Histories of Anthropology Annual

Volume 3

EDITED BY REGNA DARNELL & FREDERIC W. GLEACH

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Editors' Introduction

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With volume 3, *Histories of Anthropology Annual* has clearly established a critical mass demonstrating the legitimacy and interest within the discipline in different possible positionings regarding its history. Some readers may concentrate on the stories or case studies, others will be concerned with the theoretical voices of histories within the discipline; some will focus on individual pieces, while others may seek the strands that draw certain pieces together. The thirty-two articles published to date vary greatly in subject matter, methodologies, and links to other disciplines or approaches. There could be no clearer demonstration that there are multiple possible and actual histor*ies* of anthropology. This volume for the first time represents all four traditional subdisciplines of anthropology, and moves through several national traditions and their intersections with work in diverse ethnographic areas. Pieces range from individual biography to examinations of theoretical streams and institutional contexts, but all expand in their connections and implications to broader issues than may seem to be their subject at first glance.

While each paper stands on its own as a piece of disciplinary history, we identify a kind of unity of theory and method that engages with the significance of studying and writing histories of anthropology from within the discipline. Looking at only one volume, one might be inclined to consider the contents to be random. Looking at several volumes, however, the existence of a journal where colleagues can find historical reflection and research begins to justify itself. Thematic volumes of interest to the history/ies of anthropology have been around for a long time. But what does one do with a paper that stands alone? Often, such papers are written for conference sessions or volumes where they appear as the first contribution and set a context for what follows. Such an approach shows that anthropologists think historically about their work, but it does not focus attention on the process of historicizing and its relation to disciplinary praxis. We surmise that the majority of anthropological histori-

ans, whatever their disciplinary base, envision an anthropological audience, and journals in the history of science rarely attract wide audiences among anthropologists. We hope that this series has contributed—and will continue to contribute—to increasing awareness and use of historical approaches in teaching, learning, and doing anthropology.

We have come a long way from the days when history consisted exclusively of names, dates, and events cataloged in chronological order. Some papers may present fairly straightforward stories or case studies, but these are implicitly framed within larger issues that attract an audience for histories that are no longer firmly located in the past. Historicism and presentism no longer seem like neatly bipolar categories; rather, they challenge the historian to specify her/his position in the telling of a story that brings the past into relevance with the present and future. The long-established standards of historicism remain, but the relationship of issues chosen for historical attention and the audiences for such analysis and interpretation are more fluid—and far from shared among the diverse contributors to the first three volumes of HOAA. Yet the terms of the debate seem to us to be shared, at least implicitly.

We invite readers to enter this discourse by submitting papers, commenting on developments in the field, and recommending topics or authors. We remind readers that commentary and book review sections are envisioned for HOAA and that we welcome such contributions. One need not even consider oneself a disciplinary historian; submitted manuscripts, other publications and presentations, and reflections on possible books for review have led us to consider that many who would not bill themselves as doing history of anthropology actually contribute actively to the disciplinary conversations that we hope to sustain.

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1. *Leslie A. White and the Socio-Politics of War*

William J. Peace

There never was a good war or a bad peace.—Benjamin Franklin

Leslie A. White was a pivotal figure in the development of American anthropological theory in the mid-twentieth century. White is well known for reviving evolutionary theory in the post-Boasian era and for his highly polemical exchanges with scholars such as Alfred Kroeber, Robert Lowie, Morris Opler, and others. White's reputation as an "ill tempered polemicist" was well deserved and partly responsible for the fact White's clandestine career as a committed socialist throughout the 1930s and 1940s is not well known (Peace 1993, 2004:69–98; Price and Peace 2003). White was a member of the Socialist Labor Party, wrote under the pseudonym John Steel, recruited people into the SLP, gave stump speeches to various groups, and was in every sense of the term a political activist in a radical socialist organization. For his efforts, White feared discovery, used aliases, and was hounded by administrators at the University of Michigan who were appalled by his political views.

Given White's background, one would suspect that the social upheaval and revival of radical political activism on college campuses during the 1960s would have excited him. One would also think that White, a World War I veteran and staunch opponent of all war, would have embraced 1960s political activism that was centered on ending the Vietnam War. This was not the case. White expressed little interest in the Vietnam-era anti-war protests and had a total lack of respect for the largest and most influential group of the era (Students for a Democratic Society). He pointedly refused to get involved in anti-war demonstrations and, much to the consternation of his peers, declined every invitation to participate in teach-ins that were in vogue and highly influential. Why did White reject the political activism of the 1960s, especially since it was grounded in opposition to war? This paradox and its antecedents are the subject of my analysis. To this end, I will discuss three facets of war and how they

figured in White's thoughts, beginning with the impact White's World War I naval experience had on him and how it changed the direction of his career. Next, I will discuss how White's anti-war views led him to the Socialist Labor Party and why he came to the conclusion that all war was an abomination—a reflection of the capitalist system doomed to extinction. Finally, I will compare White's early anti-war works with those he produced at the end of his career that expressed a deeply pessimistic, if not apocalyptic, view of the future. In so doing, I will detail the reasons why a scholar who came of age during the Depression not only failed to embrace a new generation of anti-war political activists but purposely distanced himself from them.

World War I and Its Impact on White's Thought

The U.S. Navy has little information about White's experiences aside from his enlistment and discharge records. These papers record the basic facts about his naval career: he enlisted in Denver, Colorado, on March 22, 1918, and was discharged in New York on August 25, 1919. He "performed honorable active service on board the following ships and stations: Naval Training Station San Francisco; Receiving Ship Mare Island; U.S.S. *Ringgold*; Naval Hospital New York; Receiving Ship At New York; U.S.S. *Minnesotan*." On a scale of one to four, four being the highest, he earned the following proficiency ratings: seamanship, 3.5; ordnance, 3.16; signaling, 3.17; ability as a leader of men, 3.4; sobriety, 4; obedience, 4. His overall standing and proficiency was 3.63.

White's decision to join the Navy was an odd one. His formative childhood experiences were on the plains of Kansas, and before he enlisted he sought out the advice of his mother rather than his father.[1] Although White's tenure in the Navy was relatively short, it had a profound impact on his thought. White never articulated exactly why he joined the Navy, but two factors figured significantly.[2] First, like many men his age, White perceived the sinking of the passenger liner *Lusitania* to be a grave threat to democracy. He vividly recalled when his father told him World War I had begun: "I was in a corn field with the temperature about 102 and my father came out and said to me the Germans invaded Belgium. This was profound and it had a disturbing, upsetting effect upon me. Wars to me had always been in history books and they had no other reality. But now this was a real war and the Germans were real men" (White 1974). Second, White's two best friends decided to enlist in the Navy, and he concluded enlisting was the best way to actively make the world safe for democracy. White had just turned eighteen, had never had a drink of al-

cohol or smoked, and was a virgin. He had an intense romantic desire to "See the World," as the recruitment posters exclaimed. After White graduated from basic naval training, he applied for admission to the Naval School for Specialized Instruction. He worked hard and, along with two other young men, was selected to join a destroyer crew that was waiting for their ship to be completed in the San Francisco shipyards.

The U.S.S. *Ringgold* was a 1060–ton destroyer launched November 14, 1918. The majority of the crew had considerable naval experience when White participated in her maiden voyage. In a retrospective letter that sought to discover the fate of the *Ringgold*, White recounted the basic elements of his naval experience: "I served as an enlisted man in the U.S. Navy in World War I and was assigned to the nucleus crew of the *Ringgold* during the summer of 1918. I was one of the crew, both navy and civilian, that took her out on her trial run. In November we left San Francisco, went down the coast of Mexico, through the Canal (where we stopped 2 or 3 days in drydock for repairs to a propeller and other adjustments) and on to New York City" (letter to P. G. Fillip, July 31, 1972).

White's experience in the Navy was somewhat unusual because the *Ringgold* was commissioned three days after the Armistice. Accordingly, the professional naval crew and officers were no longer concerned with the war but with the downsizing that would inevitably follow.[3] Men such as White, who joined the Navy for patriotic reasons, had their motivational factor eliminated. In addition, a nucleus crew on a ship under construction lives under very difficult conditions. According to naval historian Paul Halpern, "the ship was most likely partially functional and the general living conditions squalid. It was akin to living in a home while it was under construction" (letter to Peace, September 23, 1997).

Life in the Navy was exciting, exhilarating, and enjoyable despite a few "unpleasant episodes." Brawls were not uncommon and the veteran seamen picked on the "greenhorns" who seemed the most insecure. For the rest of White's life he fondly recalled the brotherhood among sailors and, when traveling abroad, always tried to take a ship rather then fly. In a letter to his friend and former student Edward Norbeck, White recalled, "I fell in love with San Francisco long ago in 1918 when I entered the Navy and had my rookie training at Goat Island . . . and was assigned to the nucleus of a destroyer crew at Mare Island pending the completion of the ship at the Union Iron Works. My six months in San Francisco were probably the most exciting and romantic (in the broader sense of this term) period of my life" (letter to Norbeck, October 17, 1968).

There can be no doubt that White had conflicting memories about his

naval experience. Toward the end of his life, he recalled, "I enjoyed being in the Navy. It was a great experience for me, a pleasant one, and very instructive. I loved the sea life and the ships. The only thing I didn't like about the Navy was its purpose. While in the Navy no one seemed to think much of its purpose, that is, its ultimate purpose. And whenever I did think of it then, I thought it a noble one. Was it not committed to sweeping the scourge of U-Boats from the sea, thereby making the world safe for democracy?" (White 1973:45).

Based on his Naval experience, White realized that matters such as electrons and the transmission of light were of secondary importance. Moreover, the two worlds White had been exposed to were diametric opposites. There was his father's world on the farm, isolated, barren, and filled with endless drudgery; and life in the Navy, replete with liquor, exotic ports, women, and the raw edge of life. World War I was also a shock to men and women of his generation, as it destroyed their preconceptions about the nature of civilization and, for White, of capitalism. Prior to the war, many believed civilization had advanced beyond warfare and the wanton destruction of life and property. World War I provided graphic proof this was not the case. The ultimate cost of World War I in terms of money circa 1918 is staggering; the estimated direct cost was $180 million. The loss of lives for the victorious and defeated powers alike was overwhelming. Conservative estimates of the loss of life are mind-boggling: Germany suffered 6.6 million, France 2.8 million, and Russia 9 million. Great Britain, excluding Scotland and Ireland, sustained casualties of 3 million.

After White was discharged from the Navy he attended Louisiana State University for almost two years. In 1921 he transferred to Columbia University, where he received his BA in 1923 and MA in psychology in 1925. At Columbia White did not limit his activities to academics. He became active in a number of extracurricular activities—all with a political bent (Peace 2004). Among the groups White was active in was the Come Back Club. Created by fifty World War I veterans in 1919, the Come Back Club was composed of men who were disabled during the war. Members included those who served in the Army, Navy, and Marine Corps and were temporary wards of the Government under the Federal Board for Vocational Training. Between 1919 and 1925 there were about one hundred veterans at Columbia University studying under the Federal Board. Concerned with the mental, physical, and social recovery of disabled veterans, the Come Back Club sought to "promote comradeship among all Federal Board students attending Columbia University; to perpetuate the ideals of the Great War, and to maintain a determination to win through

all handicaps, knowing that nothing can stop them where the 'Spirit's the Thing'" (*Come Backer* circa 1922).[4] The Come Back Club was organized in large part because veterans believed the Federal Board was not doing enough to help students at Columbia and the club offered "a ray of sunshine in the darkness of our struggle to come back." The formal Preamble to the Come Back Club's constitution stated, "In order to foster the comradeship of the Great War; to promote mutual helpfulness in the common cause of vocational training; to stimulate a desire for exceptional individual and group effort; to inspire public confidence and create a 'commercial acceptance' of the rehabilitated man; and to provide contact and diversion, we, the Federal Board students of Columbia University, disabled in the service of the United States during the World War, do hereby organize and subscribe to the following constitution" (*Columbian* 1921:240).

White was an active member of the Come Back Club for two years, and participated in most activities sponsored by the club. Activities were diverse and ranged from putting on plays, organizing socials, and raising money for disabled veterans to visiting Washington DC and monitoring disarmament conferences. The main thrust of the Come Back Club was to secure benefits for disabled veterans and keep abreast of disarmament talks. As a member, White selected and managed student and guest speakers. At White's suggestion, during the 1921–22 academic year speakers discussed a single topic—disarmament. White himself gave talks to various groups about disarmament and the prospect for world peace.[5] White's interest in disarmament and helping Word War I veterans occupied much of his time. In his journal White expressed concern that he was spending too much time on the "disarmament business" and that it was adversely affecting his academic work. This did not disturb him because he was beginning to question the value of his formal education. White felt he had an "inner drive"—that is, there was something in his fundamental makeup that made him disinclined to accept the status quo. He wanted to make a concrete contribution to the present and improve the future. He was quite disillusioned with Columbia in general and its President Butler in particular. In his journal White noted that from the Revolutionary War to World War I the university defended the status quo. White was particularly irked by the creation of the Division of Intelligence and Publicity—an extension of the university that had nothing do with academics but rather with propaganda. He noted in his journal that "Today's students are tomorrow's soldiers. Their training in the fields of chemistry, engineering and the medical sciences is prostituted to the slaughter of human beings with whom they have no quarrel. Instead of

using their knowledge to promote human welfare, they must assume a completely destructive role" (journal entry, circa 1920).

Statements by President Butler of Columbia University such as "security of tenure is desirable, but competence and loyalty are even more desirable" led White to conclude an activist's life outside the realm of academia was desirable. Halfway through the 1921–22 academic year he wrote that he went for a long walk through Riverside Park in a snowstorm and came to the realization that his future needed to be action oriented and wrote, "I am beginning to question very closely the value of my college training. I don't believe the knowledge I will gain at college will help me much. I think that there is a force, which I think I possess, that is a driving force with which I can assume a certain degree of leadership among men. I never will be able to command the masses of men, but I seem to be able to command those who do command the masses" (journal entry, January 22, 1922).

When one combines White's departure from Kansas, his naval experience, and activities in the Come Back Club, all these variables made White's junior year at Columbia a watershed event.[6] The most explicit statement White made about the impact his junior year had on his thought was in the cover letter of a grant proposal he sent to S. P. Capen, chancellor at the University of Buffalo:

> My junior year was a revolution to me. I was overwhelmed when the full realization that everything is not as it pretends to be, struck me; almost everything seemed to me to be "out of joint." I became a revolutionist. I was eager to set out to reform the world; all the injustices, cruelties, and stupidities of our whole social system were to be wiped out. And I beat my wings against the bars of my cage until I fell down bruised and exhausted. While I rested, I wondered if that *were* the way to reform the world. Then came the second stage of disillusionment—the awful realization that the mere discovery of an abuse and a publishing of this news to the world, did not *at once* remove the abuse. With this discovery, my militant revolutionism subsided, and I discovered maturity. (letter to Capen, December 31, 1928)

White desperately wanted to undertake a world journey and wrote that "almost everything depends upon whether I receive this fellowship" (letter to Capen, December 31, 1928). White wrote that he wanted to observe as many different "primitive" cultures as possible because they were disappearing and being "engulfed by the rising tide of industrialism." White's vision of world travel was sweeping, writing he hoped to travel "through Central America, Peru, Chile; thence westward through

the islands of the Pacific; visit some jungle tribes in Malaysia; some nomads of the Siberian steppes; the remains of the great civilizations of Babylonia, Asia Minor and Egypt; a tour of remote corners of Africa—then home." Toward the end of White's proposal he wrote that his experiences in the Navy during World War I were ones that he had yet to come to terms with. Even a decade later he noted, "I did not believe that even now I fully understand and appreciate the impressions that the War had upon me." Although White did not participate in any battle, his experience led him to become adamantly opposed to all warfare:

> I entered college in the Fall of 1919. But I did not study physics (except an introductory course). My interest had shifted from the inorganic to human beings. I was awed and appalled by the war; I could not understand it. And I had the sickening feeling that our verbal explanations and professions did not really account for our behavior. I wanted to know *why* millions of men should murder and mangle each other, and why women and priests should urge them to do it. This was the central core—at the beginning, at least. Around it were gathered a host of other perplexing questions concerning human behavior. At any rate, such matters as electrons, the transmission of light, etc., although important, were felt to be decidedly secondary to the vital problems of human contact. (journal entry, December 1928)

White was convinced there was an overwhelming need for a new science of anthropology. He did not find this "science" at Columbia or in any of the fields he had studied—history, economics, political science, and sociology. He also sought out political organizations active on campus and found them equally lacking. He firmly believed the world's problems were profound, dangerous, and threatened the long-term future of humankind. While no science he could envision would solve all the world's problems, the world desperately needed help. For he was sure of one thing: "The Great War was a shock to millions of people. To a great degree it destroyed their conception of the nature of civilization. They had been deceived; they had felt that we had advanced beyond wholesale murder and self-destruction. Many lost faith in everything" (journal entry, December 1928).

Socialist Labor Party and the Science of Anthropology

White's interest in social evolution was inexorably linked with his political beliefs and, after 1929, with the Socialist Labor Party. White was drawn to the SLP for a myriad of reasons, foremost among them was the

fact social change was not linked to violence and offered a non-violent form of revolutionary change. The SLP was against all war and sought to abolish a social order that compelled capitalists and workers to wage war despite their hatred of it. The non-violent emphasis of the SLP did not mean the party was powerless or lacked the will to be forceful. Rather, force and violence meant two different things: for capitalists, force meant military action intended to protect the ruling class. For the SLP, force was designed to avert violence and promote Socialist Industrialism advocated by its founder, Daniel DeLeon. Force was thus an equivalent to the military with a twist—that is, a mighty non-military engine of physical force. White believed real power rested in the control of the nation's productive resources and that the triumph of force depended upon economic power and the ability to organize actual material instruments. In short, economic force was the control of industry. Violent protests were unnecessary and counterproductive because the workers already had de facto possession of industry. Workers were already in a perfect position to assume complete control of the nation's economic machinery and, with its non-military engine of physical force, industrial unionism was poised to revolutionize American workers and the entire social structure. The forthcoming revolution was going to be won at the ballot box—thereby averting physical violence, economic paralysis, and war. Without the socialism advocated by the SLP, there would never be peace.

In linking the theory of cultural evolution with socialism, White believed he could make an objective and concrete contribution to society and remove anthropological theory from the sphere of abstraction to the "real world." Socialism and the evolution of society represented the future—that is, a system that placed a priority on human welfare rather than on corporate profits. White embraced cultural evolution for personal and intellectual reasons—ones grounded in the deprivations facing American citizens during the Depression. Thus, White's evolutionism must be viewed as being theoretical, practical, and closely tied to a radical socialist ideology. That socialist ideology was based on the work of Daniel DeLeon and Lewis Henry Morgan, and was the platform of the Socialist Labor Party.

Although White's interest in socialism existed since his days at Columbia, he began to think about a career in radical politics when he returned from a trip to Soviet Union in 1929. White was among the first foreign scholars to visit the Soviet Union, and this trip inspired him to read more works under the rubric of the Marxist tradition (Hook 1987; Feuer 1962). In a letter to Marvin Farber he wrote that since his trip he had

> become very much interested in the Russian Revolution and its possible consequences for the rest of the world. I have done a little reading in the philosophy of the Communists—the writings of Lenin, Bukharin, Marx, etc. and I must say that I am much impressed—excited in fact. It does not seem improbable that I may be weaned away from the more narrow and specialized pursuits of anthropology in the future. . . . I must know more of the actual economic development of modern nations before I can do anything. I think the most important thing that confronts the nations of the world today is economic reorganization. And some fundamental economic change is inevitable, it is bound to come. (letter to Harry Elmer Barnes, December 20, 1929)

The next day White sent Barnes a copy of a paper entitled "An Anthropological Appraisal of the Russian Revolution" that he would deliver to the joint meeting of the American Anthropological Association and the American Association for the Advancement of Science. Elsewhere I have detailed the impact this paper had on White's career, but suffice it to say that a firm anti-war sentiment was at the core of White's address (Peace 1993, 1998). White was convinced the Russian Revolution was the most significant event in modern history, and he began his address by noting that the revolution gave "birth to a new social order, a new way of life, so different from our own that most of us are quite bewildered when we contemplate it" (White[Steel] 1931:14). He was also convinced that the Russian Revolution was not a revolution in the truest sense of the word but rather a cultural mutation resulting from centuries of cultural development. Furthermore, he maintained that communism as defined by Marx did not exist in the Soviet Union. The fact that communism had not been achieved did not make the Russian Revolution any less significant.

White's interpretation of the Russian Revolution was contrary to that of many, among them Lothrop Stoddard. Stoddard maintained the Russian Revolution was a grave threat to democracy and civilization. In White's AAAS address he took direct exception to Stoddard, whose influential book *The Revolt Against Man* held that social revolution "is not progress but regress; not a step forward to a higher order but a lurch backward to a lower plane. Therefore, countries like Russia, with veneers of civilization laid thinly over instinctive wildness and refractory barbarism, are particularly liable to revolutionary atavism" (Stoddard 1924:178–179). The main thrust of Stoddard's book was to prove that the revolutionary unrest that dominated the globe "root cause is not Russian Bolshevik propaganda . . . but a process of racial impoverishment,

which destroyed the great civilizations of the past and which threatens to destroy our own" (Stoddard 1924:1).

White's views were radically different. White maintained that capitalism was dying and that it could not continue to exist: "[S]igns of its disintegration are visible on all sides." He decried the Treaty of Versailles and the attempted "Balkanization of Europe" (White[Steel] 1931). White believed that breaking Europe into little pieces ran contrary to the age-old trend of social evolution that formed larger and more complex national entities. He predicted only one or two superpowers would emerge from any future global conflict and those powers would be ruled by capitalists. It was obvious to White that American society was ruled, and would continue to be ruled, by a few wealthy industrialists—"tycoons of finance and industry":

> Our state, in spite of our preposterous fiction of democracy, is still a committee which negotiates the lucrative affairs of the manufacturer and investment banker. The army, as of yore, and the police are employed in case of resistance; the Marines are sent to Central America to collect bank loans, and the militia and the police shoot and club union organizers. And the priests, as always, know well their master's voices. (White[Steel] 1931:16)

At the core of White's argument was the belief that imperialism, and hence capitalism, could exist only as long as there were fresh markets to exploit and raw materials to extract. Given the fact that virtually the entire world's raw materials had already been appropriated, White believed the collapse of capitalism was inevitable. The manner in which capitalism would destroy itself was through war. White's position was diametrically opposed to that of 1929 Nobel Peace Prize winner Frank B. Kellogg. In Kellogg's acceptance speech, he maintained that the carnage and loss of life during World War I was fresh in the minds of modern statesman who were working hard to ensure another war would not take place. Kellogg did not envisage any signs that would indicate another war was going to take place. He further maintained that the economic depression was a natural phenomenon of the postwar era that would pass with time. The unrest and talk of revolution was not due the possible collapse of capitalism but rather the overall dissatisfaction and agitation associated with a transient and short-term global depression. It was not a precursor to war. Kellogg was convinced enough safeguards existed for nations to peacefully resolve their differences, and maintained peace would reign for many decades. White pointed out that if he was convinced of one thing it was that deterrents to war almost always led to war. White argued,

> Anyone who has any vision and grasp of cultural processes must realize war is not only likely but even imminent. Indeed, one might say that capitalism needs a war now, for it would solve the two most pressing problems of the day. It would consume our excessive production of commodities and it would slaughter the unemployed. But the new lease on life would be only temporary at best. War will eventually destroy the system that promotes it. The logical conclusion of capitalism is martial suicide. (White[Steel] 1931:16)

World War II: White's Consistent Critique of War and Capitalism

White's anti-war views never wavered. Before the outbreak of World War II White was quick to point out the hypocrisy of war and the profiteering that would inevitably take place. He was critical of capitalists and unions who likened themselves to opponents when they were simply complementary parts of the capitalist system. Under the pseudonym John Steel, White wrote that unions were useless and did not have the workers' interests at heart. In the article "CIO Agreement Encourages Members to Enlist for War" White decried the union effort to ensure that workers who entered military service would retain and accumulate seniority in the union while on active duty. White was appalled that Walter P. Reuther, regional director of the UAW, supported this effort and labeled him a traitor to socialism and workers. Railing against this policy, White wrote, "In the deepening shadow of an imminent world war, when it should be the duty of all class conscious workers to teach their fellow toilers that war springs out of production for profit, we have the disgusting spectacle of union leaders signing a contract that makes, or at least attempts to make, the proposition of maiming and killing our brother workers of other lands attractive" (White[Steel] 1939).

White was equally critical of wealthy industrialists who stood to profit from wartime production. In a searing article, "Henry Ford Against War; Not Against Profits of War," White wrote that it was easy for men such as Ford to state they were opposed to war (White[Steel] 1940). White considered Henry Ford the archetypical industrial capitalist and his views were the embodiment of what socialism railed against. Ford's anti-war statements were, in White's estimation, a blatant attempt by Ford to raise himself above international conflict. This was disingenuous as Ford was far from objective. White pointed out that Ford stood to make a tremendous profit from any global war—a fact Ford was well aware of. White considered Ford's statement against the war a ruse and he mocked Ford. White wrote that at the same time that "poor old Henry was voicing his

hatred of war he was telling the world he could produce 1,000 planes per day":

> This same kindly old man could say, "I'm against war and always have been, and I can truthfully say that nothing good ever came out of war," is proposing to build, in his own plants, at a substantial profit, every twenty-four hours, 1,000 of the most deadly of all death-dealing devices. Is Henry Ford a liar? Does he really mean that he is against war? Henry Ford is in the same category as the majority of the capitalists. As creatures with the milk of human kindness in them, they naturally detest the brutalities of war, but as capitalists, embodiments of class relations, they are compelled to make profit from the machines that deal suffering and death to untold millions of innocent men, women and children. (White[Steel] 1940)

Even when there was virtually unanimous support for World War II, the death and destruction appalled White. Expressing anti-war views was simply not done during World War II—it was considered sedition by the Federal Bureau of Investigation.[7] While White shared Barnes's anti-war views, he did not express them openly. White knew such statements would not be tolerated; however, this did not stop White from expressing his outrage in his journal. White found fault not just with Germany or Japan but all warring nations:

> We seem to be "winning the war" now. That is, our killing and destroying seems to be exceeding those of the enemy. . . . This war sickens me. The lying, cheating bragging; the nauseating hypocrisy, the parading of vicious hate, envy, greed, as holy and noble striving and idealism. Using the war and patriotism to shackle and enslave—to humiliate and discredit—the working class—while at the same time proclaiming that this is a war of the common man—is sickening. If there is anything noble, lofty, genuine, unselfish in this war, it is among the common ignorant folk who have really been taken in by the propaganda. And even then, one wonders how much of this rises above the level of tropism and reflex. (White journal entry, circa 1944)

White's Views on the Political Unrest during the 1960s

By the time White was sixty years old he was physically and mentally exhausted. As a scholar, White's contribution to anthropological theory was negligible after 1959 (that is, aside from a remarkable record of

training anthropologists and archeologists who would go on to have distinguished academic careers). A dichotomy exists between those works written early in his career with those he wrote in the post-1960 era. Simply put, White was tired of fighting about the value of evolutionary theory. He was disgusted with people who slapped him on the back and proclaimed that they had always been evolutionists. White was also embittered about his personal and intellectual isolation in Michigan. Letters White exchanged throughout the 1960s and early 1970s all express profound loneliness, restlessness, and ambivalence about the future. White had good reason to be tired: after the age of sixty he had taught for over a decade without a sabbatical (including summers), gone through an intensive program with Alcoholics Anonymous, briefly remarried and gone through a contentious divorce, sold the house he and first wife, Mary White, had lived in for many years, witnessed the deaths of his brother and sister, and, upon retirement, faced the prospect of moving from Ann Arbor, his home for the last forty years. In a very sad letter to his friend and fellow faculty member Raymond Wilder he wrote, "I am just weary of working. I would like to take a trip somewhere; anywhere almost" (letter to Wilder, circa 1969).

Given White's early involvement in radical politics and magnetic appeal as a teacher, one would think he would have been energized by the political turmoil of the 1960s. This was not the case. White was uninterested in and critical of Vietnam-era protests and the rise of a more politicized and radical anthropology (Moore 1971). He rebuffed faculty and students alike who wanted him to get involved in the protest movement. In March 1965 White was pressured to participate in a "teach-in." He categorically refused (Sahlins 2000:209–217). White's refusal to participate bewildered Marshal Sahlins and Eric Wolf, who were very active in the anti-war movement. Their experience and involvement in the anti-war movement was a significant event in their lives and teaching careers (Wolf 2001). As Yengoyan (2001:vii) has written, "it was the Michigan and Vietnam years that I feel were pivotal to [Wolf's] writings." Sahlins, too, was profoundly affected by the Vietnam War and campus unrest. Sahlins has characterized the 1960s as the longest decade of the twentieth century, and wrote, "the Vietnam War was major intellectual watershed in the social sciences and humanities, at least in America; and particularly in the academy" (Sahlins 2000:11). For Sahlins, the struggle of the Vietnamese people, a brief tour of Vietnam, and the moral and political dissatisfaction with the war undermined his belief in scientific anthropology. This led Sahlins to distance himself from White and sent his career off

in a radically different direction. In looking back on the impact the anti-war movement had on his thought, Sahlins expressed retrospective frustration with White's inability to change and refusal to participate:

> Leslie White always used to say that "a liberal was just a human neutron in the political process," somebody who ineffectively wanted a change that was no change—not to mention White's opinion, of which this was hardly the only expression, that individual action accounted for naught in an all-determining culture. So what good could we do, beyond instantiating cultural-historical forces that were bigger than any of us? Well, one thing was to invent teach-ins as a protest form, which was done at the University of Michigan in March 1965, launching an antiwar movement of national and even international proportions out of a consternation of local circumstances and personalities which itself could not be determined from the larger structures it thus affected. (Sahlins 2000:23)

Although White never articulated why he would not be part of any "teach-in," the reason he refused to get involved with the anti-war struggle was, in part, his overwhelming pessimism about the future of mankind. In a letter written after the shooting of Robert Kennedy in 1968, White wrote to David Aberle about his views and why he did not become politically active:

> I am able to take (or, I *do* take) a more detached view of the World and its senseless, cruel and inhuman behavior than you can. Perhaps my own personal troubles have loomed so large in my life that I have not had much room for the "world's" conduct. But, I believe, the scientific—specifically, the culturological—point of view that I have tried for so long to realize and make explicit (mostly in my own way of thinking, however, rather than in writing, I am sorry to say), makes for an objective, and hence detached, point of view. "To comprehend is to forgive" is as an old saying (of French provenience, I believe). But, from the standpoint of scientific, culturological, comprehension, "forgiveness" is irrelevant. I hate to see nations behave as they do. I would hate to see the northern hemisphere largely covered once again by a huge ice-sheet. My feelings about Vietnam, e.g., are more immediate and *do* involve my emotions much more than the contemplation of the prospect of another ice age. But the intellectual attitude is much the same. The scientist, *as* scientist,

> must be a spectator, must he not? To be sure, scientists *are* human beings, also, and it is not right and good that their voices should cry out in protest against "crimes against humanity" (for which those at Nuremberg were tried). Kroeber used to argue that anthropology was more scientific than sociology because the former kept its emotions out of their science. I have tried to do this, also; emotions tend to contaminate science, and culturology has enough obstacles as it is. (letter to Aberle, January 4, 1968)

It should be noted here that White did not respect the Students for Democratic Society (SDS). He firmly believed they were ineffectual and misguided. Carmen Lodise, a graduate student in economics who took classes with White at Rice University, urged White to read Jerry Rubin's *Do It!* Lodise characterized Rubin's book as one of the most important books about politics since Marx and Engels first collaborated, and that Rubin did "a beautiful job of documenting the ideological revolution of our times and demonstrating that young people today are the communists Marx told us would emerge" (letter to White, August 19, 1971). White could not help but be aware many graduate students were enamored with Rubin and actively involved in the protest movement against the war in Vietnam. However, White failed to see why people were drawn to Rubin and the SDS, writing to Lodise,

> [T]here must be something in it I cannot see. To be sure there are thousands—perhaps tens of thousands—of young men and women in the U.S. today who think of themselves as revolutionists, as "enemies of the state," etc. But, (1) they are not organized and there is no sign of organization in progress that I can see; and (2) the masses of American people, the "working class" that the Old Marxists talked about, the men and women in the factories, mines, mills, railroads, clerks, etc., etc., show no signs of revolutionary inclinations that I can discern on the present American landscape. As a matter of fact, many of them are probably more opposed to the revolutionary youth of today than is the Capitalist class. (letter to Lodise, September 24, 1971)

In White's estimation, college protesters often came from affluent or middle-class families and believed the protest movement had less to do with political unrest than the rejection of middle-class norms with regard to the family, religion, and sexual mores. Of these variables, White felt that the invention of safe, inexpensive, and highly effective birth control had more to do with social unrest than politics. Those that railed against

the status quo, the "militants" who wanted to tear down the state had nothing to say about what would replace it. Without a plan for the future White firmly believed the protest movement was "little more than gestures, postures and slogans." He wrote to Lodise that revolutionists "must, to succeed, acquire control of the basic processes of society, and be prepared to construct, or reconstruct, the socio-economic-political structure *in accordance with a well thought-out and realistic plan*" (letter to Lodise, September 24, 1971). Against this background, White characterized Jerry Rubin as being little more than a clown in comparison to the revolutionary figures he was drawn to in his youth.

Despite the fact White did not participate in the anti-war movement or teach-ins he realized that the social and economic structure of American society had undergone, and was continuing to undergo, profound change. While a revolution had not taken place as he envisioned in the 1930s, American society transformed itself into something else via the process of cultural evolution. American society as White perceived it circa 1970 was a highly integrated system with the working class and their respective unions dominated by a capitalistic spirit. With the government firmly in control of the economic life of its citizens and the standard of living significantly higher than at any other time in history the chance of revolution was greatly diminished. However, this did not mean that a revolution could not take place in the future. White believed that the revolutionary character of American society was rapidly changing and that "everyone realizes we are living in a revolutionary era, our society has become unglued, things are falling apart, the solid rocks of yesteryear are now pulverized into fragments and chips, especially the current social organization of the family, marriage, sex etc." (White 1974).

The next great technological revolution that would occur, one that would lead to great social change, was the information age. But this revolution would not create positive change. White concluded that culture was indifferent to the needs of individuals, and that cultural progress brought measures good and bad. White's views were deeply pessimistic but not radically different from what he had written in 1949. For example, in the *Science of Culture* he wrote about the ominous dangers of nuclear energy and its fearsome military purposes as being a technological revolution:

> Here again the significance of this new factor derives from the fact that a new source of energy has been harnessed in awful form. Once more we are upon the threshold of a technological revolution. But the consequences of this new technological

> advance may possibly differ radically from those of the Agricultural and Fuel revolutions. New technologies in the past have rendered old systems obsolete but they have replaced them with new systems. The new nuclear technology however threatens to destroy civilization itself, or at least cripple it to such an extent that it might require a century, a thousand, or ten thousand, years to regain its present status. At least this is what eminent scientists and military men tell us. (White 1949:389)

Compare the quotation above with the following:

> I have come to the conclusion, and I think it is a solid one, that the function of culture is not to be the handmaiden of man, to feed him and clothe him and entertain him. We live in a cultural system that is indifferent our welfare. All you have to do is look at the history of civilization and see what culture has done to man, the agricultural revolution reduced the majority of the population to slavery serfdom and propertyless proletarians. It has brought endless and huge wars to slaughter people and incompetents. It is through medical technology that has increased the population of the world greatly so that the overpopulation for many is a greater threat than the hydrogen bomb. All the mechanisms are polluting the air, polluting the ocean, polluting the earth we live on. Progress can be a very terrible thing and the growth of technology may very well do us in. (White 1974)

At end of his life White was convinced that there was a good chance humanity would destroy itself in a nuclear war. While his pessimism ran deep, it was in keeping with what he stated earlier in his career. White considered the ability of the soul and the state to survive by which he meant "as a class of animals to which dinosaurs belong characterized by huge bodies and peanut brains which is like the cultural system we have in the United States." In White's estimation,

> There is no better cultural system found anywhere in the world. Yet these nations are incapable of behaving in an intelligent manner, they cannot sense danger at a distance as some forms of animals can and they cannot cope with a crisis when it arises. Our nation could fall apart from its own weight and if it does not there is pollution and overpopulation as threats. But the greatest threat of all in my mind is the threat of thermonuclear warfare. As long as we have national sovereignty, the threat of nuclear war will hang over us. As soon as we get enough deterrents we

> will start another war. Nothing makes war break out quicker and faster and harder than a bunch of deterrents. This is what the United States and the Soviet Union, the two great superpowers, are doing now—they are getting ready for thermonuclear war. Can you imagine any statesman anywhere in the country be he dictator or elected person who is willing to publicly say yes we will surrender our national sovereignty and merge our nations with others? Imagine Barry Goldwater doing this just to give you an example. National sovereignty is the greatest non-technological threat to the existence of the human race that we have. Personally I think the chances of survival are less than fifty-fifty but maybe I am off a few percentage points. Well these are rather discouraging words but it is not science's business to comfort people. (White 1974)

Without a thorough understanding of White's life and writing it is easy to perceive statements such as the one above and texts such as *The Concept of Cultural Systems* (1975) as turning back on his earlier formulations about the nature of human culture and evolution. This text in particular—published at the initiative of Beth Dillingham, one of White's literary executors—is misleading in that its focus on cultural systems was not White's primary area of theoretical interest in his final years. Rather, written at the urging of friend and colleague Raymond Wilder, White wrote the book as a break from his work on his mammoth unpublished "Modern Capitalist Culture," the sequel to the *Evolution of Culture*. From 1968 until his death in 1975 he spent the vast majority of his time working independently on this manuscript, an endeavor he likened to entering the La Brea tar pits—he quipped that, like the dinosaurs, once he put his foot in he was never able to re-emerge.[8]

Conclusion

White's writings make it clear that war was a "tremendously impressive expression of human behavior" (White 1949:129). War was also an integral part of capitalism—part of the struggle to survive—the "struggle for the possession and use of the resources of the earth, for fertile fields; coal, oil, and iron deposits; for uranium mines; for seaports and waterways; for markets and trade routes; for military bases" (White 1949:343). War, White wrote, had nothing to do with the individuals who actually waged war but with the social organisms—called nations—that created and encouraged them. Nothing good could come from warfare and this belief led White to remain opposed to all wars—even so-called "just" wars

such as World War II. White's opposition to war also led him to severely chastise his fellow anthropologists. White believed the attempts of his peers to explain war were woefully inadequate and he singled out Ralph Linton, Ruth Benedict, and her mentor, Franz Boas, for sharp criticism (White 1947). White's views of war were concise:

> War is a cultural phenomenon, and we can not only explain it in cultural terms, but we can account for the presence or absence of the pugnacious "instinct," the love of glory, or the loathing of slaughter, in cultural terms also. World peace will come, if it ever does, not because we have bred out the pugnacious instinct, or sublimated it in mass athletic contests, but because cultural development, social evolution, will have reached the ultimate conclusion of the age-old process of merging smaller social groups into larger ones, eventually forming a single political organization that will embrace the entire planet and the whole human race. (White 1949:133)

During White's life the biggest threat to the future of humankind was nuclear warfare. Today the threat of a nuclear war has been replaced by the threat of terrorism unleashed by the events of September 11, 2001. When reading White's work on war I often wonder what he would have thought of not only the events of that day but the consequent actions of the Bush administration. I am convinced he would be appalled by the Bush presidency and the way modern warfare is waged. The horrors of war are far from view and sanitized. Journalists are "embedded" and their access to events is carefully controlled by the Army. Too often American media outlets extol the virtues of smart bombs without consideration of the death and destruction they reap. The sound bites that result from such reports are devoid of depth and thoughtful analysis. I am convinced that White's views about war remain relevant; especially when one considers his premise that war is fought between socio-cultural systems—a fact George W. Bush has made clear via multiple proclamations that all nations are either allies of the United States or enemies and enablers of terrorism. Such a belief may appease those who are afraid of terrorism but do nothing to prevent or understand events such as those of September 11, 2001. In such a polarized environment it is worth noting that White wrote "it is the culture of any given situation that determines whether warfare shall be engaged in or not, and if so how, with whom and for what" (White 1949:343). These words are worthy of thought given the fact President Bush has declared war on terrorism—an effort that may plunge us into another Cold War, one that may last for decades.

Notes

Letters and journals cited are among the Leslie A. White Papers at the Bentley Historical Library, University of Michigan, Ann Arbor.

1. White's parents were divorced when he was five years old. His mother fell in love with another man and moved to Denver. As was the custom of the of the time in cases of adultery or abandonment, White's father was awarded custody of Leslie White and his siblings, Willard and Helen White (Peace 2004:1–34).

2. Another variable may have been the fact that the Navy had a highly visible presence at Columbia University. See Boardman 1944.

3. The U.S.S. *Ringgold* was one of fifty DD class destroyers constructed at the behest of Woodrow Wilson. The *Ringgold* was decommissioned in 1922. Two decades later it was refurbished and ownership was transferred to Britain and renamed *Newark*. The ship was scrapped in 1945.

4. The archival holdings regarding the Come Back Club at Columbia are slim. The Columbia University archives contain letters from the Federal Board to University officials, yearbooks, and one copy of the *Come Backer* newsletter. However, Columbia does have a significant collection of material about student activism from 1903 to 1959. This collection is appended to the Protest and Activism Collection. The collection is idiosyncratic material gathered by Robert Arrowsmith, the Columbiana Collection's first curator, and Milton Halsey Thomas, who was Librarian of Butler Library from 1926 to 1928, then curator of the Columbiana Collection until 1959. Although hardly a complete file of radical activity, the collection contains many fascinating articles, public notices, and leaflets about anti-war protests and the activities of various socialist and communist groups active on campus.

5. Unfortunately no record of what White said has been preserved.

6. White's radical change may be tied to a "nervous breakdown." In a blacked-out portion of a journal that White kept while a student, he wrote, "A long time has elapsed since I have written anything in here and much has happened, events which may change the whole course of my life. In a few words, I am recuperating from a hysterical nervous breakdown" (June 30, 1922). Unfortunately only this short passage is legible.

7. Harry Elmer Barnes was White's closest friend and a controversial revisionist historian. Barnes was unusual in that he publicly chastised Roosevelt and was highly critical of the war. Barnes's scholarship is subject to intense debate because his work has been embraced by those who deny the Holocaust occurred. Barnes's papers are voluminous and are located at the University of Wyoming American Heritage Center. There Barnes's son Robert has deposited his father's FBI file, which he went to great effort to obtain. Although unorganized and in poor condition, the file is extensive—the FBI considered him a dangerous subversive during World War II because of his critical remarks about President Roosevelt and the war.

8. The majority of White's later work is unpublished and voluminous. Among White's papers are several long manuscripts, some well over twenty-five hundred pages. The two largest and most important manuscripts in White's estimation were "The Fuel Revolution" and "Modern Capitalist Culture." White was convinced the whole history of European cultural evolution needed to be rewritten and he wanted to create a new culturological account of the rise of capitalism in Europe. Hopelessly ambitious, White was unable to complete this Herculean task. Over the years several publishers have expressed interest in publishing an abridged version of "Modern Capitalist Culture." This will come to fruition in 2007 through Left Coast Press. Another unpublished text worth mentioning here is "The World

Tomorrow." This manuscript is over one thousand pages long and strongly reflects the influence of Veblen on White's perception of evolutionary development.

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2. *Significant Form*

Sapir's Phonemic Poetics

Richard Handler

> Rhythm *must* have a meaning.—Ezra Pound (1916:322)

Anthropologists often assume that culture theory and the history of anthropology occupy mutually exclusive spaces in our field.[1] We might even construe them as poles of a continuum running from pure empirical work at one end (history of anthropology, along with certain types of ethnography) to pure theory building at the other. Those who hold a less dichotomous perspective might blur such boundaries by pointing out that the historical contextualization of theory sometimes facilitates a creative reengagement with "old" theory—that is, the history of anthropology may occasionally lead to theoretical "progress." The work of George Stocking (1968, 1974), Regna Darnell (1998, 2001), and their students suggests a case in point: all those "symbolic" or "interpretive" anthropologists, trained in the last quarter of the twentieth century, found it congenial to trace their intellectual genealogy back to Franz Boas, despite the fact that their more proximate intellectual ancestors are more likely to have been such people as Claude Lévi-Strauss, David Schneider, Clifford Geertz, or Victor Turner (cf. Handler 1997, 1998).

But there is an even more robust reading of the relationship between history making and theory making, to be explored in the present paper. What if it is more useful to conceive of the "life" of theory in the social sciences in terms of cyclical repetition, not progressive development? In other words, what if particular theoretical positions wax, wane, and wax again—more like "three centuries of women's [and, presumably, men's] dress fashions" (Kroeber and Richardson 1940) than "industrial takeoff" or modernization itself? In Kroeber and Richardson's paradigmatic example, features of women's fashions such as skirt length and width varied regularly, over about a 150–year period, between culturally specific minima and maxima. This might be compared to specific theoretical emphases in the history of anthropology—antagonistic "paradigmatic tra-

ditions" or enduring antimonies (Stocking 1989, 1990) that wax, wane, and wax, in competition with one another, over the decades and even centuries. From this latter perspective, the history of anthropological theory is less a story of nonlinear development than of recurring salience and irrelevance. You can be sure, in other words, that today's hot theoretical discourse was hot at an earlier moment, and eclipsed at an earlier moment as well (on the culture concept itself, see Brightman 1995).

Perhaps the most basic of all theoretical terms in the social sciences that cycle in and out of fashion, in relation to one another, are "individual," "society" (or "culture"), and "action" (or "process"). There are theoreticians who see the individual as the primary locus of reality in human life, and, correspondingly, society (or culture) as a secondary or even residual phenomenon. There are others who reverse the emphasis, seeing individual existence as a function of social or cultural formations. For both, action links the two, such that in the first perspective, individuals create social forms through their more or less autonomously motivated actions; in the second, action is first of all a collective (social or cultural) phenomenon that can be said to realize itself through the lives of human individuals. In the history of social-scientific theory, the permutations provided by this basic set of elements are legion. We can imagine theoretical fashion swinging back and forth between individualistic and collectivist orientations. Another way to imagine this history is to track the moments when "action," or "process," is offered as the key to resolving the antinomy between society and the individual.

The present essay focuses on one such moment in the history of American anthropology. The moment is the late 'teens and early twenties of the last century, a time when Boas and his students were grappling with "culture," specifically with cultures as unified patterns or totalities emergent from historical processes of diffusion and psychological processes of invention or creativity. As the Boasians were learning to work with this version of the culture concept, the relationship between culture and the individual came to the fore. And in thinking about that relationship, many of these anthropologists drew on the discourse of aesthetic modernism, specifically literary modernism, in which the question of "tradition and the individual talent," to use T. S. Eliot's phrase (1960), had assumed great importance. In American anthropology the issue was famously joined in 1917 when Alfred Kroeber (1917) and Edward Sapir (1917a) argued over the relative importance of the individual in history. As is well known, Sapir went on to elaborate a theory of culture focused on the dialectic of creative personalities and inherited forms. He developed this approach both in his linguistics (1921a, 1925, 1933) and in his

late papers on culture and personality (e.g., 1932, 1934, 1938). Outside anthropology proper, he broached the issue in his literary criticism and poetry, work that has by now been much discussed.[2]

To explicate the correspondences between the literary sensibility of the early twentieth century and Sapir's aesthetics of language and culture, there is no better place to start than Sapir's remarkable essay on "The Musical Foundations of Verse," published in the *Journal of English and Germanic Philology* in 1921. We may consider this to be a major paper of Sapir's oeuvre, but one that has not received the attention it merits. It is based on Sapir's understanding of the dynamics of phonemic patterning, sketched in his 1921 book *Language*, and elaborated in essays of 1925 and 1933; interestingly, in this essay, Sapir's approach to "sound patterns" is worked out in detail *as a theory of poetic form*.

The Controversy over Free Verse

Beginning with Baudelaire and the French symbolists, and continuing with the works of Pound and Eliot, much modern European and American poetry has been self-consciously concerned with language itself as the subject matter of poetry. "What distinguishes the Symbolism of our day from the Symbolism of the past," wrote modernist critic Arthur Symons (1919:3), "is that it has now become conscious of itself"—conscious of itself, we can add, as language and, moreover, conscious of the potential of language for self-contained patterning. The modernist poets Symons had in mind came to understand that language does not mirror the world, but molds or (as anthropologists might say) constructs it. The modernist interest in philology and translation are typical in this regard: both disciplines focused the poet's attention on the historical relativity of words and languages, and on "Language" as such (Kenner 1971:41–172). Thus Eliot defined translation as "a criticism of one language by another" (1916:102) and Pound, writing in the same volume of *Poetry*, stressed the importance of multilingualism: "The man who reads only one language is, intellectually, only half a man in comparison with the man of equal mental energy who can read two with comfort" (1917:312). Translation, multilingualism, philology, linguistics—together they "permitted Pound's generation the vision of languages as inter-textured, cognate systems of apprehension, to each its special *virtu*" (Kenner 1971:120).

To understand language as a historically given yet relatively self-contained and coherent system contradicted the moral and epistemological presuppositions of earlier European (particularly eighteenth-century) theories of literature, with their concern for classical, traditional forms

of thought to approximate the a priori orderliness of nature. In the literary magazines Sapir read, this conflict of aesthetic sensibilities was fought out in the controversy over free verse—that "ideally arguable instance of what was felt as the universal flouting of tradition and order" (Williams 1977:50). For example, shortly after *Poetry* began publication in October 1912, Wallace Rice, a critic and anthologist, attacked the magazine in the pages of the *Dial* for the "lack of standards" that led it to champion free verse (1913:370). Rice defended the "technic" of poetry as "formal rhythm" or "the metrical arrangement of words to express beauty." According to him, free verse was "formless" and without technic, hence doomed to disappear, since, as he put it, "nothing atechnical has survived in any Art" (371). Harriet Monroe, founder and editor of *Poetry*, replied by attacking Rice's "narrow conception of poetic technic," which, she claimed, was too limited to permit him to discern the formal aspects of non-traditional verse (1913a:409).

Whether Sapir followed this dispute in the *Dial* is unknown, but in 1917 he published an article in response to Max Eastman's campaign against "Lazy Verse."[3] Eastman had argued that free verse served all too often as a refuge for would-be poets without the energy or intensity to master formal technique. Yet, without such mastery, Eastman thought, true art was unattainable: "to produce anything which will compel attention . . . it is necessary to concentrate attention upon the making of something, more even than upon the passion out of which it is made; and that is one thing that the forms of rhythmic poetry compel us to do" (1916:140). Alice Corbin Henderson, an associate editor of *Poetry*, responded that free verse *was* formal, or (in the terms that Rice and Monroe had used) was possessed of its own technique: "Mr. Eastman no doubt would deny the word 'form' to *vers libre*. But *vers libre* has form exactly as clouds have form, and as infinite a variety of patterns, although none may be regular or narrowly symmetrical." And Henderson argued that "real poetry," the only proper object for critical discussion, could be attained in "either medium," free or metrical verse (1916:149).

Sapir's response to Eastman differed from Monroe's and Henderson's defense of free verse. They had simply asserted that free verse possessed form, but Sapir, characteristically, used the free verse controversy to discuss the relationship between formal constraint and the process of artistic expression. He approved Eastman's championing of formal discipline, but argued that Eastman erred by sanctioning "rigid metrical forms and even rhyming schemes" rather than the necessity of formal constraint in whatever cultural guise it came: "perfection of form is always essential, but the definition of what constitutes such perfection cannot, must not,

be fixed once for all. The age, the individual artist, must solve the problem ever anew . . . of the battle to be fought in attaining self-expression" (1917b:100).

The defense of free verse required more than the assertion that it possessed form—more, even, than Sapir's discussion of the cultural relativity of artistic forms. Passing from a defensive posture to the positive rhetoric of scientific theory, advocates of free verse attempted to specify the formal elements that characterized it as poetry. Two aspects of their efforts deserve attention as points of comparison to Sapir's analysis: first, their positivism, that is, their belief that they dealt with objective, isolable formal units that could be measured scientifically; and, second, their penchant for constructing typologies of literary genres based on the different formal elements postulated in theory. The problem, it should be recalled, was to show that free verse was indeed poetry, or, more generally, that it was an art with distinctive formal properties. To prove such contentions was to show that free verse differed as much from prose (with which its detractors equated it) as from metrical verse.

The first step of the free verse theorists was to discuss poetic form in terms of rhythm rather than the accentual units of traditional prosody. F. S. Flint's well-known statement of the principles of Imagism, first published in *Poetry*, advised, "as regarding rhythm: to compose in the sequence of the musical phrase, not in sequence of a metronome" (1913:199; cf. Pound 1945:3). Shortly after Flint's statement, Monroe published a two-part editorial arguing that English verse is "quantitative" rather than "accentual": "there is no longer any excuse for persistence in the old error. Rhythm is rhythm, and its laws are unchangeable, in poetry, in music, in the motion of tides and stars, in the vibration of sound-waves, light-waves, or the still more minute waves of molecular action. Always and everywhere rhythm is measured movement, a regular succession of time-intervals. English verse is as quantitative as Greek verse" (1913b:61–62).

She went on to translate the technical vocabulary of accentual prosody into musical notation, claiming to show the inadequacy of the traditional terms ("iambic," "trochaic," etc.) to render rhythmic distinctions accurately. As she argued in a later editorial, "musical notation is . . . a scientific analysis" (1918:30), and Monroe felt sure that despite minor individual variations, readers would generally agree with her musically notated renderings of the rhythms of Shakespeare, Wordsworth, Coleridge, and others (1913b:68). In general, Monroe placed great faith in the power of science to reveal the "truth" (1918:31) of poetic form: "more scientific knowledge of this subject is necessary . . . to remove

English poetry from the rack of 'accentual' prosody, and restore it to the great universal laws of rhythm" (1913b:111).

With analysis thus focused on rhythm rather than accentual feet, free verse theorists could construct typologies of literary genres based on the distinctive rhythmic patterns they attributed to each genre. Most theorists (e.g., Saintsbury 1912:341–346; Lowell 1914:213) posited a continuum—with prose at one end, metrical verse at the other—which they subdivided into a number of types (most commonly, four: prose, "rhythmical prose," free verse, metrical verse). Such a solution contained a fundamental (though unrecognized) ambiguity, a tension between the notions of continuum and genre. Baldly stated, what was the status of "a" genre—by definition, distinctive—if genres existed only as sections of a continuum that faded imperceptibly into one another? Henderson thought prose differed in some absolute sense from metrical verse: "prose rhythms differ from poetic rhythms according to the inherent, scientific divisions of the rhythmic wave lengths" (1913:71). But Henderson's editorial colleague, Monroe, thought otherwise: "it has never been possible . . . to draw an absolute line between poetry and prose, however sharply grammarians, rhetoricians and other rule-makers have tried to do so" (1918:98).

Whatever the facts concerning the poles of the continuum, the immediate problem facing free-verse theorists was to distinguish free verse from prose. Again, they posited a continuum but nonetheless argued that free verse was a distinctive genre with discernible formal properties that all sensitive listeners would discover. Thus Amy Lowell wrote an essay "to establish a division in the spectrum of word-values, and to show how the extreme of prose at one end changes to the extreme of poetry at the other, through the grades of metrical prose, and *vers libre*" (1914:213). Lowell concluded her attempt with an appeal to the sensitive reader: "[A]nyone who takes the trouble to read these quotations aloud and listen attentively will instantly feel the difference between them, and detect the subtle and delicate gradations by which they fade into poetry at one end and prose at the other" (220).

All attempts to subdivide the continuum of language-rhythm were based on the idea that the rhythms of metrical verse, at one pole, were more regular or more marked (however regularity or markedness was defined), hence more easily discernible, than the irregular, hence subtle and elusive, rhythms of prose. Lowell, for example, relabeled Henderson's wave lengths as "curves" and argued somewhat vaguely that "the rhythm of prose is long and slightly curved, the rhythm of verse very much shorter, with a tendency to return back upon itself" (215).

And William Patterson, a professor of English whose "scientific" analyses of language rhythms impressed both Lowell (1918:51–52) and Monroe (1918), contrasted the "unitary pulses or "coincidence" of verse to the "syncopation" of prose: "the opposite of coincidence is syncopation. If sounds do not hit together they are bound to hit apart" (Patterson 1918:259). In "the forbidding confines of the laboratory," Patterson worked closely with Lowell, whom he called an "aggressively rhythmic timer" (258, 260), but again, in these essentially positivistic attempts to demarcate genres, the listener's ability to discern distinctive features was always linked, at least implicitly, to scientifically measurable formal units thought to inhere in the genres themselves. And this was so despite continual special appeals to the "rhythmically sensitive" individual. Indeed, such appeals point to the shaky epistemology of the entire project.

Phonemic Poetics

"The Musical Foundations of Verse" presents itself as a commentary on Lowell's 1918 paper "The Rhythms of Free Verse." Sapir's paper was written sometime during 1918 or early 1919, though not published until 1921.[4] In 1919, he published a brief, unsigned review of *The Foundations and Nature of Verse* by Cary Jacob. Sapir's review is almost a synopsis of the 1921 article. According to Sapir, Jacob failed to resolve the key "problems of prosody" raised by free verse: first, "the old problem of the essential basis or bases of English verse" and, second, "the relation in point of rhythm between prose and verse" (1919:98). "The Musical Foundations of Verse" contains Sapir's "phonemic" solution to those problems. He begins by confirming an idea Lowell had discussed in her 1918 paper, in which she suggested that free verse was patterned in terms of a temporal or rhythmic, rather than accentual, unit. "For years," Lowell had written (1918:54), "I had been searching the unit of *vers libre*, the ultimate particle to which the rhythm of this form could be reduced. As the 'foot' is the unit of 'regular verse,' so there must be a unit in *vers libre*. I thought I had found it. The unit was a measurement of time."

