellectual clashes, see Bashkow and Dobrin in prep, Dobrin and Bashkow in prep.

4. This no doubt played a part in energizing the couple's virtually constant efforts during their stay in Alitoa to find an alternate field site (see Bashkow and Dobrin in prep). As Mead mentions at numerous points in her writing (e.g., Mead 1972:229, 1977:124) and as a survey of the *Diary of Events* confirms (Mead 1947), Alitoa was often deserted.

5. Though it is conventional to call them "roads" in New Guinea, they are really no more than narrow footpaths.

6. These people were called *warybim* by Mead and Fortune's informants. They are speakers of Bukiyip Arapesh (Conrad 1978; Conrad and Wogiga 1991). Fortune and Mead's translation of this term, "river-men," presumably implying the Sepik River, is almost certainly incorrect, since this meaning would be expressed as *worybim* or *worybysim* in both Mountain Arapesh and Bukiyip. The term more likely means "villages-men" (i.e., "the men from those villages"), *waryb* being the plural of *wabyr* (village). This conventionalization would conform to the local preference for nonspecific or elliptical, and thus deniable, ways of uttering names (here sorcerers) that invoke trouble or could precipitate conflict if overheard. We are indebted to Bob Conrad for confirming the relevant dialectal variants for us.

7. Although Mead and Fortune note the existence of three or even four roads, only the two easternmost roads, Cemaun and Rohwim, figure directly in their accounts (Fortune 1939:22; Mead 1935:10, 1938:331–32), and it is only these two that had primary significance to the Arapesh people with whom we worked along the northern Arapesh/Boikin border in the late 1990s. Our discussion here is therefore focused on these two roads (though see Mead 1938:332 for mention of the roads further to the west). Mead and Fortune's common spelling of the road names, Shemaun and Lahowhin, reflects their pronunciation in the Rohwim dialect.

8. Mead surely took the roads-as-arc model too far when she suggested that people had "a tendency to regard the plains and the sea as interchangeable." She based this assessment on the symbolic association the roads shared with "the great *marsalai* of the sea" (1938:331). But this appears to be a result of Mead's misinterpreting people's uses of the term *Cemaun* (the name of the eastern road) as referring to generalized directions rather than to the road affiliation of numerous surrounding villages to the north, east, and south.

9. Never one to take criticism lightly, Mead was stung shortly before leaving for her Arapesh fieldwork by A. L. Kroeber's *American Anthropologist* review of *Growing Up in New Guinea*, which raised "questions . . . about paucity of ethnographic data," questions that applied as well, Kroeber said, to Mead's earlier book on Samoa (Kroeber 1931:250; see also Hart 1932). A bristling letter from Mead in reply notwithstanding, Kroeber stood by his assessment that "you have not in your two books given all the evidence which the ethnographer wants" (MMP, MM/ALK, May 1, 1931, ALK/MM, May 14, 1931 [C3:K]; see also Mead 1933:9). Mead's *Mountain Arapesh* ethnography, with its multiple volumes of ethnographic detail, would forestall any further such criticism by establishing Mead's credentials as an anthropologist capable of producing the kind of comprehensive cultural documentation that represented solid ethnological scholarship at that time. Given that the hallmark of such scholarship (particularly in the American tradition) was to present specific cultural forms—the material culture, kinship system, social organization, economic arrangements, religious ideas, and so forth—within their areal context, it would have been too great a risk to her professional reputation for Mead to allow the areal setting of her Arapesh ethnography to be founded predominantly on Fortune's material, since this material was potentially discreditable. Hence, Mead's account of the roads, which relied unavoidably on Fortune's material, was moved to a relatively freestanding section at the very end of the volume and her major "Description of the Area" is presented as the wider Sepik region, enabling her to draw extensively on her own Mundugumor and Tchambuli work and on the published studies of researchers other than Fortune (1938:150, 153–202, 321–32).

10. Fortune's decision to attend the Kobelen feast compelled the Alitoan man La'abe, who had a gift friend in Kobelen, to contribute a pig, lest he suffer the embarrassment of showing up empty-handed (Mead 1947:359–60). Another Alitoa man, Yapiaun, later added a second pig that he had found caught in one of his hunting traps on the day of their departure (PDS 81).

11. We have regularized Fortune's spelling of the locality names Kobelen and Dogur. See note 3, above.

12. A related custom elsewhere in New Guinea is the Yupno people's habit of singing the *koñgap* melody belonging to the landowner as a form of protection when walking across that person's land. The traveler thereby "proves himself to be in the know, to be a friend" (Wassman 1997:148).

13. To our knowledge this is not a voice in which Mead wrote. Even in the famous passage in *Sex and Temperament* describing the children's fright upon the arrival of the dangerous *tamberan*, intimacy is conveyed from the standpoint of an omniscient narrator observing the characters' thoughts and feelings (Mead 1935:64).

14. Mead did recognize that the importation of dances "breaks down at the last mountain ridge, because the Plains Arapesh receive all their ceremonial importations from the Abelam peoples" (Mead 1938:335). While she sees this as relevant to the dances' progressive devaluation as they proceed inland from village to village, she does not bring this fact to bear on her delimitation of the culture area.

15. See Bashkow 2004 for a more general discussion of the inevitably etic "culture area" concept and the contrast between emic and etic (folk and analytical) cultural boundaries.

16. Here again, we are indebted to Bob Conrad, who was able to share with us a contemporary Bukiyip perspective on village groupings across the watershed.

17. In his notes Fortune groups together Kotai, Autogi, Dagur, Yaminip, Malis, and Yauiya under the "eastern road." Listed for the other road are Mogahin, Koblen, Waginara, Umanep, Liwo, Aliatoa, Bugabehem, Numinihih, and Halisimi (KFS 285; see also Mead 1938:331; Roscoe 1994; these village names reflect Fortune's spelling). Our informant from Wautogik was Clemen Hayin of the leading Abahinem clan. He was the community's preeminent authority on traditional matters.

18. A perspectival understanding of the roads extends even to the road name Cemaun (or Shemaun), which is glossed as "dugong" in all the writings of Mead and Fortune, who lived in Alitoa on the Rohwim Road. According to Bob Conrad (personal communication, January 20, 2005), this is also the understanding of the term given by contemporary central Bukiyip people, whose traditional road affiliation is Rohwim. In a speech reported by Fortune, a Cemaun man says of himself, "I am a fish of the sea" (KFS 298). But the Wautogik villagers we worked among were resolute that the Cemaun Road, with which they identify, takes as its emblem not the dugong but the shark, and they manifest this meaning visually when they represent themselves on banners using the image of a shark.

19. It is virtually certain that these concise and focused texts do not represent the Kobelen feast speeches in full. Our best guess is that what Fortune wrote down was a running summary of the speeches as well as the commentary on them given by his informant and house boy Kaberman ("Tommy"), whose home village was Kobelen.

20. The detailed example presented in Fortune (1939:34) shows that interlocality warfare did not always take place across roads; war could also be provoked by escalation of conflict between localities along the same road.

21. Indeed, the interrelatedness of marriage, adultery, and warfare *was* the subject of Fortune's 1939 article on Arapesh warfare.

22. Elsewhere, in the context of comparing the Arapesh to a more "commercially minded people like the Manus," Mead calls the buanyin relationship "not commercial but ceremonial and symbolic," though without elaborating how this might constitute a positive form of motivation (1947:227). Even where she concluded that a particular type of transaction served economic ends poorly, her interpretations were still cast in terms of the rationality governing commerce and trade (see, e.g., 1947:221–25).

23. For example, in a laudatory book review Fortune compliments the "author for going nowhere into unfounded speculation": he "gives the theories on the subject in about seven concluding pages, the evidences . . . in the earlier five hundred and ninety odd" (Fortune 1931).

24. Apparently Fortune was not always limited in this way. In his 1927 book on dreaming, *The Mind in Sleep*, Fortune elaborates a typological model of the ways in which unacceptable attitudes are expressed in dreams, although here, too, his specific examples (his own and others' dreams) lead him to convolute the model, so that it is not expressed neatly (Lohmann n.d., personal communication, March 12, 2005).

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MMP: Margaret Mead Papers and South Pacific Ethnographic Archives. Manuscript Division. Library of Congress. Washington DC. Citations give box:file numbers.

PDS: "Pigs for Dance Songs." Manuscript by Reo Fortune, reconstructed in part by Paul Roscoe and in part by the authors from fragments in Reo Franklin Fortune Papers at the Alexander Turnbull Library, Wellington, New Zealand, files 80–323–13/1 (ii) ["Arapesh Field notes (ii)" file], 80–323–10 ["Ordnance Survey" file], 80–323–21 ["Old New Guinea Notes (1)" file], and 80–323–21/3 ["Various Notes, Typescripts, etc." file]. Page numbers reference the pagination we applied to our own copy; they are not reflected in the papers in the archive.

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6. *Diamond Jenness's Arctic Ethnography and the Potential for a Canadian Anthropology*

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

Educated at Oxford and employed at the National Museum of Canada in Ottawa, Jenness was never located at the center of either tradition. He was, however, one of the first anthropology students of Robert Ranulph Marett, a leader of the British folklore movement and an important figure in the development of British anthropology; afterward, his direct superior in the museum was Edward Sapir, a leading figure in the development of the Boasian paradigm. As a result of his training and employment, Jenness occupies an interesting position at the intersection of the two traditions.

This paper examines the extent to which Jenness's Arctic work represented a distinctly Canadian approach to anthropological method and theory. Jenness was trained in the British tradition but worked in an Americanist context; he went into the Arctic equipped with British theories and methods and returned to an Americanist setting to craft his field notes into ethnography. I am interested in how Jenness's works display the influences of these disparate traditions and whether or not he represents a distinctive combination of the two. I am intrigued as well by the congruency of his work with that of his contemporaries, both in Britain and in the United

States. Though Jenness came into contact with younger scholars in the 1930s, for example Frederica de Laguna, William Fenton, and Thomas McIlwraith, he did not supervise the training of any students.[1] However, he is acknowledged as a significant figure in the history of Canadian anthropology.

In this paper I first outline the historiography of anthropology and assesses Jenness's place in the history of Canadian anthropology. Second, I consider the work of Jenness's academic mentor at Oxford, R. R. Marett, and examine Jenness's first fieldwork, in New Guinea. Third, I analyze Jenness's Arctic ethnography. Fourth, I provide a context for his Arctic ethnography by examining the approaches of Jenness's contemporaries, in particular Bronislaw Malinowski, A. R. Radcliffe-Brown, Franz Boas, and Edward Sapir. Finally, I discuss Jenness's work as a potentially distinctive Canadian approach to anthropological research. Throughout, I approach Jenness's work on its own terms, though my research on Jenness has not been motivated solely by historicist concerns.

In the historiography of anthropology, Diamond Jenness is one of the "dead ends" in the development of the discipline, interesting to historicists but not presentists largely because he "founded no formal school of Canadian anthropological thought and headed no band of followers" (Lotz 1971:18). Though he was one of the most prominent anthropologists in Canada during the period between the two World Wars (Epp and Sponsel 1980:10; Maxwell 1972:86; Kulchyski 1993:23), he is now largely forgotten.[2] As a result a strictly presentist approach to his career and works would offer little or no insight or explanatory power. Barnett Richling, who has studied Jenness in depth, offers three hypotheses for the relative invisibility of Jenness in disciplinary histories. First, he notes, Jenness "worked in the shadow of Sapir"; second, he spent his career in the National Museum of Canada, where the range of his research and his contact with colleagues and students was limited; and, third, he made "contributions to anthropological knowledge [which] were primarily substantive, not theoretical" (Richling 1989:71–72).

However, to say that Jenness is not known as a theorist does not mean that his work is atheoretical; rather, his theoretical orientation is less than transparent and must be reconstructed from his works.[3] Writing of Marius Barbeau, a colleague of Jenness at the National Museum, Derek Smith has argued that the work of this group of scholars, the first generation of professional anthropologists employed in Canada, must be analyzed and assessed: "Our task . . . is to evaluate Barbeau's work strenuously in terms of critical social theory. We should be able to do this without sentimentality or hagiography. His work demands that attention now—and it de-serves the best analysis and evaluation that we can bring to it, for it is being used uncritically and simplistically" (Smith 2001:198). Even though his approach is heavily presentist, I agree with the general thrust of Smith's argument. While both Barbeau and Jenness are central to the history of anthropology in Canada, they have not been subjected to sustained critical analysis of their particular approaches and their influence on the wider world around them.[4]

One of the few scholars to examine Jenness critically is Peter Kulchyski. Writing from a critical-Marxist perspective and limiting himself to an analysis of Jenness's Arctic diaries and single-volume survey of the *Indians of Canada* (1932), Kulchyski situates his critique in a larger discourse of nation building. Taking note of the relative paucity of theoretical work on Jenness, Kulchyski argues that

> this relative silence over a figure of such stature is suspicious in its own right. It is not only an amorphous change in times and attitudes that demand [*sic*] such an examination: the current level of political struggle engaged in by Native Canadians requires historical rereadings. . . . Jenness's intellectual project was not unrelated to a project of Canadian national definition that excluded Native peoples. It is also important that the roots of the antagonism between Natives and anthropologists be laid bare, not to exacerbate the rift further, but in the hope that understanding and openness can help to produce a meaningful *rapprochement*. And intellectual history, in which Jenness must occupy a crucial position, is no incidental aspect of this program. [Kulchyski 1993:24]

Like Smith, Kulchyski takes a presentist approach. Still, this sort of work presupposes situating Jenness in the context of his times, and not simply in the context of current concerns about relations between Aboriginal peoples and the state. While this is obviously important, I will concentrate on understanding Jenness in terms of his own time, leaving the analysis of his impact on later developments for others.

I focus on Jenness's Arctic ethnography to assess his contributions to Canadian anthropology. His Arctic works are the result of his most extensive fieldwork project, and they comprise the largest portion of his published output. As a civil servant working in a period of severe fiscal restraint, Jenness was unable, for the remainder of his career, to undertake fieldwork lasting more than two or three months. Additionally, his administrative duties prevented him from devoting as much undivided time and energy to the writing up of later fieldwork. I am interested primarily in examining Jenness's work as reflected in his books, diary, and letters.

While issues of his character are interesting, I will raise these issues only in my conclusions.

A strictly historicist view of Jenness's work is impossible. By its very nature my contextualization is based on ahistorical considerations. While I try to avoid questioning why Jenness did not think like his contemporaries, I recognize that this concern is never far from the surface. At the same time I am interested in Jenness *because* his approach was distinct from that of his contemporaries and also because it was not passed on to a subsequent generation. With no students to explicate his theory and methods retrospectively, there is an opportunity for a kind of detective work, seeking answers to questions as yet unasked.

I recognize that however Jenness's approach was viewed in its own time, it needs to be critically assessed to be useful in a current context. His ethnographic writings appear remarkable: he undertook intensive, long-term fieldwork in an era when this was not the norm; he named his informants in his publications; and he published the first major ethnography of the Copper Inuit. If, however, his work is to contribute to current debates, an analysis in modern terms should be brought to bear on it.

At one level Jenness's Arctic work is important in its own right. David Damas has argued that "the most important source for the Copper Eskimo is Jenness . . . , whose nine volumes [in the *Report of the Canadian Arctic Expedition 1913–1918*] probably represent the most complete ethnography of any Eskimo group" (Damas 1984:413–14), while David Riches maintains that Jenness's *The Life of the Copper Eskimo* (1922) "might well be regarded as the first recognizably modern anthropological production on the Eskimo, its format of closely researched socio-cultural material, coupled with snippets of local colour and personal experience anticipating such later landmarks as *We the Tikopia*" (Riches 1990:81). Though it might not be widely remembered or read, it is obviously central to the anthropological canon on the Inuit.

Training and Early Work

Jenness was born February 10, 1886, in Wellington, New Zealand, the youngest of George and Hannah Jenness's fourteen children.[5] He won a scholarship to Victoria University College in his hometown in 1904; after graduating in 1908, with first-class honors in classics, he went to Balliol College, Oxford, as a scholarship student to continue his studies in Latin and Greek. Among the people he met when he arrived at Oxford were Barbeau, a Rhodes Scholar from Québec, and Wilson Wallis. Both were anthropology students, and they convinced Jenness to take courses in that field. In 1911 Jenness was awarded a Bachelor of Arts with Honours

(Lit. Hum.) and a Diploma in Anthropology; five years later he was also awarded a Master of Arts degree (Balikci 1957:37; Richling 1989:72–73).

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

> I laugh sometimes when I think how "staid & grown up" we were in the Oxford Days—solving problems of heaven & earth & letting life slip by under our feet. You in particular burned the midnight oil over Haida crests & clan totems etc. while Wallis showed the Australian Blacks how their society ought to be organised. The only society I shall ever organise will be beside my own hearth—where you & other friends will come to expound your dreams by the firelight. [CMCB, box B206, file 27 (Diamond Jenness, 1912, 1914, 1917–24), Jenness to Barbeau, May 18, 1918]

Though Jenness does not seem to have discussed his training in anthropology, his contemporary, Wallis, published an article outlining the program at Oxford. While Wallis's description is somewhat vague, it does offer a sense of what was expected of the students (Wallis 1957). Their studies were largely self-directed, with instruction in social anthropology from Robert Marett, in material culture from Henry Balfour, and in physical anthropology from Arthur Thompson. Each student took both oral and written examinations in each of the three topics. Students took a wide range of electives as well, "in such subjects as human geography, comparative religion, psychology, the European Bronze Age, and Egyptology; I think all of us attended the osteology lectures in Medical School" (Wallis 1957:787–88). The wide range of instruction would stand Jenness in good stead when he undertook his fieldwork.

Jenness's main influence at Oxford was the classicist, philosopher, and anthropologist R. R. Marett. Marett had begun his career under the guidance of Edward Tylor, although he would later call into question some aspects of Tylor's evolutionary framework (Stocking 1995:126, 167–68). Though little known now, Marett commanded respect both from his contemporaries and from commentators. Robert Lowie, in a survey of ethnological theory generally critical of European developments outside of Germany, singled Marett out for praise, asserting that "in post-Tylorian England for poise in the judgment of theories or for a sympathetic grasp of primitive values there is no superior to this philosophical humanist" (Lowie 1937:111).[6]

Like most of his contemporaries, Marett was, broadly speaking, an evolutionist, though for him "evolutionism was only an ultimate point of reference, not a central organizing concept" (Kuper 1996:4; cf. Stocking 1995:126). In fact one of the main distinctions between the British and American approaches to anthropology is the former's lack of critiques of the evolutionary framework; "for a long time," James Urry argues, "evolutionism remained an implicit aspect of many theories, including those of both Malinowski and Radcliffe-Brown" (Urry 1993b:14), the first members of the next generation of British theorists. Marett's evolutionism was tempered, however, by his emphasis on history. He saw value in the combination of historical and evolutionary approaches, with the historical focusing on an analysis of the formation of current conditions and the evolutionary, which he associated primarily with psychology, concentrating on "the spontaneous origination, the live . . . moment of spiritual awakening, that ensues upon the fact of cultural contact and cross-fertilization" (Marett 1917:34). Marett's emphasis on change and movement in his theoretical approach to cultures privileged psychology as the central framework for understanding humanity (Marett 1920b:14). Marett's emphasis on the Tylorian notion of survivals marked him as a prefunctionalist theorist, although he argued uniquely that the only way to understand survivals is through a psychological approach, one allowing researchers "to apprehend the present not as an envisaged state but as a felt movement" (1920b:18–19).

His preferred method of psychological analysis was for the anthropologist to attempt to project him- or herself into the collective mind of the "primitive" society being studied. In a passage that also displays his particular evolutionary approach, Marett outlined his argument in favor of his method on the basis that if his assumptions were faulty, then the whole discipline of anthropology would be impossible to conceive. He defended the right of the anthropologist to place himself [*sic*] into the mind-set of a different culture on the grounds that all people experience common feelings and emotions, taking as his example the mob, common to all cultures, and concluding that "there is enough of the savage in the civilized man, or of the civilized man in the savage — for as much is to be said for putting it in the one way as in the other — to render possible a genuine introjection, that is, a sympathetic entry into the mind and spirit of another" (Marett 1929c:174–75). Marett's approach was based on psychic unity, the notion that all forms of human life are open to all humans because of some essential component of the human mind. Most importantly, he recognized the common humanity of all peoples, valorizing different modes of life. While he was at least nominally an evolutionist in terms of the development of individual cultures, he did not arrange cultures into a hierarchy of value.

Marett's main interest was religion, and he conceptualized his work in this field in terms of translation. His goal was "to translate a type of religious experience remote from our own into such terms of our consciousness as may best enable the nature of that which is so translated to appear for what it is in itself" (1929a:xxiii). This work would contribute to "a generalized history of the evolution of Man" (1929a:xxiv). However, Marett did not offer a comprehensive definition of the term "religion," arguing that "it matters less to assign exact limits to the concept to which the word in question corresponds, than to make sure that these limits are cast on such wide and generous lines, as to exclude no feature that has characterized Religion at any moment in the long course of its evolution" (Marett 1900:163–64). The main factor, for Marett, was that religion is a function of culture; there is no such thing as an innate or inborn religion (1929b:135), and he concluded that material, as opposed to psychological, explanations of religion would always be "palpably incomplete and arbitrary" (1929b:129).

For all of his theorizing, Marett did not outline a research method as such. Cautioning that primitive societies do not have "a theology, or thought-out scheme of beliefs," he warned researchers to avoid "Why?" questions in favor of "What?" questions lest the researcher, on the assumption that the people have a systematized understanding of their own beliefs, "unawares extract from the native a sort of mock theology, made on the spot, and divorced from the facts of his real life" (Marett 1912:255). At the same time he argued against a theoretical separation of the concepts of religion and magic on the grounds that a firm distinction only becomes apparent "at a later stage of human progress"; as the goal of the researcher is to capture the Native "point of view, quite uncoloured by his own," Marett suggested the term "magico-religious" (1912:251).

The closest Marett came to outlining a method was in an entry on "The Study of Magico-Religious Facts" in the fourth edition of *Notes and Queries on Anthropology* (Freire-Marreco and Myers 1912), a handbook for anthropologists and amateur fieldworkers (Urry 1972). Marett offered a counterintuitive prescription, asserting that until a researcher is in "complete sympathy" with the psychology of the Native group being studied, "direct questioning of natives can only defeat the attainment of genuine results" in an understanding of the religion (1912:257). Rather, he suggested that

> the observer must watch quietly for the thousand-and-one little signs that betray the general state of mind, and manage, as it were, to overhear the unspoken feelings and thoughts that attend on the savage when he is intent on his own business. Of course,

> when it comes to putting these things down on paper, the observer will be obliged to render his impression of the mental attitude of the savage in the terms of civilized thought. Let him, however, take great care to discount the influence of the concepts and categories indispensable for himself as a civilized man, yet unexistent for the savage. [1912:257]

Besides his work on religion, Marett is perhaps best known for his early championing in England of the work of Emile Durkheim. What impressed Marett most was Durkheim's emphasis on the resiliency of the social group (Wallis 1957:789). Marett also noted with approval Durkheim's attempt to account for an entire social system:

> More significant still is the widespread movement, . . . led by Professor Durkheim . . . , in support of a method of Anthropology that lays due emphasis on the social factor. The old way was to arrive at the savage mind by abstraction. The sociologist of yesterday was content to picture what the outlook of a man like himself would be, should the whole apparatus of civilization have been denied him, including a civilized man's intellectual and moral education. Naturally his results bordered on romance. The new way, on the contrary, is to proceed constructively. Whilst full account is taken of the effects both of heredity and of the physical environment, yet the effects of the social environment are reckoned to be determinate in an even higher degree. The mass of cultural institutions, it is held, embody and express a kind of collective soul. In this social selfhood each individual must participate in order to realize an individuality of his own. It is a corollary that no isolated fragment of custom or belief can be worth much for the purposes of comparative science. In order to be understood, it must first be viewed in the light of the whole culture, the whole corporate soul-life, of the particular ethnic group concerned. Hence the new way is to emphasize concrete differences, whereas the old way was to amass resemblances heedlessly abstracted from their social context. Which is the better is a question that well-nigh answers itself. [Marett 1929c: 173–74; cf. Marett 1929b:129–30; Marett 1908:52]

Like Durkheim, however, Marett never undertook any ethnological fieldwork himself. When Stocking asserts that "Marett's work contributed much to the reformation of British social anthropology; much of what we associate with Malinowski and Radcliffe-Brown is in fact foreshadowed in Marett—whom around 1910 both had read" (Stocking 1995:170, 172–73), he means this in a theoretical, not a methodological, sense. Marett saw himself working in a Tylorian tradition, where "the man in the study busily propounded questions which only the man in the field could answer, and in the light of the answers that poured in from the field, the study busily revised its questions" (Marett 1932:173–74; cf. Kuklick 1991:265). In his section in *Notes and Queries* on collecting magico-religious information, Marett was more direct in his injunction to keep ethnographic data and ethnological theory separate (Marett 1912:253–54). More recent commentators, however, have remarked upon the artificiality of this distinction, associated with both Tylor and Frazer, between ethnographic facts and speculative comparison (Urry 1993a:43). Fieldworkers needed a strong grasp of theory in order to select the relevant data from the masses they collected, and theorists needed a strong grasp of ethnographic materials to select the relevant materials from the masses of data at their disposal.

However, by the end of the first decade of the twentieth century most of the British anthropologists responsible for the training of students recognized the importance of extended field research for the development both of young anthropologists and of the discipline (Urry 1984:48). In fact Marett asserted in his preface to Jenness's New Guinea ethnography that "touring, indeed, proves the ideal method of anthropological research" (Marett 1920a:7), though Marett himself did not undertake any extensive ethnographic research. Jenness commented in a letter to Barbeau that Marett was "a good philosophical anthropologist, but I don't imagine he would score very highly on field-work" (CMCB, box B206, file 27 [Diamond Jenness, 1912–14; 1917–24], Jenness to Barbeau, February 25, 1918). In any event by the 1910s the strict division between the armchair theorist and the intrepid fieldworker was rapidly being replaced by "fieldworker academics" (Stocking 1983:80) such as Malinowski and Radcliffe-Brown. Despite his experiences in New Guinea and the Arctic, however, Jenness remained entrenched in the former tradition.

Fieldwork in New Guinea ended Jenness's time as a student. After leaving Oxford he wrote to Marett: "My varsity career is all over now. Will you let me say how grateful I am to you for all your kindness throughout—for your staunch championship of my anthrop[ology?] 'mania' & for all the trouble you have had over the expedition. I must succeed in it, if only to justify your faith in me" (OUAM, Jenness to Marett, August 14, 1911). The troubles that Jenness mentioned were related to the financing of the expedition. As a research student, he needed £250 for his expenses, and Marett worked to raise the funds at the university; appeals for support emphasized the growing importance of fieldwork to anthropological training (e.g., OUAM, blank form letter, March 1, 1911).

Fortunately for Jenness, the funding eventually materialized, enabling him to finance a year of fieldwork, from December 1911 to December 1912, in the D'Entrecasteaux Islands of New Guinea. His intention was to learn the language and then to collect material culture for the Pitt-Rivers Museum at Oxford, along with anthropometric and ethnographic data (de Laguna 1971:248; Richling 1989:73). In the last case his emphasis was on the "social institutions" and "ritual and economic ties" of the residents as well as on "aspects of material culture: religion, mythology, and morality" (Richling 1989:73).

Jenness outlined two main reasons, albeit contradictory ones, for his selection of this part of New Guinea for his field research. First, the locale had not been subject to previous anthropological research, or, in fact, even to much exploration by white people, perhaps due to reports of cannibalism (Jenness and Ballantyne 1920:11). Second, it was the location of a Methodist mission run by his brother-in-law, Andrew Ballantyne (Jenness and Ballantyne 1920:11). Ballantyne, who spoke Bwaidogan and Dobuan, was familiar with the local cultures and peoples; he would become Jenness's interpreter and collaborator (Richling 1989:73), though he died before the ethnography was completed.

Even though he claimed that this region was largely unknown to the outside world, Jenness selected a cultural group that had already been drawn into the colonial sphere. Richling argues that there is "little indication that [Jenness] expected to meet pristine 'primitive' peoples, untouched by Western culture, in New Guinea" (Richling 1989:73). However, Jenness's rhetoric of an undiscovered corner of the world shows that he was willing at least to imply to his readers that these were poorly known people who had had few visitors from the outside world, rather than address the facts that he spent "most of his time around the Bwaidoga mission" (Richling 1989:73) and that "his native informants and acquaintances were typically well-versed in European ways, and heartily suspicious of colonial authorities, traders, and the like" (Richling 1989: 73). As well many villages, even those "no white man had even been in," were home to men who labored throughout the region for foreign companies (OUAM, Jenness to Marett, July 26, 1912). This region was not cut off from the larger world around it, an issue that resurfaces in his Arctic ethnography.

Jenness left behind no field notebooks, so it is difficult to reconstruct his field methods (Richling 1989:74). However, his correspondence with Marett contains some insight into his approach to gathering materials. Soon after his arrival, he explained that he accompanied Ballantyne on a series of "short excursions" to a variety of regions, including one to take a census and another to investigate charges of cannibalism (OUAM, Jenness to Marett, January 20, 1920), setting a pattern of work around the mission station interspersed with short journeys around the islands that he would continue for the rest of his time in New Guinea (OUAM, Jenness to Marett, April 11, 1912).[7] Three months later, Jenness outlined his typical daily activities in and around the mission station:

> This is how I'm working at present. Rise about 6:30, breakfast about 8. When possible we have 2 or more of the old men in at the station to talk of their customs etc. (For the last week however they have all been away looking for food in the bush or fishing on the reef or visiting round the coast trying to buy food with tobacco we supply them with. For many of the natives have been sorely pressed, & some of the children & old folks would certainly have died had we not fed them with our own rice and biscuits . . .). About 1 pm we lunch then I go off map-drawing or visiting the villages or taking photos or something. The evening is taken up with writing and reading. Every now & then we take a whole day & go off to more distant villages. We are hoping soon to visit the Amphlettes; also the people in the hills in the middle of Fergusson who have never really been visited. Once or twice a government officer has tried to get at them but they have invariably fled. [OUAM, Jenness to Marett, April 11, 1912]

Generally speaking, Jenness relied upon a few informants at a time, "2 or 3 men who best knew the customs," to discuss a given topic. Sometimes he used a question-list developed by J. G. Frazer (1907), though "it only supplied broad lines of enquiry" (OUAM, Jenness to Marett, July 26, 1912).

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

> I seemed to get right down into native life. We had sing-songs at night—I copied down many of them. It was weird to sit in the circle round the fire with 20–30 natives about me swaying their heads & bodies to the tune of some mournful chant. Living & sleeping with them the barriers appeared to be broken down. They spoke quite freely of their customs—in fact, took pains to point them out to me. As for songs & legends I have quite a notoriety among them. . . . I think this proves the natives have confidence in me & regard me more as one of themselves. [OUAM, Jenness to Marett, July 26, 1912]

However, this confidence on Jenness's part was short lived. Later in the same letter, he admitted that "sometimes I fear I have not got into the real native life — it all seems too open & straight-forward but I think I have. I can't think 'native' tho' as I suppose one ought to, much as I try. Oxford skepticism is too much for me" (OUAM, Jenness to Marett, July 26, 1912). He was unable to apply Marett's method.

