

Journal Of Northwest Anthropology

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“La Gente de Washington es la Más Tranquila” (People from Washington are the Most Laid-Back): An Ethnographic Perspective on Honduran and Salvadoran Migration to the Pacific Northwest

Jordan Levy and Sandra Estrada.....1

Salish Sea Islands Archaeology and Precontact History

Richard M. Hutchings and Scott Williams.....22

A Comment on Wessen’s “Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington,” with a Reply from Gary C. Wessen

Jay Powell and Gary C. Wessen.....62

“Notes Regarding my Adventures in Anthropology and with Anthropologists” by John Swanton with an Introduction by Jay Miller

.....97

“The Haida” by Adolf Bastian with an Introduction by Richard L. Bland

.....126

W

Journal of Northwest Anthropology

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CONTENTS

- 1 **“*La Gente de Washington es la Más Tranquila*”** (People from Washington are the Most Laid-Back): An Ethnographic Perspective on Honduran and Salvadoran Migration to the Pacific Northwest
Jordan Levy and Sandra Estrada
- 22 **Salish Sea Islands Archaeology and Precontact History**
Richard M. Hutchings and Scott Williams
- 62 **A Comment on Wessen’s “Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington,” with a Reply from Gary C. Wessen**
- 62 A Comment on Gary C. Wessen’s “Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington”
Jay Powell
- 79 Reply to Powell
Gary C. Wessen
- 97 **“Notes Regarding my Adventures in Anthropology and with Anthropologists” by John Swanton with an Introduction by Jay Miller**
- 97 Introduction
Jay Miller
- 98 Notes Regarding my Adventures in Anthropology and with Anthropologists
John Swanton
- 126 **“The Haida” by Adolf Bastian with an Introduction by Richard L. Bland**
- 126 Introduction
Richard L. Bland
- 127 The Haida
Adolf Bastian, Translated by Richard L. Bland
-

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“*La Gente de Washington es la Más Tranquila*” (People from Washington are the Most Laid-Back): An Ethnographic Perspective on Honduran and Salvadoran Migration to the Pacific Northwest

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Abstract *Drawing upon engaged ethnographic research conducted during 2018 in Washington state, this paper examines how Honduran and Salvadoran transnational migrants navigate changing circumstances and turbulent times characterized by intensified forms of xenophobia and racism in the U.S., and political uncertainty in Central America. Most literature on Honduran and Salvadoran migrants focuses on the “push” and “pull” factors of international migration. Our paper engages such important questions, but also goes beyond causal frameworks about why people move—to focus instead on everyday lived experiences in the receiving country. Working from a theoretical perspective that privileges migrants’ agency in choosing to move to the Pacific Northwest, we explore peoples’ adept abilities to pursue their livelihood strategies while reading the political landscape and imagining different paths toward realizing their goals. In so doing this study contributes to anthropological understandings of Central American transnationalism in the Pacific Northwest during the post-2017 U.S. political environment.*

Keywords

Honduras, El Salvador, transnational migration, agency, xenophobia

Introduction: Moving From Central America to Washington State

Over coffee one July evening in Fife, Washington (an industrial town between Tacoma and Seattle), Mario recalled the life he once lived in La Ceiba, Honduras—the country’s third largest city. He grew up in a neighborhood affected by gang violence and drug trafficking. And his family struggled to meet their basic subsistence needs. Although Mario did manage to finish high school with training in soldering and welding, upon graduating he had no means of studying at the any of the country’s universities, nor could he find viable work in La Ceiba. To make matters more complicated for him, at the young age of 18 he

and his girlfriend discovered they had a baby on the way. Without a steady income, Mario did what he had to in order to survive.