Sapir (1921b:213) agrees with Lowell but goes far beyond her; in place of her atomistic epistemology, that is, her search for "the ultimate particle," Sapir substitutes a phonemic theory of aesthetic patterning. That theory can be conveniently analyzed in terms of two arguments and their corollaries. First, in Sapir's poetics, "units," or, better, poetically significant formal devices, can be defined only in terms of their systematic opposition within the structure of a given poem (and, implicitly, their contrast to the formal techniques of other poems within a literary tradition).

As a corollary Sapir points out that this principle of opposition will allow for many more types of verse than have been recognized by conventional critics, trained as they have been to concentrate on such features as accent and rhyme to the exclusion of other potentially poetic features of language. This in turn suggests that the potentially poetic language that seems "free" or "irregular" to those insensitive to the structural patterning upon which it depends may seem quite regular to those who know how to listen to it. The first principle and its corollaries lead Sapir to argue, second, that the dividing line between prose and verse depends upon the poetic receptivity of the listener. In other words, his is a theory of verse in which the acculturated perceptions of the listener play a crucial role in constituting the poetic object. As corollaries to this second argument, Sapir suggests first that objective measurements of poetic rhythm are not sufficient to prove the existence of poetry, and second that people's acquired receptivity to the visual presentation of poetry in print will play a decisive role in verse appreciation in a literate, as opposed to an oral, tradition.

Sapir begins his presentation with his own explication of Lowell's "measurement of time." He contrasts the "orthodox scansion" of free verse (that is, an analysis in terms of accentual feet) with an analysis that captures what he calls "the really significant form units." Sapir gives this example of "free verse" (culturally salient in the aftermath of World War I):

March!
Right face!
Right about face!
Halt!

Here we find no regularity with respect to the pattern of stressed and unstressed syllables that combine to form metrical feet, nor with respect to the number of feet per line. But since each line is recited in the same amount of time, there results, according to Sapir, "a perfectly humdrum and regular type of rhythmic movement." He finds similar rhythmic patterning "in much free verse [where] relatively long lines or sections are meant . . . to have the same time value as short lines or sections of the same stanza." He suggests that the "beautiful, quasi-musical effect" this technique can produce depends on the deliberately established contrast between this regularity of "time-units," on the one hand, and the lack of conventional metrical regularity, on the other (214–216).

Lowell (1918:55) and Patterson (1918:261) called this technique "unitary verse," which Patterson defined as "language whose chief accents

mark off impressions of equal time-intervals, regardless of the number of syllables in between." But Sapir is not happy with their term, probably because it obscures what he is most concerned to analyze: the generation of significance (in this case, of appreciable rhythmic patterning) out of the systematic opposition of formal units, in this case, regular time-units foregrounded against irregular stress units (which, as his analysis implies, are indexed as "poetry" by means of their visual presentation in print as lines). Thus in contrast to Lowell and Patterson, Sapir's discussion of the technique of free verse brings out the salient formal opposition: "the unification by means of time units of . . . irregular stress groupings" (1921b:216).

Sapir next discusses the "obverse" of "unitary verse," which he calls "time-disturbed metrical verse": "In ordinary metrical verse the stress unit or foot tends to have a unitary time value as well. The prolonged coincidence of stress units and time units, however, leads often to an unpleasantly monotonous effect. To avoid this, as is well known, retardations and accelerations of speed are introduced that give the movement of the verse greater fluidity or swing" (216; cf. Lowell 1918:53–55).

Sapir argues that the lack of "coincidence of time and stress units" (216), that is, their systematic contrast, generates the crucial poetic effect, and he gives as an example these lines from Walter de la Mare's poem "The Barber's":

> Straight above the clear eyes,
> Rounded round the ears,
> Snip-snap and snick-a-snick,
> Clash the barber's shears.
> Us, in the looking glass,
> Footsteps in the street (etc.)

Sapir claims that in the fifth line (Us, in the looking glass), the foot "Us" is the temporal equivalent of the three feet that follow it. He concludes,

> These lines of De la Mare's are a good example of the cross-rhythmic effect sometimes produced in English verse by the clash of stress units and time units. They differ psychologically from true "unitary verse" in that the metrical pattern established for the ear by the rest of the poem peeps silently through, as it were. This silent metrical base is an important point to bear in mind in the analysis of much English verse. The various types of dimly, but none the less effectively, felt rhythmic conflicts that result have not a little to do with the more baffling subtleties of verse movement. (217)

Here we see clearly how Sapir's configurationalist or phonemic approach differs from the analysis of Lowell and Patterson, who sought to isolate poetic genres in terms of the scientifically measurable units or types of regularity distinctive to each. Thus Lowell (1918:55), quoting from Patterson's *The Rhythm of Prose*, defined "swing" in terms of mathematically regular acceleration and retardation. But for Sapir, "swing" is a question of the deliberate contrast ("the metrical pattern . . . peeps silently through") between such changes of tempo and a potentially "monotonous" regularity of stress. In other words, Sapir treated genres such as metrical verse, unitary verse, and time-disturbed metrical verse not as isolated natural kinds, but as transformations of, and commentaries upon, one another.

Sapir next outlines a theory of what he calls poetic sectioning, which he defines as "a division into appreciable psychological pulses" (1921b:220–221). Sapir shows that poetry depends upon the systematic interrelation of such elements as stress, time, number of syllables, division into stanzas, rhyme and internal rhyme, alliteration, assonance, repetition, pauses, and "the rising and falling (also strengthening and weakening) of the voice" (218). His analysis suggests that none of these elements can be isolated as the kind of ultimate particle Lowell sought, for any one of them only works, or becomes poetically significant, when contrasted to other patterns, or foregrounded against poetically unmarked language.

These "unsystematic observations on the structure of verse" (221) lead to two corollaries. The first stems from the anthropologist's understanding of the potential diversity of cultural patterning. We can imagine many other poetically "appreciable" combinations of linguistic elements than those conventionally utilized in Western literature—or, as Sapir puts it, "the possible types of verse are very numerous—more so than assumed even by the *vers libristes*" (221). Second, verse that seems free or irregular from the point of view of those accustomed to one genre may seem routinely regular to those who can appreciate its rhythmic patterning. In Sapir's example time-disturbed metrical verse, despite its regularity with respect to accentual feet, is not necessarily more regular "to all ears" than unitary free verse: "much depends on the sensitiveness of the reader or hearer to the apperception of time pulses" (216).

Sapir's concern for readers' sensitiveness, and for rhythmic patterning that is psychologically "appreciable" though not universally appreciated, allows him to offer a unique solution to the problem of the dividing line between prose and poetry. Echoing Boas's seminal argument on "alternating sounds" (1889), Sapir tells us that poetry, in contrast to prose, "is *rhythmically self-conscious* speech or discourse" (1921b:224). Every-

thing depends on the ability and training of the poet and of his or her interlocutor, on their "rhythmic discipline" (222) or "rhythmic self-consciousness" (223), as he terms it:

> Of two passages that are perfectly homologous in rhythmical respects, so long as a merely formal analysis is made of their stresses, time phrases, and syllables, one may be verse because the rhythmic coutour is easily apperceived as such, demands some share of the reader's or hearer's attention, the other prose because . . . the same rhythmic contour, while necessarily making a vague impress on the fringe of consciousness, has not succeeded in clearly obtruding itself on the attention. In the former case the rhythmic construction of the passage is present, as an analyzable factor, both phonetically and aesthetically; in the latter, phonetically but not aesthetically. As far as art is concerned, rhythm simply does not exist in the latter case. (223–224)

As he puts it a few paragraphs later, "the same passage *is* both prose and verse according to the rhythmic receptivity of the reader or hearer" (226).

Other free verse theorists had acknowledged the importance of the sensitive reader, but, committed to a positivistic approach, they called on such a reader to prove that their theoretical typologies of genres corresponded to the scientific facts: the sensitive reader heard what was "really" measurably there. Thus, as we saw, Monroe was sure that most readers would agree with her musical notations of poetic rhythm precisely because she was convinced those notations were scientifically accurate (1913:68). And Lowell justified her typology of genres by an appeal to attentive listeners who, she claimed, would "instantly feel the differences" that constituted metrical prose and *vers libre* as "two steps between pure prose and pure poetry" (1914:220, 213). Contrast such arguments to Sapir's formulation, in which the very existence of a literary genre, during any instance of reading or auditing, depends on people's receptivity to it. In other words, poetry (or any other genre) does not exist as an objective reality apart from those who communicate with it, nor can the analyst prove its independent existence by measuring the formal properties upon which it might be based. Poetry exists when or while auditors apprehend it as such: "poetry does not exist in its symbolic visual form; like music, it addresses itself solely to the inner ear" (1921b:226).

From this "phonemic" understanding of the epistemology of poetic experience, it follows that purely mechanical techniques can never suffice to measure poetic form. Thus Sapir (thinking, perhaps, of Patterson's ex-

periments with Lowell) points out "the necessary limitation of machine methods in the investigation of prosodic problems." "Introspective analysis," as he puts it, will always be needed to supplement "rigorously objective methods" (224). Such language recalls the famous closing paragraph of his 1925 essay "Sound Patterns in Language," in which Sapir questioned "the adequacy of purely objective methods of studying speech sounds" (1925:51). In both cases, objective measurement falls short because the phenomena under investigation—phonemic patterning or poetic form—cannot be separated from the ability of native speakers to respond to the "appreciable" configurational oppositions of language.

Finally, Sapir's phonemic approach to genre allows him to defuse the common argument that free verse is really prose printed as poetry. For example, Rice, in his diatribe against *Poetry*, printed a free-verse poem in prose form, to reveal the work for what it was, that is, a piece of prose. "Is this anything but prose?" Rice asked his readers, concluding that "whether a given literary composition is poetry or not, does not depend upon the manner in which the type is arranged on the printed page" (1913:370–371). The response of free verse advocates was simply to deny that Rice's trick could work. Thus Henderson, defending *Poetry* against Rice, introduced her free verse evidence with this claim: "any attempt to turn the following poem into prose by omitting the line divisions would prove unsuccessful" (1913:70). The disputants agreed in appealing both to their readers' good sense and to their sensitivity, thereby conflating the two: readers were expected to be able to recognize a thing for what it was, however it was presented (or disguised) typographically. Once again, in this positivistic perspective, the structural units that define a genre are thought to exist, independent of those who perceive them, in the very language of any examples of that genre. Readers who disagreed with such critical judgments merely displayed their own lack of sensitivity!

Sapir's emphasis on the role of the reader in verse appreciation leads him to a different tactic. He offers as evidence a sentence taken from a prose work, and asks the reader to concentrate on its rhythmic contour while ignoring its unpoetic content. He then substitutes poetic language in place of the original prose, while preserving the original rhythmic pattern, to bring out "the effect of verse latent in all prose" (1921b:225). The demonstration is intended to show not that a piece of prose is "really" verse, but that a poetically sensitive (or acculturated) reader will discover the potentially poetic rhythm in all language. Sapir concludes by arguing that the typographical conventions for printing poetry have become established as a symbolism in their own right; and, further, that this *visual* symbolism may be inadequate to convey the nuances of

auditory symbolism from which, he claims, it is derived. In other words, typographic conventions have taken on a cultural life of their own, capable of interacting in not altogether felicitous ways with poetic "impressions originally meant for the ear" (227)—hence the trick of printing free verse as prose works, not because free verse is prose, but because readers respond to an acquired secondary symbolism of typographical patterning.

The Individual in History, and in the History of Social Theory

I developed the preceding exegesis of "The Musical Foundations of Verse" not only to contextualize it historically, but to make it newly available for theoretical consideration today. Sapir's treatment of the relationship between acculturated (or continually re-acculturating) individuals, aesthetic conventions, and aesthetic innovations is as fine an answer as can be found in social theory to the question of the individual and society/culture. As other commentators (Preston 1966; Silverstein 2004) have pointed out, in Sapir's best analyses, we find a wonderful appreciation of the ways in which individuals are forever emergent within culture, and culture is at once conventional, or configured, and in process. In such an approach, "action" results not simply from a self-contained, completed, rational (or intentional) actor, nor are the effects of action simply "social change" or some other kind of observable alteration in the material or social world. In Sapir's approach, "the individual," "culture," and "action" are not isolable units in a mechanical feedback system. Rather, individuals' actions are at once culturally informed, part of culture, and culturally effective—as are individuals themselves.

In "The Musical Foundations of Verse" Sapir enacts this approach to culture theory by showing the way poets, readers/listeners, and poetic language mutually constitute one another. "The world," in the form of the poetic object, does not exist by itself, but is a thoroughly human, cultural world of significant forms and acculturating writers and readers/listeners. By contrast, other free verse theorists, like Henderson, Patterson, and Monroe, posited a world of completed individuals (either the aesthete of refined sensitivity or the scientist of objective sensibility) and completed poetic forms, with the objective existence of the latter inhering in the building blocks of poetry, rhythmic units. But it is useful to remember that Sapir himself sometimes had recourse to a romantic language concerning the poetic genius, or even the individual as a reified locus of cultural creativity (Murray 1986). If, as I argued at the outset, the set of terms that links the individual, society/culture, and action appears over and over again in the history of social theory, it should not sur-

prise us that any one thinker, such as Sapir, might experiment with different permutations. As historians of anthropology contextualize such experiments, or even contradictions, in the works of particular scholars, they have a chance not only to help their readers re-engage prior work in our field, but to score a few theoretical points of their own.

Notes

1. This paper was first read at the annual meeting of the American Anthropological Association, Denver, 1984. Further work was sponsored by the National Endowment for the Humanities. I thank Dell Hymes and Michael Silverstein for extensive critical commentary on my ideas concerning the connection between Sapir's linguistics and twentieth-century poetics. Hymes, in particular, cautioned me not to reduce all of American modernist poetry to the school of Eliot, Pound, and their critics.

2. Darnell 1990:151–188; Handler 1983, 1986; see also the growing body of biographical and critical scholarship on Ruth Benedict, from the "early" biographies of Modell (1983) and Caffrey (1989) to the more recent ones by Lapsley (1999) and Banner (2003); and work in English literature on the culture concept, e.g., Hegeman (1999), Manganaro (2002), Elliott (2002), and Evans (2005).

3. Sapir's letters to Lowie (Lowie 1965:17, 31) show that between 1916 and 1920 Sapir either subscribed to or regularly read the *Dial* and the *New Republic*. In a letter of January 30, 1919, to Monroe, Sapir enrolled himself as a subscriber to *Poetry*. "This journal . . . is not accessible to me in Ottawa," he wrote, but he mentioned "browsing through recent numbers" in a Boston public library (*Poetry* Magazine Papers, Regenstein Library, University of Chicago).

4. It could not have been written before January 1918, when Lowell's essay appeared. The argument, however, is already presented, though briefly, in an unsigned review (Sapir 1919). According to Sapir's personal records (in the possession of the Sapir family), his essay was initially accepted but not used by *Poetry*, then rejected by eleven journals before its acceptance.

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3. *William Fielding Ogburn's Fostering of Sol Tax's Explorations of Small-Scale Mercantile Capitalism in Highland Guatemala*

Stephen O. Murray

In 1929 a new anthropology department broke off from the preeminent sociology department in the United States, at the University of Chicago, two years after the most prominent cultural sociologist, William Fielding Ogburn, had joined that sociology program that included anthropology.[1] Ogburn, like Elsie Clews Parsons, had been a student of Franklin Giddings at Columbia University, earning his PhD degree in 1912. Early historians of American social theory Becker and Barnes noted that a

> result of the training provided by the Columbia [sociology] department was the close relationship it established with anthropology. Although Giddings himself made little use of the great advances in ethnographic and ethnological knowledge achieved by Boas, the greater number of Giddings' students and associates assimilated enough to start cultural sociology on its way. Ogburn, [Malcolm] Willey, [F. Stuart] Chapin, and many others [including Frank Hankins, Howard W. Odum, and Bernard J. Stern] arrived at the conviction that the products of man's hands and brain, i.e., material and non-material culture, are far more important than the climatic, topographic and biological factors once rated so highly. (1938:977–978)

Like Franz Boas, the Columbia-trained sociologists were engaged in improving measurement, statistical evaluation of hypotheses, and challenging residual social Darwinism and theories of racial destiny. In the pages of sociology journals they regularly called their colleagues' attention to anthropologists' findings from "primitive" societies (see Murray 1988a).

After completing his graduate work at Columbia Ogburn taught anthropology, economics, and sociology courses at Princeton, Reed, and the University of Washington and served on the National War Labor Board during World War I. In 1919 he succeeded Giddings as chair of

the Department of Economics and Sociology at Columbia University and Barnard College (where his most famous student—and, for a time, assistant—was Margaret Mead, to whom he introduced psychoanalytic concepts).

Though in no sense an ethnographer, Ogburn was part of the Boasian circle at Columbia both intellectually and socially.[2] Ogburn was especially close to Robert H. Lowie and Edward Sapir. After meeting Sapir in 1915 (in San Francisco), Ogburn encouraged Sapir's first readings of Freud. Before moving to Chicago in 1927 Ogburn went to Europe, spending time in Vienna with leading psychoanalysts including Freud, and in Paris with French ethnologists, notably Marcel Mauss.

Ogburn was interested in technological innovation and most famous for the notion of "cultural lag" (social organization and customs adjusting to changes in material conditions, particularly technological change), though he did not consider lags a fundamental process of social change, as was diffusion (the phenomenon of central concern to Boasian anthropology into the 1920s) (Ogburn 1922).[3] Ogburn's interests were wide-ranging, evidenced by his positions and intellectual social networks. It was in editing the *Journal of the American Statistical Association* that Mead was his assistant, and I have already mentioned his contact with Freud and Mauss, as well as with prominent Boasians. He was also director of research of the President's [Herbert Hoover] Research Committee on Social Trends from 1929 to 1933, which resulted to a two-volume report (Ogburn 1933).[4]

Ogburn went to Chicago a major figure in interdisciplinary American social science (see Ogburn and Goldenweiser 1927), the major sociologist most interested in culture (see Ogburn 1937a, 1964)—and the most Boasian one, interested in social indicators of culture, economy, and society, and in increasing the availability of comparative data for social history. Ogburn was not, however, in the Jeffersonian tradition of idealizing rural yeomen and demonizing cities as dens of vice and social disorganization, which significantly influenced and was elaborated upon by the "Chicago School of Sociology" urban ethnography of Robert Park.[5] Nor was Sol Tax, whose initial formation as an anthropologist was as an undergraduate at the University of Wisconsin studying with Ralph Linton and Charlotte Gower (later Chapman [a Chicago alumna]). Before doing his University of Chicago dissertation research with the Mesquakies (then called "Fox") in Iowa (see Daubenmier 2007) and neighboring states (1932–33) at the behest of A. R. Radcliffe-Brown, Tax had participated for four months in Alonzo Pound's (Beloit College/Logan Museum) archaeological work in Algeria, two months with George Grant McCurdy's American School of Prehistoric Research in Europe (both in

1930) and in the New Mexico summer field school directed by Ruth Benedict, working with Mescalero and Chiricahua Apaches in 1931 (Tax 1988:3, Gleach 2002).

Tax was invited by Robert Redfield to join the Carnegie Institution's study of ancient and contemporary Mayans (of which archaeologist Alfred Kidder was the overall coordinator) to work in highland Guatemala. Tax finished and defended his dissertation only a few days before setting off. He recalled that Redfield had suggested learning Spanish and reading Schulze-Jena's recently published book on the Quiché (Tax 1988:3).

The plan was for Sol and his wife, Gertrude Tax, to spend eight months in Guatemala and four months in Chicago each year, and this they did for three years (1934–37). After this, he wrote,

> I had a short field season in the winter of 1938, and then—together with our newborn daughter—we had six months in 1939 and a full season of fieldwork in 1940–41. Our fieldwork consisted of fairly intensive work in Chichicastenagno (Quiché) and in the towns bordering Lake Atitlán, Particularly Panajachel (Cakchiquel). Part of this time, the Redfields lived in nearby Agua Escondida, a Ladino town, and our contact was close. I worked particularly on the world view and the economy of the Indians."[6] (Tax 1988:3–4)

Tax undertook a very detailed record of expenditures of time and money and of "primitive capital accumulation" (not a term he used) in the town of Panajachel on the shores of Lake Atitlán. The population of Panajachel in the late 1930s was approximately 800. Tax found Guatemalan highland Mayas very involved in a money economy and considerably more individualistic and, indeed, capitalistic, than rural folk living not far above subsistence level were supposed (by social scientists) to be. The findings from Guatemala did not fit well with the generalizations about Mayan culture and social organization that Redfield was elaborating in the Yucatán or the binary pastoral *gemeinschaft*: socially atomized *gesellschaft* conception of the earlier Thomas/Park Chicago tradition (see Murray 1986, 1988b; Faris 1967)—into which William Ogburn's work never fit.[7]

Ogburn's Impetus to Expanding and Publishing "Panajachel Economics"

In a letter dated February 25, 1939, Redfield wrote Tax that Ogburn (who was at that time the head of the U.S. Social Science Research Council) had visited the Redfields in Agua Escondida the previous day (Good

Friday), and that Redfield had told Ogburn of Tax's manuscript on Panajachel economics and urged Ogburn to visit Tax in Chichicastenango. Ogburn spent that (Easter) weekend in Chichicastenango, reading the Panajachel economics manuscript after their Saturday meeting, and "spent several hours going over it with me on Sunday" (Tax, letter to Redfield, March 1, 1939 [Rubinstein 1991:269]). Tax reported that Ogburn told him that the research should interest economic historians and should be expanded into a monograph, that "the tables attempting to give the 'balance of trade' represent data never worked out for any community in the U.S." (Tax, letter to Redfield, March 1, 1939 [Rubinstein 1991:270]). Ogburn urged Tax to break up the complex tables into many simpler ones (for instance, the chart on distribution of time expenditures into about ten tables). Ogburn stressed the challenge that Tax's findings constituted to assumptions that "correlate regional specialization with good transportation facilities—which hardly jibes with Guatemalans trudging the roads under their loads" and "that farms in non-industrial society are self-sufficient (largely)" (Tax, letter to Redfield, March 1, 1939 [Rubinstein 1991:270]). The last was also an anomaly to Redfield's idealization (in more than one sense—see Murray 2005) of "little communities" in general, and communally oriented Mayan society in particular.

That what was intended as a chapter was expanded (as well as how it was expanded) stemmed directly from Ogburn's response to what he read in Chichicastenango. Redfield wrote back to Tax expressing his

> great satisfaction and pleasure at the fact that Ogburn read your economics paper, that he found such value in it, and that he made such important suggestions for its development. I thought it was good, but it is much more important that an economist (for Ogburn is) has found it good, and of course the importance that he sees in it will be of much more consequence, because of wider significance, than what an anthropologist per se may see in it. I think now we must take up with [Alfred] Kidder Ogburn's suggestion that the paper be extended and revised and separately published. (March 4, 1939 [Rubinstein 1991:271])

Tax collected more economic data in his last field season (1940–41), and then was scrambling from project to project. He was involved in a nutritional survey of rural Guatemala, in teaching in Mexico City and Chicago, and in training students among Maya peoples in Chiapas during the following years, returning to Chicago for permanent residency in the spring of 1943. He was a tenured scientist within the Carnegie Institution and a research associate of the University of Chicago Anthropology Department, teaching on an ad hoc basis in the undergraduate

college. When the Carnegie Institution shut down its Division of Historical Research Dean Redfield and University of Chicago President Robert Maynard Hutchins arranged a faculty appointment for Tax (associate professor, 1944, promoted in 1948 to full professor). Settled in Chicago, Tax finished the Panajachel economics manuscript, and gave Ogburn a copy.

On July 28, 1943, Ogburn wrote Robert Redfield,

> Dear Dean Redfield:
>
> Mr. Tax left his manuscript on the economic system of the Indians of Panajachel, which I read during the past ten days. I would like to tell you that I consider it a remarkable piece of work. If the object of research is to discover new and reliable knowledge, then I consider Mr. Tax's book as valuable a piece of scientific ethnographical work as I have ever seen. I doubt if Boas ever did a better job of this nature. I wish all of our field workers in anthropology were as careful workers as is Mr. Tax. To me, it seems also a very important achievement to have made.
>
> Our economic historians, I think, with Europe and the Near East as a subject matter, have associated commercial agriculture with the development of transportation. Earlier agriculture was self-subsistence farming and also associated with war and the feudal state. Mr. Tax has uncovered a high development of commercial agriculture, almost as high as our own, without any transportation system other than that of human beings. I think economic historians after this study will have to reorient themselves in their theories of agricultural production. It's a fine thing to have a record so objective and complete as this record of Mr. Tax. How the economic historians of the past would like to have a document like this for Egypt or Greece or the Middle Ages!
>
> My congratulations on sponsoring such a valuable and able piece of work.
>
> Cordially yours,
> William F. Ogburn

This enthusiastic praise was expanded in Ogburn's (undated) preface:

OGBURN'S PREFACE FOR *PENNY CAPITALISM*

> It is more difficult to find a good treatise on the economics of a pre-literate culture than it is to find an adequate account of the family and clan, religion and mythology, customs and ceremonies, or some other aspect of society. It is true that the material culture is always described in general accounts by ethnographers,

but this description, though full in terms of material objects, is quite meager in terms of economic processes. Hence the production of this excellent study of the economic organizations and functions of the Panajachel Indians of Guatemala by Dr. Tax is an event of importance.

These Indians of Panajachel are not primitive people like most of those studied by most anthropologists, yet the number of adults that read and write is negligible statistically. Nor is their economic organization as simple as most pre-literate agricultural peoples, yet their agriculture is based on the hoe without the benefit of plow and draught animals. This monograph is significant not only because it deals with a relatively less cultivated field of anthropology and thus helps to fill a gap, but it is important for its contribution to the modern science of economics.

For instance, our knowledge of the evolution of economic institutions, as found in the contemporary economic histories, is based largely on the description of European data, and many of us have inferred that what has happened in Europe is the law of economic growth. But this Guatemalan study shows that such is not the case. Thus to the students of the economic history of Europe, commercial agriculture came with the development of transportation. When transportation was poor and hazardous, agriculture was largely a self-sufficing economy and trade was poorly developed. A farmer produced nearly all he consumed and had little left over to sell. Indeed, to readers of European economic history it seems axiomatic that the absence of the vehicular transportation determines that agriculture shall be self-sufficing and non-commercial. Yet, in Panajachel, though there is not a wagon for transportation, there is probably no more self-sufficing agriculture per 100 farmers here than we have in the United States. When Europe did not have the steam engine, the horse, or the ox, hers was a household economy, but the economy of Panajachel, without any other means of getting products to market than the human back, is not a household economy, as might have been expected. The reader who may be curious as to how this could be so is referred to Dr. Tax's interesting pages.

So, by this study of a non-European economic system, we see that agricultural evolution does not have to follow the course it took in Europe. There is nothing at all of the feudal system, but there is a good monetary development. Hence, we are led back

to the analysis of the several variables that make an economic system what it is. To be forced back to describe a system in terms of its several variables is a healthy scientific reaction, better than a mere description of the evolution of one system.

This research into the economic life of these Guatemalan Indians is important because it shows how an economic study of a pre-literate people ought to be done. The amount of objective, detailed measurement is remarkable. The author has even achieved, without any existing record of statistics, a table of "balance-of-payments" for the Panajachel economy, an achievement which was only recently attained for the United States. He has worked out the actual typical household budgets, balanced production with consumption, shown the inequality in the distribution of income, and has constructed a time budget for the community. With these detailed tables and measurement it is possible to make meaningful comparison with our own and other systems. They also afford a basis for an economic interpretation of the social structure associated with the economic system. Indeed, Dr. Tax promises in another volume to discuss the inter-relation of the economic with the social institutions. Possibly, social values help to determine the economic system as well as the economic factors determine the social systems. It may be indeed that our own economic organization is being integrated more closely with the political and social institutions and that we may learn about such inter-relations from simpler cultures.

To return to the author's careful workmanship, the reader, unaccustomed to doing this kind of work, will surely marvel at the researcher's patience and care, an example, by the way, for other anthropologists dealing with economic activities to follow. The author's consideration of detail may loom large to the reader in comparison with any majestic sweep of generalization. To any such reader who may be overly impressed with this meticulous handling of much data, I ask, is not this the method of true science and has any science ever grown up without being based on such painstaking labors?

In addition to the advocacy of careful measurement that Ogburn consistently pressed throughout his career, there is also the typical Ogburnian interest in multivariate analysis in contrast to the simplistic extrapolations and bivariate analyses of other quantifying sociologists of his time (see Ogburn 1946; Murray and Rankin 1982; Laslett 1991). Ogburn did

not mention Melville Herskovits's (1940) book on economic anthropology (that also was not cited explicitly by Tax), nor specify that among the generalizations that Tax's Guatemalan research undercut were Robert Redfield's about communitarian orientation of "folk societies" in general and Mayan ones in particular (anomalies to his folk/urban continuum conception that Redfield awkwardly attempted to deal with in his [1941] *Folk Culture of Yucatan*).[8]

In prefacing his study that had been completed a decade before, Tax wrote, "I am asking about Panajachel some of the same questions that are asked by economists of our own society" and that "the significant thing is that I am able to answer the questions, because the Panajachel economy is like ours. If I had tried to ask about a tribe of Australian aborigines what is its balance of payments, I should soon have had to reinterpret the question so drastically that it would not be the same," whereas the mercantile capitalism of Panajachel was small-scale but involved rational calculation attempting to maximize profits, even if "there is no economic theory in this book" (1953:ix).

After alluding to the economic anthropological work of Melville Herskovits and Raymond Firth (neither of whom is cited in *Penny Capitalism*),[9] Tax continued: "What I offer is a conception of how one studies a primitive *money* economy. My own work falls short of an ideal because I had no model" (1953:x, emphasis added), although the classical economics of Adam Smith (that were revived by the "Chicago school of economics") was more than implicitly a model within the analysis. Tax (1953:18) quoted Smith's *Wealth of Nations*—"every individual is continually exerting himself to find out the most advantageous employment for whatever capital he can command"—and discussed the fuzziness of Smith's explanation for the emergence of division of labor in a note (27–28).

The monograph plunged immediately into describing the local situation, with no review of literature in anthropology or economics. Intermittently throughout the monograph, Tax contrasted the absence of classes (since "the wealthiest families are not far above the subsistence level [and] they have little margin of safety" [206]) with the stratification (made possible in part by production for sales to tourists) of Chichicastenango, where Tax also did fieldwork (though he published practically nothing from the results of his fieldwork there).

The monograph was not published immediately after Tax completed it because of the wartime paper shortage. In the decade before it finally was published Tax read more economics, and later said that after revising the manuscript he had forgotten that Ogburn had written a preface

(October 19, 1990, notes on telephone conversation with Tax). Tax dedicated the book to Alfred Kidder and wrote a preface dated May 1, 1951. *Penny Capitalism* finally appeared in print in 1953 as Smithsonian Institution Institute of Social Anthropology Publication 16. The preface Tax wrote specifically acknowledged not only Ogburn's 1939 encouragement but his having written a preface for the 1943 revised manuscript: "A first short draft was written during the winter of 1938–39. I was encouraged by Dr. W. F. Ogburn to extend this to a full study. It was completed in June of 1943, when Dr. Ogburn was also kind enough to write a foreword" (Tax 1953:x).

Between 1951 and 1953 Tax may not have been able to find the preface Ogburn wrote, but he clearly had not forgotten it. On July 10, 1991, Tax wrote me, "I will probably never be able to reconstruct how I happened to publish *Penny Capitalism* without communication with Ogburn!" Aside from personal embarrassment (that included a request that I apologize to Ogburn for him), Tax realized that had Ogburn's introduction been included in the published monograph, it might have encouraged others to undertake comparable research—although John Gillin's similarly laudatory *American Anthropologist* review, presumably read by more anthropologists than the number who perused the volume—had not stimulated others to emulate Tax's research (letter to author, August 1, 1991). Tax told me that he was quite sure that Ogburn never mentioned the omission of the preface from *Penny Capitalism*; they both lived on University Avenue and "sometimes saw each other and said hello on the street," but never discussed the reception of Tax's monograph (October 19, 1990, notes on telephone conversation with Tax).

Conclusion

In 1991 when I asked Tax about Ogburn's unused preface, Tax realized that it had been a lost opportunity to gain wider attention to what he had found in highland Guatemala, which was relevant to comparative socioeconomic history and the kind of dialogue that Tax had sought to facilitate throughout his long career. I would hazard the guess that he felt less need for the validation of a powerful and widely respected elder in 1951 than in 1943 or 1939 (though wincing at the seeming ingratitude in 1991 retrospect).

I believe that what Sol Tax did in researching and explaining unexpected-to-many patterns in highland Guatemala deserves to be better known. For understanding earlier anthropology, however, what is significant about this case is a reminder that once upon a time, before the recent decades of accelerating fragmentation of anthropology, there were social

and intellectual ties between prominent American anthropologists and sociologists who were interested in what anthropologists were finding—an interest that was little reciprocated and fell off considerably when some cultural anthropologists (notably Margaret Mead, Clyde Kluckhohn, and Hortense Powdermaker) began making sweeping claims about American culture (in the singular) based on anecdotes and intuitions or on patently biased sampling during and after the late 1940s (see Murray 1988a). The field of "culture studies" that has emerged in recent years exhibits little knowledge about or interest in the studies of diffusion and conceptions of "culture areas" of Boasians, the ideal types of Redfield, or the kind of cultural sociology developed by such Columbia-trained sociologists as Elsie Clews Parsons (though interest in her as a feminist icon exists) or William Fielding Ogburn (though "culture lag" continues to seem relevant to pop analysts of social change, and the work on social indicators and quantification that he pioneered is flourishing within academic sociology). Social history of the emergence of capitalism (like social theory) looks to French explicators rather than to American anthropologists, even to the few who addressed mercantile proto-capitalism on the basis of fieldwork done between the world wars. The contributions of these American social scientists remain unfortunately neglected.

Notes

The author is deeply indebted to the late Sol Tax, who granted me access to his then-restricted papers in the University of Chicago's Regenstein Library and encouraged me to write about the lost Ogburn preface to *Penny Capitalism*, and to Willard Ogburn for permission to quote from previously unpublished writings by his grandfather, William Fielding Ogburn. The preface is in the Tax Collection, and the Ogburn letter in the Redfield collection; correspondence between Tax and Redfield is in both collections, and published in Rubinstein (1991).

1. Different reasons for the break have been put forth, including Leslie White's typically egocentric one that the break was due to the outrage on the part of his mentors, Fay Cooper Cole and Edward Sapir, at the rough treatment of White by sociologists at White's thesis defense in May 1927 (see Peace 2004:23–27). Fred Eggan (on the basis of what his elders had told him) told Stephen Murray at the Sapir Centennial Celebration that the move preempted a reorganization of the sociology program's core courses (see Murray 1986:267). The official explanation put down by Cole in a November 26, 1928, letter to University president Woodward may be diplomatic "spin," but also seems plausible: "In this request for a separate department nothing of a personal nature is involved. We do believe, however, that the time has come when Anthropology at the University of Chicago would be greatly strengthened by being made independent, and we do not think that Sociology would suffer by its separation."

2. See the photograph of picnic-goers reproduced in Goldfrank (1978:82). Ogburn was seen as too Boasian and too influenced by Robert Lowie and Clark Wissler by Abel

(1930:741) and Huff (1973:272). For more detail about the integration of Ogburn in the circle of first-generation Boas students see Murray (1986:246–248).

3. On the shift from mapping distributions of culture traits to psychoanalytically informed work on cultural integration during the 1920s, see Darnell 1977, 1990; Murray and Darnell 2000.

4. Also see the less comprehensive analysis in Ogburn (1930). On the methods see Bulmer 1983.

5. See Faris (1967), Murray (1986, 1988a). Robert Redfield, Park's son-in-law, received his second professional training in this department (the first being law school, also at the University of Chicago), and went on to pursue his career in the Chicago departments. On Redfield's development of studies of "folk" societies and antipathy for Boasian particularism, see Wilcox 2004 and Murray 2005. The dismissal of Boasian anthropology by Murray Wax (1956), a student of Redfield, derives to a considerable extent from the critiques Redfield made in his theory classes (Wax kindly sent me syllabi from ones he took).

6. For the classic account of highland Guatemala worldview, see Tax 1941.

7. See especially Ogburn (1937b) on the reconceptualization of American urbanism, the work on rural society by Kolb and Brunner (1946) that he supervised, and that of his protégés Duncan and Reiss. (1956). A special focus for Ogburn's earliest sociology students was on patterns of mental illness, including the ecological work of Robert E. L. Faris (e.g., 1938) and the challenge to "Noble Savage"/primitive mental health and urban American pathology of Ellen Winston (1934).

8. As Wilcox (2004:58) noted, in the final chapter of Redfield (1941) "Redfield's prose lost its fluidity, it became awkward and labored. Rather than supporting or complementing the argument drawn from the Yucatan materials, the Guatemalan data seemed to be contradictory. The more Redfield labored to account for this divergent data, the weaker his overall argument seemed." Redfield published the article "Primitive Merchants of Guatemala" in 1939 (Redfield 1962:200–210).

9. One footnote (Tax 1953:185 n.173) cites the existence of (rather than any findings from) Charles Wagley's (1941) study of Chimaltenango. The head of the Smithsonian Institute of Social Anthropology, who authorized the publication of *Penny Capitalism*, was George Foster, who had studied economic anthropology with Melville Herskovits and whose 1941 (Berkeley) dissertation was about a not-so-primitive Mexican Indian economy.

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4. Stephen Leacock

The Not-So-Funny Story of His Evolutionary Ethnology and Canada's First Peoples

David A. Nock

Stephen Leacock the Popular Intellectual

Daniel Francis has drawn attention to something which many of us might have preferred were otherwise. Canada's preeminent humorist, Stephen Leacock, whose very name identifies Canada's major award in this genre, wrote extensively on Canada's First Peoples using a tone described by Francis as "dismissive, even vicious" (1992:55). Francis also drew attention to Leacock's reliance on the evolutionary stage perspective in his depiction of Leacock's view that "Canadian history . . . was the struggle of civilization against savagery. There was never any question on which side Indians stood" (1992:223).

The aim of this paper is to examine in greater depth Leacock's writings on Canada's First Peoples and then to examine the proposition that Leacock was disposed to this portrayal by the ethnography of America's aboriginal peoples by the Scottish historian and proto-anthropologist, and proponent of evolutionary stage theory, William Robertson.

Of some interest also is Leacock's status as a "public intellectual," a term that has come to denote in more recent usage a generally positive connotation with regret over the lack of public intellectuals in today's world. The term was popularized by Russell Jacoby in his *The Last Intellectuals* (1987). He meant this by it: "the old fashioned generalist intellectual who wrote clearly about social, political, and cultural issues" as opposed to today's "specialized scholars who write about narrow academic debates in often tortured or highly technical prose" (McLaughlin n.d.:2). In addition, the public intellectual reaches out to the wider public by utilizing generally accessible media rather than low-circulation "scholarly" journal articles and monographs.

Arguably, the "improving" Victorian and Edwardian eras were well-stocked with public intellectuals but Leacock's example may lead us to a reexamination of the recent and favorable portrayal of them. Rather

than speaking "truth to power," Leacock's writings demand some other epithet such as the reinforcement of conventional stereotypes still widely held by the Canadian public at that time. However that may be, Leacock's "dismissive, even vicious" portrayal of aboriginal peoples did have an academic lineage, one that is linked to evolutionary stage theory. On the other hand, it is arguable that even if Leacock's views were in accord with widely held academic and popular views, there did exist more favorable portrayals of aboriginal cultures which Leacock could have drawn from, most notably the American-Canadian scholar, Horatio Hale, F.R.S.C., (1817–96). One is reminded here of the three axioms of sociology's symbolic interactionist perspective: that we act toward things (including social categories and concepts) on the basis of the meanings that such things have to us as individuals, that such meanings arise out of the social interaction we have with others (social interaction here must be defined broadly as including social and cultural influences in general), and that these meanings, so derived, are modified in an interpretive process by the person dealing with the social concepts he or she encounters (Blumer 1969:2).

Leacock, of course, was not an anthropologist or ethnologist by profession. His profession was that of political scientist and economist. He had earned his PhD in 1903 in these subjects at the University of Chicago and then joined McGill's faculty, where he stayed until retirement in 1936. In 1906 he had published *Elements of Political Science* which "became a best-selling book in his lifetime." Leacock's avocation was his long string of "funny books" which are still widely read and about which Canadians generally think of when the name Leacock comes to mind. These included his "masterpieces," *Sunshine Sketches of a Little Town* (1912) and *Arcadian Adventures with the Idle Rich* (1914). Gerald Lynch (2000:1312) writes that Leacock held the rank of "the English-speaking world's best-known humorist" during the years from 1915 to 1925.

Leacock's willingness to venture beyond the fields of political economy and humor can be inferred from his comment, "I can write up anything now at a hundred yards" (Lynch 2000:1312). Lynch points out that Leacock wrote "prolifical"[ly] in magazines (an indicator of the public intellectual) in addition to his many books, and that in many cases, the books were collections of the magazine pieces. Leacock's topics included "humorous fiction, literary essays and articles on social issues, politics, economics, science and history" (Lynch 2000:1312).

It was in this role of popular intellectual that Leacock came to write *The Dawn of Canadian History: A Chronicle of Aboriginal Canada* in

1914 (reprinted in 1915). This book appeared as the first of "thirty-two tersely-written narratives for popular reading, designed to set forth . . . the principal events and movements in Canada, from the Earliest Explorers to the Railway Builders" (Leacock 1915:113). The overall title of the series was the "Chronicles of Canada," and each volume contained a subtitle using "chronicle" in it as indicated above in Leacock's initial contribution. The prospectus of the series, as quoted, makes plain its aim as popular history from the pens of public intellectuals.

The series was edited by George M. Wrong and H. H. Langton of the University of Toronto. Wrong taught history in the university's department from 1892 until 1927, after an earlier career as a cleric and seminary professor in ecclesiastical history and liturgics at Wycliffe College from 1883 to 1892. From 1894 until his retirement, he served as head of the department of history. Part of his task was to promote history as a distinct discipline and "Canadian history as a legitimate field of study" (Taylor 2000:2256). In 1896–97, he co-founded the *Review of Historical Publications Relating to Canada*, which in 1920 became the *Canadian Historical Review*. In 1905, he co-founded the Champlain Society. He was also known for his "numerous monographs and texts on Canadian history" (Taylor 2000:2256).

Wrong was not a narrow expert or specialized scholar. He fit comfortably into Jacoby's mold of "the old fashioned generalist intellectual" (McLaughlin n.d.:2). As such, Carl Berger points out that Wrong "laid great stress on communicating the results of historical research to the general reading public and students in the school" (1976:14). Berger then provides as evidence of this proclivity his editing of the Chronicles of Canada series and concludes by pointing out that as "a conscientious member" of the Ontario Educational Association he "wrote a number of texts in British and Canadian history that were widely used in schools throughout the country" (1976:14). Berger also links Wrong with several other university "figures" of his generation, including Stephen Leacock, "who saw themselves as men of general culture and members of an educated elite that included enlightened businessmen, clergymen, and politicians [who] detested narrow specialization" (1976:31). In other words, Wrong stands as a representative example of the still thriving category of the late Victorian–Edwardian public intellectual.

Langton, a frequent collaborator of Wrong's, was the University of Toronto's first full-time registrar from 1887 until 1892 and then its head Librarian from 1892 to 1923. In addition, he co-edited series such as the Chronicles of Canada, jointly edited the *Review of Historical Publications Relating to Canada* and wrote, edited, and translated much else relating to Canadian history (Blackburn 2000:1292).

Altogether Leacock contributed three of the thirty-two volumes in the series: numbers 1, 2, and 20. These included volumes on the voyages of Jacques Cartier and one on the Arctic region and explorers in addition to *The Dawn of Canadian History*. Aboriginals were not totally ignored in the series—other volumes treated Pontiac, Joseph Brant, and Tecumseh, described as "the last Great Leader of his People." For a general estimation of the character and abilities of First Peoples, however, one must look to Leacock's first book, and we must contemplate the effect it might have had on readers from the general populace, many of them at an impressionable age as young students.

The Theme of Aboriginal Deficiency in Leacock's Ethnography

Most of this relevant depiction is recounted in chapter 3 of *The Dawn of Canadian History*, entitled "The Aborigines [*sic*] of Canada." The general tone concentrates on what aboriginals lacked in terms of traits deemed important by the Euro-Canadian newcomers. This theme of aboriginal deficiency starts almost immediately with the observation that "Very few of the tribes possessed even a primitive art of writing" (Leacock 1915:25). This led Leacock to introduce the Aztecs, Toltecs, Mayans, and "ancient Peruvians" and some of their achievements in what Leacock designates not as a system of writing but of record keeping. Discussion had been brisk for several centuries over what level these aboriginal societies of Central and South America had attained compared to Natives further north. But despite a small concession to these ancient peoples of having "understood how to write in pictures and that, by this means, they preserved some record of their rulers and of the great events of their past" (25), the real conclusion Leacock drew was that "nowhere was the art of writing sufficiently developed in America to give us a real history of the thoughts and deeds of its people before the arrival of Columbus" (25–26).

This theme of aboriginal deficiency, said Leacock, was especially true of those families of the great red race which inhabited what is now Canada" (26). He pointed generally to the "primitive existence" of First Peoples. Leacock frequently revisits his theme of aboriginal deficiency by examples such as their lack of "any settled abode or fixed dwelling-place" (26), their lack of knowledge of "the use of the metals" (26), their lack of "anything but the most elementary form of agriculture" (27), their generally "poor weapons and tools as they had" (26) made of stone, wood, and bone, and inferior to metal.

In this discussion of material culture, Leacock could discern only one

impressive invention, the bark canoe, which he acknowledged as "a marvel of construction and wonderfully adapted to its purpose. This was their great invention." Given that Leacock spent long summers every year at Old Brewery Bay, Ontario, I speculate that his admiration was stimulated by his own use and enjoyment of this "great invention" on Lake Simcoe. However in citing the canoe as the one-off glory of Native ingenuity, Leacock relied on evolutionary stage theory to re-emphasize this theme of aboriginal deficiency by observing, "In nearly all other respects the Indians of Canada had not emerged even from savagery to that stage half way to civilization which is called barbarism" (27). After several pages describing more specifically the Eskimo (Inuit), Beothuks, and Algonquians of Eastern Canada, Leacock once again rendered his verdict regarding the material culture of aboriginals north of Mexico, that their "attempt to utilize the materials and forces supplied by nature had made only slight and painful progress" (33). He did not avoid attributing a state of "backward[ness]" to them when Europeans first arrived, and he commented that it was no wonder that "any advance towards art and industry was inevitably slow and difficult" (33).

Depictions of the Hodensaunee (Iroquian Confederacy of Six Nations) had periodically drawn more favorable coverage than other First Nations in Canada. This was acknowledged by Leacock, who commented, "The Iroquois were in some respects superior to most of the Indians of the continent" (38). He grudgingly admitted their "limited agriculture," their longhouses constructed of logs, their fortified villages, and their storage of food supplies. Leacock granted them superior skills in "organization" (39) and stated that "they had advanced further in other directions than most savages" (38). Strikingly, his discussion did not refer to the intricate organization of the Confederation agreement between the Six Nations, which had already drawn admiring attention in the work of Horatio Hale and others. Another feature which had drawn the attention of Hale and others was the elevated status of women. Leacock did quickly acknowledge that "Among the Iroquois . . . women were not wholly despised" and that if of "forceful character" they could have "great influence in the councils of the tribe" (39).

However, any reader of this passage is likely to find it overwhelmed by subsequent passages referring to the treatment of women among the "kinsmen" of the Six Nations, their fellow Iroquoians, the Hurons. In a sweeping statement, Leacock described the treatment of women in "all Indian tribes" as "degrading" and the women themselves as "drudges" (39). The subsequent passages relating to Huron women are unredeeming in their negativity: Huron women, Leacock suggested, quickly lost

their "charm and attractiveness" and for the most part, "degenerated into . . . shriveled hag[s], horrible to the eye and often despicable in character" (39–40). The "inborn gentleness of womanhood" assumed by Victorians and Edwardians had been driven from the Huron woman by "ill-treatment." He ended this vivid passage by referring to "the unhallowed fiendishness of the withered squaw in preparing the torments of the stake and in shrieking her toothless exaltation beside the torture fire" (40). Such a depiction of aboriginal deficiency makes Francis's designation as "vicious" easy to comprehend.

The viciousness of this depiction extended to the Huron in general and they certainly stood out as an example of Leacock's theme of aboriginal deficiency. Going beyond comments on their material culture, Leacock decried their "glutton[y]," their "gambling and theft." Leacock's conclusion was that the Hurons were among "the most despicable of the Indians in their manners" and "we can find scarcely anything in them to admire" (40). Leacock drew from the language of bestiality when he depicted them as possessing "the rapacity of vultures" (40).

The Six Nations Confederacy Leacock had depicted as "somewhat superior" to other "Indians of the continent." This conclusion was based on their material culture and organization. However what otherwise might have truly raised them above other aboriginal cultures was counterbalanced by their moral failings. He charged them with "diabolical cruelty" which had rendered them an object of "contempt" which mingled with admiration for their "bodily strength and physical endurance." Horatio Hale's eloquently argued reply to this charge had already appeared by 1883, but Leacock either ignored it or was unaware of it.

In general, then, Leacock's theme was of aboriginal deficiency in both material and moral culture. The canoe might be cited as the one impressive innovation of aboriginal technology, but this was depicted as being so singular as to be almost a miracle. Aboriginal moral culture was also depicted consistently as deficient. The word "rude" was a favorite in Leacock's vocabulary whether in reference to the "rude nature worship" (34) of the Algonquians or to their "rude pictures scratched or pained on wood" (35). A third instance of this adjective comes when Leacock refers to "these rude peoples . . . so backward and so little trained in using their faculties" (33). The *Gage Canadian Dictionary* defines this term, beyond its primary modern connotation of impoliteness, as "without finish or polish, coarse; crude" and even more saliently as "not having learned much; uncivilized; rather wild; barbarous" (1983:981) and it is these somewhat dated meanings which Leacock is drawing upon.

The Dawn of Canadian History was not the only work of Leacock's re-

ferring to the First Peoples of Canada. A generation later he wrote *Canada: The Foundations of Its Future*. This was commissioned and published by The House of Seagram as a patriotic gesture during a time of war. This is indicated in the preface written (or signed) by distiller Samuel Bronfman, who pointed out that "as our country stood engaged in battle for the defence of its most precious ideals, this volume was conceived, planned, and prepared" (Bronfman 1941:n.p.).

Once again Francis pointed to Leacock's consistent "dismissive" depiction of First Peoples (1992:55). Compared to his book of a generation earlier, Leacock more succinctly voiced the themes of aboriginal deficiency and bestiality. This is clear from statements such as this: "Their use of the resources was scarcely more than that by crows and wolves, their development of it nothing" (1941:19). In both books, Leacock had severely discounted the numbers of First Peoples even before their depopulation following the arrival of Euro-Canadian colonization.

William Robertson and Evolutionary Stage Theory

At this point we must ask what sources contributed to Leacock's emphatic theme of aboriginal deficiency. As a university scholar, Leacock was used to consulting the work of other scholars—"authorities," as they were often termed. To return to Blumer's axioms of symbolic interactionism, we act toward things (social categories) on the basis of the meanings that these things possess for the actor (meaning anyone who engages in action), and we learn these meanings from the process of social interaction, broadly defined to include all the factors involved in scholarly activity.

For the moment, let us look at the academic sources influencing Leacock in his ethnography and his reiterated theme of aboriginal deficiency. Here I think the key is found on page 34 of chapter 3, "The Aborigines [*sic*] of Canada" in *The Dawn of Canadian History*. Leacock cites and then quotes "the historian Robertson" as describing "in a vivid passage the backward state of the savage tribes of America." The passage actually quoted is about ten lines long and emphasized the limited technology of the aboriginal peoples, which rendered "the most simple operation . . . an undertaking of immense difficulty and labour." As quoted by Leacock, this deficient technology had made "their operations in agriculture . . . equally slow and defective," owing to the "much time and great toil" that clearing "woods of the hardest timber" would require."

Who was this "historian Robertson" who had so influenced Leacock in pursuing his theme of aboriginal deficiency? I rather suspect that many

readers today will recognize the names of Edward Gibbons and David Hume, the second known more as a philosopher and the first the celebrated author of a detailed history on the decline and fall of the Roman Empire. How many of us would instantly recognize the name of William Robertson? Yet D. J. Womersley refers to these three as "the triumvirate of eighteenth-century British historians" and points out that of this distinguished trio, "only Robertson published more than one history" (1986:497). These rather diverse histories included the *History of Scotland* (1759), a *History of Charles V* (1769), and—most importantly for our purposes—the *History of America* (1777), which Hoebel has described as deserving "recognition as a significant landmark in the development of cultural anthropology" (1960:648).

Robertson (1721–93) became a minister of the Church of Scotland, rising to the post of moderator of its General Assembly. His *History of Scotland* helped secure him the principalship of the University of Edinburgh. In 1763, a lapsed sinecure as historiographer to His Majesty for Scotland "was revived for his benefit" (Hoebel 1960:648). Robertson's "masterpiece," according to Hoebel, was the work on Charles V, appearing in three volumes. The *History of America* also appeared in three volumes in 1777. Between then and 1812 it went through ten editions in Britain. The first American edition was dated 1812. Robert F. Berkhofer Jr., refers to it as "particularly influential in the newly independent United States in shaping its leaders' comprehension of the Indian" (1979:48).

For our purposes, what is striking in Robertson is the theme of aboriginal deficiency. Jeffrey Smitten has pointed out that in writing about "North American Indians," Robertson was confronting "a thicket of controversy" replete with "competing theories . . . some extolling them as noble, others condemning them as degenerate" (1985:57). Generally speaking, there can be no doubt that Robertson came down significantly on the side of aboriginal deficiency in book 4. It constitutes the analysis of aboriginal life and is described by Smitten as "the most celebrated portion" of the work" (1985:58). Smitten cites numerous scholarly readers who interpret Robertson as depicting the Amerindians as "an immature or degenerate species of humanity" (1985:58) Smitten provides a variety of quotations from Robertson supporting such a reading such as "The intellectual powers of man in the savage state are destitute of their proper object, and cannot acquire any considerable degree of vigour and enlargement" (1985:58). Inspiring Leacock, Robertson emphasized that the aboriginal arts and technology were "restricted" and their domestic relations "perverted" (1985:58).

Berkhofer Jr., quotes an extensive passage from Robertson to illus-

trate his low estimation of aboriginal societies. It is especially instructive as it may have directly inspired Leacock. I suggest this because of Leacock's parallels in language as well as general theme. Thus Robertson asserts that, "In America, man appears under the rudest form in which we can conceive him to subsist" (1979:48) anticipating and probably inspiring Leacock's repeated use of the same word used with the same meaning. The same passage goes on to describe aboriginal societies as "in the infancy of social life," using the biological analogy of the life cycle and as characterized by "primeval simplicity." The general theme of aboriginal deficiency is made more specific by describing "the greater part of [America's] inhabitants" as "strangers to industry and labour, ignorant of arts, and almost unacquainted with property" (1979:48).

Hoebel points out Robertson's "disdain" for "the minutiae of culture which are the building blocks of any good ethnography" (1960:651). Instead Robertson was "disposed to write on a generalized level about the culture of the American Indians, while for the most part ignoring [specific] cultures" (1960:652). As described by Hoebel, this generalized summary resulted in a depiction of most aboriginals of eastern North America that was "in the main, dreary, flat, essentially accurate, yet markedly biased with unflattering value judgments. . . . His Indians were, therefore, loosely portrayed as feeble, indolent, improvident, lacking in the virtues engendered by developed property interests, intellectually unimaginative, devoid of love between the sexes, and near anarchists in civil affairs" (1960:652).

It is important to perceive that both Robertson and Leacock based their general themes such as that of aboriginal deficiency on evolutionary stage theories, which generally moved from "savagery" to "barbarism" to civilization." This is clear in Daniel Francis, who points out, "Like most of his contemporaries, Leacock ranked civilizations in an ascending scale, with modern industrial society at the top. He believed that North American Indians did not even make it at the bottom of the scale" (1992:54). Here I might have to argue with Francis, as it seems clear that both Robertson and Leacock did, in fact, place Amerindians in the bottom stage of "savagery." Logically, I do not see how any people or culture can fail to make the bottom of a scale.

This evolutionary stage theory, based on steps, levels, or stages ascending from savagery through barbarism to civilization had a long history in scientific circles and only died out in the course of the twentieth century. These very terms continued in use into the second half of the twentieth century by several British scholars, especially Gordon Childe and Grahame Clark, although often in reference to continents other than North

America and to ancient and archaic eras. In the United States, the use of such terminology died out "due to the negative connotations of the words" (Leacock 1963:xi) and due to the influential circle of Boasian anthropologists. Evolutionary stage theory itself has not disappeared but has revamped its terminology. Eleanor Burke Leacock points out that "other terms for these levels are generally employed" (Leacock 1963:xi).