Jenness realized that he had not grasped the complexity of the Bwaidogan culture. Three months earlier, he had written to Marrett that "society here seems very simple. There are practically no traditions — their memory reaches at the best to a vague recollection of the days of their grandfathers" (OUAM, April 11, 1912). He identified classificatory kinship, patrilineal descent, and taboos and their methods of inheritance (OUAM, Jenness to Marett, April 11, 1912), concluding that "property seemed at first equally simple — descending regularly to the eldest son. But in working out two or three cases genealogically curious anomalies arose which so far remain unfathomed" (OUAM, Jenness to Marett, April 11, 1912), in particular, cases of exogamous marriage without prescriptive rules for postnuptial residence. In any event he was able to conclude that "I believe the information I am getting is sound. Ballantyne knows the language well & we are taking great pains to check" (OUAM, Jenness to Marett, April 11, 1912; cf. OUAM, Jenness to Marett, December 11, 1911, January 29, 1912; CMCB, box B206, file 27 [Diamond Jenness, 1912–14; 1917–24], Jenness to Barbeau, May 6, 1912).

Jenness's work with his brother-in-law was an unexpected help to his research. Upon arriving in New Guinea, Jenness discovered an equally unexpected hindrance to his work. The islands on which he concentrated his research were undergoing one of the worst famines on record (OUAM, Jenness to Marett, July 26, 1912). Jenness lamented that "the famine makes anthropological work very slow. The natives spend every hour of the day in their gardens or hunting for food in the bush or fishing on the reefs. Still it is going slowly ahead" (OUAM, Jenness to Marett, May 4, 1912). At one point he asked Marett plaintively, "why did not I come a year earlier? — it would have made anthropologizing much easier" (OUAM, Jenness to Marett, July 26, 1912). Richling argues that this famine led Jenness to develop "an appreciation for the precariousness of local subsistence, the fine line between well-being and disaster, and the mitigating role of mutual aid" (Richling 1989:73).[8] These lessons would come in handy for Jenness during his time in the Arctic.

Preparation of the D'Entrecasteaux manuscript for publication was delayed by several unforeseen events: the Arctic trip, participation in the Canadian Expeditionary Forces (CEF) from 1917 to 1919 (Marett 1920a:

8), and Ballantyne's untimely death (Jenness and Ballantyne 1920:12). On leave from the CEF during demobilization, Jenness returned to Oxford to finish the manuscript (Collins and Taylor 1970:74). *The Northern D'Entrecasteaux* was a rather unremarkable ethnography, by Jenness's own assessment (CMJC, box 648, file 6 [A. L. Kroeber, 1930–39], Jenness to Kroeber, January 11, 1932). Its most interesting feature is what it reveals about Jenness's views and values: his desire to depict the New Guinea peoples in rhetorical terms that reflected "civilized" ways on the one hand and "savage" ways on the other hand. Jenness emphasized the latter, based on what he saw as a lack of common-sense knowledge and a preference for cannibalism.

Jenness's rhetoric covers all and sundry practices. Villagers rouse wild pigs from their hiding places "with their musical shouting, like English boys who call out in the early morning to frighten the birds away from the corn" (Jenness and Ballantyne 1920:20). Men (and, as it turns out, women as well) tally their successes at romance in the same way that war veterans and Indians count their victories in war medals and scalps, respectively (1920:61). A man with ceremonial knowledge or power "may set up his name-plate and advertise his special department" (1920:73). In his discussion of land ownership, Jenness employed English concepts such as "land titles," "alienation," and "usufruct" (1920:71–72), using Ballantyne to describe Native practices rather than attempting to interrogate the Native concepts.

The congruency between the Native culture and the "English" culture with which Jenness so readily identified began to crumble. For instance he asserted that "even from our standpoint the natives would be regarded as exceptionally clean people, but for one or two customs that seem repulsive" (Jenness and Ballantyne 1920:206). He highlighted certain groups' "improvidence," for example (1920:208), and characterized the Natives as impudent children (1920:125). Jenness never questioned the insights or purported superiority of his own "English" approach to the world; if only the Natives were more like Englishmen, they would be more "successful" (1920:45). It appears that Jenness was practicing a variant of what Marett called "old sociology," where the researcher "was content to picture what the outlook of a man like himself would be, should the whole apparatus of civilization have been denied him, including a civilized man's intellectual and moral education" (Marett 1929c:173). In contrast Marett argued, based on his reading of Durkheim, that "no isolated fragment of custom or belief can be worth much for the purposes of comparative science. In order to be understood, it must first be viewed in the light of the whole culture, the whole corporate soul-life, of the particular ethnic group concerned. Hence the new way is to emphasize concrete differences, whereas the old way was to amass resemblances heedlessly abstracted from their social context" (Marett 1929c:174). Jenness's use of rhetoric, as a function of his perception of the Natives as capable of being just like Englishmen with the aid of a little civilization, shows how much his approach differed from that of his teacher.

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

> It seemed to us that, taken in the mass, they are not markedly inferior to white people whenever their interest is aroused. They are keenly observant of all natural phenomena, and there are few birds or fish or plants whose name even a small boy does not know. This closeness of observation is especially noticeable in all that pertains to fishing and to the gardens, and has led to the creation of an extensive vocabulary connected with these pursuits; the varieties of yams and shell-fish for example appear to the foreigner numberless. Some natives, again, display wonderful accuracy in locating sounds, and without the slightest hesitation will lead the way through half a mile of dense forest to the exact tree on which a blue pigeon sits cooing. [Jenness and Ballantyne 1920:52]

These people had come to terms with their environment and developed a set of tools with which to make the most of their surroundings. Yet Jenness dismissed them as stupid when they showed no desire to learn the ways of his world:

> No one, however, can be more stupid than an uninterested native. Many things which appeal to us have no interest whatever for him. At the mission station the best boat boy and the best hunter could never be taught his twice times tables. He tried, and tried hard, but somehow it did not appeal to him, and he would forget to-morrow all that he learned to-day. Another lad, of a rather similar type, by sheer application and force of will learned his arithmetic tables, because he knew that otherwise he could not go up to the mission station at Ubuya. [Jenness and Ballantyne 1920:52–53]

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

While disturbing in contemporary terms, this discussion shows that Jenness felt the Natives could still become English, provided that they set their wills to the task. However, when he shifted his attention to cannibalism, the Natives ceased to be Englishmen manqués and became complete and total savages. Writing about a famine that hit the area in 1900, Jenness describes a time when cannibalism was so prevalent that "it was dangerous for a child to leave his parent's side for a single moment lest he should be carried off to swell the cannibal pots" (Jenness and Ballantyne 1920:32). He detailed "the last case" of a cannibal feast to occur in one of the villages he visited, where after a gruesome depiction he concludes that "each family received a portion, which it cooked like ordinary meat, and every one down to the smallest child shared in the feast" (1920:88). In the process of killing and eating a victim, the Natives turned from average, if stupid, people into inhumans, from people who ate normal food into inverted people who treated the most unnatural type of meat, human flesh, as if it were any other kind of meat. The only other people less human were the women who engaged in cannibalism outside of the sanctioned revenge feasts, because "they derive no magic power apparently from their ghastly banquets" (1920:119). These women operated beyond the standard system of value, seeming to gain nothing from their "ghastly" routine (therefore making them inhuman?); even the participants in the sanctioned cannibal feasts gained the satisfaction of revenge against a vanquished foe. Jenness made no attempt to understand what motivated these women, although even other members of the community did not seem to understand what motivated them. On the other hand community members might have been unwilling to share their knowledge with Jenness, a problem he encountered also during his time in the Arctic.

Jenness's ethnography was largely ignored when it was published. Malinowski cited his work in *Argonauts of the Western Pacific*, where he disparagingly challenged Jenness's interpretation of missionary influence on the cessation of meaningless religious customs: "It is strange to find a trained ethnologist, confessing that old, time-honoured rites have no meaning! And one might feel tempted to ask: for *whom* it is that these customs have no meaning, for the natives or for the writers of the passage quoted" (Malinowski 1922a:467 n.).[9]

On the other hand Alfred Kroeber was more positive in his assessment, sending Jenness a short note in early January of 1932: "I want to congratulate you belatedly on an unusually fine piece of work. Not only are the data good, but the writing is compact, pregnant, and well-rounded. I feel you have done easily one of the best pieces of work extant on Melanesia, and am sorry I had not made its acquaintance before" (CMCJ, box 648, file 6 [A. L. Kroeber, 1930–39], Kroeber to Jenness, January 4, 1932). In his response to Kroeber, Jenness characterized his New Guinea manuscript as being the hurriedly written and revised result of his first fieldwork experience (CMCJ, box 648, file 6 [A. L. Kroeber, 1930–39], Jenness to Kroeber, January 11, 1932). He would come to see the results of his second fieldwork trip, to the Arctic, as much more substantial.

The Canadian Arctic Expedition

For almost a century, between the 1840s and the 1920s, a number of scientific exploring parties joined whalers in bringing the wider world to the Arctic. Before this time commercial needs had motivated Arctic exploration, searching for a trade route from Europe to Asia (Cooke 1981:53). However, the fact that most of these expeditions took place under foreign flags raised serious issues about who held sovereignty over the Arctic, particularly given that so much of it remained unexplored (Zaslow 1971:251). By the 1870s the British government was receiving requests for land grants in the Arctic, usually from Americans. The British did not want to get involved but feared that if they "disclaimed jurisdiction, the United States would immediately claim the territory for itself and interfere with future Canadian expansion in that direction" (Zaslow 1971:251–52). Canadian politicians were divided over a British offer of jurisdiction over the area, and formal transfer was delayed by indecision on both sides, with the British wondering about the most desirable way to transfer control and the Canadians wondering if they wanted control at all (Zaslow 1971:252–54). Finally in 1895, after more than 20 years of discussions and delays, a bill was passed in the Canadian parliament "constituting the Provisional Districts of Ungava, Franklin, Mackenzie, and Yukon" (Zaslow 1971:255). There were still questions about jurisdiction, however, as the Canadian government had replaced a series of vague proclamations and laws with one that was equally unclear about what, precisely, was being claimed.

Concerns over the potential of continued challenges to Canadian sovereignty in the Arctic finally forced the federal government into action. In 1897 the government sent an expedition to Hudson's Bay and Baffin Island (Zaslow 1971:255, 259); in 1903 it established North West Mounted Police posts on Hudson's Bay and at the mouth of the Mackenzie River (Zaslow 1971:262); and in 1906 it sent a ship north to enforce recent legislation calling for all whalers to be licensed by the Canadian government (Zaslow 1971:265–66).

One by-product of the continued concern for strengthening Canada's claim to sovereignty over the Arctic was the Canadian Arctic Expedition. Vilhjalmur Stefansson, an "ambitious, headline-hunting anthropologist" (Zaslow 1971:247; cf. Collins 1964) who had just returned from leading a four-year expedition to the Arctic (Zaslow 1971:246; Diubaldo 1978:57), was searching in 1912 and 1913 for financing to mount his next trip north. He had received offers of funding from the American Museum of Natural History and the National Geographic Society (1978:58–60), but also looked to the Geological Survey of Canada (1978:62), a subsection of the Department of Mines. The federal government took over entire responsibility for the endeavor, renaming it the Canadian Arctic Expedition.

This decision was motivated largely by politics. The recently elected Conservative government under Robert Borden wanted a program of northern research to match the one that the previous Liberal government had undertaken in the eastern Arctic (Diubaldo 1978:63). Sovereignty was still an issue. The government feared that Canadian claims to the territory would be adversely affected by foreign expeditions finding uncharted land in the north; the mandate of the expedition explicitly emphasized the search for new land (Diubaldo 1978:64; Zaslow 1981:63). Finally, the Geological Survey wanted to expand its scientific research into new areas of Canada (Zaslow 1981:63) and was particularly interested in studying "the remaining primitive Eskimo bands not yet completely transformed by contact with the white man" as part of its project to expand "the anthropological and ethnological sides of its museum activity, as well as pushing geological mapping and studies beyond their present northerly limit" (Zaslow 1971:272).

The expedition was marked by divisions and conflict from the beginning. Planning was a tremendous undertaking, and the necessity of departing by May 1913 meant that many corners were cut, with many of the decisions deferred to Stefansson, much to the later regret both of the government and of the scientific staff. As it was, responsibility for organization was divided between the Naval Service and the Departments of Marine and Fisheries, Interior, Customs, and Mines (the home department of the Geological Survey) (Diubaldo 1978:66). The expedition was broken into two parts: the Northern Party, under the command of Stefansson, which was to focus on exploration and the discovery of new land; and the Southern Party, under the command of the zoologist R. M. Anderson, which was to concentrate on scientific work and whose staff was to report to the Geological Survey (Diubaldo 1978:74). This division of duties led to conflict both in the field and at the administrative level, where the Naval Service emphasized the exploration aspect and the Geological Survey stressed the scientific component; Stefansson's priorities lay with the former (Zaslow 1975:321).

The scientific staff was impressive in both size and scope. It included marine biologists, botanists, meteorologists, topographers, a photographer, and anthropologists. Anderson, the highest-ranking member of this group in the field, served as commander. Jenness and Henri Beuchat were hired as the anthropologists on this team.[10] The scientific staff did not appreciate Stefansson's severe manner of command, leading to division in the ranks of the expedition; discontent with Stefansson reached such a point that he accused the scientists of mutiny at least twice. To make matters worse, while Stefansson and some others were off hunting, the expedition ship *Karluk* became embedded in the encroaching ice and was carried off, eventually foundering off Wrangell Island north of Russia. Of the 28 people on board 16 died, either before the ship sank or while trying to cross the ice to land afterward. Among those left on the ailing ship were most of the scientific members slated to join the Northern Party (Diubaldo 1978:83). Stefansson attempted to commandeer most of the remaining resources for his Northern Party, again leading to a great rift with the scientific staff. By the first spring in the Arctic, tensions between Stefansson and the Southern Party had reached a critical point, beyond compromise, exacerbated by poor planning and confusing instructions received from the government (Diubaldo 1978:101).

Due to ice conditions and the loss of the *Karluk*, the expedition spent its first winter in the Arctic among the Eskimos of far northeastern Alaska. Jenness began his research in earnest, but he found this work in Alaska difficult due to what he perceived as the negative effects of years of contact with whites. The Copper Inuit had been selected for this project because they were believed to be, contrary to the Alaska Eskimos, largely free from such outside influence. Jenness himself stated upon his return that the Copper Inuit were "the only branch of the Eskimo race which still retained its primitive mode of life unaffected by the great world beyond" (1917b: 392). He aspired to a rapid, comprehensive recording of "traditional" Copper Inuit life and culture before it was lost, a project with which Anderson concurred (1917:329). It requires close reading to see that the Copper Inuit were already undergoing changes due to prolonged contact. Jenness records them in his diary and published reports but usually only in passing. Contradictions abound in his ethnography: sometimes the Copper Inuit are presented as pristine examples of a precontact primitive society; at other times they are depicted as debased examples of culture contact and loss.

Jenness's research was not only of scientific value; it also offered practical assistance to the Southern Party. One of Anderson's annual reports praised Jenness's "linguistic abilities and acquaintance with the Eskimo character," which made him the most suitable "official purchasing agent for the expedition in practically all business transactions with the local natives, including the purchase of meat, fish, and clothing" (Anderson 1916:224). He also acted as the expedition's interpreter.

On account of the war in Europe, the Southern Party of the expedition was ordered home in 1915. However, due to the difficulties of communication and travel in the far north, it was the summer of 1916 before the order was obeyed, ending the three-year trip (Zaslow 1971:275). Stefansson and the Northern Party stayed in the Arctic for another two years, traveling and mapping the islands of the Arctic archipelago. Jenness worked in Ottawa for a short period, writing up his reports, before enlisting in the CEF and serving in France from 1917 to 1919.

It is unclear why Jenness chose to participate in the Canadian Arctic Expedition. He had shown no familiarity with, or even interest in, Canada (Richling 1989:75), preferring instead to work in the South Pacific. His goal was to return to the latter after the war (Richling 1989:75; OUAM, Jenness to Marett, October 17, 1913, January 6, 1915). However, the difficulty of finding satisfactory employment after returning from New Guinea, combined with the opportunity to work with someone of Stefansson's stature, made the Arctic attractive (Richling 1989:75). "The salary," he reported to Marett, "was not princely — expenses + 500$ a year while in the field & a salary (unstated) while working up the report. But it was the only thing except teaching that was offering, and I understand the Expedition is rather important and likely to lead to something afterwards" (OUAM, Jenness to Marett, March 9, 1913).

Sapir, who, through Barbeau, had invited Jenness to participate in the Canadian Arctic Expedition (Richling 1989:74), was an enthusiastic supporter of the expedition. Its focus on ethnography fit with his goals as chief of the Anthropological Division of the Geological Survey:

> Now or never is the time in which to collect from the natives what is still available for study. In some cases a tribe has already practically given up its aboriginal culture and what can be obtained is merely that which the older men still remember and care to impart. With the increasing material prosperity and industrial development of Canada the demoralization or civilization of the Indians will be going on at an ever increasing rate. No short-sighted policy of economy should be allowed to interfere with the

> thorough and rapid prosecution of the anthropological problems of the dominion. What is lost now will never be recovered again. [Sapir 1911:793]

Sapir's instructions to Jenness emphasized salvage. He urged a comprehensive survey of traces of precontact Inuit life, physical characteristics, and material culture:

> The main part of your work is to be the collection of a full ethnographic material, based on study and observation among the Eskimos of the Arctic region. In connection with your research work, it would be advisable for you to assemble rather full ethnographical collections from the various tribes visited, these collections to be forwarded to the Victoria Memorial Museum at Ottawa. As complete data as possible should also be obtained on the physical characteristics of the natives visited, including systematic anthropometric data. . . . Inasmuch as the technology of the Eskimo has been more fully studied than any other phase of their culture, it is suggested that you concentrate as much as possible on the non-material side of culture, including such topics as religion, shamanism, social organization, and various beliefs and customs. [CMCS, folder "Jenness, Diamond 1913–1919," Sapir to Jenness, March 6, 1913]

This work was to be divided between the expedition's two anthropologists—Jenness and Beuchat. Jenness had asked Sapir about undertaking linguistic work, and Sapir responded enthusiastically, outlining his views of the value of such work: "Of course, the very best sort of ethnological material that you can get would be texts obtained from dictation. Such texts are apt to be extremely valuable, not only in studying mythology, but also other aspects of ethnology, particularly rituals and religious ideas" (CMCS, folder "Jenness, Diamond 1913–1919," Sapir to Jenness, May 7, 1913). Less than six weeks later, however, Sapir wrote to Jenness stating that if the two ethnologists were forced to work in the same area, "it is perhaps as well that M. Beuchat is to do most of the linguistics while you are to undertake all the anthropometric work" (CMCS, folder "Jenness, Diamond 1913–1919," Sapir to Jenness, June 19, 1913). In light of later comments on his lack of "any special training in linguistics" (Jenness 1916:612), Jenness likely found this arrangement most agreeable,[11] though the disappearance of the *Karluk* and the subsequent death of Beuchat made the arrangements moot.

Because none of Jenness's field notes are extant, the only way to assess his fieldwork is through his diary (Jenness 1991) and correspondence.

Diary keeping became one of his strengths, as he revealed to Marett, and he wished that he had kept a similar record of his time in New Guinea (OUAM, Jenness to Marett, June 29, 1914). The Arctic diary is rich in detail, containing descriptions of incidents and activities that elucidate Jenness's methodology and theoretical orientation. In his preface Jenness's son explains the nature of the diary:

> Not simply a routine account of a series of chronological events, my father's daily entries also provide a view of the feelings and responses of an idealistic, sensitive, and dedicated young scientist thrust into dire living conditions in a culture totally foreign to any he had known previously. The three-volume diary [manuscript] is also, of course, an extraordinary account of a very modest man's industriousness and perseverance in carrying out far more than was expected of him, in spite of a multiplicity of delays, frustrations, perilous experiences, and recurring ailments. [Stuart Jenness 1991b:xx]

The diary offers a personal view of a generally private man. Though Sapir had promised Jenness that the contents of the diary would remain confidential (CMCS, folder "Jenness, Diamond 1913–1919," Sapir to Jenness, May 20, 1913), Jenness "carefully refrained from [recording] anything personal against members of the expedition, or anything of that nature" (OUAM, Jenness to Marett, August 2, 1914). Countermanding earlier instructions from Ottawa, Stefansson demanded access to the private journals kept by the members of the expedition to gather ethnographic data from them (Stuart Jenness 1991b:xxi). In spite of this constraint, Jenness's diary allows access to his relatively unguarded reactions to his daily life and events.

Stuart Jenness's romantic view of his father's time in the Arctic is characteristic of the general perceptions of Jenness's fieldwork, which have emphasized the difficulties associated with long-term residence among the Inuit. For example Henry Collins and William Taylor describe his work in Alaska in all of its arduous detail, arguing that the disappearance of the *Karluk* "was the inauspicious beginning of Jenness's Arctic career. Few young anthropologists have faced such difficulty in beginning field-work in a new and unfamiliar area; yet none, surely, has emerged from the test with a more brilliant record of work accomplished" (1970:72). Stuart Jenness contrasts the harsh living conditions his father faced with the latter's moderate descriptions: "Throughout the diary there is a genteelness of prose in his descriptions of the primitive living conditions he was experiencing and few expressions of complaint or criticism (although these would have been perfectly understandable considering his almost daily hardships, repetitious and often dreary routines, frustrations, and interpersonal irritations)" (1991b:xxi).

In contrast, rather than focusing on the details of Jenness's travails in the Arctic, I highlight the implications of what he chose to elide and downplay. Following Kulchyski, I suggest that the diaries themselves constitute an "ideologically rich text, frequently providing glimpses and interpretive threads that seem to go against the grain of its overall impulses; a text that often offers its revelation in spite of itself" (Kulchyski 1993:39).

Collins asserts that Jenness "faced a challenge and an opportunity rarely offered [to] a 20th century anthropologist," namely, the chance to study "a virtually unknown people who had been brought to the attention of the scientific world only two years previously" (1971:9). While his New Guinea fieldwork offered limited opportunities for participant-observation research, in the Arctic "social intimacy, like cooperation in the daily round of subsistence activities, was inseparable from the work of anthropology" (Richling 1989:74).

Jenness's exposure to Inuit life began almost immediately upon his arrival in the Arctic. During the first winter and spring in Alaska he lived part-time with Eskimo families near Point Barrow (CMCS, folder "Jenness, Diamond 1913–1919," "Summary Report Covering the period from Sept. 1913–July 1914"), an approach that met with Sapir's approval (CMCS, folder "Jenness, Diamond 1913–1919," Sapir to Jenness, June 22, 1914). The first major problem was lack of equipment. Jenness had left most of his anthropological instruments, papers, and books on the *Karluk* (CMCS, folder "Jenness, Diamond 1913–1919," Jenness to Sapir, October 26, 1913, May 30, 1914). His research was handicapped, because the only way he saw to recover the Eskimo's "ancient customs" was through their language, which he found extremely hard to learn, especially without his reference books (CMCS, folder "Jenness, Diamond 1913–1919," "Summary Report Covering the period from Sept. 1913–July 1914"). In spite of the complexity of the language, Jenness set to work simultaneously to learn it and describe its grammar (OUAM, Jenness to Marett, June 29, 1914). He outlined his approach to Marett and explicitly discussed the difficulties he faced in his work; he was constantly revising his orthography and had great problems distinguishing phonemes (OUAM, December 2, 1913).

Many of the Eskimos whom Jenness met in Alaska reflected the blending of Eskimo and outside cultures. Of the first Eskimos he saw, while searching for the *Karluk*, he noted that "all of these people had flour, tea sugar matches etc primus stoves, kerosene, frequently sewing machines, besides of course rifles and shotguns" (OUAM, Jenness to Marett, October 17, 1913), showing how widely outside items had entered the far north. He commented on the influence of Christianity, remarking that no work was done on Sundays and that crosses marked recent graves (Jenness 1991:40, 10).

Jenness also frequently hypothesized about the racial composition of individual Eskimos, based on their physical appearance. For example he noted that "one child appeared to be half-Eskimo, half-Polynesian, judging from its appearance" (Jenness 1991:32). A young woman who "was very good looking — very different from the ordinary Eskimo type . . . resembled rather the Arab or North African type. Probably she has foreign blood in her" (1991:37). The most striking comment was his assertion that "Aksiatak has quite a Roman nose, his face is long and flat, the chin almost pointed, but there is no doubt that he is of pure Eskimo descent" (Jenness 1991:67). It is unclear how Jenness could make such definitive judgments based on so little time in Alaska, unless he relied upon superficial judgments based on his own prejudices.

However, Jenness was careful to note that he was working with a family relatively free from outside influences, both physically and culturally (OUAM, Jenness to Marett, October 17, 1913); Jenness alludes to a theme that would dominate his Arctic research, that is, the discovery of islands of cultural "purity" in an otherwise inundated world. In the sea that is culture change — culture loss, for the Eskimos — he found an island relatively free from the deluge of the outside world. In a letter to Sapir, for example, he explained that "the two families with whom I am staying are inland Eskimos from the Colville River region, & have come less into contact with the whites than most of the Eskimos here. One of them Aluk is reputed to be well acquainted with the old songs & traditions, but is said likewise to be unwilling to talk about them" (CMCS, folder "Jenness, Diamond 1913–1919," Jenness to Sapir, December 2, 1913). These two themes — finding informants who were less affected by outside influences than their fellow Inuit, but who were less than willing to impart all that they knew — would recur throughout his time among the Copper Inuit as well.

A third theme of the Alaskan work was Jenness's comparison of Eskimo culture with English culture. For example, although "Eskimo manners at 'table' seem rather strange to a European" (Jenness 1991:129), "most families appear to have a small 'table cloth' (more correctly perhaps 'food cloth' for it is laid on the floor) of what we commonly call oil-cloth. It is kept very clean, as cleanliness goes here" (1991:44). Like Europeans, they also displayed a "marked" affection for their young children: "They play with them, hug them, and in general behave towards them just as English parents do" (1991:45).

At other points Jenness was a harsh critic of the Eskimos, regularly lapsing into broad generalizations. He often referred to them as children or childlike; for example he described their seeming cruelty to animals as the result of "a child-like thoughtlessness which permits them to torment an injured bird or thrash unmercifully a dog which has provoked them," and he extrapolated that "they are unable, I suppose, to project themselves out of themselves — to love their neighbours as themselves" (1991: 244–45). Later, he accused them of an acute lack of foresight, stopping just short of calling them stupid for not planning ahead and describing their waste of ammunition and fuel as a character flaw (1991:216).

Later commentators focus on how Jenness arrived among the Copper Inuit, in late July 1914, "just in time" (Collins and Taylor 1970:73) as the Inuit were on the cusp of change. Trading ships had begun to visit the area, bringing goods from the south (Collins and Taylor 1970:73–74), but the arrival of outside influences had little impact upon the Copper Inuit:

> Fortunately, these beginnings of change in the Eskimo's economy had no serious effect on Jenness's work. The rifle was coming into use, to be sure, and a few of the Eskimos were beginning to trap white foxes, but caribou were still being hunted with bow and arrow or speared in the water from kayaks. And in its nonmaterial aspects their culture remained unchanged. Thus in the two years that he lived among them Jenness was able to observe and record the life of the Copper Eskimos as it had existed for centuries or millennia before the white man's "civilization" had reached them. [Collins and Taylor 1970:74; cf. Tepper 1983:4]

Jenness's goal — something no other ethnographer had done before — was to accompany a family through its summer rounds. As a result of his plan to travel with the Inuit during "the long period in which small, flexible, highly mobile family groups rely upon fish and caribou for their livelihood," he "was confident that his observations would make a significant and original contribution to northern ethnology" (Richling 1989: 79). Between April and November 1915, he lived with an Inuit family, justifying this approach on the grounds that "it is better ethnologically to spend a summer with the band I am with and watch their summer life than to run around the country, now meeting them, now alone" (Jenness 1991:447). He assumed the role of an adopted son in the family with which he traveled, though he noted that "in my case the adoption is very special — more in the nature of a business proposition if they understood what that meant" (1991:463). The family had been promised an array of goods, to be given when they returned Jenness safely in the fall.[12]

In a self-congratulatory summary report sent south near the end of his Arctic stay, Jenness outlined the advantages of long-term intimate fieldwork. His travels with a family allowed him to clear up misunderstandings about Inuit summer lives. He spend the months

> sharing their life in all its details, living in the same tents, hunting and fishing with them to obtain our common food, and accompanying them in all their movements. The information thus acquired proved beyond doubt that the old theories concerning their social and religious life during this period are entirely erroneous, at least as far as this branch of the Eskimo race is concerned. While it is difficult, perhaps impossible, for a civilized person fully to understand the mental attitude of a savage people towards the phenomena of life, yet the many shamanistic performances which I witnessed, and in many cases took part in, leave a general notion concerning their religious life which cannot be far from the truth. [Jenness 1917b:614–15][13]

Jenness's notes on his Inuit family display a certain tension. The Inuit had been subject to a great deal of outside influence, both from other Aboriginal groups and from southerners (CMCS, folder "Jenness, Diamond 1913–1919," Jenness to Sapir, January 15, 1915). Anderson also reported on Jenness's research in this regard, noting that Jenness found "that these groups are not as definite as was formerly supposed, in fact the groups are pretty thoroughly mixed, both by intermarriages and by families shifting from one group to another, nearly every group containing individuals from other groups more or less remote" (1916: 230).

Jenness was also aware of the impact of southern culture on the Copper Inuit: "The presence of white men, and of their new tools and ideas, posed serious challenges to traditional Inuit concepts of order and action" (Richling 1988:16). Jenness himself noted that

> special attention was paid to the material culture of the Copper Eskimo and a large collection made of their weapons, household utensils, and clothing. These are rapidly being changed through the influence of the western Eskimo and of the whites. Already the natives have an abundance of iron to replace their copper; rifles are beginning to supersede bows and arrows; European pots and tin cans take the place of stone pots; garments of cloth are in great demand; and even the style of clothing is undergoing change. For

> this reason a special endeavour was made to procure numerous specimens of those objects which were most likely to suffer modification or disappear entirely. [Jenness 1916:613]

Obviously, Jenness was cognizant of the significant changes occurring in the lives of the Copper Inuit; however, as I will discuss below, his ethnographies usually present a static portrait of a pure Copper Inuit culture. This denial appears in a letter to his former colleague at the museum, Sir Francis Knowles: "I do not think that the transition from the bow to the gun had affected their archery when I was there. There were only five rifles in the country when we arrived and they had been obtained only two years previously from the trader. The vast majority of the Eskimos had never touched a rifle" (CMCJ, box 647, file 55 [F. H. S. Knowles, 1926–1941], December 28, 1928), but the vast majority of Inuit Jenness met seemed familiar with firearms.

During his fieldwork Jenness was often annoyed and disgusted by the actions of his hosts. He was constantly reprimanding them, complaining that they "seem not to have developed a sense of gratitude" (Jenness 1991:352) and that they "will beg and clamour for anything they fancy, like children without least shame or hesitation" (1991:341). If a man were annoying, Jenness wrote, then the only way he could put up with him was if his wife "is useful sewing and mending" (Jenness 1991:460). When he could, he incited his companions to behave in ways he found more appropriate. Jenness was concerned with the peoples' treatment of their family members and used his control of "luxury" items to influence behavior. He reported that "I spoke to Ikpuk today about Kannayuk's fearing to sleep in their tent because they beat her. He told the others, and they thought it rather a joke, saying it was the custom; however, I assured him I should be very angry if it continued and believe it will cease. They dare not offend me because I control the supply of ammunition and other desirable things, and can refuse to allow them any this winter" (Jenness 1991:507). In these and other cases (e.g., Jenness 1991:511), Jenness unabashedly tried to influence the Inuit, forcing them to adapt to his preferences lest he withdraw his largesse.