Like so many other young unemployed men in Central America, he would spend his days loitering the streets, attempting to do occasional odd jobs for an under-the-table tip—an endeavor which relied upon his entire social network in his neighborhood. Being out in public spaces socializing with other unemployed young men meant that Mario was becoming particularly susceptible to recruitment into the world of illicit drug trafficking. He revealed to us in his interview that there was a time in La Ceiba when he did sell drugs as a way of surviving and providing for his family. But he knew this was not a viable solution to his

problems because he was aware of others who had become victims to the violence associated with the trade. Cognizant that this was not a lifestyle he wanted in the long term, Mario decided to leave Honduras, migrating first to Florida in search of a construction job. He successfully crossed the Guatemala-Mexico and then the Mexico-U.S. border clandestinely, quickly found a job and moved in with extended family. At first, things were going well in Florida for Mario; he worked harder than he ever had and began sending remittances to support his family in Central America. But all this changed when he was then pulled over by traffic police for speeding and the officer discovered he had been driving without a license. Summoned to appear in Florida traffic court, but worried about his undocumented status and how this minor traffic violation could lead to his eventual deportation, Mario decided to leave Florida for Washington state, where he had heard about migrants obtaining a driver's license legally, even without immigration paperwork.

Drawing upon engaged ethnographic research conducted during 2018 in western Washington, this article examines Honduran and Salvadoran transnational migrant experiences in the Pacific Northwest. Our research takes place in a historical moment characterized by intensified forms of xenophobia and racism in the U.S., and significant political uncertainty and turmoil in Central America. Working from a theoretical perspective that privileges migrants' agency in choosing to move to Washington state, we explore peoples' adept abilities to pursue their livelihood strategies as they read the shifting political landscape of the United States—the new immigration policies coming from the federal government—while also imagining different paths toward realizing their socially-constructed goals. In so doing this study contributes to anthropological understandings of Central American transnationalism in the Pacific Northwest during the post-2017 U.S. political environment.

Background: Theorizing Contemporary Honduran and Salvadoran Migration

Anthropologists and others who study the contemporary Latin American diaspora continue to approach migration vis-à-vis three main arenas of inquiry: 1) the various reasons why people leave their countries of origin; 2) migrants' lived experiences while in route to, and crossing, the international borders of receiving countries; and 3) what migrants actually do once they are living in the receiving country, attempting to make a better life for themselves. Our project engages all three arenas but focuses on the third realm of inquiry. In relation to this first question of why people move, perhaps the most significant "push" factor driving the contemporary exodus from Honduras and El Salvador is that these two countries have some of the highest homicide rates per capita outside of a warzone, hitting an all-time high of 108.64 murders per 100,000 inhabitants in El Salvador in 2015, and 93.21 per 100,000 in Honduras in 2011 (UNODC 2018). The allure of being able to remit money to support one's family's basic subsistence needs is also commonly conceived as a kind of "pull" factor to explain why people move where they do. In the case of Honduras and El Salvador, remittance money far surpasses earnings from other industries; it also represents a major source of state revenues as people spend remittance money in local economies. In 2016 the amount of Gross Domestic Product comprised of remittance money was 17.1% in El Salvador, and 18% in Honduras (World Bank 2018). Regardless of paperwork status in the U.S., contemporary Honduran and Salvadoran migrants come fleeing violence while also in pursuit of economically stable living conditions.

Major U.S. media outlets have recently begun to focus on the Central American exodus, highlighting the fact that Hondurans and Salvadorans are coming to the U.S. with their children. That people leave the isthmus in large numbers due to rampant violence, extreme poverty, and significant political uncertainty

is, however, not a new phenomenon; neither are the various interventions by the U.S. in the affairs of Honduras and El Salvador. Imperialist interventions of all kinds have been shown to hinge the national and economic development of Central America—benefiting foreign interests while impoverishing local people from the isthmus. Most reasons for emigrating can be linked to civil wars in the 1980s, followed by the imposition of neoliberal development policies from the 1990s onward.