However, the use of this theoretical perspective had had a long history. Berkhofer Jr., suggests that the idea was "not new in the eighteenth century" (1978:47), as there were earlier uses of it dating back to classical times. However he adds that "the intellectual context that gave real meaning to such a sequence did not develop until the latter half of the eighteenth century (1978:48–49). Its use can definitely be traced in Adam Smith's classic *Wealth of Nations* (see book 5, chapter 1) which will be more accessible than anything by Robertson. By the nineteenth century evolutionary stage theory was a staple of the early contributions in the increasingly specialized fields of ethnology and anthropology. As Berkhofer points out, its use is explicit in Edward Tylor's influential *Primitive Culture* (1871), by "the man who is usually claimed to be the founder of modern anthropology" (1978:52), and it is actually utilized in the leisurely title of Lewis Henry Morgan's masterpiece *Ancient Society, Or Researches In the Lines of Human Progress from Savagery Through Barbarism to Civilization* (1877), a work which greatly influenced Friedrich Engels and the Marxist tradition.

There could be divergences of opinion among evolutionary theorists on which level or stage had been achieved by a particular culture. Thus Morgan asserted that although American aboriginals had "commenced their career" on this continent "in savagery," most of them had attained the Lower" or even "Middle status" of "Barbarism" (Berkhofer 1979:53).

Robertson, on the other hand, classified "most of the New World tribes under the rubric of savagery "because of their lack of writing, domesticated animals and metals" (Hoebel 1960:649).

Although most of us now are likely to shrink from a theoretical perspective which relies on such a terminology involving affectively charged labels, it is important to realize that scholars, at least, associated these stages with the possession or not of certain technological and/or organizational attributes and not as a means to engage in vulgar denigration or racism. Morgan, for example, hoped that his first major work would serve "To encourage a kinder feeling towards the Indian, founded upon a truer knowledge of his civil and domestic institutions, and of his capabilities for future elevation" (Berkhofer 1979:52). Eleanor Burke Leacock

writes of Morgan that "He incorporated these people [Six Nations] into his world view as the first Americans, to be understood and respected, and allowed to take their rightful place as equals in the life of the nation" (Leacock 1963:vi).

A modern reader may jump on a phrase such as "capabilities for future elevation" but it is well to remember that there existed at the same time an entire school of scientific biological racists who were certain that future elevation was impossible because of an inadequate innate intelligence (Gould 1996). Samuel Morton represents the best known of this species but there were numerous others. Morton devised his ranking of innate intelligence on the basis of a collection of skulls from various races, providing measurements on the size of brains and then assuming a link with intelligence. A typical sentence of Morton's reads, "The benevolent mind may regret the inaptitude of the Indian for civilization. . . . The structure of his mind appears to be different from that of the white man"—adding that Indians "are not only averse to the restraints of education, but for the most part are incapable of a continued process of reasoning on abstract subjects" (Gould 1996:88–89). Lest it be thought that Morton was an outcast in the world of ideas, the *New York Tribune* wrote on his death in 1851 that "probably no scientific man in American enjoyed a higher reputation among scholars throughout the world, than Dr. Morton" (Gould 1996:83).

Even in the case of Robertson, who placed Amerindians at the lower stage of savagery rather than barbarism, and who so influenced Leacock's conception of aboriginal deficiency, Smitten points to his use of "counterbalancing" as "permeat[ing] all of Robertson's thinking about the degenerate race thesis" (1985:59). Robertson was not simply an unrelieved denigrator of aboriginal life but did point to certain "virtues" which Smitten refers to as "few" but "substantial" (Smitten 1985:59). These virtues, were, in fact, "strong attachment to community and independence of spirit" and "[i]n this regard . . . nearly a match for any European hero" (Smitten 1985:59). Smitten concludes that Robertson's view of "Indian life is unattractive, but we cannot conclude that it is only unattractive" (Smitten 1985:59).

Another topic which attracted much attention among evolutionary theorists was what caused certain societies to languish in the lower stages, and others to rise to civilization. Some authors attributed this to climate and environmental influences alone; others attributed it to innate intelligence or lack of it. Robertson did pay some attention to climate but rejected innate differences in intelligence pointing to the sameness of human beings "everywhere." Instead, he supplemented some reliance on

the effects of climate and environment with moral and political values and institutions. Robertson rejected a monocausal explanation in favor of what Smitten refers to as an argument for "multiple" causes. Smitten points out that Robertson included both climate and moral and political factors as explaining the level achieved by aboriginals, and that as moral agents they were "capable of education and physical development."

To return to Leacock, one sees some effort in counterbalancing at work, especially in his reference to the canoe. In addition, there are some positive comments about the Eskimo (Inuit); however this counterbalancing does not go very far in Leacock's work on First Nations, perhaps influenced by later intellectual currents such as social Darwinism that would have postdated William Robertson.

The one significant example of counterbalancing arises out of explaining the low placement of aboriginals at the stage of savagery. Here Leacock attributes an explanation to the general populace of "mere laziness of Indians which prevented more rapid advance" (1915:33). Leacock countered that "It may be that we do not realize their difficulties." The next several lines summarize aboriginal deficiencies that have been quoted above. The point Leacock makes here, echoed by numerous evolutionary theorists, was that Europe had achieved its high-level civilization only after lengthy epochs spent in the lower stages. In Leacock's words, "This was also true, no doubt, of the peoples who, long centuries before, had been in the same degree of development in Europe, and had begun the intricate tasks which a growth toward civilization involved" (1915:33–34). Leacock's (and Robertson's) analysis, then, is in conformity with Berkhofer Jr.'s statement: "By analogy between the life cycle of a human being and the history of the species, philosophers in the eighteenth century, especially in France and Scotland, produced a theory of the sequence of stages of society that the race had passed through to reach the height of progress exemplified by Europe at that time. Just as a single person advanced from infancy through youth to reach adulthood, so all humankind had passed through savagery and barbarism before gaining civilization" (1979:47).

Thus Leacock appears to distinguish himself from biological racism, albeit rather tentatively. In reviewing Robertson's and Leacock's views on this matter, one is reminded of Berkhofer Jr.'s words about Thomas Jefferson, that he "could both hold to the equality of the human species and yet rank Indians as inferior in achievement and act accordingly in attitude and policy" (1979:49). With Leacock's emphasis on aboriginal deficiencies in culture, innate equality of intelligence would scarcely be

enough to avoid a lengthy period in "the infantile state" of society before progress to one characterized by mature adulthood.

What may be open to debate is the degree of choice in his depiction of aboriginal cultures that would have been open to Leacock. On the one hand, we can speculate about how a distinguished Scottish historian and principal of the University of Edinburgh might have impressed a Canadian scholar of the time, especially because of the prestige of Scottish academic influence in Canada (especially at McGill). As indicated previously, the evolutionary stage paradigm remained influential in Britain long after it had passed from the American scene. It may be argued that Canada did not fall into the American academic orbit until after World War II. On the other hand, by 1914 the Boasian school, with its rejection of the evolutionary stage perspective, was stirring. Edward Sapir, one of the first Boasians, spent a decade in Canada right when Leacock was writing this popular ethnography. It is probable, however, that this school's impact was not fully felt, and certainly in the 1920s, American discourse was still heavily influenced by the assumptions and vocabulary of the evolutionary stage perspective.

Horatio Hale and His Theme of Aboriginal Giftedness

One of Canada's leading ethnographers and philologists of the nineteenth century had been Horatio Hale. A Fellow of the Royal Society, he was involved in an executive capacity in numerous American and Canadian scholarly associations, including a stint as president of the American Folklore Society. He wrote several dozen articles but was best known for his 1883 book, *The Iroquois Book of Rites*. Although an important mentor and influence on the development of Franz Boas, the mature and secure Boas promptly and conveniently suffered amnesia and as a consequence Hale's memory was largely forgotten by the twentieth-century circle of Boasians who effectively founded American anthropology as it subsequently has become known. When Leacock was writing, however, it was reasonable to suppose a knowledge of his career and writing. The *Encyclopedia Britannica* of 1910, in its eleventh edition, contained a major description of Hale's work and career—longer than that devoted to Boas. Such entries have continued in the twenty-first century, although entries on Boas have lengthened as those on Hale have diminished over the years.

The point here is that Hale took the strongest exception to portraits of aboriginal deficiency, favoring instead a view which emphasized aboriginal giftedness. For Hale, the Iroquois (including Six Nations and

Huron) represented an almost entirely admirable people and culture (however, it is important to recognize that his positive depiction of aboriginals extended beyond that particular aboriginal family). These positive representations are apparent in passages such as that found in the preface, attributing to them the "love of peace, the sentiment of human brotherhood, the strong social and domestic affections, the respect for law, and the reverence for ancestral greatness" (1883:3) that Hale knew would "strike most readers as new and unexpected developments." Even more impressive is his later passage:

> Instead of a race of rude and ferocious warriors, we find in this book a kindly and affectionate people, full of sympathy for their friends in distress, considerate to their women, tender to their children, anxious for peace, and imbued with a profound reverence for their constitution and its authors. We become conscious of the fact that the aspect in which these Indians have presented themselves to the outside world has been in large measure deceptive and factitious. . . . The persistent desire for peace, pursued for centuries in federal unions, and in alliance and treaties with other nations, has been manifested by few as steadily as by the countrymen of Hiawatha. The sentiment of universal brotherhood which directed their polity has never been so fully developed . . . unless it may be found incorporated in the religious quietism of Buddha and his followers. (1883:37–38)

Douglas Cole has written a fascinating article which provides insights into how Hale could develop such a portrayal of aboriginal giftedness rather than aboriginal deficiency. It has much to do with Hale's rejection of evolutionary stage theory—even that associated with his friend and correspondent, Lewis Henry Morgan. As Cole explains it, Hale's academic formation had been as a philologist. He became convinced that the languages of aboriginal societies were by no means simple; in fact many of them he esteemed as impressively developed and sophisticated, pointing to the intelligence of their speakers. Such a high estimation included the Iroquoian languages but also the Algonquian and Athapascan families. Referring to the language of the Athapascans, he considered it "one of the most remarkable emanations of the human intellect" (Cole 1973:38).

Such views led Hale to an attack on the "developmental stage theory of evolutionary progress" (Cole 1973:39). Hale pointed out that "every form of government and social institutions" could be found among primitive societies and that such societies were as "well organized and as ex-

pressive as the most civilized races" (Cole 1973:39). Hale warned against "the delusions of self-esteem" which might lead us to the conclusion that "the particular race and culture which we happen to claim as our own are the best of all races and languages" (Cole 1973:39). Most astonishingly in an age which associated Europe with the highest stage of civilization, Hale criticized the cultural and social value orientations of most of the peoples of Europe who, in that pre-Hitler Nazi period, were generally referred to collectively by scholars as "Aryans" (with a few minor exceptions). Hale severely criticized the "servility" of the Aryan race, preferring instead the liberty-loving aboriginal peoples of the Americas and such as survived in Europe (the Basques). He criticized the Aryan peoples of Europe as attracting "undue admiration" for their qualities, and stated that "we need not be surprised when modern researches demonstrate the fact that many of our Indian communities have had political systems embodying some of the most valuable principles of popular government" referring in conclusion to the Indian as "this nobler type of man, whose inextinguishable love of freedom has evoked the idea of political rights, and has created those institutions of regulated self-government by which genuine civilization and progress are assured to the world" (Hale 1883:190).

Clearly Leacock either did not know of Hale's work, or knew of it and chose to ignore it. It is possible that Leacock did not know Hale's work but as I have pointed out, Hale was hardly an unknown. He had been active in the American and British Associations for the Advancement of Science, vice-president of the AAAS's section H on anthropology, and member, secretary, and research director of the BAAS's committee on research into the Indians of the Canadian North-West. In this later capacity, he crossed swords with the youthful Franz Boas. He had also been a member of the American Philosophical Society, and as previously mentioned, president of the American Folklore Society and a member of the Royal Society of Canada. On his death in 1896, he received lavish obituaries and his photograph and a generous obituary by his publisher D. G. Brinton appeared in the January 1897 issue of the *American Anthropologist*.

Although I have no concrete evidence of Leacock's knowledge of Hale, it is likely that other influences led Leacock to his picture of aboriginal deficiency and the evolutionary stage paradigm. Although Hale had a strong reputation, many of the obituaries stressed his work as a philologist or as an Iroquoianist. Many of them rather skate over the radical implications of his themes of aboriginal giftedness and rejection of evolutionary stage theory. This was an era when Tylor and Morgan (and

perhaps Robertson in the minds of a Canadian oriented to Scotland) and their espousal of the evolutionary perspective still stood supreme. Boas and his circle started to chip away at this perspective in the twentieth century (largely reinventing the wheel after "forgetting" Hale's contributions). The smug assumption of the superiority of European civilizations started to decline with the world wars, especially World War II and the Nazis' assertion of racial superiority.

A final factor influencing Leacock may have been the scholarly racism which intertwined with lay perceptions of the evolutionary perspective. Here I would point to the production of school history textbooks. Patricia Ofner's 1982 MA thesis was a study of the representation of aboriginals in textbooks approved for use in Ontario between 1857 and 1980. Her method was a content analysis of the use of language as used to describe First Nations. She found that in the five books she surveyed published up to 1900, 82.5 percent of the terminology used was pejorative. In the period from 1901 through 1941, that figure actually rose to 91 percent. The negative words most commonly used in the years from 1900 through 1941 included "savage," "killer," "warlike," "torture(r)," "murderer," "cruel," and "massacre."

Although one would not expect a serious examination of the evolutionary stage perspective so widely used in history and the social sciences, it is clear that the same words were used by textbook authors in a looser sense. Examples would include this statement in Buckley's 1891 textbook: "Nor were the people now known as North American Indians the first to inhabit this Continent, as many remains exist of a more civilized race" (Ofner 1982:46); or William Stewart Wallace's of 1930: "the civilization of America to-day owes very little to the aboriginal inhabitants of the country" (1982:49). The same Wallace referred to Canadian aboriginals as "backward" and "savages of a primitive type" (Ofner 1982:50). Thus popular intellectuals who wrote these textbooks for use in schools drew on the language of the evolutionary stage perspective, although loosely and in a fashion that some of its originators would have likely found objectionable (one thinks of Morgan certainly and perhaps even Robertson in his search for counterbalancing). Many of the textbook writers lacked balance as aboriginal deficiency rather than aboriginal giftedness comprised 90 percent of their attention.

One of the books Ofner surveyed was George M. Wrong's *History of Canada*, published in 1921. She quotes Wrong as depicting the Indians as "howling like wolves," and more "like hogs than men," bringing us back to the bestiality theme that Leacock had used. Other negative depictions include the "neglected" state of children, the "dirt and squalor" of ab-

original life, the "craze" which alcohol produced in the Indian male, and the exotic appeal of "the wild dancing of their naked women." Not surprisingly, Wrong depicted aboriginals as possessing "dim minds." Still a conservative Protestant at heart, Wrong pointed to the "pagan Indians" as remaining "for long years, in spite of Christian teaching . . . still savages"; however eventually, the "ultimate gift" for a Native convert was to have a "white man show his missionary zeal by standing as godfather" (Ofner 1982:50–51). Although Wrong's book was too late to specifically influence Leacock, the influence may have worked in the opposite direction. Certainly, it may have influenced Leacock's later 1941 book with The House of Seagram. In general, though, Ofner's study shows that the portrayal of aboriginal deficiency utilized by Leacock was widely influential during the entire period from 1857, but that it may have increased its stronghold on the Euro-Canadian mind in the decades from 1901 to 1941. No doubt, the counter-perspective of a Horatio Hale was either forgotten *in toto* or discredited as the *idée fixe* of an otherwise sound philologist and Iroquoianist.

Conclusion

Many readers will be fascinated to learn that Stephen Leacock wrote significant material on First Peoples at all. Many will be disconcerted to learn that Canadian's leading humorist wrote what now seems a less-than-funny narrative on this important topic, especially since his texts were intended to educate a popular Euro-Canadian readership. One imagines individual copies or sets of The Chronicles of Canada donated as graduation gifts to eagerly expectant youthful Canadians!

Nevertheless, to understand Leacock's themes in his narrative such as aboriginal deficiency, we must discuss the social context in which he was writing. Certainly this context was one in which both aboriginal deficiency and evolutionary stage theory loomed large. Certain alternatives did exist such as Horatio Hale's theme of aboriginal giftedness and his developing rejection of the evolutionary stage perspective. It was one perspective that a scholar might have known and at least taken into account. However Hale's themes had undoubtedly appeared as radical even in the years they appeared in the 1880s and 1890s. This was an era of Social Darwinism and the apex of European and European stem societies. Although Franz Boas and his circle started to challenge the evolutionary stage perspective and champion cultural pluralism and relativism, these ideas had little general support until the 1920s. In Canada, the prestige of British scholarship ensured the work of William Robertson and

Edward Tylor would remain influential, and the evolutionary stage perspective was carried on far into the twentieth century by other British scholars. In addition, the work of many popular intellectuals supported the theme of aboriginal deficiency. The work of Stephen Leacock and George Wrong represent just two of these. If Leacock had encountered the work of Horatio Hale or other challengers to the prevailing orthodoxy, it might have appeared as the work of a "crank" and deviant from the scholarly norm. Leacock's writing may differ from what we would write in substance; however, we are all influenced in our day and generation by prevailing hegemonic conceptions and that hegemony undoubtedly helps to explain Leacock's First Nations ethnography.

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5. *J. N. B. Hewitt*

Elisabeth Tooker and Barbara Graymont

At the beginning of an article published in 1913 in the first issue of *The Quarterly Journal of the Society of American Indians*, J. N. B. Hewitt posed this rhetorical question: "Should the ethnology of the American Indian be taught in the schools provided for the American Indian student?" He went on to answer it by stating that he believed "that anthropology, or at least, the elements of American Indian ethnology should be taught in such [Indian] schools and institutions. It has been his business for more than twenty years to collect and record information regarding the ethnology of the American Indian from the members of many American Indian tribes in North America, and it has been his experience, as it has been that of other investigators, that only a few persons in every tribe knew what the characteristic culture of his tribe was and is" (Hewitt 1913:30).

This knowledge of Indian culture and history, Hewitt asserted, should not be lost, for "there is no proof that the mental and the physical capacity of the American Indian race, as expressed in terms of past achievement and present ideals of accomplishment, is inferior to that of any other race of mankind. And the great body of brilliant facts to support this statement should be made the common heritage and property of every American Indian through judicious and effective instruction in schools which are devoted to his or her education" (Hewitt 1913:30–31).

Hewitt himself had been one of those Indians who learned little of his tribe's traditional history and culture while growing up on the Tuscarora reservation in western New York State. Both his parents were Christian, and on May 11, 1873, he himself joined the Tuscarora Presbyterian church, a church that until 1860 had been a Congregational mission church of the American Board of Commissioners for Foreign Missions. Accepting Western culture as superior in many ways, Hewitt remained an active church communicant throughout his life.

But Hewitt was also of "mixed blood." His father, although raised in

1. J. N. B. Hewitt, ethnologist and linguist. Not dated. Early 1900s. National Anthropological Archives, Smithsonian Insitution (#39).

a Tuscarora family, was of "white" descent. Hewitt, then, was conscious not only of his Indian "racial" heritage, but also his white one. As he wrote to Arthur C. Parker, at the time Secretary-Treasurer of the Society of American Indians, on September 6, 1912,

> In defining the status of the American Indian we should not overlook the fact that those of mixed blood inherit something from their white forebears; in other words, that the American Indian has a legal status and a blood status. In appreciating the attainments of an individual he must receive the credit for his accomplishments; but if he is of mixed blood, the American Indian is

> not, in my view, entitled to all the honor: a square deal makes it incumbent on us to recognize this fact in defining the status of the individuals of the American Indian race. . . . We must not confound the status created by law with that established by blood. (Hewitt 1912)

These themes are evident in Hewitt's life: his pride in both his Indian and white heritage and his desire to learn more about both. A member of the staff of the Bureau of American Ethnology for over a half century, he spent virtually his entire adult life studying the languages, culture, and history of the Iroquois and other Indians. At the time of his death in 1937, he was the leading anthropological authority on the Iroquois and had to his credit a number of monographs and articles, among the more significant of which are translations of texts. His most important legacy, however, is perhaps the unpublished manuscripts, many of them Iroquois texts transcribed in the native language, now in the National Anthropological Archives, Smithsonian Institution, Washington DC.

These texts are something more than examples of discourse useful in the analysis of language or accounts by Indians in their native language of their culture. To the extent that "civilizations" (a term that often denotes "societies having writing") define themselves by the great documents they have produced, so also may other cultures be said to define themselves by their oral equivalents, some of which members of these societies themselves reduced to writing after Western contact. For Iroquois these include the origin myth, or cosmology as Hewitt more aptly termed it, the Iroquois equivalent of the first chapters of Genesis, but longer; the Code of Handsome Lake, the account of the life and teachings of the Seneca prophet Handsome Lake, equivalent to the New Testament gospels of the Christian Bible; and the Thanksgiving Address, the speech that returns thanks to those various beings on this earth and above, as ubiquitous in Iroquois rituals as the Lord's Prayer is in Christian ones, but, like the cosmology, much longer. They also include the great oral documents of the League of the Iroquois: the traditional accounts of the founding of the League of the Iroquois and the liturgy of the Condolence ceremony, including the Roll Call of the Chiefs (the list of the names of those chiefs who made up the council of the League, grouped into classes by which they consulted among themselves in reaching decisions).

In doing this work Hewitt in some respects was both extending what other Iroquois had done, and were also then doing—recording in English or their native language for their own use Iroquois oral documents—and searching out the traditions of his own people. Horatio Hale, for ex-

ample, found two texts written in Mohawk (at the Six Nations reserve in Canada) and one in Onondaga (at the Onondaga reservation in New York State) of the speeches—more accurately, some of the speeches of the Condolence ceremony; these he published in 1883 in *The Iroquois Book of Rites*. In another instance, the Code of Handsome Lake was written out in the 1860s by John Jacket, who passed on the pages to Henry Stevens, who in turn passed them on to Edward Cornplanter, who lost the sheets. Cornplanter started to write the Code of Handsome Lake out again, but before he finished, Arthur C. Parker found what Cornplanter had already done and obtained the translation by William Bluesky of Cornplanter's dictation in Seneca, which Parker subsequently published (Parker 1913:7–8). In yet another case, the Tuscarora Indian David Cusick collected materials that he subsequently published in *Sketches of Ancient History of the Six Nations*, which was probably first printed in 1827.

At the same time, as an ethnologist in the employ of the Bureau of American Ethnology, Hewitt also addressed issues important in anthropology. Among them were the classification and description of North American Indian languages and the nature of religion. His work, then, was as rooted in the anthropology of his day as it was in the traditions of his own people that he recorded.

Early Life

John Napoleon Brinton Hewitt was born on December 16, 1857, on the Tuscarora Reservation in Niagara County, New York.[1] He was the eldest of seven children—five of whom survived into adulthood—of Dr. David Brainard Hewitt and his third wife, Harriet. His father also had a son, Alvis David Hewitt, by his first wife. Of Scotch and English descent, Dr. Hewitt had been orphaned as boy, adopted by a Tuscarora family on the nearby Tuscarora reservation, and raised as an Indian. After receiving—in the phrase of the day—a classical education, he had studied medicine. A skilled herbalist, who used both white medicine and traditional Indian remedies in the treatment of patients, he enjoyed a wide practice in Niagara County while continuing to live on the reservation.

Hewitt's mother was of Tuscarora, French, English, and Oneida descent. Her maiden name was Printup, possibly a variant of the original French Printemps. Her family appears to have been descended from William Printup, Jr., who had an Indian son and who was Sir William Johnson's most skilled Indian interpreter and blacksmith (Lawton and Printup 1989:xiii–xiv, xvi–xviii, 6–20, 22–24). An enrolled Tuscarora, she was

the daughter of Chief Jonathan Printup and his wife, Eliza Mt. Pleasant, a sister of the noted Chief John Mt. Pleasant and member of the White Bear lineage of the Bear Clan.

Although both Dr. Hewitt and his wife spoke Tuscarora fluently, they used English in the home. Taught to read and write by his parents, John did not go to the reservation district school near his home until he was eleven, and it was there that he picked up from his classmates a knowledge of the Tuscarora language. He next attended the local white school just off the reservation and, at the age of sixteen, he entered the union school in Wilson, New York, where he studied for two years. Hoping to go to college, he next attended the union school in Lockport, but overstudy and a sunstroke so affected his health that he was unable to finish his last term there. Returning to the reservation, he became a farmer and newspaper correspondent, and also established a private night school for Tuscarora men in the Mt. Hope district schoolhouse on the reservation. He might have remained on the reservation if he had not, in 1880, met Erminnie A. Smith.

Assistant to Erminnie A. Smith, 1880–86

Erminnie A. Platt was born in Marcellus, New York, not far from the Onondaga reservation in central New York State, in 1836. In 1855, two years after graduating from Emma Willard's noted Troy Female Seminary, she married Simeon H. Smith, living in Chicago until 1866, when they moved to Jersey City, New Jersey. She had four sons, whom she took to Europe for their education, and while there she herself studied French, German, and geology. In 1876, after returning home, she founded the Daughters of Aesthetics, also known as the Aesthetic Society, an association of women interested in the study of science, literature, art, and the domestic arts—home beautification and hospitality among them. That same year she also joined the American Association for the Advancement of Science (AAAS).

Mrs. Smith was a cousin of the noted anthropologist Frederic Ward Putnam, then curator of the Peabody Museum at Harvard University and permanent secretary of the AAAS. Her interest in anthropology, however, seems to have been a consequence of meeting John Wesley Powell, a geologist like herself. Just when and where they first met is not known. It might have been at a meeting of the AAAS. Both attended the meetings of the AAAS in Saratoga, New York, in August 1879 (where she gave her first two papers to the AAAS—both on mineralogy—at these meetings), and Powell may well have talked with her there, if he had not before, about the Bureau of Ethnology he had established a few months before.

Whatever the case, Powell, who like other anthropologists of the time was particularly interested in North American Indian languages and mythology, offered her the support of the Bureau for a study of Iroquois folklore and languages, and in May 1880 she left Jersey City for an extended research trip to New York State. Stopping in her old hometown, she made a brief visit to the Onondaga reservation, and then went on to the Tuscarora reservation near Niagara Falls where, at Powell's request, Lewis H. Morgan had arranged for her to stay with Caroline Parker Mt. Pleasant, a sister of Ely S. Parker, Morgan's principal interpreter and collaborator, in the fall of 1845 on his first visit to the Tonawanda Seneca reservation.

Mrs. Smith undoubtedly met Hewitt, grandnephew of John Mt. Pleasant, through the Mt. Pleasants. She promptly engaged him as her assistant. After collecting folktales and information on the Tuscarora language, Mrs. Smith—accompanied by Hewitt—went to the Cattaraugus Seneca reservation for the same kind of information. At Powell's suggestion, she attended the AAAS meetings in Boston in August, delivering two papers, one on Iroquois mythology and the other on Iroquois languages, and then returned to New York State for more research.

In the following years, with the continued financial support of the Bureau, Mrs. Smith repeated this pattern of work: accompanied by Hewitt, field research in the summers among the Iroquois of the United States and Canada, with an interruption in August to attend the meetings of the AAAS; winters in Jersey City, partly spent writing up materials. Her monograph "Myths of the Iroquois" was published in the Second Annual Report of the Bureau in 1883, and in 1885 she was elected secretary of Section H (Anthropology) of the AAAS.

Hewitt's work followed somewhat the same pattern as Mrs. Smith's: summers devoted to field research, part of the time with Mrs. Smith; winters in Jersey City analyzing materials. In the winter of 1881–82, Hewitt also collected for Powell information on Creek culture from two Creek Indians then visiting Washington: Legus F. Perryman and General Pleasant Porter (Hewitt 1939:123–124). By at least mid-1882, while still working with Erminnie Smith, Hewitt was also employed as a streetcar conductor with the Jersey City Railways Company. During 1884–85 he was a weigher with the Jersey City Abattoir Company and from 1885 to 1886 he worked with the Adams Express Company. He was in Jersey City when Mrs. Smith died on June 9, 1886, after a relatively brief illness.

During these years, Mrs. Smith and Hewitt had been working on a Tuscarora-English dictionary, a project that had its beginnings in 1880, but was greatly expanded in 1881. Late in 1885, Mrs. Smith had submitted

the dictionary along with an outline of a Tuscarora grammar to the Bureau for publication. On June 11, 1886, two days after Mrs. Smith's death, Hewitt wrote to Powell applying for the support of the Bureau to complete this work. Exhibiting an attitude that was to prove characteristic of him, Hewitt observed in this letter that he was of the opinion that the dictionary, having been hurriedly prepared, contained too many errors, that the grammar was too superficial a treatment, and that the dictionary—by not analyzing the words into roots and "increments" (prefixes and suffixes)—was both incomplete and inadequate. Powell hired him to do this work, and Hewitt moved to Washington DC, beginning his employment at the Bureau on July 20, 1886.

Work at the Bureau under Powell, 1886–1902

The Bureau of Ethnology, renamed the Bureau of American Ethnology in 1894, grew out of the four geological surveys funded by the federal government after the Civil War. One was headed by Powell, the other three by George M. Wheeler, F. V. Hayden, and Clarence King, respectively. The King survey produced virtually no information on the Indians; the others did.

Powell's interest in anthropological research may well have originated in the suggestion of Joseph Henry, who in 1868 suggested to Powell that while he was in the west he collect ethnographic and linguistic data. Towards this end, Henry furnished Powell with a copy of George Gibbs's *Instructions for Research Relative to the Ethnology and Philology of America* published by the Smithsonian in 1863. On this and later expeditions Powell collected data on vocabulary and grammar and on ethnography and mythology. Then, in 1876–77, perhaps foreseeing the day when the geological surveys would be consolidated and himself denied a government position, Powell extended the ethnographic and linguistic work of his survey. In 1876, he asked for and received from Joseph Henry the Indian vocabularies then at the Smithsonian, including those that Gibbs had been preparing for publication at the time of his death in 1873. The next year (1877), he published An *Introduction to the Study of Indian Languages*. He also hired James Owen Dorsey and Albert S. Gatschet to do linguistic work. That same year, at Powell's suggestion and to avoid duplication of the collateral work of their respective surveys, Powell suggested that he take the ethnological work and Hayden take the work in natural history. Hayden had no other recourse but to agree.

Hayden had hoped to become director when in 1879 the geological surveys were finally consolidated into the United States Geological Sur-

vey (USGS), but Clarence King was appointed to the position. At the same time, Powell became director of the newly established Bureau of Ethnology.

As Powell envisioned it, the work of the Bureau was to be an expansion of the anthropological studies he had undertaken earlier and was to include all aspects of Indian life—culture, language, and history. Some projects, such as the linguistic map (Powell 1891) and the *Handbook of American Indians North of Mexico* (Hodge 1907–10), involved virtually all members of the staff; others were undertaken by individual researchers. Powell as director ran the Bureau much as he earlier had his noted geological survey, deciding what was to be done, how, and by whom. Little changed when in 1881 Powell also became director of the United States Geological Survey.

Hewitt's work at the Bureau necessarily reflected Powell's interests and was under his direction. In 1887, the year after he came to the Bureau, Hewitt undertook for Powell a comparative study of Cherokee and the Iroquois languages that firmly established the relationship, long suggested, between these languages. The following year (1888) he went into the field, spending seven weeks on the Tuscarora reservation and seven on the Grand River Reserve in Canada collecting texts and other linguistic materials, thus extending the work he had done for Mrs. Smith. He returned to the field the next year, spending seven weeks on the Onondaga reservation in New York State and nine on the Grand River Reserve. While continuing to work on the Tuscarora-English dictionary and the texts and other materials he had collected, he also studied the works of the early French writers on the Iroquoians: Champlain, Lafitau, Charlevoix, and the Jesuit Relations.

A year after coming to the Bureau Hewitt also joined the Anthropological Society of Washington, and when that society began publication of the *American Anthropologist* (old series) in 1888 he contributed a number of brief notes. In 1892 he published in this journal his first long article, "Legend of the Founding of the Iroquois League," a translation of the traditional account of the founding of the League that he had obtained in 1888 from John Skanawati Buck at Grand River. Two years later, in 1894, he published in the same journal "Era of the Formation of the Historic League of the Iroquois," a discussion of the date the League was established; Hewitt concluded that it was probably between 1559 and 1570.

The collection and translation of texts in the native language necessitates study of the language itself and provides materials for such studies—a task to which Hewitt devoted considerable time. In 1893, Hewitt

presented some of the results of his study of the grammar of the Iroquois languages in a paper delivered at the AAAS meetings, "Is the Polysynthesis of Duponceau Characteristic of American Indian Languages?" and in an article, "Polysynthesis in the Languages of the American Indians" covering the same ground published two months later in the *American Anthropologist*. At issue was the suggestion of Peter Duponceau earlier in the century that American Indian languages were characterized by a common ground plan—what he termed "polysynthesis"—having a quite different character than those of the Old World: a chaotic style "in which the greatest number of ideas are comprised in the least number of words," among other things, not simply by joining two words together or by inflection but "by interweaving together the most significant sounds or syllables of each simple word, so as to form a compound that will awaken in the mind at once all the ideas singly expressed by the words from which they are taken" (Hewitt 1893d:381). Hewitt found no such chaotic style, and in his opinion Duponceau had based his notion of polysynthesis on a too superficial study of the morphologies of Indian languages.

At the time, the three leading anthropologists in the country were Powell, Frederic Ward Putnam, and Daniel G. Brinton. Both Putnam and Powell were natural scientists whose field experiences had led them into anthropological studies: Putnam into archaeological and Powell into linguistic and ethnological ones. Brinton had no such field experience; he did, however, have a better formal education: a BA from Yale College and an MD from Jefferson Medical College in Philadelphia. If there had been jobs available in anthropology after the Civil War, Brinton might well have taken such a position, but there were not and Brinton became editor and later also publisher of a weekly medical journal. After receiving his medical degree in 1860 he had spent a year abroad, studying in Paris and Heidelberg, and later, in connection with his medical writing, frequently returned to Europe.

Brinton had come to anthropology not because of an interest in natural history, but of one in literature—a concern expressed in his interest in mythology and in language, especially in texts in the native language written by natives themselves. Mythology and language were also Powell's particular interests, and as Powell and Brinton each wished to be regarded as the leading anthropologist in the country, a rivalry developed between the two. In fact, it may be that Hewitt wrote on "polysynthesis" at Powell's suggestion and in vindication of Powell.

In his 1885 article "Polysynthesis and Incorporation as Characteristics of American Indian Languages" Brinton had faulted Powell's knowledge of linguistics. Therein, after first discussing the ideas of Duponceau,

Wilhelm von Humboldt, Francis Lieber, and Heymann Steinthal on the structural processes characteristic of American Indian languages, Brinton observed,

> Finally, I may close this brief review of the history of these doctrines with a reference to the fact that neither of them appears anywhere mentioned in the official "Introduction to the Study of Indian Languages" issued by the United States Bureau of Ethnology! How the author of that work, Major J. W. Powell, Director of the Bureau, could have written a treatise on the study of American languages, and have not a word to say about these doctrines, the most salient and characteristic features of the group, is to me as inexplicable as it is extraordinary. He certainly could not have supposed that Duponceau's theory was completely dead and laid to rest, for Steinthal, the most eminent philosophic linguist of the age, still teaches in Berlin, and teaches what I have already quoted from him about these traits. What is more, Major Powell does not even refer to this structural plan, nor include it in what he terms the "grammatic processes" which he explains [Powell 1880:55]. This is indeed the play of "Hamlet" with the part of Hamlet omitted! (Brinton 1885:59)

In his article Brinton had given his definitions of "polysynthesis" and "incorporation." These Hewitt quoted in his own article on polysynthesis, and then observed, "All this doubtless has a certain plausibility so long as it is tested solely by the faulty and equivocal works of the pioneers in American Indian philology; but, by the light of the facts of language which are gradually being made available, these polysynthetic dogmas are being dissipated" (Hewitt 1893d:393). The remaining half of the article Hewitt devotes to a discussion of the inadequacies of Brinton's ideas.

In an angry, intemperate reply to Hewitt's paper in *The American Antiquarian*, "Characteristics of American Languages," Brinton suggested that Hewitt would not have written what he had if he had studied the works of the German linguists, particularly those of Humboldt and Steinthal, adding, "as I observe no reference to any of the numerous German writers who have touched the question—is it that Mr. Hewitt is unfortunately unacquainted with the German language? It would be a sad plight for so slashing a critic! It would certainly be profitable to him to rest awhile on his arms, and learn that tongue; an ignorance which incapacitates any man from acquiring a knowledge of the Science of Language" (Brinton 1894:34).

Hewitt did not let Brinton's comment pass unnoticed. In "Grammatic Form and the Verb Concept in Iroquoian Speech," a paper delivered at the AAAS meeting the following year, Hewitt took up Humboldt's and Steinthal's ideas concluding—as might be expected—that contra Humboldt and his followers, "There are true grammatic forms in these [American Indian] dialects and they possess true verbs; the grounds upon which these have been denied are not sound, being due to misinterpretation of certain facts of grammar" (Hewitt 1896b:252).

Hewitt's original paper and its confirming comments by J. Owen Dorsey on Siouan and Athapaskan, however, went unnoticed.[2] Part of the reason was, perhaps, Brinton's stature and the authority of German scholarship; part also because so few had the knowledge to judge the merits of Hewitt's observations and the weaknesses of Brinton's.

Also involved in this dispute between Bureau men and Brinton was a somewhat different attitude toward acquisition of knowledge. Powell was most interested in the analysis of new data he had acquired in the field. Largely self-taught in natural history, he put little stock in formal education, hiring men more for their demonstrated ability than for previous training. Brinton, on the other hand, had almost no field experience, and put his faith in what others had written on both whites and Indians. Given this bent, it is not entirely an accident that in 1886 Brinton became the first professor of anthropology in the United States (at the University of Pennsylvania) and advocated university training. Powell regarded apprenticeship to be of more importance.

Perhaps his experiences in his early years at the Bureau contributed to Hewitt's skepticism regarding the necessity of formal education before undertaking research. As he expressed it in a letter to Laura Cornelius, a Wisconsin Oneida, dated March 1, 1902,

> [Y]our having only a Seminary education should not in the least lessen your worthy ardor or curb your noble ambition. When I came here that was the whole of my school education. But, from the very nature of my work I have acquired information not known or taught in the schools, truths that it is rank heresy to repeat, but which shall in its own time be accepted as the truth. A college education is in most cases a valuable training—and it is never anything more—to do excellent and original work in scientific research, but in the others it becomes a leaden cowl, making its wearer a narrow-minded bigot and pedant fully knowing the full sum of human and divine knowledge. (Hewitt 1902a)

In a letter dated April 5, 1902, Hewitt returned to the subject:

> I mean in saying that some who pursued original research outside the methods prescribed by College discipline often gained truths not found in the College text-books is that generally speaking the College insists too much on established authority and is often apt to generate a feeling of caste in those who have had the good fortune to graduate from its walls, both of which tendencies are incompatible with human progress in the attainment of truth and justice. . . .
>
> However, it must not be forgotten that Colleges conserve and instill knowledge, though they do not usually produce it, any more than does the waterwheel cause the flow of the mill-race. But with a careful avoidance of the ultra conservative tendencies which I have mentioned, a good college education is a great advantage to the real student and investigator who is never deterred from crossing the Rubicon dividing a mere authority from truth and justice. (Hewitt 1902b)

There are, of course, many reasons to study languages. One of these, more prominent in nineteenth century thought than in the twentieth century, concerned the meaning of words: the meaning of certain words, including their etymology, can provide insights into the fundamental nature of religion and even the origin of religion itself. Hewitt, too, believed that a people's religion and culture could not be understood without a knowledge of their language. Throughout his life, Hewitt devoted considerable effort—what today would seem inordinate amounts of time—to ascertaining from knowledgeable Indian sources the meaning of words and also to the etymology of words. These concerns are evident in three papers he published between 1895 and 1902: "The Iroquois Concept of the Soul" (1895), "The Cosmogonic Gods of the Iroquois" (1896), and "Orenda and a Definition of Religion" (1902).

Of these three articles, the best known is the last, "Orenda and a Definition of Religion," because it dealt with a presumed earlier form of religion. As Hewitt saw it, the "savage mind" inferred a mystic or magic potence that was the property of all things and bodies, a property that was the efficient cause of all activities (Hewitt 1902:36). This magic power was called by different names in different languages (for example, "wakan" in Siouan languages and "manitou" in Algonquian ones), but Hewitt proposed it be designated by the term "orenda"—what he believed was the Huron form of the Iroquoian word. Basing his ideas in part on Hewitt's article, R. R. Marett (1909) later labeled the phenomenon "anamatism": the attribution of life—or, better, supernatural power—to inanimate

objects, or the belief that (pure) supernatural power may reside in objects—what he saw as an earlier form of religions than what Edward B. Tylor (1871:2,8) termed "animism," defined as the "belief in spiritual beings." As an example of "anamatism," Hewitt's ideas regarding orenda received even wider recognition than they probably would have otherwise.

During these (and subsequent) years, Hewitt was also engaged in other work for the Bureau. Over the years the Bureau had accumulated a number of manuscripts in its vaults, which in 1893, J. Owen Dorsey (assisted by Hewitt) began to arrange and classify and to prepare a catalogue of them. After Dorsey's death in 1895, that work devolved onto Hewitt alone. It was, as Hewitt later remarked, a "difficult and tedious task" given that the manuscripts "were in a majority of instances unsigned, undated, and unidentified" (Baldwin 1914:148). Hewitt was increasingly given the task of answering inquiries addressed to the Bureau, a number of which concerned matters relating to language and hence were within his special area of expertise.

One of the greatest tasks of the Bureau in the 1880s had been the classification of the languages of North American Indians, what Powell regarded as an updating of Gallatin's (1836, 1848) classification. After Gallatin's death in 1849, William W. Turner took on himself the task of furthering Gallatin's work, a task that was taken up by George Gibbs after Turner's death in 1849, and by Powell himself after Gibbs's death. The interest of the Bureau in the subject, however, did not end with the publication of Powell's own classification in 1891. In 1894, Hewitt wrote a long report on the relationship of the Shahaptian (Sahaptin–Nez Percé) languages to the Waiilatpuan (Molale-Cayuse) family and another on the relation of these languages to the Lutuamian (Klamath-Modoc) family, coining Shahapwailutan for the whole group (renamed Plateau Penutian in the Sapir classification). Shortly after, to test Cyrus Thomas's idea that the Maya languages were related to those of the Malayo-Polynesian stock, Hewitt made a comparison of these languages, writing a long report showing no relationship. For W J McGee, he wrote a comparison of the Yuman, Serian, and Waicuri languages published by McGee in his *The Seri Indians* (Hewitt 1898a).

In 1896, Hewitt returned to the field and in each of the next four years spent a number of months in the field collecting linguistic and other materials. Among the texts he obtained were some long ones recording how "this earth, this island on the turtle's back" came to be and the subsequent exploits of the twin brothers, the good older brother and the evil younger twin. In 1902, Hewitt completed and sent to press the first part

of his two-part "Iroquoian Cosmology," the text and translation of three of these: an Onondaga version he had obtained from John Buck in 1889 at Grand River, revised and somewhat enlarged in 1897 with the aid of John Buck's son Joshua; a Seneca version obtained in 1896 from John Armstrong at the Cattaraugus reservation, revised with the assistance of Andrew John; and a Mohawk version obtained in 1896–97 from Seth Newhouse at Grand River. At the time he was also preparing for publication a long Onondaga version he had obtained in the winter of 1899–1900 from John Arthur Gibson at Grand River. He might have finished it except for the death of Powell that year (1902).

Work at the Bureau under W. H. Holmes, 1902–1909

In 1893, after Henry W. Henshaw had resigned for health reasons, Powell brought over W J McGee, like Powell a self-trained geologist, from the USGS to take over the day-to-day administration of the Bureau. The next year (1894) Powell resigned as director of the USGS, but then in declining health and most interested in writing more philosophical works on science, he left the running of the Bureau to McGee. McGee expected (and was expected by others) to succeed Powell as head of the Bureau on Powell's death, but Samuel P. Langley, Secretary of the Smithsonian, had no intention of appointing him to the position. His choice was William Henry Holmes, artist, geologist, and archaeologist, who was then head curator of the Department of Anthropology, United States National Museum. So Holmes it was.

The first secretaries of the Smithsonian, Joseph Henry and Spencer F. Baird, encouraged anthropological research. The next two secretaries, Langley and Charles D. Walcott (who succeeded Langley in 1907), did not. Langley believed that the Bureau ought to be doing work of more practical value than it had been and proposed a revision of a project that had begun as a synonymy, that is, a list of names by which the various North American Indian tribes has been known and referred to in the literature. Henshaw had been in change of this project as well as that of the linguistic map (the Powell classification) and when he resigned, it was taken over by Frederick Webb Hodge. By this time, it had grown to include much ethnographic information on the various tribes, and Hodge renamed it the "Cyclopedia of the American Indians."

As originally planned, the synonymy/cyclopedia was to be a series of volumes, each devoted to a single language family. Langley proposed that the number of volumes be reduced to two and that the scope of the subject matter be expanded to include not just description and location of

the various tribes, confederacies, bands, and settlements, but also biographies of noted Indians and essays on topics of special interest. Hodge was assigned half-time and later full-time to the task of editor of what was now called the *Handbook of American Indians North of Mexico* and the members of the Bureau staff, including Hewitt, turned much of their attention to writing articles for it. At the same time, the number of contributors was enlarged to include anthropologists not on the staff of the Bureau.

In January 1907, with volume 1 of the *Handbook* virtually finished (it was published later that year), Hewitt went into the field for three months collecting texts in Onondaga and Mohawk pertaining to various aspects of the League of the Iroquois. After his return, he worked on the translation of these texts for what he now envisioned would be a monograph on the founding, constitution, and structure of the League. He also worked on a grammatical sketch of the Iroquois languages for the *Handbook of American Indian Languages* that Franz Boas was editing for the Bureau, a description promised several years before but not yet finished. He continued work on these and other projects as well as the second volume of the *Handbook* (published in 1910) for the next several years.

Work at the Bureau after Holmes, 1910–37

Hewitt was, as Swanton (1938:289) observed, "blessed with a quiet, even disposition and an attractive personality. He was always an interesting conversationalist, possessed of a keen sense of humor, and a rich fund of anecdote." He was also, however, noted for procrastination. As Swanton put it,

> he was not the type to be hurried. He had to do things in his own way, and his own way consisted largely of detours. Set upon any job, there was always some good and sufficient reason why it could not be completed immediately. Equally plausible was the excuse brought forward [the] next day, next week, or next year. . . . If, however, anyone attempted to discuss things Iroquois Hewitt's defense went over into the offensive with great promptitude. His adversary might well complain that he had to do the best he could because of Hewitt's failure to give what might well have been the orthodox version. On such an occasion Hewitt might be stimulated to promise that his conclusions on this phase of Iroquois life were "soon to appear" after which he continued on as before. . . . His criticisms of an opponent were most peculiar. He would never call anyone a liar or a fool in good

> old United States but approached the subject through a most delightful and characteristic maze of verbiage. "Mr. Jones had unfortunately exhibited inhibiting tendencies toward the facts in the case which should have been evident even to highly abbreviated intelligencies [*sic*]." If in conversation or in any other way you trenched upon his areas of belief, he might not dissent immediately, and the first intimation of your trespass might come through some remote member of the staff to whom Hewitt had communicated his grievances (Swanton n.d.:46).

Some years before, W J McGee had said, "Hewitt is handicapped by unsatisfactory literary methods and practically no ability to express himself orally, yet he has an enormous fund of information and he not only possesses the body of facts in his mind, but he has ability to adjust them, and does adjust them in groups, in classes, in such manner as to make his general judgment concerning any linguistic subject of high value; I am also tempted to say of the highest possible value." But, McGee added, "As I say, he is handicapped in several ways. In the first place, he is exceedingly slow in preparing matter for publication, and in the second place, he does not do himself justice when he prepares matter for publication. Beyond that, as I have intimated, he is almost wholly lacking in the power of expressing himself, either before an audience or in conversation. The only way to find out anything from him is to go to see him in his den and talk to him" (McGee 1903).

If Hewitt was notoriously slow in preparing the results of his research for publication, part of the reason was his obsession with accuracy. This obsession probably had many sources. On was the ordinary scientific desire to get things right; such work, after all, is measured by depth, scope, and accuracy. But another reason may well have stemmed from his consciousness of his Indian ancestry. It is, of course, one thing to write about another people; quite another to write about one's own, particularly about matters so central to the society and culture as Hewitt did—among them, the cosmology and the League. Here accuracy did matter. Hence, perhaps, Hewitt's constant desire to collect more data. Once gathered these data presented yet another difficulty: describing them in a way that would make sense to those unfamiliar with Iroquois language and culture. The task was not an easy one, and some complaints about Hewitt's writing undoubtedly have their source in this fact.

Then, too, in the 1880s when Hewitt joined it, the Bureau was guided by Powell's vision of what its specific tasks should be. It mattered little who did this work as long as it got done. And although it was anticipated

that the results of the research done would be published, the emphasis was on the collection of data which, given the small size of the anthropological community at the time, might be left in manuscript form.

In the 1890s this situation began to change. The growth of universities and museums opened up jobs, and this increased demand for qualified personnel led to the introduction of graduate training. (The first graduate students in anthropology enrolled in 1890; two at Harvard and one at Clark University). One measure of the worth of these new academic entrepreneurs was the number of their publications.

Hewitt, however, had originally been hired for his linguistic abilities, not on any demonstrated ability to produce publishable manuscripts. (Hewitt later characterized his position under Powell as "adjuvant secretary.") His shorter, synthetic publications had a mixed reception. His "Orenda" paper (1902) was widely noticed, but "Polysynthesis" (1893) had been attacked by Brinton. Franz Boas had praised Hewitt's abilities highly in his 1909 article, "Notes on the Iroquois Language," which compared his own transcriptions of Mohawk with those of Hewitt. Although Boas did not note the reasons for the variance in these two transcriptions, Hewitt had recorded the Mohawk language basically phonemically, whereas Boas had transcribed the material from his informants phonetically. Hewitt had also done his fieldwork at Six Nations while Boas had worked at St. Regis (Akwesasne), where there would have been dialectical differences.

What mattered most to Hewitt's employers, however, was the amount of published material he produced for the Smithsonian's Bureau of American Ethnology, which was paying his salary. By this measure, Hewitt fell short. In an effort to make him more productive, Charles Walcott took Hewitt off salary effective August 1, 1909. He was to be paid by the number of words he wrote. (Later he was also given a small monthly stipend for his work as the custodian of the Bureau's manuscripts.) At the time he was taken off salary and put on contract, he had written over half of some sixty entries that he contributed to the second volume of the *Handbook*.

Deeply stunned and offended by the change in his status, Hewitt began writing protest letters to some of his prominent friends. In response to one of these letters, Hewitt's good friend, Jean Baptiste Bottineau, father of Marie L. Baldwin and a Chippewa lobbyist in Washington DC, had independently appealed to United States Senator Charles Curtis, himself an Indian from Kansas, to come to the rescue of Hewitt in this salary matter.

On January 7, 1911, Senator Curtis wrote an inquiring letter to Wal-

cott on the matter of Hewitt's salary. Walcott very politely replied on January 16, noting that he had looked very carefully into the matter at the time, "and it was not until I was fully convinced that Mr. Hewitt was not rendering adequate return for the salary granted him that I felt impelled to alter the manner of payment by compensating him for the labor which he actually performed" (Walcott 1911).

Walcott then went into detail with his list of grievances in regard to Hewitt's dilatoriness in preparing his assignments, concluding with the item that had probably exhausted his patience: Hewitt's failure to provide his assigned chapter on the Iroquois languages for the Boas *Handbook of American Indian Languages*:

> Mr. Hewitt has accumulated a large fund of information, but the trouble has been getting it out of him. He has had every opportunity to present the results of his studies for publication, but he seems to be so constituted that although he apparently devotes very much time to his work, he lacks that systematic application necessary to the production of results. As an example, I may mention that as far back as a couple of years before the change in his status became necessary, or at least four years ago, Mr. Hewitt was directed to prepare a chapter on the Iroquois language (a subject which he has at his fingers' ends) for the Handbook of Indian Languages, which the Bureau of American Ethnology is publishing, but up to the present time not a line of it has been presented, although other members and associates of the Bureau furnished similar chapters many months ago. This is only a single instance of many that could be cited, yet Mr. Hewitt expects to be compensated at a higher rate than others of the Bureau whose work is prolific of results.

It was therefore Walcott's seasoned judgment that Hewitt, under the circumstances, had not been unjustly treated and certainly could not "be compensated for what he does not do. The present basis of payment is a liberal one, and there is no reason, aside from the apparently fundamental lack above mentioned, why, under existing conditions, he should not earn fully as much salary as he has ever received."

So the matter stood.

Holmes resigned as chief of the Bureau in 1909 and Frederick Webb Hodge replaced him as "ethnologist-in-charge" on January 1, 1910. Later that year, the second volume of the *Handbook* was published and Hewitt turned his attention to editing the Seneca legends and myths that Jeremiah Curtin had collected on the Cattaraugus reservation in 1883,

1886, and 1887, to which he added those he himself had collected at Cattaraugus. He also returned to writing his sketch of the Iroquois languages for Boas's *Handbook*, a task he never completed.

On February 1, 1914, Hewitt was restored to salary, albeit at an amount less than what it had been in 1909.[3] The long Curtin-Hewitt manuscript "Seneca Fiction, Legends, and Myths" was submitted for publication in October (it was published in 1918) and in December Hewitt returned to the field, visiting the Grand River Reserve for a month, and taking up again the project he had begun years before: the study of the League of the Iroquois. He returned to the field almost every year thereafter, collecting and revising his materials.

Hewitt's renewed research into matters concerning the League led him to give a paper at the International Congress of Americanists held in Washington in December 1915 on "Some Esoteric Aspects of the League of the Iroquois," a discussion of some of the principles embodied in the organization of the League. At the time he was also translating the text of the Requickening Address (a part of the Condolence ceremony for the raising up of League chiefs) he had obtained from John Arthur Gibson; it was published in 1916. Then, in March 1917, Hewitt attended a Condolence ceremony at Munceytown as an official delegate of the Grand River Six Nations council, where he intoned the Requickening Address in Onondaga (presumably the version he had recently translated) and also acted the part of the Seneca chiefs in the ceremony. In 1920, he published "A Constitutional League of Peace in the Stone Age of America: The League of the Iroquois and its Constitution"—an article that included a discussion of Iroquois kinship organization as well as much of the material in his earlier "Esoteric Aspects" paper. But except for his "Status of Women in Iroquois Polity before 1784" (1933), a revised version of "The Requickening Address" (1944) submitted for publication in 1936 and edited by William N. Fenton after Hewitt's death, and an unfinished paper "Mnemonic Pictographs of the Iroquois" revised by Fenton and published in 1945, Hewitt's study of the Iroquois League remains in manuscript form only.

In 1918, the year Jesse Walter Fewkes became Chief of the Bureau, Hewitt was appointed the Smithsonian's representative to the United States Board of Geographic Names, a position he held for the remainder of his life. Also that year he returned to working on the second part of his *Iroquoian Cosmology*. It was completed in 1923 and published in 1926. He next edited Edward Thompson Denig's *Indian Tribes of the Upper Missouri*, published in 1930. In January 1931 Matthew W. Stirling, who had succeeded Fewkes as head of the Bureau in 1928, asked Hewitt to

edit the *Journal of Rudolph Friederich Kurz*. It was completed in 1932 but not published until 1937, the year of Hewitt's death.

Life in Washington

Hewitt had joined the Anthropological Society of Washington in November 1887 and remained active in it for the remainder of his life, serving as its treasurer from 1912 to 1926 and its president from 1932 to 1934. He was also a charter member of the American Anthropological Association, founded in 1900.

Living as he did in Washington, Hewitt met a number of other Indians, both those who visited and those whose residence was more permanent. One of these was Andrew John, a Seneca Indian, who was frequently in Washington lobbying for his people. When the Vreeland bill was introduced in Congress in 1902—a bill that would have allotted the lands of the Seneca Nation and the Tuscarora reservation—Hewitt did considerable research on his own time for John who, among others, was trying to defeat the measure.

Hewitt also knew J. B. Bottineau, a lawyer and Chippewa living in Washington, and an active champion of the Turtle Mountain Band of Chippewas. After Bottineau died in 1911, if not shortly before, Hewitt moved into his house. He lived there for several years until Bottineau's daughter Marie L. B. Baldwin decided to try to rent or sell the house. Baldwin, who was an accountant in the Education Division of the Bureau of Indian Affairs, had graduated from the Washington College of Law and was admitted to the Bar of the District of Columbia in 1914 (Houghton 1918:172–173). When Hewitt received the Cornplanter Medal for Iroquois Research from the Cayuga County Historical Society in 1914, Baldwin wrote a biography of Hewitt for *The Quarterly Journal of the Society of American Indians*.

Baldwin and Hewitt were both active members in the Society of American Indians—an association whose members were largely progressive educated Indians. (Hewitt served for a time on its executive council.) The Society's aim was the advancement of the Indian by, among other things, instilling racial pride, promoting education, and supporting fair treatment of the Indian people. In keeping with these ideals, Hewitt contributed an article on "The Teaching of Ethnology in Indian Schools" to its *Quarterly Journal* in 1913. The year before, he had delivered the commencement address at the Carlisle Indian School, "The Indian's History—His Ideas; His Religion; His Mythology; His Social Organization," which was published in the school's journal *The Red Man* that year.

At the time Hewitt was a member of the Ingram Memorial Congregational Church, apparently having joined not long after it was founded in 1908 or after the building was built in 1910. Hewitt was, as Swanton (1938:288) observed, "deeply religious and profoundly interested in [and] versed in the results of Biblical scholarship." He taught an adult Sunday school class at the church, was a deacon, and on at least two occasions preached from the pulpit. He remained a member of this church until 1925 when he joined All Souls Unitarian Church, where he became active in its Layman's League. He was also a member of the La Fayette Lodge of Masons, which he joined in 1932.