At times Jenness spoke explicitly of wanting to teach the Inuit lessons. For example he seized the rifle of a man suspected in the theft of some ammunition (Jenness 1991:582); giving it back after the man had proved his innocence, Jenness remarked in his diary that the man "has received a good lesson, if nothing else, as indeed have all the Eskimos round here" (1991:584). Jenness also refused to trade with individuals or groups suspected of stealing from the expedition's supplies (Jenness 1991:566, 581). He was careful to punish the transgressors in ways that suited their individual characteristics: "Patsy told me tonight that Niptanaciak was implicated in the stealing of the pemmican the other day. I taxed her with it and she admitted it, saying that she and the others are hungry. I tried to make her feel a little ashamed, the correction which seems to be most suitable in unimportant cases of this kind, for really some of them are like *children*" (Jenness 1991:563–64, emphasis added).

Jenness worked to keep himself aloof from the Inuit. Not only did he try to make it clear that his supplies were not to be plundered in times of hunger, as Inuit caches often were, but he sought to exclude himself from certain elements of reciprocity. After a successful hunting trip, he wrote that he "presented two of the caribou skins, heads, leg bones, and carcasses to the *Kanghirjuarmiut*. I told them that it was a free gift, but they each made me a present, one of deerskin socks, the other of winter boots. They offered me more but I declined" (1991:449). Jenness wanted to keep the Inuit at some distance and in his debt, rather than accepting some items in exchange, closing the circle of reciprocity, and binding him to the group.

Though he was their guest, Jenness often acted as if he were doing the Copper Inuit a favor by being with them. He complained about their willingness to let him contribute more than he thought was his share of caribou to the group, writing that "I don't like the prospect of their depending on me for hunting, but can't very well avoid it. However, I am obtaining some good ethnological notes, and the more there are to travel with the better opportunity there is of seeing native life" (1991:429). He worked to avoid fostering the community's dependence on him, refusing to hunt if others were unwilling to join him, hoping that some of the families in the area would leave to find food elsewhere (1991:428).

Jenness recognized an obvious hierarchy of Inuit personality types. At the bottom were the childlike, insolent individuals who taxed him with their constant demands and petty thievery. Those at the top were docile and more respectful. The latter group included a married couple who were "very quiet and decent—keeping away from everything but doing any little thing we want" (1991:574), and others who were "real treasures compared to those we have met [to the] west—not officious or bothersome, and perfectly honest" (1991:569).

His time with the Inuit challenged his values. The incident that affected Jenness the most was an exchange of wives he witnessed soon after meeting the Cooper Inuit:

> Itoqunna, I believe, slept with Niq's husband Akhiatak—an exchange of wives for the night. . . . No words passed between

> the two women, but when Itoqunna entered, Haviuyaq laughingly asked me "where is Itoqunna?" — alluding to my question of the night before, whereupon everyone laughed. I do not know if everyone exchanged wives last night, though Haviuyaq asked me if I still wanted to sleep alone. The custom is, of course, well known among *savages* from books, but strangely enough it shook my nerves more than anything else I have seen in the Arctic. . . . Itoqanna resumed her usual place in the house today and is sleeping with her husband tonight. I have not dared to enquire yet whether it was in connection with the sealing — though I feel rather ashamed of my weakness in this respect as an ethnologist. [Jenness 1991:350, emphasis added]

A month later, Jenness wrote that "one of the women offered to sleep with me tonight, but I declined. Like the others these people cannot understand a man not wishing that sort of thing" (1991:370). The third time the topic was raised, Jenness managed to add a comment expressing his sense of superiority over not only the Inuit but also over the other southerners who would soon be heading north:

> Last night Ikpuk and Tucik were talking about the strangeness of the members of the Expedition not wishing to "marry" any of their women, and I tried to explain to them that we considered it wrong and to warn them of the fate which probably awaits them when other white men, less scrupulous, enter their land — a fate which has overtaken the Eskimos to the west and carried many of them off. It is sad to see the ravages our diseases make among the natives in all parts of the world, but it seems inevitable. [Jenness 1991:481]

Both Richling and Kulchyski comment on these episodes. Richling, emphasizing Jenness's sense of vulnerability, asserts that the first reported episode of wife exchange "reveals a deeper conflict between the anthropologist's personal and professional personae. His response to the temporary wife exchange also gives voice to a sentiment of moral offence. Suddenly the mythical character of a customary practice . . . became real, and in so doing, engendered a separateness between the scientist reared in an atmosphere of late Victorian mores, and his 'subjects' " (1988:9–10). In contrast I interpret Jenness's assertion that the Europeans view such exchanges as wrong to show that he was truly troubled by the events. I agree with Kulchyski, though, that Jenness's disgust led him to try to render the exchanges as less upsetting:

> This event, that so shakes the nerve of the ethnologist producing feelings of shame and disquiet, constantly slips free of the objective language Jenness uses to try and contain it. . . . Finally, he resorts to the language of otherness: "the custom is, of course, well known among savages from books"; but is it more than that. This last attempt at containment is perhaps the most powerful: it involves positioning Inuit in the category of "savage" in order to simultaneously excuse, explain, contain and reduce to normality what in fact was, for Jenness, an extraordinary event. What he does not know—why it happens, if everyone participates—is greater than what he does know. The event destabilizes Jenness as objective inquirer. . . . Jenness's nerves are shaken. He has been invited to forsake his position as objective recorder; he struggles back by classifying the people he lives with as savages, by trying to find available explanations. But somehow, in the end, he cannot quite do his job as an anthropologist and feels ashamed. [Kulchyski 1993:42–43][14]

Jenness continued to have difficulties with the language, though he eventually developed enough facility to make jokes (1991:423, 577). Even with the assistance of a translator, it was difficult to induce the people to speak, particularly about their religion and folklore (Jenness 1991:549, 581; CMCS, folder "Jenness, Diamond 1913–1919," Jenness to Sapir, December 26, 1915). Eventually, Jenness resorted to threats to overcome their reticence:

> Uloksak was in my tent during the day, and I told him that he could not expect me to treat him very liberally if he did not tell me any stories. He said there was someone always hanging about the tent and he was afraid to tell. However, he came over late in the evening and told us a few shamanistic stories. I asked him whether he would care to have Ikpuk present, and he said no, Ikpuk would be angry with him. [1991:556]

Even when his threats were successful, Jenness still had difficulties communicating his objectives. He complained about inability to understand his directions and inability to tell a story in the "proper" way, with a set beginning, middle, and end, seemingly without realizing that their methods might lend an insight into the culture (1991:559).

Jenness's enthusiasm about living with the Inuit for an extended period eventually waned. Two months into his summer's journey with the Copper Inuit, he commented on the effects of cultural immersion. "I am growing Eskimo in many ways—," he wrote, "careless about dirty pots or dirty person—drink more cold water—tend to have my mouth agape when traveling. It requires an effort to keep 'white'" (1991:451). Less than six weeks later, he wrote in his diary that "I am heartily sick of Eskimo life with its filth and squalor, and long for decent food and rest and quiet" (1991:486). He described the pleasure of having a tent to himself (1991: 481) and noted with anticipation that "winter will soon be at hand when I can return to the station and enjoy good well-cooked food cleanly served, and the pleasant company of the other members of the Expedition" (1991:480).

His writings presented the Copper Inuit as a still "authentic" culture and the Eskimos of Alaska as highly acculturated. Jenness arrived among the Alaskan Eskimos at a point when they had been exposed to southern culture for an extended period of time: "Very little in the outward culture now differentiates the Eskimo from the white" (Jenness 1918:93). It was so bad, he wrote, that "life's three great necessities, . . . food, shelter, and clothing, the Eskimo is no longer able to provide for himself. Remove the supply from without and he will perish within a few years" (1918:91). These changes reflected the primitive nature of the Eskimo culture:

> The changes produced in the life and habits of the Eskimos of Northern Alaska during the last thirty-five years afford an interesting example of the effect European civilization may have upon an uncivilized unprogressive people. . . . That these Eskimos were incapable of developing internally to any marked degree is fairly evident from the fact that during all the centuries that have elapsed since their separation from the other branches of their race no fundamental change has taken place in either their social or their mental life. In fact, the environmental conditions to which they were subjected were unfavorable to any great development. Year by year the seasons returned unchangingly, each with its different pursuit, but all alike periods of strenuous quest for food. . . . The great world beyond was too remote ever to reach or affect them, and their own life involved too arduous a struggle for existence to allow them that leisure which alone enables a people to develop. [1918:89–90]

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

> First the old social system breaks down, carrying with it the morality that it supported. This opens the road to self-indulgence and excess of every kind, followed by disease and misery, which, partly directly, partly indirectly, by undermining the virility of the race, cause its decline and sometimes its extinction. . . . Sometimes, under counteracting influences, the people recover, . . . and such recovery seems to be going on in Northern Alaska. There the very simplicity of the social organization and its adaptability to new conditions prevented its destruction; it altered without entirely breaking down. It still lends support to the respect with which property and persons are regarded and binds the people together in harmony and goodwill. [1918:98]

Jenness links the Alaskan Eskimos and the southerners in an evolutionary framework, in which the presence of the latter paradoxically destroys the Aboriginal culture and simultaneously regenerates it out of its simple constituent pieces. A prominent example is the impact of Christian missionary teachings on the Eskimo communities. Such teaching, "however imperfectly understood, and however misinterpreted, has been on the whole beneficial to the Eskimos" (1918:99), as, generally speaking, "a native no more than the average white man can reason out a set of moral rules to guide his conduct. He depends on custom to tell him what to do and what not to do, and custom unfortunately prescribes or allows many undesirable practices" (1918:99). Religion was a "gift" from a group of conscientious southerners to the Eskimos, and even "if the Christianity of the Eskimo today is very crude and full of superstition, it is nevertheless free from many of the injurious practices of his old religion and contains in itself the germs of a higher development" (1918:100). Once the seed has been planted, he noted, the growth of religion in the Eskimo soul would elevate individuals above their current lot. They would progress from superstition to superstitious Christianity to true faith.

Jenness's views on evolutionism emerged further in discussions of Inuit "sexual morality" (1918:98). Conditions of the latter have "greatly improved, partly from a growing knowledge of the evils to which loose living gave rise, partly as a result of missionary teaching. Much progress must still be made, however, before the standard of civilization is attained" (1918: 98). This passage makes clear his assumption of a single evolutionary path, leading at its apex to the culture of which Jenness considers himself a member.

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

> authenticity in culture or art exists just prior to the present — but not so distant or eroded as to make collection or salvage impossible. Marginal, non-western groups constantly (as the saying goes) enter the modern world. And whether this entry is celebrated or lamented, the price is always this: local, distinctive paths through modernity vanish. These historiocities are swept up in a destiny dominated by the capitalist west and by various technologically advanced socialisms. What's *different* about peoples seen to be moving out of "tradition" into "the modern world" remains tied to inherited structures that either resist or yield to the new world but cannot *produce* it. [Clifford 1987:122]

Jenness short-circuited the salvage paradigm by denying that he had to reach back to the past to recover the essential components of Copper Inuit culture. Though elements of outside culture had begun to creep into their territory, he presented them as being unaffected by these developments. Thus, salvage was obviated by the continued presence of precontact culture even as the hallmarks of contact facilitated his presence among the Inuit.

Jenness explicitly targeted his major Arctic ethnography, *The Life of the Copper Eskimos* (1922), at a popular audience, referring readers interested in more detailed scientific descriptions of the topics covered to the other publications of the expedition (1922:11, 13). Throughout, he used concepts and descriptions familiar to this lay readership. For example, he continued to refer to the Inuit as being childlike in their behavior, particularly with regard to their inability to reason or to control their tempers; as he wrote elsewhere, "the greatest check on theft is the extreme intimacy of social relations, everyone being aware of what is said, done, or owned by all the rest. Nevertheless a little pilfering does occur, even among themselves; and, in the absence of any established authority, the victim's only redress is by an appeal to physical force, which, with a people whose emotions, like those of children, have not come under the control of a developed temperament, frequently means murder" (Jenness 1917a: 86). At the time he was writing, English law still provided for a system of capital punishment, so it is curious that Jenness would view retaliatory murder as a sign of an undeveloped temperament.

Jenness's description of Inuit life in various publications was crafted to appeal to a popular readership. The Inuit behaved in a predictable way. Nothing in his ethnography challenged popular expectations of a primitive society. Arguing that the Copper Inuit understanding of shamanism threw "a considerable light on the mentality of the people" (1922:198), he described their rituals in extremely unflattering terms, while allowing that this sort of explanation did not necessarily do justice to the Inuit conception:

> To a critical and unsympathetic outsider it may seem that a séance of this type is simply a case of palpable fraud on the part of the shaman, and of almost unbelievable stupidity and credulity on the part of the audience. A little very amateurish ventriloquism, a feeble attempt at impersonation, and a childish and grotesque blending of the human and the animal, all performed in full daylight before an audience incapable of distinguishing between fact and fancy, between things seen and things imagined, or at least so mentally unbalanced that it reacted to the slightest suggestion and hypnotised itself into believing the most impossible things — that perhaps is all there may seem to be in Eskimo shamanism. [Jenness 1922:194]

After such a careful consideration, Jenness's conclusion was no less patronizing. On the contrary he saw nothing in the shamanistic practices that would be unknown to a European:

> Hysteria, self-hypnosis, and delusion caused by suggestion are well-known to every psychologist and medical practitioner, and everything that I witnessed could be explained on one or other of these grounds. The natives have many more tales of far more wonderful phenomena, phenomena which, if true, would be as mysterious and inexplicable as the much-discussed walking over red-hot stones that is practiced by a certain Fijian tribe. But of these marvels I myself saw nothing, and until we have the evidence of some more critical eye-witness than the Eskimo himself, it is safest perhaps to attribute them to the over-wrought imaginations of a people whose knowledge of the workings of our universe is far more limited than our own; a people who have no conception of our "natural laws," but in their place have substituted a theory of spiritual causation in which there is no boundary between the possible and the impossible. [Jenness 1922:217]

Not only did Jenness discredit insider knowledge, but he also demonstrated the impact of his evolutionary approach to culture. Because the Inuit had not attained a high level of scientific knowledge, they tended toward "the over-wrought imaginations" of an ignorant people, striving vainly to make sense of their world. His ethnographic writings show that Jenness valorized the achievements of his culture at the expense of the Inuit culture. In this evolutionary framework the Inuit will always fare poorly in comparison to the English.

A striking example of Jenness's evolutionary thinking is his assertion that the Copper Inuit represented a transition point between a Stone Age and an Iron Age culture, reflecting his training in European archaeology. Because the Copper Inuit worked the native copper as a "malleable stone" rather than smelting it, they were only at a "pseudo-metal" stage of evolution (Jenness 1923:540). His evolutionary perspective was also evident in his discussion of Inuit custom, as distinguished from law: "Established authority among the Copper Eskimos is unknown. . . . The only law is custom, handed down from generation to generation; it alone upholds the structure of society, maintains the taboos, and regulates the relation of family to family and of man to man. Its sanction is religion, and violation of custom is punished, through spiritual powers, by sickness and death, or ill-success in hunting and fishing" (1917a:86). At the same time that Jenness argued that the Copper Inuit were at a low evolutionary level, he also linked them to western Europeans: "Family organization is, in its general features, very similar to our own" (1917a:89). But he added the important proviso that the "interchange of wives, however, is common, polygamy frequent, and polyandry not unknown" (1917a:89). Similarly, "the Eskimos, like ourselves, have that indefinable feeling of home in the country they have known since childhood" (1922:32). In his discussion of hunting and fishing, he mentioned that "what he lacks in weapons, however, the Eskimo makes up for in craft. All the precautions and tricks of the European hunter are known to him" (1922:146), and that "primitive as are the methods of fishing that the Copper Eskimos employ they are nevertheless in most cases very effective" (1922:152). At some points during his stay with them, the Copper Inuit transcended their primitiveness and impressed him with their skill and knowledge.

Jenness also commented on the similarities between Inuit and English parenting styles. He gave the Inuit a rather backhanded compliment, saying that "however rude and uncultured these Eskimos may be, the bond that binds the mother to her child is an enduring one, lasting as long as life itself" (1922:170). He also noted in passing that Inuit "parents frequently massage their own children while nursing them, as our own parents do" (1922:165). Jenness's monograph contains his only direct confrontation of the prejudices of his readership. Inuit behavior, while seemingly neglectful, actually makes sense in an English context:

> A casual visitor might gather the impression that children are badly cared for by their parents. Both boys and girls run about in their most wretched clothing, full of gapes and rents, often cut down, indeed, from the worn-out garments of their elders. Even their footgear is of the same description, and often it is soaking wet. It must be remembered, however, that these are their oldest clothes, and that there is always a good warm set of garments carefully stored away for special occasions. Our children do not wear their Sunday clothes at school, nor do the Eskimo children wear their cleanest and finest garments when playing about in the greasy snow in and around their houses. [Jenness 1922:169]

This appreciation of Inuit rationality, however, was an isolated moment in his ethnographic corpus.

Throughout this description of his evolutionary perspective, Jenness's view of Inuit culture relative to his own shaped the way he described the value and meaning of the culture. His lack of respect for Inuit knowledge and practices made it easy for him to advocate imposed change and assimilation of the Inuit. Moreover, the changes that occurred while he was living among the Copper Inuit received scant mention in his ethnography. He noted the likelihood of a significant decline in population in the recent past (1922:37) and that the arrival of Europeans, while reducing the chances of famine, replaced this threat to life with imported diseases that likely caused just as many, if not more, deaths (1922:43). In this light Jenness ended his major ethnography with a passionate, if somewhat misguided, appeal to the outside world:

> Rapid changes are taking place in the culture of the natives, and implements of iron and steel, rifles, fish-nets, open boats, European textiles and sewing-machines, European foods, cheap musical instruments and the development of trapping at the expense of hunting and sealing will work at a complete transformation within the space of a very few years. Already the new culture elements and the new teachings that are filtering in from the west have profoundly modified their social and religious ideas, and before the present generation passes away the primitiveness of the Copper Eskimos will have ceased to exist. How many will remain by that time, and whether they will be able to take any part in the development of this region depends largely on the manner in which we fulfil our trust. For in throwing open their country to outside invasion we have incurred a heavy responsibility towards the natives. We may increase the security of life among

> them by checking infanticide and murder, we may protect them from unscrupulous exploitation and from the ravages of intoxicating liquors, but all this will be of little avail unless we immediately take measures to secure them against the introduction of our diseases. . . . The Copper Eskimos have no diseases of their own, or at least none were known up to 1916; but white men and western Eskimos are flocking into their country, and in a few more years perhaps they too will fall victim to some of the scourges of our civilization. It may be impossible to prevent this calamity entirely, but at least we could do something to check it. [Jenness 1922: 242]

Jenness seemed to assume that the Copper Inuit were doomed; either they would cease to exist as a distinct culture or they would succumb to the ravages of imported diseases. Perhaps this rhetoric simply valorized his own research, positioning it as the only record of their authentic life, which was then disappearing; or perhaps he was pessimistic to the point of fatalism, having realized that the invasion of the north, in which he himself had played a part, would forever change the lives of the Copper Inuit and certainly not for the better.

The Wider Context

Though there is still some debate about the extent and impact of the development of functionalist method and theory on British anthropology of the 1920s (Kuper 1996:1; Stocking 1995:283), the 1922 publication of Malinowski's *Argonauts of the Western Pacific* and Radcliffe-Brown's *The Andaman Islanders* marked a significant shift in thinking from previous British approaches. Prefunctionalist British anthropology focused on data collection (Kuper 1996:5), reconstructing cultural histories archaeologically on the assumption "that 'customs' are imperishable artifacts, as hard and enduring as flint tools and sherds of pottery" (Leach 1966:566). Theoretical concerns were central to the discipline. The new approach, however, emphasized field research over armchair theorization and rejected "the whole ethnological enterprise" (Kuper 1996:3, 5). Another important distinction, "the rejection of survivals[,] was a precondition of the emergence of functionalism, insofar as it facilitated (and necessitated) the explanation of sociocultural phenomena without reference to diachronic assumption" (Stocking 1995:320 n.). Initially, this emphasis on synchronic analysis "was not necessarily seen as an approach which would displace evolutionist and diffusionist concerns, but rather as something to be added to them" (Kuper 1996:8). These two developments, the emphasis on field research and the adoption of a synchronic perspective, were interrelated.

The new method and theory were quickly accepted by a majority of British anthropologists, and soon "earlier anthropological life forms, before social anthropology, were viewed as positively antediluvian, and of little value" (Urry 1993b:14). The new ethnographies were perceived as far superior to those produced by anthropologists working in the earlier tradition (Urry 1993a:56). Of all the anthropologists responsible for the development of the functionalist approach, Malinowski and Radcliffe-Brown were at the forefront. "Malinowski brought a new realism to social anthropology, with his lively awareness of the flesh-and-blood interests behind custom, and his radically new mode of observation [and] Radcliffe-Brown introduced the intellectual discipline of French sociology, and constructed a more rigorous battery of concepts to order the ethnographic materials" (Kuper 1996:35).

In a comprehensive survey of twentieth-century fieldwork methods, James Urry has argued that the shift to intensive residential fieldwork was neither sudden nor the result of the efforts of one person (1984:35; cf. Stocking 1989:209). However, as Stocking notes, the first chapter of *Argonauts* is remarkable because it contains "Malinowski's deliberate archetypification of the role of 'the Ethnographer' [which] offered, both to prospective anthropologists and to various publics at the boundaries of the developing discipline, a powerfully condensed (yet expansive) image of the anthropologist as the procurer of exotic esoteric knowledge of potentially great value" (1989:209). In the context of Jenness's Arctic ethnography, the fact that Malinowski's first chapter is less a description of his own fieldwork experiences than a prescription for future researchers (Stocking 1983:104) does not diminish its importance.

Argonauts was seen at publication "as a useful addition to the literature rather than as a call to revolution" (Kuper 1996:9), although some argued that "the type of material Malinowski had collected, and the manner in which he presented it, did amount to a radically new view of a 'primitive culture' " (Kuper 1996:9). Stocking stresses novelty; the opening chapter of *Argonauts* "was a 'mythic charter' for what was to become the central ritual of social anthropology. A motivating myth for 'apprentice ethnographers,' it reassured them that a difficult and even dangerous task was possible, that those who would follow in Malinowski's charismatic methodological footsteps could in fact 'get the work done'—even to the point where it would become a matter of disciplinary routine" (1991:11). Though some would question its revolutionary character (e.g., Paluch 1988:72), Malinowski's concept of fieldwork reshaped conceptions of anthropological research. At its root fieldwork "depended ultimately on placing oneself in a situation where one might have a certain kind of experience" (Stocking 1995:273). Malinowski himself asked: "What is then this ethnographer's magic, by which he is able to evoke the real spirit of the natives, the true picture of tribal life? As usual, success can only be obtained by a patient and systematic application of a number of rules of common sense and well-known scientific principles," including possession of "real scientific aims," execution of research immersed in Indigenous communities away from other Europeans, and use of specific methods of collecting data (1922a:6).

The researcher had to have a firm grasp of current theory in the field. Malinowski carefully pointed out that knowledge of theory "is not identical with being burdened with 'preconceived ideas'" (1922a:8–9); while "pernicious in any scientific work, . . . foreshadowed problems are the main endowment of a scientific thinker, and these problems are first revealed to the observer by his theoretical studies" (1922a:9). Despite his theoretical emphasis, Malinowski still divided research and theorizing, in the vein of Frazer and Marett (1922a:9). Emphasizing the difficulty in finding and relying upon a Native expert for data, he stressed "collecting concrete data of evidence and drawing the general influences for himself" (1922a:12).

Malinowski enjoined ethnographers to ensure that their fieldwork took place under the proper conditions. These "consist mainly in cutting oneself off from the company of other white men, and remaining in as close contact with the natives as possible, which really can only be achieved by camping right in their villages" (1922a:6). Immersion in Native culture would lead to better information than short contact with paid informants (1922a:7). Malinowski's introduction contains the paradigmatic statement of the paradoxical position of the participant-observer in Native cultures of being both inside and outside of the Native culture, both like and unlike the people he is studying (1922a:21). The need to learn Native standards of etiquette and conduct was central to the success of Malinowski's style of fieldwork (1922a:8).

The collection, recording, and manipulation of evidence was the final concern addressed in the introduction to *Argonauts*. By choosing to live in the village, the ethnographer could observe "the customs, ceremonies and transactions over and over again" (1922a:18), seeing "examples of [Native] beliefs as they are actually lived through, and the full body and blood of actual native life" (1922a:18). Attention to detail was the one area where professional ethnographers had something to learn from earlier preprofessional workers (1922a:17).

Malinowski argued that these details, which he termed "*the imponderabilia of actual life*" (1922a:18, emphasis in original), could only be gathered through the observation of the ethnographer. It was also necessary to understand the motivations for action, "the natives' views and opinions and utterances" (1922a:22). These details showed the divergence between professional and amateur ethnographers: "All these facts can and ought to be scientifically formulated and recorded, but it is necessary that this be done, not by a superficial registration of details, as is usually done by untrained observers, but with an effort at penetrating the mental attitude expressed in them" (1922a:19) At the same time these details, these facts, should "speak for themselves" (1922a:20).

The best method was to record Native speech verbatim and to come to terms with Native concepts (1922a:23). This would move the ethnographer toward "grasp[ing] the native's point of view, his relation to life, to realise *his* version of *his* world." Malinowski focused on the individual (1922a:25) but was not interested so much in what particular individuals thought as in how individuals were shaped by their culture while simultaneously shaping it (1922a:23).

In an article also published in 1922, Malinowski expanded on anthropology's role in changing Western perceptions of Native cultures. In particular "it would be much better if ethnographical knowledge could altogether change the average white man's whole outlook on savage morality" (1922b:211). For example a Native "belief, which appears crude and senseless in isolation, a practice which seems queer and 'immoral,' becomes often clear and even clean if understood as part of a system of thought and practice" (1922b:218). Malinowski's work shifted in emphasis from salvage to "the study of social change and culture contact" as wider interests shifted to "practical anthropology," that is, anthropology addressing colonial administration (Urry 1984:52). Malinowski himself argued that "the survival of natives — apart from humanitarian, aesthetic, or moral considerations — is a matter of vital importance for practical purposes" (1922b:209). His nascent applied anthropology combined practical and theoretical concerns, while always stressing respect for the Natives and their cultures (1922b:208).

The other significant anthropological publication in Britain that year, Radcliffe-Brown's *The Andaman Islanders*, is the preliminary culmination of several years of theoretical thinking. His ethnography "presented as its final reconstituted product a closed timeless picture of the integrated organic life of Andamanese culture. It might be said that he in fact transformed the nature of ethnography: from the privileged domain of practice, it became the site for the authoritative work of theory" (Tomas 1991:102–3).

Like Malinowski, Radcliffe-Brown argued that researchers must be conversant with contemporary theoretical developments. However, in his insistence that an individual researcher undertake both observation and hypothesization, Radcliffe-Brown moved forward from the prefunctionalist division of labor. If ethnology were to develop as a science, it had to overcome "the false division of labour whereby theorists and observers work independently and without systematic cooperation" and replace it with an approach "in which the observation and the analysis and interpretation of the institutions of some one primitive people are carried on together by the ethnologist working in the field" (Radcliffe-Brown 1964: 231–32).

Radcliffe-Brown outlined two possible approaches to "dealing with the facts of culture or civilisation amongst primitive peoples who have no historical records". The *ethnological* was an attempt to "reconstruct hypothetically the past history of a people in its main outlines", based on "the co-ordinated study of physical characters, language, and the various elements of culture, and with the help of such archaeological knowledge as is available" (1964:39). While he did not deny the interest of such research, it "has given rise to a literature of which a large part is of little or no scientific value, owing to the utter disregard of the laws of scientific evidence and the need for the verification of hypotheses" (1964:39). As a result, Radcliffe-Brown continued, this approach "does not often provide, and does not seem likely to provide, results that will be of any assistance to the administrator or the educator in the solution of the practical problems with which he is faced" (1964:39). In place of the ethnological method, Radcliffe-Brown argued for a *sociological* approach. Stressing the fundamental interconnection of sociology and psychology, he searched for sociological and psychological laws to understand the institutions of another culture. This approach would aid administration and education, because it "would enable the anthropologist to foretell with some degree of certainty . . . the general effects on the life of a tribe of an attempt to abolish the custom in question" (1964:40).[15]

Despite problems with his data, which he recognized, he used them as the basis of broad theoretical generalizations (1964:82 n. 1). Radcliffe-Brown got around these problems by working in an explicitly deductive manner. Starting with the assumption that the explanation of customs or beliefs of a particular group has its roots in "some general psychological hypothesis" (1964:232), he asserted that "the sound rule of method is therefore to formulate clearly and explicitly the working hypothesis on which the interpretation is based. It is only in this way that [the custom or belief's] value can be properly tested" (1964:232).

Radcliffe-Brown assumed as well that useless customs or beliefs do not last long in a society and argued against the notion of survivals. In a way savages were as rational as Westerners, with a sense of utility in both social organization and beliefs. Among the Andamanese "customs that seem at first sight meaningless or ridiculous have been shown to fulfil most important functions in the social economy, and similarly I hope to prove that the tales that might seem merely the products of a somewhat childish fancy are very far indeed from being merely fanciful and are the means by which the Andamanese express and systematise their fundamental notions of life and nature and the sentiments attaching to those notions" (1964:330). Like Malinowski, Radcliffe-Brown argued that the people he studied had their own ways of fulfilling the functions required of each society.

Getting a grasp on contemporary trends and selecting appropriate examples of these trends is somewhat more difficult in an Americanist context than in the British field. While appreciating the diversity of the Americanist approach developed by Boas and his students in the interwar period (Darnell 2001:35, 12; Stocking 1976), I have chosen to focus on an article published by Boas in *American Anthropologist* in 1920 and on the contemporary work of Edward Sapir, Jenness's direct superior at the National Museum while he was completing his Arctic ethnographies.

Boas began his article "The Methods of Ethnology" by attacking previous theoretical approaches to the study of culture. Continuing a quarter-century critique of evolutionary thinking, he asserted that the unilineal evolutionary approach "presupposes that the course of historical changes in the cultural life of mankind follows definite laws which are applicable everywhere, and which bring it about that cultural development is, in its main lines, the same among all races and all peoples" (1920:311). Boas's main critique of these approaches was that their emphasis on "obtaining a consistent picture of cultural development" (1920:313) led to their arbitrary application to cultures. Without proving or even trying to prove the validity of their interpretations, these approaches were, in Boas's terms, "essentially forms of classification of the static phenomena of culture according to two distinct principles, and interpretations of these classifications as of historical importance" (1920:313).

Boas argued that no overriding psychological need would lead to "uniform evolution the world over" (1920:317); he asserted that "each cultural group has its own unique history, dependent partly upon the peculiar inner development of the social group, and partly upon the foreign influences to which it has been subjected. There have been processes of gradual differentiation as well as processes of levelling down differences between neighbouring cultural centers, but it would be quite impossible to understand, on the basis of a single evolutionary scheme, what happened to any particular people" (1920:317).

Identifying the evolutionary approaches within European anthropology, Boas went on to outline the method then being employed by American anthropologists. The latter group, in his formulation, emphasizes "the dynamic phenomena of cultural change" and attempts "to elucidate cultural history by the application of the results of their studies" (1920:314). American scholars generally "relegate the solution of the ultimate question of the relative importance of parallelism of cultural development in distant areas, as against worldwide diffusion, and stability of cultural traits over long periods to a future time when the actual conditions of cultural change are better known" (1920:314). At the same time as he outlined a methodology based on the examination on the comprehensive examination of cultural features, to understand cultures on the level of their individual members Boas emphasized "the important problem of the relation of the individual to society, a problem that has to be considered whenever we study the dynamic conditions of change" (1920:316) and outlined a feedback relationship between a culture and the psychology of its members (1920:316).