The Cold War had immediate consequences for the poor of Central America who became caught in the crossfire in a mix of capitalist and socialist interests. During the 1980s in El Salvador the left-wing guerrilla group, Farabundo Martí National Liberation Front (FMLN) was coming closer to its goal of toppling the U.S.-backed right-wing government of José Napoleón Duarte (Woodward 1985:251). (A similar situation was underway in Guatemala, and socialist revolution was already successful in Nicaragua.) The U.S., worried about the spread of Soviet- and Cuban-influenced ideologies in its “own backyard,” saw Honduras as key to its geopolitical interests, and installed a total of 18 military bases throughout the country in order to train and deploy capitalist-allied troops in their fight against armed socialist revolutionaries (Lapper 1985; Alvarado 1987; Salomón 1989; Pine 2008). The result was both a civil war in El Salvador and persecution of Honduran working class and peasant organizations who were deemed a “communist threat” when advocating for labor and land rights. While some Salvadorans were ardent supporters of a given side, the estimated 75,000 murdered and 10,000 “disappeared” victims of war were not ideologically-driven. When Salvadorans began to flee from civil war violence and arrive in large numbers to the U.S., the official response of the State Department was to deny their need for refugee status (given that they were leaving a U.S.-sponsored government), thus relegating most Salvadoran migrants at the time to an undocumented status (Alvarado et al. 2017:9). While Salvadorans fled from civil war in large numbers during the

1980s, the amount of Hondurans fleeing from state-sponsored persecution during this period of militarization and national security doctrine was comparatively low.

By the 1990s with the close to the Cold War and onset of peace accords in Central America prompting the formal end to civil wars, both Honduras and El Salvador had become further in debt to foreign lending institutions and dependent upon the income that U.S. military aid provided their economies. Worried they would default on foreign debt loans, the response from the International Monetary Fund and World Bank was to demand via “structural adjustment” programs. These aimed to significantly decrease government spending on social services, while encouraging privatized enterprises previously run by the state. Such “neoliberal” policies required attracting foreign capital and setting up conditions that would attract transnational corporations in Central America—incentives such as charging few taxes, while granting access to cheap labor, cheap or free land, and minimizing environmental and health regulations. The imposition of neoliberalism in Central America from the 1990s onward has since forged policies of governance aimed at curtailing state-sponsored services and programs, turning them over to the private sector.

At a political and economic level, neoliberalism in Latin America has been associated by some with attempts to create a better climate for attracting foreign investment through the understanding that this “opening up” of the economy will stimulate the “free market.” And cutting basic social services from the state’s budget is also one of the primary methods by which international financial institutions have argued indebted countries can pay down their development loans—resulting in coercive agreements that require they cut such services. Yet neoliberalism has also been associated by scholars and activists alike with dangerous and poorly-paid working conditions, and tremendous increases in poverty and social inequities—creating lived experiences of “misery” for the vast majority as the most

basic government services are privatized and subsequently become too expensive for the majority of people to afford (Phillips 2017). In Central America the increasingly high price of health care, education, road construction, and the delivery of utilities is downloaded onto the shoulders of individuals (Harvey 2005; Goodale and Postero 2013). While neoliberalism is global in scope, its effects, experienced locally, are diverse in terms of what exactly is privatized.

At a social and cultural level, under the philosophy of neoliberalism, individuals and local communities are encouraged to take responsibility for meeting their own basic needs without the assistance of the state. Through a discursive framework of efficiency, self-regulation, and accountability (values that might well resonate positively with a range of people), proponents of neoliberalism argue that it is the best way to achieve financial well-being. Like all social processes, the formation of neoliberal subjectivities in El Salvador and Honduras is a gendered phenomenon, which connects with previously-existing cultural understandings of gendered household tasks. For instance, young men such as Mario experience pressure to provide for their families, but if massive unemployment and underemployment conditions cannot forge the right kinds of opportunities for these men to provide economically, many turn to migration as one viable solution. In other cases, young unemployed males seek out, or get coercively recruited to, the infamous *maras* (street gangs), which do form a kind of community for youth who are abandoned. *Maras* tend to engage in illicit activities in urban centers in Honduras and El Salvador where they frequently have violent encounters with the state's security apparatus (e.g., Wolseth 2008). Over the years different governments in both countries have adopted a series of policies known locally as "*la mano dura*" (an "iron fist"), which give the poorly-trained and chronically under-paid police and military officials the ability to arrest anyone who they suspected of being a gang member. Anthropologists working on these topics have demonstrated the extent to which