Hewitt's 1882 New Jersey marriage to Agnes Bridget Naughton seems to have dissipated. No further record of her or their son, David, could be found; although, it appears that, at least for a time, she was still alive and that she and Hewitt were separated. By 1900, and possibly by 1895, Hewitt had formed a connection with Catherine Stuart, a native Washingtonian with whose family he was familiar. The 1900 U.S. Census for the District of Columbia shows Hewitt and Catherine living at the same address, where she is listed as Catherine Hewitt. They both indicated that they had been married five years.[4] In 1901, however, when Hewitt filed his Kansas Claim petition, he listed his wife as Agnes and his son as David Manton, age eighteen, born October 20, 1883.[5]

The 1910 Census for the District of Columbia shows Hewitt and Catherine living at another address where he is listed as married and she is listed as single. Although no marriage record for Hewitt and Catherine has been found, they may have been married at a later date or in a locale other than Washington DC There is no doubt that he considered her his wife, spoke of her in that category in his letters, and took her with him on his ethnographic field trips.

Sometime after 1910, Catherine began to have health problems that worried Hewitt. He thought that a vacation from Washington and work would do her much good, and he asked his kinsman Matthew Johnson at Tuscarora to find a place for her to room and board on the reservation. Sophronia and Samuel Thompson took her in for a couple of months and received great thanks from Hewitt for their care of her.

By 1917, Catherine was seriously suffering from what her doctor described as "congested liver." From July on, she was nearly helpless, confined to bed, and had to be lifted and fed. Hewitt evidently had a caretaker for her during the day and cared for her tenderly himself when he came home from work.[6] In early March 1918, Hewitt transferred her to the George Washington University Hospital, where, after eleven days, she died on March 20, 1918, of liver cancer and asthenia.[7] On March

21, Hewitt arranged a private family funeral and had her buried in the Stuart family plot in Mount Olivet Cemetery, beside her parents.[8] Deeply affected by Catherine's death and overcome by his endeavors to catch up with his scholarly work that had languished during his care for her, Hewitt had a series of nervous breakdowns—one shortly after her death that lasted ten days, and later two others, each of two months' duration.

After he had recovered from Catherine's death, Hewitt, in his quiet, diffident way, began courting May S. Clark, the office clerk and ethnographer-typist at the BAE. Although Miss Clark valued Hewitt's friendship and occasionally corresponded with him when he was away on his field trips, she had no intention of entering into a romantic relationship with him (Laird 1975:85, 149–150).

In 1925 Hewitt married Carrie Louise Hurlbut and moved into her house on East Capitol Street. He died there on October 14, 1937, and was buried in Rock Creek Cemetery in Washington DC.[9]

Bibliographic Notes and Acknowledgments

The sources on which this biography is based are sufficiently numerous and diverse as to make too cumbersome any attempt to cite the authority for each statement. The account of Hewitt's early life is based on Baldwin 1914 and Swanton 1938, and supplemented by Graymont's research in local archives. What is known of Erminnie A. Smith's life has been ably summarized by Lurie (1971). Letters of Smith and Caroline Parker Mt. Pleasant to Morgan in the Lewis Henry Morgan Papers, Department of Rare Books and Special Collections, University of Rochester Library, document Morgan's role in guiding Mrs. Smith to the Tuscarora reservation in 1880.

A year-by-year accounting of Hewitt's work at the Bureau is given in the Annual Reports of the Bureau of American Ethnology. These materials have been supplemented by information in the Hewitt and other papers in the National Anthropological Archives, Smithsonian Institution.

The bibliography of Hewitt's writings includes all of Hewitt's publications known to us. Hewitt's manuscripts in the National Anthropological Archives are listed in *Catalogue to Manuscripts at the National Anthropological Archives* (National Anthropological Archives 1975). (Users of this catalogue should note that not all of Hewitt's manuscripts are listed under the entry "Hewitt." A number are listed only under the tribe that the manuscript is about.)

Hewitt's Tuscarora folklore manuscripts in the National Anthropological Archives have been published by Rudes and Crouse (1987). Various reasons why Hewitt's work is still so highly regarded by linguists today are discussed in Rudes 1994. Some quotations from conversations with Hewitt circa 1911–13 are to be found in Densmore 1949.

In the course of our study of Hewitt's life and work, we have had the benefit of discussion with various individuals, including William N. Fenton, who also read an earlier draft of this paper, and with Nancy Bonvillain, who provided linguistic insight. Douglas R. Parks read the manuscript and offered useful suggestions. We are also indebted to James R. Glenn, Mary Elizabeth Ruwell, and other members of the staff of the National Anthropological Archives for their help. A portion of Graymont's research in these archives was supported by a travel grant from the offices of Fellowships and Grants of the Smithsonian Institution.

Notes

1. Both Hewitt's birth year and given name are in question. In an early autobiographical sketch for the Bureau of American Ethnology, he gave his birth date as December 16, 1858 (Pilling 1888:82). Subsequently he settled on December 16, 1859, which has become the generally accepted date. However, baptismal records of the Tuscarora Congregational Church, which was then supported by the American Board of Commissioners for Foreign Missions (it later became the Tuscarora Presbyterian Church), list Hewitt's baptismal date as March 8, 1858, indicating that his birth year was, in fact, 1857. Also, when Hewitt married Agnes Bridget Naughton in Jersey City Heights, New Jersey, on August 1, 1882, he listed his age as twenty-four, which would also establish his birth year as 1857. He gave his full name as John Napoleon Bonaparte Hewitt and his mother's maiden name as Harriet Printup (State of New Jersey, Marriage Returns, 1882). It was this name by which Hewitt continued to be known by reservation elders. Tuscarora reservation resident Titus Patterson, who knew the Hewitt family well, always spoke of Hewitt as John Napoleon Bonaparte Hewitt (Graymont interviews with Patterson's daughter, Nellie M. Gansworth, July 1965).

When Hewitt became an employee of the Bureau of American Ethnology in 1886, he evidently was sensitive about his prominent middle name and conscious of his image as a Smithsonian scholar. It was at that time that he transformed his mother's maiden name from Printup to Brinton and adopted it as his own in place of Bonaparte. In his contacts with the Tuscarora Indian community, however, he always spoke of his maternal lineage as Printup.

2. J. Owen Dorsey's comments are appended to the last five pages of Hewitt's article (Hewitt 1893d:403–407).

3. During his long employment at the BAE, Hewitt was always the lowest paid ethnologist on the staff. During Francis LeFleshe's tenure at the BAE, he and Hewitt were paid the same salary and both were listed as P-2 (a lower professional ranking) in the government service. Hewitt ended his career as a P-2 in 1937 at a salary of $2900 per year, financially far behind the ethnologist college graduates, who were classified as P-5. He was never pro-

moted beyond P-2. (J. W. Powell to Spencer Baird, Secty., July 13, 1887, For Fiscal Year Ending June 30, 1888, p. 127, Microfilm Box 5, Smithsonian Archives; Manuscript 93 Payroll Records, Smithsonian Archives, 1893–1937).

4. The Census notation for Hewitt under "Years Married" was "5x8", perhaps indicating that his previous marriage had lasted eight years. The obituary for Catherine's father in 1907, however, mentioned his daughter as "Miss Catherine Stuart" (*The Evening Star*, Washington DC, May 20, 1907, 3:4).

5. The petition is in the National Archives, U.S. Department of Interior, Bureau of Indian Affairs, RG 75, Special Case File No. 29, Kansas Claims by New York Indians. John N. B. Hewitt, #2285, Tuscarora.

6. Mrs. C. C. Hewitt to Mrs. Hill (Canada), January 31, 1918, Manuscript 4271, Box 2, Hewitt Letter Files, National Anthropological Archives, Smithsonian Institution. The letter was undoubtedly given to Hewitt by the recipient after Catherine's death and he carefully preserved it in his personal papers at the BAE.

7. Although Catherine Hewitt's death certificate lists her age as fifty, this is in error. The age probably should have been fifty-nine. The 1880 U.S. Census for Washington DC, listed her age as twenty-two, which would indicate a birth year of 1858. The 1900 U.S. Census gave her birth date as May 1858 and her age as forty-two. The 1910 U.S. Census listed her age as fifty. Mount Olivet Cemetery records give her birth date as February 1858. Depending on which month is correct, she would have been either fifty-nine or sixty at her death.

8. The Mount Olivet Cemetery office wrongly lists the family name as Stewart. There is no monument of any kind on the Stuart plot. Mount Olivet is a Roman Catholic cemetery of the Archdiocese of Washington DC. Catherine came from a Catholic family, although she may have attended Hewitt's church during their years together. In order to be buried in a Catholic cemetery, however, Catherine would have to have been recognized as a Catholic and to have had a Catholic funeral.

9. When Hewitt married Carrie Louise Hurlbut, he indicated on his marriage license application that he had been married only once before and that wife was dead, which only adds to the mystery. Hewitt, his last wife and her two sisters, and his sister Hattie E. Hewitt are all buried in Edward Shaw's cemetery plot in Rock Creek Cemetery. Shaw was an elderly Washington civil servant, formerly a clerk with the War Department, who was cared for in his declining years by the Hurlbut family and who made his home with them. Hewitt was one of the six pallbearers at Shaw's funeral in 1914 (*The Evening Star*, Washington DC, September 29, 1914, 10:5).

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6. Cushing at Cornell

The Early Years of a Pioneering Anthropologist

Frederic W. Gleach

> If, moreover, I am at times seemingly too personal in style of statement, let it be remembered that well-nigh all anthropology is personal history; that even the things of past man were personal, like as never they are to ourselves now. They must, therefore, be both treated and worked at, not solely according to ordinary methods of procedure or rules of logic, or to any given canons of learning, but in a profoundly personal mood and way.
>
> Cushing 1895:309–310

Frank Hamilton Cushing is one of the best known of American anthropologists, primarily for his work in the American Southwest and the often reproduced portraits of him in his Zuni costume—which have prompted a wide range of responses, favorable and negative, in both professional and Native communities. He also increasingly is remembered for his important work with the Smithsonian Institution in preparing the exhibits for the 1876 Centennial Exposition in Philadelphia, his archaeological work in Florida (later in his life), Virginia, New York and Pennsylvania, and the pioneering nature of his research and writing as a kind of humanistic science. Several studies explore his life and work, generally with an emphasis on the Southwest research.[1]

Although mentioned in several sources, very little detail has ever been given concerning Cushing's sole experience in higher education: a brief period spent at Cornell University. As curator of the Anthropology Collections at Cornell—a descendant unit of the University Museum, where Cushing briefly worked—I have sought any remaining traces of Cushing's time at Cornell, particularly since he was reported to have collected some artifacts near the campus and given them to Charles Frederick Hartt, the geologist who oversaw the Museum.

Cushing's early life has generally been given summary treatment in recent scholarship, so in order to contextualize his Cornell experience I will begin with a full account of his childhood leading up to that point.

We are fortunate in having several manuscript and typescript drafts of Cushing's autobiographical accounts, none of which seem to have been published previously.[2] A memorial collection published shortly after his death (McGee et al. 1900) also includes numerous biographical details, many clearly derived directly from Cushing himself; Cushing also included biographical details in some of his publications (e.g., Cushing 1895), and there is an extended biographical account published years later by journalist and explorer George Kennan (1923–24), who influenced and helped Cushing when the latter was a young man, lived with him for a time in Washington DC, corresponded and knew him well.

Faced with such a multiplicity of sometimes inconsistent sources, I opted to treat the drafts like transcriptions of interviews to be edited into a single coherent narrative, cross-checked where possible with other sources. All of Cushing's drafts, like the biographical accounts of Kennan and McGee et al., were written in the third person, so they seem more like biography than autobiography; because of the intervening hands in the various pieces, as well as my own, I refer to this product as a mediated autobiography. Where not otherwise identified this narrative is drawn from the manuscript and three typescript drafts; I have regularized spelling, punctuation, and grammar, brought pieces from the different drafts together, and made occasional changes in structure and wording to improve clarity while keeping the sense and feel of the original as much as possible.[3] In places the narrative has been supplemented by passages from the memorial collection and the Kennan notes; these published sources are cited in the text. I leave off the narrative when Cushing leaves for the Southwest, as that part of his life is much better known.

The Mediated Autobiography

Frank Hamilton Cushing was born July 22, 1857, in the town of Northeast, Erie County, Pennsylvania. His parents were Dr. Thomas Cushing, son of Enos Cushing, and Sarah Ann Harding Crittenden. Thomas Cushing was a physician, a graduate in the Medical Colleges of Buffalo, New York, and Albany, New York, and a student under Drs. Austin Flint and Frank H. Hamilton, after the latter of whom he named his youngest son.[4] The boy was born prematurely, and to this fact may be attributed his diminutive size and extreme delicacy of health during childhood, which to a notable extent probably led him into the studies in which he has since been engaged.

> At birth he was a mere mite of humanity, weighing only a pound and a half. For a year or two he grew but little, and was kept

> always on a pillow; but it is said that his mind developed more rapidly than his body—that in after years he could remember faces seen and aches felt before he was able to form words or to move from his place on the pillow. (McGee et al. 1895:356)

When this child was three years of age his father removed his family from Northeast to Barre Center, in Orleans County, New York, and though delicate in health, and small even for that age he was accustomed to go about alone, and employed himself in searching in the gravel-walks for peculiar little stones—especially such as resembled living creatures.

During his early childhood he was not instructed in any religious matters whatsoever.

> When he first came into direct contact with death, at the age of fourteen or fifteen, he said to his father one day "Cushing, when a man dies does he die altogether?" His father replied brusquely "I don't know a thing about it; neither does anybody else; but if a man lives again he doesn't go to hell." I seldom discussed a religious question with him, but when I did it seemed to me that his agnosticism was the result of solitary thinking, rather than the reading of sceptical books. (Kennan 1923–24)

This led him into a peculiar state of mind, and rendered him so unlike the other children around him in character that, in connection with his weakly condition, he was not an acceptable play-fellow for them, and he was therefore thrown upon his own resources for occupation and amusement. An occurrence in his fourth year will illustrate a phase of his mind at that time: A little sister having been born, he, like other children, inquired in regard to the origin of babies. He was not told that they came from God, but was informed by his nurse that they were found by the parents in the corners of fences. Thenceforth his chief ambition was to get away into the fields; and one spring morning he was permitted, when a wagon was going to the woodlots on his father's farm, to ride in it. At the first fence he came to he was set down by his request, and he found in one of the corners of the fence the skeleton of a horse; but nothing else rewarded his search except rocks, with which he was already familiar. He therefore concluded that there must be some connection between the bones of the horse and the origin of children, and on being told on inquiry that the bones were those of a horse, he inferred that in them dwelt the power of life-giving. On returning home he searched in the gravel walks until he found some clay-stone concretions resembling infants. Knowing that in the garden seeds were planted and produced plants, he

inferred that if some of those bones were scraped and put with the concretions so strangely resembling human creatures, and then buried for a time in the corner of a fence, they would grow until they became babies, and could then be brought home.

So vividly does Mr. Cushing recall this state of mind, this peculiar fetishism of his childhood, that it has become of immense service to him in the understanding of the somewhat allied conditions of mind among the Indians whom he has studied.

When he was seven years of age, as the Civil War began, his father removed to South Barre, Orleans County, New York, and then went as a surgeon of the New York Volunteers to the south.[5] The region of country in which he now lived abounded in forests and in rocks bearing various fossils. Led by his natural inclinations to spend much of his time in the woods and along the fences of the country he lived in, it was not strange that for his amusement he looked to the trees and rocks. The trees became, with their ceaseless motions, companions, and to a certain extent were personified to him. The curious rocks along the fences were gathered by him and hidden away in the woods which he frequented.

The feeling which he had for the woods and for these things may be illustrated by another occurrence. He had observed that the stones cast motionless shadows, but that the branches of trees often cast shadows which moved, like his own shadow. He therefore reasoned that the trees must be living creatures like himself, and he always endeavored to enter a forest so that his shadow should not be entangled with shadows of the trees, believing that there was some connection between the two. In his attempts to explain the differences between the shadows of trees and his own shadow and those of motionless bodies like rocks and dead wood, he gathered all the fossils and curious rocks that he could.

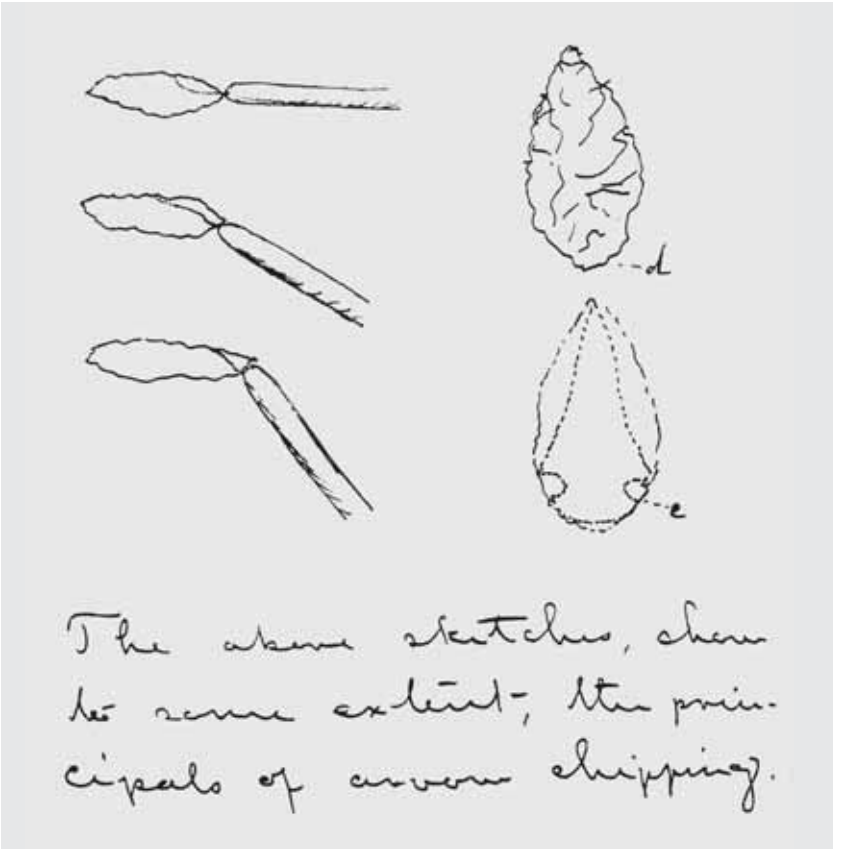
> When he began to read, a new world opened to him. He found but few books in his father's house, aside from medical works, and one of his early recollections was that of building up a pyramid of boxes and other articles high enough to enable him to remove the volumes from the shelves. Among the books he found a dictionary, which proved a great mine to him, and no doubt had marked influence upon his subsequent career. Every evening he consulted this book, seeking the new or difficult words that came up during the day, and these studies were often continued far into and even through the night. His schooling was rather meager. and by far the larger share of his education was obtained by reading and study along the few lines that offered themselves to the ambitious boy. (McGee et al. 1895:356–357)

> At one time he conceived the notion that he might fly, and to aid in the flight constructed himself wings made of a light wooden frame covered with paper or cloth. . . . He conceived that in some way strong faith in his power to fly would help the rather shaky wings. He climbed to the barn loft, and, appearing at the wide doorway, adjusted the wing-fastenings, gave a few preliminary flaps, and boldly spread his pinions for flight; but the faith was not strong enough, and he came down with terrific force. Although no bones were broken, the flying habit was very thoroughly broken—but this, he added ruefully, did not deter his irate father from breaking it again. (McGee et al. 1895:358)

When about eight or nine years of age, he discovered in wandering about the fields a flint arrow-head. More than anything he had found this attracted his attention, and on inquiring of a workman what it was, he was informed that it had belonged to the Indians, who had formerly lived in that country, and whose descendants he had visited with as they went about during the summer, selling baskets. These Indians had always been of much interest to him; he had never feared them and they had been strangely familiar with him. Therefore his interest in the flint arrow-head was increased, especially when he was told moreover by the colored man whom he had asked that if he wore it about his neck it would be a protection from lightning. He did not put it on his neck, but took it home.[6]

Thus he discovered a new and absorbing interest—so great, indeed, that on visiting his uncle in Michigan shortly after, he closely observed the Indians there, listened to stories of them, and on returning made a complete costume, donning which, he would go away into the woods and live the days through, building huts of bark and grass, following as nearly as he could the life he had learned of.

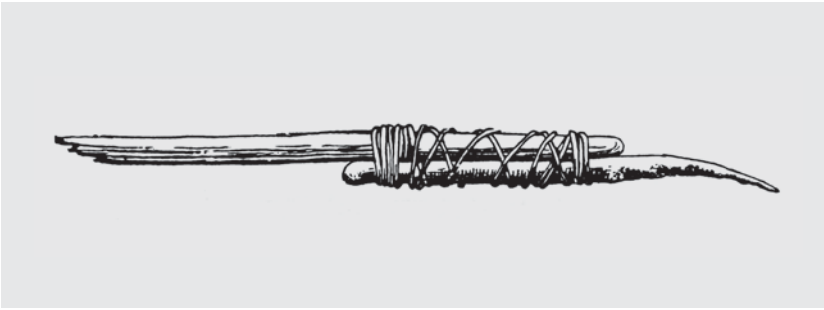
Ever after this, he searched industriously for more relics of the Indians. His efforts were rewarded to a remarkable extent, for his familiarity with the woods and fields had led him to be able to observe any peculiarities in the color or configuration of the soil or of vegetation; and he soon learned that wherever Indian encampments had been established, not only were the surroundings invariably alike in reference to elevation and contiguity of streams, etc., but that the soil had been changed in color, and to his eye there were clearly distinguishable traces of the paths originally made by the Indians in going to and from their camps, no matter how much the fields had been cultivated and trodden; and that wherever these paths occurred, in cultivated fields or woodlands, and wherever their huts had stood or refuse been thrown, so great had the change been in the soil that a slight difference was observable in the hue of the



2. Sketch by Cushing in 1877 showing the process of flaking stone tools. Courtesy of the Autry National Center/Southwest Museum, Los Angeles CA (MS.6.SI.3.1).

vegetation—so that by lying down and looking over the fields and places, he could tell at once where to search for the relics he sought. This enabled him to rapidly locate ever fresh finds, and to enrich his collections from them, by excavation, with many more than ordinary delicate specimens not only of pottery, but also of shell, horn and bone utensils and ornaments, then practically unknown in collections of the inland lake-regions.

Meanwhile he had been attending the District School, where his studies in relation to geography, history, and kindred matters progressed most favorably, but in other studies, especially mathematics, he was very deficient. His father, who returned home at the close of the war, looked with little favor on his amateur archaeological studies, being himself a thoroughly cultivated and mathematical man, with no small amount of scien-



3. The pressure-flaking tool made by Cushing from a whalebone toothbrush handle c. 1870. Illustration from Cushing (1895:313).

tific training. He therefore often tried to discourage the desultory studies of his son, occasionally bringing the latter to great grief by his expostulations, and on one occasion turning his cumbersome collections out of doors. In 1870 his father, desirous both of resuming a town practice and of placing his son in an academy—if only to settle him in a course of study—moved the family to Medina, a larger town in the western part of the county.[7] Here however the school studies progressed less than the researches in the fields, for in the town of Shelby, nearby, were ancient remains of Indian fortifications, rich in relics, which were soon the source of a wealth of collections.[8]

Cushing had no books relating to archeology, excepting Layard's *Nineveh and Its Remains* (which he read from beginning to end before he passed his twelfth year), but he had observed that the flint of the arrow-heads and the flint found on a certain hill on his father's farm were identical, and he began to make attempts to manufacture arrow-heads of his own, in order to learn how the Indians had manufactured them. He only partially succeeded by using flint with which to strike fragments from other pieces of flint, but continued to experiment unweariedly, until in his fifteenth year he was rewarded by the discovery of a process of pressure with bone, which enabled him to make arrow-points, knives, etc., of glass, flint, obsidian, and other substances possessing a conchoidal fracture, with ease and rapidity, by pressure with bone and horn points (a process that is now known to have prevailed quite generally amongst stone-age tribes).[9] He also determined the process of shaping and smoothing stone chisels etc. by means of pecking and oblique impact with flint nodules and "hard-head" cobble stones. No less diligently, he experimented with clay in order to learn how the Indians had formed by hand, and decorated, their pipes of terra-cotta and their earthen vessels

until he succeeded in restoring nearly every form indicated by the fragments he had found in ancient camp-places, by concentric building with the hands or by enclosure in meshes. These discoveries were important as tending to show that the ordinary arts of the stone age, while ingenious, were, nevertheless, simple, comparatively easy with practice, and far less tedious than is almost universally supposed. He also restored skulls in clay—modeling the typical faces of Indians as he supposed them to have been, gaining thereby considerable proficiency in plastic art.

> It was by these experiments that he discovered the significance of the beaver teeth found so extensively in the village sites of the region. He found that a beaver tooth made an admirable carving tool. In later years he discovered many other carving tools, especially those of sharks' teeth found elsewhere in the United States. One of the remarkable discoveries made in this forest workshop was the method by which the Amerinds [*sic*] wove their rush mats. First he obtained a sample and then set his wits to work at the problem of its manufacture. The rushes which constitute the woof could not be handled in a shuttle, and the ordinary device of the hand-loom he supposed to be beyond the art of the Amerinds. Then he devised a new method of weaving such fabrics. He cut into lengths the warp which he desired to use, using ordinary twine for experimental purposes; then he made two stiff rods which he placed upon two sawhorses, so that they were parallel and about six inches apart. On this framework of rods he placed his strings of warp, one end over each pole, so that the middle portion of the string fell down to the ground, while the ends of the string turned over the poles. Then he attached to either end of the string a stone weight, and having a succession of warp-strings distributed at intervals along the poles, he placed several rushes upon the warp-string between the rods, then taking the ends or the strings with their weights one in either hand, he crossed them; and then left them to again hang down over the rods. Having crossed all of the warp-strings in this manner, he again placed one or more rushes over the first bundle on the crossed string of the warp, and continued this process until the rush mat was completed. Prior to this time these warp-weights had been found widely distributed over the United States and were considered to be plummets or sinkers. You will find them still labeled in this manner in most collections. But Cushing was not sure that he had yet found the purpose of these so-called

> sinkers until in after years he found them used in this manner in the Far West. I have myself seen them used as warp-weights by the mat-makers of California.
>
> It was in this workshop of technologic investigation that Cushing gained that marvelous skill in handicraft for which he became so well known among ethnologists, and which ultimately led to the preparation of his paper on *Manual Concepts: A Study of the Influence of Hand-usage on Culture-growth*. (McGee et al. 1900:362–363)

In 1871 the young man became acquainted with the Siberian traveler George Kennan, who joined in and encouraged his field excursions.[10] The two became warm friends, and made many expeditions about the country together; and Mr. Kennan threw open to his young friend his large library, containing many archeological and ethnological works, such as those of Tyler, Lubbock, Wilson, and Evans. These were further added to by the Hon. Lewis H. Morgan.[11]

> He did not then have much book knowledge of archaeology, and the information contained in Tylor's "Primitive Culture" and "Early History of Mankind" was as new to him as it was to me. He absorbed it with eager interest, and especially the part that related to prehistoric methods of making fire. He had never before heard of the fire-drill, but when I showed him a picture of it in Tylor's "Early History," he made one for himself, and we tried it together one day in my brother's house on the corner of Center Street and West Avenue, where I was then living. We charred the wood and filled the house with smoke, but we were not able to produce a flame. Frank, however, persisted, and by changing his materials finally succeeded in getting a blaze, not only with the fire-drill, but with two suitable pieces of wood briskly rubbed together. It was a long time before he could get a flame by merely rubbing two sticks together, but Tylor said Darwin had done it, and that encouraged him to persevere until he finally discovered suitable kinds of wood. He wanted also to make a fire as the Malays did, by compressing air with a piston in a tight cylinder, but he could not get or make the necessary tube. (Kennan 1923–24)

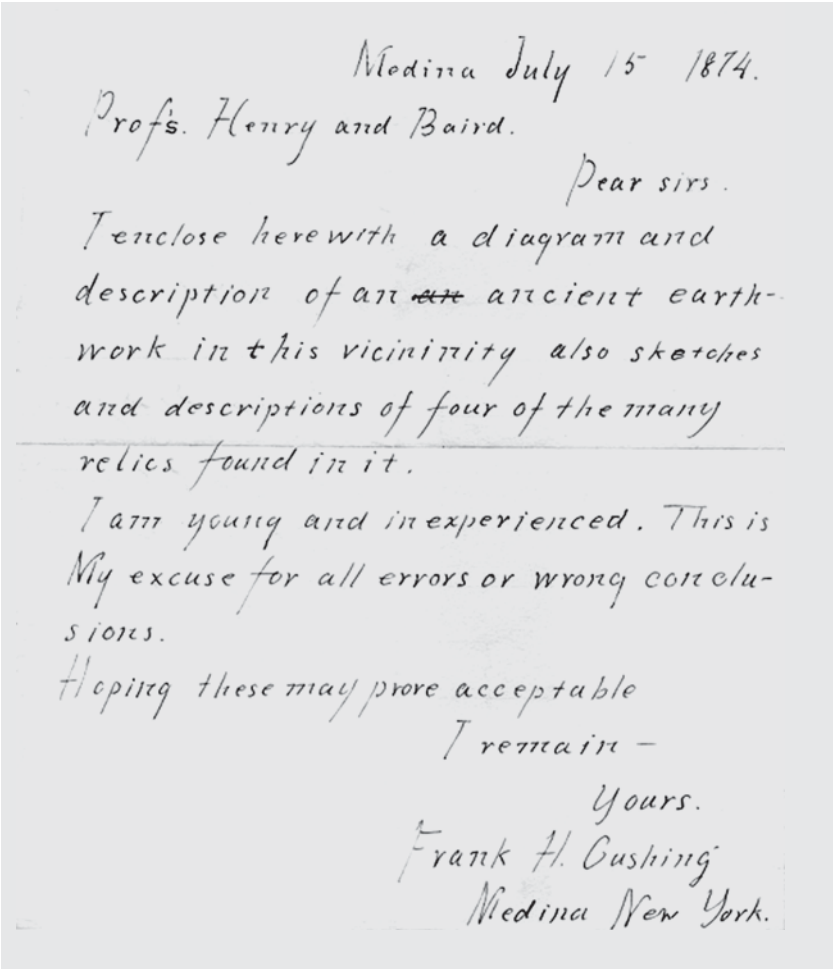
The young enthusiast had explored afoot all the country round about his native place, and now his expeditions widened into neighboring counties. When he could get away, he would be absent two or three days, sleeping in the woods, and living on whatever he took with him or could procure

en route. He invariably returned laden with rare specimens of the antiquities of the sections of country he had visited.

In the summer [of 1874], the youth accompanied his mother on a visit to some relatives at and near Cazenovia, in central New York.[12] The ancient fortifications, burial grounds and camp sites of Madison and Onondaga Counties are especially rich in remains, as are the shales of the Hamilton group of rocks rich in fossils. This expedition therefore brought him into an exhaustless field for exploration. During the summer his collections made in these two counties trebled the number he had left at home.

On one occasion when searching for fossils he accidentally met Mr. Lambertus Wolters Ledyard, one of the wealthiest and most cultivated of the citizens of Cazenovia.[13] The young man had fallen over a cliff during the day, and lost the hammer with which he was accustomed to break fossils from the rocks. Returning, he had found a fossil on the borders of Cazenovia Lake, at the foot of a lawn belonging to this gentleman, and not knowing to whom he was applying, requested Mr. Ledyard to lend him a hammer in order that he might get the fossil from the rock. Mr. Ledyard accompanied him to the spot, and found it was a rock containing a fossil of great beauty, and also showing glacial marks, for which reasons he had had it transported many miles to its present site on the lake.[14] Without telling the young collector this, he advised him not to take the fossil, as it was too firmly embedded, but he loaned him a hammer, and told him of a quarry some miles away, containing large numbers of fossils. As it was late in the day he suggested that he make a visit to this quarry at some other time, but being anxious to improve every moment of his time, the young man insisted on going at once. On his way to the quarry he collected several Indian relics, and on reaching the place he filled his large leather bag with rare specimens of fossils, not reaching Mr. Ledyard's house until late at night. This gave the public-spirited gentleman an interest in the young geologist and his studies, and he planned an expedition with him, and they drove through a country new to the latter. Passing over the hills and through the valleys of that section, Mr. Cushing could point out at any distance an elevation or other spot where relics might be looked for, owing to his knowledge of the conditions pertaining to Indian encampments. As his indications were invariably found to be correct, Mr. Ledyard was impressed with the sagacity of his young friend, and mentioned him to Professor Spencer F. Baird of the Smithsonian Institution.[15]

After Mr. Cushing's return to Medina he was surprised to receive by express a number of books relating to archeological and geological sub-



Medina July 15 1874.

Profs. Henry and Baird.

Dear sirs.

I enclose herewith a diagram and description of an ~~old~~ ancient earth-work in this vicinity also sketches and descriptions of four of the many relics found in it.

I am young and inexperienced. This is My excuse for all errors or wrong conclusions.

Hoping these may prove acceptable

I remain –

Yours.

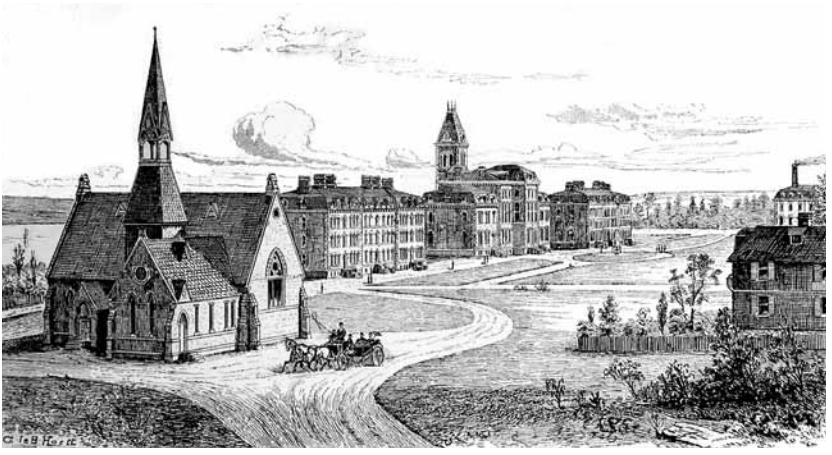
Frank H. Cushing

Medina New York.

4. The earliest found letter from Cushing to the Smithsonian. The enclosures presumably led to the account that constituted Cushing's first publication (1875). Courtesy of the Smithsonian Institution Archives (Record Unit 26, volume 142, p. 220).

jects [in fall 1874].[16] They came from Professor Baird, accompanied by a proposition that during the next summer [of 1875] Mr. Cushing should make some explorations for the Smithsonian Institution.[17]

About this time Dr. Thomas Cushing began to encourage his son's studies, seeing that they were taking definite shape. Through his brother Enos, who had been taking a course of study at Cornell University at Ithaca,[18] the young student became acquainted with the name and work



5. View of the Cornell campus in the 1870s, from a photograph by C. Fred Hartt. McGraw Hall, home of the University Museum and Hartt's and Derby's offices, is the building at the center with a tower. Published as the frontispiece in *The Cornellian* for 1873-74; reproduced with the permission of the Cornell University Archives.

of Charles Fred Hartt, the geologist of Cornell University.[19] It led to a brief correspondence between them, which ended in Mr. Cushing's visiting Ithaca [in the summer of 1874].[20] The day he was there, he had the good fortune to discover, immediately opposite the campus of the college, on the other side of the ravine called Buttermilk Canyon, a wonderfully rich Indian camping ground. He returned to Prof. Hartt's laboratory in the evening laden with rare Indian remains.[21] One of that gentleman's chief interests was in archaeology, which he had vigorously studied in Brazil (whither he had accompanied Agassiz in the Morgan and Thayer expeditions), and he conceived an interest in Mr. Cushing, and offered to secure him free tuition in the Natural Sciences if he would come to Ithaca and study under him and his assistant, Professor Derby.[22]

This led to Mr. Cushing's removal to Ithaca during the spring of 1875, where he was entered as one of the students in Natural History.[23] His connection with the college, though it proved of great value to him, was but brief. In the summer of 1875, his explorations in central and northern New York, undertaken for the Smithsonian Institution, resulting in a large and valuable collection, led to his appointment by Professor Baird as one of the assistants in the preparation of the Indian Collections of the National Museum for the International Exposition at Philadelphia.[24]

At the Smithsonian Institution he was placed under the charge of Dr.

Charles Rau, the archeologist of that institution, as one of his assistants. The studies and discipline which he underwent with Dr. Rau among the Museum collections, which are the richest in America, turned his attention rather from collecting to studies connected with collections. He accompanied Dr. Rau as assistant in charge of the Ethnological Collections to the Philadelphia Exposition in 1876, and remained there after its close to take charge of the collections which were contributed by the various foreign exhibitors to the National Museum, returning to Washington in February 1877 as curator of the Ethnological Department of the National Museum.

> In 1878, when I went to Washington to enter the service of the Associated Press, Frank was living near the top of the south tower of the Smithsonian building south of Pennsylvania Avenue. As he had two rooms and two beds, he asked me to come and stay there until I should find a boarding place. . . .
>
> The room under ours at that time was the work-room of the well-known ornithologist, Robert Ridgway. It contained hundreds of skins of birds, which had been treated with benzene to protect them from insects, and the floor was covered with fragments of wooden boxes, wrapping paper, excelsior, and other combustible materials used in packing. The whole room was as inflammable as a powder magazine. One morning Frank happened to want more heat than he could get over a mere cooking lamp, and he thought he would go down and make a fire in Mr. Ridgway's stove. Not noticing on the floor an open can of benzene which the ornithologist had been using, he struck a match. Instantly the vapor in the air flashed and set fire to the benzene in the can, which sent up a column of flame four or five feet high. In half a minute the can would have melted apart and the burning benzene would have run all over the tinder-like stuff on the floor. Frank, with perfect self-possession, lifted the blazing can between his hands, carried it steadily across the room without spilling a drop, and set it down in a solid iron coal-scuttle, where it burned itself out without setting fire to any thing else. Not one man in a thousand would have thought of this in time, or have had coolness and steadiness to do it. (Kennan 1923–24)

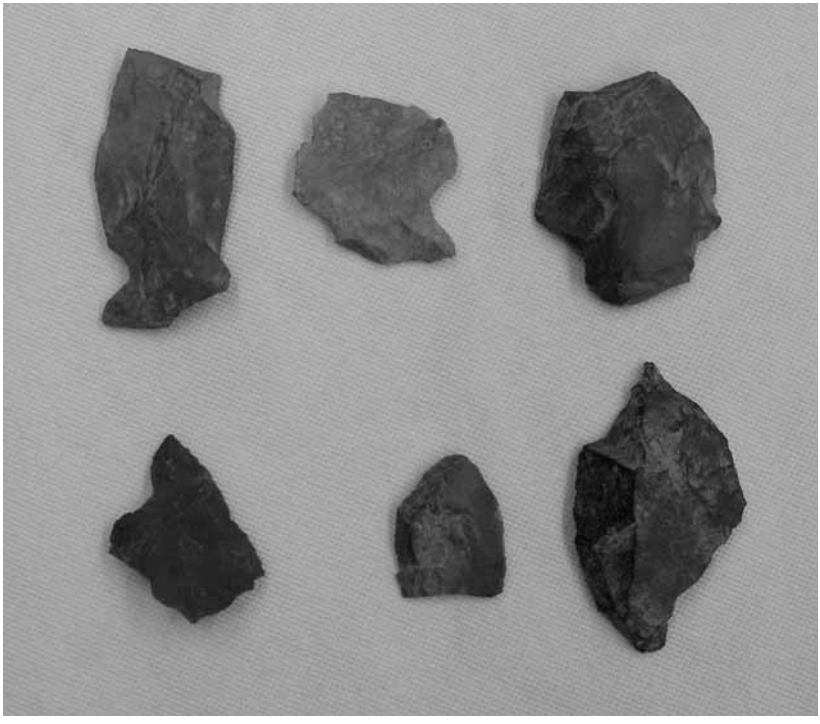
During the summer of 1876 he gained his first knowledge of the Pueblo Indians—the Moquis, Zunis, and others—from some sketches which had been brought home by one of the U.S. geologists, and were exhibited

at the Centennial. Investigating this subject, he found it of great interest, and became deeply interested, moreover, in the ruins and cliff dwellings which had been recently discovered by Messrs. Jackson and Holmes in the geological survey in the Southwest. This led him to desire to visit the region, a desire which steadily increased for two years, during which time he undertook, by order of Prof. Baird, some explorations of caves in Pennsylvania, near Carlisle, and of ancient Indian steatite and mica quarries in Amelia Country, Virginia, and Rock Creek, in the District of Columbia.[25]

In 1879 the original U.S. Geological Surveys were combined into one survey, under the Directorship of the Hon. Clarence King, and Major J. W. Powell with wise foresight besought Congress to establish a U.S. Bureau of Ethnology, which being established Major Powell was made Director, and an expedition was formed to go out under charge of Colonel James Stevenson to undertake explorations in the Pueblo country. Professor Baird, knowing Mr. Cushing's desire to visit that country, and finding the gratification of that desire compatible with the interests of the Institution expedition, called the young man to him one day, and asked him how he would like to go to the Southwest, to make a study of some typical pueblo and of the ancient remains there. Mr. Cushing unhesitatingly acknowledged his anxiety to accompany such an expedition, and was ordered to prepare to go with the party, for the official purpose of making such observations at some typical pueblo as would enable him to explain the uses of and properly label the Indian Collections which might by made by Colonel Stevenson's party.

The Cornell Artifacts

Although little trace of Cushing's presence at Cornell remains, by fortuitous chance the handful of artifacts he collected near the campus managed to survive. When Hartt left Cornell in 1874 he planned to return, and some of the materials he had collected in Brazil with the Morgan Expeditions in 1870–71 remained uncataloged in boxes. Years later—in the early to mid-twentieth century, judging by the handwriting—one or more boxes were found in storage and entered into the old museum catalog. In a still later hand and ink is one final entry: "Flint flakes + arrow point rejects; 6." Although cataloged years later as from Brazil, on examination these pieces all clearly appear to have originated in central New York, both by material (Onondaga chert and closely related variants) and in form (the top three in the photo are points apparently broken in production, and fit comfortably in the range of points commonly found in the area).



6. The artifacts believed to have been collected by Cushing near the Cornell campus and presented to C. F. Hartt in 1874. Photo by the author.

It is my belief that these pieces turned up in a box of Hartt's materials at some point decades after his death, and were presumed by someone unfamiliar with the stone tools of Brazil and New York to be part of his Brazilian collections, like other materials in the Cornell University Anthropology Collections. The records cannot prove it, and the individuals involved are long gone, but I am confident that we have located the artifacts that a young Frank Hamilton Cushing brought to Charles Frederick Hartt on his first visit to Cornell University in 1874.

Notes

Thanks to David Holmberg and Terry Turner for first alerting me to Cushing's presence at Cornell; to the librarians and archivists who have helped me locate materials at Cornell (particularly Elaine Engst), the Smithsonian Archives (Ellen Alers), the Southwest Museum (Kim Walters, Manola Madrid, and Marilyn Kim), and the Lee-Whedon Memorial Library (Catherine Cooper); and to Kurt Jordan, who confirmed my identification of the pieces as local. I also acknowledge and thank Arthur Bloom for his help in identifying Buttermilk Canyon, and William Brice for sharing his knowledge of Hartt and enthusiasm for disciplinary histories and early geology at Cornell.

1. Among scholarly sources, Brandes (1965) offers the most comprehensive treatment of Cushing's life. Green (1990:2–27), Hinsley (1994:191–207; 1983), and Mark (1980:96–130) also discuss his life and work at some length, with emphasis on the Zuni research, and further biographical and analytical information can be found in Hinsley and Wilcox (2002), Kolianos and Weisman (2005:1–13), Parezo (1985), Widmer (2000:ix–xxii), and others. A Zuni perspective on Cushing is offered by Hughte (1994; cf. Jojola 1995).

2. Although the mechanical authorship of these documents is uncertain—they are written in the third person, and include occasional constructions such as "he says he distinctly recollects"—the detail and tone clearly indicate that the source was Frank Cushing himself. Much of the material in McGee et al. (1900) and in Kennan (1923–24) also clearly came from stories told by Cushing, but in some cases details likely were lost or changed in the transmission.

The autobiographical drafts were possibly prepared in the first half of the 1890s for Frederick A. Ober. A letter dated October 8, 1891, to an unspecified recipient indicates that Cushing was preparing "the few data making up all that can be of any importance for you, relative to my life," and a manuscript autobiographical addendum from 1896 is addressed to Ober (both documents are filed with the manuscripts in MS.6.BAE.4.2, at the Southwest Museum). Ober worked with Cushing both at the Smithsonian and at the Columbian Exposition at Chicago; best known for his extensive travels and writings on the Caribbean, if he planned a biographical work on Cushing it was never published.

3. In most cases I gave precedence to the manuscript draft, in which extensive passages deleted from the typescripts can be read. Many seem to have been deleted later either for length or to shape the tone of the piece, often to de-emphasize his more romantic side. I have added most such deletions back into the narrative, but when Cushing changed a word or phrase in a later version for clarity or mellifluity I retained the change.

4. The 1865 New York State census for the Town of Barre, Orleans County, recorded the family as Thomas Cushing (forty-three); wife Sarah A. (Crittenden) Cushing (thirty-five); sons Enos L. Cushing (fourteen), Clark Harding Cushing (twelve), and Frank H. Cushing (nine); and daughter Mary L. Cushing (five).

5. Brandes (1965:3) reports that "Thomas was commissioned as an assistant surgeon in the 28th New York Volunteers and served in Virginia and Maryland. He was later appointed a surgeon in the 29th U.S. Infantry and saw duty in Louisiana, Texas, Mississippi, and Alabama. He cared for wounded at Second Bull Run, South Mountain, and Antietam."

6. Cushing published a slightly different version of this episode, in a paper that originated as his vice-presidential address to Section H of the American Association for the Advancement of Science in 1895:

> When I was a boy less than ten years of age, my father's hired man, while plowing one day, picked up and threw to me across the furrows a little blue flint arrow-point, saying: "The Indians made that; it is one of their arrow-heads." I took it up fearfully, wonderingly, in my hands. It was small, cold, shining, and sharp—-perfect in shape. Nothing had ever aroused my interest so much. That little arrow-point decided the purpose and calling of my whole life. It predestined me, ladies and gentlemen, to the honor I have in addressing you here today, on Arrows; for I have studied archeology far more, alas, than anything else—ever since I treasured that small arrow blade on the lid of an old blue chest in my little bedroom, until the cover of that chest was overfilled with others like it and with relics of many another kind.
>
> I was fortunate enough, not long after, to find in a neighboring field a place where

some of these blades had been made. I could see that they had been fashioned in some way by chipping, for the scales lying there were like those I had been wont to strike off to see the sparks fly. When in course of time I had gathered a collection of some hundreds of relics from all over central and western New York, I began a series of experiments to learn how these arrows had been made. No one could tell me, and I had no books on subjects of anthropology then.

There was a farmer in our neighborhood who, when young, had gone to California. It was in the days of "Forty-nine," and he had been pricked in the shoulder by an Indian arrow. He may not have killed the Indian, but had, at any rate, his whole sheaf of arrows—quite as perfect a set as I ever saw. They were all pointed with obsidian tips, like mine in shape and finish, but smaller. I thought the points were of glass, and forthwith added all the thick pieces of bottle-glass and window-plate I could gather, to my store of raw material for practice. With this I worked, now and then, throughout a whole season, but the products of my hammerings, though fair, were but crude compared with those of the field. (1895:311–312)

7. The family first lived in a brick house at the corner of Oak Orchard Road and Maple Avenue, in Medina, and later built two houses on Old Orchard Road, all of which still stand. In 2005 a collection of Cushing family documents and photographs were discovered in an attic in a Medina-area house, but they were sold by estate auction to a private individual (Kropf 2005).

8. This was the circular ditch-and-embankment site reported by Squier (1851:50), which Cushing later described in his first publication.

9. Cushing's published version of this episode offers further detail; it is worth noting for modern readers that toothbrushes of the time were made from whalebone (with bristles typically of boar bristle):

When nearly fourteen years of age I discovered in the woods south of Medina, New York, an ancient Indian fort. I built a hut there, and used to go there and remain for days at a time, digging for relics while the sun shone, and on rainy days or at night in the light of the camp-fire, studying by experiment how the more curious of them had been made and used. One evening I unearthed a beautiful harpoon of bone. I had a tooth-brush. I chopped the handle off and ground it down on a piece of sandstone to the shape of the harpoon blade, but could not grind the clean-cut barbs in its edge. I took my store of flint scales and set to work on it, using the flint flakes in my fingers, or clamping them between split sticks, saw-fashion. The flint cut the bone away as well as a knife of steel would have cut it, but left the work rough. Now, in trying to smooth this I made a discovery. No sooner had I begun to scrape the bone transversely to the edge of the flint than the bone began to cut the flint away, not jaggedly, as my hammer-stone would have chipped it, but in long, continuously narrow surface flakes wherever the edge was caught in the bone at a certain angle. I never finished that harpoon. I turned it about and used it as an arrow-flaker by tying it with my shoestring to a little rod of wood for a handle and pressing it at the proper angle to points on the flint which I wished to remove. I made arrow after arrow thus, in the joy of my new discovery, until my hands were blistered and lacerated, in one place so deeply that the scar remains to this day, and, worn down to a mere splinter, I still preserve my first tooth-brush flaking tool.

I did not know at that time that archeologists the world over were ignorant, as I had been, of just how flint implements had been made, and I did not learn until my

now so lamented friend, Professor Baird, called me to the Smithsonian Institution, in 1875, that I was the first man, or rather boy of our day who had practically discovered how to make implements of glass and flint flaked from side to side, and in this indistinguishable from those made by primitive peoples. (1895:312–313)

10. George Kennan is best known as a Russian explorer and journalist, but the talent that got him started was telegraphy, and his first trip to Russia was in connection with a transnational telegraph line. His brother John had become president of the Union Bank in Medina, and George moved in with him when he returned from Russia in 1871. He took a job as cashier in the bank, and after periods in New York and Washington, in 1879 he married Emaline Weld, the daughter of the local miller. During his time in Medina he became friends with Thomas Cushing and his son Frank, and Kennan and Frank later roomed together for a time in Washington DC. Years after Cushing's death, having returned to Medina, Kennan wrote his recollections of the Cushings for the Medina newspaper (Travis 1990:47–51, 57; Grinnell 1996:116–117; Kennan 1923–24).

11. Cushing does not specify how he met Morgan, but it seems to have happened in the first half of the 1870s, possibly through the Chatauqua system of traveling speakers.

12. At a distance of about 150 miles, this was a significant journey that may have been done by horse or, in part, by canal boat. The manuscript draft and some other versions date the trip to 1870 or 1871, but the later typescript gives "summer of 1873–74," and George Kennan dates the event to "A year or two after I first knew him [in 1871] . . . I think in the summer of 1873" (Kennan, G.K.'s Column, Dec. 20, 1923). Cushing's earliest known letter to the Smithsonian, a connection that followed from this trip, is dated July 15, 1874 (Smithsonian Institution Archives, Record Unit 26, volume 142, p.220). Cushing may have made this trip in summer 1873, with the subsequent correspondence taking place over the following year, but I think it more likely that it took place in summer 1874, with that correspondence compressed into a few months; Cushing refers (below in the main text) to the package of books from the Smithsonian arriving "After Mr. Cushing's return to Medina" from this trip, and that package was in Fall 1874.

13. Lambertus Ledyard, a lawyer, was born in Cazenovia, New York, April 8, 1836. He studied at the Cazenovia Seminary in 1848, then continued his studies at Poughkeepsie Collegiate School, and returned to Cazenovia. His house is known as The Oaks, and had been built just a few years earlier; it is adjacent to the home of his brother Lincklaen Ledyard, now the Lorenzo State Historic Site.

14. Lorenzo State Historic Site staff tell me that there are many rocks on the grounds with fossils, which interested the two Ledyard brothers as much as they did Cushing. Cushing's friend William Henry Holmes described the fossil as a "fine . . . large trilobite" (McGee et al. 1900:359).

15. Baird was a friend of Ledyard's (McGee at al. 1900:361), although the precise nature of the connection remains unclear. Baird was at the time assistant to the Secretary of the Smithsonian, Joseph Henry, whom he succeeded as Secretary a few years later, in 1878.

16. Baird sent a second parcel of books in January 1875, which Cushing acknowledged in a letter of February 2, 1875 (Cushing 1875a). Interestingly, the handwriting here is recognizably Cushing's, unlike the schoolboy-careful lettering of the 1874 letter; presumably, Cushing was feeling more at ease in the correspondence already at that date.

17. Cushing's (1875b) first publication was a report on the earthwork he had found at Shelby, and was accompanied by the donation to the Smithsonian of a collection of bone artifacts from Medina, in 1874; he was seventeen at the time. He made further donations in 1875 (listed in that year's annual report).

18. According to alumni files, Enos Cushing entered Cornell in 1870, with his course of study listed as "optional," and left in 1872 without completing a degree. He then became a dentist, working first in Middleport and then in Albion, New York, where he died in 1937.

19. Hartt, a student of Louis Agassiz, was a geologist who also contributed some of the earliest archaeological and ethnographic work on the Amazon, traveling with the Thayer (1865–66) and Morgan (1870–71) expeditions; some of his collections from the latter are held in the Cornell University Anthropology Collections. In April 1873 Hartt sent president Ezra Cornell a note suggesting an interest in investigating the Indian antiquities of central New York, with a letter for Cornell to forward to the editor of the Ithaca newspaper soliciting information on "Indian burial places, sites of villages, or encampments, or of localities where stone axes, arrowheads, pottery or other Indian antiquities occur" (Hartt 1873b, 1873c). It is not clear that Cornell ever did so, but clearly Hartt was thinking along these lines at the same time Cushing's activities were becoming known to scholars in the region. I have no record of Hartt's being aware of Cushing's activities at that time, but it is not out of the question that he may have heard something from others who had met the young explorer. Hartt was a very popular teacher, seen by students as kind-hearted and sympathetic, according to his obituary in *The Cornell Era*. On Hartt see Brice (1989:1–15) and Freitas (2001).

20. The dating is ambiguous, with different sources suggesting anywhere from 1873 to 1875. Hartt was too ill to travel in the summer of 1873 (letter from Hartt to Richard Rathbun, Smithsonian Institution Archives, Record Unit #7078, Richard Rathbun Papers, 1870–1918 and undated, kindly supplied by William Brice; cf. Hartt 1873d), and the account from Holmes (see note 22) indicates that Hartt was ill at the time of Cushing's first visit; he also had been ill in January 1873 (Hartt 1873a). The 1870 Morgan Expedition had been in "malarious districts of the Lower Amazonas," and at least one other member of the party contracted the disease (T. H. Comstock, as noted in *The Cornell Era* (Dec. 2, 1870) 3(2):91; note supplied by William Brice). But whether Hartt in fact had malaria, as Holmes indicated, or was suffering from exhaustion or some other disease, as his letters suggest, he could as well have been laid up in the summer of 1874, and I believe that year more likely for Cushing's visit. Cushing's contacts with the Smithsonian began in 1874, following from his meeting Ledyard in Cazenovia, and that summer trip seems to have been catalytic in his development. It is possible that Cushing visited Ithaca from Cazenovia before returning to Medina in 1874, or he may have made a separate trip.

21. That is, artifacts.

22. Orville Derby was a pupil of Hartt's, and was appointed to take over Hartt's teaching in January 1873 when Hartt was ill, and succeeded him briefly at Cornell before following him to Brazil in December 1875 (Hartt 1873a, Russel 1873, Brice 1989:16–22).

William Henry Holmes gave a somewhat different version of this event, with considerably more detail, but it feels rather embellished and is inaccurate at points; I suspect Holmes elaborated on the story he learned from Cushing:

> At the age of eighteen Cushing found his way to Ithaca, and at Cornell University sought Professor C.F. Hartt, geologist, and a well-known student of archeology. The Professor was in his workroom, stretched out upon a table suffering from an attack of malaria. The boy introduced himself, but was not especially welcomed, and when he broached the subject of Indian relics and the search for them in the neighborhood, he was told brusquely that there were none, for Hartt and his students and the farmers about Ithaca had looked for them in vain. "But," replied Cushing, "there are

Indian relics everywhere, and I can find them; I can find plenty of them right over there on that point of land." The Professor did not believe it, and suggested that the lad had the privilege of proving his assertion. So Cushing set out without delay and in a few hours returned with a sack full of implements. But by this time he was thoroughly exhausted from hard work and lack of food (for either he had no money with which to buy food or had forgotten to eat), and he climbed the hill to the college with great difficulty. Finally, reaching Hartt's place, he staggered in and began to empty the contents of the bag upon the floor. "Stop!" shouted the Professor, astonished at what he saw. "What are you doing? Take care of those things; bring them here to the table." He was not only surprised at the ample proofs brought so promptly, but when he looked again at the boy he was deeply impressed with his exhausted and pitiable appearance. "Here, Darby," he cried to his assistant, "take this chap and give him something to eat."

Thus Cushing made himself a place in this great center of learning and soon afterward returned to take a special course of study under the supervision of Professor Hartt. (McGee at al. 1900:359–360)

Although Cushing's own account describes the location as "immediately opposite the campus," it most likely was across Buttermilk Creek, a bit over two miles distant. There are two gorges immediately adjacent to campus, but neither seems ever to have been known as Buttermilk Canyon—and the more distant site is in accord with Holmes's account. Several local authorities consulted feel that the Buttermilk Creek area would have been visible from campus at that time, although today it is blocked by trees.