Sapir, a former student of Boas, was Jenness's superior at the museum in Ottawa. A leader in the study of interactions between culture and psychology, he, along with Margaret Mead and Ruth Benedict, moved the discipline in the 1920s "from trait-oriented survey ethnology to a more processural focus on what Mead and Benedict called culture and personality"; Sapir termed this approach the study of " 'the impact of culture on personality' " (Darnell 2001:327). Darnell argues that Sapir's "interest in the individual Indian was not unrelated to his insistence on the uniqueness in his own culture, what he came to call " 'the locus of culture' in each individual" (2001:224). His research methodology explored the perspectives of individuals in the culture being studied. Richard Preston describes Sapir's method as comprising "the description of social and cultural terms as they are defined in the lives of specific individuals-in-culture" (1966:1122). This emphasis on individual understandings, however, Preston stresses, was only in the context of shared meanings; Sapir was interested in examining the individual as she or he was embedded in her or his culture (1966:1121).

Sapir's approach required an emphasis on language; in particular, as he noted to Wilson Wallis, collecting Native language texts was the best way to understand Native ways of classifying the world—in his mind the central purpose of anthropological research (Darnell 1990:71). Shortly after his arrival at the National Museum, Sapir outlined the areas of research he hoped to see undertaken in the coming years. Linguistics figured in all research projects: "all investigation of native mythology, rituals, songs and allied subjects, undertaken without the help of linguistic study, must fail to result in a complete understanding of the native concepts involved" (1911:791 n. 4). While the study of language was important in its own right, such investigation would also contribute to an understanding of other fields (1911:790).

The Arctic was key. Though the Inuit had been studied to an extent, they still "present[ed] many problems of interest" (Sapir 1911:791). In particular, Sapir asserted, "several of the less easily accessible tribes are as yet practically unknown. Until these have been investigated it will be difficult to undertake a satisfactory analysis of Eskimo culture as a whole, and, consequently, of its relations to the neighbouring cultures" (1911:791). The anthropological component of the Canadian Arctic Expedition's research program was intended to fill the gaps in understandings of Inuit groups and their relation to other Aboriginal groups in Canada's north.

American, like British, anthropology was going through significant changes in the years after the First World War. In particular anthropologists working in both national traditions were moving away from an evolutionary perspective and striving for an understanding of the "Native" point of view. Although these developments occurred simultaneously with Jenness's production of his Arctic ethnographies, his works show no evidence of engagement with such theory.

Diamond Jenness and Contemporary Anthropology

In the context of other materials produced by his colleagues in the Canadian Arctic Expedition, Stuart Jenness notes that his father's "share of the reports issued by the Canadian government with the results of the . . . Expedition . . . amounted to four volumes, totalling 1436 pages, far exceeding the contribution of any other member of the Expedition. Collectively they constitute the definitive early work on the Copper Eskimos" (1991a:624–25). Riches (1990:72) raises the possibility that the early ethnographers of the Inuit, including Jenness, did so thorough a job as to undermine latter attempts at describing Inuit culture.

A comparative examination of his Arctic ethnography and his earlier New Guinea work suggests Jenness's ambivalent relationship with the methodology and theoretical approaches of both his mentor and his contemporaries. Like his teacher, Marett, Jenness operated within an evolutionary framework; unlike Marett, who emphasized culture, however, Jenness focused on a racial hierarchy. That is, while Marett was interested in the development of elements of individual cultures over time, Jenness criticized the Inuit and Eskimo cultures for not being as highly developed as his own. Also, unlike Marett, Jenness did not concentrate on trying to understand the thought patterns of the Inuit. He commented in his diary about three Inuit women: "The three sisters form an interesting trio — the woman just past her prime — the woman in her prime . . . — and the girl just preparing for marriage. It would be still more interesting if one could discover their different outlooks upon life" (1991:371). When he did address Inuit psychology, he tended to describe the people he met as childlike, reflecting a kind of Victorian chauvinism that reduces the insightfulness of his descriptions.

At the same time Jenness's Arctic research methods met Malinowski's standard for anthropological rigor. Richling argues that Jenness's "Coronation Gulf fieldwork is a far better illustration of what Malinowski professed to have done in the Trobriand Islands than what Malinowski himself actually did" (personal communication, December 6, 2000; 1989:81). Jenness's desire to accompany an Inuit family on its summer travels demanded a complete immersion into their life. The high mobility of the groups required that he travel and live with them for seven months. Unlike Malinowski, he had no refuge from his companions. Because he packed goods on foot, he could not transport reading material to alleviate what Malinowski called the "periods of despondency" (1922a:4)[16] that confronted the ethnographer in the field.

In many ways Jenness's work reflected the split between ethnography and theoretical innovation that was current at the time of his training. In Radcliffe-Brown's terms, it was ethnological rather than sociological, emphasizing hypothetical historical reconstructions rather than descriptions of contemporary social structures. Jenness, for example, did not construct an integrated, functional portrait of Inuit culture at the time that he visited the Inuit. Jenness's elision of his own theoretical approach in his ethnographic accounts also ran counter to Radcliffe-Brown's position that anthropologists should approach the study of a culture in deductive terms.

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

> There is the further question as to what use could be made of the Copper Eskimos in the future development of the country. At the present time the only asset which the country possesses is its fur. These Eskimos should be as successful trappers as any in the North, once they have learned the value of the different kinds and qualities of fur. It is not improbable that the copper deposits in the region may eventually prove of great value, but little can be expected from a purely hunting people in the way of labour for mines. Nevertheless it might be possible to utilize them in other ways, as in the handling of freight and in transport generally. [1917a:91]

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

His interest in this field was motivated by his perception that Inuit culture was on the cusp of major change, change that would come from the outside. Richling defends Jenness's lament over the inevitable changes that accompanied the influx of southerners and their culture (1989:81). In his ethnographic writing Jenness portrayed the Inuit as passive, compliant people, unable to stand up to the influx of Westernization; they were, he believed, unable to take from the invading culture only those elements that would be of use in their setting while rejecting the rest. Jenness's diaries noted the Inuit reactions to outsiders:

> Another point common to them I imagine to be an undeveloped personality or rather individuality. Hence the individualist is the man of note and influence. The easy merging of one man's will into another's makes for the "tolerance" of Eskimo society, where each person does what he likes without interference. It would account in part for the ease with which they are dominated by Europeans, their pliant wills yielding submissively to the aggressiveness of the outsider. Perhaps too it accounts in part for the hold that missionary teaching has upon the Mackenzie and Barrow natives, the driving power of the missionary forcing his convictions—in so far as they are understood—upon his auditors. Even with us it is always easier to acquiesce than to oppose. [1991:331]

If they were only able think for themselves, he noted, they would be able to withstand the onslaught of change. At the same time he neglected power dynamics, even in relation to the impact of his own expedition. The

Inuit, for example, had little choice but to maintain good relations with the expedition. In one notable case Jenness made an agreement with the family he accompanied during his research in the summer and fall of 1915 that upon his safe return to the expedition's camp they would be given a range of desirable goods, including guns and ammunition. When the Inuit did not acquiesce, Jenness often grew frustrated and threatened to punish them like children.

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

> All I would note, rather hesitantly, is this, that you seem to be a little afraid of digging into your people's insides — Eskimo and whites. Could you contrive to give a somewhat livelier sense of the Eskimos as differentiated people, also of the more serious aspects of the personalities of your companions? As it is, you depend rather too much, it seems to me, on whimsical anecdotes to give a feeling of humanness — such anecdotes come into their own, after all, when set in a more massive context of serious portraiture than you give. [CMCJ, box 656, file 37 (Edward Sapir, 1924–1929), Sapir to Jenness, June 7, 1927]

In other ways, however, Jenness's work did follow the Boasian model. Like Boas and his students (cf. Briggs and Bauman 1999), Jenness assumed that Aboriginal cultures were dying and neglected their contemporary conditions to highlight their "ancient" practices.

Jenness, moreover, seems to have had little prolonged contact with students of Boas other than Sapir. The professional and personal relationship between Jenness and Barbeau, his colleague and fellow Oxford alumnus, was deeper and more complex. Although some work has been done on Barbeau's perspective (Nowry 1995; Preston 1976; Barbeau n.d.), little has been written about Jenness. Andrew Nurse, in a recent study of Barbeau, identifies patterns similar to those presented here (2001: 452). Given that Jenness and Barbeau studied at Oxford University at the same time and then spent their careers working in the same government office in Ottawa, one would expect close interaction. When Jenness was selected over Barbeau to replace Sapir as head of the Anthropology Division of the Geological Survey in 1925, Barbeau was enraged. He remained bitter throughout their years under the same roof. Consequently, there was very little intellectual exchange between the two.

Jenness's relationship with Sapir was more positive. Jenness remarked in a letter to Marett, for example, that "I like Dr. Sapir very much indeed. He is extremely capable, and seems to make a great success of the department [in the Museum]. Also he is extremely easy to get along with; he trusts you to do your work in your own way, though he is always ready to give you any assistance in his power" (OUAM, Jenness to Marett, November 22, 1916). It is difficult, however, to assess the extent to which Sapir influenced Jenness's work. Although Jenness's Arctic research was superficially similar to Sapir's cultural reconstruction, his larger objective to place the Inuit in an evolutionary scheme was far removed from Sapir's approach.

Jenness's relationship with the wider anthropological community is even harder to discern. If he corresponded with Malinowski or Radcliffe-Brown, no record exists in the Jenness correspondence file in the Canadian Museum of Civilization Archives. An examination of the Franz Boas Correspondence turns up a considerable correspondence with Jenness, but these letters included little beyond administrative trivia concerning various later projects Jenness undertook. Any mention of the Arctic Expedition work concerns physical anthropology (e.g., APSB, Jenness to Boas, March 19, 1920, March 23, 1920, April 9, 1920, May 4, 1920, May 10, 1920; Boas to Jenness, April 5, 1920, October 25, 1922); cat's cradles (e.g., APSB, Jenness to Boas, January 4, 1922, January 18, 1922, April 3, 1923, April 12, 1923, April 19, 1923, May 3, 1923; Boas to Jenness, April 16, 1923, April 17 1923); and language (e.g., APSB, Jenness to Boas, April 12, 1927; Boas to Jenness, April 13, 1927), rather than general ethnography. Curiously, Boas's correspondence with Sapir and Marett during the late 1910s and early 1920s includes no mention of Jenness.

Jenness himself made only incidental references to his relationships with other anthropologists. Upon his return from a major meeting in Boston, he wrote to Sapir that "I was particularly glad to be able to have long chats with Boas and Wissler, both of whom I liked very much. Now that I know them personally I have ceased to dread their criticisms and shall rather enjoy it if they take me to task now and again" (CMCS, folder "Jenness, Diamond 1920–1924, Jenness to Sapir, January 3, 1922). However, five years later Jenness expressed some reservations about his dealings with North American anthropologists, writing to the Dane Kaj Birket-Smith that "one of my greatest pleasures here is to be able to correspond so freely and frankly with you and other Danish ethnologists. There are one or two anthropologists with whom I feel always that I must be on my guard, but with my European friends I am able to discuss problems and ask for help as I do from my colleagues here in the Museum" (CMCJ, box 640, file 31 [Kaj Birket-Smith, 1922–1939], Jenness to Birket-Smith, May 30, 1927).

It is just as difficult to assess the critical reaction to Jenness's Arctic work. Neither *American Anthropologist* nor *Man* published reviews of *The Life of the Copper Eskimos*. The closest thing to a review of the Arctic work was a short note on Jenness's section on Inuit music (prepared with Helen Roberts) (Lehmer 1927). This is surprising given that both journals had earlier published reviews of Jenness and Ballantyne's work on New Guinea. The review in *Man*, by F. R. Barton (1921), was simply a summary of the contents of *The Northern D'Entrecasteaux*, while Robert Lowie, in *American Anthropologist*, noted that "Mr. Jenness has presented his results with obvious care and may be sure that his future publications, both in the same domain and the widely different Eskimo field, will be received with respectful attention by his colleagues" (1921). Neither of these was an enthusiastic endorsement.

In any event during a period of significant developments of anthropological methods and theory, Jenness managed to set himself on an independent course. While his contemporaries in the United States and Britain were moving away from an evolutionary perspective, he remained fully ensconced in the earlier paradigm. Though Richling has argued that Jenness's work fits comfortably within the Boasian four-field paradigm (1989:81), this is not quite true.[17] Jenness remained most comfortable within British tradition, as evidenced by his hiring preference for new researchers in the museum (Richling 1990:251). For example, during one of his searches for an anthropologist to employ at the museum, he wrote that "there seems to be no one in Canada who has the necessary qualifications for a position on our staff, and while we could doubtless obtain someone in the United States, we prefer an English graduate who would be more likely to stay with us permanently than someone from the United States. Moreover, such a man would have the advantage of European training and outlook with which he would be surrounded" (CMCJ, box 654, file 11 [Harold J. E. Peake, 1928–1940], Jenness to Peake, September 23, 1930). Four years earlier, Jenness conveyed this to T. F. McIlwraith, another British-trained Canadian anthropologist: "English methods differ from American in many ways and an Englishman coming over here to work would be likely to have a broader outlook than an American" (CMCJ, box 649, file 32 [T. F. McIlwraith, 1925–1927], Jenness to McIlwraith, March 1, 1926).

Perhaps Jenness's search for British-trained anthropologists reflected a preference for researchers familiar with the contemporary applied research in the colonies (Richling 1995:50) rather than for the American anthropologists, who were increasingly taking a culture-and-personality approach. The former approach would likely be seen by Jenness (as it was by Radcliffe-Brown, but for different reasons) as better suited to contribute to questions about Aboriginal policies. In fact I concur with those who have argued that Jenness was a five-field anthropologist, combining work in the "traditional" four fields with research of an applied nature (Collins and Taylor 1970:71; Epp and Sponsel 1980:10). I take an inclusive view of "applied anthropology." Rather than limiting it only to research with "a focus on issues with policy implications" (Richling 1995:50), I include any work that is undertaken for purposes other than pure theoretical exploration. As a federal government employee, Jenness undertook research with more serious practical than theoretical concerns.

Richling's thesis that Jenness was committed to the Geological Survey's salvage objectives does not hold true for his early Arctic ethnography. Richling asserts that "there is reason to assume that most public officials did not regard anthropology pertinent to national priorities because they deemed the aboriginal peoples about whom anthropologists were concerned largely irrelevant to national priorities" (1995:52). However, the mandate of the Canadian Arctic Expedition was largely exploratory. Jenness himself addressed the potential benefits and problems of opening the resources of the north to economic exploitation.

Perhaps the applied nature of his research goes a long way toward explaining why Jenness emphasized description in his ethnographic writing. He was a civil servant, a bureaucrat whose expertise happened to be anthropology (Richling 1995:57), and the government he worked for required information suitable for administration of the Inuit. It did not seek to develop theories on the nature of culture in general. As a civil servant with extensive administrative responsibilities, Jenness was not able to devote any significant amount of time to writing up his own work (CMCJ, box 649, file 32 [T. F. McIlwraith, 1925–1927], Jenness to McIlwraith, March 1, 1926), let alone keeping up-to-date with theoretical developments; however, this was obviously not the case for Sapir when the latter was in charge. Both Richling and John Van West acknowledge that Jenness was less a theorist than an ethnographer (Richling 1989:72; Van West n.d.:20). Writing from a historicist perspective, Richling argues that "it would be wrong to dismiss or undervalue Jenness' numerous contributions to the four sub-fields of anthropology because his experience did not culminate in a synthesizing work, or because no 'school' of anthropological thought or practice, Canadian or otherwise, may be credited to him.

Instead, we are obliged to consider his professional accomplishments within a socio-historical context defined by the specific conditions under which anthropology existed in Canada, and elsewhere, during the first half of this century" (1989:72).

My objective has been to show the extent to which Jenness, during his early career, was isolated from developments in anthropological theory. His personal views and preferences may have influenced his observations of the Copper Inuit. Kulchyski accuses him of promoting "outright assimilation" on the grounds that Jenness later "proposed measures even more forceful than those the state had already adopted and was prepared to countenance an approach that was in many respects even more paternalistic than the bureaucrats were advocating" (1993:27–28). Kulchyski concludes that "the political bias in Jenness's work was markedly against the people whose cultures he was attempting to represent" (1993:46).[18] Now that Kulchyski has provided a better insight into his later politics, it is clear that all of his work, including his early Arctic ethnography, is in need of careful historical scrutiny. Jenness's work cannot be taken at face value today, and attempts to elide the racism of his approach reflect a certain type of racism that still afflicts the discourse on Indigenous issues and research in Canada.

At the center of Darnell's examination of the uniqueness of Canadian anthropology is an assertion that a simple listing of names, dates, and facts, focused on a limited number of major figures, is an inadequate approach. Instead, she argues, it is necessary to understand the contributions of particular individuals to the development of a national tradition (1975:403). Contrary to Henry Epp and Leslie Sponsel, who argue that Jenness "in many respects . . . is the Canadian equivalent of Boas, except that he was never an academic anthropologist" (1980:10), I maintain that, having never trained students in this country, Jenness cannot be compared to Boas in this way. Jenness, unlike Boas, was not a founding father of a distinctive national approach to anthropology. And, again unlike Boas, Jenness's ethnography did not usher in a new paradigm for the discipline. On the contrary, it reflected an approach that was outdated even at the outset of his early work in the Arctic.

Notes

This essay is a revision of my master's thesis (Hancock 2002). I am grateful to my committee members — Ian MacPherson, Michael Asch, and Robin Ridington — for their careful reading and helpful suggestions when I was writing the original text. This version reflects ongoing discussions of the history of anthropology in Canada with Wendy Wickwire, who supervised the original thesis. I also appreciate very much the comments of Regna Darnell and Marc Pinkoski on earlier drafts of the current paper.

1. For an example of his interaction with younger scholars, see Jenness's correspondence with Fenton (CMCJ, box 644, file 25 [William N. Fenton, 1932–1942]).

2. On the other hand Andrew Nurse maintains that Marius Barbeau "was arguably the most prominent anthropologist in Canadian history," or at least "he may certainly have been the best known" (Nurse 2001:436, 466 n. 11).

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

4. To a certain extent, however, this has begun to change (e.g., Nurse 2001; Kulchyski 1993).

5. Jenness's biographical details are drawn from Maxwell (1972), Balikci (1957), de Laguna (1971), and Collins and Taylor (1970).

6. It is important to keep in mind that at this time the terms *primitive* and *savage* were (pseudo)scientific descriptions and not necessarily explicit evolutionary judgments of the culture. For example Lowie, as a leading student of Boas's, would not have been using the term *primitive* here in an evolutionary sense.

7. For example at one point, traveling with the policeman, Jenness claims to have visited 30 villages in 15 days (OUAM, Jenness to Marett, July 26, 1912).

8. Some of the other problems Jenness faced in New Guinea seem more prosaic. For example he asked Marett to "tell Prof. Thompson not to be too critical over my [anthropometric] measurements. The long frizzy hair makes it difficult to be correct within a millimetre or so & one is sometimes puzzled how much to allow for the hair" (OUAM, Jenness to Marett, December 6, 1911).

9. For Jenness's reaction to Malinowski's critique, see CMCJ, box 648, file 6 (A. L. Kroeber, 1930–1939), Jenness to Kroeber, January 11, 1932.

10. For more on Beuchat, see Beuchat (n.d.) and Barbeau (1916). Beuchat was a colleague of Marcel Mauss, the leading student of Emile Durkheim in France. It would be interesting to speculate elsewhere on the differences in Jenness's approach should Beuchat have survived and the men worked together.

11. In fact, given his trepidations about linguistic work, I wonder how enthusiastic he was about undertaking such research; perhaps his offer to Sapir was an attempt to curry favor with the latter, as Jenness must have known the theoretical emphases of the man who could later play a role in the securing of a permanent position in Ottawa.

12. Anderson recorded the arrangement in a summary of the expedition's work: "Mr. Jenness made his final start for Victoria island on April 13, with his Eskimo companion, a middle-aged man named Ikpukkuaq. The said Ikpukkuaq was supplied with a Winchester .44 rifle and some cartridges, and was promised that if he did well by Mr. Jenness during the summer he was to keep the rifle on his return and receive a certain number of cartridges" (Anderson 1916:228; cf. Jenness 1991:416).

13. Here he is arguing against Mauss, who maintained that the social organization and religion of the Inuit changed with the seasons (Riches 1990:81; cf. Mauss and Beuchat 1979). Interestingly, he is also making an argument contrary to Marett, who said that it was possible for researchers to see the world from a Native point of view.

14. Not surprisingly, Jenness adopted a much more levelheaded tone in his discussion of these events in his published ethnography, noting the importance of this practice in the integration of outsiders into the local social group (Jenness 1991:85–86, 91).

15. Radcliffe-Brown argued that the sociological method was the most useful for the administrator and offered advice to the researcher:

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

He was careful to differentiate between impartial research undertaken as the basis for the formulation of administrative policies and research undertaken with the goals of administration in mind, arguing that the former offered the best strategy for understanding the ways in which administrative decisions would affect Native life (Radcliffe-Brown 1922:40).

16. However, Jenness did have a copy of Homer's *The Odyssey*, which he read with great enthusiasm during his travels (Jenness 1991:420, 422, 427, 431, 455, 535).

17. See also Sapir's assessment of Jenness's proficiency in each of the four fields (CMCS, folder "Jenness, Diamond 1913–1919," Sapir to McConnell, September 14, 1916).

18. Jenness's wider political views are difficult to ascertain; he certainly felt constrained by his status as a civil servant in his ability to critique government policy (CMCJ, box 640, file 12 [Julia Averkieva, 1932–1935], Jenness to Averkieva, February 5, 1932). Some items in his correspondence files certainly seem to reflect anti-Semitic views. For example, during his term as president of the American Anthropological Association in 1939, he commented in a letter to the association's secretary that "I was glad to see that Sapir's resolution was amended to read 'in many countries', without naming any specific ones. I am afraid that the 'Semites' often harm their own cause and create ill-will, through a lack of moderation and tact. However, all is well that ends well" (CMCJ, box 639, file 12 [American Anthropological Association, 1938–1939], Jenness to Setzler, January 5, 1939). Another, more curious example is a letter of introduction Jenness wrote near the end of his career to Sir Frederick Banting: "May I introduce to you by this letter Dr. Jankowsky, of Breslau, who, besides being a well known anatomist and physical anthropologist, is deeply interested in genetics, and is officially connected with the sterilization program of the German Government? I am sure that you will find him extremely interesting, and that he on his side will be deeply interested in the work that is being done at the Connaught Laboratories" (CMCJ, box 640, file 17 [F. G. Banting, 1929–1937], Jenness to Banting, July 12, 1937). Obviously, Jenness's politics are complicated and difficult to pin down conclusively.

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7. Reflections on Departmental Traditions and Social Cohesion in American Anthropology

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This study was loosely sponsored by the Centennial Executive Commission of the American Anthropological Association (AAA), although it articulated questions I have thought about and hoped to explore formally for many years. Like most things worth doing, the project has taken longer and proved more complicated than I expected at its onset. I envisioned being able to speak systematically about the demographics of career mobility from department of training to present employment and wondered how, or even if, ties from professional socialization were maintained within present practice. In many ways the study raised more questions than it answered. But I believe that these questions are integral to the reflexivity of the contemporary discipline and that it is time to share the reflections that emerged as I attempted to assimilate the insights and queries shared with me by colleagues, both through a formal questionnaire and through recurring discussions over the intervening several years. I hope that this report will encourage ongoing reflexivity about our collective interactional, institutional, and paradigmatic networks.

Centennials are times for taking stock — of where we have been and of where we are going. Let me trace the process that led to my survey and the tack it took as it acquired a life of its own. I am not a survey researcher at heart. The most intriguing patterns emerged for me in reading each questionnaire as a whole, as a statement about the professional life world of a single responding colleague. Many individuals did not respond formally to the questionnaire but chose to talk to me about particular issues that captured their attention. Relative to the possible number of respondents, very few questionnaires were returned.[1] Although there is nothing statistically significant in the questionnaire results, answers to most questions reached a saturation point where new answers were already familiar in outline. Not everyone agreed, even in assessing the same departmental tradition at the same time, but the range of responses was far from random as colleagues accepted the challenge to reflect on their own career trajectories. Each response added a dimension to the interpretive context of departmental organization of professional socialization in American anthropology.[2]

The problem of social cohesion among an amorphous agglomeration of individuals sharing a profession cannot be tied to any single unifying characteristic. We anthropologists imagine ourselves to constitute a community, but most of us do not know one another or ever expect to do so (Anderson 1983). Despite the apparent contextuality and fuzziness of our sense of solidarity, however, a few institutional infrastructures suggest how social networks might function sufficiently broadly that their collegiality can be attributed in principle to others who are not in fact part of a given individual's personal network. Two of the major ones are the umbrella mandate of the American Anthropological Association (of which more below) and the academic departmental structure of professional socialization (although no longer exclusively of employment).

Professional socialization, usually resulting in a doctorate in anthropology or a closely related discipline, takes place in (at least one) academic department that imposes a personal face on the larger discipline and provides an entrée to its social networks, theoretical potentials, and methodological predilections. Each such department develops over time a particular slant on the wider possibilities in the discipline and, whether consciously or unconsciously, orients its students thereto. Some of these identities persist across professional generations, while others are more ephemeral.

I have been musing for many years about what makes the identity of anthropologists unique among social scientists. I remain convinced that we are different from our nearest academic bedfellows in the social sciences and humanities. Against all odds and despite high levels of internal bickering both within and across subdisciplines, we seem to maintain an overriding sense of intimacy and social cohesion.

A few variables come immediately to mind. Perhaps the most salient feature that distinguishes us is scale. Despite quite remarkable post–World War II expansion, anthropology remains the smallest of the social sciences. Although the AAA certainly no longer meets in a single room where all colleagues know one another personally, this is our nostalgic origin myth and ongoing heritage. Most of us would not need more than one or two links to connect with any practicing anthropologist, at least in North America. For younger scholars, the first link might well be a mentor or teacher from graduate school. History of anthropology holds a surprising salience within the discipline, suggesting that many individuals are concerned to establish personal intellectual genealogies (Darnell 2001) to frame themselves within the variant traditions of American anthropology or their crossovers.

The relative youth of anthropology as a professional discipline allows anthropologists, through oral tradition, to retain a sense of continuity (which includes implicit or explicit contention framed in terms of prior positions) with many of our professional ancestors. Ongoing debate about the role of Franz Boas serves as exemplar. Contemporary colleagues care about how they relate to the Americanist tradition that Boas established, whether they value or critique it. Moreover, the intellectual genealogies of our own teachers go back to the founding ancestors. The history of sociology, in contrast, focuses on Emile Durkheim, Max Weber, and Karl Marx rather than on near contemporaries known personally to the gatekeepers of professional socialization.

There is, perhaps, an institutional precariousness to a small discipline with a short time depth in the academy that encourages its practitioners to know who they are and what they stand for. The distribution of practicing anthropologists across universities, museums, government organizations, NGOs, and so forth has further reinforced the impulse of many to counter centrifugal forces. The fragmentation often attributed to subdisciplinary specialization further underscores the need for a personal position on articulation to the discipline as a whole.

Anthropologists study the relationship between structure and agency. Most of us believe that individuals have some degree of creative capacity that transcends institutional and cultural constraints. The discipline was established and is sustained by real people whose life stories inform us about the structures they have created. Anthropologists both write and read professional biographies of major disciplinary figures. The great men (some of whom are women) of our history are parallel analytically to the key informants who appear in our classic ethnographies. The ethnographic method of framing an individual within a cultural context, particularly characteristic of Americanist anthropology, transposes effectively to the study of a professional culture. Under newer guises, reflecting the individualism of American public culture, the questions of culture and personality have survived the particular methodologies of its heyday.

Anthropology emerged as a discipline centered around firsthand fieldwork in small face-to-face communities; those of us who now work in complex societies, urban areas, or multiple ethnographic sites recreate such boundaries around personal networks of face-to-face interaction. The participant-observation method is adapted thereby to new circumstances and contexts, including, perhaps, the history of our own discipline, at least insofar as we tell it to ourselves. Many anthropologists approach their own history ethnographically, as A. Irving Hallowell (1965) long ago pointed out.

Dell Hymes wrote back in 1962, just as History of Anthropology (HOA) was emerging as a disciplinary specialization:

> After all, we have our own accounts of our origin, nature and destiny. Our revered elder men [*sic*] have often transmitted them to us in that part of the initiation known as the course on History and Theory. And who should know about these things of our past, if not ourselves, who have been initiated into the ways of the group, who are privy to its oral traditions, who can speculate retrospectively about our past with such authority and confidence in our identity as insiders? Others may know or obtain knowledge of the names, the dates, and the Important Theories; some of us have even led the public into thinking of such externals as the whole story, by publishing them as our history. But of course there is lacking the esoteric lore that elders sometimes impart to us, as badge of their status and sign of favor, orally in little groups —the personal detail that shows the trickster side of a culture hero, the exemplum that reveals the true hagiology of the field. [81–82]

This passage was written in the midst of an anxiety-laden debate within the profession about its shaky paradigmatic status as a science in the wake of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962). Anthropologists who wanted to see themselves as scientists did not look to historians to formulate their professional identity. Historians could not be trusted, because they were not stakeholders.

Nonetheless, George Stocking, the American historian who rapidly became anthropology's premier chronicler, formulated the question of disciplinary history in terms of a binary opposition between historicism and presentism (1968). He questioned whether anthropologists could avoid falling into the trap of interpreting the past in terms of contemporary standards, although even then most anthropologists would have laid claim to the methodology of historicism for their own discipline.[3] In the ensuing three decades, spent in the Department of Anthropology at the University of Chicago, Stocking's position has moved toward presentism in the selection of questions in order to create and sustain a reading audience among anthropologists. Historicism comes into this model as a subsequent methodological constraint rather than as a double-bind binary discrimination.

I included questions about the history of anthropology in the survey because I wondered how reflexive my colleagues outside this specialization were about their own past. Perhaps because teaching generally involves situating the current status of a subject matter, all academic anthropologists are regularly forced to think about how their areas of study have coalesced and are changing from some at least implicit baseline.[4]

Most respondents were very aware of issues that have concerned me as a specialist in HOA, but they resisted simplistic priorities for what that history might involve. When asked to rank the relative importance of theoretical paradigms, institutional frameworks, and interactional networks, many protested that these factors could not be separated and that all were significant. Those who did rank them cancelled one another out remarkably evenly—with a slight tendency to favor paradigms as the most significant variable and institutional context as the least. Moreover, several informants reported that their abstract ranking did not correspond to their personal experience as a participant in the making of anthropological history. There is an intriguing exceptionalism here, an apparent discomfort with seeing one's own professional experience as paradigmatic. Anthropologists do not generalize easily from individual case study to culture as a whole.

I hypothesized that the interaction between personal networks, unique but overlapping, and professional socialization plays a crucial role in how North American anthropology actually works. I wanted to explore how each practicing anthropologist constitutes and sustains a network of social and intellectual relations as the context for research and teaching. This meant returning to the institutional context of professional socialization and exploring how anthropologists draw on their training to form intellectual networks and to use it as a baseline for their theoretical work. I further hypothesized that at least the largest and longest established departments (but perhaps any Ph.D.–granting institution) develop a distinct and recognizable version of the discipline, which I reified for analytic purposes as a "Departmental Tradition."[5]

The research design focused on a few core departments, namely the oldest and largest ones (Berkeley, Chicago, Columbia, Harvard, Pennsylvania, Yale). I added the University of Arizona because of its historical emphasis on archaeology and the University of Michigan for its longtime association with a particular variant of sociocultural anthropology (although this department reinvented itself in the 1990s as "postmodernist" or "feminist" in the eyes of several respondents).