23. By then Hartt was in Brazil, having left Ithaca in early September 1874 for what was supposed to be a four-month trip (*The Cornell Era*, Sept. 11, 1874:7). I believe that he had made these arrangements for Cushing in good faith at their meeting in the summer of 1874, planning to be back in Ithaca in January 1875. But Hartt's dream of an office for conducting geological surveys in Brazil, to be modeled after the U.S. geological surveys, came to pass—probably more quickly than he had even hoped—and in late 1874 he was invited to direct the new Seção de Geologia do Museu Nacional (Freitas 2001:186–195). Derby had been left in charge of Hartt's responsibilities at Cornell for the fall, and continued through the spring, but he, too, left Cornell for Brazil in December 1875 (Brice 1989:18).

Cushing thus would have arrived at Cornell expecting to work with the noted geologist, to find instead his young protégé Derby, only six years older than Cushing himself. There is no record of Frank Cushing's admission to Cornell in any status, and I suspect he spent just enough time around the University Museum and Derby and the other students to decide that this was not what he wanted to be doing. Since Cushing had Baird's encouragement to make collections in central New York for the National Museum, he spent more time in that activity, cultivated the connection, and by September 1875 had offered "to accept any position in the Institution which he could properly fill" (Brandes 1965:11).

24. A letter from Baird to Cushing discussing the position is dated December 3 (Baird 1875); Brandes (1965:11–12) cites other letters from the negotiation.

25. Baird, who had spent much of his youth in Carlisle and taught at Dickinson College there, had previously made collections from at least one of the Carlisle caves, and accompanied Cushing on some if not all of the cave explorations. Dall (1915) provides biographical data on Baird; Snyder (1993) describes the caves and Baird's explorations. Baird published his findings from the 1840s, but the visits with Cushing were not published. Neither was Cushing's work on the soapstone quarries published, but later BAE researchers Gerard Fowke, W. H. Holmes, and David Bushnell followed (Bushnell 1908:546–547).

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7. The American School and Scientific Racism in Early American Anthropology

Adam Dewbury

Nineteenth-century America was a tumultuous place, wracked by war and suffering the growing pains of both westward expansion and industrialization. The intellectual world was equally turbulent, as a great battle of ideas challenged the Jeffersonian ideal of equality. The issues at hand were the very origins of humanity and the unity of humankind, and the social, political, and intellectual climate of nineteenth-century America proved to be fertile ground for the debate. Science captured the interest of a broad cross-section of Americans, including farmers, laborers, and tradespeople as well as educated professionals (Goldstein 1994). The natural sciences were of particular interest, evidenced by the founding of professional scientific organizations such as the Academy of Natural Sciences of Philadelphia in 1812 and the Smithsonian Institution in 1846. While the natural sciences were maturing, anthropology hardly existed as a professional discipline for most of the nineteenth century, in the United States or elsewhere; even the term "anthropology" saw little use, with "ethnology" being the favored term for most scholars. Early anthropologists were usually scientific professionals in other fields (medicine, zoology, biology) and they drew upon the core concepts, theories, and philosophies of their primary disciplines to craft their visions of anthropology.

Three such theorists in the United States were Samuel George Morton, Louis Agassiz, and Josiah Nott: all European-trained physicians with strong professional interests in the natural sciences. These men formed a crucial part of the small group of scientists known as the "American School" (Stanton 1960; Gould 1978, 1996), a group of strong and influential proponents of the theory of polygenism: the idea that human racial variation could be explained as the result of separate creation, rather than genesis from one primordial pair, which led to a belief in the separation of species. The antithesis of polygenism, monogenism, postulated the single origin of humankind and the unity of the human species. Historians of anthropology have often noted the importance of polygenism

in the history of anthropology (e.g., Erickson and Murphy 1998; Barnard 2000); for example, Barnard writes, "Their battles helped to form the discipline, and it would be denial of this fundamental fact if we were to ignore the battle and remember only our victorious intellectual ancestors, the monogenists, in isolation" (2000:25).

It is also important to remember the less savory aspects of our early forebears, and this paper focuses on the scientific racism espoused by Morton and especially Agassiz and Nott. The work of the American School lent support to the institution of slavery, and also to the virulent racism of nineteenth-century America, a condition which in no small part persists today. Recent publications such as *The Bell Curve* (Herrnstein and Murray 1994) and *Race: The Reality of Human Differences* (Sarich and Miele 2004) rehash many of the American School's basic arguments about biologically determined inferiority. Couched in modern scientific language, these "new" ideas may seem compelling—until one realizes that the same arguments were presented and refuted over a century ago. I show in this paper how the personal prejudices of three influential scientists engendered a particularly racist early American anthropology.

Contexts: Social, Political, Intellectual

The question of human origins was of tremendous interest in all spheres of American society in the nineteenth century. The future of the nation literally depended upon the question's resolution. With westward expansion came the question of whether slavery would be allowed in the new territories, and a growing movement was pushing hard for the abolition of slavery in all the United States. Each side had a vested interest in proving (or disproving) the shared humanity of blacks and whites.

American science was strongly influenced by the European tradition, and it is within this tradition that the conceptual framework supporting polygenism was created. Mayr (1972) identifies two major paradigms, creationism and essentialism, that formed the cornerstones of nineteenth-century natural science theory in Europe and the United States. Disparaged by most modern scientists, creationism was embraced by a majority of scientists in the first half of the nineteenth century (Lovejoy 1959), and was seen as a satisfactory explanation for diversity (Wilson 1959:405). Natural science and religion had enjoyed a close relationship for the previous two centuries in the form of "Natural Theology," a philosophy that attempted to gain "Knowledge of God from the works of creation" (Barker 1674). Paley's influential book *Natural Theology* (1802) was a standard academic text in America and England (Wilson 1959:409), re-

inforcing the viewpoint that design in nature existed. Given this context, it was perhaps natural for nineteenth-century scientists to embrace a supernatural rather than a material explanation for their observations.

One of the most enduring and important concepts in creationism's construction was the idea of the *scala naturae* or chain of being. This concept that all life on earth occupied a fixed, predetermined point in a grand linear organization, with humankind occupying the uppermost positions, dates back to Aristotle (Lovejoy 1963). Because organisms were placed in the chain of being by the hand of God, it was believed that species were fixed and immutable entities, existing just as they were created and undergoing no modification. The idea of species fixity was embraced by Georges Cuvier (1769–1832), who believed that the complex relationships of the internal parts of organisms made significant change impossible (Bowler 1989:112–114). Cuvier established the fixity of animal species through the study of Egyptian animal mummies (Bowler 1989:116), foreshadowing the use of Egyptian human mummies by Morton (1844) to "prove" that the races of humankind had always been distinct. Cuvier passed his firm belief in the immutability of species on to his student Agassiz, for whom the concept became a central part of his theory of geographic distribution—one of the major scientific arguments for plural origins.

A second paradigm in the conceptual framework of nineteenth-century science was essentialism, the idea that everything in nature was a derivation of an ideal form or essence, that existed outside of and unaffected by the material world (Mayr 1972, Bowler 1989). Plato's (1987) allegory of the cave illustrates the basic premise of essentialism: the shadows seen on the cave's wall are simply distortions of an unseen and idealized reality, not that reality itself. In the biological realm, every organism observed in nature is derived from an ideal type, and can be classified on the basis of the deviation of certain characters from the essential, unchanging type. This "typological thinking" (Mayr 1972:983) was a very powerful idea in biology, persisting into the twentieth century (Mayr 1972).

The socio-political context of nineteenth-century America provided a solid foundation for polygenist theory. Slavery had been a part of American life almost from the nation's beginnings. It was codified into the laws of several colonies shortly after 1660 and had become a fully racialized institution by 1700 (Jordan 1976:3). By the 1820s it had "matured into an established economic interest" (Fredrickson 1988:16), both agrarian and industrial (Starobin 1970). Coupled with the "peculiar institution" was a pervasive anti-black sentiment, an attitude shared even by certain abolitionists (Fredrickson 1988).

The burgeoning abolitionist movement also fueled the rise of polygenism. Southern slaveholding states needed a strong argument in defense of slavery against pressure exerted by the primarily Northern abolitionists. This pressure would increase after the territorial expansion of the United States following the Mexican-American war, when "Republicans [sought] ways of emphasizing that slavery was incompatible with the hopes of many for a better life in the new territories" (Fredrickson 1988:29). "Scientific" theories that supported the idea of black inferiority provided particularly powerful arguments, given the American public's newfound interest in the natural sciences.

Samuel George Morton (1799–1851)

A respected physician, scientist, and scholar, Samuel George Morton was the de facto leader of the American School. In addition to being the first member of the school to publish an anthropological work, Morton was "one of the giants of the American scientific community" (Michael 1988:349), and the first American to explore human diversity in a rigorously scientific manner. His infamous craniometric studies provided a rallying point for the polygenist movement as objective proof for the diverse origins of humankind and the superiority of the white race.

Morton was born in Philadelphia in 1799 and attended the University of Pennsylvania, graduating in 1820. After finishing his undergraduate studies Morton was sent to the University of Edinburgh by a wealthy Irish uncle, to obtain a medical degree (Stanton 1960). After three years in Scotland Morton earned an MD and returned to the United States to establish a medical practice. Though he would successfully practice medicine in Philadelphia until just before his death, Morton's other scientific interests would be the catalyst for his fame.

Like many scientific men of his day, Morton held broad interests in the natural sciences, and it was his interest in fossils which led to his first taste of the scientific limelight. In 1834 Morton published *Synopsis of the Organic Remains of the Cretaceous Group of the United States*, a description and analysis of the fossils collected by the Lewis and Clark expedition. The publication brought Morton to the attention of the editor of the *American Journal of Science* as well as other prominent geologists (Stanton 1960:26). This work created the scientific reputation Morton would enjoy for the rest of his life, and was important enough for him to be remembered as "a founder of invertebrate paleontology in the United States" (Stanton 1960, Gillespie 1974).

Morton had developed an interest in crania early in his career (Erick-

son 1997), and was actively collecting skulls in the early 1830s; his position as secretary of the Academy of Natural Sciences of Philadelphia afforded him contact with scientists from across the globe (Stanton 1960). An admirer of Johann Friedrich Blumenbach, the pioneering German analyst of human cranial morphology, Morton was sure that the answers to the questions of the unity of the races of man could be found through cranial analysis (Morton 1839). Where Morton diverged from the works of earlier scholars was through his methodology; Blumenbach's analysis of skulls was primarily morphological, but Morton took a strict metrical approach. Morton performed metric analyses of the skulls in his collection and published the results in his "landmark" (Erickson 1997:690) work of anthropology, *Crania Americana*.

Published in 1839, *Crania Americana* is a physically impressive book, befitting its revolutionary content. The volume itself is massive, and its large format (approximately 18" by 13") accommodates strikingly beautiful full-size illustrations of the crania described therein. As its title suggests, the focus of the book is the cranial characteristics of native North and South American populations. Morton prefaced *Crania Americana* with "an essay on the varieties of the human species" (Morton 1839), and it is this essay in which our primary interest lies.

Though reluctant to overtly declare the plural origins of humanity at that time, Morton was the proto–American School polygenist and his essay touches on several of the core concepts of their argument. He notes that "From remote ages the inhabitants of every extended locality have been marked by certain physical and moral peculiarities, common among themselves and serving to distinguish them from all other people" (1839:1); such geographic distribution of traits would become a foundation of polygenist theory, much discussed by Louis Agassiz and adopted by all members of the school. Monogenist theorists also recognized geographic distribution, but postulated that environmental conditions *caused* variation. Morton found the argument that climate and "various collateral circumstances" (1839:2) influenced variation to be simplistic and based on inference rather than proof.

Morton's other reason for doubting that geographical conditions produced variation was a theological one. Religion and science were less separate in the nineteenth century than now, and Morton was only one of many scientists to employ theological reasoning in his science. In this case it seemed implausible to Morton that an omnipotent creator would leave his creations to fend for themselves in unsuitable environments until they adapted. Instead he reached the conclusion "that each Race was adapted

from the beginning to its peculiar local destination. In other words it is assumed, that the physical characteristics which distinguish the different Races, are independent of external causes" (Morton 1839:3).

Morton looked to Blumenbach for his basic classification of mankind and adopted the five-race scheme for his purposes. He was aware that questions of race encompassed questions of unity of species, and states that while Linnaeus, Cuvier, Blumenbach and others affirm that humankind is composed of one species, an opposite view is maintained by scientists like Virey, Desmoulins, and Bory—who would divide the human race into two, eleven, and fifteen species, respectively (Morton 1839). Ever cautious, Morton refrains from offering his judgment and instead calls for a classificatory system based on physical and what he calls "ethnographic" traits (i.e., language). He proposes dividing humankind into twenty-two families within Blumenbach's five-race system.

Morton then enumerates the general properties of each race and of the families within each race. Along with brief descriptions of skin color, hair form, head shape, and various craniofacial features, Morton also describes the intellectual and moral characteristics of the races and families. It is here where Morton's scientific objectivity falters and his work becomes tinged with racial prejudice, especially in statements concerning the Caucasian and Ethiopian races. The Caucasian race, according to Morton, "is distinguished for the facility with which it attains the highest intellectual endowments," while members of the Ethiopian race "present a singular diversity of intellectual character, of which the far extreme is the lowest grade of humanity." The Americans are "slow in acquiring knowledge; restless, revengeful, and fond of war, and wholly destitute of maritime adventure" (Morton 1839:5–7). Describing the various families within the Caucasian race, Morton writes that they are "distinguished by elegance of shape, a fine athletic people," "literary and refined," "polite and polished," "models of perfection, seldom equaled and still more rarely surpassed" (Morton 1839:8–11). His assessment of the Ethiopian families is a stark contrast: "nearest approximation to the lower animals," "repulsive in appearance," "filthy in their persons and gluttonous in their eating; and their dances betray the licentiousness of their morals," "sulky, stupid, and ferocious" (Morton 1839:88–95). Morton's extreme prejudice against non-whites is readily apparent, but not unusual for the time; what really made Morton unique was his reliance on cranial measurements to create a ranking of human races.

Anthropometry is the science of measuring people, both alive and deceased. Within this broad discipline are the subfields of osteometry—measurement of bones—and craniometry—measuring skulls. Interest in

the skull flourished with the rise of the pseudoscience of phrenology in the early nineteenth century (Luyendijk-Elshout 1997). Phrenologists developed many instruments for taking measurements, which were later adapted and adopted by anthropologists (Spencer 1997a). The latter half of the nineteenth century saw the establishment of national "schools" of anthropometry in France and Germany led by Paul Broca (1824–80) and Rudolf Virchow (1821–1902) respectively. Though their work forms the foundation of modern anthropometry, Morton was measuring skulls and publishing the results at least a decade before either Broca or Virchow became involved in anthropology (Spencer 1997b). Morton employed a battery of cranial measurements in his analyses, some nearly identical to those employed by modern anthropologists—the only ambiguity being that of the cranial landmarks. While Morton took a variety of measurements, he held the internal capacity (cranial capacity) as most important and used this measurement to determine his racial ranking.

Morton was the first American to attempt a racial ranking using cranial measurements, but not the only scientist of his time to do so. In Sweden, the anatomist Andreas Retzius created the cranial (cephalic) index, a comparison of the relationship between cranial length and breadth expressed as a single value (Sjovold 1997). Retzius's cranial index (slightly modified) is still used, although as a way to describe a skull's shape rather than to create a racial ranking. Retzius and Morton exchanged crania for study (Morton 1849), but while they took some of the same measurements it is interesting to see how they used these data: where Morton looked to size (internal capacity) as a criterion for classification, Retzius was more interested in shape itself.

Though Morton based his racial classification on cranial capacity, he took a series of thirteen measurements for every skull described in *Crania Americana*; many of these are similar or identical to measurements used by modern biological anthropologists. Using measured cranial capacities—and by extension brain size—Morton produced a racial ranking in accord with his previous observations: Caucasian on top, Ethiopian on the bottom, Mongolian, Malay, and American in the middle (for the modern debate on the accuracy of Morton's cranial measurements and rankings, see Gould 1978 and Michael 1988).

Whether or not Morton's anti-black prejudice influenced the outcome of his study is a moot point. His data and tentative conclusions were widely accepted by the scientific, medical, and academic communities. However, at this point Morton himself refused to make any firm commitments regarding plural origins, human races, and correlation of cranial capacity with intelligence. Morton was a conscientious and cautious

scientist, and felt more observation was needed before he could reach a conclusion.

In 1844 Morton published another craniometric study, this time on Egyptian skulls; many were provided to him by George Gliddon, an ancillary member of the American School and co-author (with Josiah Nott) of the influential polygenist text *Types of Mankind* (Nott and Gliddon 1854). Morton had intimated an interest in Egyptian anthropology in a footnote in *Crania Americana*, asserting that evidence from Egypt demonstrated "that the Caucasian and negro races were as perfectly distinct in that country upwards of three thousand years ago" (Morton 1839:88). Maintaining that it would have been a "physical impossibility" for the races of humankind to diverge from a single pair within the time span allotted by the accepted biblical chronology of four thousand years (Morton 1839), *Crania Aegyptica* would be Morton's empirical study of this hypothesis.

By arguing that the "Caucasian and negro races" had been distinct for a large span of time, Morton could finally begin to overcome a major stumbling block to his support of polygenism—the question of species. Steeped in the tradition of scientists like Linnaeus and Cuvier, Morton accepted that species were immutable. Proof of this fixity could be found by comparing the distinctiveness of the human races in the context of their putative greatest antiquity, ancient Egypt.

In *Crania Aegyptica* Morton relied on the same methods he used in *Crania Americana*, again placing great weight on cranial capacity as a mark of racial distinctiveness and character but also making greater use of the facial angle (first introduced by Petrus Camper in the eighteenth century) as a measure of racial affinity. Not surprisingly, Morton's results closely mirror those he achieved earlier in *Crania Americana*: whites on top, blacks on the bottom. In this study, Morton also proposes a new racial classification of *Negroid* for heads "with decidedly mixed characters, in which those of the Negro predominate" (Morton 1844:96); such Negroid crania are firmly lumped with their Negro brethren and banished to the lower end of the scale.

Morton concludes that the ancient Egyptians were, in physical conformation and cultural achievement, members of the Caucasian race. He also notes that "Negroes were numerous in Egypt, but their social position in ancient times was the same that it now is, that of servants and slaves" (Morton 1844:158). He finally affirms "The physical or organic characters which distinguish the several races of men, are as old as the oldest records of our species" (Morton 1844:158).

In this rather short work, Morton "scientifically" proved that the races

of humankind have been distinct for a very long time—long enough to rule out any external causes for this diversity. While he never says it outright, the implication is that groups of such ancient diversity must be separate species, given the immutability of species. Morton remained dissatisfied with how his work fit within the context of the species concept, however, and it would be several years before he would resolve the species problem to his liking. In the interim he came into contact with other members of the American School, Louis Agassiz (on whom he is said to have exerted tremendous influence [Marcou 1896]) and Josiah Nott, who would take up the cause of arguing for polygeny.

Though he had already produced two works of major importance to the polygenist cause, Morton still had other contributions to make before his death in 1851. In 1849 Morton published a short synopsis entitled "Observations on the Size of the Brain in Various Races and Families of Man." He had revamped his technique for measuring the internal capacities of crania. His original method involved filling the cranium with white pepper seed, and then measuring the volume of the seed in a graduated cylinder (Stanton [1960:31–32] reprints a description of this technique). Morton realized that fine lead shot, with its smaller diameter and greater density, might make for a more accurate measurement, and he set out to remeasure his collection of crania with this method. But even with a different tool, the results were essentially the same; Morton's racial ranking of Caucasians above all others remains unchanged. This essay gave Morton the chance to reevaluate his data and reintroduce his conclusions to an ever more interested audience. Morton also finally offers here his unequivocal support for plural origins: "I may here observe, that whenever I have ventured an opinion on this question, it has been in favor of the doctrine of *primeval diversities* among men" (Morton 1849:223).

But one sticking point remained for Morton: the issues of interfertility and the definition of species (the question remains a contentious point in the biological sciences even today; a working modern definition of species is a "group of interbreeding individuals" [Bowler 1989:13]. Early naturalists like Linnaeus had grouped organisms into species on the basis of morphological similarities and dissimilarities (Linnaeus 1735). George-Louis Leclerc, Comte de Buffon, a contemporary of Linnaeus, was dissatisfied with the species concept as defined by the taxonomists, and defined species as a group that maintained itself through reproduction (Buffon 1749). Implicit in this definition is that mating between members of the same species produces fertile offspring. Since it had long been observed that members of the various human races could mate and produce fertile

children, the question of interfertility provided a major theoretical impasse for the American School.

Morton tackled this problem in 1850 with an address to the Academy of Natural Sciences in Philadelphia, voicing his dissatisfaction with Buffon's definition of species by noting that they "apply as readily to mere varieties as to acknowledged species" (Morton 1850:82). Morton admits that there was not a widely agreed upon definition of what exactly constitutes a species, and joins the fray with a definition of his own: a species was "*a primordial organic form*" (Morton 1850:82). With this definition Morton aligns the concept of species with the idea of *type*, stating that "if certain existing organic types can be traced back into the 'night of time,' as dissimilar as we see them now, is it not more reasonable to regard them as aboriginal, than to suppose them the mere accidental derivations of an isolated patriarchal stem of which we know nothing?" (Morton 1850:82). Morton then addresses the question of interfertility by dealing with it as a question of affinity between species: "*Remote species* of the same genus, are those among which hybrids are never produced. *Allied species* produce *inter se*, an infertile offspring. *Proximate species* produce with each other a fertile offspring" (Morton 1850:82). Thus Morton shows how members of different species are able to produce fertile offspring by virtue of their affinity to each other—though the nature of this affinity is nowhere explicated. It was then possible to declare that the various races of mankind fulfilled all of the criteria of separate species and could be scientifically classified as such.

It must be noted that Morton uncharacteristically provides no evidence for these assertions, but as with the validity of his measurements, this is a moot point for the history of the argument. The polygenist movement (especially Nott) seized upon Morton's new definition of species without question, using this newly opened territory as proof of the plurality of humankind.

Morton was a thorough scientist, committed to empirical methodology and observation. Though his results have been questioned, Morton was the only member of the American School to make use of rigorously collected data in the furtherance of his argument. In most cases he showed great restraint when drawing conclusions without sufficient data, making him unique among his polygenist colleagues. Morton also made important contributions to physical anthropology with his development of craniometric methodology. Unfortunately, his observations also fueled the arguments of the pro-slavery movement in America, and his stature as a physician and scientist lent credence and legitimacy to the scientific racism espoused by other members of the American School.

Louis Agassiz (1807–73)

Born in Switzerland in 1807, Jean Louis Rodolphe Agassiz was a consummate nineteenth-century natural scientist with expertise in biology, zoology, anatomy, and geology. A pupil of Georges Cuvier, Agassiz came to prominence through his study of fossil fish (Agassiz 1833) and it was this passion which drew him to the United States in 1846 (Stanton 1960:100), where he almost immediately became embroiled in the debate over the unity of the human races.

Central to his zoological interests was the geographical distribution of animals, including humankind. A pious man and devout creationist, Agassiz initially saw humankind's universal distribution upon the earth as evidence for its divine creation and unified origin (Agassiz 1845; Agassiz and Gould 1848). He publicly espoused this view during a series of lectures entitled "Plan de la Création" given in Switzerland shortly before his departure for America (Stanton 1960). Seemingly committed to the theory of unified origin, and mainly on the basis of his 1845 article "Notice sur la géographie des animaux," Agassiz was cited as an authority on the subject by John Bachman (1850). Ironically, by that date Agassiz had undergone a profound ideological shift and had become an outspoken advocate of polygenism.

What was the cause of this sudden shift? Lurie (1954) postulates that as Agassiz refined his theories of geographic distribution he began to see inconsistencies within the idea of a unified center of origin. Agassiz's friend, former secretary, and biographer Jules Marcou wrote, "It was hard for him to abandon this view; but he was too thorough a naturalist, and had a too exalted an idea of the immutability of species, like his master, Cuvier, to believe in only races for man" (1896:293). While these explanations are supported by his work to some extent, Agassiz himself offers an equally compelling personal reason. Prior to his arrival in Philadelphia in 1846, Agassiz had never seen a black person. His immediate reaction was visceral and extremely negative:

> I can scarcely express to you the painful impression that I received, especially since the feeling that they inspired in me is contrary to all our ideas about the confraternity of the human type [*genre*] and the unique origin of our species. But truth before all. Nevertheless, I experienced pity at the sight of this degraded and degenerate race, and their lot inspired compassion in me in thinking that they are really men. Nonetheless, it is impossible for me to reprocess the feeling that they are not of the same blood as us. In seeing their black faces with their thick lips and

> grimacing teeth, the wool on their head, their bent knees, their elongated hands, their large curved nails, and especially the livid color of the palm of their hands. I could not take my eyes off their face in order to tell them to stay far away. And when they advanced that hideous hand towards my plate in order to serve me, I wished I were able to depart in order to eat a piece of bread elsewhere, rather than dine with such service. What unhappiness for the white race—to have tied their existence so closely with that of negroes in certain countries! God preserve us from such a contact! (S. Gould 1996)

The above passage is from a letter written by Agassiz to his mother in December 1846. His disgust and hostility towards blacks is palpable, and likely a major factor in his sudden shift towards polygenist philosophy—especially considering that in the weeks just prior to his arrival in the United States Agassiz had been a proponent of the unity of origin of humankind.

Agassiz publicly took on the mantle of polygenism during his first series of American lectures, given during the winter of 1846–47 in Boston (Lurie 1954:234). In 1848 he accepted a professorship at Harvard University and continued publishing in earnest, especially works on zoology and his theories on the "structure, development, distribution, and natural arrangement of the races of animals" (Agassiz and Gould 1848). His first American book was *Principles of Zoology* co-authored with Augustus Gould. *Principles* was intended for beginning students of zoology and thus makes a useful starting point for the examination of Agassiz's scientific ideas concerning biological diversity. For the sake of context, several generalizations about Agassiz's worldview must be made:

> 1. *A "Divine Creator" was responsible for all life on earth.* "The history of the earth proclaims its Creator" (Agassiz 1842:399).
>
> 2. *The natural world followed a pre-determined plan towards a goal.* "It is only as it contemplates, at the same time, matter and mind, that Natural History arises to its true character and dignity, and leads to its worthiest end, by indicating to us, in Creation, the execution of a plan fully matured in the beginning and invariably pursued; the work of a God infinitely wise, regulating Nature according to immutable laws which He has himself imposed on her" (Agassiz and Gould 1848:10).
>
> 3. *Organisms have a natural rank in nature's order.* "A sketch of this nature should render prominent the more general features of animal

life, and delineate the arrangement of the species according to their most natural relations and their rank in the scale of being" (Agassiz and Gould 1848:5).

4. *Man occupies the top position in the natural order.* "Man, who stands at the head of Creation, is in this respect also the most highly endowed being" (Agassiz and Gould 1848:21).

It is within this divinely inspired, teleological framework that Agassiz drafted his theory of geographical distribution, which in turn was used to support his theory of diverse origins.

Agassiz begins *Principles* with a brief explanation of how animal life is classified. Central to this explanation is his definition of what exactly constitutes genera and, more importantly, species. Agassiz warns the beginning student that while these terms are most common in zoology, they are not necessarily easy to understand. In his own words,

> The Genus is founded upon some of the minor peculiarities of anatomical structure, such as the number, disposition, or proportions of the teeth, claws, fins, & c. and usually includes several kinds. The Species is founded upon less important distinctions, such as color, size, proportions, sculpture, & c. Thus we have different kinds, or species, of duck, different species of squirrel, different species of monkey, & c., varying from each other in some trivial circumstance while those of each group agree in all their general structure. (Agassiz and Gould 1848:xiv)

This reliance on physiological expression of traits for species classification creates a very loose, open system. Agassiz was known to be a "splitter" when it came to classification, and this system met his needs nicely; closely tied to this conception was Agassiz's view that species, as the products of divine creation, were absolutely fixed and immutable (Bowler 1989:128).

In addition to being immutable and of divine origin, species also were distributed geographically in their ideal habitats: "Each grand division of the globe has animals which are either wholly or for the most part peculiar to it" (Agassiz and Gould 1848:175). Agassiz explains that organisms were created in favorable geographic zones, and humankind, while universally distributed, nevertheless conforms to the law of geographical distribution through racial differences, with geographical factors such as climate and availability of food animals shaping the "physical constitution of man, which would contribute to augment any primeval differences" (1848:180–181). Curiously, here Agassiz and Gould return to the

monogenist position that while there are many races of man, they belong to the same species, directly contradicting the position Agassiz took during his first American lecture tour one year earlier. It would be two more years before Agassiz explicitly published his stance on the plural origins of humankind.

"The greatest obstacles in the way of investigating the laws of the distribution of organized beings over the surface of our globe, are to be traced to the views generally entertained about their origin" (Agassiz 1850a:181). Thus begins "Geographical Distribution of Animals," Agassiz's second American work on his distribution theory and a refinement of his earlier ideas. Agassiz is quick to note that his scientific views are in accord with scripture and that the notion of a unified origin is not. He states that proof of unified origin is nowhere to be found in Genesis and that the account of Adam is not representative of the creation of all life on earth, but rather one instance of many creation episodes; the idea of Adam and Eve being the first or only human beings ever created is directly contradicted, he argues, by Moses' account of his cursed son, Cain, wandering and finally building a city in the land of Nod, from whose inhabitants he took a wife (Agassiz 1850a). Agassiz concludes his exposition of human and animal origins: "It is not for us to inquire further into the full meaning of the statements of Moses. But we are satisfied that he never meant to say that all men originated from a single pair, Adam and Eve, nor that the animals had a similar origin from one common centre or from single pairs" (Agassiz 1850a:185).

Agassiz notes that the earth may be divided into "zoological provinces," each encompassing a range of land and sea environments and their respective flora and fauna. The inhabitants of each province are uniquely suited to live there and cannot naturally survive outside of it. Moreover, different species will be found within subdivisions of each province, a result of their being placed there by a divine power. Agassiz uses the example of fish in three European river basins: the Danube, the Rhine, and the Rhône. Being in the same province, the three rivers share some of the same species of fish, notably eel, trout, and pickerel, but in the case of perch different species are found in the three rivers. Agassiz asks why these closely allied species are not equally distributed if they originated centrally and migrated to the rivers, and takes this differential distribution as proof that the species were created separately in each river (Agassiz 1850a:191). He concludes, "Here, again, we arrive at the conclusion . . . that species do not originate from single pairs, but in natural proportion with the other species with which they live simultaneously over the whole ground which they cover" (Agassiz 1850a:192).

Agassiz refrains from addressing the origins of humankind again until the last paragraph of the essay. He notes that since the "principal races" of man are distributed amongst the zoological provinces in the same manner as the animals the differences among them must also be the result of their separate creations. Foreshadowing his second 1850 publication, Agassiz concludes, "But for the present we shall abstain from further details upon a subject involving so difficult problems as the question of the unity or plurality of origin of the human family, satisfied as we are to have shown that animals, at least, did not originate from a common centre, nor from single pairs, but according to the laws which at present still regulate their existence" (Agassiz 1850a:204).

July of 1850 saw the publication of what is perhaps Agassiz's most infamous anthropological work: "The Diversity of Origin of the Human Races." Offering much contradictory information, this article lacks the clear and precise writing found in Agassiz's earlier works. He jumps immediately to the offensive, stating that questions of human variation should be examined solely as scientific questions and without reference to politics or religion (Agassiz 1850b:110).

Agassiz makes a distinction between the "Unity of Mankind and the Diversity of Origin" (1850b:110), speaking of the spiritual bond amongst all members of the species. He asks "Do we cease to recognize this unity of mankind because we are not of the same family?—because we originate in various countries, and are born in America, England, Germany, France, Switzerland?" (1850b:110). It is already obvious that Agassiz seems to reserve his feelings of unity for other white men. He contends that Genesis accounts only for the history of the white race, and holds no mention of "colored" races of men such as "Mongolians" or "negroes," and asserts that the notion of common descent is "a mere human construction entitled to no more credit, and no more confidence, and no more respect, than any other conclusion arising from philosophical investigations of this subject from a scientific point of view" (Agassiz 1850b:135). Unfortunately, Agassiz fails to provide sound scientific evidence for his own conclusion. Gone are the myriad examples drawn from observation that characterize his earlier works on geographic distribution, replaced by unsubstantiated assertions (cf. Gould 1996:76).

Louis Agassiz, while a brilliant naturalist, allowed his prejudices to influence his analyses and relax his scientific rigor when writing on human diversity. As a nineteenth-century scientist, Agassiz's scientific paradigm was centered on classification and ranking. A longtime follower of the German Idealist tradition (Bowler 1989), Agassiz believed that organisms had a relative rank according to their degree of perfection as

measured against a primordial ideal. Agassiz based much of his work on human diversity and ranking upon analogy with animals, and pursued it with zeal:

> That there are upon earth different races of men . . . requires farther investigation, and presses upon us the obligation to settle the relative rank among these races, the relative value of the characters peculiar to each, in a scientific point of view. . . . [A]s philosophers it is our duty to look it in the face. It will not do to assume their equality and identity; it will not do to grant it, even if it were not questioned so long as actual differences are observed. (Agassiz 1850b:142)

In his biography of Agassiz, Marcou speaks of him as a "savant whose domain is entirely outside of all institutions of society" (1896:294). Agassiz fails in this respect when we consider his conclusion to "Diversity of Origin of the Human Races":

> What would be the best education to be imparted to the different races in consequence of their primitive difference, if this difference is once granted, no reasonable man can expect to be prepared to say, so long as the principle itself is so generally opposed; but, for our own part we entertain not the slightest doubt that human affairs with reference to the colored races would be far more judiciously conducted, if, in our intercourse with them, we were guided by a full consciousness of the real difference existing between us and them, and a desire to foster those dispositions that are eminently marked in them, rather than by treating them on terms of equality. (Agassiz 1850b:144)

When Agassiz offered suggestions for the treatment of others based on his scientific views of race, he became a scientific racist. It is unimportant whether or not his advice was followed by policymakers; it is clear that his words are aimed at those who make public decisions. Agassiz was a prominent scientist well aware of his position of influence; he was instrumental in improving the quality of government-funded scientific publications in America (Marcou 1896) and assisted in the founding of Cornell University (Lurie 1960). Many educated and powerful people admired Agassiz and listened intently to what he had to say.

That Agassiz's personal prejudices manifested themselves in scientific racism was not unusual in nineteenth century America. Prejudice and racism were rampant in all spheres of society, not just science. Even those who opposed slavery and were proponents of equality often held deep

prejudices against non-whites. This is not meant to excuse Agassiz, but rather to illustrate how his personal prejudice, combined with peer influence and broadly held social ideas, became scientific racism. In turn, this ideology was embraced by Josiah Nott and elaborated in his *Indigenous Races of the Earth* (Nott and Gliddon 1857) and *Types of Mankind* (Nott and Gliddon 1854).

Josiah Nott (1804–72)

The American School's most passionate defender of polygenism, Josiah Clark Nott was born on March 31, 1804, in Columbia, South Carolina. In the fall of 1822 Nott entered South Carolina College as a sophomore (Horsman 1987:16); there he encountered Thomas Cooper (first a professor then a president of the college), an educator who would leave indelible impressions.

Cooper held unorthodox views on religion and was especially passionate about challenging the scientific authority of the Bible (Horsman 1987). Perhaps even more importantly, Cooper was a staunch proponent of states' rights, especially pertaining to slavery. The economy of South Carolina depended on slavery for industry as well as agriculture (Starobin 1970), and Cooper saw fit to defend these interests on the grounds that blacks were inherently inferior to whites—a statement he published in a Columbia newspaper in 1823 and continued to defend (Malone 1926). This zeal to separate biblical from scientific concerns coupled with the doctrine of inherent racial inferiority would be a hallmark of Nott's scientific paradigm and anthropological works (Erickson 1986, Horsman 1987, Stanton 1960).

Nott was first and foremost a physician. He embarked upon his professional training immediately after his graduation from South Carolina College in 1824, first studying in New York and then Philadelphia, where he earned the MD degree from the University of Pennsylvania in 1827 (Carmichael 1948). After a few years of private practice, Nott traveled to Paris to continue his medical education. It was something of a fashion amongst American physicians in the nineteenth century to obtain a European medical degree (Erickson 1986:105), and 1830s Paris was a favored destination, with over two hundred American physicians studying there over the course of the decade (Horsman 1987).

Nott's year of study in Paris exerted tremendous influence over the rest of his career. The focus of the French system of medicine was observation and analysis: physicians attempted to ascertain the causes of illness by observing both living patients in hospitals and through autopsy of those

who had succumbed. A great emphasis was placed on statistics and numerical analysis (Horsman 1987:45). French physicians followed a strict physiological approach to medicine and made contributions to the sciences of physiology and pathology (Erickson 1986:106). This rigorously scientific approach coupled with its emphasis on physiology was enthusiastically adopted by Nott. After his return to the United States he often performed autopsies and published the results (Erickson 1986). The Nott family soon settled in Mobile, Alabama, where Nott established a medical practice and organized the Mobile Medical Society in June 1841 (Horsman 1987). During this time Nott began publishing in the prestigious medical journals of the day and also began to produce his first works on anthropology.

Nott's first anthropological publication appeared in the July 1843 edition of *The American Journal of the Medical Sciences*. Entitled "The Mulatto a Hybrid—Probable Extermination of the Two Races if Whites and Blacks Are Allowed to Intermarry" (hereafter "The Mulatto"), the article serves as a good introduction to Nott's version of the anthropological paradigm.

Nott's primary source for "The Mulatto" was an article published in the *Boston Medical and Surgical Journal* (1842). The anonymous author, "Philanthropist," argues that mulattoes are weaker, more susceptible to disease, and suffer higher mortality than their parents. Nott agrees with this position and expresses his pleasure that the article appeared in a Northern journal and was thus free from "sectional prejudice or the influence of self interest" (Nott 1843:253). After citing "Philanthropist's" "notoriously inaccurate" (Erickson 1986:108) medical statistics, Nott offers his own observations based on his fifteen years of medical practice in the South, "where the population is pretty equally divided between the blacks and whites" (Nott 1843:253).

Nott's conclusions concerning mulattoes run the gamut from comments on their "intermediate" intelligence, their "less prolific" fertility, and, paradoxically, both their being "subject to many chronic diseases" and their greater resistance to yellow fever (1843:253). Nott admits that his assertions have no statistical backing and insists he is offering "materials for reflection" (1843:254). Clever Nott puts the burden of proof on the North, writing that the "habits and condition of the Mulattoes in the South render it extremely difficult to obtain satisfactory statistics," and "In the Northern cities ample materials exist, for investigating this subject" (1843:253), leaving him free to make claims with little chance for rebuke.

Nott's main premise is that mulattoes are *hybrids*. Hybridity is inex-

tricably linked to the concept of species, which in turn formed the basis of Nott's anthropology and his polygenist views. Nott states: "I do believe, *that at the present day the Anglo-Saxon and the Negro races are, according to the common acceptation of the terms, distinct species, and that the offspring of the two is a Hybrid*" (Nott 1843:254), although as previously noted there was no "common acceptation" at the time on the definition of species. Nott offers a comparison of a white and a black female to illustrate the great differences between them: "Look first, upon the Caucasian female with her rose and lily skin, silky hair, Venus form, and well chiseled features—and then upon the African wench, with her black and odorous skin, woolly head and animal features—next compare their intellectual and moral qualities, and their whole anatomical structure, and say whether they do not differ as much as the swan from the goose, the horse and the ass, and the apple and pear trees" (Nott 1843:254). Nott's distaste for blacks is evident throughout his writings, and this emotional reaction was likely the impetus behind his defense of polygenism.

Nott's next foray in anthropology took the form of two lectures he delivered and published in Mobile entitled "The Natural History of the Caucasian and Negro Races" (Nott 1844). One focus of these "Lectures" was an attack on biblical chronology. Like Morton, Nott points to evidence from Egypt to argue the antiquity of the separate races and to show that the chronology of the Mosaic account is faulty. Nott accounts for the diversity of races (both human and animal) as the result of separate events of creation and destruction. He also attacks the argument for transformation, noting that when wild animals are domesticated they undergo morphological and behavioral changes. Nineteenth-century proponents of unity believed that the same process took place in humans, but Nott categorically rejects this theory as being logical rather than observational. As a physician and scientist Nott refused to believe anything he could not observe, and as a product of the nineteenth-century Southern intelligentsia he particularly rejected evidence that even intimated the unity of humankind.

Nott's two lectures sparked a lively debate with Reverend Moses Ashley Curtis in the *Southern Quarterly Review* (Nott 1845a). Curtis (referred to as "C" in the journal) rebuffed Nott's polygenist theorizing in a critical letter to the journal. Nott, who delighted in baiting and attacking members of the clergy, obliged with a response of his own. Nott's reply is primarily composed of attacks on biblical chronology and Curtis himself, and it needn't be considered here except as another illustration of Nott's *modus operandi*: in lieu of scientific facts, attack and obfuscate.

Nott's next anthropological publication also appeared in the *Southern Quarterly Review*, entitled "Unity of the Human Race." Here Nott returns to the concept of species in regards to human races. He admits that a precise definition of species is difficult and relies on one offered by Desmoulins, a French naturalist. He writes: "it is the permanence of type under opposite influences which constitutes species" (Nott 1846:4). The concept of fixity of species was of primary importance; if a species was unable to undergo modification, diversity could only be accounted for by the creation of new types.

Nott also expanded his views on the geographic distribution of humans and animals, enthusiastically supporting the idea that plants, animals, and humans were created in and restricted to their ideal habitats. He attempts to show how organisms that are adapted to certain environmental conditions cannot adapt to greatly different ones: "I can find no well authenticated facts to prove that the Caucasian race can be successfully colonized in tropical Africa, while we have many facts to prove the contrary. Even in the milder climates, as the Southern parts of Spain, Italy, Greece, the West Indies, Southern potion of the United States, etc., do we not see the White race manifestly deteriorate physically?" (1846:9). What sparked Nott's interest in the idea that different races were suited to specific environments (and by extension were separate species) were observations he had made during the epidemics of yellow fever that scoured the South in the late 1830s and early 1840s. Nott observes, "I have never, during my residence in Mobile, seen a full blooded Negro attacked by yellow fever, though there are in the city, during the summer, more unacclimated Blacks than Whites" (1846:10). He then employs specious reasoning to explain this immunity as a difference in species: "Negroes are unquestionably very similar, in their organization and functions, to the White race,—and both analogy and observation would teach us, that most external influences would produce on them similar effects. It would really be very singular, that any poison, as malaria, etc., should produce a baneful influence on one race of men, and not effect, at all, the others" (1846:11). Here Nott implies that if human races were really the same species, one could expect to see them affected by disease in the same way. Though Nott was unquestionably an expert on yellow fever (Nott 1845b, 1848; Erickson 1986; Horsman 1987) and witnessed a differential susceptibility to the disease, his explanation falls short and derives more from his own personal prejudice than fact.

Despite Nott's questionable practice of anthropological science, he enjoyed a sterling reputation as a physician. It was the strength of this reputation (much like Morton's) that made him such a dangerous an-

thropologist. It was assumed by the public that his anthropological observations rested on as solid a foundation as his medical works, though I have shown above that this is not the case. It was precisely this reputation that led to the publication of Nott's 1847 article "Statistics of Southern Slave Population with Especial Reference to Life Insurance."

Commissioned by the *Commercial Review of the South and West*, this attempt to assess whether or not slaves make good risks for life insurance is one of the earliest works of medical anthropology. Despite offering statistics demonstrating the greater longevity of blacks in the South, Nott concludes that they are poor insurance risks as their owners, when in need of money, could easily kill them off to collect the premiums, thereby penalizing the other members of the mutual insurance group (Nott 1847). This analysis makes up only a very small part of the paper, which steps beyond its focus to defend slavery as the natural condition of blacks. He goes so far as to say "I think we may safely conclude, that the negro attains his greatest perfection, physical and moral, and also his greatest longevity, in a state of slavery" (Nott 1847:281). Here Nott addresses the issue of unity very simply: "No one at all familiar with the past history of the negro and his present peculiarities, can entertain a doubt that he is now very widely separated, both in *physique* and *morale*, from the white man" (1847:277). The only proof Nott offered is the assertion that Egyptian monuments and art show "that the negroes existed at that early day with all the physical characteristics they now possess, and that they were treated and spoken of as slaves and barbarians" (1847:277).

Appended to the 1847 article was a letter to the editor written by Nott entitled "The Slave Question," essentially reiterating his claims made in "Statistics of Southern Slave Population." With an air of mock philanthropy, Nott expresses his intense personal connection with the issue: "I am a slave owner, and while on the one hand I shall, in common with the Southern people, resist all encroachments on our constitutional and natural rights, I am, on the other hand, free to say that I am ready to advocate any scheme of emancipation which will insure to the slaves of the South greater happiness than they now enjoy" (1847:288).

Defending slavery through science would remain Nott's focus in the following years, and although a fairly prolific writer he relied on the same basic arguments time and time again. In 1851 he published *An Essay on the Natural History of Mankind, Viewed in Connection With Negro Slavery*. What is new in this work is Nott's formal recognition of the role of science in the slavery controversy and the political implications of his work, the ferocity of his attack upon the intellectual, physical, and moral attributes of blacks, and his militant stance in defense of slavery. He

begins with a statement about the growing public and political interest in the unity of humankind:

> These investigations are assuming a peculiar interest in our country, from their connection with certain absorbing political conditions now deeply agitating the American people, and shaking the very existence of our Government to its foundations; and knowing that I have been engaged in the study of the Natural History of Mankind, I have been requested to prepare and lay before you in a condensed form, an abstract of those facts and deductions, which have a practical bearing on the great issues now pending between the North and South. (Nott 1851:4)

Nott is obviously aware of his influence and position of authority in the debate. Moreover, he writes, "In all I have written heretofore about the Races, I have attempted as much as possible to confine myself to the *scientific* view of the subject, hoping that the facts once made out would gradually find their practical application through others" (1851:5).

In closing this essay Nott engages in some saber rattling. The issues of states' rights were growing ever more contentious and much of the controversy centered on slavery. Nott claims that the Southern states were being denied the right to secede from a federal government which enforces its policies "at the mouth of the cannon" (1851:24). He advocates drastic action: "I would say to this Association, that I think one of their most prominent objects should be, to urge upon the Legislature, the establishment of a Military School—a better organization of the Militia—a provision for all the munitions of war & c." (1851:25).

Nott intentionally used his influence and his science to defend the subjugation of blacks through slavery, reiterating the same flimsy arguments throughout his publications, never offering sound evidence. One could perhaps make the argument that Nott was just a poor scientist, but a review of his "On the Pathology of Yellow Fever" (1845b) shows this to not be the case. As a scientist, Nott rigorously adhered to the practical philosophy of nineteenth-century French medicine, especially autopsy and pathological analysis. This approach is apparent in the thorough descriptive writing that characterizes Nott's medical work, where he also proffers conclusions based on the observation of empirical evidence rather than personal feelings. It is hard to believe, given his competency in medical science, that Nott was unaware of the serious flaws in his arguments about human variation, and we must conclude that ultimately, like Louis Agassiz, Nott's personal attitude towards blacks was at the root of his unabashedly racist anthropology.

The Legacy of the American School and Its Polygenism

Morton, Agassiz, and Nott presented a unified front in the defense of polygenism for over a decade before the American School went into sharp decline. With the death of Morton in 1851, Nott and his partner George Gliddon rose to the helm and assumed leadership. Together they would publish one final book of their polygenist views, *Indigenous Races of the Earth*, in 1857. Though smaller than its predecessor *Types of Mankind* (1854), this book is even more disjointed and rambling and presents no cohesive argument. Nott himself commented on the work "Gliddon has surpassed himself in folly & confusion" (Horsman 1987:219). Later in the year Gliddon would be dead and Nott, though remaining a vocal supporter of slavery and states rights, would turn his attention away from anthropology and back toward medicine until his death in 1872.

Always a man of diverse interests, Agassiz would also shift his attentions away from ethnology; an introductory letter in *Indigenous Races of the Earth* would mark his final association with the American School. A new enemy would soon appear in the form of Charles Darwin and *On the Origin of Species* in 1859, and Agassiz would gain even greater notoriety for his anti-evolutionist views. His racism continued to grow, however, and in an 1863 letter to Samuel Howe of the United States Sanitary Commission he advocates limiting the freedoms of blacks: "Let us beware of granting too much to the negro race in the beginning lest it become necessary hereafter to deprive them of some of the principles which they may use to their own and our detriment" (Haller 1971:86).

The dissolution of the American School and the arrival of Darwin didn't necessarily spell the end for polygenism. John Van Evrie, A New York physician, would take up the cause and publish two editions of his overtly racist book *White Supremacy and Negro Subordination; or, Negroes a Subordinate Race and (So-called) Slavery Its Normal Condition* (1868; cf. 1861), using many of the core concepts presented by the American School for his defense of slavery and black oppression. The work of the American School regarding the antiquity of human races in Egypt also helped to fuel the "pre-Adamite" and "Hamitic" theories that flourished in the last decades of the nineteenth century (Sanders 1969; Lester 1875; Philips 1868), but eventually the American School and the bulk of their ideas would fade into obscurity, save for a paragraph or two in texts.

Nevertheless, the work of the American School was an important part of the genesis of biological anthropology. Though I found no direct evidence, it is plausible that Franz Boas's landmark studies of the cranial

form of immigrants (Boas 1912, 1916) were a reaction to ideas put forth decades earlier by Morton, Agassiz, Nott, and Gliddon. Additionally, modern forensic anthropologists use cranial measurements, many of which are nearly identical to Morton's, to determine the biogeographical ancestry of unidentified human remains (Ousley and Jantz 1996).

The positive contributions of the American School scientists are overshadowed by the lasting negative impact of their core ideal—that certain races are inherently, biologically, inferior. While Morton, Agassiz, and Nott were not the first scholars to put forth the idea of inherent racial inferiority, they were certainly the first to bring this ideology to the general public on a large scale. Their numerous publications and lectures coupled with their formidable academic and scientific credentials insured that their ideas were widely disseminated. Their legacy is still felt today as modern writers like Herrnstein and Murray (1994), Rushton (2000), Sarich and Miele (2003), and Entine (2000) perpetuate the twin myths of race and biologically determined inferiority. Clearly, anthropology has an influence far beyond the borders of our discipline. As socially responsible scholars (and it is imperative that we are indeed socially responsible), anthropologists need to look back at the history of our discipline and ask hard questions about the nature of our research, particularly concerning the value of attempting to rank humans along a scale of superiority and inferiority. If we are honest, and if we have learned the lessons of history, the only answer must be a resounding "no value."

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8. Missing Ancestors and Missing Narratives

Andrew Lyons

This paper evaluates Sir Richard Burton's claim to be an intellectual progenitor of a discipline which abhors his values and ignores his work. Burton's reputation recently underwent a paradoxical revival, particularly in some gay circles, although he was overtly homophobic, racist, sexist and anti-Semitic. His links to critics of Victorian censorship and sexual orthodoxy (as well as to the pornographic imaginary) are examined along with his writings on African, European, Indian, Mormon, and Semitic sexuality and marriage. His career may be compared with that of other, rarely acknowledged disciplinary ancestors such as Verrier Elwin, Edward Carpenter, and Havelock Ellis, all of whom discussed sexuality (see below; see also Lyons and Lyons 2004:100–130, 231–238). In general terms we ask why some names, careers, and narratives are included in or excluded from histories of anthropology. These processes obviously influence our choices as to which books we shall read and which messages we shall heed, whether we are anthropologists, historians of anthropology, ethnohistorians, students or lay people. A decision to omit someone from a historical survey may be overdetermined by many disciplinary and political traditions.

Sir Richard Burton (1821–90) was vice president of the Anthropological Society of London—one of the two parents of the Royal Anthropological Institute—and briefly president of the London Anthropological Society, although he took part in few meetings because he was continually abroad. He served as a soldier in India in the 1840s and took part briefly in the Crimean War. He served as a somewhat undiplomatic diplomat in Fernando Po, Brazil, Damascus, and Trieste. He explored Arabia, Somalia, Ethiopia, and the area near Lake Tanganyika. His acrid dispute with John Hanning Speke over the source of the Nile led to the latter's (presumed) suicide. Burton spent three months in Dahomey and visited the Yoruba. He traveled through the North American Plains and visited Utah. Most notably, he visited Mecca disguised as a pilgrim. He

translated the *Kama Sutra*, *The Perfumed Garden*, the erotic poetry of Catullus, and produced a multi-volume edited translation of *The Arabian Nights*. He spoke twenty-five languages or dialects. He was a pioneer in the anthropological study of sex and a savage critic of Victorian sexual orthodoxy ("Mrs. Grundy"). He developed the first anthropological theory of homosexuality. He also edited a collection of West African proverbs, and wrote about Plains Indian sign language. We shall document many reasons why he should be included in histories of anthropology.

Burton has been the subject of many major biographies. These include the works of Byron Farwell (1963), Fawn Brodie (1967), Edward Rice (1990), Frank McLynn (1990), Mary Lovell (2000) and Dane Kennedy (2005). McLynn and Kennedy (particularly) have quite a bit to say about Burton's anthropology. Other scholars who have written about Burton include Stephen O. Murray (1997), Patrick Brantlinger (1988) and Mary Louise Pratt (1992). To the late Edward Said (1979:194–197) Burton was the least intolerable of the major nineteenth-century Orientalist scholars: "He was preternaturally knowledgeable about the degree to which human life in society was governed by rules and codes" (1979:195). Because of his reputation as a swashbuckling explorer Burton is the subject of TV documentaries from time to time and he was also the subject of a popular film, *Mountains of the Moon*. Because of his terminal essay (in volume 10 of *The Thousand Nights and a Night*) and his unpublished (and perhaps nonexistent) report to Napier about male brothels in Karachi, the ostensibly homophobic Burton has become an avatar of gay liberation. He was revived from the dead in order to reappear in 1990s Toronto as a curator of an exhibit about AIDS who is dissuaded from his homophobic obsession with Patient Zero when he falls in love with another male protagonist in the film musical *Zero Patience*. However, he was not revived in the pages of Robert Lowie, T. Penniman, Marvin Harris, John Honigmann, or Alan Barnard. He makes a brief and derisory appearance in Stocking's *Victorian Anthropology* (1987:253) and in *After Tylor* (1995:28)—significantly enough in a footnote in which the author explains why he excludes people like Burton and topics in the history of sexuality. I suspect that he may be included in some future histories of the discipline, because we may be less inclined to find charter myths for the present in the histories of our past. However, having documented Burton's exclusion from the histories of our discipline, I must now try to explain it.

Histories of anthropology in many ways reflect the newness of the discipline and the development of its identity. Initially, it was necessary to distinguish armchair anthropology conducted by academics from the incidental collecting of data by missionaries, travelers, and traders. The

latter work was often unsystematic. At first, the armchair theorist was accorded status, and the collector of field data was regarded as a rank amateur. By the turn of the nineteenth and twentieth centuries, the new model of anthropological fieldwork, based on geology and biology, accorded respectability to the gatherers of data at a time when armchair speculation was getting more than a little jaded (Kuklick 1997). The institutionalization and professionalization of anthropology were in full swing at the beginning of the last (twentieth) century, although there was still a long way to go.

Half a century earlier, Burton's travels in Arabia had been undertaken for his own purposes. However, his trip to the inland lakes with Speke was financed by the Royal Geographical Society, who demanded detailed descriptions of flora and fauna of all kinds (humans included), thereby making it patent that Burton was merely their servant (Kennedy 2005:101–106). Burton inserted comments on a number of anthropological topics into portions of his travel narratives as well as in miscellaneous papers and reviews, but he was not prepared to accept the status of anthropological gopher. His decision to support James Hunt's new Anthropological Society of London, a somewhat ragtag body, shows, if nothing else, that he did take his anthropological proclivities very seriously. Nonetheless, the fact he traveled far and was literally marginalized (in a geographical sense) may have led to his exclusion from our discipline's received history as much as his failure to produce an armchair piece of evolutionary theory, as did Morgan, McLennan, and Tylor.

Patrick Brantlinger has suggested that the fact that Burton "shows up in none of the standard histories of that discipline perhaps stems from the understandable desire of modern anthropologists to play down an influential but embarrassing heritage of racism and imperialism" (1988:159). There can be no doubt that Burton's racism was excessive even by the standards of the Victorian period. He was a polygenist like many other members of the Anthropological Society of London. His racism is particularly apparent in some of his discussions of black Africans. He despised most of the inhabitants of East Africa. Burton regarded the "pure negroes" of West and Central Africa as cruel and stupid (McLynn 1990:215–216). Burton thought that "the peculiar development of destructiveness in the African brain" was "the work of an arrested development, which leaves to the man all the bloodthirstiness of the carnivore" (Burton 1924:100). During discussion of his paper on Dahomey, which he read to the Anthropological Society of London in the mid-1860s, Burton agreed with Governor Freeman of Lagos that Islam had deservedly had better success than Christianity in converting blacks. He attributed Islam's suc-

cess to the simplicity of the religion (Burton 1865c:ix, 1924:132). In answer to a question, he stated his belief that the "pure negroes" would be "improved off the face of the earth" by intermixture with mixed-race "negroids," many of whom were Muslims with partial North African ancestry (Burton 1865:vi, x; 1924:131). Skeptical about the benefits of humanitarian intervention and missionary education in Africa, Burton made some exceptions to his bleak portrayal of the "pure negro." He seems to have had a little respect for the Yoruba of Nigeria who, despite their "prognathous, chinless, retreating face, simulating the Simiadae," possessed admirable forms and figures (Burton 1978:44, 1967:127). This beauty was probably the result of prolonged taboos on intercourse during the periods of gestation and lactation, a practice that regrettably had been abandoned in monogamous European societies (Burton 1978:45). He publicly defended slavery because it was one of the few institutions that might improve blacks, although he condemned it in a privately published poem, "Stone Talk" (Burton 1865a).