Because I am based in Canada and therefore define American anthropology as continental, I also included five Canadian departments: McGill, Toronto, British Columbia, francophone Laval, and Calgary (the latter because of its longtime departmental segregation of archaeology and sociocultural anthropology). The already intimidatingly large questionnaire included a supplementary set of questions targeting the uniqueness of

Canadian anthropology,[6] a question that mirrored the search for a stable and unique Canadian national identity (Darnell 1997, 1998b, 2000). Few colleagues responded to the questionnaire, but many initiated discussions of the uniqueness of Canadian anthropology with me and called for a collaborative effort at clarification. The resulting conference volume (Harrison and Darnell 2006) includes my report on the Canadian results.

The questionnaire was sent by e-mail to all members of the selected departments. The choice of "core" departments was made partly to impose a manageable size on the project and partly because larger, long-established departments seemed likely to manifest the strongest patterns of influence on their former students as they went on to careers elsewhere. In practice, however, some colleagues felt excluded from full membership in American anthropology by the emphasis on "elite" departments. Because part of my goal in following doctoral graduates after they left the departments of their training was to challenge the centrism of such an institutional elitism, the study was expanded to incorporate comments from anyone who approached me to talk about the questions raised. This invitation was confirmed in several issues of the "Centennial Countdown Column" in *Anthropology News*.

The question is a significant one for the history of American anthropology, particularly given George Stocking's recent explicit insistence that he writes and thinks "from the center," as one "belonging to a 'symbolic anthropological' elite" at the University of Chicago (2001:304). In relation to national traditions, Stocking suggests that anthropologists reproduce the discipline of their training, recombining features from the center rather than introducing innovative traditions at the margins (2001: 297–98). Stocking's reflection on his own collected essays takes "a present-day Chicago anthropologist" (2001:216) as the standard for his unabashed presentism, without problematizing the possibility of excluding anyone not trained in this departmental tradition. Disaffected respondents to my survey challenged such a disciplinary centrism even as they acknowledged the depth of their ties to the anthropological tradition(s) of their training. This suggests that many colleagues want to deny or at least rethink the hegemonic control exercised by core departments.

I hypothesized that the core departmental traditions diffused across North America to all kinds of institutions as most doctoral graduates moved out of the home fold, taking with them paradigms and social networks established during graduate training. I was interested less in the purported, often self-proclaimed, elitism of the core departments than in the process of diffusion and its role in creating an intersecting national or continental sense of professional identity. I assumed that students of so-called elite departments did not cease to be elite when they took jobs at a variety of institutions, by no means all of them academic. Through the chain of transmission of disciplinary oral tradition, their students and colleagues had potential access to the departmental traditions of their teachers in an increasingly wide variety of locations and contexts.

This is the process whereby American anthropology expanded from its base at Columbia University to establish footholds in other major academic centers. Although the spread of Boasian anthropology can be read as an imperialistic process, its very hybridity suggests enrichment and expansion of the original Columbia departmental tradition that coalesced around Franz Boas. Ripple effects of Alfred Kroeber's importation of Boasian anthropology to California in 1901 produced a new genealogy of Berkeley anthropologists, while still maintaining ties to Columbia. Edward Sapir's effort to graft the same tradition onto a Canadian base stock was less successful, largely because the supporting academic infrastructure was unavailable from his museum position (Darnell 1990).

Obviously, departmental traditions change over time, requiring attention to the variant perceptions of successive professional generations. The generation of Ph.D. was captured by decades. Responses for a particular core institution were variable even at the same period, but the similarities in response across institutions were even more interesting. Respondents reflected quite similarly on their experience of training and maintaining contacts with the home base, regardless of the institution or the time of their training. Because comments on each institution were both positive and negative, responses are summarized, preserving the anonymity of the departmental tradition wherever possible. Comments on the legacy of Boas obviously refer to Columbia, but other responses were virtually interchangeable with reference to the variables investigated. Coding the responses by decade alone lost some of the local detail, while following each institution through time did not produce contrasting patterns. Thus, each institution is assigned a letter and not further identified.

Respondents seemed to consider the genealogies of their professionalism most intensively at the beginning and end of their careers. The greatest number received their degrees in the 1970s or earlier, followed by the 1990s and 1980s. Fewer responses were received from those receiving degrees in the 1960s and earlier, although many of these came from emeritus professors reflecting (both positively and negatively) on the institutional correlates of their long careers in the academy. Thirteen percent of the respondents were retired, although all continued to see themselves as professionally active.

By the mid-1970s there were not enough jobs in the academy, never mind in its elite institutions, for the burgeoning number of Ph.D.'s in anthropology. Some stayed where they were trained or at similar institutions, but many very good people were located outside the mainstream as understood in the 1960s heyday of expansion. It probably is not surprising that our last bout of disciplinary navel gazing came with the job crunch of the late 1970s. Rereading several papers in the *American Anthropologist* while selecting articles to represent the years from 1971 to 1995 (Darnell 2002), I was struck by the cynicism and pessimism, never mind the blatant self-interest, of these reflections. The social reproduction of academic anthropology was threatened. Moreover, the worries came from the core as well as from the un- or under-employed peripheries. The prominence of this debate in the flagship journal reflects its salience. Oral tradition cites a golden age when expectations of elite employment were normative (and few nonelite institutions taught anthropology in any case). Forgotten is the number of anthropologists who disappeared from the disciplinary record because they failed to obtain any professional employment.

Despite retrenchment in many universities, perhaps with greater effect on small disciplines such as anthropology, the job market is now becoming more open ended than it has been since the 1970s as a result of retirements from the 1960s expansion. Yet we appear to have entered into a period of renewed angst about the nature of the discipline, its holistic four-field structure, its ethical and epistemological foundations, and its role in the academy and broader public forum. Segal and Yanagisako (2005), for example, argue that the "sacred bundle" of subdisciplines is a myth that curtails innovation in the contemporary discipline. Within the AAA this debate centers on the sections' versus the umbrella organization's capacity to represent all anthropologists. In some cases the relationship of core departments to the conventional institutional structure has been changed, as in the split of Stanford's anthropology program. The controversies surrounding the contrasting *American Anthropologist* editorships of Barbara and Dennis Tedlock versus Robert Sussman also derived from such issues. Departmental structures as well as those of the AAA will determine the future of anthropology as a discipline. Applying lessons of the past to this new context, let us turn to the 1970s debate.

Roy D'Andrade et al. kicked off with a pessimistic job projection in "Academic Opportunity in Anthropology, 1974–90" (1975). The authors, all based in elite departments and speaking from a position of self-confident privilege, predicted that the demographics of exponential growth and the end of the baby boom soon would force most new Ph.D.'s in anthropology out of the academy. Their students could not expect the same privilege they had enjoyed. They predicted that the increasing imbalance of supply and demand could not be compensated for by increased enrollment or retirement. The wake-up call to adjust expectations downward proved prescient, although actual figures have proved less dire than then envisioned, primarily because the number of new Ph.D.'s produced has leveled off at about 400 annually. Finding jobs for students would produce "great strain on the large graduate departments which have a highly developed academic orientation" (1975:770). The authors apparently did not notice the patronizing quality of their dismissal of "practicing anthropology," which soon became the fifth subdiscipline in the AAA structure and demanded respect for its resetting of research priorities as well as locations of employment.

Givens and Jablonski (1995:311) reported in the AAA survey of 1994–95 that 87 percent of Ph.D.'s worked in the academy during the 1970s, whereas almost half were employed in "applied" or nonacademic contexts. During the same period, the proportion of women among these doctoral graduates rose from 32 percent in 1972 to 59 percent, while the non-Euro-American proportion declined from 96 percent to 84 percent. The grounds of privilege certainly have shifted from what Stocking calls the "center" to perspectives from the purported margins (which perhaps are becoming heterogeneous new centers).

A. E. Rogge continued the debate in "A Look at Academic Anthropology: Through a Graph Darkly" (1976). Exponential growth of the AAA and specialized professional organizations (especially in applied anthropology), increase in number of journals and number of departments teaching anthropology, and the number of Ph.D.'s produced all seemed to approach a fixed limit. But feedback mechanisms could correct such imbalance without conscious effort. Rogge predicted a "built-in oligarchy" in which a very productive minority would become even more productive, while the less productive majority increased (1976:836). Unlike D'Andrade et al., Rogge emphasized the undesirability of this trend and expressed an egalitarian ethos in the discipline, at least outside the self-styled elite, among whom things presumably went on much as they always had done.

The elitism implicit in this literature may be mitigated in practice, however, by a social context of "invisible colleges" or "coherent social units with a maximum size of 100 researchers." Rogge has "often heard senior anthropologists fondly recall an earlier time when they knew practically everyone in their field socially. Exponential growth has apparently atomized the field" (1976:837). This is the intimate social network that I suggest underlies the existence and persistence of departmental traditions. It is invisible, because each practitioner combines connections into unique personal networks. This often face-to-face, long-term interaction sustains an oral tradition that constitutes our disciplinary imagined community.

Beverley McElligott Hurlbert's "Status and Exchange in the Profession of Anthropology" (1976) documented the accuracy of the "common suspicion" that "elite" (long-established) universities had great prestige and hired primarily their own or each other's graduates. Even more significantly, nonelite institutions also preferred to hire graduates of elite institutions. Elite institutions were defined as those that graduated Ph.D.'s before 1960, produced the most Ph.D.'s, and were "highly ingrown" (1976:279). Despite several subdivisions of nonelite departments (with UCLA, Stanford, Arizona, and Cornell having the most elite institution hirings), the few elite departments stood out. This hierarchy presumably originated in an old boy (and, to a much lesser extent, old girl) network, but its elitism did not filter down to graduates of elite institutions teaching elsewhere, at least from the standpoint of those who remained at the elite institutions.

I asked respondents whether they saw the discipline as elite or egalitarian. The normative response was "slightly elitist" both for access to graduate programs and employment thereafter. Employment was perceived by many, however, as more elitist, and thus exclusive, than initial enrollment. This suggests that the elitism Rogge described has not disappeared, despite a wistful ethos of egalitarianism (expressed, for some, in the intimacy of continuing graduate departmental cohort interactions, often in the context of the AAA). One respondent observed that we may assert our egalitarian values at a personal level of private culture, but that we need "anthropological analysis to see the sharp elitist distinctions," since "schools could be ranked and their hiring clearly reflects that ranking." Hegemonic consent sustains the hierarchy, despite widespread reservations.

A question about perception of existing at the center of the discipline during graduate training and at the present career stage confirmed that many of our colleagues feel extremely isolated from the centers of their training and/or unsupported by their home department ties, because they function in dramatically different institutional contexts (smaller universities, often without graduate programs, museums, or nonacademic employment). For some, this solidified ties to departmental traditions of training, while for others the isolation alienated them from a mainstream they felt had abandoned them. One respondent bitterly characterized anthropology as "an unpalatable career choice," because there are no jobs and the ones that exist do not facilitate research.

Roger Sanjek's "The Position of Women in the Major Departments of Anthropology, 1967–76" (1978) took for granted an elitist structure, selecting 22 departments with at least 30 Ph.D.'s employed in departments listed in the then most recent AAA *Guide*. (The 8 selected for this study are all in the top 10 of Sanjek's sample, which also included UCLA and Cornell.) Most of the women specialized in sociocultural anthropology, with the smallest proportion in archaeology. The duration of women's appointments was shorter, perhaps reflecting more recent hirings (some based on affirmative action policies) as well as disproportionate failure to obtain tenure. Men who left an appointment were much more likely to obtain other employment listed in the *Guide*, whereas more women disappeared from this public record. Interestingly, institutional patterns in these elite departments varied considerably, ranging from virtually no women, at least until quite recently, to steady increase to hiring beyond the frequency in the profession as a whole. Patterns of senior women's appointments were also institution specific; that is, gender discrimination did not take the same form everywhere or uniformly characterize the discipline.

Respondents were divided almost equally between men and women. Given the demographics of the profession, of course, this means that women responded in greater proportions. Women were also more likely to mention their spouse or partner, as collaborator or as sharing their departmental tradition. Feminist anthropology was mentioned as an area of specialization by a number of young women in sociocultural anthropology and by all age groups in archaeology (possibly signifying increased acceptance of women among the most recent professional generation of archaeologists).

Over half of the respondents listed sociocultural anthropology as their specialization, although this was also combined with archaeology (most frequently), practicing anthropology, and linguistics. About one quarter of the respondents claimed archaeology as their field, some combining it with sociocultural, biological, and practicing anthropology. Slightly over 10 percent were biological anthropologists, combining this with sociocultural anthropology more often than with archaeology. Commitment to four-field anthropology survives in the frequent rejection of a single subdisciplinary identity. The few linguists who responded tended to be trained in departments of linguistics at the same institutions selected as core for anthropology (i.e., elitism is defined primarily by overall institutional stature and only incidentally impacts on anthropology). Those linguists who held no degrees in anthropology often felt uncomfortable identifying themselves as anthropologists.

A number of respondents rejected the idea of ongoing mentorship, but many of these simultaneously acknowledged more-or-less useful advice, especially at the beginning of their careers. The continuing asymmetry of a mentoring relationship may be rejected as anti-egalitarian and uncollegial. Questions of continuing contact with mentors were difficult to evaluate, however, because many had died, failed to obtain tenure, moved to other institutions, left the discipline, and so forth. These factors increased in weight with time since leaving graduate school.

Several respondents reported continuing contact with their graduate-school cohort but considered this more a question of friendship than of professional activity. "It is difficult to see if it is just departmental network connections or sociality." That a professional network might be based on personal friendships did not seem to enter into such a characterization. No respondent articulated a connection between sociality and remaining part of the disciplinary elite. Many noted, however, that meetings, especially AAA meetings, kept them in contact with the full scope of the discipline as their specialized work did not. Large departments, including the core ones, are more likely to provide such an extensive network.

Characterizing the Departmental Traditions

Comments on departmental traditions and the experience of graduate training at elite institutions are candid and often negative. This summary will juxtapose them to show both contrasts and similarities. Anonymity of both respondents and departments requires that this material be presented according to themes rather than individual overviews or histories of particular programs (although Columbia is inevitably identified by comments on its ongoing assessment of the legacy of Boas). Responses are organized by decade. Former students have high expectations, largely met, for quality education and for an enabling social environment, less often met. Sharply negative comments are often qualified or balanced by more positive experiences reported elsewhere in the same questionnaire. Undeniably, however, many recall their professional socialization with ambivalence, resentment, or even distaste. Moreover, the same period at the same institution may be assessed very differently by different respondents. In short one cannot condemn programs criticized in this kind of response. One can, however, acknowledge the legitimacy of the feelings expressed by the respondents.

One respondent suggested that disciplinary cohesion might result from the fact that "we are always 'out of place.'" Although anthropology may look fairly cohesive from the outside, "some of the growth of the field is rooted in controversy and alternative views." For many this kind of conflict may be positive, a source of intellectual vibrancy.

The Fifties

A few emeritus respondents, mostly at Columbia, still considered it important to come to terms with the Boasian legacy of the discipline. One considered the four-field approach an "imperialistic academic ploy" for the early 20th century, now "outdated, irrelevant and obfuscating." At Columbia in the 1950s and 1960s, one respondent reported that Boas was "dead and buried, not in fashion and we didn't learn about him." Columbia was "eclectic" then. This alumnus felt himself to be a student of the whole department, because he was interested in more than just cultural anthropology. He began thinking neoevolution was the center, although his later work has moved increasingly toward theory and HOA. There were "wonderful fellow students . . . and we spoke to each other." Although he now recognizes that he may have been naive, he never felt competitive within his cohort.

A woman who listed Ruth Benedict, Margaret Mead, and Alfred Kroeber as mentors entered Columbia "the year a great schism was launched." Benedict "and the few other weak Boasians" found themselves opposed to an anti-Boasian contingent with "male clout and non-support of women students." Julian Steward "gathered the male students around him." The two years with Benedict were a "very good time" despite this pervasive sexism. Columbia certainly saw itself at the center of the discipline. Kroeber was sympathetic but had "bigger objectives" and was dubious about supporting women. In practice this respondent has had no departmental network. (A later alumnus thought Columbia's discontinuity of tradition was "such a waste, when the spirit and fieldnotes of Franz Boas lingered in the building.")

An emeritus graduate notes that his mentors (Everett Hughes, Robert Redfield, and Herbert Blumer) all would have denied the existence of a Chicago tradition. Like them, this respondent never wanted to be at the center of anything. Yet this attitude came from, or at least was reinforced by, the departmental tradition. It is taken as normative by other respondents.

A Department B graduate from the 1950s considers the methods learned there and in Department A for previous degrees as continuous and productive, because some of the same teachers worked at both institutions and "perhaps because both were long-standing departments." She worked part-time at the associated museum and found this part of her career a good time. A strongly empirical, holistic four-field approach emphasized fieldwork, and she still finds this a tradition worth passing on.

The Sixties

Department D in the 1960s was "quite vital" and featured "humanistically oriented cultural anthropology," a tradition that the respondent tried to impart to his own students. This individual felt at the center of the discipline then and now, a student of the department as a whole.

In the 1960s Department F was a good place to be because of the "leading lights" teaching there. Feeling at the center then, this respondent now feels marginalized by teaching mostly applied anthropology. His has been a move of breaking away.

Department B was characterized by a biological anthropologist trained in the 1960s as a four-field program facilitating a "biocultural approach to health and disease." This individual identified with the department as a whole and considered it at the disciplinary center. The "formative state" of the department in the 1960s and 1970s expansion of anthropology, and of the academy generally, made it a good time to be there.

Another student from the 1960s characterizes this departmental tradition as deriving theory from empirical ethnology, with students undertaking "community/regional ethnographic research in complex nation states of Europe and North America." Theory "does not become an 'ism' to be demonstrated or upheld by future ethnographic research." The camaraderie was notable: "I felt that we were students of the whole department . . . touching all of the subfields, connected to all of the professors and graduate students. . . . I still feel part of the whole but at a tremendous distance and in a sadly attenuated way" (as a practicing anthropologist). "We knew that we (they) [the tenured professors] were squarely in the right with respect to the great debates of the day." There was a continuous faculty tradition back to the departmental founders. "At the same time there was a significant sub-theme of application [applied anthropology] being valued both for its ethical merit and for its contribution to knowledge." The museum coffee shop was a meeting place for this synthesis.

A Professor Emerita trained at Department A in the 1960s had "no real mentors" and was very naive about "different treatment for students with some kind of social and economic standing, especially if they were male." When she asked a friendly faculty member for a job reference, he responded that he "didn't know much about me, but I seemed to have a good moral character." "I didn't realize for years that I might have expected support from [the institution] after I left." She was never part of any "inner circle" at the department, although "multiple traditions" tied together professors and their students. Her husband was "truly a [departmental] man" and received more support. This response suggests that the elite departments claim only some of their alumni/ae. The value of the prestigious institution was fellow graduate students, libraries, and "the very great education I received." There was a "reflected glory of the [institution] degree."

> I was at [the institution] when [it] was at the pinnacle. I think my cohort was the best I know about. It was post WW II, the GI bill had broadened the socio-economic background of students, and we felt we were getting instruction at the cutting edge of the discipline. Many of those post World War II students went on to jobs in many of the established and growing universities.

Identification with the department did not extend to the more social end of anthropology (the respondent is an archaeologist).

At Department H during the 1960s, there was a "spirit of mutual support and study groups." "This was [the institution's] best time." These models were useful in building a department later in the respondent's own career. "Graduates of [the department] in the pre-pomo period feel we are products of a now-dead tradition, which we still champion." As a woman, this respondent felt far from the center then but now feels closer.

The Seventies

An alumna of Department D during the 1970s "never much liked the Anthr [*sic*] department tradition there" and maintains little contact with it. The area studies program "was rather a sink-or-swim proposition, with a lot of antagonisms." But the other elite institution where she did her master's (Department A) was worse. She remembers her undergraduate degree at Department B "as a wonderful time" when she hung out with "a terrific cohort of graduate students" with more pleasure. But the doctoral department was at the center of the discipline, one of the few places for Near Eastern archaeology at that time. Everything changed when one senior faculty member left. She was one of a few archaeologists who took sociocultural courses.

A practicing anthropologist trained in the same program during the 1970s notes a limited continuing departmental network, because he is not an academic. But the characteristic methodology, previously also learned at Department B, facilitated the "invention" of cultural resource management. The new archaeology made it an exciting time, with archaeology becoming "a bit more real and a bit more anthropological." This respondent now appreciates the education he received and identifies more with the sociocultural as well as archaeological parts of the departmental tradition. On the other hand his kind of work is "definitely stigmatized." Thus, there has been little career help from the departmental tradition. Another 1970s alumnus sees this as "the Golden Age of Department D—a rare moment in time." Surprisingly, his interests were not matched there and he is somewhat alienated.

A linguist trained at Department D during the 1970s notes that the program emphasized critical thinking rather than specialization, forcing students to identify with the department as a whole. "Challenge," rigor, and clear writing were the standards. Areal ties were "eschewed" to "simply hire 'the best.'" Interdisciplinary work increased the sense of being at the center, although this feeling is not quite so strong for this individual now. Another linguist from the same period emphasized a four-field approach with crosscutting themes. It was a dynamic time, because "the Linguistic Wars were just heating up." The linguistic tradition was "agnosticism and occasional apostasy vis-à-vis structuralism and its Chomskyan variant."

At Department F in the 1970s, it was possible to be a student of the whole department, because students were not competitive. Mentors helped students feel at the center of the discipline, although this respondent now feels more identification with his area of specialization. It was a good time to be studying anthropology, because of the "ferment" of the 1960s and 1970s, "both academically and politically." During this decade, ethnography was the "core method." Despite this optimism in principle, however, for this informant it was "not such a great time" to be there, because the department was "still recovering" from faculty departures in the 1960s and the new trends of that time did not fit the respondent's interests. But the institution itself was "an exciting place." He hung out in other departments, and there was "Vietnam of course." This respondent felt at the center then but now works outside an anthropology department and feels central to something other than anthropology.

It was a good time to be in biological anthropology at Department F in the 1970s, because primatology was "trendy." But this was not and is not the center of the discipline.

In the late 1970s Department F facilitated its own version of sociolinguistics and a Kroeberian version of Boasian cultural theory, with a "schizoid" shift between "remnant British social-functionalism and U.S. cultural particularism" just as postmodernism was creeping in. Although affiliating with a subcamp as a student, this respondent, like many others, now feels an identification with the department as a whole. He felt some confusion about the centrality of the department then and now feels far from the center. It was “perhaps not” a good time because it was “splintered and incoherent” (which may *be* the tradition). With “little substance to training and experience,” a small cohort, and “petty jealousies and minor neuroses,” breaking away has been the primary pattern.

Department G in the 1970s favored “certain ideas about adaptation, environment and a sort of materialist approach.” Ties between Michigan, Columbia, and CUNY have maintained a “genetic line” for this tradition, mostly through hiring. Identification was with the department as a whole. Another alumnus of the decade located the department at the center then but far from it now. He is “not in a departmental network any more.” Despite good faculty and classmates, “the late 60s were not calm,” everything was “politicized,” and there were “too many distractions.” Another 1970s graduate characterized the tradition as cultural evolution, “not a hot topic these days.” It “seemed good at the time” but is “not all that valuable now.” There were role models but not mentors.

A Department B archaeologist from the 1970s cites strengths in fieldwork, especially with state-level societies. Archaeologists were trained in cultural anthropology, although archaeology was less theoretical. She felt at the center then but does not now, despite the continuity of running a field project over half a century old (through a university-affiliated museum).

A graduate of the 1970s describes a senior faculty that had little contact with graduate students and a junior faculty that did not get tenure. The interdisciplinary Social Relations program dissolved while he was there, leaving him “mostly on my own,” although the first-year seminar produced a certain degree of solidarity. This respondent still feels on his own and was never at a disciplinary center. It was not a good time to be there. The continuity in his career has been to “keep plugging away.”

Department E in the 1970s was “highly and adamantly traditional” and “academic (as opposed to applied).” Its elitism meant that graduates were expected “to go into academia.” Students were pushed into being students of the whole department. It was a good time to be there: The institution “had a great deal of respect at that time. It was easy to establish a network among anthropologists then. The whole field was so small. Fieldwork was demanded and so was [*sic*] excellence and professional contributions.”

Department H in the 1970s was an exciting place. This respondent “made it my business to establish intellectual and personal relationships.” This was the time before (a single faculty member) “almost personally, destroyed the department.” But another respondent from the same period found the same department a positive experience because of “strong, prominent faculty members although the treatment of female faculty made [her] personal experience somewhat negative.” The Graduate Anthropology Association helped to unite some former students of the department. This respondent did not think the departmental tradition was at the center then or now. Another respondent suggested that the department in the 1970s privileged a "materialist, culture evolutionary perspective." "Interdisciplinary in-fighting" made it not a good time to be there, though the respondent now identifies with the department as a whole. Yet another graduate from the 1970s cited indirect effects of the departmental tradition as a counterexample. A "sink-or-swim" method in training for fieldwork, student rioting, and a "highly fractionalized social structure" produced an identity only with the specialized field. The respondent's work in cultural ecology was central to that field but peripheral to the discipline as a whole.

The Eighties

Department C in the 1980s was a holistic four-field department with graduate core courses, but it "seemed to favor archaeology," the respondent's subdiscipline. "Relatively little cut-throat attitude" made departmental identification possible. It was a strong department then, with big names who mentored their students (despite a high attrition rate). The respondent feels less central to the discipline now than then. Influence has been more from "coming out of a good department" than anything particular about this program.

Another archaeologist from the same period reacted quite differently, finding the departmental tradition "destructive and corrupt." It was an "awful time," with students drawn into factionalism, favoritism, sexual relations with graduate students, and an indifferent faculty. The exception was a mentor, now deceased, who neither fit this negative pattern nor modified the respondent's view of the general ethos. It was "a great education in many ways," but he has chosen to go his own way in theory with no help from the departmental tradition.

A sociocultural anthropologist, trained at the same institution in the 1980s, found "an archaeological school" somewhat less welcoming. Few people "did what I did." Although she took sociocultural and sociology courses with faculty blessings, "cultural has fractured into a mismatch" in this department. It was "a great education" but without support for her interests in museology or ethnohistory.

A 1980s alumnus characterized Department G's tradition as a "not terribly rigorous cultural materialism" with archaeology (his specialization) separated off from sociocultural anthropology. He "naively" felt at the center then; now he feels intellectually but not socially well connected, with significant discontinuity from his training. He would not want the departmental tradition to influence his students. A biological student from the same period was not as fanatic as his mentors about a "lumping" tradition and a regional-continuity approach in paleontology. He now identifies more with the department as a whole but feels somewhat less central to the discipline. He is uncertain that one time is better than any other — "a vibrant, active department is always good." An archaeologist identified with a specialty in processual archaeology, seeing it as the center then but not now. It was a good time because of "faculty at the peak of their careers, vibrant intellectual environment, active research."

Another 1980s respondent began as an archaeologist but has moved into forging a bridge between biological and sociocultural perspectives in order to "contribute to the population under study." Neither subdiscipline has been aware of the insights of the other. The departmental tradition of "culturally molded evolutionary biology" produced little contact between biological and sociocultural perspectives, with archaeology caught in the middle. She pitied sociocultural students, because they were "undirected and unmentored." "We were not one department" because of "the isolation of the cultural faculty." Her indictment continued:

> Aside from doubling my knowledge in two years among some of the most accomplished scholars in the world, my emotional and intellectual experiences at that time were horrifying. As a whole, the department was sexist, cruel, elitist and the faculty spent their free time in using graduate students as power pawns in struggles for power. It was not a good time. For the most part, the faculty ate their young, leaving [a faculty member] to carry out their fantasies.

The biological faculty were also "weird." "One professor constantly sexually attacked new students which led me to organize the women." Cultural faculty were "insular, elitist and happy to control their own students" (still an improvement from the 1960s). Nonetheless, everyone felt themselves at the center of the discipline then and now; she acknowledges the arrogance of this position. Her cohort relations depend on a "study-self-criticism group" from graduate-school days that is "still going." In her present employment she feels very isolated, in a four-field department that avoids discussing theory and "has no tradition." Survival as retirements are not replaced has become the priority. She could not manage without the support of her departmental tradition cohort.

Department A in the 1980s emphasized interdisciplinary work and lacked close mentoring. Biological anthropology was comparative and evolutionary. Despite specialization, the respondent was "very aware of fit with others," with "an intense intellectual excitement about what I was doing." The tradition was at the center of "the interesting part" of anthropology. This optimist thought it was a good time to be there despite the uncertainties of a transitional period of hiring, because there were excellent visiting professors.

Another 1980s alumnus identified a holistic tradition of critical scholarship in which students identified primarily with their subdisciplines. Expectations were different for archaeology and sociocultural anthropology. "Colleagues were always trying to put me in a box." It is getting harder, across the discipline, to be an anthropologist first and then an archaeologist. This respondent, one of Gordon Willey's last students, was glad to be part of the end of an era, "a transitional time for Mayan studies."

Department A archaeology in the 1980s was "eclectic, data oriented, not terribly theoretical, individualistic." At the time this respondent identified with the department as a whole, but he now sees the special field as the center for him and for the discipline. The high quality of faculty and students made it a good time to be there. This individual stayed at the institution where he obtained all of his degrees.

Department A in the 1980s struck one alumna as "a dying set of concerns of the discipline." She is unable to define a center for the discipline then or now; her subsequent career has involved breaking away. "My training was with individuals and a department that was top-heavy with individuals who were not innovating in the field."

A linguistic anthropologist trained at Department H during the 1980s laments the demise of an independent linguistics department during graduate tenure, encouraging great identification with the anthropology department. This individual does not feel at the center then or now.

The Nineties

An archaeologist trained in Department C during the 1990s moved toward advocacy on land use and intellectual property issues in response to tribal cultural-preservation work and teaching Native students. "Ethnographic detail, especially where information may be sensitive," seemed less important than the potential utility "to them" of broad comparison. The archaeology program was "very sociable," with few barriers between faculty and students and a diversity of theoretical approaches. Team archaeology contrasted sharply with the atomism of single mentors in sociocultural anthropology. Although the institution was "research-oriented," students in the era of NAGPRA were becoming more applied. It was a good time to be there because of distinguished elders and extensive field and lab projects. The respondent moved away from environment and technology, but mentors encouraged cognition and iconography.

Another 1990s graduate cited a great cohort of supportive women who regularly meet at conferences. They serve as one another's trusted assessors but "tend to work behind the scenes, . . . not formally." A smaller cohort of graduate students met through fieldwork. The archaeologists were processualists "with a strong commitment to science," which was not "a hard-line positivist position." The department was "still at its peak," although students "missed a lot of cutting edge stuff" just beginning to come out.

A recent graduate of Department D reports a departmental transition during his program from a "male, white and quite old" faculty to a younger one with at least half women and a broader range of theory. This respondent emphasized the individualism of faculty approaches and denied the existence of a center to the discipline, especially in its four-field manifestations. In addition to this individualism, the departmental tradition included "writing clearly and working with a clear political goal in mind."

For Department D in the 1990s, "theory drives ethnography," with clear links to history of philosophy and social theory. This respondent felt himself a student of the whole department then and now, but the certainty of centrality to the discipline during graduate school has declined since his leaving. The transition caused by many retirements made it a good time to be there. Another student from the same period notes a "stimulating tension" between "Americanist culture-fetishizing" and British social anthropology. "A rigorous course of study" involved the reading of canonical anthropologists. Departmental unity was enhanced by the weekly seminars. Teaching in a peripheral geographical area, however, makes this respondent feel much less central now.

Another 1990s Ph.D. who was "formed by that tradition" and remains close to her cohort cites a departmental tradition focused on "culture, power, history, including both historical ethnography and a historical perspective on our own discipline." Though her own project was peripheral, the department itself was at the center of the discipline. Her interests have "moved closer to the center." Her cohort "built on our teachers' work in ways that were surprising"; in a time of "flux," they were less clones than previous generations. "I *do* feel connected by virtue of my [institution] mafia connections and identity."

Department F in the 1990s was characterized by one respondent as good for people and experiences but bad because of departmental decline and infighting. Identity was by specialization, although this respondent felt at the center both then and now. Another respondent, "no longer committed to ethnography" because he was unable to establish "a paying career" in the discipline, "no longer affiliate[s] with the discipline." "I was born too late for the kind of career I wanted in anthropology." Resulting relationships with "mainline employed anthropologists" have become "very awkward." The department was "rather post-symbolic with some power/hegemony thrown in." There was a huge gulf between archaeology and sociocultural anthropology. Professorial fiefdoms were "very isolating" and "superpolitical." Cognitive anthropology disintegrated just as cognitive science was becoming important elsewhere on campus.

Department A in the 1990s felt like a center of the discipline, although the respondent now feels isolated from its elite network by teaching at a small liberal arts college. It was a good time during the "transition from traditional to post-modern thinking, and back again, while I was there." Active faculty research and publications kept students linked to the larger discipline.

The American Anthropological Association

Fifteen percent of the respondents are not members of the AAA. Listening to or giving papers tied with meeting friends as primary reasons for AAA attendance. Book exhibits and publishing concerns came most frequently in third place, far ahead of job search or interviewing and organizational responsibilities. The latter category, however, strongly motivated the attendance of those to whom it applied. Many noted the AAA as their "primary association," maintaining their ties to the disciplinary center, although almost all also attended smaller, more specialized conferences.

One emeritus respondent mused that anthropology was "weird" as a distinct discipline and deemed archaeology and most of biological anthropology irrelevant to the sociocultural core. Fashion and fads rather than intellectual unity dominate. Archaeologists and biological anthropologists feel most alienated. A biological anthropologist complained that "more respect" for this specialization was needed; for example, sessions should be scheduled in reasonable sized rooms. Others thought that biological papers served mostly to inform sociocultural anthropologists about this specialization and were not useful to specialists who gave papers. Many feel that the AAA is dominated by sociocultural anthropologists who wish the others would stay away. Those with a four-field approach have not been successful "in making the meetings important to the other subdisciplines." An archaeologist argued that the AAA was too big and diffuse, with sociocultural papers unintelligible to the uninitiated. Biological anthropologists are alienated, especially during a recent perceived postmodernist turn in the editorship of the *American Anthropologist*. The AAA "doesn't really cater to my field," says a respondent who finds herself "on the edge of about six different meetings" without a single comfortable venue.

The AAA focuses on "what you have called core departments. I don't work at one of them, so AAA doesn't do much for me." This respondent "detests[s] the knee-jerk politics" (e.g., Patrick Tierney's critique of Amazon development), "unremitting focus on ethics (other people's bad, mine good) and its naive devotion to 'public policy issues.' " The AAA is too big and "obsessed" with jobs and networking with "the right people."

The lack of emphasis on volunteered papers makes it hard for "people who are not very well connected." Many respondents found the meetings "too large, diverse and impersonal." "The AAA is no longer fun to go to or very useful intellectually."

The AAA got good ratings for ethics and global events where anthropology has potential to be useful. The AAA is recognized as an effective advocate for sociocultural anthropology. Many praised the journals and maintained multiple section memberships in order to obtain them. Some subscribe to the *American Anthropologist* primarily for its book reviews; others praise the flagship journal's role in the whole profession. The meetings help people keep abreast. The website and the meetings received kudos from many. One respondent thinks the AAA is effective because it "doesn't try to 'affect' lots of things." Job services are praised. The AAA is "an effective rite of renewal." The meetings are often best outside one's specialization. The meetings gather people with common interests. They "signify disciplinary history, in which all of us have a place, even if at arm's length like myself. The organization symbolizes the discipline."

Though generally effective, the AAA "could achieve a higher degree of public recognition and authority." We need "a credible capitalist economic imperative" to protect academic freedom for politically incorrect or innovative thought. "With luck, a person influences students to be better citizens by transferring one's anthropological view of system and ethics and experience." It is easier to influence "the larger, current society of one's ethnographic research site . . . because your 'story' is about them. . . . They have a commitment to, and deep understanding of, the ethnographic context." Our own society simply sees exoticism and must compare it "to our own way of life."

Many who now work away from the supposedly elite centers of their training remain optimistic. The AAA provides "an exiting time." It is "refreshing" to see other anthropologists. The meetings are crucial to maintaining a sense of the discipline while teaching in a small liberal arts college. This respondent "hangs out" with a graduate-school cohort at the meetings.

For many the AAA has lost its sense of holism. Students no longer "get to understand the uniqueness of the anthropological approach." A respondent who attends annually reports "a terrible meeting that reminds one of the truly sorry state of the discipline. The great majority of the papers are bad and the general mood is extremely unprofessional, if not pathetic in its sad attempt to define a powerful role in a larger society that has no idea what anthropology is." Another respondent finds "services poor, meetings chaotic, politically hyper-sensitive." A respondent now employed outside the discipline found the AAA ineffective in job searches, despite the promised personal effort of an AAA president at the time.

Most respondents who stipulated AAA membership listed the sections to which they belonged. About one-quarter each listed one or two sections. The most frequent stand-alone sections were general anthropology, biological anthropology, and archaeology; this confirms the explicit statement of many respondents that the AAA is not a welcoming space for members of the latter two subdisciplines. Withdrawal of the Conference on American Indian Linguistics and the Society for the Study of the Indigenous Languages of the Americas from meeting with the AAA, even in alternate years, may reflect an even more distancing breach. About 10 percent each claimed three, four, or five section affiliations, with more sporadic memberships up to eight. For many the sections work as a way to monitor the "direction of the field."

The majority of respondents, however, were indifferent to the section organization, although almost none were actively opposed to it. Many found the sections a fertile field for networking and a way of putting a personal face on a large and unwieldy meeting. "The sections helped to build it" says a recent Ph.D. recruited through departmental connections for section leadership. Some, however, complained at the expense of (multiple) section membership. Some felt that the sections duplicate specialized meetings. "The holistic approach is dead." For many respondents AAA sections differ in effectiveness. No respondent noted the reserved program committee and presidential sessions designed to cross section boundaries and enhance unity beyond specializations.

On Influence

Many respondents felt that great persons "set the conceptual agenda." They draw the readership and thus demonstrate the consequence of ideas (although timing is critical to attaining individual influence). Influence is possible if the individual is clever and the spirit of the times is receptive. Great persons "set traditions in motion"; real influence, however, needs a rare combination of scholarship and application. The greats are "the ancestors of still-vital lineages, like mine." Charisma was necessary for the small number of exploratory greats of our first century; they now serve as straw men. Those who choose anthropology are vulnerable to a rhetoric based on idealized romanticism.

Other respondents felt that great persons must not be "too original or have too broad a knowledge." "Only a handful" have the ability to "set the agenda and change it." Great persons often have a "stultifying effect." It is hard to influence trends, but this respondent does not "follow trends much" anyway. There is little larger impact, because the powerful do not want "objectivity" and our culture denies the significance of culture. There are fewer great persons "because of the fragmentation of the discipline." Influence works best in the "sub-sub-disciplines." Only "tireless self-promoters" make it in a larger discourse.

It was suggested that individual impact could be much greater when the discipline was smaller. Specialization decreases the size of the potential audience. Nonetheless, we recognize important individuals: "Some even do real fieldwork and have to analyze data." The increasing size of the discipline, loss of focus, and "the increasingly personal focus of much socio-cultural" work make it harder to influence anything. Individuals are important only if they represent "larger bodies of thought." The greats are important, because "our discipline is one of ideas."

Respondents emphasized egalitarian as well as elitist influences. The creativity of great individuals comes in "interaction with others"; HOA must include persons not included in the conventional catalogs of greatness. The greats are important for their effect on students. The importance of great persons may come from the way we construct disciplinary histories; influence is easier if one has access to grants or influential journals. It is easy to be influential if you publish in the right places. But great persons have the capacity to create cults, "like Boas," making inclusion of more people critical. Great persons have a "critical power, for good or ill, in shaping the direction of research." Although history may repeat itself, "primacy" should get credit and "without knowing work that has gone before one is not truly educated." Continuity is valued by many respondents.

Many believe that all of us influence the discipline, another resistance to the centrism of elite programs only. It is unclear whether institutional hegemony is distinguished from the version of anthropology transmitted by training in a particular departmental tradition. Speaking of all anthropologists, one respondent suggests: "Over time, we as a group can have an effect on policy." It is even easier to affect smaller segments of a larger society where we work; we are breaking away from isolated fieldwork in exotic locales. And we certainly affect our students.

One respondent suggested that the sociocultural focus on ethnographic area kept each anthropologist thinking independently, making it difficult to influence the discipline as a whole. Another noted the significant role of a few people, with a masculinist bias toward what theory counts. Yet another noted little academic focus on the past and so doubts the great influence of individuals. "That's the stuff we're all taught," but this is not necessarily the way it should be. Some argue that the role of great persons is exaggerated, with many never recognized; it takes years to influence the discipline. It is easy to be influential but more difficult to be *the one* who is influential. It is much harder for a woman to influence trends.

Several respondents acknowledge that we have less control than we think. The greats both create and express a trend. Anthropology gives us the reflectivity to understand these processes. "We are a society. We have our elders who have pioneered the way. Yes, individuals can change things." The history of anthropology is "all about the people in it." Another noted that "if they are brilliant and can express their ideas, it's relatively easy in such a small field; otherwise, their ideas may be unrecognized for decades." We have had little impact outside our own field.

One respondent used Binford's new archaeology to consider "the creation of high profile disciplinary leaders. . . . Many of them take a firm stand against a previous way of doing things." The initial stance was "overdrawn," but a "new equilibrium" had lasting effects.

Respondents were extremely pessimistic about the possibility of influencing public discourse. "We get called for our expertise when a crisis occurs but otherwise I think we do not have as much influence as we should." "We need to be better at PR. . . . I think that the anthropological cross-cultural perspective is so important in today's world that we must pay more attention to ways to disseminate it." Another noted that "we have the knowledge to share," but the AAA is "unrealistic" and often unethical in seeking influence by serving those in power.

Public influence is hard to attain, because the collective forces in the larger society are more impersonal. Networks work for us internally but rarely extend beyond the discipline. Moreover, anthropologists working outside the academy, often in interdisciplinary contexts, do not identify themselves as anthropologists. "That makes anthropology cozy and communal but it means that we may be losing a big opportunity to make a difference in the world." Only by establishing networks outside anthropology can real influence be attained. Influence requires getting organized. "If you are not from a high profile program or [do not] have a job in a high profile program, it's easy to become quickly marginalized . . . [and] the likelihood of getting the time and support to sustain a productive research career is tough at best. . . . I would like to think of anthropology as increasingly becoming more of a policy discipline but I fear that we are pretty marginal to the overall political discourse in the US and elsewhere in the first world."

Virtually every respondent acknowledged the difficulties of keeping up with current literature and trends. There were considerable differences in the strategies for fields of specialization and for general anthropology, with the latter approached largely through reading journals and used primarily in teaching.

Networks, including departmental-tradition ones, were listed by many. E-mail, list-serves and the Internet are becoming increasingly significant for many, not just the most recent Ph.D.'s. There are some unusual strategies, including collecting other people's syllabi, revising one's own syllabi, writing to authors of interesting papers, reviewing manuscripts and grant proposals, advising students, "gossip," book catalogs, and listening to department speakers. General anthropology poses more extreme difficulties in keeping up. Many cite the AAA, through both papers and networking, as a way to keep abreast. Some respondents have virtually given up. "How many journals can you read in a day?" Or: "Read like crazy." One respondent says that "general anthropology is not very interesting any more."

Respondents to a question about influential national traditions rarely listed an American or Americanist tradition, presumably taking this for granted. British and French traditions appeared most frequently, along with other European alternatives (including German, Belgian, Norwegian, Romanian, Russian, Dutch, and eastern European). Other suggestions correlate highly with fieldwork and ensuing engagement with the development of national traditions in research areas; respondents listed Canadian, Australian, critical Latin American, Mexican, Indian (South Asian), Turkish, and First Nations/Native American.

Further Reflections

Some of the respondents were puzzled and/or annoyed by some of the questions I asked. Indeed, not all of the questions made sense when respondents were compared. Many switched from talking about the departmental tradition of their training to the circumstances of their present employment. Core departments have an advantage in establishing networks: "Institutions with greater longevity to their intellectual traditions have developed substantial networks that their students may avail themselves of." Nonetheless, numerous respondents are trying to create their own versions in local ethnographic and areal contexts.

Some of the open-ended questions were perceived as "profoundly vague" — which they doubtless were, because I did not want to prejudge the responses. But I acknowledge that more precise definition of terms such as *departmental tradition* might have helped some. One respondent had "trouble interpreting" questions, because I "seemed to come from a very different paradigm." In this case I think the paradigm in question had more to do with HOA than with my own departmental affiliation to Pennsylvania (Ph.D. 1969). What I brought to this project from my own graduate experience, however, was an optimism about the holism and networked collegiality of the field — indeed, that was what drew me into it. I found Penn intimidatingly impersonal at first, coming from a very small undergraduate program at Bryn Mawr, but quickly found a place there. Members of my graduate cohort were, and remain, friends, across subdisciplines and with little sense of engagement in a zero-sum game. I keep in touch with a few who have not pursued academic careers (though individually rather than as part of my larger cohort). I have always supposed that my experiences of networking and departmental tradition were widely shared, but the survey results suggest that much ambivalence surrounds both the elitism of some institutional contexts and the experiences of being at the center by those who left it to do their lifework elsewhere (as, indeed, I have also done).

Fascinating as these preliminary results have been, much remains to be learned. We need histories of the major departments as a context for respondents' memories and continuing affiliations. I have written about Pennsylvania and Yale (Darnell 1970, 1998a) and have explored the histories of most of the other departments at a more superficial level. We need to know how people who are now teaching at core or "elite" departments feel about their networks and about the dispersion of their students after the doctorate. We need to add more departments to the equation. And we need to know how the students of the dispersed elite feel about their professional ties to the departmental traditions of their teachers. In short this study is not finished. I report on it in this reflective mode to stimulate further discussion and to acknowledge the thoughtful responses of many colleagues to survey and discussion.

Notes

A preliminary version of this paper was read as a Presidential Session at the 2002 AAA meetings. I thank the colleagues who attended for their feedback. The research has profited especially from the collegiality and imagination in revisioning our discipline that arose from the Centennial. I thank Lee Baker, Don Brenneis, Jennifer Brown, Ray DeMallie, Frederic W. Gleach, Richard Handler, Jane Hill, Louise Lamphere, Jonathan Marks, Stephen O. Murray, Gary Dunham (University of Nebraska Press), and AAA staff members Lori Van Olst, Susi

Skomal, and Bill Davis. Electronic access to the *Guide to Departments of Anthropology* for 2001 made this study feasible.

1. In practice most of those who returned the questionnaire (identifiable before coding from e-mail or postal address) were people I knew personally in one or another context. I did not, of course, read the responses as those of known individuals, but I did reflect on the fact that anthropologists apparently prefer to discuss their career paths with people they know. The survey as such seemed less intriguing to respondents than the possibility of an ongoing conversation.

2. I want to thank those who responded to the questionnaire and those who discussed issues raised in it with me. I hope that each of you will find some of your sentiments captured in the excerpts I have included here. Because responses clustered around a few patterns, however, precise words are not easily attributable to particular individuals.

3. The straw person for this argument was Marvin Harris's *The Rise of Anthropological Theory* (1968), with disciplinary history modeled as a unilinear trek to "techno-environmental determinism."

4. This expectation of historicist contextualization may be a holdover from the historical vistas of Boasian historical particularism. British social anthropology, for example, does not seem to have developed disciplinary history in the same relationship to disciplinary practice.

5. "Departmental tradition" will refer hereafter to department of training rather than to present employment, both to protect the anonymity of respondents and to avoid confusion. Virtually all respondents, however, compare the department(s) of their training to those with which they later became involved.

6. The Canadian questions were:

> What do you think is unique about Canadian anthropology?
> Should Canadian anthropology focus on the study of Canada?
> How do you feel about non-Canadian anthropologists working in Canada?
> What can anthropology contribute to the study of Canadian society?
> What is the place of applied anthropology in the discipline in Canada?
> Do the First Nations have a unique place in Canadian anthropology? In Canadian society?
> Would you advise an outstanding student to pursue a doctorate (in Canada or elsewhere)? Why?
> How do you see the demographics of the professoriate changing over the next decade?

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8. *Anthropology, Theory, and Research in Iroquois Studies, 1980–1990*

Reflections from a Disability Studies Perspective

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Having played a primary role in producing and disseminating representations of North American Indians, the discipline of anthropology also became a major contributor to the critical analysis of those very representations (Strong 2004:341). Indeed, in their assessment of relations between anthropologists and Indians 20 years after Vine Deloria's famous critique in *Custer Died for Your Sins* (1969), Thomas Biolsi and Larry Zimmerman note that a fundamental change in the field has been an increasing awareness of the social process of producing knowledges about Indians in America. "Most 'informants' and 'anthros,' " they tell us, "no longer believe that what passes for scholarly knowledge is ever universal, value-neutral, or unconnected to professional, class, and other interests (although there are always 'holdouts')" (1977:7). I suggest that Iroquois studies represented a group of such holdouts. In this paper I ask: In what ways, for what reasons, and with what effects were Iroquoianists "holdouts"?

As a point of departure for my analysis, I offer the following story from my graduate-school experience. It was October 1979. The annual Conference on Iroquois Research, usually a separate and "by invitation only" event, was being held in Albany that year in conjunction with the Society for Ethnohistory; the meeting was advertised in the *Anthropology Newsletter*, and I jumped at the chance to attend. With my dissertation proposal having been recently approved by my doctoral committee, I was preparing to go to the field. Driven by a theoretical focus on the process of mobilization in social movements, I had decided to study the conflict between a group of Mohawk Indians who had taken over land in the northern part of New York State, calling it Ganienkeh and claiming it as sovereign territory, and the local white communities that had organized to resist Ganienkeh in its two locations — its original encampment within the Adirondack Park and its current site near Altona, New York.

Many years earlier I had begun my undergraduate studies at Cornell University in an era of political turmoil and on the heels of Deloria's biting, though humorous, critique of my soon to be chosen discipline: "Into each life, it is said, some rain must fall. Some people have bad horoscopes, others take tips on the stock market. McNamara created the TFX and the Edsel. Churches possess the real world. But Indians have been cursed above all other people in history. Indians have anthropologists" (Deloria 1969:78). By the time I was preparing for fieldwork, I therefore had no illusions of being particularly welcomed into life at Ganienkeh; I knew my presence there, as among the groups opposing Ganienkeh, would be a privilege that could be revoked at any time. And I believed that it was only right that it should be so. My understanding of academic life, however, was remarkably less sophisticated and much more romantic; I *had* naively expected to be accepted, if not welcomed, into the established community of scholars studying the Iroquois, one of the most documented of all Native American peoples.

I appeared at the Conference on Iroquois Research hoping for some expert advice and guidance, and I nervously and humbly introduced myself and my proposal to individuals I had come to recognize as the leading scholars in the field of Iroquois studies. However, there was to be found among them no enthusiasm for my planned research. One comment, made by a renowned anthropologist who, I believe, intended to be offering constructive advice, particularly stuck in my mind. Referring to the Mohawks who had come from Kahnawake and Akwesasne and who now occupied the territory they called Ganienkeh, the scholar straightforwardly asked me: "Why do you want to study *them*? They're a pathological group. Why don't you study *real* Indians?" The comment was followed by specific suggestions for more appropriate subject matter.

I use this Iroquoianist's comment to frame the remainder of this paper. The term *pathological group*, saturated in what disability-studies scholars refer to as the medical model, encapsulates a number of assumptions I wish to interrogate. These assumptions bear on the state of Iroquois studies in the decade 1980–90, the very time the discipline of cultural anthropology was experiencing an "explosion of paradigms" centering on ethnographic writing and practice (Geertz 2002:11) and was becoming actively engaged with what became known as the "crisis of representation" (Marcus and Fischer 1986). Thus, in addition to the questions I presented above, I also ask in what ways insights from disability studies might contribute to interpreting the history of anthropological research on the Iroquois in particular and to analyses of identity and of anthropological representations of Native peoples more broadly. What, then, is implied by labeling a group as "pathological"?

Setting Research Boundaries

"To be situated within a discourse of 'pathology,'" disability-studies scholar Bill Hughes reminds us, "is to be delegitimized" (2005:83). Insofar as a group of Indians is delegitimized, that is, treated as not being real Indians, analysis of that group's beliefs and actions falls outside the field as authoritative experts define it. Perhaps the most immediate consequence of the binary of "pathological" versus "real" Indians in Iroquois studies was, as illustrated by my experience at the 1979 conference, the fixing of boundaries around acceptable research topics.[1] The notion that there is little of anthropological significance to be learned from certain categories of people, including those located at the margins or out of the mainstream of any society, had in the past arbitrarily shackled the development of knowledge and theory about the human condition. This, of course, was a basic point made early in the development of feminist anthropology, as scholars first decried the absence in anthropological literature of analyses of women's activities, including reproduction (see Rosaldo and Lamphere 1974; Jordan 1983; Ginsburg and Rapp 1995), and it underlies and fuels (although in itself does not define) the growing field of disability studies today.

Demarcating pathological from real Iroquois groups was nevertheless consistent with the long-standing agenda of Iroquois studies to document and authenticate pattern rather than to examine process. In what Audra Simpson refers to as an "industry of fact-checking," ethnographic, linguistic, archaeological, and historical methods are employed by Iroquoianists to confirm and affirm earlier accounts of Iroquois life (2003:115). These earlier accounts include those produced by Lewis Henry Morgan, Horatio Hale, and Arthur C. Parker. In marked contrast, in the research proposal I had tried to introduce to Iroquoianists at the conference, I did not ask how the Mohawks at Ganienkeh — or later, the traditionalist Iroquois Indian writers of a much maligned curriculum resource guide to supplement the state's social-studies curriculum — match a list of traits that experts have predetermined as characteristically Iroquois. Indeed, as I had framed my research, it was not a concern that the group be representative of "authentic" Iroquois culture, anymore than it now concerns me that American mothers of young disabled children are not representative of all American mothers. Using the dispute as my unit of analysis and what would later be called "multisited" ethnography, I sought then to understand processes of mobilization in social movements and to learn how identity is expressed, negotiated, and reconstructed over time and in different settings. The term *pathological* removed this and many other questions as worthy of investigation and presupposed the answers to the very questions the research asked about the ongoing constitution of identity.

There was no shortage of issues that interested the general public concerning the Iroquois of this time, and the popular media provided ample coverage of Iroquois issues during the decade 1980–90. Local and national newspapers covered the armed conflict between Mohawk traditionalists and supporters of the elected government at the St. Regis/Akwesasne reservation, appeals by Iroquois to the European Parliament for support, the opening of New York State's first Native American historic site at Ganondagon, the return of wampum belts held in the New York State Museum to the Onondaga Nation, various Iroquois land claims, Mohawk criticism of pollution at Akwesasne, a dispute over cigarette taxes and Indian rights to free trade across the U.S.–Canadian border, the shooting of a helicopter over Ganienkeh, the tense standoff at Oka, the debate concerning whether the Iroquois had influence over the framers of the U.S. Constitution, and most especially, the bloody conflict, or "civil war," at Akwesasne over gambling on the reservation.

While for long stretches of time these issues affected Iroquois people in their daily lives and often engaged them in the most profound, sometimes life-and-death decisions, there was little scholarly analysis of the issues. William Fenton's major contribution was his volume, *The False Faces of the Iroquois* (1987). The largest number of articles appearing on the Iroquois in this decade were published in the journal *Ethnohistory* and included Thomas Abler's article on cannibalism (1980); Robert Beider's on the influences of a fraternal order on Lewis Henry Morgan (1980); Richard Haan's on Iroquois policies regarding neutrality in the 18th century (1980); William Starna et al.'s on the role of insect infestation on Northern Iroquoian village removal (1984); Starna's on revising Iroquois population data (1980); Elisabeth Tooker's on Morgan's research on the structure of the Iroquois League (1983); Tooker's on the significance of Isaac Hurd's ethnographic studies (1980); Nancy Hagedorn's on Indian interpreters as cultural brokers in 1740–70 (1988); Fenton's on Iroquois suicide (1986); Francis Jennings's review on works of Anthony Wallace (1990); and Tooker's critique of the notion that the Iroquois had influenced the framers of the U.S. Constitution (1988). Other than to counter a claim being made by contemporary Iroquois people over the writing of history, anthropological articles generally focused on issues from the past rather than on contemporary concerns in Iroquoia.[2] Even Tooker's response to the contemporary debate over Iroquois influence on the U.S. Constitution followed the Iroquoianists' agenda of "upstreaming" and centered on authenticating the historical facts marshaled by those who argued against such influence.

Just as disability in the medical model (discussed below) itself defines the normal and sets *it* as the reference of value, labeling a group "pathological" removes the need to understand the perspective of that group. For instance in his analysis of American Indian policy in New York State, historian Laurence Hauptman (1988), one of the few scholars to address contemporary policy, refers to "angry Indian dissidents" (41) and "fringe elements" (102). He sees as fallout of the state's long delay in sitting down and negotiating with "the recognized leadership of Indian nations" (xiii) that many Indians "cynically 'manipulate the system,' either for individual gain, for the media value of embarrassing state officials, and/or for political advantages in tribal in-fighting." In meticulous detail Hauptman recounts events and players but nevertheless turns the activism of these groups into a raw political agenda devoid of content. *Why* are these Indian dissidents angry? Why, for that matter, are they dissidents? What are these groups seeking political advantages *for*? We need not explore these questions, Hauptman implies, as the answers reside in the groups' internal pathology, of which their activism is merely a symptom exacerbated by the state's actions or inaction.

Supporting this marginalization of dissident groups from scholarly analysis as "real" Indians is a static view of culture in general and the myth of the vanishing Indian in particular. Academics still holding to one or another version of the vanishing Indian concept perpetuate the view that contemporary Indians do not know about, fully understand, or practice their own traditions; they are not the authentic Indians. Mohawk scholar Taiaiake Alfred, in his unpublished manuscript "From Sovereignty to Freedom," suggests that this view serves the state's interests.

> The maintenance of state dominance over indigenous peoples rests on the preservation of the myth of conquest, and the "noble but doomed" defeated nation status ascribed to indigenous peoples in the state sovereignty discourse. . . . Framing indigenous people in the past allows the state to maintain its own legitimacy by disallowing the fact of indigenous peoples' nationhood to intrude upon its own mythology. . . . One of the fundamental injustices of the colonial state is that it relegates indigenous peoples' rights to the past, and constrains the development of indigenous societies by only allowing that activity which supports its own necessary illusion — that indigenous peoples do not today present a serious challenge to the legitimacy of the state [2000:13]

Tooker exemplified the way in which Iroquoianists of the 1980s tended to relegate the Iroquois — and their ability to exert agency — to the past. In accepting an award for her Iroquois research, the anthropologist noted with admitted exaggeration that without the Iroquois Indians we might all be speaking French; she was then quoted as saying that "the Iroquois, whose five nations once populated much of Central New York, are indeed a people worth remembering" (Cazentre 1986:C1). Here legitimate Indians with the power to affect the course of affairs are safely located in and fondly remembered as part of a noble past.

By the same token not only current dissident fringe groups, but entire Iroquois communities whose histories do not meet the experts' criteria of real Indians of the past, may also be elided from anthropological analysis. Kahnawake, for instance, rarely made it onto the radar screens of Iroquoianist scholars of the decade. Having been described by J. N. B. Hewitt in 1929 as possessing "no trustworthy knowledge of the structure and institutions of the ancient League" (Simpson 2003:105), Kahnawake was "conceived as a village of 'praying' Indians, as travelers, showmen (and women) and now ironworkers," never grabbing hold of the anthropological imagination as did communities such as Six Nations or Tonawanda (Simpson 2003:102). Its history failing to confirm traditional Iroquois patterns according to established canon, Simpson writes of her home community, "early ethnology and later ethnography have elaborated the territorial and cultural bias in order to further construct Kahnawake as an 'out of the way place,' and most specifically, a place away from Iroquois culture (2003:103).

Pathology and the Medical Model

Perhaps most striking to a scholar versed in disability studies is the way in which applying the term *pathological* medicalizes the situation under discussion. Characteristic of contemporary mainstream American cultural views as well as modern policy making and professional practice (Longmore and Umansky 2001:7), the medical model defines a disability as pathology within an affected individual. As a physiological deficiency or abnormality, disability is a "departure from both what is normal (usual or expected) and the norm (the implicitly valued usual)—which impairs a person's ability to function in society" (Vedder 2005:111). This model served as the framework for the World Health Organization's 1980 International Classification of Impairments, Disabilities and Handicaps, in which a disability represents a reduction of a person's abilities to perform basic tasks as a direct consequence of an internal deficit or abnormality: a missing limb, a genetic defect, impaired senses, and so forth.

In its rehabilitation variant, the medical model labels some children as "developmentally delayed" (see Landsman 2003); the goal of therapy is for the patient to approximate the norm in appearance and behavior or, better yet, to "overcome" disability. The common ground in all versions of the medical model is the assumption that disability "is personal, unfortunate and should be fixed or prevented at the individual level" (Vedder 2005:111).

To define a trait as pathological, then, is to refer to an internal defect. In terms of both scholarship and public policy, the implication of the medical model is that we do not have to explore the way in which that which is labeled "pathological" is constituted by the social relations of power. The latter is precisely the basic concern of what is known in the United Kingdom as the social model and in the United States as a minority model of disability. Central to *these* models is the distinction between impairment, which is a matter of anatomy, and disability, which is a matter of oppression of those who manifest bodily, cognitive, or emotional difference. Explaining the new paradigm of disability studies, David Pfeiffer states: "A disability comes not from the existence of an impairment, but from the reality of building codes, educational practices, stereotypes, prejudicial public officials . . . ignorance and oppression" (1999:106). Applying the rhetoric of other disenfranchised groups in society, proponents of a minority group model of disability have made the claim that "the physical, cognitive, sensory and emotional make-up of the individual was not the problem but was a problem only because social institutions and human-made environments were created without taking into account the characteristics of all people" (Asch 2004:13).

In the political arena of social models, disability may therefore be presented as a consequence of physically inaccessible architecture and transportation systems; slogans on T-shirts and posters supporting disability-rights groups often refer to issues of "barrier removal" such as replacing stairs with ramps. However, stereotypes and attitudes can also be represented as being disabling; slogans appearing on merchandise sold at the activist Nth Degree website include "Your Attitude Just Might be My Biggest Barrier" and "It's the Labels That Are Confining!" in which a comparison is drawn between the terms "wheelchair bound" (with a picture of a person literally tied to a chair) and "wheelchair user," showing a picture of a person moving freely down a ramp in a wheelchair. The Americans with Disabilities Act, with its rights-based language intended to protect people with disabilities from discrimination and to promote access to employment and public accommodations, is the most significant piece of legislation to emerge from the social or minority group model.

Just as "gender is not simply a matter of genitals nor race a matter of skin pigmentation," a social model presents disability not as a physical defect inherent in bodies "but rather as a way of interpreting human differences" (Garland-Thomson and Holmes 2005:73). The new paradigm in the scholarship of disability studies, then, is not about providing the authoritative truth about bodies, but about exploring how bodies have been and are represented in different times and places. Research carried out within the framework of the social model focuses on "issues such as equal access for all, integration of institutions, and the historical exclusion of people with disabilities from the public sphere" (Garland-Thomson and Holmes 2005:73).