After a bitter encounter with Jewish moneylenders and conflict with Turkish authorities, Burton was recalled from his position as British consul in Damascus, and his anti-Semitism grew by leaps and bounds. He wrote an essay accusing Jews of human sacrifice and ritual slaughter of Christian children, reviving blood libels from the supposed murder of Hugh of Lincoln in 1255 to a nineteenth-century incident in Damascus. The essay, "The Jew," can be found on the neo-Nazi web site JRBooks Online.com; it is not otherwise available. Burton is much admired by many neo-Nazis.

One does not have to be professing political correctness to find passages in Burton so offensive that one grimaces when one reads them. The idea of polygenesis in which Burton professed belief was built on sometimes conflicting ideas of hierarchies in sexual morality and sexual expression, sexual incompatibility and improper sexual crossings (the term "miscegenation" apparently dates from the mid-nineteenth century). Like other polygenists from the eighteenth century to the present day, Burton indulges in the usual fictions about genital size among male Africans, but he does not confine himself to regurgitating old chestnuts. As Harriet Lyons first noted in 1981, nineteenth-century ideas of sexual and racial hierarchy were strongly expressed in occasional discourses about male circumcision and subincision and the various forms of female genital cutting and elongation. Arguably Burton made a genuine contribution to anthropological scholarship by making investigations and uttering remarks on the distribution of these customs, although the distribution of those very remarks was limited by publishers' unwillingness to include

them in his published travel books (the publishers omitted an appendix from *First Footsteps in East Africa* and omitted a footnote in some editions of *A Secret Pilgrimage to Mecca and Medina*, e.g., the posthumous 1898 edition that was edited by his widow, Lady Isabel Burton). He argued that the incidence of genital cutting and elongation reflected an evolutionary hierarchy (he was an evolutionist in a loose sense): the higher races did not need to practice barbaric rites whose purpose was to control an excessive sexuality typical of less civilized peoples. Australians, Africans, Arabs, and Jews performed such operations, but Caucasians did not do so. When Burton discovered that Dahomean women elongated their labia, he saw the practice as an attempt at sexual control: "The sole possible advantage to be derived from this strange practice is the prevention of rape, but the men are said to enjoy handling the long projections, whose livid slatey hue suggests the idea of the turkey-cock's caruncle" (Burton 1863–64:319). In other words, an argument that might otherwise seem to indicate that Africans, Jews, and Muslims were sexually controlled was turned against them: "The moral effect of clitoridectomy is peculiar. While it diminishes the heat of passion it increases licentiousness, and breeds a debauchery of mind far worse than bodily unchastity, because accompanied by a peculiar cold cruelty and a taste for artificial stimulants to 'luxury.' It is the sexlessness of the spayed canine imitated by the suggestive brain of humanity" (Burton 1885–87, 5:279, n.5; see H. Lyons 1981:507).

The yawning gap which separates Burton's anthropology from our own is nowhere more evident than in these remarks, which are more pornographic (if the word has any meaning) than any behavior Burton describes. Thirty-five years ago Willard Willis (1972), in his contribution to Dell Hymes's *Reinventing Anthropology*, talked of anthropology's need to exhume and purify the skeletons in its closets. Many histories of anthropology do contain abbreviated accounts of the debate between the monogenists and the polygenists, but racialized sexual hierarchies and all that they imply are still partially closeted. We talk much of reflexivity and of the exposure of the relationship between knowledge and power, but we still need to implement our nostrums in our textbooks. If I may be excused a naïve indulgence in the Manichean language that has typified political discourse since September 11, 2001 (George W. Bush is not the sole offender), anthropologists—nice, progressive people that we are—find it troublesome both to acknowledge and to exorcize evil ancestors. The presence of sex (until recently rarely regarded as a "serious" topic) in this ugly picture is an added stimulus to repression in some forms of talk and writing.

In *Irregular Connections* (Lyons and Lyons 2004) Harriet Lyons and I employed the term *conscription* (rather than "appropriation") to describe the deployment of data about sexual discourses and practices among "others" within discourses of power, morality, pleasure, and therapy in the metropolitan cultures where anthropological texts have predominantly been read and produced. Conscription may be *positive* or *negative*: it may imply the reaffirmation of existing social hierarchies, or it may involve what Marcus and Fischer (1986) call "cultural critique." The two positions, of course, need not be mutually exclusive—critiques of some social practices may reinforce others. Conscription is a live metaphor. It implies force and inequality and, more often than not, the absence of true dialogue. Many of Burton's observations about "primitive" sexuality clearly fit our description of negative conscription: narratives of primitive sexual behavior supposedly show us how biologically different "they" are from "us," how lucky or righteous we are that we have evolved morally and they haven't, or indeed how their "degeneracy" is clear evidence of what will happen if we allow our own social misfits to survive or take control of our destinies.

If Burton had said nothing more about sexuality in non-Western societies, he would be a far less interesting and less puzzling figure. His remarks on polygamy throughout his career and on homosexuality during the last part of it are not instances of pure *positive conscription* (which means that the sexual practices of "primitives" are viewed as a *natural*, uncorrupted form of behavior from which "we" have wrongfully departed and towards which we should now return). However, they do form part of a *critique* of Victorian sexual morality.

Richard Burton hated "Mrs. Grundy" as much as he hated many Jews and blacks. Furthermore his perpetual wandering and his propensity for cultural cross-dressing and disguise indicate a deep-seated ennui with his own culture. It is essential that one does *not* separate Burton's polygenist prejudices from his critique—his "libertarian" views about sex, his "proto-fascist" (McLynn's term) views about race, and his antidemocratic politics must not be dissociated. Cultural relativism in the Boasian sense (with its ideas of holism and its rejection of psychic unity) can sometimes resemble the polygenist biology it replaced, inasmuch as both assume that one begins one's analysis with *difference*, and that human behavior is always geographically contextualized. Where Burton's biases lead him to suspend his usual cultural ranking, he makes statements that seem to foreshadow relativism, although, unlike Dane Kennedy, I would argue that they do not constitute "a relativist stance" (Kennedy 2005:165) or a "venture into the realm of relativism" (202).

Positionality is nonetheless important: Burton was able to observe some things and say some things about polygamy and homosexuality which distinguished him from all but a minority of his contemporaries, and they connect with later anthropological thought.

Burton's public position on homosexuality was sufficiently subtle or superficially ambiguous that his "true" opinions and actions have been a constant source of speculation. According to Frank McLynn (1990), Burton was an individual who failed at heterosexual love, and failed to satisfy his concubine in India, and furthermore had strong homosexual inclinations. When he became fascinated, indeed obsessed by the subject in the last decade of his life his wife, Isabel, appears to have been somewhat distressed and for this reason may have burnt his unpublished and unexpurgated translation of *The Scented Garden* (not to be confused with the earlier, expurgated translation known as *The Perfumed Garden*) after his death. If one reads "with the text," Burton repeatedly condemns homosexuality and regards it as a sickness, albeit a product of nature. That is why Lady Isabel referred to his "unbounded contempt for the Vice and its votaries" (I. Burton 1893). If one reads "against the text," Burton took pains to defend the performance of sexual acts by himself and others which were perfectly defensible provided their frequency did not diminish the birth rate!

> But I repeat . . . there is another element in The Nights and that is one of absolute obscenity utterly repugnant to English readers, even the least prudish. It is chiefly connected with what our neighbours call le vice contre nature—as if anything can be contrary to nature which includes all things. And they, methinks, do abundant harm who, for shame or disgust, would suppress the very mention of such matters: in order to combat a great and growing evil deadly to the birth-rate—the mainstay of national prosperity—the first requisite is careful study. (Burton 1885–87, 10:204)

In the terminal essay, Burton claimed that the frequency of homosexual acts as well as the tolerance extended toward them varied geographically. The greatest frequency was found in the area between 30 and 40 degrees North latitude. The area covered included the southern and, in pre-Christian times, the northern Mediterranean regions, Egypt, Turkey, the Fertile Crescent, Arabia, Mesopotamia, Persia—where it was treated as a "mere peccadillo"—and parts of the Indian subcontinent. Further east, the zone widened to include all of China and Indochina, the South Seas, and both American continents. The "Sotadic Zone" thus encompassed

a large part of the world's population. Burton claimed (Burton 1885–87, 10:207) that "geographical and climatic, not racial" factors were responsible for the creation of a zone where "there is a blending of masculine and feminine temperaments, a crisis which elsewhere occurs only sporadically" (1885–87, 10:208). Here there was a suggestion that the "unnatural" might indeed be "natural" in certain ecological conditions. There is, of course, no such thing as the Sotadic Zone, but it is a persuasive fiction. All the contemporary reader needs to do is enter these two words on any search engine and it will be apparent that Burton created *volens nolens* one of the charter myths for postmodern gay dialogue online. Much of this writing makes no reference to Burton's other less palatable opinions.

Burton must inevitably have known many homosexuals, including his close friend, the poet Swinburne. He corresponded with John Addington Symonds, whose posthumous defense of the rights of homosexuals was to provoke so much controversy a few years after both men died. In the climate of the 1880s when the social purity movement achieved several successes, including the repeal of the Contagious Diseases Act and the Labouchère Amendment to the Criminal Law Amendment Act, Burton's stance must have involved a certain courage.

Although Burton's stance on homosexuality may indeed have contributed to his popularity among some recent gay writers, it was probably one more reason for his obloquy among his successors in the field of anthropology. In truth homophobia is a relatively rare motif in anthropological discourse, although in *Irregular Connections* (2004:156, 194, 254, 274–275) we noticed a slight homophobic tinge to remarks made at various times by Malinowski, Mead, and Goldenweiser, and strong homophobia in a work by Suggs and Marshall that appeared as late as 1971. However, until the 1970s overt advocacy on behalf of sexual minorities is rare (covert critique just within the mainstream [e.g., Edward Westermarck], or overt talk at the discipline's fringes [e.g., Carpenter], is all that occurs.) Is the rarity of discourse and debate about sexuality in general and homosexuality in particular reason enough for the disregard of people like Burton in the historiography of anthropology?

It should be noted that Burton's work on polygamy has also been neglected, that it was perhaps less shocking than his ideas on homosexuality to the sensibilities of his contemporaries, and that, if one ignores its political subtext, it adumbrates the later observations of Mary Kingsley and the much later insights of Malinowski and the functionalists. There is certainly much of value in it even to post-functionalist anthropologists. Polygamy was, in Burton's opinion, very comprehensible in certain

social contexts and no more demeaning to the status of women than many forms of monogamy. Indeed women were honored in many barbarian societies (see below). But what do we mean by the "subtext" in Burton's account?

Harriet Lyons (2005) has recently observed that there are really two traditions in anthropological writing about sex that mirror narrative traditions within the broader culture. There is a masculine narrative concerned with sexual liberation and freedom from societal, religious and feminine restrictions. Sometimes the narrative is brashly heterosexual; sometimes it is homosocial; sometimes it is gay; sometimes it is all of the above. Furthermore, sexual politics makes for some strange bedfellows: political reactionaries may also be sexual libertarians. The feminine narrative may also embrace the idea of sexual freedom, but the pursuit of sexual freedom is tempered by concerns about perduring love and loyalty and immunity from male abuse. If we follow Michael Mason's argument (Mason 1994), "Mrs. Grundy" (based on a character in a 1798 play) was not always the unfeeling puritan of popular portraits; she might well have been politically progressive, perhaps even a religious freethinker, but she did want to stop the exploitation of women by men. In the following extract from *Personal Narrative of a Pilgrimage to Al-Madinah & Meccah* Burton demonstrates an early form of ethnographic pastoralism, conscripting traditional Arabs and Islam in a verbal attack on both a Christian priest who gave "lectures to working men" and Harriet Martineau, the feminist writer who also had traveled in the Middle East. Martineau wasn't a typical target for Burton, inasmuch as she was an atheist and a friend of the Darwin brothers, but she was an abolitionist and she believed that women should have the right to vote. For Burton that surely was enough. Whatever Burton's motives may have been, his observations on polygamy among the Arabs should have given his readers some needed mental unrest.

> The Rev. Charles Robertson, author of a certain "Lecture on Poetry, addressed to Working Men," asserts that Passion became Love under the influence of Christianity, and that the idea of a Virgin Mother spread over the sex a sanctity unknown to the poetry or to the philosophy of Greece and Rome. Passing over the objections of deified Eros and Immortal Psyche, and of the Virgin Mother—-symbol of moral purity—-being common to every old and material faith I believe that all the noble tribes of savages display the principle. Thus we might expect to find, wherever the fancy, the imagination, and the ideality are strong, some

traces of a sentiment innate in the human organisation. It exists, says Mr. Catlin, amongst the North American Indians, and even the Gallas and the Somal of Africa are not wholly destitute of it. But when the barbarian becomes a semi-barbarian, as are the most polished Orientals, or as were the classical authors of Greece and Rome, then women fall from their proper place in society, become mere articles of luxury, and sink into the lowest moral condition. In the next stage, "civilisation," they rise again to be "highly accomplished," and not a little frivolous.

Miss Martineau, when travelling through Egypt, once visited a harim, and there found, among many things, especially in ignorance of books and of book-making, materials for a heart-broken wail over the degradation of her sex. The learned lady indulges, too, in sundry strong and unsavoury comparisons between the harim and certain haunts of vice in Europe. On the other hand, male travellers generally speak lovingly of the harim. Sonnini, no admirer of Egypt, expatiates on "the generous virtues, the examples of magnanimity and affectionate attachment, the sentiments ardent, yet gentle, forming a delightful unison with personal charms in the harims of the Mamluks."

. . . Europe now knows that the Moslem husband provides separate apartments and a distinct establishment for each of his wives, unless, as sometimes happens, one be an old woman and the other a child. And, confessing that envy, hatred, and malice often flourish in polygamy, the Moslem asks, Is monogamy open to no objections? As far as my limited observations go, polyandry is the only state of society in which jealousy and quarrels about the sex are the exception and not the rule of life.

Were it not evident that the spiritualising of sexuality by sentiment, of propensity by imagination, is universal among the highest orders of mankind,—c'est l'étoffe de la nature que l'imagination a brodée, says Voltaire,—I should attribute the origin of "love" to the influence of the Arabs' poetry and chivalry upon European ideas rather than to mediaeval Christianity. Certain "Fathers of the Church," it must be remembered, did not believe that women have souls. The Moslems never went so far. (Burton 1898, 2:89–92)

Perhaps this is why Edward Said granted Burton some absolution for his sins. Burton continued to defend polygamy in a variety of settings. Many Mormons were pleased by Burton's account of their own peculiar institution in Burton's *City of the Saints*. He contextualized polygamy in terms

of mode of production, ecology, and demography, noting that it was economical in places where the farming economy was simple and there was a shortage of labor rather than land. This was true of Utah, but, were polygamy to be introduced to Paris or London it would, "like slavery, die a natural death" (Burton 1861a:522).

Just two or three years after his visit to Salt Lake City, Burton was to visit the Yoruba city of Abeokuta. The missionaries had got there first. To his horror he discovered that the missionaries were encouraging converts to divorce their second and subsequent wives who were then remarried to bachelors. "This appeared to me the greatest insult to common sense, the exercise of a power to bind and to loose with a witness, to do evil that good may come out of it, a proceeding which may make any marriage a no-marriage" (Burton 1978, 1:214; 1967:134, 135).

What we have called conscription is but one facet of the relationship between Western power and the anthropological knowledge we seek from others. In Burton's time participant observation did not exist. Inasmuch as he slept with an Indian concubine Burton was typical of the officer class of his time. Inasmuch as he adopted disguise he was able to penetrate cultural and religious and sometimes sexual boundaries. This was a breach of the emergent color-bar taboos. After the Mutiny (and well after Burton's departure from India) the barriers became stronger. In *Irregular Connections* (Lyons and Lyons 2004:60) we noted the following passage from Burton's "little autobiography" and we note that Dane Kennedy (2005:44) has also seen its significance:

> [T]he white man lives a life so distinct from the black, that hundreds of the former serve through what they call their "term of exile" without once being present at a circumcision feast, a wedding, or funeral. More especially the present generation, whom the habit and means of taking furloughs, the increased facility for enjoying ladies' society, and, if truth be spoken, a greater regard for appearances, if not a stricter code of morality, estrange from their dusky fellow-subjects every day and day the more. (I. Burton 1893, 1:155)

That ethnographies have been the products of processes tantamount or almost tantamount to espionage should come as no surprise to readers of some of the essays in *Anthropology and the Colonial Encounter* (Asad 1973) and *Writing Culture* (Clifford and Marcus 1986). Only very recently, and paradoxically in an era of rigid ethics guidelines, have some anthropologists begun to contemplate whether or not anthropology (or

anyone) benefits when ethnographers sleep with their subjects (see, for example, Kulick and Willson 1995, Markowitz and Ashkenazi 1999). One does not wish to imply that Burton was a participant observer, but rather that participant observation itself is undergoing a process of scrutiny and reevaluation. The boundary (both in space and in time) between fieldwork and other modes of inquiry and between ethnography and other genres is less clear than we thought (see, for example, Pratt 1986; Malkki 1997).

Burton's ethnographic observations vary in quality because of the varying time he spent on his "field trips" and the racism that so often mars his accounts. Sometimes he does not venture beyond stereotypical descriptions of dress, weaponry, and moral character. On occasion he reveals an astonishing capacity for cross-cultural analysis.

In a discussion of societies of sub-Saharan Africa, which is contained in a review of a book by the explorer Paul du Chaillu, he notes that exogamy, the levirate, and matrilineal descent are widespread (he does not employ the modern terms), whereas cannibalism is not; and also observes a number of common cultural traits: the prevalence of elaborate greetings, ritual abuse of a king about to ascend the throne, the attribution of illness to witchcraft, the general concern of religion with the fending off of death, as well as the belief in animated spirits rather than permanent ghosts (Burton 1861b). In other words, Burton was engaged in an attempt to define sub-Saharan Africa as a "culture area," to use the parlance of twentieth-century anthropology.

At this point we have determined that Burton's writings on homosexuality and polygamy, his advocacy of cross-cultural contact and his analytical powers are reasons to include him in histories of anthropology, and that his racism and sexism are also grounds for inclusion because they are very much part of our discipline's history.

Perhaps we may understand a little more about Burton's case if we look at other cases of inclusion and exclusion in the history of anthropology (and the history of sexuality). Edward Westermarck (1862–1939) seldom occupies much space in the history of our discipline except for the fact that he was Malinowski's teacher at LSE, wrote *A History of Human Marriage*, debunking ideas of primitive promiscuity, and developed a well-known theory of incest. His great work, *The Origin and Development of the Moral Ideas* (1906–1909), is known by philosophers as a treatise in ethical relativism, but few anthropologists are aware that it contains a an essay on homosexuality considered cross-culturally. Few anthropologists read Edward Carpenter (1844–1929), although his *Intermediate Types among Primitive Folk* (1914) contains a fair survey of

what was known about homosexuality cross-culturally at that time. In that book Carpenter isolated two types of homosexuality in traditional societies: love between soldiers among Dorian Greeks, such as the Spartans, and the Japanese samurai; and homosexual roles which stressed artistic expression and/or spirituality, such as those of shamans, diviners, magicians, priests, temple prostitutes, and artists in Siberia, Alaska, the Pueblos, the North American Plains, Polynesia, and biblical Syria and Canaan. In all the above cases, "the intermediate sex" played an important social role. *Intermediate Types* reflects the work of Carpenter's contemporaries such as Westermarck, Symonds, and Ellis, but it is itself an adumbration of current gay sociology and anthropology, particularly the work of Stephen O. Murray and David Greenberg. However, we must acknowledge that *Intermediate Types* conscripts the sexuality of others into European sexual politics, despite advancing an argument with which many contemporary readers are likely to have considerable sympathy.

In the early twentieth century Carpenter lived with his lover on a farm in Derbyshire. He was a socialist and a pacifist who had been a friend of Eleanor Marx, Havelock Ellis, and Ramsay Macdonald. He played no role in the early institutional history of anthropology and I do not think his name is to be found in any history of the discipline. In a book chapter written for a 1929 volume, *Sex in Civilization*, Alexander Goldenweiser ridiculed Carpenter's work: "A quaint fantasy this—homosexual men and women as culture heroes of mankind! And it has a delightfully primitive flare about it. Primitives think this way. Unusual people do unusual things. Those marked by the gods become responsible for great events. The Indian, Australian and African Negro would readily accept Carpenter's theory as a creation myth" (Goldenweiser 1929:62). Until recently that was all the notice Carpenter received from anthropologists.

Verrier Elwin (1902–64) is still well respected by anthropologists in India. One of his books, *The Muria and their Ghotul*, is known to many members of the anthropological profession primarily because it was mentioned in a well-known introductory text of the 1970s and 1980s. An Anglican priest and Oxford don, Elwin left England to enter an Anglican ashram in India. He became a devotee of Gandhi and broke with Anglicanism. Although he eventually had some disagreements with Gandhi, he became a strong supporter of the independence movement and had to resign from the Church. He married two "tribal" women, breaking simultaneously the rules of English class, Indian caste, and the anthropological profession. He "married his fieldwork," as one non-tribal Indian academic remarked. Elwin finished his life as a government anthropologist working with tribals on India's North Frontier. His book on the Muria contains a fervent argument for the regime of sexual freedom supposedly

present in the dormitories in which adolescents of both sexes live before their arranged marriages. Ghotul partnerships, although transitory, are based on sexual attraction and mutual affection: "The Muria believe that sexual congress is a good thing; it does you good; it is healthy and beautiful . . . it is the happiest and best thing in life" (Elwin 1991:419). Elwin's account failed to address a degree of sexism that may always have been present in Muria society, inasmuch as girls, while they might be satisfied with their ghotul partners, were often anything but happy with their assigned spouses. Furthermore the sexual regime of the ghotuls was variable, as Simeran Gell (1992) has noted, and in many cases sexual intercourse did not occur unless the partners became closely attached. In other words, Elwin's account of the ghotul is subject to many qualifications, it is largely bereft of theory and it is unbearably long. However, could Elwin's exclusion from histories of anthropology say more about his lifestyle, his eccentricity, and his location than about the significance of his work? After all, he described and identified an institution that is in some but not all respects unique.

Nobody doubts the place of Margaret Mead and Ruth Benedict in the history of anthropology. We have said quite a bit about them in *Irregular Connections*. Mead's book on Samoa appeared at one of the few times before the late 1970s when sexuality occupied anthropology's center stage. One still wonders what would have happened to both women had their bisexuality been known to a wider group of people. Presumably, had they been candid, they would have had to move out of academia or out of New York City. Benedict's much quoted remarks on berdaches and the like occupy just a couple of pages of her body of writing (Benedict 1959:267, 268), and she does not discuss lesbianism directly. Mead avoided the issue until the last decade of her life, except for her remarks in *Coming of Age in Samoa* that tolerance of adolescent homosexual experimentation as well as the freedom to explore a range of sexual techniques within heterosexual marriage reduce the incidence of homosexual behaviors among adults (Mead 1928:148, 149; Lyons and Lyons 2004:194).

We may now look at the overdetermined answer to our question about the inclusion and exclusion of certain people, primarily Sir Richard Burton, from the history of the discipline. We are looking at the location of writers and academic positions; the institutional development of anthropology; and the kind of writing, the kind of topic, and the kind of lifestyle that is seen as appropriate for professional anthropologists. We are also asking questions about the relationship between the development of anthropology and broader social forces in Britain and the United States.

Burton actually played a small role in the institutionalization of the discipline, but it was as a leader of an ultimately unsuccessful secession. Despite his fame and his many contacts, he spent much of his time out of Britain. There were no academic positions in anthropology until the 1880s, but that would have made no difference to Burton, who left Oxford because of his unruly behavior and, despite his extensive learning, never completed a degree. By way of contrast E. B. Tylor, who eventually became Professor in his own right, was substantially self-educated and was raised as a Quaker, but his lifestyle and his ideas about psychic unity and evolution were respectable and not overly controversial. We may note that Elwin left his country and lived in isolated parts of India for much of his life, and that in his later years Carpenter chose relative isolation in the Derbyshire countryside.

Burton's embrace of polygenesis (fuzzy though his racism was) makes him an unattractive figure today. His views on homosexuality and female circumcision would still annoy a lot of people, if they were ever to become aware of what he said. No direct line leads from him to any body of disciples. Those who use some of his ideas today (most of whom are not anthropologists) have little understanding of the extent of his work or the social context that determined it.

Leading figures in the history of anthropology (Stocking, Kuklick, and Darnell) have tended to concentrate on the history of professional bodies in major urban centers. Most of the acknowledged leaders of the discipline found a position in a central place. They were primarily middle class in origin, as Kuklick (1991) noted in her excellent book about British anthropology, *The Savage Within*, and they were anxious that their discipline would appear to be both scientific and respectable. It had to say responsible things about safe topics. This anxiety is still with us today. The corollary of all this is that those who were not respectable, defied class boundaries, and did not live in central places might find themselves excluded from the discipline's institutional structures and eventually omitted from its history.

The conventional history of anthropology thus deals with people who are safely part of anthropology's social structure rather than its liminars—its shamans, magicians, and witches. Those conventional scholars form part of a history of anthropology as a culture all by itself—after its Victorian infancy it is assumed to have functioned independently of external influences. Furthermore, despite Stocking's warnings about presentism, we still tend to have a Whig approach to our own history—people and ideas who might make us question our belief in the progressive movement of anthropology are cast aside. Burton's writing on race is

disturbing because it challenges the narrative that leads from Tylor to Frazer, and so on. It is also disturbing because it makes us realize that some of our ancestors were quite nasty people in many ways. Furthermore, "race" (and sometimes the denial of race) and sexuality are concepts that through their very content (they may seem to invoke both biology and politics) challenge the idea that one can study anthropology as a culture all by itself.

Irregular Connections was based on the premise that we cannot and should not write the history of social anthropology without searching for relationships between what anthropologists say and currents of opinion in the broader society. We named those relationships "conscription." A genealogy based on such a conception of our history would include some names and some topics that the conventional history of anthropological "-isms" has omitted or neglected. It would allow for rule breakers and ruptures, for people who ended up on the losing rather than the winning side, and for those who chose to stay on the margins. It might include the missing narrative of missing ancestors like Richard Burton.

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9. Anténor Firmin, Nineteenth-Century Pioneeering Anthropologist

His Influence on Anthropology in North America and the Caribbean

Carolyn Fluehr-Lobban

Anténor Firmin was a pioneering anthropologist in the nineteenth century whose major work, *De l'égalité des races humaines (Anthropologie positive)* was published in Paris in 1885 and was largely ignored or dismissed as a foundational text in anthropology (Fluehr-Lobban 2000). The text only recently has been recovered, translated, and introduced into English as *The Equality of the Human Races (Positivist Anthropology)* (2000), 115 years after its original publication. Thus, it is being evaluated as an anthropology text for the first time after 2000.

Firmin was one of two Haitian members of the Paris Anthropology Society from 1884–88, during his years in France as a Haitian emissary, although apparently his name remained on the roster until years after his death in 1911 (personal communication Ghislaine Geloin).[1] Although a member of the Société who attended many of its meetings his voice was effectively silenced by the racialist physical anthropology dominant at the time, and by his race. In the *Memoires* that provide a transcript of the Société's deliberations, apparently Firmin rose to speak only twice, and on both occasions he was silenced by racialist or racist comments. At one point he rose to challenge the biological determination of race that pervaded the prevailing physical anthropology of Broca and others when he was confronted by Clemence Royer (a pioneering woman of science who translated Darwin's *Origin of Species* into French), who asked Firmin if his intellectual ability and presence in the Société were not the result of some white ancestry he might possess. Firmin tells us in his own words in the preface to *The Equality of the Human Races* that he wanted to debate those who "divide the human species into superior and inferior races" but he feared his request would be rejected. "Common sense told me that I was right to hesitate. It was then that I conceived the idea of writing this book" (2000:liv). We now know that a signed copy conveying "Hommage respective à La Société d'anthropologie de Paris, A Firmin" was

presented to the Paris Anthropological Society in 1885, and that no review or further mention of the book, beyond it having been received, was made in the *Memoires d'anthropologie*, the periodical of the Society.[2]

The publication date of 1885 of *De l'égalité des races humaines (Anthropologie positive)* marks it as a pioneering text in anthropology well within the time framework of the other foundational texts in the field such as L. H. Morgan's *Ancient Society* (1877) and E. B. Tylor's *Anthropology* (1881). Although Franz Boas began his "geographical" writings about Cumberland Sound and Baffin-land in 1884–85, he did not produce a synthetic work of anthropology until his 1911 *The Mind of Primitive Man*, that being the year of Firmin's death. P. Topinard, one of the French racialist physical anthropologists to whom Firmin devotes a great deal of his criticism, published in 1885 his *Éléments d'anthropologie générale*, the same year as *De l'égalité des races humaines*. In this work Topinard outlines the general principles of French physical anthropology—racialist, polygenist, grounded in biologically fixed notions of race proved by the science of anthropometry, brought to technical perfection by the French physicians-anthropologists. The difference between Firmin's perspective on anthropology as a new discipline and race as a scientific category and these other pioneers of anthropology are dramatic and significant.

Firmin, Anthropologist and Scientific Positivist

The Equality of the Human Races (Positivist Anthropology) in twenty chapters and 451 pages (662 in the original French), embraces topics in what became the four-field study of humanity including physical anthropology, archaeology, linguistics, and ethnology. It is clear from chapter 1, devoted to "Anthropology as a Discipline," that Firmin's vision of anthropology is one of a comprehensive study of humanity with such potential breadth that all other sciences become as "tributaries to it" (2000:3). Firmin reviewed and assessed the philosophical and scientific tradition that had shaped the nascent science of anthropology from Immanuel Kant to Herbert Spencer. He defined anthropology as "the study of Man in his physical, intellectual, and moral dimensions as he is found in any of the different races which constitute the human species" (2000:10). A broad integrated science of anthropology was envisioned in which he distinguished between *ethnography*—the description of peoples—and *ethnology*—the systematic study of these same peoples from the perspective of race. The anthropologist comes in once the ethnographer and ethnologist have completed their work. Anthropology is com-

parative, separating mankind from the other animals by addressing the questions "What is the true nature of Man? To what extent and under what conditions does he develop his potential? Are all of the human races capable of rising to the same intellectual and moral level?" Anthropology requires the effort of the best minds—"It goes without saying that if they are to come up with valid results, anthropologists must do more than establishing some arbitrary ranking of the human races and their respective aptitudes" (2000:12–13). Anthropology has been the discipline which can best provide answers to the great problem of the origin and nature of Man and the question of his place in nature (2000:2).

Although Marvin Harris (1968:464) credits Émile Durkheim with founding a school of anthropology that led French racialist and biologically reductionist social science to a modern "sociology" of man, it may well be that the credit belongs, in part, to Anténor Firmin. Firmin grounded his scientific study of humanity and his anti-biological argument for the equality of humans in Comtean positivism. His book is subtitled "Positivist Anthropology" and Firmin pays a deep bow to Auguste Comte as he argues that the case for racial equality is to be made using scientific facts, not a priori assumptions about racial difference or philosophical assertions of racial difference.

Auguste Comte is identified as a founder or the founder of sociology and wrote in the 1830s and 1840s inspiring a number of social scientists, including anthropologists (besides Firmin) from John Stuart Mill to Lévy-Bruhl, Marcel Mauss, and Émile Durkheim. He asserted that mankind is part of the natural world and must be studied as part of it, a perspective that nearly all social scientists in the nineteenth century shared (Adams 1998:171). Comte's use of "fact", "theory", "law"—a clear empiricism—is probably what most attracted Firmin. "Facts" corresponded to what we call "data" today, however "theory" for Comte encompassed models, classifications, generalizations—any framework into which facts might be fitted. He stressed that the comparative method must be applied to a worldwide ethnographic data base (Adams 1998:45). "Positivism," thus understood, is more a methodology than a philosophy, but Comte succeeded in giving this methodology to social science, establishing it as a part of natural science that broke away from the tradition that has previously been associated with moral philosophy (Adams 1998:341–342). It was this empirical methodology and the placing of anthropology as a new social science in the natural sciences that was useful for Firmin.

Durkheim's study of mankind was to become more associated with sociology due to a conservative French science education that continued

to view "anthropology" as physical anthropology only, relegating it to a narrow, biological treatment of humans. Although Durkheim wrote a great deal about ethnography and his landmark publications, such as *The Rules of the Sociological Method*, *The Division of Labor in Society*, *The Elementary Forms of the Religious Life*, contributed to anthropological theory and formed the basis for social anthropology and British structural-functionalism, positivism was divorced from the anthropological traditions. Talcott Parsons wrote that "Durkheim is the spiritual heir of Comte and all of the principal elements of his earlier thought are to be found foreshadowed in Comte's writings" (1949:307).

Firmin places Comte in a line of great scientific codifiers from Aristotle to Bacon, Bentham, Ampere, and Herbert Spencer. He traces anthropological interest among philosophers from Kant to Hegel, Buffon and Cuvier, Linnaeus to the French Geoffroy Saint-Hilaire and Bory de Saint-Vincent. Following Comte and the positivist school, Firmin encouraged the thorough study of humanity from the cosmological, biological, sociological, and philosophical perspectives (2000:11). Throughout his tome Firmin practices positivist science, examining tables of comparative craniometric data noting their irregularities and the means by which Broca and Morton and other racialist scientists manipulated the numbers: by cubing skull measurements, these physical anthropologists were able to draw their intended conclusions. After analyzing various scholars' data on the cephalic index, measuring degrees of superior dolichocephaly and inferior brachycephaly he concludes that this index provides anthropologists with insufficient grounds for dividing the races into distinct groups (2000:100). Broca's facial index is also subjected to critical scrutiny whereby the most primitive black race may be close numerically to the Parisian! He subjects the nasal index, the vertical index, and the orbital index to similar scrutiny, noting that a rational classification is impossible when the data used to generate it are "not only erroneous and irregular, but also often contradictory" (2000:109). The confusing and often conflicting craniological charts are entertaining to Firmin, who would normally dismiss them were they not taken so seriously by the anthropologists. And forecasting a different composition of the scientific cmmunity in the twentieth century, he comments,

> Can anthropologists continue to record these figures without modifying those so assertive theories they have erected? Their science will face certain discredit when, in the twentieth century, it is subjected to the critique of Black and White, Yellow and Brown scientists who can write as well and handle as expertly

> the instruments manufactured by the Mathieu Company [producers of anthropometric instruments], instruments that bring such eloquent results, even in the hands of scientists who doubt their effectiveness. . . .
>
> Motivated by an insatiable thirst for truth and the obligation to contribute, no matter how modestly, to the scientific rehabilitation of the Black race whose pure and invigorating blood flows in my veins, I take immense pleasure in navigating through these columns of figures arranged with such neatness for the edification of the intellect. (2000:102)

Firmin pursued the study of all of those "anthropological doctrines which have assumed the mantle of the august name of science while actually usurping its place" (2000:108).

Not only do craniological measures fail the test of positivist science for Firmin but all other racialized anthropometric devices and classifications—such as those dealing with hair and skin pigmentation—are also treated by Firmin as arbitrary and subjective (e.g., the arbitrariness of making a distinction between inferior woolly hair and superior straight hair) and ultimately non-scientific. Firmin is among the first to locate skin color with the substance melanin, constituted of "fine granules under the epidermis" giving the Ethiopian's skin its black hue (2000:118).

All of this "science" amounts to the "Artificial Ranking of the Human Races," the title of chapter 6 of *The Equality of the Human Races*. Indeed, Firmin devotes almost half of this work to a critical analysis of racialist anthropometry and racist classifications lending support to the doctrine of the inequality of human races. Rather than using the terms "racist" and "racialist" that I have employed elsewhere (2006), Firmin's text notes the "bizarre," the "curious," the "illogical" notion of the inequality of the races.

The Comtean-Durkheimian tradition diverged from the path of American anthropological historical particularism led by Franz Boas. And both French and American mainstream traditions diverged from Marx, Morgan, and Spencerian dialectics and evolutionism. For Comte positivism was supposed to steer a course between the materialism of Hegel and Marx and the Revolution and the idealism of the counter-revolution more associated with the French (Harris 1968:473).

Over five decades before Boas made a similar observation in *Race, Language and Culture* (1940), Firmin devoted considerable effort in separating language from race, noting that it is an unreliable basis for the classification of race (2000:120–135).

Anténor Firmin (1850–1911), Inspiration to Caribbean Anthropologists, Especially Jean Price-Mars (1876–1969)

Anténor Firmin is either a marginalized or a forgotten founder of anthropology whose contributions might have been made in French anthropology, but were ignored due to their critical treatment of the race concept in anthropology. It appears that the members of the Paris Anthropological Society never considered, nor did they ever review, *De l'égalité des races humaines*. Firmin's tome was lost to Francophone anthropology, but was remembered in Haiti and outside Haiti among Pan-Africanist and vindicationist scholars of color. His distinctive and pioneering contributions to an empirical and critical anthropology regarding race and other topics, were lost to the development of mainstream European and North American anthropology in the formative years of the nineteenth century and thus to the decades of consolidation and growth of the discipline in the twentieth century.

Firmin was primarily known in Haiti as a politician and diplomat, less so as a scholar—much less one who was a pioneer in the young science of anthropology. I learned about Firmin in 1988 when a Haitian student of mine, Jacques R. Georges, extolled *De l'égalité des races humaines* as I was lecturing about de Gobineau's *Essai sur l'inégalité des races*.

Firmin's example was inspirational to Jean Price-Mars, the founder of ethnology and folklore studies in Haiti. According to Magdaline Shannon, Price-Mars "had early in life developed goals based upon the ideas of such leaders as Anténor Firmin and Hannibal Price" (1996:163). Admiring the accomplishments of U.S. Negroes, some of Price-Mars's earliest public lectures focused on the equality of human races, sounding so much like Firmin that President Nord Alexis accused Price-Mars of being a "Firminist" and labeled his talk "seditious" (Shannon 1996:21). This was around 1906 while Firmin still was actively opposing the Alexis government from exile in St. Thomas.

The influence of Firmin on Price-Mars is acknowledged in many of his writings and especially in the last work of Price-Mars's long career, *Joseph Anténor Firmin* (1964). Both were scholar-politicians in the Haitian way of not dividing the world of ideas from the world of politics. They were separated in age and time by a generation: Firmin was born in 1850 and died at sixty-one years of age in 1911, whereas Price-Mars was born in 1875 and died at the age of ninety-one in 1964. Price-Mars taught at the Lycée Pétion where Firmin had been a student. As intellectuals and politicians, both were appointed ministers to Paris—Firmin in 1884, Price-Mars in 1915—as the usual means of removing troublesome

individuals. As scholars both were committed to the proof of the equality of races, Firmin challenging racist French physical anthropology which he encountered during his years in Paris and represented by Paul Broca, and Price-Mars responding to Gustave Le Bon, who formulated a collective and racist social psychology emphasizing the inferiority of the black man. As a medical student in Paris Price-Mars read Le Bon, whom he later met and challenged; the latter replied by asking him why if he believed so strongly in the equality of races he did not write about his country (Shannon 1996:233). This became his vocation; he became a student of the Haitian peasantry as he traveled about the countryside ministering to them as a physician. It was in this manner that Price-Mars observed voodoo and was impressed with it as a religion syncretic between African animism and French Catholicism.

Négritude and Pan-Africanism

Firmin was recognized more as a pioneer of négritude and early Pan-Africanism than as an anthropologist. Coulthard notes, "Writers like Anténor Firmin, Hannibal Price, Claude McKay, George Padmore, and Jean Price-Mars were in the vanguard of the revaluation of African culture long before the nationalist awakening in Africa and before the concept of négritude was developed in the Caribbean" (1962:117).

Although Jean Price-Mars is usually credited with being the founder of "noirism," and later Léopold Senghor hailed him as the "Father of Négritude" (Fouchard 1990), it is probable that Firmin and other illustrious members of Haiti's nineteenth-century intellectual elite laid the primary foundation for what was to become the négritude movement. At least four of the twenty chapters of *The Equality of the Human Races* speak directly to the primary role played by the black race in world history and civilization, including "Egypt and Civilization," "Intellectual Evolution of the Black Race in Haiti," "Evolutionary Pace of the Black Race," and "The Role of the Black Race in the History of Civilization." A cursory reading not only of these chapters but of the entire tome reveals Firmin to be "noirist" without arrogance or apology. Firmin attended the First Pan-African Congress in London in 1900 which W. E. B. DuBois also attended. Had he not been preoccupied with Haitian politics and a bid to become president as head of a Firminist movement, ending in his exile in St. Thomas by President Alexis Nord, Firmin might have continued this international involvement with the nascent Pan-Africanist movement. Kwame Nkrumah acknowledged Firmin as a New World pioneer of Pan-Africanism at a speech at the University of Ghana in September 1964: "And let us not forget the important contributions of others in

the New World, for example, the sons of Africa in Haiti such as Anténor Firmin and Dr. Jean Price-Mars, and others in the United States such as Alexander Crummell, Carter G. Woodson, and our own Dr. DuBois."

Lyle Shannon, the husband of Magdaline Shannon, the translator of Jean Price-Mars's classic work *Ainsi parla l'oncle*,[3] wrote to me upon learning of the translation of *De l'égalité des races humaines*: "Firmin was one of Haiti's most distinguished patriots. His ideas led to the concept of négritude. Although people did not at the time think of it that way and despite the appreciation of Jean Price-Mars who predicated his own work on that of Firmin, he remained relatively obscure except in Haiti" (October 20, 2000, pers. comm.).

Although many white and black intellectuals see Price-Mars as the greatest of Haitian intellectuals, and attribute to him the title of "Father of Négritude," he never acknowledged this claim although he lived well into the twentieth century and witnessed both the end of European colonialism in Africa and the postcolonial rise of ideological and political négritude. Others can claim this title; in 1934 Aimé Césaire of Martinique, French Guyanese Léon Damas, and Senegalese Léopold Senghor launched the L'Étudiant Noir, a movement of universal consciousness that Césaire called "negritude." Despite this self-assessment, Price-Mars was unanimously chosen as president of the First Congress of Black Writers and Artists held in Paris in 1956, and Presidents Ahmed Touré of Guinea and Léopold Senghor at Conakry proclaimed him, at ninety years of age, "the incomparable Master." It may have been from Price-Mars that Kwame Nkrumah learned of Anténor Firmin.

Although Price-Mars grounded the study of Haitian ethnology, religion, and folklore within the African continent, he nonetheless saw Haitians as a part of humanity without the need of "noirism." This view parallels that of Firmin, whose positivist assertion that the races are biologically equal was matched by a moral imperative that in mind and spirit as well humanity is unitary, drawing upon common heritage. He writes in his conclusion to *The Equality of the Human Races*,

> Returning to the truth they will realize that human beings everywhere are endowed with the same qualities and defects without distinction based on color or anatomical shape. The races are equal; they are all capable of rising to the most noble virtues, of reaching the highest intellectual development; they are equally capable of falling into a state of total degeneration. Throughout all of the struggles that have afflicted, and still afflict, the

> existence of the entire species one mysterious fact signals itself to our attention. It is the fact that an invisible chain links all of the members of humanity in a common circle. It seems that in order to prosper and grow human beings must take an interest in one another's progress and happiness and cultivate those altruistic sentiments which are the greatest achievement of the human heart and mind. (2000:450)

Both Anténor Firmin and Jean Price-Mars are giants of Haitian anthropological scholarship and thought—Firmin in the late nineteenth century and Price-Mars, for well over half of the twentieth century. Firmin being the intellectual antecedent of Price-Mars, Price-Mars pays him the greatest respect and bows deeply to Firmin as scholar, diplomat, patriot, and politician. No where is this more obvious than the biography of Firmin that is Price-Mars's last work, *Anténor Firmin* being published posthumously in 1964. In this 423–page work of thirty-eight chapters, Price-Mars surveys and comments upon Firmin's political and diplomatic career in Haiti and in France, and his successful staving off of the cession of the Môle St. Nicolas to the U.S. while Frederick Douglass was U.S. ambassador to Haiti.[4]

He also devotes a chapter to Firmin as *homme de science*, in which he extols the importance and value of the contributions made in *De l'égalité des races humaines*. Referring to Firmin as a prodigy, he marvels at the remarkable achievement of writing a book of the scope of *De l'égalité des races humaines* in only eighteen months, during his first brief years in Paris (1883–88)—noting that this is the sort of work that it would take others years of research and reflection to accomplish. He notes that Firmin, as one of two Haitian members of the Paris Anthropology Society with Louis Joseph Janvier, he was subject to the polygenist ideas of French anthropology that pronounced him inferior. Knowing this to be a false science, he nonetheless embraced the potential of anthropology and this is how he came "to his vocation as an anthropologist" (Price-Mars 1964:148). Price-Mars remarks especially on Firmin's critique of anthropometry and craniometry, noting that had the world heeded his ideas the tragedy of "Hitlerism" or Nazism might have been avoided. He points to Firmin's recognition of Egypt as an African civilization, including Nubia and often referencing Meroë ("Ethiopia") in his praise of ancient Africa. His arguments would have provoked a "scandal" in Europe, so much did they represent a break with the prevalent ideas of his time. He suggests a close linkage with the ideas of Cheikh Anta Diop in *Nations nègres et culture* and acknowledges the work of Basil Davidson *Africa before the*

Europeans, both popular in the 1960s when his *homage* to Firmin was published. Acknowledging both *De l'égalité des races humaines* and Firmin as "pioneering" among Haitian scholars, Price-Mars also mentions others in this constellation of scholarly greats, including Beauvais Lespinasse (*Histoire des affranchis de Saint-Domingue*), and Hannibal Price (*De la réhabilitation de la race noire par la République d'Haiti* [1900]), from whom Price-Mars took his name.

On Race

Comparing the thought of Firmin and Price-Mars, they can be considered to be parallel and complementary. Both are dedicated egalitarians on the matter of race; both are committed "vindicationists" and "rehabilitationists" recognizing that the ideology of the inferiority of the black race had scarcely changed from the nineteenth to the twentieth centuries, or for that matter from its origins in the dark ages of the eighteenth century "Enlightenment." It may be this vindicationist focus that isolated Firmin and Price-Mars from mainstream Euro-American anthropology. The fact was that few black voices on the subject of race were to be heard in any field, including anthropology, which was one of the stated reasons for Firmin's book. Responding to the assertion by French scholar de Quatrefages that black people sweat less than white people, he speaks with authority on the subject:

> I am Black and nothing distinguishes me anatomically from the purest Sudanese. However, I transpire abundantly enough to have some idea of the facts. My congeners are not beyond the laws of nature. I shall not bother to discuss the issue of the putative sui generis odor that is supposedly a particular characteristic of the Negro race. The idea is more comical than scientific. (2000:61–63)

Price-Mars regarded the race concept as a myth, like his intellectual predecessors Firmin and Hannibal Price. And, like Firmin, he recognized the fundamental *métissage* (mixed-race heritage) not only of New World populations, but of humanity itself. In the context of Price-Mars's appreciation of his and Haiti's African ancestry, he takes the opportunity to comment upon this diverse heritage:

> Well, our only chance to be ourselves is by not repudiating any part of our ancestral heritage. And, as for this heritage, eight-tenths of it is a gift from Africa. Moreover, on this small planet which is but an infinitesimal point in space, men have intermin-

> gled for millennia to the point that there is no longer a single authentic savant, not even in the United States of America, who seriously supports the theory of pure races. And if I accept the scientific position of Sir Harry Johnston there is not a single Negro, as black as he may be, in the center of Africa who does not have some Caucasoid blood in his veins, and perhaps not a single white in the United Kingdom of England, France, Spain, and elsewhere among the most haughty, who has not some drops of Negro or yellow blood in their veins. So, it is true according to the verse of the poet: All men are man [Victor Hugo]. (1967:6)

Price-Mars's last line repeats the last words of Anténor Firmin in *The Equality of the Human Races*, in which Firmin quotes Victor Hugo: "All men are man," completing his thought with the "divine instruction" to "love one another" (Firmin 2000:451). This intergenerational message—from Firmin and Price-Mars—of a core of diverse humanity from which common heritage, respect, and love can flow speaks volumes about the humanism which is at the base of both Firmin's and Price-Mars's anthropology.

On Africa and Africans

The knowledge of the two scholars of Africa and Africans can be contrasted. Firmin was largely ignorant of the contemporary African continent of his day, recalling that much of the African interior had yet to be fully explored and mapped at the time of the writing of *De l'égalité des races humaines*. The Berlin Congress, dividing the continent among the major European powers, had occurred the year before, in 1884. Myth, inferiorized peoples, and tales of monstrosity characterized European views of Africa. Lacking accurate knowledge of the present and adhering to the dictates of science, Firmin declared, "I want to limit myself to generally known fields where serious discussions can be conducted with evidence and verification" (2000:401). Thus, he turned to Africa's glorious past in support of his anti-racist theory. He devoted much attention to the ancient Nile Valley, recognizing the achievement of Nubia (Ethiopia) as well as the better-known Egypt, understanding well ahead of his time the rivalry between the two separate, yet fraternal, civilizations.

By contrast, more than four decades later, Price-Mars used Haitian folklore as a key to Haiti's African past and present links. Price-Mars referenced the Africa of "the Congo, the Sudan, Dahomey," while Firmin conjured the ancient Nile, Egypt, and Ethiopia—from "Memphis to Meroë"—in support of Africa's contribution to the civilization of the

black race. Price-Mars rejected the "Dark Continent" characterization, relying upon works of Joseph Deniker, W. E. B. DuBois and Maurice Delafosse. He found the heart of Haitian culture through its folklore and religion in *Ainsi parla l'oncle* (1928), a classic work of anthropology and ethnology that demythologizes voodoo and establishes its study as a legitimate branch of comparative religion that can only be understood by looking to French Catholicism and West African animistic beliefs and practice.

At the "Address Given at Primavera" eight years earlier in 1922 he discussed peasant life, showing "survivals from the land of Africa" (Price-Mars 1990)—just as the geologists' studies do, we can uncover vestiges, survivals of ancient customs, beliefs, mores, and a process of recovery of African civilization sifted through the French sieve. Moreover, he told the story in Creole using the words and sentiments of Haitian masses. Both Price-Mars and Firmin emphasized the importance of Creole as transitional to French in Haitian education, and both saw in the American Tuskegee approach of self-determination an example for Haitian development.

Like Firmin, Price-Mars accepted the strong influence of environment upon African cultural development. As Jacques Antoine noted "the Blacks of the U.S. and Africa are no longer his brothers, but himself, this is the transformation that occurs in *Ainsi Parla l'Oncle*" (1981:140; Shannon 1996). Price-Mars used a shared African heritage to try to fuse a national consciousness, although he understood very well that many Haitian intellectuals critiqued *Ainsi* as too African, or as Haitian anthropologist Rémy Bastien thought, the book idealized too much the peasant. Price-Mars was as conscious that he was entering racially contested ground by evoking the African present in *Ainsi* as Firmin was in his assertion that ancient Egypt is "wholly African" in *De l'égalité des races humaines*:

> Shall we go back together to those times to compare the establishment of a family somewhere in the Congo, in the Sudan, and in Dahomey? Ah! I understand full well the repugnance with which I am confronted in daring to speak of Africa and African things. The subject seems vulgar to you and entirely devoid of interest, am I not right?
>
> Beware, my friends, are not such sentiments resting on the grounds of scandalous ignorance? We subsist on ideas . . . in which we glorify "the Gauls, our ancestors. (Shannon 1996:204)

In doing so, both Haitian scholars—*Ainsi parla l'oncle* in 1928, like *De l'égalité des races humaines* in 1885—legitimated the study of Africa

on its own terms and exerted a major effort to rehabilitate Haitian culture for itself and for the world by valorizing not only Haitian intellectuals and folklore but the past and present grandeur of Africa. Price-Mars was less skeptical than Firmin was in his day that contemporary African culture was as great a source of pride in Africa as its glorious past.

Firmin's Indirect Ties to American Anthropology via Price-Mars to Melville Herskovits

An intellectual relational line can be drawn from Anténor Firmin to Jean Price-Mars, and from Jean Price-Mars to Melville Herskovits, and, thus, indirectly to Franz Boas and the mainstream of American anthropology. There may be comparable connections between Haitian ethnology and French anthropology, however, these have yet to be lifted from the submerged history of science and intellectual life among the enslaved and colonized peoples and nations.

The admiration of Firmin by Jean Price-Mars is clear. That he is the most obvious twentieth-century intellectual descendant of Firmin's anthropology and the founder of ethnological studies in Haiti is also clear. However, Firmin's contribution to American anthropology came indirectly through his profound influence upon Jean Price-Mars, the dean of twentieth-century Haitian ethnology who was closely associated with Melville Herskovits, the student of Franz Boas and the founder of African Studies in America and of Afro-American anthropology.

Correspondence between Melville Herskovits and Jean Price-Mars from 1928 to 1955 reveals a warm and affectionate professional relationship between the elder scholar Price-Mars and the young Herskovits.[5] This began as the American anthropologist developed an interest in Caribbean and South American Negroes, after his original studies of the American Negro, and as he later planned a period of research in Haiti. Price-Mars responded generously to Herskovits's requests and he replied to his questions about Haitian ethnology, even as he encouraged the young American scholar. Eventually, Herskovits did conduct several months of fieldwork in Haiti in 1934, with Price-Mars arranging the visit and hosting Herskovits and his wife, selecting the field location of Mirebalais, assisting with housing and introductions, and providing a senior ethnologist's advice, perspective, and scholarly resources. This resulted in the publication of Herskovits's classic *Life in a Haitian Valley* in which he credits Price-Mars and his colleague J. C. Dorsainvil with critical assistance: "Aid and inspiration were derived from many friendly conversations with two outstanding students of Haitian folk-life, Dr. Price-Mars and Dr. J.C. Dorsainvil. My indebtedness to the works they have

published on the subject is shown by the references to their publications given at the end of this volume, for their researches must be accepted as basic by any serious investigator of Haitian customs. Their cordial advice to a fellow-student constitutes one of the most pleasant memories of this fieldwork" (1937:x).

This published acknowledgment of Price-Mars is corroborated in his private correspondence:

> I was able to finish my book on Mirabalais—you will see that I am restricting myself to life in the valley where I studied, and not attempting to write about the whole of Haiti—and at the same time get a much needed rest in the countryside where we were living. The book should appear during the winter and, needless to say, I will see that a copy is sent you. You will find that I have made good use of your work which I found of great value; I shall look forward with interest to your comments on my report."(letter to Jean Price-Mars, September 23, 1936)

> I do want to write and tell you how much I enjoyed the time I spent with you while I was in Haiti and also to convey to you my appreciation of the material aid you gave me in making possible our research in Mirabalais. Just as it would not have been possible without your aid to have got my films through the customs so our work would have been less productive without the help which General Cantave gave me as a result of my having been introduced to him by means of the card from you which I carried." (best wishes on your senatorial and presidential campaigns) (letter to Jean Price-Mars, September 26, 1934)

Price-Mars continued his interest in Herskovits's work, later suggesting that a translation of *Life in a Haitian Valley* be undertaken. In return in 1933 Melville Herskovits nominated Jean Price-Mars for membership in the American Anthropological Association.

In 1920 Price-Mars held an historic conference on "Haitian Folklore" at the Cercle Port-au-Princien, at which he introduced the serious study of voodoo gaining the respect of Haitian and French intellectuals. He drew upon Durkheim enabling him to see voodoo as a syncretic blend of African animism and Roman Catholicism. Price-Mars's opening line in his classic work *Ainsi parla l'oncle*—"What is Folk Lore?"—is an eminently anthropological question to which Boas and many of his famous students often turned. To this question he responded, "folk lore is com-

prised of the legends, customs, observances which form the oral traditions of a people. And for the Haitian people, they are the fundamental beliefs upon which have been grafted or superimposed other more recently acquired beliefs" (Price-Mars 1990:13). Uncle Bouqui is the prototypical Haitian story teller. "Cric?" asks the story teller, "Crac," replies the audience (1990:17).

Price-Mars received other American students of anthropology besides Herskovits, including Katherine Dunham (letter to MH, June 29, 1935), whom he helped with her work in Afro-Caribbean dance as a new subject of research in anthropology, and George E. Simpson (described in his introduction to *So Spoke the Uncle*, 1983 edition). He wrote a number of anthropological and ethnological books, including *Ainsi parla l'oncle*, *Formation ethnique, folk-lore et culture du peuple haïtien*, and *Une étape de l'évolution haïtienne*.

Price-Mars enjoyed a long and vital career as a scholar and politician. In addition to his being a founder of the Société d'Histoire et de Géographie d'Haïti, he also ran for President of Haiti in 1940, and in the 1950s he was appointed head of the Haitian delegation to the United Nations. During his long career he achieved some of the international recognition that Firmin's briefer time on this planet did not permit. Louis Mars, Jean Price-Mars's son, became a recognized scholar in his own right as director of the Institute of Ethnology in the 1950s.

Concluding Remarks

As we have assessed the contributions of great figures in American anthropology, such as Franz Boas and Melville Herskovits, a broader net must be cast to include the scholars from outside North America, whom they influenced and who influenced them. It is particularly important for an inclusive anthropology to acknowledge its non-European contributors from the formerly colonized world. A global survey of the founders of anthropology no longer can be confined to males in Europe and America. Two Haitian giants, Anténor Firmin and Jean Price-Mars, deserve admittance to the mainstream of world anthropology; they are worthy of assessment for their signal contributions to ethnology and anthropology. In my view, the factor of race, so far, has been the main hindrance to their recognition.

Firmin died in 1911 at age sixty-one, in exile in St. Thomas, while Price-Mars lived out his nine-plus decades mostly in Haiti, where members of his family still reside. Magdaline Shannon explains in her translation of *Ainsi* that America only became interested in Price-Mars in the

late 1970s, while the academic world outside of Haiti is only now being introduced (or in the case of France, reintroduced) to Anténor Firmin. As Leon Damas noted when Price-Mars was honored by the Académie française "not only as a writer but also as a man, especially for his courage, his mettle, and for that intellectual honesty which he never ceased to display in the interest of the triumph of ideas *not long since regarded as subversive*" (Price-Mars 1990:xxvii, emphasis added). Firmin's ideas were exactly the same sort of subversive writing in the late nineteenth century that kept French intellectuals, and thus the Francophone and wider worlds, from his pioneering, anti-racist thought and early critical and progressive vision of anthropology.

Notes

1. The other Haitian member was Louis-Joseph Janvier, who remained in Paris for twenty-eight years and wrote a number of works, including an essay with a similar title, "L'égalité des races" in 1885.

2. I express my gratitude to Ghislaine Geloin, professor of French at Rhode Island College, for discovering these facts at the Archives of the Société d'Anthropologie de Paris in 2000. Further discussion of her findings can be found in her paper on the reception of Firmin's book in France presented at the June 2001 conference "Anténor Firmin: Pioneer of Anthropology, Pan-Africanism and Post-colonial Studies" held at Rhode Island College.

3. Lyle Shannon has assumed the considerable mantle of responsibility for maintaining contact with scholars interested in the lifelong work in Haitian studies of his wife, Magdaline Shannon, who is no longer able to do so. In addition to her translation of *Ainsi parla l'oncle*, she is the author of *Jean Price-Mars, the Haitian Elite and the American Occupation, 1900–1915*.

4. The encounter between U.S. ambassador Frederick Douglass and Haitian minister of foreign affairs Anténor Firmin as well as the diplomatic affair between the two countries when the U.S. attempted to acquire the Môle St. Nicolas as a naval base has been examined by Richard Martin, who incorporates both Haitian and American perspectives, in "Frederick Douglass, Anténor Firmin and The Môle of St. Nicolas," MA thesis, African and Afro-American Studies, Rhode Island College, 2001.

5. I am grateful to Kevin Yelvington for sending me a copy of this entire correspondence as a resource for the preparation of this article. As a graduate of Northwestern University's Program of African Studies, which Herskovits founded, I feel a personal connection to these Caribbean and American scholars—Anténor Firmin, Jean Price-Mars, and Melville Herskovits.

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10. Salvaging the Delaware Big House Ceremony

The History and Legacy of Frank Speck's Collaboration with the Oklahoma Delaware

Brice Obermeyer

The Delaware Big House Ceremony was an annual religious event of thanksgiving and renewal that involved twelve days of prayer, dance, and song. Families would travel from their homes and camp on the grounds of the Big House Church for the duration of the observance. The church building was a dirt floor, roofed structure made of wood and bark. The image of the Mising, or spirit face, was carved on its interior posts. The last Big House Church to be built in such fashion stood along the western banks of the upper Caney River a few miles west of Copan, Oklahoma.

The last ceremony held in the church west of Copan was in 1924, yet the Delaware did not give up efforts to hold the ceremony until 1945. Work continued after 1924 to revitalize the Delaware Big House Ceremony and three ceremonies held in 1944–45 were the result. In 1944, a structure of bark and canvas was erected on an allotment along Post Oak Creek just north of Dewey, Oklahoma, and celebrants performed an abbreviated ceremony. The ceremonies were organized by Charlie Webber with the help of several men among the Big House followers. The ceremony did not last the entire twelve days and the officiants lacked many of the ritual objects needed to perform the ceremony. While there were no Mising images present to witness the ceremony, the Delaware today remember those wartime Big House Ceremonies as their last.

Documented here is the way in which the Big House adherents sought to keep the Big House Ceremony viable during the ceremony's interregnum between 1924 and 1944. The early decades of the twentieth century brought difficult times for the Delaware. The impacts of land loss, poverty, and waning interest among the younger generation made it clear to the Big House leaders that something had to be done in order to preserve their traditional faith. The Big House faithful soon found a strong ally in Frank Speck and the emerging discipline of American anthropology. While Speck sought to help record Delaware religion, the Delaware hoped to employ ethnography as a means to revitalize their spiritual be-

liefs. This chapter describes such collaboration between Speck and the Oklahoma Delaware with a focus on reconstructing the local social context and emphasizing the Delawares' role in their own ethnographic project. Also discussed is the legacy of Speck's work with the Delaware in the anthropological literature and in contemporary Delaware society.

Frank Speck's Collaborative Approach

Speck's work produced a tremendous body of scholarship that now serves as the foundation for contemporary ethnology on the Eastern Woodlands. Speck did extensive research with the Algonquian and Iroquoian groups resident in or removed from northeastern North America (Blankenship 1991). His efforts were also responsible for enhancing the ethnographic collections of many museums in the northeastern United States (Medoff 1991). Speck's collaboration with Eastern Delaware of Oklahoma produced, among others, two foundational works on the Delaware Big House Ceremony (Speck 1931, 1937).

As a student of Franz Boas, Speck's ethnographic work was salvage oriented. Speck believed that the aboriginal beliefs and practices of the American Indians were in declining usage and searched diligently to locate, record and preserve such indigenous lifeways. He thus wrote in the ethnographic present and sought to reconstruct American Indian lifeways as they would have existed prior to European contact. Such was the approach of ethnographers working with American Indians of the early to mid-twentieth century (Berkhofer 1979:62–69).

It is not my intention to offer a critique of Speck's approach. Sufficient critical attention has been given elsewhere and the limitations of early American anthropology have been well established (Deloria 1969).[1] Instead I will show that despite the limitations of early Americanist scholarship, there were beneficial elements that often go unnoticed. Most notable in Speck's method were the collaborative relationships that he developed with his American Indian consultants (Gleach 2002).

While I see Speck's approach to ethnography as a collaboration, he did not produce what has become known as collaborative ethnography. Recent ethnographers have developed a refreshing new method for interpreting culture (See Lassiter 1998:10–14). Also called dialogical or reciprocal ethnography, a collaborative ethnography is the product of an ongoing process in which both the ethnographer and consultants interpret, revise, and modify a textual account of their lived experiences. The final manuscript is not a definitive account of a particular society but a dialogue between the ethnographer and a particular group of consultants who worked collaboratively on the text's construction.

Such a collaborative ethnography was not Speck's method. By collaborative approach, I am referring to the applied nature of many of Speck's ethnographic projects. Speck took an active role in the empowerment of indigenous peoples, and his work with the Delaware was no exception. Speck's Delaware consultants shared in his belief that rapid acculturation was taking place in Delaware society and such culture loss compelled salvage work. The Delaware consultants with whom Speck worked believed that Delaware traditions were being lost and something needed to be done in order to preserve their way of life. The Delaware recognized the movement of the younger generation away from the traditional Delaware faith and were convinced that such acculturation necessitated a call to action.

However, by characterizing Speck's work as collaborative I seek to encompass more than the mutual respect and goals for cultural persistence shared between Speck and his consultants. I am referring to Speck's work with the Delaware as a collaboration in recognition of the fact that it was the Delaware who initiated the project. In order to save their traditional religion, a Delaware man, Charlie Webber, approached Frank Speck and asked for his assistance. Webber and several other Delaware men wanted to begin their own ethnographic project in order to revive and preserve the Delaware Big House Ceremony. They hoped that ethnography could serve as a potential means for cultural survival. Frank Speck, it seems, was a useful and willing conduit for the Delaware to rely on in their efforts to salvage the Delaware Big House Ceremony.

The Delaware Big House Community

It is important to understand the social setting that motivated the Delaware to seek Speck's assistance. First, there was a clear socio-political division within the Delaware Tribe, and this split disempowered the leadership of the so-called Big House Community.[2] The Big House Community consisted of a network of Delaware families within the Delaware Tribe that lived along the upper Caney River valley. They shared a commitment to traditional Delaware spirituality, ceremonialism, and government. Many within the Big House community also faced an impoverished and desperate financial situation. The allotment of Indian Territory and the oil-boom days of early Oklahoma statehood stripped the Delaware of their lands and encouraged mass non-Indian immigration. Exacerbating an already difficult situation were the constant attacks on the cultural traditions that the Big House community held so dear. Fortunately Big House leaders persevered. Having a strong desire to maintain

their traditional faith, they suffered poverty, diminishing political influence, and zealous Christian evangelists for the sake of what they felt was most important. The Big House community struggled to maintain a sense of Delaware traditionalism that they believed was embodied in the Delaware Big House Ceremony.

The Delaware were one of many northeastern tribes removed to Indian Territory, now Oklahoma.[3] Delaware removal was actually a series of removals finalized in 1867, when the Delaware were obliged to give up their reservation in what is today northeastern Kansas.[4] This last removal created the division between the Big House and the Christian Delaware families. The events surrounding removal not only cleaved Delaware society, it undermined the legitimacy of the traditional form of government preferred by many of the Big House followers while empowering the Christian Delaware families (Haake 2002:424–425).

A minority within the Tribe that supported the removal were led by the Christian Delaware families associated with the local Baptist and Methodist Missions. The Delaware Christian families moved to Indian Territory and settled along the Verdigris and Lower Caney Rivers. There they established new churches as well as a new form of tribal government. A Business Committee was adopted, and operated as the recognized governing body until the Delaware restructured and formed a Tribal Council in 1982.

The majority of the tribe in 1867 were followers of the traditional Delaware faith and resisted removal. Those opposed to removal also wanted to retain their old form of government, based on the Delaware clan system. Some families even sought refuge with other tribes in Indian Territory before reluctantly moving to the area designated for Delaware settlement. Many Big House leaders were hesitant to move and accept the authority of the recently empowered Delaware Business Committee (Haake 2002:425–427). Those families that did not support removal and adhered to the traditional Delaware religion would later become known as the Big House community.

The Big House families eventually acquiesced and by 1871 had moved to the upper Caney River valley (Haake 2002:426). There they rebuilt the Big House Church and may have continued their traditional form of government into the early twentieth century.[5] The Big House community also established social networks with the Osage and Shawnee while remaining somewhat detached from the Christian-dominated Business Committee.[6]

Oklahoma statehood in 1907 and the preceding allotment of Indian Territory had disastrous effects on the Delaware. The region settled by

the Big House Community was an agriculturally productive prairie-plains environment. Thus Delaware lands were very attractive to immigrant farmers and ranchers. The discovery of oil only accelerated the already heightened pace of non-Indian land ownership (Bays 1998:178). In the 1930s, the Great Depression and the severe droughts of the Oklahoma Dust Bowl further encouraged many Delaware to give up their agrarian lifeways in exchange for employment in the larger urban centers. Today, only a few isolated Delaware allotments remain in the rural areas along the Caney and Verdigris River valleys.

The Big House Community also faced a difficult obstacle when trying to pass on their traditional faith to the next generation. Delaware children were either educated in the local public schools or sent to different Indian Boarding Schools. There they faced punishment and ridicule for public displays of Delaware dress, religion, and language. Boarding School students who lived far removed from their homes throughout their childhood years were not able to attend the Big House and other important ceremonies. Many Delaware students also developed relationships at the boarding schools that would take them away from their home community after graduation.[7]

Remarkably, the Big House followers were not even free from harassment in their own church. The Delaware Big House Ceremony was periodically harassed by friends and relatives who urged the abandonment of the traditional religion. Descendants of the Big House followers remember that Christian Delaware preachers would disrupt the Big House Ceremony in an attempt to convert the followers to the Christian faith (Dean 1984:48). A U.S. Marshal, who also was Delaware, was brought in to protect the celebrants while they met for some of the last Big House Ceremonies (Grumet 2001:168).

While so many obstacles confronted the Delaware locally, a few men within the besieged Big House community looked for assistance in the emerging discipline of American anthropology. Previous work with other anthropologists such as Mark Harrington (1913) and Truman Michelson (1912) had introduced the Delaware to the discipline and some believed ethnography could help in their efforts to preserve Delaware traditionalism. Big House leaders lacked the financial resources needed to maintain the church and they were faced with constant assimilation efforts that sought to discourage the practice of the Delaware faith. Big House leaders thus looked outside of their own community for someone who had both the resources and willingness to help. Frank Speck was more than eager to provide this necessary funding as well as his ethnographic expertise to help preserve and revitalize the Big House Church.

Charlie Webber, Frank Speck, and the the Revival of the Big House Ceremony

James Charlie Webber, also known as Charlie Webber and Chief War Eagle, arrived unannounced at Speck's office at the University of Pennsylvania in late February 1928. Webber's purpose was to seek Speck's cooperation on a project to record the origin and procedures of the Delaware Big House Ceremony, and Speck delightedly agreed and made arrangements for financial support and a release from his teaching responsibilities to begin working with Webber (Speck 1928).[8] The result of Webber and Speck's collaboration was the book *A Study of the Delaware Big House Ceremony*, published by the Pennsylvania Historical Commission in 1931.[9]

Webber and Speck's collaboration is significant because it was Webber who sought the ethnographic project. Even more important is that Webber was not just interested in employment or even producing a written account of the Delaware Big House Ceremony. Webber was clearly bringing Speck in on an ongoing effort to revive the Delaware Big House Ceremony (Webber 1929a).

Prior to Webber's work with Speck there was a separate plan to recover the Big House Ceremony. In 1927 some Big House leaders proposed to sell the Big House to the Moving Picture Company of Oklahoma. The idea was that the Delaware would perform the ceremony for the film makers in exchange for considerable payment. The money would then be used to build a new Big House and thus enable the Delaware to continue the ceremony (Washington n.d.).[10]

Selling the Big House to the Moving Picture Company apparently never happened. Other Big House followers, including Webber and his cousin Fred Washington, wanted to preserve and revive the ceremony, but they opposed what Washington referred to as the moving picture option. When recounting the debate over the moving picture issue, Washington (n.d.) wrote, "I am [word unclear] against a disposal of historic customs of my People as well at the disposal to the movie firms also destruction of the Big House. I am for rebuilding the old Delaware Church, and be kept private [word unclear] as Historical event, as a up keep. An preserve our traditions." Webber, Washington, and other Big House adherents were thus working on ways to revive the Delaware Big House Ceremony prior to Speck's involvement.

When Speck became a part of the Delaware project in 1928 he not only began to document the elements of Delaware religion, but he also became a part of the overall revitalization effort. It is clear from Webber's correspondence that the Big House community in Oklahoma was

well aware of Webber's collaboration with Speck and of Speck's involvement in the project to continue the Big House Ceremony (Webber 1929a, 1929b). While working with Speck in Philadelphia, Webber maintained contact with his friends and relatives in Oklahoma and placed Speck in contact with members of his extended family. Soon, Speck began corresponding directly with Webber's cousin Fred Washington and others within the Big House community (Speck 1929a). In the letters that Speck sent to Fred Washington, he periodically encouraged the Delaware to hold the Big House Ceremony and continued to make regular inquiries about the ceremony's renewal throughout his involvement (1929b, 1930, 1946). In one letter to Fred Washington, Speck wrote, "I hope they will hold the Big House this fall. It is *wrong* to give it up. *Stick* to the old Delaware customs and beliefs. They are as good as any of the white mans" (1929b). Webber, then in Philadelphia working with Speck, was also curious about efforts to hold the Big House. In one of Webber's letters, he inquired about the plan to rebuild the Big House and reminded Washington that he would be back to help with the construction soon (1929a). Fred Washington kept up correspondence with Speck and sent news on the progress being made in Oklahoma (Washington 1934). For instance, Washington informed Speck of the previous plan to sell the Big House to the Moving Picture Company (Washington n.d.)

Speck and Webber also sought financial support to rebuild the Big House Church while working in Philadelphia. Speck must have approached the Pennsylvania Historical Commission for funding because the Commission subsequently donated $500 to the Delaware for the purpose of rebuilding the Big House Church (Big House Committee 1929a).[11] The donation was deposited in the First National Bank at Dewey, Oklahoma, and the Delaware Big House Committee was organized by the Big House leaders to oversee the expenditure of the funds (Big House Committee 1929a). The members of the Big House Committee were James Thompson, George Falleaf, John Falleaf, Willie Longbone, Charlie Elkhair, Frank Frenchman, Samuel White, Jack Longbone, Fred Washington, and Joe Washington serving as the secretary (Big House Committee 1929a). There were no other offices beyond the Secretary position (Big House Committee 1929a).

In June 1929, the members of the Delaware Big House Committee met and signed a formal agreement to "accept the proposed plan offered by the Historical Commission of the State of Pennsylvania and agreement to preserve and build a new Big House" (Big House Committee 1929a). The following September, the Committee resolved to identify a new location on which to build the new Big House Church (Big House Commit-

tee 1929b). A subcommittee made up of three men—George Falleaf, Willie Longbone, and A. T. Hill—were selected to identify the new building site (Big House Committee 1929b). A few days later, two members of the Committee, George Falleaf and Joe Washington, signed an agreement to accept the new location for the Big House and authorized Joe Washington as secretary to begin drawing on the donated funds for the construction of the church (Big House Committee 1929c). The new location was to be on Fred Washington's allotment, which was several miles to the east of the Big House's original location (Big House Committee 1929c).

While there is no evidence that a new church was ever built on Fred Washington's allotment, efforts to revive the Big House Ceremony continued in different forms throughout Speck's work with the Delaware. Speck began consulting with Washington and other members of the Big House community in order to document Delaware religious beliefs and practices. Speck sustained such work until after the Big House Ceremony was performed again in the mid-1940s (Fred Washington Collection; Frank G. Speck Papers). Speck published another book in 1937, entitled *Oklahoma Delaware Ceremonies, Feasts and Dances* and based on this collaboration with different leaders within the Big House Community. Speck also provided Delaware craftsman, which included Fred Washington, a market for original and recreated objects associated with the Delaware Big House and other ceremonies. Speck acquired many Delaware items from different artisans on behalf of museums in the northeast (Fred Washington Collection, Frank G. Speck Papers). Speck's involvement thus provided awareness for traditional Delaware beliefs as well as a source of economic support for his collaborators.

The 1944–45 Big House Ceremony may well have been the culmination of efforts that began with Webber's collaborative work with Frank Speck. Consider Ollie Beaver's memory of Charlie Webber's role in the 1944–45 ceremonies:

> During the war they had it at Fout's in a tent. We went nearly every night. Charley Webber started that. They began to say, "We could have those meetings," so they began to fix a little house made of bark and a tent the best way they could fix it. They had a fire in the middle. . . . Charley Webber was always the one that started it; He Began to sing. There were three of them—the other two were Reuben Wilson and Joe Washington. Also, Ben Hill. . . . There were not very many there, everyone was down and out. A lot of boys were gone to service, be we tried to have it the best we could. (Grumet 2001:155)

Though not constructed at Fred Washington's allotment, Webber and others did build a new Big House Church for the 1944–45 ceremonies. The structure was erected on an allotment a few miles north of Dewey, Oklahoma, at the home of Minnie Fouts. Charlie Webber is credited as the organizer of the service and those in attendance included the members of the Big House Committee.

Also evident in Ollie Beaver's narrative is the memory of the war and economic hard times during which the revival effort took place. The ceremonies were reportedly well attended by the elders but few in the younger generation were present due to lack of interest or active service in World War II (Weslager 1972:14). The Great Depression was an especially difficult time for the Big House followers. It was not an easy task to sustain the traditional Delaware faith during a time in American history when being Indian placed a person at such an economic, political, and social disadvantage. But they tried; they tried the best they could.

The 1944–45 ceremonies were thus the last Big House Ceremonies despite the ongoing efforts to continue the traditional Delaware religion. Twenty years passed between the 1924 and 1944–45 ceremonies as several leaders including Charlie Webber sought ways to save the Big House Church. Frank Speck was one important component of the Delawares' overall project. Speck provided what limited funding he could find and helped record the traditional Delaware faith while religious leaders organized efforts to rebuild the church and remobilize the faithful.

The Legacy of the Delaware Ethnographic Project

The physical remains of the Big House Churches can no longer be found along the upper Caney River valley. The original Big House Church slowly fell into disrepair and was finally removed when the land was purchased by a non-Delaware landowner. Some of the structural elements from the original Big House Church were sold to museums and most are found in the Philbrook Museum in Tulsa, Oklahoma and the Woolaroc Museum near Bartlesville, Oklahoma. Nothing remains of the 1944–45 Big House structure (Weslager 1972:14–16). While the structures may be lost, the legacy of the Big House Ceremony remains among the Delaware and in anthropology.

Today, the Big House remains present in the social lives of the contemporary Delaware community. The memory of the Big House inspires a sense of traditionalism and tribal identity that is shared by the descendants of the Big House community. The Big House was a particularly

Delaware ceremony and the Delaware take great pride in asserting that cultural uniqueness. Terry Prewitt has written about the way that the Delaware Big House continues to inspire a sense of traditionalism among the Delaware:

> For a six-year-old Delaware boy dancing in a powwow today, detached and totally self-controlled in his art, the act of a handshake from an elder extends a formal blessing on the new focus of traditional life, and (the) great cultural distance of five generations is bridged. This is the greatest significance of the Big House for the contemporary Delaware community, a significance that cannot be written about or reported upon because it has not yet come to its completion. (1981:72)

The legacy of the Delaware Big House thus continues in contemporary Delaware society.

The Big House also continues to be a source for community action in the Delaware Tribe. New events and institutions are established in the name of the traditionalism that the Big House represents. For instance, the Eagle Ridge Dance Ground was built in the vicinity of the original Big House location to provide a place for the Delaware to hold social dances and ceremonies. Even Christian churches have been founded in the name of the Delaware Big House. One example is the Indian Methodist Church in Dewey, Oklahoma, which was established as a church for the local Indian community. One Delaware woman told me about her mother's role in helping establish the Indian Methodist Church following the last Big House Ceremony:

> The Big House, that was really important to her and the last time they had it then was during the war, and they quit after that . . . at the time we were going to stomp dances at Bill and Thelma Pace's . . . down from their house to the south is Coon Creek. . . . Well they fixed a stomp area and we'd have; go to stomp dances down there and it was just, it would be just packed. I mean there would be a hundred Indians down there and they'd be, you know, having a good time and everything. And mom was, she talked to Bill Pace and told Bill, said Bill we should have a church for all these people, said there is no church for them and they'd go [to] different churches, some of them did and he said, well we'll see about it, you know, because Bill had been a minister at one time.

From this it is evident that while the Big House ceremony ended, the socio-religious life of the Big House community continued through different institutions. The Delaware woman explains that both the stomp dance and the Methodist Church were established to provide the Big House Community with a place to socialize and congregate in the spirit of traditionalism embodied in the Delaware Big House Ceremony. It is through such contemporary institutions that the Delaware Big House continues to inform the social identities of contemporary Delaware people.

The Big House also continues in the ethnographic literature on the Delaware. Though not the first ethnologist to document Delaware religion, Speck's publications stand as seminal works in Delaware ethnography.[12] Speck's collaboration with the Delaware brought a sense of awareness and empowerment to the Big House community that was not present in previous ethnographic work.

A more substantial contribution might also be in the collaborative approach taken by Speck and his influence on subsequent ethnographic work with American Indian communities. Speck is noted for his applied efforts in several of the communities with which he worked and his involvement with the Delaware illustrates a case in which Speck's assistance was even sought. Today such an approach is relatively common among ethnographers. Contemporary ethnographic work in American Indian communities is often the result of a larger project instigated by an Indian community who seeks the assistance of an ethnographer (Basso 1996:xv; Jackson 2003:5). Speck may not have been the first applied anthropologist, nor did he recognize that he was establishing an applied component to an emerging discipline, nor was the applied methodology fully developed. But what Speck did accomplish was to set an example for future ethnographers. His work demonstrates what good ethnographic work could be, and it is Frank Speck's example that many subsequent Americanists have sought to follow.

Such is the legacy of the collaborative effort to salvage the Delaware Big House Ceremony. The Big House Ceremony is among the most thoroughly documented of American Indian religious events, and Speck's collaborative approach is continued in the methods of many contemporary ethnographers. Though the Big House does not remain structurally or in practice, it does remain alive in the minds and practices of Delaware people. The Big House is present at every social gathering where Delaware people meet to reaffirm a sense of shared Delaware identity. The efforts of those committed Big House leaders and their ethnographer were thus able to salvage the Big House Ceremony despite the passing of the ceremony's annual performance.

Notes

1. Ethnography is a creative process influenced by the dominant ideologies of the time (Clifford and Marcus 1986). The work of American Anthropologists, especially in the discipline's formative years, has been scrutinized for its biased representation of indigenous peoples (Deloria 1969). It is true that early twentieth century ethnographers assumed that American Indian culture was soon to be lost and their task was to record only those cultural traditions that had not been influenced by Anglo-American society. Constructing an idealized version of American Indian culture did create an image that was damaging to Indian people. Such an approach created a mythical cultural entity that did not represent the lifeways of American Indian people and presented the acculturation of Indian people as a self-fulfilling prophecy. For more specific treatments on how western images of indigenous peoples have influenced early twentieth century ethnography see Stoler's (1995) critique of Freyre's work in Brazil, Blackhawk's (1997) critique of Julian Steward's portrayal of Great Basin peoples, Biolsi's (1997) critical look at early work among the Lakota and Nurse's (2001) examination of Barbeau's work with the Huron-Wyandot.

2. Terry Prewitt (1981:64–66) provides the most detailed discussion of the so-called "Big House Community" as it existed during the era discussed. Jim Howard (1980:159) found that it was common among eastern Oklahoma groups in the early twentieth century for traditionalist leaders such as the Big House leaders to lose their secular power.

3. The original Delaware homelands were in the Delaware and Hudson River valleys that are now the states of New Jersey, Delaware, eastern Pennsylvania and southeastern New York.

4. Weslager (1972, 1978) provides a complete outline of Delaware removal.

5. For instance, Speck (1928) introduces Charlie Webber as "the Chief of the Turtle clan and Secretary of the Council of that tribe." Yet there is no record of Webber ever serving on the Delaware Business Committee (Carrigan and Chambers 1994:35).

6. For a more detailed discussion of the historic settlement patterns and ethnogenesis of contemporary Delaware identity see Obermeyer (2003).

7. The information here on the Delaware experience with the Indian Boarding Schools is based on my conversations and interviews with Delaware elders who experienced the forced education firsthand. Many Delaware attended Chilocco Indian School and their memories are consistent with those reported by K. Tsianina Lomawaima (1994).

8. Webber and Speck's work was funded by the Pennsylvania Historical Commission (Speck 1931).

9. Though the accuracy of Webber's account has been questioned, it remains an important contribution to Delaware ethnography. See Grumet (2001:96) for a concise discussion of the debate over Webber's credibility.

10. Another issue was the financial support needed to hold the ceremony. The original Big House was located on the allotment of Walter Wilson, relative of the last leader of the Big House Ceremony held in 1924. Based on information gained from Webber, Speck (1931:18) reports that one of the reasons for the Big House's abandonment was the lack of wampum. In a letter home to Joe Washington, Webber mentions wampum again and identifies Wilson as the man who sold the wampum (Webber 1929a).

11. It is possible that the money was to be used to build a replica Big House in Pennsylvania according to the specifications given by the Delaware Big House Committee. Correspondence between Speck and the Historical Commission mentions the possibility of having the Big House built in Pennsylvania (Shoemaker 1932). Such a plan would have been

consistent with Speck's method of acquiring objects of traditional craftsmanship from the Delaware for local museums. More research is needed, however, to determine if indeed the Historical Commission's donation was used to build a replica Big House in Pennsylvania rather than Oklahoma.

12. The Big House Ceremony has been given tremendous ethnographic attention both before and after Speck's work. Grumet (2001) provides the most recent and comprehensive collection of essays written by both scholars and Delaware people on the topic. Miller (1980, 1997) gives a historical overview of the aboriginal aspects of the ceremony and a structural analysis of its performance. Wallace (1956) considers the Big House as a "new" religion first developed in the late eighteenth century as part of a Delaware revitalization movement. Other work includes Mark Harrington's (1913) ethnography of the Delaware Big House, Truman Michelson's (1912) account based on an interview with Charlie Elkhair, and Terry Prewitt's (1981) ethnohistory of the Delaware Big House community. There are also a number of works on the Big House by Delaware preservationists and scholars. Richard Adams's (1904) account was collected while working on legal issues concerning the tribe's relationship with the Cherokee Nation, Nora Thompson Dean (1984) gives accounts based on childhood experiences, and Ruthe Blalock Jones (1973) provides a series of oral history interviews with former Big House participants.

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11. Applying Anthropology in the Interest of the State

John Collier, the Indian Office, and the Bureau of Sociological Research

Elizabeth Guerrier

This paper will trace the development of the Bureau of Sociological Research through its roots in the use of social science by the Indian Office. The Bureau of Sociological Research instituted at the Colorado River Japanese American Relocation Center, as the Poston camp was formally called, was developed out of the experiences of social scientific advising in the Indian Office and in industry. At this moment, at the height of World War II, the social welfare state was emerging as a central component of the growing and changing industrial economy. In the interests of securing a large and stable workforce to fulfill the needs of the expanding industrial sector, social-service programs including health and educational institutions combined with popular cultural discourses to inculcate and promote lifestyles, values, and desires that supported a social structure conducive to the industrial labor system. Wherever "problems" were encountered, resistances met, or transitions were not seamless and unencumbered, social scientists were increasingly called upon to assess, analyze, and advise. The wartime incarceration of the Japanese Americans was one such "problem," and the Bureau was formed in response. Because there has been little critical work written about the Bureau thus far, I will begin by describing what it was, who was involved, and how it was situated within the context of the development of American applied anthropology.[1] I will then establish the connection between the Bureau and the Indian Office, particularly through a consideration of John Collier's influence in both of these institutions. Finally I will discuss how Collier's social philosophy and program of applying social science was situated within the expansion of the social welfare state and within what Joel Pfister terms a "protomulticultural" ethic that served to support the Nation State and economy.

American applied anthropology has from the outset seen as its purview the application of anthropological knowledge to the solving of "practical" problems of human organization. The nature and specifics of these

problems have ranged over a wide spectrum of issues and concerns. Perhaps the greatest volume of this work, in the early years, was on Native American education and administration, and on industrial labor relations. Although these areas of emphasis appear to suggest entirely different types of projects, in fact, the coincidence of these two foci is not at all coincidental. The example of the industrial application of applied social science merely articulates the logic underwriting the multiple projects of social organization/control with certain clarity. For just as with the Western Electric project, in which the objective was to produce more productive and efficient workers, the Indian Office was too employed in a project of turning communities of Native peoples into individuals who would fit smoothly into (a specific) place in the American economic and political order; an objective that changed in strategy and form over the years, but not in intent. The applied work functioned to expand the reach of state power, encouraging people to "adjust" to dominant economic and political forms in a more "humane" fashion, as an alternative to physical violence.

Looking at this context for the development of American applied anthropology shows how social science, like the helping professions more generally and in spite of many good intentions, has functioned as a form of social engineering in the interest of state and capital. The objective of this investigation is not simply to chastise a discipline or branch thereof, or to condemn individuals for their participation in a project that, according to today's standards, is judged to be reprehensible. Such a practice might satisfy a particular inclination towards judgment, but would do little to advance a productive dialogue on the relationship between social science and state power. Within the critical literature on "internment anthropology" the focus of debate has been on evaluating the intentions of the programs of research and of the individuals involved. In spite of Orin Starn's attempt to evaluate the consequences of the research, the response to his 1986 article in *American Ethnologist* by former internment researchers pulled the conversation back to oppositional grounds; either they were guilty of participating in the perpetration of a great injustice, or they were admirable for defending the rights of the internees and attempting to assist them during this regrettable incident. This debate assumes the choice to be mutually exclusive. Instead, this paper, like Starn's earlier critical work, interrogates the complex of ways through which inequalities are produced even through discourses that attempt to defend against them. Moreover, in tracing the relationship between internment anthropology and the practice of applied anthropology in the Native American context, I am rejecting any assertion that this use of an-

thropology was "our worst wartime mistake," as the removal of the Japanese Americans itself has been characterized. Here I focus on the continuities between different instances in applied anthropology so that the unintended consequences of anthropological practice can emerge as coherent effects that can be subjected to scrutiny and critical reflection.

The Internment of the Japanese Americans and Establishment of the Bureau

During World War II approximately 110 thousand Japanese Americans were imprisoned in ten internment camps scattered throughout Arkansas, Arizona, California, Oregon, Utah, and Wyoming. Rationalized as a move to protect national security, Executive Order 9066 excluded all residents having any Japanese ancestry, citizen or no, from the so-called "exclusion zone" along the western coast of the United States. After first being housed in temporary relocation centers the internees were sent to inhabit the camps scattered throughout the interior. Within the camps the "loyal" were separated from the "disloyal" and classes in "Americanization" were promoted as important mechanisms for preparing internees for successful reintegration into American society. Overseen and administered by the War Relocation Authority (WRA), the camps became not only places of imprisonment for the Japanese Americans, but field sites for anthropologists and other social scientists.

The first of three research projects carried out within the camps was initiated at the Poston, Arizona, camp, located on the Colorado River Indian Reservation, and undertaken under the initiative of John Collier, then commissioner of Indian Affairs. Collier had previously drawn heavily on social science in the administration of Native American populations, and quickly integrated a similar program within the camps.[2] This first project was organized under the Bureau of Sociological Research and headed by Alexander Leighton. Leighton describes the purpose of the Bureau thus:

> From the very beginning, the research project was conceived as having three functions. First, it was to provide the Center Administration with facts and suggestions on current problems appertaining to the attitudes and sentiments of the people. Second, it was to gain experience and compile data that might be of use in the governing of occupied areas. Third, it was to train a research staff that would be capable of working in occupied areas and providing the governing body there with the same kind of service it gave the Center administration. (Leighton 1946:656)

By the end of 1942 the War Relocation Authority, under the advice of acting consultant and University of Chicago anthropologist Robert Redfield, and under the influence of John Provinse and John Embree, anthropologists working within the administration of the WRA, formally incorporated applied anthropology within its operations (Leighton and Spicer 1946:374). The Bureau is credited with promoting the incorporation of anthropologists within the all of the camps after they had positively influenced the outcome of the "Poston Strike." As violence began to surface within other internee populations, WRA anthropologists known as "community analysts" were deployed to each of the ten camps. However, relations were strained between Collier at the Indian Office and Dillon Myer, head of the War Relocation Authority, eventually resulting in Collier's departure from Poston.[3] Leighton went on to work for the Office of War Information as head of the Foreign Morale Analysis Division, and the War Relocation Authority's Community Analysis Division took over the Bureau activities at Poston.

The third research project undertaken in the camps, the Japanese Evacuation and Resettlement Study (JERS), was ostensibly independent from the War Relocation Authority, and was headed by Dorothy S. Thomas, University of California, Berkeley. The JERS, unlike the WRA and Bureau research, was not intended to aid in the administration of the camps; rather, it was conceived as a study of "enforced mass migration" which would assist in the effort to deal with population upheavals in postwar Europe (Ichioka 1989:5). This project was carried out primarily at three camps, Poston, Tule Lake and Minidoka, although short periods of observation were carried out in five other camps as well. There were three books published as a result of this research. *The Spoilage* (first published in 1946), which focused on those internees deemed "disloyal," and *The Salvage* (1952), a study of the "loyal" internees who relocated to take up employment or education opportunities in the midwestern states, were both authored by Thomas and Nishimoto. Nishimoto also contributed to the third JERS publication, *Prejudice, War and the Constitution* (Ten Broek 1968), that considered the context of the internment and the legal issues it raised.[4] Nishimoto was an internee and field worker with JERS who initially worked for the Bureau but resigned over differences with Alexander Leighton (Hirabayashi and Hirabayashi 1989). A collection of his notes and letters has been compiled by Lane Ryo Hirabayashi and published under the title *Inside an American Concentration Camp: Japanese American Resistance at Poston, Arizona*. Hirabayashi has also published a volume examining the experiences of another field researcher who, like Nishimoto, left the Bureau because of disagreements with Al-

exander Leighton. Tamie Tsuchiyama was an anthropology student at the University of California, Berkeley, at the time of the internment and had been working for both projects until December of 1942 when she took up full-time work with the JERS. Nishimoto and Tsuchiyama were just two amongst many internees who worked as field researchers under all three projects. Some went on to graduate with degrees in anthropology, gaining some of their credits through their participation as researchers and at seminars held within the camps. All of these researchers, however, held junior positions within the projects. Camp analysts under the Bureau and WRA were brought into the camps from previous postings with the Indian Office, the Department of the Interior, and from positions where they carried out applied work with industry around the United States. Of the social scientists involved in research as community analysts with the Bureau and WRA, twenty out of twenty-seven did their graduate work or received their terminal degrees from departments of anthropology and sociology (Suzuki 1981:24). In this paper I use the term "social scientists" to refer to the various individuals involved in the projects, because of the diverse backgrounds from which they came. However, regardless of the specific disciplinary training of the particular authors, the methodologies employed, theoretical orientation, and the form of the final texts falls within a particular paradigm that I am here calling "internment anthropology."

Internment Anthropology and the Development of Applied Anthropology in America

This was a heady era for applied social scientists. The burgeoning discipline had grand ideas about its potential for influence and was increasingly being sought out by government and industry alike for advice on the effective management of human populations. Thomas James, writing about the use of social science in the development of "community schools" for the Navajo, situates John Collier's incorporation of social science within the Indian office in the context of this disciplinary history. "Applied social science," he observes, "had come into its own, fully credentialed and professionalized in academia, equally applicable to public welfare and private enterprise, bringing new conceptual instruments to bear on the management of public life, and subsequently discovering many more applications in the nation's widening sphere of military and economic pre-eminence" (1988:601). The internment of Japanese Americans during World War II and the formation of the War Relocation Authority occurred at a time when applied social scientists who had been

working with marginalized peoples and those whose experience was developed in work with industry were coming together under the Society for Applied Anthropology to promote their methods and perspectives. Internment anthropologists published extensively within the Society's journal, *Applied Anthropology*, and made up a significant contingent of those practicing within this new discipline. The battle for professional and institutional recognition was far from won, however, and assertions of the importance of a social scientific perspective to "practical" administrative work pepper the writings of the internment anthropologists. John Provinse, for example, expresses concern that social scientists are not taking up the kinds of research that can be easily drawn upon in the formation of policy. He cautions:

> Compared to the physical sciences, the social sciences are making a rather pitiful showing in the translation of their scientific knowledge into the "know-how" of action. Because of this development of know-how in the physical field, the physical scientists are rapidly usurping the function of the social scientists, and they will continue to do so as long as social science limits its definition of problems as narrowly as it now does, and resists as strongly as it has in the past entering into problems of applied character. (Provinse and Kimball 1946:401–402)

In spite of Provinse's concerns, however, the use of social science in government grew to unprecedented proportions during the 1930s and 1940s, "an infusion so dramatic in its effects that more social scientists worked for the federal government during those years than ever before in the history of the United States" (James 1988:599). One of the most vociferous advocates for the expansion and development of applied social science from amongst the internment anthropologists was Alexander Leighton, the director of the Bureau of Sociological Research. Even before the time of his appointment at the Bureau he wrote in *Applied Anthropology* calling for a uniting of the social sciences to address common problems. A U.S. Navy psychologist with anthropological training and experience, Leighton argued for the practical application of interdisciplinary approaches to the administering of human populations.[5] Advocating for a "scientific humanism," he saw applied social science as a democratic practice necessary for the creation of a better world. "We cannot expect to achieve a more stable world by simply putting a stop to Axis plans. We must offer better plans. This requires the social sciences to develop fully all their potentialities as applied sciences. Democratic thought and action may then incorporate the best that science has to offer instead of polit-

ical bias, hate reactions, and sentimental stabs in the dark" (1942:26). Perhaps an indication of the close relationship that would develop between the two, Leighton offers praise for John Collier's use of social sciences within the Indian Office and for his utilization of Rockefeller funds to provide internships for graduates in different fields to gain experience in administration and working with Indians (1942:29).

Leighton saw great potential for the use of social science in spreading "democracy" at home and abroad and would become involved in the administration of occupied territories in Japan after the war. He understood this practice as continuous with past uses of social sciences and as contributing to the development of the disciplines as well as aiding administration of populations. "One can apply the concepts and methods which in the last twenty years have not only widened the horizons of social psychology and anthropology, but have proved their practical value in problems of education, land management, agriculture, industry, welfare work, mental hygiene, and in the administration of Indian tribes and colonial possessions" (Leighton et al. 1943:653). The largest and perhaps most frequently referenced book produced as a result of the internment research is Leighton's *The Governing of Men*. In this volume he presents and assesses the Poston experience from the perspective of internment as an administrative problem and draws from the Bureau's analysis principles and recommendations for governing populations under stress and in conditions of dislocation. Included in this volume is an appendix co-authored by Edward Spicer, "Applied Anthropology in a Dislocated Community," that presents a discussion of the history, organization, and methodology of the Bureau.

The applied anthropological work was often published in the form of program analysis reports describing the work of the camp analysts and their contributions to the practical problems of administering internees. The head of the Community Analysis division of the War Relocation Authority, John Embree's 1944 article in the *American Anthropologist*, "Community Analysis—An Example of Anthropology in Government," for example, provides background and overview of the establishment and function of the Community Analysis division of the WRA including job descriptions and required methodology. In this article he also makes recommendations for forging successful relations with camp administration and for further use of community analysis in meeting military objectives. The use of social science is presented by Embree as being in opposition to an increased use of military force, thereby positioning social science as the peaceful, respectful, and preferable method for ensuring the maintenance of order and for the prevention of violence. In his

description of the initiation of the Community Analysis Division, Embree reflects upon the administrative response to unrest amongst the internees at two camps, Poston and Manzanar. Although some at the Authority were concerned that the two incidents were evidence of organized resistance that would likely spread to other camps, Embree was confident that "the problem was primarily one of local administration and what was needed was not so much an increase in military control but rather an improvement of the administrative organization on the one hand, and a better understanding of the attitudes and social developments within the center population on the other" (1944:280).

The removal of the Japanese Americans to the camps presented a number of other administrative challenges. By the winter of 1942 questions as to the fate of the internees arose on various fronts. While some (especially the American Legion) argued the camps should be more tightly secured and run by the army, others, including the War Relocation Authority, were becoming increasingly aware that many of the internees did not pose a threat to the security of the United States. In addition, labor shortages in industry and agriculture were putting a strain on the wartime economy. The WRA was becoming eager to resettle internees throughout the country, arguing that "the best solution to the problems of the evacuees lay in getting them out of the centers and re-established in the industrial and agricultural life of the country" (Spicer 1952:142). Moreover, a significant number of Nisei were upset by the denial of their right to join the armed forces and serve in the battle overseas.[6] As a result, a joint Army-WRA effort was developed to administer a questionnaire assessing the "loyalty" of all adult internees, register their response, and sort them accordingly. Once the loyalty status of the internees had been established the War Relocation Authority was eager to both reduce the burden on their resources and avoid replicating the "problem" of Indian reservations, and so embarked upon yet another process of relocating the internee population. Dillon Myer, head of the WRA, testified before the U.S. Senate Military Affairs Subcommittee in January 1943 regarding the importance of moving internees out of the camps and integrating them into the broader population:

> I sincerely believe, gentlemen, that if we don't handle this problem in a way to get these people absorbed as best we can while the war is going on, we may have something akin to Indian reservations after the war. . . . We will have a racial issue which I don't think we need to have, and I am frankly hoping that quite a number of those people will get established in positions in dif-

> ferent parts of the country other than on the Pacific coast, where they can be accepted as part of the population, where they can gradually be absorbed as American citizens, and thus dispose of a racial problem that has been a pretty tough one for the coast people and for the United States. (Drinnon 1987:29)

The related processes of determining loyalty and encouraging dispersal were central concerns of internment anthropology. The 1943 article authored by the Bureau of Sociological Research, "The Japanese Family in America" is an early example of these social scientists addressing the loyalty question. This article seeks to understand the dynamics that had developed within Japanese American families throughout dislocation, internment, and the resettlement of some segments of the population. The authors argue against assertions that Japanese were unassimilable due to racial characteristics, and suggest that Japanese have faced barriers to assimilation because of racial discrimination. They categorize the different reactions of families to their circumstances focusing on the ascription of loyalty and the degree of realized and potential Americanization. They suggest that Japanese family characteristics can aid in the process of Americanization.

When met with the reluctance of the internee population to participate in this dispersal, the camp analysts were charged with the task of forging some understanding of their hesitation in order to better effect the objective of relocation. Embree (1943a), Yatsushiro (1944), and Spicer (1946, 1952) published articles on the question of their reluctance to participate in the relocation program. On the difficulties in getting internees to leave the camps, Luomala bemoans, "Not since an earlier period of American history when the United States had evacuated Indians and placed them on reservations had there been as thankless a task as this. . . . Like the Navahos [who had been captured and incarcerated for five years at Bosque Redondo in 1863], the Japanese Americans were for the most part indifferent to opportunities to resettle elsewhere than 'back home'" (1947:27). In his 1946 article on "The Use of Social Scientists by the War Relocation Authority," Edward Spicer reports on the influence of community analysts' observations regarding resistance to resettlement on War Relocation Authority policy. According to Spicer, at the time resettlement policy was being considered there were two competing ideas about the best approach to encouraging resettlement: one group within the WRA believed that decreasing jobs available within the camps thereby increasing feelings of insecurity would lead to a greater interest in leaving the camps; another believed that it was important not to increase the

insecurity felt by the internees, in order to give them the confidence to resettle. The community analysts surveyed internees and came up with recommendations, two of which were incorporated into policy: discussing resettlement with the Issei leaders, and utilizing family discussion and parental consultation to promote resettlement (Spicer 1946).

Internment work was a prime example of the possibilities for providing practical advice to those involved in the administration of large populations. Some of the issues for which internment research was most heavily drawn upon were the administration of loyalty questionnaires, the settling of conditions of unrest within the camps (Poston and Manzanar), and encouragment of dispersal and resettlement outside the camps by those deemed "loyal" Japanese Americans. Internment research was also drawn upon in the formation of strategies for gaining the surrender of Japan, and for governing populations in occupied areas in Japan after the surrender. In these capacities, internment anthropologists were very clearly involved in the direct practice of governing internees.

Another, less obvious way social science was involved in governing internee populations was more insidious, and perhaps less understood at the time as being part of a practice of governance. Based on their work on "acculturation" and on understanding internees' beliefs, values, and practices, they advised the administration on how to create conditions that would foster compliance with camp rules, structures, and social organization. Working with internees in an attempt to "help them adjust," they were participating in the construction of Japanese American subjects who were inculcated with values and practices consistent with the dominant American society. Furthermore, social scientific modes of classification and representation contributed to a larger process of constructing an identifiable group of "Japanese Americans" that could be imagined, known, isolated, and eventually interned.

Critical Literature on Internment Anthropology

An understanding of this role of social scientists as "technicians of consent and control" (James 1988:600) receives surprisingly little attention within the body of critical work on internment anthropology. In fact, the extent of this work has been limited. The first scholar to critically examine this research, Peter Suzuki, didn't publish on this topic until 1980. In "A Retrospective Analysis of a Wartime 'National Character Study,'" published in *Dialectical Anthropology*, Suzuki writes a damning indictment of Weston LaBarre's national-character study of the Japanese that he based upon a short stint as a researcher at the internment camp "To-

paz." Suzuki uses other analysts' work, especially that of John Embree, to contradict LaBarre's assertions that the Japanese character is compulsive and neurotic. Summarizing LaBarre's method, Suzuki claims,

> The method used in LaBarre's 1945 article is now fairly obvious. To suit his own purposes he arbitrarily, and tautologically, devised 19 neat categories ("traits") of the compulsive neurotic and found none of them wanting in the Japanese. He then proceeded to fit virtually every aspect—unique and general—of Japanese culture, such as poetry, music and dance, the Noh drama, the tea ceremony, ikebana (flower arrangement), bonsai (the art of miniature-tree cultivation), language and honorifics, social relations, child-rearing practices, intrafamilial relations, religion, technology, ad infinitum, into the appropriate compartments of neurotic behavior. Interlacing all this were emotional appeals to patriotism to convince the reader of the efficacy of LaBarre's labels, method, and thesis. (1980:41)

Suzuki's next published article on the subject speaks to the breadth of social scientific work done in the internment camps. In this paper, also published in *Dialectical Anthropology*, he critiques the work of the anthropologists, the role they played in intelligence gathering and the publications that resulted from the research. He reviews some of the published material making note of factual errors, the lack of context provided, and the biased perspectives of the researchers. Clearly not a fan of applied anthropology or of policy analysis studies, Suzuki expresses his disappointment in the lack of detailed ethnographic work, and complete absence of kinship and linguistic studies (1980:34). Furthermore, he argues, "by assimilating the categories of thought and the perspectives of the bureaucracy, the social scientists were also vulnerable to reifying concepts and terms which had very little grounding in reality" (1980:43)—by which he refers to categories such as community and community government. In both of these critical pieces Suzuki's analysis centers on the content of the research carried out, the types of reports that were produced, and the intentionality of the researchers. Were they right or wrong to participate? Were they implicated in the injustice perpetuated against the internees? His critique is limited to looking inside internment anthropology per se, and though he appropriately contextualizes the research within the history and politics of the Japanese experience in America he offers no consideration of the research as an outgrowth of an *anthropological* tradition.

Orin Starn was the next scholar to critically address the role of

anthropologists in the internment, pointing out that there are two predominant perspectives on the anthropological work on internment: first, that they helped to make the process easier on the Japanese Americans and the WRA; this is the dominant view of those who were involved. Second, that the ethnographers were accomplices of government, the view espoused by Peter Suzuki. Neither perspective satisfies Starn because, as he points out, the first position doesn't acknowledge the involvement of the researchers in a project that was "denounced even at the time as racist and unconstitutional; it does not adequately address the questions about science, power, and politics that the internment episode so sharply poses" (1986:702). Moreover, the second perspective doesn't acknowledge the good intentions of the researchers. Says Starn, "No serious attempt is made by either side to combine close analysis of the circumstances surrounding anthropological participation, the intentions of the ethnographers and the outcome of their work. Context, intention, and consequence are artificially separated" (1986:702).

Caroline Simpson's focus on the politics of representation questions the very categories that are assumed by all, regardless of intention. In her 2001 book *An Absent Presence: Japanese Americans in Post-War American Culture*, she explores the multiple ways that Japanese Americans were constituted as "Oriental Other" in postwar America. She identifies social scientific theories and methodologies as complicit in constructing seemingly bounded categories such as "Japanese American" that facilitated the removal of all persons of Japanese ancestry from the "exclusion zone." The social scientists' way of thinking constructed the Japanese Americans as the "Oriental Other," and consequently they knew how to resolve the "problem" of their otherness by determining the most effective ways of going about assimilating them. But the "problem" was a problem of their own construction, and one that conveniently resonated with the racist (and economic) objections of the non-Japanese population to their residence and participation within their prewar local communities. All of this focuses on the Japanese as the locus of the "problem" and denies any American responsibility for the exclusions suffered by the Japanese (or Indians, Blacks, or others). Simpson draws upon the critique of representation looking at how the researchers constructed the "Oriental Other" and, in effect, the very "Japanese problem" that anthropologists were either doing enough, or not enough, to solve (according to the majority of the critical work). Simpson's analysis calls into question the nature of that "problem". Was it, in fact, a "Japanese problem" (or an "Indian problem"), or was it a problem of the majority society? Starn's work advances the analysis of internment anthropology from a critique of in-

tentions to a study of the unintended consequences of anthropological practice within the camps. Simpson focuses on the construction subjectivities, specifically of Japanese Americans as the "Oriental Other." Turning to the role of social science in the Indian Office, we will now consider the work this research has done in production of forms of power that operate through the construction of subjects, citizens and knowledge.

John Collier, the Indian Office, and the Bureau

Critical work on Collier and the use of social science in the Indian context is much more engaged with understanding social science as a mechanism for social engineering and enforced assimilation. There is, however, some debate in the "Indian" literature as to how much Collier represented a new way of thinking and a new policy direction. Some (Rusco, for example) argue that his approach represented a break in the assimilation policies that had dominated prior to his tenure. When his assimilationist agenda within the internment camps is considered it becomes clearer that he did, in fact, continue to hold assimilation as central to his vision of the necessary steps toward a more harmonious American society. When the Indian work is considered alongside internment anthropology, it becomes clearer that the social scientists were participating in a project of social engineering and that the camps were a location for the production of "good citizens" and for the reproduction of governmental power.

One of the features of Collier's Indian Office administration that distinguishes it from its predecessors is the extensive use he made of social science. Collier brought to his Indian Office work the techniques that were being increasingly incorporated into regimes of human management throughout the U.S.[7] Understood as a methodology for gaining insight into the nature of communities of all varieties, applied social science was a valuable tool in devising programs of social control. "Social policy could accomplish little without social engineers, the technicians of consent and control who could devise legitimate ways of redirecting traditional loyalties toward emerging patterns of planned development in society" (James 1988:600). These social scientists envisioned a world in which they could employ their scientific methodologies in the service of creating a better society, reducing violence, and easing the lot of new immigrants and visible minorities. However, "this faith in the social sciences has serious implications," as Steven Kunitz argues: "While it is true that they may lead to greater understanding of the dynamics of various groups, there is no guarantee that they will have the humanizing effects

which Collier envisioned. Although they may tell us how to deal more effectively with concentration camps, Indian reservations, and problems of colonial administration, there is no certainty that they will be able to tell us how to keep concentration camps from being built" (Kunitz and Collier 1971:225).

Kunitz situates Collier's social philosophy in the context of a broader based, mid-twentieth-century disillusionment with industrialized, individualized society. Collier's desire to create communities that could model alternative forms of social organization to the deterioration of American society drove his work in Native reservations and Japanese American internment camps. From his earliest writings about Native peoples it is evident that he saw in the Indians (particularly at Taos Pueblo) what he thought was missing in American society.[8] Influenced also by Gaelic revival and European cooperative movements of the day, he sought out examples of social structures that could retain and develop a strong sense of "heritage" and "personality" while simultaneously adapting to rapidly changing technology. Collier looked toward the majority European society and despaired: "individual personality has not grown more; certainly it has grown less. Because *heritage* has been neglected, assimilation has failed; and at present Europe, dominated across half its area by the mob mind, and meshed and paralysed through nearly all its nations by cynicism, is the dreadful fruit of that twofold failure due to the fatal divorce of the two goals which in the way of life can only be one goal" (Kunitz 1971:221). In Native American communities he imagined a better possibility: "The Indians, and those who work with them, can make a true contribution to this world problem by choosing *not* assimilation and *not* heritage, but by ardently and skilfully choosing both" (Kunitz 1971:221).

Assimilation, although not the only goal of Collier's social plan, nonetheless remained an important objective. And when it came to what particular aspects of "heritage" were to be promoted and retained, it was through the expertise of the social scientists, not the peoples themselves, that the determination would be made. Schwartz suggests that in spite of Collier's assertions in his earlier Red Atlantis work that American society has much to learn from Indians, his later writings and career advance the idea that social science and social engineering are required to assist the Indians to make the necessary transformation to more modernized societies. "Collier's most consistently expressed ideas about community were that cooperation and the appearance of consensus are good for people; that social scientists can learn to create and regenerate communities; and that communities should be led by self-effacing experts. His working

concept of democracy was a justification for indirect rule and scientific management" (Schwartz 1994:525).

Collier's orientation towards "community" and "heritage" did, however, situate him apart from many of his fellow bureaucrats in that he promoted elements of Native culture and sought to encourage the development of Tribal societies through the expansion of their land base and funding of cultural initiatives. Set against a policy history of expropriation of lands and an assumption that Native Americans were "a dying race," Collier appeared, and indeed functioned to an extent, as a protector of Tribal rights. There is a significant debate amongst scholars of American Indian history about the legacy of Collier and the "New Deal" era.[9] Richard Drinnon, for example, a historian who has written perhaps the only volume looking at the overlap between Indian and Japanese American wartime administration, praises Collier as a defender of Indian cultures and rights, emphasizing Collier's "nurturing of tribal cultures and self-government" (1987:168–169). According to Drinnon, Collier "was a charismatic contradiction, a bureau official who treated tribal cultures with respect. He worked to promote their revival, extend religious freedom, foster political self-determination, and 'curb the administrative absolutism' of the Indian Service" (1987:40).

Thomas James, on the other hand, argues, "Despite its rhetoric of liberation, the Indian New Deal strengthened indirect controls through administrative consolidation programs, and localized community education where none had existed before, thus reaching more deeply than ever into the social and family structure of the tribes" (1988:604–605). Biolsi, likewise, describes the Collier administration's policies as being policies of cooptation rather than self-government. Although he acknowledges that there were differences between Collier and his assimilationist predecessors, Biolsi argues: "What clearly unifies United States Indian policy during both the Progressive Era and during the New Deal, however, are the root propositions that there is an 'Indian Problem' and that it can be solved by professionals applying rational administrative techniques. The locus of power did not change; it remained in the BIA" (1991:27).