If, in contrast, disability is internal pathology, the appropriate way for an impaired person to live is to accept one's fate of being tragically flawed or, often in ways deemed by the public as inspirational, to seek to be normal. Until cured or obtaining credibility through valiant efforts to normalize or "overcome" one's disability (what is often described as taking on the role of "supercrip"), the disabled, like the "vanishing" Indian, can be safely pitied from afar for being noble, perhaps, but nevertheless doomed.

The in-your-face response of disability-rights activists to the discourse of pity is the slogan "Piss On Pity." Pity and sympathy toward people with disabilities, featured prominently in telethons, are rooted in a medical model and are held from a position of power; in contrast, the demand emerging from a social model of disability is not for pity but rather for *rights*. Explaining how she turned from telethon poster child to disability protester, Laura Hershey explains that "the cure is simple, magical, nonpolitical solution. . . . That's why it's so appealing and so disempowering. The other solutions we have to work for, even fight for. . . . The idea of a cure is at least in part an effort to homogenize, to make everyone the same" (1993). A disability-rights poster makes reference to the "March of Dimes," a nonprofit organization devoted to prevention of birth defects: Depicting the universal handicapped sign of a person sitting in a wheelchair but now holding a placard as in a protest, the poster reads, "YOU GAVE US YOUR DIMES. NOW WE WANT OUR RIGHTS." The demand is not for help to change one's body, but for a change in society, and the demand is made from a position as active subject rather than passive object of pity.

Controlling Definitions

The application of the term *pathological* to a group postulates a binary: a normal versus an abnormal. In the literature of Iroquois studies, we have seen, there are real (legitimate, authentic) Iroquois and there are pathological (fringe, inauthentic) groups. Only by setting a standard reference point of "real" or "normal" or "legitimate" can the pathological be identified as such. In the medical model the statistical norm has provided the reference point. This idea of the norm in relation to the body emerged in Europe in the 19th century, replacing the earlier concept of the ideal body, epitomized in sculpture; the norm, Lennard Davis explains, "is less a condition of human nature than it is a feature of a certain kind of society" (Davis 1995:24). In this model the power to define and treat disabled people resides within the medical profession; it is the responsibility of disabled individuals or their caregivers to seek their expertise. For young children in the United States, for example, eligibility for early intervention services is determined either through medical diagnosis of a specific condition or through expert documentation of a specified percentage of delay in reaching developmental milestones in different domains as measured against a norm.

But the content of definitions as well as their applications have not gone uncontested. My study of mothers of disabled children, based on observation of 130 physician evaluations and 9 interviews with 60 mothers, shows that through various strategies, mothers of disabled children challenge the meanings of labels attributed to their own children by physicians (Landsman 1999; 2003; 2005). Against doctors' well-intentioned efforts to help mothers "face reality" as they define and predict it, mothers often lay claim to their *own* expertise, born of intimate interaction and daily lived experience; these women also accept, more easily than do doctors, that future reality for young children is in fact unknowable, uncertain, and open to change.

More broadly, through political action disability activists have sought, often successfully, to wrest power over the lives of disabled people from the medical and helping professions. It is this latter point upon which Bill Hughes focuses his argument regarding the limits of a Foucauldian analysis of disability. He points out that while the "history of impairment throughout modernity has been a history of pathologization and supervision," it is also the case that "escape from supervision and struggle for citizenship by a self-conscious collective movement of disabled people have in addition characterized the contemporary history of impairment" (Hughes 2005:80). Refusing society's stereotypes as recipients of charity and pity, disabled activists have claimed the status of subjects with agency (Hughes 2005:80); they have acted to define themselves against the definitions of credentialed authorities.

There are dangers in taking an analogy too far. Nevertheless, I would like to suggest that while understanding themselves to be benevolent experts documenting and preserving the integrity of an authentic Iroquois culture, Iroquoianist scholars played a role in relation to Iroquois people not unlike that played by the medical profession in relation to people with impairments. And like disability-rights activists, Iroquois people themselves challenged, sometimes successfully, the representation of their identity by authoritative experts.

Writing in 1997, Vine Deloria identifies the conflict between Indians and anthropologists in the previous two decades as "at its core, a dead struggle over the control of definitions. Who is to define what an Indian *really* is?" (215).

The power relations inherent in this enterprise appeared dramatically in the conflict over the writing of the curriculum research guide for New York State social-studies teachers. Central to the criticisms leveled by non-Indian Iroquoianist scholars was the issue of the role of Iroquois in the framing of the U.S. Constitution. According to Deloria, the "generation of anthros now retiring and passing away has not been at all willing to surrender its entrenched position on this matter"; they made their arguments against Iroquois influence "under the assumption that non-Indian scholars know more about the Six Nations than do the Six Nations People" (1997:215).

Before proceeding to the conflict over influence on the U.S. Constitution itself, let us take a brief look at the generation of anthropologists to whom Deloria refers. The acknowledged "dean of Iroquois Studies," William Fenton, remained remarkably consistent in his agenda of "the authentification of facts about Iroquois culture and history" (Voget 1984:348) over a long and extraordinarily productive career. His focus on culture as patterns, traits, and artifacts that can be traced to the past and confirmed in documentary sources led him to the expert opinion that there has been a "gradual breakdown of Iroquois culture" over time (Fenton 1940:159), that the Iroquois "have grown poor in knowledge of their former ways" (Fenton 1965:259), and that Iroquois efforts at cultural revival are spurious and ineffective (Fenton 1975). Fenton claims, for example, that demands by warriors to oust elected officers at Grand River and restore government by hereditary chiefs bear upon the mythical and traditional past; the warriors "have given little thought to what happens if they gain their demands, how they can adapt the traditional system of confederate government to present day needs, or to what alternate forms of government are available to them" (Fenton 1975:133). Yet we are given no evidence to suggest that Fenton has sought or obtained information directly from the warriors themselves about what they have or have not given thought to; their voices are not represented.

What we are told instead is that warriors take for granted that the hereditary chiefs control the literature on the Iroquois Confederacy, when they actually "are familiar with only part of its rich symbolism" (Fenton 1975:133), and that there has been a degradation of Iroquois mythology generally from the time it was first collected until the 1920s, such that today (1975), "Iroquois folklore has reached the vanishing point" (1975: 139). In this way foreshadowing Deloria's characterization of Iroquoianists in the influence debate, in his 1975 article "The Lore of the Longhouse: Myth, Ritual, and Red Power," Fenton had in essence implied that he and other Iroquoianists knew "more about the Six Nations than do the Six Nations people" (Deloria 1997:215).

Anthropologists and historians in the field of Iroquois studies have proceeded under the assumption that boundaries exist between authentic and inauthentic Indians, and it is they who have actively patrolled those boundaries. The challenge to the authority of Fenton and other ethnohistorians in contexts including disputes over Iroquois influence on the U.S. Constitution and the writing of the Iroquois curriculum resource guide came to be, in the assessment of Pauline Turner Strong, "perhaps the most important debate for the discipline of anthropology" (2004:350).

At a conference in 1988 a prominent anthropologist and ethnohistorian asked a group of non-Native scholars to reflect upon why the Iroquois/Constitution issue "bothers us" so much. One of the responses was a suggestion that those promoting the argument that the Iroquois influenced the framers of the U.S. Constitution were not in fact "real" traditionalists. Another made the statement: "They're on *my* turf now." This respondent elaborated, saying that Indians have often suggested that he (the scholar) cannot appropriately speak about Indian life because he is not an Indian, yet now Indians presume to speak about American history. Another Iroquoianist at the conference recommended that the American Society for Ethnohistory investigate the matter of Iroquois activists' gaining public support for the concept of Iroquois influence on the Constitution (such as a ceremony on the Mall in Washington DC involving some Iroquois representatives and sanctioned by the U.S. Constitution Bicentennial Commission) and take a formal position. He continued that we should each go back to our specific discipline's professional organizations and request that they also issue formal positions on the debate.

Such arguments are not unlike the advice provided by Hauptman that the "governor's office would also be wise to negotiate *only* with the formally recognized leadership of the Indian nations bringing suit" (1988: 112). Hauptman claims as well that "New York State's Indian policies should not evolve because of fears of armed confrontation with angry Indian dissidents but should be carefully planned in conjunction with responsible representatives from the state's Indian communities" (1988:

41). The latter, while certainly appearing a reasonable suggestion, nevertheless begged the question of who determines who the "responsible representatives" are.

The issue here was not whether scholars should take a position on political issues. Critiques by feminist anthropologists and indigenous activists, academicians responding to controversy over the morality and wisdom of the Vietnam War, and the reflexive turn in anthropology of the time had not only allowed for, but often encouraged political engagement by scholars. However, they did so with the recognition that each of us, in the very nature of things, can possess but partial, "situated knowledges" (Haraway 1998) rooted in our class, race, gender, generation, ethnicity, and culture rather than having access to an all-knowing view from above, and that scholars, like ethnic activists, themselves serve and are constrained by political interests and by the scholarly discourses of their time (Landsman 1992:248). The unrealistic and, from the point of view of the development of theory, stifling aspect of anthropologists' and historians' laying claim to exclusive expertise is rather that it leaves no room for Indian people to do the hard work of considering for themselves and contesting among themselves the questions of how best to be Indian and what it means to be Indian.

Here again, I would like to point to the similarities to the debates in disability studies. The medical and social models discussed above are both currently being contested. Some scholars argue that neither model can capture what it means to be disabled and that the experience of disability is neither the exclusive consequence of an internal defect nor a purely cultural construction, but rather an interaction with an environment that takes no account of the knowledge rooted in different types of bodies. Some disability theorists now focus on ways in which experience is embodied (Hughes 2005; Hughes and Paterson 1997; Landsman 2005) and address how different impairments have radically different implications, or how the same impairment may affect individuals differently depending on a range of other factors in the environment or, indeed, may affect the same individual differently at different times of life (Asch 2004:13). Others claim disability as a culture (similar to the pan-Indian experience) in which differences among types of impairments pale by comparison with the experience of being outcast. Critiques of the very notion of "normal" are now appearing, questioning any distinction between impairment and disability or between impairment and normalcy (Davis 2002). There is general acknowledgment that by locating disability as rare and deviant, the medical model repressed the power of anomalous bodies to unhinge the notion of a specific body type as normative and "cordoned off disability" from the range of differences that characterize the human condition (Snyder and Mitchell 2001:377); on just what the experience of disability means however, there is currently no consensus. Instead, the debate over what constitutes disability has revealed itself to be a source of creative action and ongoing dissention, animating the scholarship of disability studies. And disabled people are claiming the right to participate in that debate: "Nothing about us without us" is the demand.

The founding of Ganienkeh, the writing of the curriculum resource guide in New York State, the conflict over gambling at Akwesasne, and the changing band membership policies at Kahnawake all emerged during the 1970s and 1980s from debate within Indian communities concerning what it means to be an Iroquois and how Iroquois people might best represent who they are to others and to themselves. There was not and is not now consensus. Is this because some Indians are real and others are pathological? If there is only one way to be a *real* Indian and that way is defined and labeled by historians and anthropologists, the entire enterprise in which Indians have been engaged is debased. Just as the medical model of disability locates power within experts, a notion of pathological groups versus real Indians attributes to experts the role of authenticating identity and dismisses from both scholarly analysis and public policy the active engagement of participants themselves in issues central to their lives.

In our research on the debate over the curriculum resource guide, graduate student Sara Ciborski and I found that to speak of more than one version of history was itself considered by some Iroquoianists to be a betrayal of academic standards (Landsman and Ciborski 1992). This approach leaves us only to authenticate pattern rather than to understand process. The generation of anthropologists alluded to by Deloria thus employed the notion of authenticity to authorize some arguments for how to be an Indian over others, and for some histories over others. In doing so, they lost access to one of the most profound questions of anthropological interest: How do people come to understand who they are?

Iroquoianists and Theory

What accounts for the failure of Iroquois studies in this decade to address theory? While the rest of the social sciences were experiencing a range of new approaches gathered under the banner of postmodernism (Geertz 2002)—a critique of the objectification of Natives, a rethinking of the notion of value-free science, influence from social movements such as feminism and indigenous rights, an awareness of disciplines as discourses, and condemnation of ethnographic presentations of the static "other"—how did Iroquois studies remain outside the bustling, turbulent experimentation and critiques of the field? Answers certainly lie in part in Fenton's method of "historical 'upstreaming,'" in which he used his "own field data to afford a perspective for evaluating earlier field reports and particularly historical records (Fenton 1951). Focusing his attention on "the authentification of facts about Iroquois culture and history," Fenton's method was "in effect a kind of ethnographic historiography, which explains his unswerving determination to establish the true nature of recorded facts and events, and to keep theorizing about processes in check until full historical-ethnographic documentation was at hand (Voget 1984:348–49; see also Landsman 1988:174–79).[3]

Answers also lie, however, in the role of Fenton as the acknowledged "dean of Iroquois studies," in the micropolitics of publishing, in the old-boy network of the annual Conference on Iroquois Research — in short in a control, intentional or not, of which voices came to be heard. Describing the literature on Iroquois ritual, Sturtevant points out that "for the last 45 years all anthropologists who have studied this topic have closely followed William N. Fenton's lead, reading his publications carefully, sending him manuscripts for comment, and consulting with him at the annual Iroquois conferences. We have been in touch with each other, reading manuscripts and publications, listening to oral papers at the Iroquois conferences, and often exchanging letters and field notes. . . . There is a tradition of analysis shared by ethnographers and the ritual specialists they work with" (1984: 133–34). In many ways this functioned as a closed loop. As Wallace points out, Iroquoianist scholars used each other as a primary reference group (1984:9). Iroquoianists talked to each other, and they did so under the influence and guidance of the dean of Iroquois studies. In fact the editors of the interdisciplinary volume *Extending the Rafters* suggest that a recounting of Fenton's career "amounts to a recounting of the development of Iroquoian studies as a whole" (Foster et al. 1984:xv). "Fenton not only defined the ethnographic and historiographic objectives and methodology of Iroquois Studies," wrote Fred Voget in the same volume, "but also, through his own researches, persuaded and encouraged others to follow his example" (1984:357). The common perspectives and methodological principles employed by those who regularly participated in the annual Conference on Iroquois Research founded and dominated by Fenton constituted an "Iroquoianist school" (Richter 1985:365).

William Fenton was by all counts an extraordinarily prodigious scholar. He was generous and encouraging to those he mentored. Yet for many Iroquois traditionalists and activists, Fenton came to symbolize the opposition of academia to Natives' control over the representation of their own culture, history, and identity (Landsman and Ciborski 1992:428). Many activists with whom I consulted in my research made a point of establishing their ownership of knowledge about Iroquois culture as against that of Fenton. It was not uncommon for me to hear that years ago their elders had lied to Fenton and made up interesting stories that Fenton and other white scholars "picking the brains of Indians in order to make their careers" mistakenly took as truth. Whether this happened or not we may never know, but the belief and statement that it did speaks to the struggle for power over Iroquois culture of the past by Iroquois people of the present.

Activists I consulted also expressed concern with what they saw as Fenton's paternalistic desire to preserve Iroquois artifacts while keeping Iroquois people in the past. This concern revealed itself in debate over the proper location of Iroquois wampum. While Fenton felt some historic wampum belts were too fragile to be entrusted to contemporary Iroquois, they countered that the belts — as Jake Thomas explained at a 1987 conference on the Great Law of Peace — were always being fixed, cleaned, and restrung by women as part of a living, ongoing culture.

Reviewing the festschrift to Fenton, *Extending the Rafters*, Starna makes the expansive claim that "this series of papers covers effectively the scope of things Iroquoian" (1990:51). He thereby expresses the view that the range of issues to be addressed on the Iroquois is encompassed in the literature in the book. Divided into sections entitled "Changing Perspectives in the Writing of Iroquoian History," "Aspects of Iroquoian World View," "Iroquoian Origins: Problems in Reconstruction," and a brief conclusion entitled "The Fenton Tradition and Fenton as Applied Anthropologist," the volume's clear focus is on prehistory and history. Controversies raging within Iroquoian communities of the present time simply do not appear. Starna concludes: "The contributors represent the establishment or elite of Iroquoian scholarship but also include younger, active scholars in Iroquoian studies" (51).

The Elite of Iroquoian Scholarship

The latter statement bears further examination. What is the impact on research of having a group of scholars that can be labeled "elite"? I hope to provide some insight into this question through my own experience of conducting research on the controversy over the Iroquois curriculum resource guide, which is described elsewhere in greater detail (Landsman 1997).

Funded by the New York State Education Department and written largely by traditionalist Iroquois writers, the draft of the guide had as its stated aim the provision of an indigenous people's perspective as a supplement to the state's social-science syllabus for seventh and eighth grades. Among the people asked to review the draft in 1988 were five non-Indian Iroquoianist scholars who were extremely negative in their reviews; some of these scholars went so far as to demand that the project director be fired. Iroquoianists' comments on the draft of the guide included that the guide was "a disaster," was "worthless" and contained "grievous, and even irresponsible distortions of fact." Copies of the negative reviews were seen by some of the draft's writers, and in turn some of them called for an end to cooperation with Iroquoianist scholars. Graduate student Sara Ciborksi and I sought to analyze the conflict in which, it became clear, scholar and Native, each responding to previous representations, "never truly stood separate and apart from each other" (Landsman 1997:166). We interviewed writers of the guide as well as academic reviewers and New York State Education Department staff involved in the project. Our interest was not in determining the truth of the resource guide's content, but in understanding the process whereby history is constructed, represented, and contested (Landsman and Ciborski 1992; Landsman 1997).

We submitted an article based on our research for publication in a major anthropological journal. With the submission I made the suggestion that it would be inappropriate to have it reviewed by those scholars who had actively participated in the controversy under study, some of whom we had interviewed as informants as part of the research. The article was sent out for peer review. One reviewer, identifying him/herself as having known nothing about the controversy, found the article "fascinating" and claimed it raised "issues that will inevitably affect all anthropologists . . . in the coming years, as more and more 'Natives' begin to represent themselves." In sharp contrast another reviewer claimed not to see any scholarly purpose served by the article. "Its aim—to see 'how and why . . . scholars, Indians, and State Education officials are in conflict—seems journalistic, without redeeming long-range interest or value for anthropology." Our scholarship was described as "suspect," our "reportage . . . patently unbalanced," and our prose "strewn with fashionable jargon and gobbledygook." Another reviewer claimed to have been "party to the Iroquois-influence-on-the Constitution fakery" and described having to "put up with that nonsense for more than twenty years." This reviewer said that our manuscript "oozes unctuous, pseudo-scientific rhetoric"; noting that he or she was "personally acquainted with nearly all the 'scholars' promoting 'the Grand Council's cause,' " the reviewer described the latter's scholarship as "contemptible," demanded "that special pleaders should not be allowed to foist their propagandas onto history under specious alibis," and criticized Ciborski and me for failing to mention in our article that Fenton had been excluded from Onondaga and "foully libeled" because of his testimony that wampum belts should remain under care in the State Museum rather than being returned to the Iroquois community. In an enclosed letter to me acknowledging and expressing appreciation of my suggestions for choosing reviewers, the journal editor reminded me that the journal's editorial council aids in the selection of reviewers and cannot be bound by authors' suggestions.

I present excerpts from these reviews not because they lead the reader to the obvious—that the article was not accepted for publication in the particular journal (although it did later find a good home in *Cultural Anthropology*)—but because the excerpts speak more accurately than any paraphrasing could to the tenor of the times in Iroquois studies, to the inevitable political casting of scholarship that addresses contemporary Iroquois issues. In particular Iroquoianist scholars accused us of being allied with the political dissidents, as if to study them without condemning their actions was to be one of them. Ironically, this was at the same time that the message Native peoples were themselves sending scholars was essentially: "Don't you dare imagine that you can know us as we know ourselves to be. You are and ever will be outsiders." My experience at the Newberry Library's Native Voices in the Academy project, for instance, had exposed me to Indians whose avowed goal was to eventually remove all non-Indian scholars from the faculty of tribal colleges. With this broad critique of anthropology in mind, our identification with Native activists rang false, and we were admittedly frustrated that we seemed unable to move Iroquoianist scholars beyond a binary us-or-them conceptualization of the controversy.

Yet from the perspective of a medical-model approach, the very fact that we had included analysis of the perspective of indigenous activists in our study *did* serve to legitimize them. By denying they were unworthy of inclusion in the study of "real" Indians, we challenged their positioning as intrinsically inauthentic or pathological. And as with the demedicalization of disability by activists employing a social model, with that challenge came a challenge to the power of the established elite.

Viewing the controversy through the lens of disability studies also helps to make sense of the series of exchanges I had with one of the resource guide draft's writers. While my coauthor and I had actively sought and considered feedback from participants on all sides in the controversy, we agreed not to allow censorship of our work. Concerned that a certain traditionalist Iroquois writer of the guide might have misguided expectations about our research, I wrote a letter in which I specifically cautioned the individual *not* to consider me an ally. I stated that while the temptation to be considered an ally might be great, I could not guarantee that he or anyone else would be pleased with everything we might write; what I could promise was that as we would with other participants, we would send a copy of the paper for his comments before sending it off for publication and that "what we write will be faithful to our best understandings and interpretation." This prompted an interesting and lengthy correspondence regarding what being an ally actually meant. After seeing a draft of our article, the traditionalist Iroquois writer responded by observing that in the article I had called him neither a liar nor a racist; he therefore did, after all, sense an ally in me. "An ally," he wrote, "is someone who will listen, and . . . will believe that I'm telling the truth as I see it" (Landsman 1997:169). We were allies, then, not in any agreement about the content of American or Iroquois history, but in our challenge to the Iroquois studies equivalent to the medical model. Defining disability as pathology, academia had "traditionally housed disability in a sequestered area—how to fix people and take care of them," writes Simi Linton. "Disability studies is us looking out at the world and seeing how that looks to us" (Tuhus-Dubrow 2005). In my analysis of the process of Indians writing the curriculum resource guide for the state, I had lent credence to the notion that Iroquois people actively defied their relegation to a sequestered area, looked out at the world, and told what it looked like to them.

Epilogue

With the recent death of William Fenton, a giant in scholarship on the history and cultures of some Iroquois people has been lost. His publications remain as a legacy and are a vast set of resources for future generations of Iroquois. To his students and fellow Iroquoianists, Fenton was a generous mentor and an unparalleled repository of knowledge.[4] To others who admired his tremendous store of data, he and those of the "elite" of Iroquoian Studies were nevertheless authoritarian holdouts, impeding scholarly exploration of questions, social issues, and perspectives of people they deemed inauthentic and pathological and retaining for themselves exclusive rights not only to represent, but to determine that which could legitimately fall within "the scope of things Iroquoian."

I once worried that if "those with established reputations in Iroquois studies fail to nurture, or at least tolerate, those with innovative approaches and alternative perspectives, the field will suffer." Under those conditions, I speculated, Iroquoianists "will continue to read and congratulate each other, and those whose work is affected by theoretical advances in various academic disciplines will take their business elsewhere. We will all pay a heavy price for failing to learn from each other" (Landsman 1991:309).

It has now been many years since I have actively engaged in research in Iroquois studies. Drawn by compelling anthropological questions about personhood, identity, meaning, and the body swirling about the field of disability studies, I moved in a different direction. As I now listen to mothers of disabled children and to disabled people themselves, I see emerging new models that do not appropriate the discourse of victimization as an oppressed minority or of the medical model, with its implications of intrinsic defect; nor do they adopt the goal of independence that is a value rooted in Western, temporarily able-bodied society. Whether in mothers' definitions of their children as givers (Landsman 1999) or their challenges to the concept of "normal" (Landsman 2005); in disability theorists' call to examine the interconnection of impairment and disability; or in disability-rights activists' suggestion to rethink the disabled body/brain/machine interface (Hockenberry 2001) as well as the very nature of human relationships, what is emerging is a perspective that confounds the notion of a single set of specifications for being a "real" human and that embraces the absolute reality of interdependence. This developing perspective and the debates and struggles that preceded it may serve anthropologists well as they seek to interpret the meaning and ongoing process of being Indian or, indeed, of being any other identity we choose to examine. Other rich sources abound as well.

I have reflected here on one particular decade in the history of cultural anthropology and in Iroquois studies. Years from now, another scholar may take the opportunity in this journal to look back and assess the current decade's contributions. Given the exciting new scholarship of both Native and non-Native scholars that I have encountered as I recently and ever so tentatively stepped once again into the field of Iroquois studies, I suspect that such a scholar will find my earlier worries about its future, while not unfounded, nevertheless unrealized.

Notes

An earlier version of this paper was presented as a guest lecture at the American Indian Studies Program at Cornell University, March 31, 2005. I am grateful to all those who attended and whose interesting insights, including those of Audra Simpson, Jon Parmenter, and Kurt Jordan, helped me to improve the paper. I owe a particular debt of gratitude to Audra Simpson for her invitation and persistence in encouraging me to reflect on this period in Iroquois studies. My research on mothers of disabled children, to which I refer in this paper, was funded by a generous grant from the National Endowment for the Humanities.

1. This is not to suggest that boundaries should never be set. Certainly our research is constrained by personal and professional ethics.

2. Exceptions include some of my own works. Historian Laurence Hauptman (1988) published a book-length study of New York State's Indian policy covering the period 1970–86. Anthropologist Sara Ciborski carried out research at Akwesasne at this time, and her dissertation, "Iroquois Traditionalism as Ideology: The League and the Iroquois History of the Western World," included analysis of the bitter gambling dispute there; with the exception of the one article she and I coauthored, however, her work in Iroquois studies, to my knowledge, was never published.

3. Jon Parmenter (personal communication 2005) suggested, as another interesting factor that might have had an impact on Fenton's research focus, Fenton's particular fascination with the issue of death, and especially suicide, among the Iroquois.

4. I should point out that while I was not among those who considered Fenton a mentor, our interactions were always cordial and respectful.

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9. A Swedish Ethnographer in Sulawesi

Walter Kaudern

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Walter Kaudern was born near Stockholm on March 24, 1881, and died of heart failure the 16th of July 1942 at the age of 61. He was academically educated at the University of Stockholm, where he took his Ph.D. in zoology in 1910. Thus from the beginning he was active in the field of natural science, having a well-documented training and knowledge not only in zoology, but in geology, botany, and geography as well. In 1928 Kaudern was made curator of the geological and mineralology department of the Gothenburg Museum. When the director, Erland Nordenskiöld, died in 1932, Kaudern became the new director. On April 1, 1934, this appointment was made official, and he held the position until his death in 1943.

Kaudern's career in ethnography had developed contemporaneously with his expeditions in natural science. His interest had been awakened early, that is, as early as his first expedition to Madagascar in 1906–7, and increased during the second expedition to the same island in 1911–12. In addition to writing numerous zoological papers, he described these expeditions, chiefly the second, in the Swedish work *På Madagaskar* (Stockholm 1913). The ethnographical collections he made in the first expedition counted about 50 items, including several musical instruments. A much larger collection was made during his second expedition — several hundred objects, including weapons, ceramics, cloth, and musical instruments (EMS 1907.58, 1913.6). All objects are housed at the Ethnographical Museum in Stockholm.

Less than four years after his return from Madagascar, he and his wife, Teres, and their two children started out for yet another long expedition in December 1916. Their destination was Celebes/Sulawesi, and the expedition lasted four years. Their aim was to make zoological-geographical and ethnographical studies of the interior and for the most part unknown sections of central Sulawesi. Due to circumstances following his return home in 1921, Kaudern devoted himself increasingly to ethnography. His series *Ethnographical Studies in Celebes,* published in five volumes from 1925 to 1938, is known by all who are concerned with Indonesia. Volume 6 of the series, on Celebes art, was published posthumously. For Swedish readers he has compiled the experiences and results from this more extensive expedition in a book rich in information, *I Celebes Obygder*, which was published in two volumes in Stockholm in 1921.

Of his large collection of ethnographical items from Sulawesi, over 3,000 in number, a fourth were taken over by the Gothenburg Museum in 1926. It was also during the Sulawesi expedition that Kaudern executed the series of large oil paintings of the natives, of which several color reproductions have been made.

Walter Kaudern as an Ethnographer — Aims of the Present Study

Being an Americanist specializing in Native American religions and cultures, I am not at all trained in Southeast Asian Studies; thus I am not able to evaluate Kaudern's work in the light of more recent studies of Sulawesi. On the other hand I have done considerable research in the history of anthropology and have written extensively on such Scandinavian scholars as Erland Nordenskiöld, Kaj Birket-Smith, Knud Rasmusson, Gunnar Landtman, Hjalmar Stolpe, and Rafael Karsten as well as on Bronislaw Malinowski, Franz Boas, Paul Radin, Marcel Mauss, and Claude Lévi-Strauss.

Hence the goal of this paper is to consider the works of Walter Kaudern in the context of the "Swedish School" of ethnography, that is, the comparative ethnographical studies initiated by Baron Erland Nordenskiöld in Gothenburg. Nordenskiöld was the teacher of Kaudern, and volume 2 of his series was dedicated "to my friend Erland Nordenskiöld with gratitude and esteem" (Kaudern 1925b). It was not easy for pioneers such as Nordenskiöld and Kaudern to gain financial support for their research and publications. Again, in connection with his series, Kaudern wrote: "The reason that so many years have gone by since Volume IV was published is mainly that my activities at the Gothenburg Museum have claimed all my time. But there have also been economic factors which have lain in the way" (Kaudern 1938:v).

Kaudern's initiative in 1935 in founding and publishing the first volume of the periodical *Etnologiska Studier*, which has been open to the writing of Swedish and foreign ethnographers, including their longer theses, was a significant step in the formation of the scientific field of anthropology in Sweden. His wish was to cooperate toward reaching the common goal, the improvement of the reputation enjoyed by ethnographical research in Sweden and the widening of this research. The publication was financed almost entirely from his own income, a gift to science and ethnography that gave him no material return.

Theoretical Background

The year Kaudern made his second expedition to Madagaskar, 1911, was also the year that an earlier "pioneer ethnography" and a more "modern anthropology" met in the international book market. James G. Frazer published the first volumes of the second edition of his best-seller *The Golden Bough*, while Franz Boas released his most important book, *The Mind of Primitive Man*. In the same year *Methode der Ethnologie* arrived and, in his introductory note, Fritz Gräbner symptomatically concluded that there was no unified ethnological method. It was becoming increasingly clear that there existed a division between those researchers who analyzed human activities from an evolutionary perspective and another group who emphasized cultural influences in terms of cultural diffusion. Following a long dominance of theoretical views that tend to be encompassed under the term *classical evolutionism*, the discipline began to seek out new theoretical, methodological and institutional routes.

Despite distinctive characteristics, classical evolutionist theories were founded upon a series of related suppositions that make it possible from a historiographic perspective to view them as a paradigm. The point of departure was that sociocultural phenomena are guided by laws that science can discover and that these laws operate in the same way today as they did in a distant past. The relationship between the past and the present is constituted in a change from the simple to the complex. Human nature is uniform, and the power of development rests in the interaction of nature with external surroundings. The cumulative result of this interaction is manifest in the levels of development of various ethnic groups, and it is therefore possible to rank them hierarchically. Humanity's evolution can be divided into different stages, and societies exist that still find themselves at stages through which civilized ethnic groups have passed; for want of data, the earlier developmental stages of civilizations can be reconstructed through comparisons with such societies. Via this comparative method it is possible, with the assistance of these remnants, to determine the character of the lower developmental stages.

If the early development of this thinking in England, France, and the United States can be subsumed in what one can historiographically label *classic evolutionism*, then the direction in Germany was all the more diverse. Concurrently with German evolutionism's studies of the spread of social systems in time, ever more ideas were launched concerning their distribution in space, that is, by diffusion. The combination of geography and anthropology, primarily represented by Friedrich Ratzel, was designated *anthropogeography*.

The similarities among human societies were explained from a diffusionist perspective based upon cultural contact and cultural borrowing. The point of departure for this approach was humans' attachment to traditions and their strictly limited capacity for invention. While the evolutionists attempted to decide on the different stages of development, the diffusionists' goal was to reconstruct the original cultural forms from which differentiation took place and which resulted in cultural diversity.

The basis for cultural-history studies can be divided into two groups: direct and indirect evidence. Included in the category of direct evidence are historical documents that provide the opportunity to compare factual material from different periods. This includes negative documents, that is, the fact that chroniclers failed to mention the existence of a cultural element. When such an element, for example, a tool or some special form of embroidery, appears in more recent material, it is possible to deduce that it derives from European impulses. As direct evidence one can also include dating of ethnographic and archaeological artifacts and native informants' testimonies, either in the form of personal information or as legends and myths.

Fieldwork

Kaudern's first expedition was undertaken "in order to contribute to the solving of the zoo-geographical questions connected with the island of Celebes in the Dutch East Indies." "Besides the zoological work, I intended to study the natives of the country as far as time would allow," Kaudern wrote (1925a:1). As it turned out his zoological studies were limited for various reasons, and he instead focused on ethnographical research (GUB:Kaudern to Furuskog September 29, 1928). He also introduced himself to botanical research, but this was something he hoped to do more systematically if he ever was to return to Sulawesi (GEM: Kaudern to Evans Schultes, January 10, 1940).

In central Sulawesi he came in contact with the Kaili-Pamona. Traveling with his wife and two boys, Kaudern found it easy to make contact with the natives. "My children playing with theirs, learning the language, while my wife interacted with the females," he wrote (1925a:2). Except from the writings of a few Dutch missionaries in the area, these peoples were unknown from an anthropological viewpoint. The Kaili was divided into three classes: nobles, farmers, and slaves. Among the many papers Kaudern prepared was an outline of the noble families of Koelawi (GEM:

Kaudern to K. O. Bonnier, January 4, 1941). Head-hunting, human sacrifices, and secondary funerals were the core of Kaili traditional religion. Also headhunters, the Pamonas were an egalitarian society divided into two classes: farmers and slaves. Men earned prestige and status as warriors, while women gained fame as shamans. The practice of shamanism in Sulawesi was described by Kaudern in letters to the Danish ethnographer Kaj Birket-Smith at the National Museum of Copenhagen (GEM: Birket-Smith to Kaudern, December 4, 1933). But at the time of Kaudern's fieldwork, the core of Kaili-Pamonas cosmology and religious ideas had been outlawed by the Dutch authorities since 1905.

At the time the inhabitants of central Sulawesi were called the Toradjas tribes, and Kaudern classified them as (1) Poso Toradjas; (2) Paloe Toradjas; (3) Koro Toradjas; and (4) Sadang Toradjas. "This classification is somewhat different from the one used by the two Dutch missionaries Doctor Adriani and Doctor Kruijt," he wrote (Kaudern 1925b:1).

Working mainly with the Paloe (or Pamona) and Koro (Kaili) Toradjas, Kaudern stayed at Goeroepahi in June and July 1917, then he moved to Lake Danau and made several motorboat journeys along the north coast. In the spring of 1918 he made his headquarters in Paloe Valley, and in the summer he visited Winatoe and Lindoe. In October he undertook excursions to the districts of Tobakoe, Bangakoro, and Tole, visiting several villages in the area. Christmas was celebrated in Koelawi, with excursions to the districts of Bada and Behoa. By February 1919 the expedition had moved to Kalawara. The Kauderns continued to northeastern Sulawesi and visited the districts of Pada and Mori in June, then going on to Kolone Dale on the east coast. On September 22, 1919, they sailed from Soekon back to Loewoek, where they made their base camp for the remaining part of the year. Excursions in the southern part of Peling Island were carried out before the ethnographical collections were packed as a preparation for going back to Sweden. The expedition was concluded with a six-month sojourn in Java for studies in the ethnographic library (Kaudern 1925a:2–6).

Ethnographical Studies in Sulawesi

The first problem Kaudern encountered was the classification of the Toradjan tribes; he said that "a real classification of the Toradja cannot be based solely on the languages spoken by the different tribes, allowance must also be made for the culture of the tribes" (Kaudern 1925b:2). From his teacher Erland Nordenskiöld, he had learned that geography was an important complement to ethnographical investigations. Nordenskiöld emphasized that nature forced a series of changes upon culture that could be traced through specific historical sequences of adaptation. The environment was primarily a limiting factor, but that did not exclude the possibility that it could also function as a cultural generator of new inventions. Mapping migration was therefore of top priority. Nordenskiöld was convinced that when an ethnic group migrated from one area to another, it attempted to retain and adapt its old culture to the new environment to as great an extent as possible. This made it historically possible to reconstruct migratory patterns and simultaneously form an understanding of the extent to which humans were molded by their surroundings. The school also showed interest concerning what caused groups to migrate in large migratory waves, seasonal migrations, and smaller-scale relocation.

Kaudern devoted the entire volume 2 of *Ethnographical Studies in Celebes* to tracing the migrations of the Koros and Paso Toradjas. On the whole migration went from the south toward the north, but Kaudern tried to learn how each tribe moved as the groups spread over central Sulawesi. He adopted Nordenskiöld's use of maps and investigated historical migrations, traditions, and legends of prehistoric migrations and kinship between the tribes based on cultural, linguistic, and anthropological evidence (Kaudern 1925b:6).

Other areas of interest were houses and temples, house construction, and village patterns, these studies ranging from the mapping of geographical distribution of temples to highly technical descriptions of house construction. His "structuralistic" approach to villages and settlements has recently gained the attention of French anthropologists. Again inspired by Nordenskiöld's studies of South American Indians, Kaudern included fortifications in his study of structures and settlements.

Imitative games, problem-solving games, round games, and gambling games are described in "Games and Dances in Central Celebes," the fourth volume of his series. For the study of musical instruments of the Toradjas, he used the comparative method in order to determine the geographical distribution in southern, southeastern, northeastern, north, and central Sulawesi. Particularly fascinated with musical instruments, he tried to discover which ones were really native to the Toradjas and to trace the origin of instruments introduced among them from other countries (Kaudern 1927:1, 5–9).

Archaeological Studies in Sulawesi

The major archaeological question that Kaudern faced in Sulawesi concerned the gigantic stone images and stone vats found in the hill districts, especially in the northwestern and central parts of central Sulawesi. These were worked by humans and dated from a cultural period previous to the present one, he wrote; "the natives of our days do not know the art of forming stone into images" (Kaudern 1938:2).

The "Swedish School" did not make a sharp distinction between archaeology and ethnography. It was thought that both disciplines dealt with the same problem and could together achieve promising results in the reconstruction of cultural history. History reconstructed the prehistoric epoch as a sequence of events, and ethnography, complemented by archaeology, traced this sequence of events based upon its consequences. As for the stone images of Sulawesi, Kaudern rejected the earlier theories that claimed the statues with oval and slanting eyes suggested a Mongolian race, presumably related to the Japanese, while those with round eyes portrayed the Aborigines of the country. Kaudern said that "I cannot see that there are any facts speaking in favour of this fantastic theory" (1938: 169–70). He argued that one cannot be certain that all the stone objects in question belong to one and the same period (Kaudern 1938:179). It does not seem impossible, he said, that oval eyes belong to an earlier artistic trend and that round eyes, occasionally with a pupil, represent the progress of a later day, when the sculptors had learned how to put more life into a face (1938:170).

Art and Material Culture

As a museum man Kaudern enjoyed a well-deserved reputation. His initiative in rearranging and re-creating the mineralological and ethnographical exhibitions in the Gothenburg Museum made both aesthetic and pedagogical contributions. There are two principal ethnographical collections from Sulawesi in the museum, one made by traveler Sven Fremer in the southern part in the 1920s and 1930s and the other made by Kaudern himself. The latter consists of more than three thousand objects and is accompanied by hundreds of photographs, all objects thoroughly cataloged and systematized.

Masks, wooden figures, coffins, drums, wooden hooks, stamps, cutting boards, wood paintings, painted bast cloth, basketwork, and brass objects are among the many types of objects in Kaudern's collection. Native musical instruments with percussions, cymbals, rattles, bells, drums, flutes, trumpets, and so forth, are also very well represented.

Being an artist himself, Kaudern took great interest in native art. He recorded and described adorned posts, planks, boards, beams, and so forth, and in his notebooks he carefully reproduced geometric designs, dragon and serpent motifs, and reptile, crocodile, bird, and buffalo designs. Regarding wood carvings, he concluded that though whole human figures are scarce, human genitals and breasts are more common motifs (Kaudern 1944:43). In the native temples some pieces of sculpture in relief were found that were not part of the structures themselves, including chairs, detached boards, and even detached pieces of sculptures (Kaudern 1944:61).

"Originally intended to include Art in Celebes in general, but I soon found it necessary to confine my studies to a smaller part of the big island," Kaudern wrote in an outline to the sixth volume of his series (1944: 1–3). It was never completed, due to his untimely death in 1942. Some parts of the manuscript were edited and published by his wife, Teres, and ethnographer Henry Wassén.

Conclusion

Kaudern's studies sought answers to three overall questions: what did the Kaili-Pamona cultures look like in prehistoric times, what changes had taken place since the arrival of Europeans, and how could such changes be explained in terms of migration, diffusion, innovation, adaptation, or acculturation (sometimes, but not often, in terms of evolution)? Like his teacher Nordenskiöld, he emphasized that the question of independent invention and cultural borrowing was of great, perhaps even of the greatest importance within ethnographic science. The problem tangibly captured the general theoretical positions of evolution versus diffusion, which had become extremely polarized via the dominant schools of anthropological science. Cultural change is a result of innovations, dispersal, and adaptation. The question, he felt, is a great deal more complex than the obvious polarization.

As has already been noted, the "Swedish School" sought answers to these problems by using a comparative method based upon analyses of ethnographic collections, cartographic reconstruction, and a meticulous research of older literature. The comparative analyses were primarily based upon comparisons among tribes, geographic prerequisites (comparison of contexts), or artifacts. In connection with the spatial dispersal of ethnographic artifacts, Kaudern could provide a discussion supported by documented material, but when it concerned the determination of temporal sequences in cultural development, he had to rely completely upon indirect evidence. His primary database consisted of archaeological discoveries, and these relied upon somewhat relative dating methods. As a secondary source, to the extent that it was possible, Kaudern used linguistic evidence and based early documentation and reconstructions based upon the later spread of material cultural elements.

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10. Culture and Personality In Henry's Backyard

Boasian War Allegories in Children's Science Writ Large Stories

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> Western civilization allows and culturally honors gratifications of the ego which according to any absolute category would be regarded as abnormal. The portrayal of unbridled and arrogant egoists as family men, as officers of the law, and in business has been a favorite topic of novelists, and they are familiar in every community. Such individuals are probably mentally warped to a greater degree than many inmates of our institutions who are nevertheless socially unavailable. They are the extreme types of those personality configurations which our civilization fosters.
>
> Ruth Benedict, "Anthropology and the Abnormal," in Mead, *An Anthropologist at Work*

> An intimate and understanding study of a genuinely disoriented culture would be of extraordinary interest.
>
> Ruth Benedict, *Patterns of Culture*

> "Oh," said Henry, "I'm beginning to get it . . . we're not born haters. Our Green Devils of prejudice and fear grow inside us . . . because we are worried and afraid."
>
> Ruth Benedict, with Gene Weltfish, *In Henry's Backyard*

Ruth Benedict's ethnographies are often remembered as the most literary, even poetic, produced by the first generation of Boasians. But the creative process, and the slippage of selves and genres, that led up to Benedict's version of the culture-and-personality dynamic in anthropology is less well known. Before coming to ethnography and the final, authoritative name that we know her by, she wrote poetry under pseudonyms ("Anne Singleton" among others), at least one "chemical detective story" with a pseudonym she derived using her husband Stanley's name ("Edgar Stanhope"; see also Caffrey 1989:361, fn. 19), and "empirical biographies" of radical historical figures she termed "highly enslaved women," including a finished piece on Mary Wollestonecraft (Mead 1959:491–519; for Benedict's journal entry in 1914 on this, Mead 1959:132). Interestingly, Mead even recounts Benedict's attempts to keep part of her life separate when she recalls Benedict's writing to her that "signing her married name ('which I always think of as a *nom de plume*,' she used to say) to such papers as 'A Matter for the Field Worker in Folklore' " (Mead 1959: xix; for comments on pseudonyms, see Stassinos 1997:3).

I have written in other places of Benedict's process of sloughing off pseudonymous selves with genres such as poetry as a way to understand her ethnographic writing about "highly enslaved" cultures, embedded as they are with deviants who are used to measure, even test, the homogeneity of cultural norms (1997). And in my dissertation I trace Benedict's theoretical development through Stocking's reading of Boas's work, where we find her "cosmographical" and subjective anthropology in a dynamic tension with the social science of her time (Stassinos 1998; Stocking 1974:10). In Benedict's last genre, this children's story, published in 1948 just before her death September 17th of that same year, Benedict and Weltfish, though, do not describe a deviant who is having difficulty conforming to the cultural "personality"; instead they describe and appeal to the "ordinary" American, a middle-aged conformist, and make him an object lesson in change.

Benedict borrowed heavily from Boas's intellectual trajectories. In the first phase of her work, from 1922 to 1934, she meticulously applies his diffusionist attack on evolutionary stages of culture, locating what she calls a "fixed causality" of culture in "centers" where traits "amalgamate."[1] In the second phase, from 1934 until her death in 1948, she again borrows from his work when she locates this "fixed causality," now termed the "integrating force" of culture, in her notion of a "personality writ large." I argue that after 1934, Benedict had achieved a merger of Boas's "cosmographical" or subjective science and causality with her preethnographic and literary penchant for pseudonyms, for writing herself large and Other, for being her own best informant, having interjected the "deviant's" point of view as a sage and critical voice within the study of a culture's norms. That is, like her biographies of "highly enslaved" women, Benedict's ethnographies contain within them internal critics, individuals who have paid dearly for their insights into the "personalities" that their cultures create and then reward.

Written in 1948, *In Henry's Backyard* can be read as post–World War II propaganda. The text has no page numbers and is "based on the pamphlet

Races of Mankind" adapted by Ruth Benedict and Gene Weltfish, both at Columbia University at this time. The illustrations come from a color-animated film, *Brotherhood of Man*, based on the same pamphlet. *Brotherhood* was produced in Hollywood, according to the frontispiece, "on the initiative of the United Automobile Workers–CIO as a contribution to the American people."

"Henry" marks a real shift in the "personality writ large" she chooses to engage. Instead of being a deviant, a shaman or diviner who, only through rigorous ritual and persecution, finds a place of respect within society, he is a cartoon patriarch, an American everyman with no special talents or "abnormalities" (for Benedict's study of abnormals and shamans see Stassinos 2000). "Henry" is about to get an education in cultural relativism, as he has nothing in common with the "highly enslaved" consciousness-raising feminist radicals in a past century nor knowledge of the "highly enslaved" cultures of today. "Henry" is that American who lives in potential, in the future, who is friendly, open-minded, and excited to meet new people and maybe even learn some anthropology in the process. Although "Henry" comes with pseudoscientific biases about others, he has nevertheless already dreamed of them, or at least of traveling to their lands, thinking in the opening pages that "with this new jet propulsion and atomic energy, a man could really go places . . . maybe a weekend on the Congo . . . or Christmas in Greenland." So how much more wonderful is it that they fly from his dreams and land very literally in his backyard?

Although we could dwell on her Boasian propaganda against race prejudice in her own book and the similar pamphlet, also coauthored with Gene Weltfish, *Race, Science and Politics* (1940) as well as the coauthored book *The Races of Mankind* (1943), both cosmographical expositions on race, I think the space here is more wisely spent on how she uses "Henry" as a popular way to reach the American public, who in 1948 were, although themselves immigrants, buffeted by waves of more and more immigrants, now refugees and survivors violently displaced from an infrastructurally devastated Europe. It is telling to document the xenophobic myths about race and even the religious icons that she and Weltfish target. The book is ambitious. Benedict and Weltfish, as we shall see, hope to do nothing less than replace self-serving ethnocentrism and religiously supported myths with science, war with peace. No small feat for a children's allegory. I will note areas where Benedict actually interjects imagery from her own childhood experiences. I take these images from the autobiographical piece she wrote for Mead, included in Mead's biography of Benedict, *An Anthropologist at Work* (1959).

Henry's Green Devils

> "Every one of Henry's new neighbors had his own Green Devil, and each Green Devil began to whisper to the person he lived in, "psss, pppsss, sss . . . look! . . . They're DIFFERENT . . . stay away from them . . . pppsss."
>
> Ruth Benedict, with Gene Weltfish, *In Henry's Backyard*

> But certainly all my ideology connects my tantrums and my depressions as two different manifestations of the same kink, one supplanting the other. Both I have always called my "devils," not realizing until now that I had slipped into the same usage about my depressions that I had always had for my tantrums.
>
> Ruth Benedict, "The Story of My Life"

We meet Henry, lousy with "ordinariness" but open, through his dreams of a changing, "shrinking" world. Henry was a man who was given to dreaming about "what the world is going to be in the future." He was an ordinary, friendly person, who lived in an ordinary house with an ordinary yard in back of it, where he raised tomatoes and petunias — the usual patch.

But just as hyper-normal Henry raises the "usual" normal patch, he has certain "unusual" or distinctive qualities that are not so enviable, and these qualities are "raised" like those in his backyard: "He also did his best to raise hair on his head, but it was a losing battle. Only three surviving hairs grew there, lonesomely, but that didn't worry Henry too much." Henry resolves worrying about his only abnormality by seeing it as a sign of solidarity with his patriline, we might say: " 'I take after my father, I guess,' he would say to himself, 'and my grandfather, and probably all the other bald heads in my family going back to Adam, for all I know.' " Adam and Eve are inserted in the text later, and comically so, to begin the science lesson, but at this point Henry simply keeps imagining the future, because he knows that "it wasn't the hair on one's head that was important, but the thoughts inside it . . . the thoughts."

Benedict and Weltfish's Henry is a dreamer. He fantasizes a world of different people whom he will visit "with this new jet propulsion and atomic energy." He then dreams "that the whole world became so small that it fitted nicely into his own backyard and all sorts of odd people had become his neighbors." So just as in Benedict's movement from biography to ethnography, in which Benedict begins writing the biographies of cultures with biographies of deviants, providing the measure of that culture's tolerance, Henry's world is now "writ small" and into his backyard. He is the measure of all Americans and their ability (or lack thereof) to adapt to and encourage the acceptance of immigrants in the United States. This "backyard" scenario is reminiscent of Benedict's travels with her imaginary friend whose "family lived a warm, friendly life without recriminations and brawls" in her backyard, with whom she "explored hand in hand the unparalleled beauty of the country over the hill" in her autobiographical piece written in 1935 for Mead (Mead 1959:100].

Waking, Henry is overjoyed to find that his dream has come true: " 'Holy smoke,' said Henry, 'it's really happened.' " But instead of being able to enjoy this most precious moment, Henry is suddenly seized by a demon, his own "Green Devil" of fear of Others, of race prejudice, reminiscent of another character from Benedict's autobiographical fragment (in Mead), her "Blue Devils" of violence, and as she grew older, depression. Henry's "Green Devil" also lives inside him, indeed, in this cartoon, looks just like him:

> It had slithered . . . out . . . of him. And it whispered, "Don't speak to these people, Henry! You won't like them. They're DIFFERENT!" And to make matters worse . . . every one of Henry's new neighbors had his own private Green Devil, and each Green Devil began to whisper to the person he lived in, "psss, ppppsss, sss . . . look! They're DIFFERENT . . . stay away from them . . . pppsss." And when Green Devils get remarks like that listened to . . . Biff! Ugh!! Bang!*?! Zowie!!! . . . which means fight in any language. But fighting leaves you out of breath.

Henry's "Green Devils" have the same effect Benedict's did on her as a child, causing a violent tantrum that not only separated her from an episodic bliss but from all those around her. The cause of Henry's tantrums, even Devils, is easy for the narrator to objectify, as if the revelation is coming from within Henry himself: "And . . . you begin to wonder why you're fighting. Is it because you're afraid?"

Henry is now going to unlearn his fear, unlearn his Green Devil, through a science lesson. Benedict and Weltfish interject a little camp humor through the segue into a sexual difference, using the figures of Adam and Eve as the transitional myth between the issue or "Devil" of race prejudice (or even Henry's more benign ignorance) and the scientific "truth" about peace and modern progress that Benedict and Weltfish want to convey.

The frame that begins Henry's (the reader's) science lesson is a frame showing cartoon Adam and Eve figures surrounded by a green wash. These figures stand on either side of a tree, Eve on the left, Adam, the right.

One bright red apple hangs from a branch dangling over Eve's head. The caption under Adam and Eve seems to respond to the questions Henry has about racial difference on the page opposite with a preliminary exercise in gender differences. Thus the opposite page, still in Henry's inner-dialogue voice, reads: "We *are* different! Look at their colors! How do you figure that?" But the caption under the Adam and Eve figure reads, as if now the narrator has been replaced by the voice of Science (anticipating a tone almost Carl Sagan–like), not God, "Well, Henry, it began a long time ago. (That's Eve on the left.) At first . . ." Not only is the shift in narrator-voice jarring, the interjection to clarify gender is unexpected. Note that gender is already coded into this scene, a frame in which "Eve" is almost overdressed, wearing a two-piece green outfit, with "Adam" wearing only one strategically placed green leaf over his genitals. It is redundant if not an almost camp, not just culturally relative, read on the biblical scene. That is, the narration about race differences, the main example in this argument for cultural not biological understandings of Others for Henry's lesson, begins with a reminder for the reader about sex differences.

In the last years of her life and career, Benedict's thinking about gender, her "woman issue," seems to have leveled out to the point that one has to be sardonically told to differentiate what might be androgynous figures, figures who are, for her and Weltfish, individuals before they are sexually or racially differentiated. And what "race" are Adam and Eve after all? The authors or the illustrators chose to give them an intermediate brown color that none of the race- and culture-types — yellow, black, brown, white, or green (for Green Devils) — have. They do give Adam a kind of fine zigzag afro, but this is nothing like the later lonesome hairs on top of the head of "Henry," his supposed descendant.

What comes next are panels that depict the entire world in green with a blue backdrop of ocean. On the green land masses are dots representing the diffusion of peoples and populations over the earth according to the "races" of yellow, black, white. The authors say that the first peoples were "tan-skinned people, not very different from each other." They say that "as they spread out over the face of the earth, differences in people's skin color gradually grew more marked." The next panel shows the changes in skin color with the earth itself now white, black, and yellow. The strangest feature, seeing as this book comes out in 1948, is that green, and later the colored land masses, exclude the land masses that are home to the United States and Japan.

Henry's Green Devil does not notice that the United States and Japan are excluded from the map, but what does bother him is the nature of the argument. He sneers: "You and your science! . . . Maybe skin color doesn't matter, but lots of other things do. How about brains. That's what *we've* got. Those others have only brute strength. There's a scientific fact for *you*!" And in the frame accompanying this outburst at his black and yellow neighbors, his Green Devil appears coming out of his head, wearing a frown, his three hairs drooping in the opposite direction of Henry's, in a kind of mirror image.

But now Henry is prepared—he has learned "science" now and can question the "facts" his Devil is using to seduce him away from the unprejudiced viewpoint he is preparing to articulate. The narrator tells him, in a frame that shows three brown fetuses becoming, respectively, yellow, black, and white men flexing bulging arm muscles, "No, Henry, it is not a fact. In the first place, strong men come in all colors, and secondly . . ."

At this point Henry undergoes a dramatic pictorial change. In the next frame he is shown with a green head; his revelation about the "facts" of brains has turned him into his Green Devil, and yet the face of the Henry-Devil is no longer frowning although he is green. His white fingers scratch his green head—he is confused. The caption reads: "But Henry interrupted, 'I can believe what you say about strength, but what about . . . BRAINS?'" At this point we get another lesson, the lesson of Benedict's "abnormal" as she learned it in "Anthropology and the Abnormal," which she wrote in 1934 (Mead 1959). Henry has Othered himself, not racially, but in terms of his own worst prejudices. He is no longer at the mercy of embodying the prejudices of his group, because by becoming his Green Devil, he is somehow cured of being one ever again. Thus he learns to objectify his prejudice and interrogate it scientifically in order to guard against, ironically, himself.

The next frame is split, with the upper half showing the three race-colors, white, black, and yellow (the yellow man has slanted eyes and a small blue hat on his head), with the caption reading, "All right, Henry, if you really want to know. On the average, there are small differences in brain sizes. The Eskimos have the largest average brain." But the bottom frame relativizes these "small differences" even further. It shows a man with a small blue hat like the one on the yellow man above, running away from the reader's view with an incredibly large blue head, darker blue than the hat. This monster-headed man is wearing a white shirt and black pants, while the yellow man looks on as if his hat were stolen, perhaps as if he has taken the yellow person's hat. All the colors are rearranged between the top and bottom frames; the caption goes on to read, "And the *largest* brain on record belonged to an imbecile, implicating the running man. So you see, Henry, it isn't size of a brain that counts, it's what you can *do* with it." Having rearranged the "facts" about brains and strength, having shown that quantity is not quality and that qualities such as intelligence "come in all colors," Benedict and Weltfish go into the next "fact" to be relativized — blood.

Henry is told the four different blood types, A, B, AB, and O, and is shown four different gray silhouettes with hearts on their chests (much like college letter sweaters) that contain the letter of their blood type. These race-neutral but blood-differentiated characters give way to Henry's recollection of the time when his neighbor Joe's kid brother Stanley (the name of Benedict's husband), who is depicted as green and sickly and in a hospital bed, was ill but could not take his own brother's blood. A black figure appears, robust and vital and with an A on his heart, and is shown giving blood to Stanley, who, interestingly enough, turns from green to white. These captions ignore the black color of the blood donor's skin; two of them standing together say, "The doctor brought in a man whose blood matched Stanley's. This fellow was a total stranger but his blood . . . did the trick!"

With the Green Devil exorcised as both hate and illness, and with the world expanded to the neighborhood of so-called strangers who do good neighborly things like donating blood, our Henry (and our reader?) is a new man: "Henry got the point . . . strength, brains, blood." But at this point another, different anthropology lesson comes into play. In the next few frames Henry learns the history of the material culture that he believes is evidence of the superiority of Western culture, the superiority of jet propulsion and cars that he assumed made America different, better. Henry learns that while the caveman in Europe was still making "crude stone axes . . . Africans were forging them out of iron." He learns that the wheel "was discovered by the Babylonians, who first used it for their oxcarts." The wheel is diffused across culture until people from Henry's culture are shown using it to steer ships, to pioneer America, and to fly. Of course weapons of war are not mentioned as products of these material innovations, even though the mention of "flying fields of countries all over the world" brings to mind the uses to which planes have been put, such as bombings during the few years before the book was written.

Benedict and Weltfish then do something that may or may not reflect their need to normalize all of this new and probably radical information for Henry and the reader. They postulate universal desires on the part of humankind, teaching what all races and cultures want. First they show the different races as represented by individual men, then as humans paired heterosexually, then as families with many children, then again individually in front of places of worship, with the caption that all human beings desire "love and home . . . a family growing up . . . and the right to worship in their own way," and later, that each is entitled to "good health . . . to a good start in life, and a good school . . . and those who want higher education should have it." At this point the reader is Henry, or at least shares his viewpoint. This is clear from the way the illustrations on the panel depict individuals in front of various houses of worship signaled by a star, a minaret, a cross, and a pagoda-style frame, implying a Jewish white individual in front of a synagogue, a Muslim and black individuals in front of a mosque, and a yellow individual in front of a temple, but there is no individual in front of the cathedral with its cross. I assume that the latter is Henry's and, perhaps, the reader's house of worship. If this is the case, the reader inhabits Henry's Christian but now, presumably, educated and peaceful point of view.

Children from the different races and cultures and work environments are then shown as wanting to imitate adults. Benedict and Weltfish again appeal to universals, writing that "sensible" people everywhere want peace and friendship, that differences "are not inherited" but come from "something called cultural experience or environment." Then they recapitulate the source of the trouble in the first place — the Green Devils, saying: "Sensible people stop kicking each other around and apply their boots to the seats of . . . the ugly Green Devils of prejudice stupidity, hate."

Benedict and Weltfish probably very consciously arranged for the depiction of the violence caused by "Green Devils" to implicate the United states as being "possessed" by these devils post–World War II. The next frame tells the reader a kind of liberal "origin myth" about these Green Devils. The Devils arise, we are told, because "frightened people are apt to do foolish things," and what frightened them in the first place includes "an unhappy childhood," the fear that "they'll lose their jobs or their savings," the thought that "they'll be sick and unable to afford a doctor," of "getting old . . . of being has-beens . . . and losing the respect of their community," making many people "jumpy . . . and suspicious . . . and too ready to take it out on the other fellow . . . especially a *different* kind of fellow." Scapegoating theory could not find a clearer, simpler statement.

The second-to-last frame invokes the Protestant work ethic as a remedy for any prejudice: "So no matter where they were born, or what the color of their skin, they'll have the chance to work together at the jobs *that need to be done*. . . . Does that make sense to you, Henry?" And in the very last frame we have many different kinds of workers turned toward the reader, wearing "different hats" and clothes characteristic of business, police work, railroad work, with one yellow man at the end in a medical doctor's outfit. The story ends with a plea to the reader. What we think is the last frame (because it is illustrated) says: "We've only got one world and we're all in it. If we can adopt this scientific way of looking at things, we can rid ourselves of useless anxieties and fears, and all get together to contribute to the coming of a better world." But the very last frame has no picture with it at all. Instead of the conventional "THE END" of a children's book, it exclaims, "OKAY HENRY?" on a white background.

This book is a wonderful biography of a cartoon everyman at a time when many countries were trying to rebuild their infrastructures without the fascism and nationalist propaganda that so divided Europe during World War II. Henry is the "ordinary" man who is willing to learn but needs to be taught that his "fears" are "Green Devils" that promote his race prejudice and his inability to envision the "small world" that his material culture is making smaller every day. This last experiment in genre styles, after ethnography, of a children's story is a fascinating way to read how Benedict finally separates her average American from the prejudices that, in her other works, she argues against. She also uses the genre of a biography of a "normal" American to include her reader in the action of the book, the anthropology lesson that will "save" her reader from the devastation of yet another World War fueled by ignorance of universal needs.

In other words the subjectivity of the scientific "abnormals" in her ethnographies, particularly her depiction of a shaman's possession rites (see Stassinos 2000), is reversed in this work. Here the "ordinary" man is the one "possessed" by Green Devils; he struggles to defeat them, and Science, not God, allows him to exorcise them. On several occasions he changes places with the reader. For example an American reader (maybe an adult and not a child at all) is implied when the United States is not shown on the map or when there is no individual standing in front of the Christian cathedral. In this story the everyman American Henry is divided, unaware of his hateful prejudices. But the difference between Henrys is only one of education and priorities. Henry can realize that other American ethic besides nationalism—work. Here the Protestant ethic is shown to be the measure of the man, for it is prejudice that gets in the way of the greater capitalistic model for nuclear family, freedom of worship, and even middle-class educational striving. Work absorbs all of these differences, relativizes them, renders them null, cartoonish.

Benedict and Weltfish probably saw *In Henry's Backyard* as an extension of their war work, bringing Americans, even newly minted ones, into an anthropological frame of mind. As a theory of war, it is almost psychological, with war is based on fear and ignorance. To return to the opening quotation, which Benedict used twice in her work, it is often the personality that a culture most rewards, the conformists, who are the most dangerous.

Notes

1. The term *fixed causality* comes from Benedict's Ph.D. dissertation, "The Concept of the Guardian Spirit in North America" (1923:7); the term *amalgamate* comes from her paper "The Vision in Plains Culture" (Benedict 1922), reprinted in Mead (1959:20).

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