Joel Pfister, in his 2004 work *Individuality Incorporated: Indians and the Multicultural Modern*, compares the approach of Collier to that of Richard Henry Pratt and his Carlisle Indian School. While Carlisle was informed by the objective voiced by Pratt, "Kill the Indian in him and save the man" (Pfister 2004:20), Collier advocated culturally and physically healthy schools that cherish "diversity" rather than "uniformity," says Pfister. Collier, like the other Taos Pueblo "literati," admired "Indians" for their therapeutic potential—as repositories of knowledge and

ways that could offer an alternative to the repressiveness of Euro-American society, and the key to their "lost individuality." For Collier culture was not seen as a source of repression, but as a productive source to support and enhance "personality." Pfister understands this approach as a kind of "protomulticulturalism," and argues that although multiculturalism is usually positioned as opposed to assimilation (à la Pratt and the Carlisle approach), they are, in fact, two aspects of the same hegemonic form:

> Thus, the racial and ethnic "raw materials" that Pratt imagined as needing to be "fed" to the "civilizing mill" so that they would come out as well-mannered worker-individuals, would in liberal-corporate modernity be used increasingly to feed dominant White fascination with exploring "individuality" and "freedom," which in turn could be used as a powerful ideological and "psychological" lubricant to keep the American liberal corporate machine in fluid motion and seem, at its liberal best, multiculturally meaningful, inclusive, therapeutic, and relatively receptive to non-conformity. (Pfister 2004:227)

In other words, although the two approaches are seemingly oppositional, they both do the work of supporting the state and maintaining existing class relations. Pratt's Carlisle approach was through "worker-izing" "Indians," as Pfister calls it—transforming Native Americans into a form appropriate to the developing American industrial economy, and Collier's approach also produced a compliant citizenry believing their country to be a land of freedom and equality for all, and a place inclusive of difference (in spite of evidence to the contrary). "Indians"—indeed all (or almost all) minority groups—become therapeutic for American society in this way. Both processes are individualizing, argues Pfister: "In the nineteenth century 'worker-citizens' were motivated through discourses of self-making and character building. In the twentieth century discourses of both individuality *and* 'ethnoindividuality' (those incorporating group difference, autonomy etc) structured incentive and promoted the appearance of democratic inclusion" (2004:245). The use of anthropology in the Indian Administration and in the Bureau of Sociological Research was informed by this "protomulticultural" ethic. Anthropologists were called upon to establish a knowledge base upon which culturally appropriate, effective ways of influencing the development of Native and Japanese American populations could be designed. Informed by this anthropological knowledge, administrators could draw upon existing values and perspectives in the formation of governing practices.

Moreover, this recognition, inclusion, and sanction of scientifically identified difference served to reinforce the very system that produced the demand for conformity.[10]

It is important to note that this may be so, but there are significant differences in outcome for Native people accruing from these two approaches. At Carlisle children were abused for attempting to retain important cultural characteristics; Collier, on the other hand, encouraged the development of some cultural forms (albeit, the ones he and his team of "experts" deemed appropriate). Under Collier some Native communities got back land that under the other regimes was expropriated. These are no small differences in the lives of real people. Feeding and teaching is undoubtedly better than fighting and killing. However, if this "feeding and teaching" is serving the purpose of promoting the *appearance* of democratic inclusion in the interest of the continuing reproduction of *exclusions*, then it needs to be brought under critical scrutiny. Collier's approach was arguably more benign than Pratt's, and it is clear that the man himself was a staunch supporter of Native American rights through decades of work with different organizations lobbying on their behalf. However, it is also clear that his agenda and philosophy was rooted in his own Euro-American perceptions and priorities.

Collier's vision of "self-governing" tribal communities has been critiqued from a number of angles as a continuing practice of imposing colonial forms on Native communities. Likewise, the enthusiastic fostering of "self-governing" "communities" within the camps surrounded by barbed wire and machine gun–carrying guards was clearly a practice in social control, not an exercise in democracy. I certainly don't want to conflate the two legacies. Undeniably, there were some ways in which tribal peoples were better off under Collier. But it is also important to recognize that both ways of thinking had negative consequences for Japanese and Native Americans. To simply understand one as bad and one as good doesn't interrogate the systemic, political, economic, and cultural context that gave rise to both types of personalities and approaches, and that ultimately strives to eradicate meaningful difference. We can, I think, appreciate the policies and particular decisions made by Collier that challenged the prevailing assumptions of the time while simultaneously critiquing the assumptions and aspirations that Collier held that were of complicit with the colonial imperative.[11]

I find Pfister's conceptualization "good to think with" in my study of the Bureau. In Poston, it is clear that the "protomulticultural" and assimilative objectives work together to create conditions of stability within a repressive regime. Discourses of democracy and citizenship proliferate in the Bureau archive, in stark contradiction to the conditions of the

archive's production, and to the imperative to assimilate and Americanize. The meaning of democracy becomes obscured within the confines of a concentration camp. As one ninth-grader in a Poston high school wrote, "I guess everything here is Democratic because we're in a U.S. Government camp." Classes in Japanese flower arranging and dance became opportunities for teaching "democratic principles." Some internees took Japanese language classes in which students translated *The Life of Benjamin Franklin*, and administrators eagerly encouraged the Buddhists to emulate the Christian churches' form in the establishment of service groups. Stories and histories of previous generations of migrations of ethnic minorities were brought into use as a framework or narrative structure within which the internees could incorporate their experiences in the camps. One of the Bureau staff wrote: "Dr. Leighton used the word 'therapeutic' in discussing a course in Minority Peoples of the World, curative of bitter, etc. attitudes which some Japanese here now have. The term took all around."

Not all identifications were understood to be therapeutic, however. In spite of, or perhaps because of, the geographical, administrative, and ideological overlap between the situation of internees and their Native American hosts, the Japanese Americans were explicitly discouraged from identifying with "Indians." "We're not going to be kept here forever like the Indians?" some worried to each other and to the administrators. No, the Japanese Americans were not to identify through commonalities of oppression and injustice. Rather, they were to identify with the American pioneers. Children in grades one through twelve learned about the struggles of the pioneers and were encouraged to identify their hardships at Poston with these early American stories. They made little covered wagons out of old chalk boxes. In ceremonies for groundbreaking and stone laying for the new schools, for graduation, for most any occasion, children performed pioneer scenes, telling stories of happy "Indians" helping hardworking and self-reliant pioneers bring progress and civilization to America.

Although the internees were discouraged from making these connections, the social scientists were deeply embedded in them and acknowledged them in their work. The (uniformly Caucasian) senior staff had almost all previously worked in Native American communities; many would return to that work after the war. As in their work in other communities, here too they studied their subjects, categorizing them into groups (Issei, nisei, kibei, loyal, disloyal) to facilitate their management, and learning how best to implement programs that would encourage the

Japanese Americans to govern themselves—to become appropriate citizen-subjects of the state and the developing economy.

But it was not only Japanese American citizen-subjects being produced in the camps. The social scientists were also doing their part to "keep the American liberal corporate machine in fluid motion" (Pfister 2004:227). In their active commitment to the power and authority of scientific method to overcome hardships and injustice, in their faith in progress, and their participation in a national effort to redeem the image of America and refute the racism that contributed to the removal, they too were being produced as appropriate citizen-subjects—their critiques were being contained, recuperated, by the American dream.

Orin Starn quotes Robert Redfield's reflections on the internment: "It is a strikingly un-American thing we have done. It may be added that we have done it in a strikingly American way" (1986:708). When the internment is understood in the context of other exclusions in American history, and the role of social science is understood within this context as well, it appears that internment can be seen as a very American thing to do.

> All shared with that earlier generation of reformers a zeal to cleanse, purify, and perfect industrial America by imposing an orderly existence on backward farmers and inefficient workers; on "new" immigrants just off the boat; on tramps and paupers; on criminals, lunatics, and idiots; on loose women and prostitutes; on Native Americans, Afro Americans, and Chicanos; later on Puerto Ricans and Asian Americans—on everybody. At bottom the protean term *Progressivism* meant reform from the top down through repression—the subjection of individuals, families, and kinship societies to the organized discipline of centralized power. Militaristic abroad and therapeutic at home Progressives marched onward and upward to control everything through a vastly expanded state. They were driven forward and sustained by their limitless faith in the benevolence of this Leviathan and in its power to "Do Good" when in the hands of "scientific" experts such as themselves. (Drinnon 1987:267–268)

It is abundantly clear from the internment literature that social science has been used extensively as a technology of governance. The work of Alexander Leighton perhaps most explicitly embodies this role, and his writing clearly articulates his aspirations for the use of social science in this context: "Even in the occupation of an enemy territory, the chief aim of the agents of a democracy will not be to out-Fascist the Fascists, but,

after the necessary housecleaning, to rehabilitate the country by meeting the basic needs of the people and encouraging forms of self-rule which will lead to a peace that is more than an enforced interlude" (Leighton et al. 1943:653).

Social scientific thought and practice was and is a product of its own cultural, political, and economic context and history and therefore, unsurprisingly, reproduced and promoted the values, forms, and forces of its own society's growth and reproduction. Liberal anthropology was a part of that system, a part of "the American problem." And yet, surely the answer is not to abandon all attempts at creating a better society. Although from a contemporary standpoint internment anthropology may be judged as complicit with the repressive objectives of state and capital, these were, for the most part, people who were strongly committed to anti-racism and wanted to help make bad situations better, people who sought to improve America, perhaps in many ways, people like us. Social scientists are, as they have been in the past, eager to find ways to alleviate the suffering of individuals and address the inequalities that groups continue to endure. Understanding the past practices of the discipline is a necessary step in the development of a social science that is politically engaged and yet vigilantly critical of its own participation in a system of liberal governance that at once produces and limits the freedoms it celebrates.

Notes

1. Toshio Yatsushiro, Iwao Ishino, and Yoshiharu Matsumoto. 1944. The Japanese American Looks at Resettlement. The Public Opinion Quarterly 8(2):188–201. See Feeley 1998; Simpson 2001; Starn 1986; Suzuki 1980, 1981, 1986.

2. An example of a sort of discussion of the major project he spearheaded can be found in Collier and Thompson 1946.

3. Dillon Myer, in fact, replaced John Collier as head of the Indian Office shortly after the end of the war and was at the helm during the era of Indian policy known as "Termination," when Indian policy reverted to a more overt practice of divesting Tribes of lands and what little local powers they had.

4. Peter Suzuki suggests that this book was published in refutation of the work of another JERS researcher, Morton Grodzins, whose 1949 publication *Americans Betrayed: Politics and the Japanese Evacuation* condemned the role of prominent Californian politicians in the decision to remove and disperse the Japanese Americans (Suzuki 1989).

5. Leighton also had experience working with Native American communities. In 1944 he and his wife, Dorothea Leighton, co-authored *The Navaho Door: An Introduction to Navaho Life*. Interestingly, John Collier wrote the introduction to this volume.

6. Issei refers to first-generation Japanese Americans, nisei to second generation, and kibei to nisei (usually men) educated in Japan.

7. "It appears, in fact, that the Progressive Era was perhaps the era of social control par

excellence. During this period there was the development of the mental hygiene movement; the birth control movement began; Prohibition and the Harrison Narcotics Act were passed at about this time; food and drug legislation of all sorts were passed; major changes in medical education were brought about by the Flexner Report; city planning and the conservation movement began; and so on" (Kunitz 1971:224).

8. See, for example, *Indians in the Americas: the Long Hope* (1947), and *From Every Zenith: A Memoir and Some Essays on Life and Thought* (1963).

9. See, for example, Barsh 1991a, 1991b; Biolsi 1991; Kelly 1983; Philip 1977; and Rusco 1991.

10. For a similar critical reading of contemporary multiculturalism, see Bannerji 2000 and Chow 2002.

11. Similarly, it is important to recognize the different outcomes for Native Americans and Japanese Americans. Statistics indicate that Japanese Americans now are overwhelmingly part of the middle or even upper-middle class. They are "successful" and do not live with the poverty and related social and health problems that plague Native American and Canadian communities. Some see this as evidence of the "success" of the assimilation policies of internment and dispersal. Others, however, understand that the social and cultural costs of assimilation have been high.

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12. Minorities in American Anthropology

A Personal View

David J. Banks

America's historical minorities have been largely excluded from mainstream anthropology since the 1960s, and I will present my own experiences from the University of Chicago and elsewhere to help understand why. Let me begin by defining historical minority here in a narrow way, as including only those people of African American, Native American, Latino, and Pacific Islander groups who attended high school in the United States. There were also members of non-white groups from other regions (Africa, Asia, and the Caribbean) and recent immigrants studying anthropology at Chicago, but members of these categories were and continue to be treated differently than members of the traditional minority groups. They were usually thought to have clearer goal direction, although they were considered less idealistic.[1]

I entered anthropology to find out about my own cultural background, which I knew to be very different from the culture in the white American mainstream. I also thought that anthropology could be helpful to others in my own group who had long suffered from discrimination, and to other Americans, by providing new group and personal alternatives. I knew there was little place for independent black intellectualism in America. Blacks had to consider themselves as part of some pressing issue for white America—say racism and its heritage—to be successful. I also, like many black people my age, wanted to expand my horizons by going abroad. Understanding our differing imaginations would provide a better basis for authentic contacts between members of the groups in our melting pot. Unlike St. Clair Drake, who studied the Chicago black community, I looked forward to doing fieldwork in a foreign land that had evolved differently than the United States, and wanted to look for analogous personal experiences to my own abroad. This search for analogies far from home was certainly a reason for my choice to study Malay kinship using the Malay language as virtually my only medium of communication. Let me repeat, I am presenting my own points of view and experiences as an African American anthropologist.

My Background in Anthropology before Chicago

The anthropology that I brought with me to Chicago from Brooklyn College had emphasized the Boasian outlook, with its distrust of using external theories to understand the complexity of the cultural worlds studied. I had taken courses with Joseph Jablow, Solomon Miller, Robert Ehrich, and Gerald Henderson, and received help from many others in the New York City anthropological community (Colin Turnbull, Alexander Lesser, Margaret Mead, and Hortense Powdermaker, to name a few). My teachers thought that I had the kind of questioning mind that would have a place in anthropology more than in other social science disciplines, which they considered stagnant in comparison. Malcolm X was another New Yorker who strongly encouraged me to continue in anthropology. I met him first after hearing his debates with black intellectuals on the radio, and then at Brooklyn College when we invited him to address an NAACP audience. After that, Minister Shabazz and I kept running into each other around town and talking about cultural topics. He needed help with several of his speaking projects and used students like me, who shared his interest in liberation movements everywhere, to gather data for him. He urged internationalism and learning about the world beyond Europe. Malcolm X was working on a solution to the problem of racism but never finished his work. His influence on my anthropology, like that of other prominent people that I met in New York, was indirect but definite. I met fiercely independent jazz musicians in my neighborhood. These musicians had appeared on cherished LP recordings. For me good anthropology approached jazz musicianship in its structured creativity and spirituality. My civil rights activities included arranging a jazz benefit at NYU at a time that seemed make or break for the cause.[2]

The possibilities of the Boasian outlook go far beyond what Mead and Benedict had used it to create. Boas saw a world in which many studies of a setting could be carried out fruitfully, each with its own methodology of observation. Later Thomas Kuhn (1962) suggested to anthropologists that they think of the field as a constant search for new paradigms, any one of which might lead to a scientific revolution that could change its strategies of data collection; what might seem less valuable data can become essential. Perhaps most importantly, Boas's historical particularism remained aloof from Western ethnocentrism. He advocated getting out there and finding out what the average man thinks and feels (see Boas 1940). There have been many critical evaluations of Boas's style and his problems with members of the groups he studied, but the possibilities that his culture-centered studies, focusing on non-Western peoples with-

out dogmas and parading themselves as science, made Boas's legacy an honored field, and one that seemed to encourage strong contributions from minorities.

Sixties Anthropology at Chicago

The growth of anthropology through fellowships and grant support during the sixties was part of a Vietnam War–era Johnson administration plan to study and confront the achievements of non-Western peoples through better understanding. This would justify our mission to save Southeast Asia from communism. The Johnson planners' image of anthropology was as a humanistic discipline with a group of changing theories, none dominant. I was entering a field that the president referred to in positive ways. This was the plan, but the war ended and the people who shared this view of anthropology's potential lost power. Scholars like Clifford Geertz, and many others in the profession, were on a mission to take back anthropology from the Boasians, who were regarded as anti-theoretical and intellectually weak. Boas's approach suggested that anthropology's issues would be generated at the grass roots level. In Geertz's view Western intellectual issues should chart anthropology's direction, not the imponderables of foreign cultures and their potential scholarly concerns.[3]

Kinship studies interested me because kinship theories dealt with how people make interpersonal moral choices in distant settings. Malinowski's focus upon human agency was especially interesting from this perspective. I was not as interested in top down social structural theories relevant to public administration, important in some Oxford and Cambridge kinship theories. Kroeber and Lowie had placed kinship studies in much wider cultural contexts but Chicago—save for Fred Eggan—tilted in a more Anglo-European direction. Eggan remained steadfastly loyal to the Boasian heritage of ethnology even though he had studied with A. R. Radcliffe-Brown and mentioned their intellectual relationship and friendship frequently in his lectures. Eggan's kinship studies were a branch of cultural and social history.

Disenchantment with the freewheeling studies that earlier American anthropology had encouraged became for some Chicago faculty what Bernard Cohn, one of my professors and an advisor, called the "hate the natives" movement, a turn that was accompanied by an acceptance of the dominant role of power in social relations. They adopted an idea popularized by E. R. Leach, who presented the clearest form of it in the early pages of his *Political Systems of Highland Burma*: "the conscious

or unconscious wish to gain power is a very general motive in human affairs. Accordingly I assume that individuals faced with a choice of action will commonly use such choice so as to gain power" (1954:10). Leach's idea became the kernel of a growing ideology surrounding power as a universal force shaping all cultures and even perceptions of reality. Anthropologists often labeled the political administrations of Asian and African countries corrupt and incompetent. The peoples that they administered, including their intellectuals, were seen as tools of the forces above them. A possible alternative view, the UN's Universal Declaration of Human Rights, is deeply cultural in its tone. Although the Declaration does not use the word culture, this document expresses what it hopes can be shown to be universal human ideals. But the anthropologists at Chicago rarely mentioned it or considered its possible usefulness in their work. This Declaration could certainly have provided a framework for field observations.

As a result of this identification with formerly imperial Britain's intellectual heritage, and later with that of formerly imperial France, modern anthropology was becoming a kind of rural political science. Drawing upon discourse analysis and literary criticism, it urged us to consider political thoughts and feelings, the political culture, and to emphasize potential and real conflicts. The reason that these tendencies developed as quickly as they did reflects the origin of many of the students at Chicago. Many were graduates of private colleges where the roll-up-your-sleeves empiricist Boasian style was far weaker than the clean-fingernail Western humanities tradition and its styles. Peoples beyond their culture were to be a testing ground for their theories, not a potential model for cultural rebirth. A colleague laughs that the profession was being made over in the image of educated white kids from suburbia. These were people whom no one would ask for advice or ideas about anything in a crisis. They rarely did conventional fieldwork—that is, showed up in their village, got housing there and lived alone, without transport, through fluent use of the local language. They arrived in style in Land Rovers with their wives along and talked about the economic bottom line and corrupt local administrators while drinking their coffee and eating their cake. In fact, when there had been colonial administrators before them, informants asked them for intervention and help.

The professors that I interacted with had various approaches to anthropological studies. There was really no "Chicago school," although the currents described above were in the air. David Schneider had convinced me to come to Chicago when I was seriously considering going to Harvard or UCLA, both of which had made generous offers. He told

me I could learn there and go my own way. He said he was doing what he called culture categories research, using ideas and distinctions from within the cultures studied to help revise naïve and ethnocentric theories of kinship. This was a very appealing starting point for a student with my background. Schneider also told me that there were others there who would woo me but that he urged independence. I think he was referring to Fred Eggan and Bernard (Barney) Cohn. Eggan studied kinship systems and trained several non-white anthropologists. He had a way with us interpersonally. Barney was easy to talk to and always actively engaged in the intellectual worlds around him. He was also from Brooklyn. Schneider was my advisor at Chicago at the beginning, but I became less certain toward the end since he hardly discussed my thesis with me and did not attend its defense.

Chicago Faculty

I got to know David Schneider as his advisee for two academic years. Schneider had strong individual relationships with students and this included a discussion of his own ethnicity. He constantly bemoaned his German background. Several sources have now noted that his ancestry was European Jewish.[4] He would not speak German with me although he knew that my grandmother had spoken to me mainly in German. He knew Weber and Durkheim adequately and also was good teaching Parsons, his old professor. I liked Parsons's approach to nineteenth-century sociology, which Schneider showed me how to learn. Schneider avoided the legacy of Boas. Boas was to him a dreamer and visionary. Schneider was convinced that new ideas always are modifications of prior ones and emerge through clever critique. Many of his likes and dislikes seemed personally motivated (cf. Schneider 1965). Despite his military service he considered the U.S. diplomatic effort to be Central Intelligence Agency–dominated, and I quickly learned to disregard his advice that one should avoid American diplomats at all cost. They were an important positive influence for Americans traveling in Asia and were acutely sensitive to the bad treatment that minorities often receive in academia. They wanted to prove to locals that we United States citizens are part of one family, and gave us front row seats. We blacks often consider this a start in the right direction, not hypocrisy.

Schneider, for all his attempts to be a creative contributor, was a reactive scholar, whose influential works were critical essays. His major idea, that there is a dialogue between nature and culture permeating all peoples, came from Lévi-Strauss's early efforts. Ironically, Lévi-Strauss told

me, while I chauffeured him around for a couple of days during his visit to Chicago in 1967, that he did not recognize this idea in the form Schneider gave it. Lévi-Strauss considered Boas a kind of godfather for his work. (Lévi-Strauss was very interested in music, his avocation, and encouraged my own efforts in this area. He even discussed piano playing as a source of great sanity in this world. He loved Mozart and Haydn, as had my father.) He was a wonderful man to talk with, the kind of person that one can meet once and find an inspiration for a lifetime. After I met him I struggled through several of his *Mythologiques* in French (cf. 1964). They struck me as insightful, and as leaving many loose ends for others to resolve. His concluding essays in each volume are marvelously nuanced. Ultimately, however, his structuralism was arid and, although suggestive in many ways, did not suit me.

Schneider's personal commitments fascinated me. He thought that California college girls were the hope of the nation and talked about them when the conversation got around to what he admired. When he asked me about surfers and I yawned, he was aghast. He probably had a Beach Boys collection, I speculated. His worship of the American WASP–West Coast pattern resembled the fitness-to-rule complex of Caribbean peoples in their identification with aspects of their British heritage and its dialect and tastes, a heritage that they knew would gain them credibility in an academia that feared and still fears local minority intellectuals. He assumed that the American spirit was good even while conceding that there weren't many good Americans over thirty. After he moved from Chicago to Santa Cruz his colleagues asked me, at various conferences after they had discovered that I had been his advisee, how a forthright black person like myself hadn't put him straight on a huge number of things. They said he exercised a major voice while close to retirement. My response was that ours was one of those complex relationships and we were moving in different directions and parted company amicably.

Schneider's nature and culture view of kinship replaced culture categories and offered no useful methodology to African Americans. His attitudes were a kind of ethnic hero worship, a set of attitudes about America that were surely not shared, at least not openly, by others on the Chicago faculty. He also understood that blacks had different goals and aspirations, but thought that these were probably ideas of the oppressed that would quickly vanish when they got to his California heaven. Describing a performance by John Coltrane, the tenor saxophonist, that he attended in Chicago, he commented on its anger and dismissed my hearing of its spirituality.[5] I had just returned from California when I entered Chicago in 1965 and had never seen a place where the races associated

less and condemned each other so openly (this was just after the 1964 Olympics in Los Angeles and the Black Panther activism that shortly followed). Schneider had a deep and confident voice and air and loved to talk about his days on the Santa Cruz coast in the military as an armed guard.

Perhaps his most significant contribution to the Monday afternoon seminars, which Chicago held to introduce visiting speakers, was frequently expressing the opinion that the politics of the profession always takes precedence over truth. This explained to me his unwillingness to respect applied anthropology or any other humanistic trends. Anthropology was for Schneider becoming the study of finding truth through politics and power. He was unwilling to listen when others told him that his ideas would set conflicts in motion that would hurt the weak and the poor just as had the alleged social sciences of Germany and Russia. He enjoyed the support of Ortner (1972, 1984) and others who interpreted his nature and culture distinction in terms of conflict between the sexes. Schneider continually reminded me that women only listen to power. He had a right to think and say that, I suppose, but this was only shadowboxing. All over academia there was an assumption that women were poised to advance. Scholars making alliances with feminists were not warriors in a battle with risks; they were cashing in their chips at the bank. Later this "power is truth" group of feminists and others led Schneider's dedication night at the AAA—an all-white podium.

Schneider would not receive my criticism that there was a danger of excessive commitment to any technological development. He was extremely positive on the suburban way of life despite its spreading pollution problems. His anthropology was not to be directly critical of central tendencies of white America. I was already wondering how long the West could use its technology to bully a position of leadership for itself. Schneider also did not question basic ethnic loyalties. His use of in-group language dialect with Jewish acquaintances was understandably related to the continuing oppression of Jews in academia. But use of in-group language does not negate oppression or create equality. Lévi-Strauss once asked me which members of the Chicago faculty were and were not Jewish. Shocked by his question, I answered that I did not always know. Lévi-Strauss then said, "these styles are impossible for me, I am quite French."

Schneider's model for the Jews seemed utterly inapplicable for minorities and explains his search for immigrants of color to compensate for his flight from American historical minorities. His inability to face the aspirations of our historical minorities was usual in the sixties and seventies.

His scholarly work offered no methodology that included the wealth of ideas held by non-whites on social relationships. Instead he argued that kinship was another false religion (Schneider 1984) that has many forms and is power based.

My comments on Schneider should not leave the impression that we did not have an interesting relationship. He was a very thorough critic of British social anthropology and helped make the French structuralists and other European scholarly schools more acceptable in the United States (Schneider 1964). He was interested in people's ability to make numerous kinship distinctions, but he did not approve of the potent approach of Lounsbury and Scheffler (see Scheffler and Lounsbury 1971) that makes easy sense of the ways that Brazilians tell us that they choose their terms for kin. One of his most useful methodological pieces, "What Should Be Included in a Vocabulary of Kinship Terms?" (1970), grew out of discussions that I had with him during a reading course in my second year, and the "vocabulary of kinship terms" was my suggestion in those discussions; a suggestion I made a basis of my own research. The article does not mention my contribution. He paid for my trip to a conference on kinship theories in the South Seas at Santa Cruz, where he noticed that I was more interested in the ideas of the participants than in the weather or the women. Santa Cruz was his heaven. For me it was just a place, misty and damp; nearby Fort Ord was just as interesting, and far more integrated (these were the days of Vietnam). It is amazing how much credit Schneider got by criticizing the creative efforts of others. Sherwood Lingenfelter (1985) has harshly criticized Schneider's Yap ethnographic material; perhaps there were regional variants of the same attitudes at play there.[6]

Clifford Geertz was a top-down thinker in virtually every way. We became acquainted through my serving briefly as his teaching assistant in late 1966. He wanted students to follow his lead, at the beginning at least; his research suggested that most ideas, values, and innovations filter down from the elite levels of society to the peasants and the workers, and these processes were analogous to his model of academia. Redfield's Great and Little Traditions (Redfield 1960; Geertz 1960) were not separate in his eyes. Geertz's Great Tradition was the prime mover whether it was *prijaji* aristocrats or settled *santri* Muslim traders directing peasant consciousness from above. The *abangan* peasant tradition, with its solidarity ritual, the *selamatan*, was the recipient (see Geertz 1960). Strangely, the approbation that he found outside the department among students, who filled his classes from all areas of the University, was far more restrained among Chicago's anthropology graduate students, who

were overawed by his strong scholarly credentials. He did not create a theoretical or methodological school of his own, and soon moved far beyond his initial concerns.

Geertz's study of Moroccan Islam centered on the kingship. Muslims in Morocco and Java patterned their faith on the royal culture; Geertz saw little room for popular movements affecting royal patterns. In Bali, his next major fieldwork, kingship had a similar role (see 1968, 1980; comp. Barth 1993). Early on, my readings found scholars, often from Indonesia, who distrusted Geertz's ideas about this unidirectional pattern of culture change. These doubts came together in the well-annotated work of Mark Woodward (1989), who shows that as in most areas of the Muslim world, Sufism is a system with appeal among peasants and rural tribesman that nobility have to be quick to accommodate (see Levtzion and Fisher 1986). Islam's appeal to universalistic human values of equality before God and brother- and sisterhood were a driving force among Muslims that has even spread to non-Muslims apart from any political system, as has the veneration of the prophet Muhammad (see Schimmel 1975, 1985). Even James Scott's (1976, 1985) discussions around the same time of peasant moral economy and weapons of the weak tend to look at peasant behaviors as responses or resistance, rather than as at the creative forefront of change. It is certainly easier to collect data from peasants about how they deal with their elite than to attend social action gatherings in mosques, prayer halls, and impromptu gatherings, but I am surprised at how few colleagues actually attend this kind of gathering. Some have studied Muslims without ever attending prayers or listening to the related speech at Friday communal prayers—although anyone, Muslim or not, may attend these gatherings in most localities.

Sidestepping issues of peasant revolution and the Marxist intellectual camp, in the 1960s and 1970s discussions about Southeast Asian cultures hinged on the manipulation of the peasantry by distinct elites. Nationalisms became manipulative inventions and reinventions by these elites (see Anderson 1983; Hobsbawm and Ranger 1983). The extent to which anthropologists identified with Asian elites by studying them and their more or less successful cooptation of the poor is startling since fieldwork had long been the life blood of the field, but times were changing. The same emphasis on political leaders in recent terrorism, whether in Ireland, Israel, or Iraq, has turned out to be a hollow exercise, since terrorist acts seem to emerge spontaneously from the grass roots. Leadership elites cannot turn them on or off.

If Schneider was the outsider boring his way into the mainstream, Geertz wanted to create an anthropology that could be influential with

policy makers. But his goals were not always clear. He found one after another reason to impugn local traditional elites and to consider their behavior similar to past European patterns. Compare the scholarship on Bali of John Stephen Lansing (1974, 1995) with the theater-state paradigm offered by Geertz. One is top-down and the other bottom-up. Lansing helps the Balinese use their calendar system to solve problems of drought. Lansing's work is egalitarian in that the two peoples put their heads together and come up with a solution to a problem for both of them, an intellectual and humanitarian one for Lansing and a matter of survival for the Balinese involved in his project. His first monograph about the Balinese conceptions of evil (1974) is fascinating as is his summary monograph, *The Balinese* (1995). Geertz's images of Bali are often fanciful. They are more than "romantic," as Boon (1977) suggests. Having observed a Balinese cockfight and a full funeral with cremation, I would have handled the write-up quite differently, even though I concede that these events are memorable and transfixing. The Balinese were extremely kind to me, perhaps because my phenotype suggests the slave trade, of which, like my own group, they were victims for centuries (Reid 1988:129–146, esp. 133; Sullivan 1982). Barth (1993) and his wife, Umi Wikan (1990), brought some ground-level specificity to discussions of Balinese ceremonialism and present a more detailed treatment of the relationship of ritual to daily life there, and are a necessary supplement to the Geertz works on Bali.

I recall on one of my trips to Bali (this one in the 1980s, with my family) getting in a taxi with the family and talking to the cabbie in Indonesian. Once he determined that I was more fluent than my wife (whom he called "Mem" and "Vrou") he began telling amusing stories, as often happens with cabbies everywhere. I asked him about Kuta Beach, the place popular with nude white bathers and the site of the recent terrorist bombings. He told me "Brother, stay away. There's trouble brewing there." The driver agreed to take me to the stone ruins near the beach but not to the beach itself. Whites had already worn out their welcome there, but the locals could not expel them because their friends were making a good living exploiting the "girls have sex more readily on vacation" syndrome that we all should urge our daughters to avoid. Colin Turnbull had told me about these attitudes and even written about them, but Europeans and Americans often have had trouble understanding that they have worn out their welcome long ago in these cultural wonderlands. As an African American, I have usually been regarded abroad as part of an inchoate nation that is somehow not fully responsible for the attitudes and behaviors of Americans abroad.

Tearful memories of enslavement, passed down orally, deeply penetrate the consciousness of the peoples that were its victims centuries ago. In Southeast Asia, they often do not deny or attempt to deny this unfortunate aspect of their pasts. African Americans are often surprised by this and will even ask if they should or can date and get serious with Southeast Asian women. The slave trade across the Pacific to Africa and within Asia has provided a medium for equality and intermarriage. This is something that several years' residence in Asia teaches. I gave a grandfather in Sabah a Paul Robeson CD recently and he cried as he put it on. Once I suggested to a Sabah resident that the Orang Suluk and Bajau (trading peoples living in boats) were black because of a New Guinea strain. They laughed. That and Africa, they said.

But Geertz was and is a transfixing lecturer and a wonderful discussant of issues in private. He is able to see social issues from sides that students do not, and even when his writings did not reflect it I became convinced that he hoped for a better world for tomorrow and thought that the American middle class had to be moved there in stages. I always sought out his ideas, in articles and books I collected and often chuckled about. In his many articles he often concedes that the best anthropology consists of fieldwork at the grass-roots level. I doubt that he is amused by one after another top-down approach by fieldworkers who use interpreters and do not spend the usual half year or so fumbling around before reaping the reward of speaking the local language. In our final contacts before his death, Geertz said that he was aware of and troubled by the problems raised here. He was preparing an essay on diversity to address them.

Fred Eggan was an important part of my intellectual development at Chicago. Schneider considered Eggan old-fashioned and uncreative. As much as that may have been true from Schneider's perspective, I remember Eggan's great tolerance for innovation even though he was hard to convince. He taught us to be clear about our data and methodology. He insisted that we tell what we were not collecting as well as what we were collecting, and urged us to conclude papers with a question for further thought and research, beyond the paper itself. We should criticize ourselves brutally, as Eggan often did our papers and in completing his own published work. No wonder Eggan is so frequently mentioned in the new world of ethnic studies departments and programs: he did not exile himself, and he was acutely aware of the potential of anthropology to be a weapon of domination but had a faith in empirical research to interpret and reinterpret evidence of the senses. Eggan loved people, all of them, and had much the feeling of an orchestra leader or a general who happily sees disparate groups, men and women, working together well.

I met several other professors at Chicago who influenced my later work. Melford Spiro was interested in applying psychiatry to ethnographic materials. He showed me that since people are all one species their behavioral forms have inevitable consequences for human feelings and impulses. I have thought that finding some patterns of positive affect—that is, cooperation and protectiveness—among non-human species is a logical extension of his work. Spiro's work on Burma, although not exciting, was stimulating, as was his rewriting of the Trobriand material (1967, 1970, 1982). Paul Friedrich was another positive influence. He used a variety of historical and linguistic methods to wring new insights out of old sources. He loved methodological consistency and candid admissions of the difficulty of proceeding within one's own guidelines. I particularly have enjoyed his classic *The Meaning of Aphrodite* (1978).

Manning Nash and I did not get along. We squabbled after he called a Chinese-Malaysian girl whom he did not know impossibly vulgar names in Alor Setar, Kedah, and I had to apologize for the University of Chicago, even going to the Chief Kathi (religious official), whom I had known when he was a local *kathi* in Sik, where I did kinship fieldwork. I never forgot Nash's crude feeling that everything in Malaysia was inferior to the West. Perhaps he shared Schneider's hero worship of WASP culture, but there are some things one never does. Nash considered Islam crude Judaism for nomads. Non-subservient blacks were always angry in his view. I wonder what he would have responded had I shown him a recording of his unprovoked outburst in Alor Setar. Besides these personal issues I always found Nash's writings simplistic, although sometimes amusing.

One of the pleasures of studying at a great university is meeting scholars informally. This is how I met Milton Friedman, the economic historian from Brooklyn. Staff of the department introduced me to him as someone looking for a tutor in economic theory. Instead of recommending someone else, Professor Friedman asked me why I wanted to understand economics and I told him about my disappointing experience at Brooklyn College.[7] He assigned me economic history texts to read and said that his father, like mine, also had trouble with doing honest business. I felt forever strengthened and continued with his reading list years later. His influence with governments must have been through his ability to make complicated specialized jargon approachable by common people and leaders. He was a wonderful teacher who didn't waste a minute with trivia and concentrated upon the crux of an economic matter. He made his political choices obvious without discussing them. There was a spiritual side to the man that endeared him to me.

I also met and had a pleasant relationship with Elizabeth (G. E. M.) Anscombe, who was then a research fellow at Oxford on leave at Chicago before her election to a Cambridge philosophy professorship in 1970. She and I would match wits at her apartment amidst her children on topics related to her interest in justifications for faith in God. She was a Catholic and had quite a few children despite living apart from her husband, Peter Geach, another prominent philosopher. She liked to smoke cigars and drink small amounts of strong liquor. Elizabeth taught me that even the simplest experiences are related to many different patterns of thought, and that interviewing people will help us to understand these complex interrelationships, but our informants' ideas should not become a utopian place for us to hide. She thought that using power or sexual motives to help us to understand behavior is a self-delusion that analytical philosophy could readily expose. We must ask what underlying ideas we have about who we are and where we are going, and question these using the analytical method. She hoped that I would leave Chicago and take a degree in Oxford anthropology. Friends in New York thought that I would never figure the British scholarly scene out, but Elizabeth laughed that this was an advantage since people would have to tell me what they meant and their fundamental premises. There would be no assumptions. She also introduced me to her daughter my own age, a nurse practitioner, whom I later saw socially in London. Elizabeth was offended by racial violations of human rights and thought that people should live together amicably despite superficial differences like race and gender. She taught me to question myself and my ideas, as had Eggan. She liked to take me to dinner at the faculty club that, she said, had not previously admitted blacks.

Power, Truth, and Gender

The sixties was the decade when politics became a controlling model for social anthropology, replacing social structure. Life was seen as a struggle for power. Most people have little idea how extreme the political determinism of many modern anthropologists can be. I learned more about this while taking a summer seminar—Modern Asian Literatures in Translation—at the University of Michigan in 1992. In the view of textual analysts and their postmodern, anthropological followers, thought is only significant as political text; politics becomes a synonym for reality. For these scholars, other ways of considering observed materials are unscientific, backward, and useless. All cultural forms are only considered when reduced to text. There are no values, only poles in a constantly changing

set of processes of conflict revealed through textual analyses. These ideas developed from the rather more empirical ethnographic structuralism of Lévi-Strauss, followed by various post-structural and postmodern trends that include the ethnographer in the same matrix of power and conflict as the informant. In my view, studying power relations as the essential feature of social and cultural order has the effect of dignifying power, the weapon of the strong against the weak. Anthropology then becomes a broker defining real and potential conflicts; its information and analyses encourage those who would exploit conflict such as the world arms industry—power brokers are usual villains in influential Southeast Asian literature (Banks 1987, 2006).

Anthropology's debates after the 1960s no longer honored the special place of the informant, her feelings, and her complex predispositions to act, but instead looked—either from textual materials or other structures—for the most powerful hegemonic and global forces controlling the relations among observed people and between observer and observed. Anthropological studies showed that local oppression reflected global practices. Foreigners, including anthropologists, are merely border crossers in a global matrix of power. In public debates, power as a dimension of all social relationships is often confused with the political order. It is no wonder that both globalization and postmodernism have become such controversial and despised (they would say contested) concepts everywhere but the West. With globalization Westerners are equal with foreigners everywhere; national loyalties are either fantasies or tactics to ensnare the poor in their matrices of power.

The patterns that I describe coincide in time with the emergence of academic feminism as a major force shaping anthropology, and the common feminism of white Americans fit the same mold. Their assumption that physical sexuality is largely a competitive political struggle, found in the books of Helen Fisher (1992, 1999) for example, does not have a goal but rather adopts the capitalist model that competition works. Power was the overt and covert agenda of the emerging feminism, not making better or happier individuals, families, or homes. This privileging of power consequently downplays cooperation, compatibility, romance, and other such motivations—romantics were and still are frowned upon in feminist circles—in favor of competition and conflict. I guess this is why so many feminists had histories of cozying up to the powerful and challenging the weak. Western feminist claims to leadership in the struggle for gender equality have made them hated abroad in many places, and minority intellectuals have usually distanced themselves from this brand of feminism.[8]

American Anthropology versus the World

When a student of mine, ignoring my advice, attempted to find out whether ritual payments in Sumatra really did constitute a weapon that preserved patronage relationships and strengthened them as the postmodernist theories of the time suggested, villagers held a meeting and decided to poison him, we later learned. I had spent a considerable time trying to convince the student before he went to the field that he would be in danger if he tried to apply this methodology. Other anthropologists have been shot, beaten, etc., for similar misguided projects. Countries have begun expelling and refusing re-entry to anthropologists at the behest of the peoples studied. These governments can argue that it is their duty under the UN human rights charter to respect the wishes of their constituent peoples. Anthropologists are shocked to find that people they have considered friends abroad are often waiting for someone to talk to—about them. I have been approached several times in Malaysia. I am sometimes asked whether the white anthropologist treats me any better than she or he treats them. These commentators rarely think that I will be surprised at their comments anymore. After all, they have gotten used to what is for them the unemotional style of Western backbiting, forbidden in Islam. My pain at home is their pain abroad. The issue here cuts to the heart of the interaction patterns between peoples and the kinds of scholarly interactions that are acceptable and honorable.

There was no model during the sixties and seventies of a worldwide anthropology emerging that ensured an active presence for weaker nonsponsored peoples in the field. Sol Tax's vision of a democratic and worldwide anthropological discussion was looked at with skepticism, even though it reached its height at the 1973 International Congress of Anthropological and Ethnological Sciences held in Chicago (Stocking 2000). Schneider commented that the Congress would be a zoo that he had no intention of attending. Tax was concerned with world anthropological participation as well as recording of the world's vanishing cultural heritages. Anthropology was rapidly defining itself as a post-colonial discipline that did not take nationalism or national boundaries as basic building blocks. White American intellectual nationalism had exercised a subtle domination through its emphasis upon the dynamics of power as a necessary force, explaining interaction patterns in field situations—and perhaps, more subtly, explaining their own acquiescence in American arms sales abroad.

Anthropological theories seldom honored the concept of a people building up rights in a land, and when they did they tended to limit it to

aboriginal peoples. Such a conception would require representation for all of a nation's peoples in all scholarly areas. Without such a conception exchanges between scholars across national borders would become dangerous, as Western conceptions of postmodernism and globalization denigrated governments in other parts of the world. Previously, a scholar traveling abroad could be seen as part of an exchange, but true exchanges became increasingly clouded and one-directional.

I feel that minority anthropologists at Chicago and other departments did have an impact on the development of anthropology after the seventies. This impact, however, was largely outside the mainstream since their work suggested very different directions from that of the white majority. Once black anthropologists had rejected the top-down study of power as the central contribution of anthropology to the study of primitive, peasant and modern peoples, the white center developed elaborate defenses of that position. These defenses allowed for a non-racist but nonetheless all-white anthropology, and led to the ironic conclusion that the least culture-bound of the social sciences could exclude on the grounds of cultural difference, in the guise of quality; the ruling cliques in journals rejected articles as not germane or poorly supported. Schneider and Geertz knew early of this minority rejection of the power model, and nevertheless began its implementation. For Schneider there was no kinship, even though some idea like it is easily translated into most languages. Why refine it or confront its cultural nuances? After all, they—"the natives" as he called them—are often deluded. The divorce from the historical minorities, who did not think or act at all like the white anthropologists' immigrant forefathers, continues. These minorities believed—and believe—that their ideas should receive recognition and support even though they are not part of the white mainstream.

Many forces contributed to this outcome. The mainstream studies were easy to carry out and involved less empathy with local moral attitudes and values. Scholars were largely secularists who accepted the Newtonian boundary between body and mind, and their approaches tended to ignore studies of "how culture works" that attempted to include corporeal and biological dimensions.[9] Aesthetic areas, central to many people's self-definition, were hardly approached apart from their power dimension, although they are rich in potential for model building. For example, African American fascination with the middle of a performance, in jazz and blues, and not its beginning and ending, provide a clear contrast with singing or playing closed songs as in country and "pop" music. This study could lead one toward the analogues of African American music in

Sufi chanting, etc. and even in ways of approaching social situations and moral dilemmas.

The major challenges that anthropology faces continue to involve the ability to present an outsider's perspective on a culture while honoring those cultures by allowing their intellectuals to participate in the world movement of cultural anthropology on an equal basis. Chicago did not clearly face this challenge—and no African American anthropologists trained at Chicago have moved on to positions of leadership in the discipline—nor has the profession truly faced it in the decades since the 1960s.

Conclusions: Antinomianism versus Human Rights and the Possible

The sixties at Chicago saw the growth of trends in anthropology that were soon to deemphasize inclusion and move in a more closed theoretical direction. This had some ironic results. Schneider, a major figure in American kinship studies, declared that family concepts were less important than study of the dilemmas of individuals in their universal quests for power. People were living in a bubble that was far more closed off from the rest of the animal world than it had ever been before, and even behavioral observations of restraint and affection among people and lower animal forms were seen as aspects of a quest for power. Anthropology's theories began to approach antinomianism, hated by the monotheistic religions: the ideology that evil is just a word in pursuit of self-interest and has no real analytical place in understanding the human condition.

Minority scholars were instead moving toward human rights as a revisable standard in which good and evil receive clear definitions and separation. Cultural studies should cast light upon the dilemmas of human rights, even when these dilemmas were perceived through other eyes, in other times and places. Minority studies also showed great interest in areas in which non-Western peoples had taken the lead in understanding some common problem that might imply different directions for change: anthropology could become the study of the possible. Majority group anthropologists were moving in a completely different direction, one that cast doubt upon cherished social institutions like the family, religious worship, and the bases of human sexuality. Western cultural anthropology pursued a series of Euro-American agendas that marginalized non-Western intellectuals and intellectualism save as they were incorporated into what was seen as a permanent Western hegemony. Emphases on power and conflict clearly pleased arms lobbyists, but anthropologists have become less and less influential at home and abroad. They have

moved into a kind of self-contained intellectual oblivion; minority anthropologists warned them of this four decades ago.

Minority anthropology was and is going in other directions. Minorities were studying the little person from the bottom up, and took an intense interest in local responses to legal and political trends that they saw as outside of their communities and experiences. For example, my own study of Malay kinship at the village level was an attempt to start at the village level and move on to the ways that Malay intellectuals had tackled similar data from their own experiences. My work with Malay novels showed that we shared a common desire to understand Malayness and its nationalistic expressions in terms of the life of the poor peasant (Banks 1983, 1987). Walker investigated the common affects of trance in ritual experiences in Africa and among African Americans (1972). She remained active in pursuing the cultural uniqueness of African American identities (Walker 2001). Daniels has done important work on Muslim movements in Malaya that attempt to revitalize the practice and experience of Islamic religiosity through consciousness of Islam's role in Malay ethnicity (2005). Daniels's work, along with my own articles and more recent essays, attempt to define Islamic piety in local terms, apart from the theological treatises or *syariah* law (Banks 1990, 2006).

These brief examples do not suggest a completely different minority or African American anthropology but they do suggest interests rarely shared by the majority. When I mention projects to white scholars interested in religion that involve patterns of guilt, I often get yawns unless I make the power dynamics of worship central rather than aspects of productivity and creativity; guilty people are often thought less productive and if that is a problem for them, it surely is interesting to me. Senior Asia scholars are not and never were bored with such issues.

Power models really only observe and describe. They tell us at least as much about the observer as the observed. I also feel that the Newtonian division between levels, implicit in Parsons's and other models that Schneider considered part of his basic social science, is no longer useful in the age of developed quantum theory. Quantum studies confront the failure of classical physics to offer useful predictions in the subatomic world. These studies imply models of wholeness, far closer to social structuralism's consensus theories of the mid-twentieth century than to the conflict- or opposition-dominated work of later decades (see Sherden 1998). The concepts of wave function and the particle/wave duality of light and other forms of energy that flowed from quantum research suggest uncertainty, suspicion about attempts at methodological consistency and

prediction. Newtonian determinism is obsolete in the new science; subatomic behavior suggests free will. Physicists have taken the lead in going back to William James's attacks on social science's reductionism, and urge a movement in social science back to a kind of less theoretical relativistic description that states its premises (see Stapp 1993). F. David Peat (1994, 2002a, 2002b), a prominent quantum physicist, has written two historical ethnographies suggesting that ideas analogous to those of the new science were present among primitive and ancient peoples. Both of these authors conclude that humans have the potential to take charge of and change our world for the better. There is no reason to regard power and its manipulation as worthy of such a huge professional investment.

Susan Strehle (1992) suggests that a quantum ideology and worldview permeates the novels of Thomas Pynchon, Joyce Carol Oates, John Updike, and Margaret Atwood, among others. Strehle, in putting fiction's Newtonian past on the obsolete shelf, quotes Norman Mailer that reality "is no longer realistic." Some Malay novelists lectured me on their new fiction methods, as they called them: no main characters, no theories or forces driving the plot forward. Everyone in these novels counts equally. Understanding some aspects of the subatomic world and its likely relation to consciousness and thought processes suggests a broader, more open anthropology that is not dominated by any clique or sense that we are soon on the way to methodological completeness. The world is whole. Theories are not. Let's all be tolerant and try to comprehend each other.

In summary, when I consider the impact of my intellectual experience at Chicago from my entry in Fall 1965 to my graduation with a PhD in December 1969, I can only conclude that it had less of an effect on later commitments than my field experience in Malaysia, which had begun with a junket there as student leader in the summer of 1965. In Malaysia, I was able to enter a new world of ideas relevant to my own and to revise the understanding of morality and society that I had learned previously. Deeply religious before going there, I adopted a new religion in Malaysia, Islam, which I first took on as a moral and intellectual commitment but eventually as a deeply felt religious practice. My adopting Islam has been more like a return than a conversion. I did not feel comfortable with studies of power, conflict, and competition—or any studies that I thought related to them.[10] I have not found such studies of much interest among minority colleagues, who will include these aspects but not make them central. Perhaps it relates to the Christian spiritual line "I'm not going to study war no more."

The reason that Euro-American colleagues and some teachers moved in the power, conflict, and difference direction seems to be the different

points of cultural origin that have made these ideas and methodologies related to them more comfortable and acceptable. As Milton Friedman told me, economics is an important tool that need not dominate our lives or way of thinking beyond it.[11] My interest in the new science reflects the multiple perspectives that it implies, which bring back the kind of thick descriptions that Geertz (1993) considered to be a basic element of anthropology, as had Boas and his students. The severely self-critical attitude that I have seen in anthropology has led us to adapt the field to a narrow set of ideas rather than to accept the commitments that emerge from the cultural backgrounds of members of other groups.

Notes

1. I discuss these foreign intellectuals as members of groups in diasporas, who do not share the ideals of local minorities. Such individuals share a common mentality that leads to sharply different commitments. Their mentality assumes that the migrant intellectual's experiences should be a window on the world to understand the moral views and behaviors of resident populations. Strong rejections of local nationalisms (Appadurai 1996) and lack of careful study of local cultures typify the direction that the intellectuals in diaspora may take and, because variants of this ideology are so comforting to the majority, use to thrive. The problem of intellectuals in diaspora and the devastating effect of their being used to prove the sensitivity of universities, professions and publication should be studied and reported on separately. The hostility against these intellectuals is real and possibly growing. In upstate New York some of my students, who had been inmates in Attica during the rebellion of 1971, said that the refusal of immigrant Indian physicians to treat herpes-pain patients after hours was one of the straws that broke the camel's back. My students said that the media kept hidden this hostility between blacks and a group that they called Cootyanies [sic]. Former inmates claimed that even prison guards were surprised at the physicians' arrogance. The Indian deans on my campus have been noted for their insensitivity in racial and ethnic issues. White administrators point to them as proof of their own good will.

2. When one mentions Malcolm X as an influence today, the usual thoughts are of radicalism and angry violence. In fact, Malcolm had a soothing influence and urged people to use their intellectual powers to solve problems without anger. He presented his fiery speeches to help his audiences to decide what they thought was right in a social situation. Usually he relied on principles of human rights that the United Nations presented in the Universal Declaration of Human Rights in 1948 (see United Nations 1948). While Malcolm may have been a media creation for white Americans and others, he was a positive moral force for us in New York. I have many memories from my youth of Malcolm asking me whether he had gone on too long or belabored a point and wondering whether anyone was listening. Malcolm X and James Baldwin, whom I met much later, were moral forces among black intellectuals, who have always faced fierce resistance among the white elite, but no anthropologist has had their influence in American society since Margaret Mead. By the late 1980s, Baldwin told me at the University of Buffalo that he had moved much closer to Malcolm's human rights perspective.

New York's public intellectuals would sometimes spend time answering questions when accosted on the street. I met and chatted with several famous people that way, not even

seeking them out. I was surprised also how genuinely friendly these people could be when I met them at work, in nightclubs as musicians, at churches as ministers, and so on.

3. Many graduate students were on NIMH fellowships. At that time, the Surgeon General was Verne Booth. I talked several times on the phone with him and he was aghast when I told him about my experiences at Chicago. Black students were X-rayed every six months because they were more likely to have many diseases that X-rays detect (TB, for example). They also wanted venereal disease tests from all minorities. I protested to Booth and he intervened. He was also disappointed at the direction anthropology was taking. He thought that anthropology's job was to gather information about people worldview, their hopes, fears and aspirations. America needs to know about the world beyond its borders, as we are all in the human family. He was deeply respectful of the field's past.

4. Ira Bashkow (1991:171–172) tells a different story of Schneider's background, which he says was of Eastern European Bolsheviks. He was born in Brooklyn, my hometown (and also that of Barney Cohn), but Schneider told me that he did not reside there long and had no positive recollections of it. He also told me that he had dyslexia during youth and lapses in reading and thought. The symptoms associated with this condition lasted on into adulthood but he had found conscious mechanisms to control them. He did not present this as secret information but rather the language of the survivor. I respected him for this attitude. Schneider knew that my grandmother was German from Hamburg, had come from Germany to New York via Chicago around the turn of the century, and had taught German on Long Island. He knew that I could still speak a bit of German, a language I spoke in early childhood.

5. John Coltrane was then playing in an intensely lyrical period when I entered Chicago. He asked me at Birdland in 1964 what I felt when I heard him playing his Crescent suite. I asked him how he could live with playing such beautiful songs nightly. He said that you get used to it and you hear different things each time you perform; each night is different. I entered Chicago in 1965.

6. Bashkow does not include Lingenfelter's (1985) critique of Schneider's ethnographic findings. I contacted both Lingenfelter and Schneider, and they confirmed a major difference in field data and analysis. This was my last phone call to Schneider.

7. After getting one low grade on a quiz in elementary economics, I got a low final grade despite getting the highest grade on the final exam. I made no errors and got extra credit as well. The professor stigmatized those who did not catch on right away.

8. Minorities consider the feminism of white feminists as divisive and destructive of homes and families. They also support full equality for women. In my work as teacher in Malaysia I have enjoyed having women as supervisors. My female students often consider themselves feminists in that they as I support full equality for women. I have had the good fortune of helping women become lawyers, doctors, film directors, and other professionals. My feminist students usually are non-white, local and foreign alike. They strongly dislike what they call white feminism. The black community generally supports equality for women in the workplace. Black women tell me that white feminists constantly use feminist ideas to critique areas like gay marriage (which blacks are not against in the main) but they do not use feminism to oppose, for example, the American bombing of the children's hospital in Baghdad, or its veterinary hospital, sources of great distress during the first month of the invasion.

9. Schneider's group-authored article (Schneider et al. 1963) on the incest taboo, in which he dismisses behavioral evidence of avoidance behavior in other animals as an explanation of human sexual avoidance, makes a cognitive divide between men and lower animals that is difficult to support today.

I had taken a tutorial on Buddhist techniques of meditation with my geology professor at Brooklyn College, who had studied with monks in Vietnam while on an Allied mapping project there. He said that subjectivity and objectivity, mind and body, are different forms of energy. Keeping them absolutely separate was not warranted since they interact in complex ways that other sciences are beginning to unravel through studies of brain activity and studies of the energy content of particulate matter. He thought that the Parsonian tripartite distinction between the ideological, the social, and the psychological was unnecessary and would soon cease to provide useful interpretations of data. I kept the geology professor's ideas in mind as I listened to Schneider's literal use of Parsons's worldview.

10. I converted to Islam in 1968 but did not practice the prayer ritual regularly. My local *kathi* had learned about my research and thought that local British- and Indian-trained Malay administrators were treating me far worse than they would if I were a white American citizen. He called me to his office and told me that he was working behind the scenes to have them replaced by morally better people (this was easier to do because of retirement at age fifty-five there). The *kathi* said that I should learn gradually, no matter how long it takes. He said that he regularly listened to the Voice of America Gospel Hour and listened to the Christian religious ideals of black Americans and noticed that they are at least 90 percent the same as Muslim ones. He said I should have and practice both religions. He took my photograph and said that he would recommend that I take a brief course and then go on the hajj, which he used for Malay youth to learn their faith better. His alternate plan was that I should date a Muslim girl and study with her and asked me if I had any hesitancy to date Malay women. He even had one of his daughters in mind. Thanks to the ideas of Sheikh Abdul Aziz Ahmad Wan Besar I have always regarded the religious establishment of Malaya as quite flexible. He told me how they make changes in religious law and I was impressed. They hold hearings and consider the likely effect of decisions about important matters. I have come to deeply respect the work of colleagues like Rosen in his attempt to demonstrate a permeation of Moroccan culture by Islamic legal concepts (2006). Sheikh Abdul Aziz passed on to his reward in about 1970. He explained and showed me law books that he used in decisions. He was Egypt-trained and fluent in Arabic. He had more than one wife but did not recommend that the youth go in that direction. He was also quite tolerant of village ritualism but thought that religious people, *imam*s and *kathi*s, should see it for what it is—*syirik*, that is, polytheism prohibited in Islam. We had profound but always pleasant discussions. He was also an acquaintance of Dr. Mahathir Muhammad, another prominent Muslim, who hoped I would eventually practice the faith. My friendship with Dr. Mahathir continues. I hope to see him again, if God wills.

11. Friedman had a wonderful intuition about where I would wind up intellectually. He mentioned the volumes of Marx to read but knew that they were general education for me. He wanted me to read general economic theory and its relation to politics so I could identify economic approaches. I worked on his ideas in my spare time for a decade.

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