such policies have adverse effects for young men (Pine 2008, 2010; Wolseth 2008; Phillips 2015), some of whom are not actually gang members, but get arrested nonetheless while wandering the streets and loitering (the police having identified them by their tattoos, short haircuts, and clothes, which are sometimes also gang symbols). Thus, in addition to widespread poverty exacerbated by the negative effects of neoliberalism, Hondurans and Salvadorans now also flee from everyday forms of violence (caused by either gangs themselves or the state's security apparatus—police and military corruption has now become commonplace throughout both Central American countries).

In the 1990s these consequences of neoliberalism coincided with efforts by part of the U.S. to deport undocumented Salvadoran youth, some of whom had formed gangs in the U.S. (in response to their exclusion from mainstream society). Then in 1998 Hurricane Mitch struck Honduras—arguably the deadliest and most destructive natural disaster in the history of Central America—killing 7,000 and displacing nearly 20% of the country's population (Alvarado et al. 2017:14). These processes characterize much of the migration patterns from Central America to the U.S. As Central Americanist scholars Karina Alvarado, Alicia Estrada, and Ester Hernández summarize:

Thirty-five years after the initial mass civil war migrations primarily from Guatemala and El Salvador from the '70s to the '90s, out-migration continues because of post-war economic free-trade programs, growing economic dependence, and the proliferation of *maras* (gangs). Central Americans have grown permanent roots in the United States. (2017:6)

Thus, while the civil war in El Salvador produced its first exodus to the U.S. during the 1980s (Coutin 1993, 2000, 2007), it was not until the onset of neoliberalism and aftermath of Hurricane Mitch in 1998 that Honduran migrants began to move to the U.S. in large numbers

(cf. Stonich 1991; Schmalzbauer 2005; Barrios 2009). Most Salvadorans and Hondurans in the U.S. move to cities such as Los Angeles, Houston, and Miami—where there is already a large Central American migrant community. In Washington state the Central American population is relatively small, although the Latino/a/x population is the fastest growing ethnic group in the state—comprising 13.1% of the total population (CHA-Washington 2020).

Anthropological attempts to understand this group of Latin Americans in the U.S. aim to study a diverse set of processes. Some scholars of Salvadoran and Honduran out-migration have focused specifically on how the lack of opportunities for young people and increased everyday forms of violence push people to leave Central America (Wolseth 2008; Reichman 2011; Dyrness 2012, 2014). Others have illuminated migrants’ paradoxical experiences of life once integrated into U.S. society—with a hegemonic discourse of inclusiveness on the one hand, and migrants’ actual lived experiences of exclusion and racism on the other (Coutin 1999, 2005; Schmalzbauer 2004, 2005, 2008; Alvarado et al. 2017; Cárdenas 2018). Still too, others have examined the contrast between the various ways migrants and their families in Central America imagine a “good life” in the U.S. and migrants’ actual lived experiences of underpaid and dangerous working conditions that do not allow them to fulfill their goals, especially if they are undocumented and struggling to meet their basic substance needs (e.g., Reichman 2011; Alvarado et al. 2017). Finally, scholars working in political-economy and world system theory frameworks have unveiled how the bodies of Salvadoran and Honduran migrants have been transformed or commodified by elites (e.g., Vogt 2013), in a world system that exploits the labor of poor individuals in both the “periphery” and “center” of global capitalist relations.

Our study draws upon knowledge produced by scholars of contemporary Central America about the various ways the U.S. has forged conditions for its own transnational corporations to profit in the isthmus, since at

least the early twentieth century—contributing to much of the extreme poverty that such capitalist relations produces for majority (see, for instance, Portillo-Villeda 2014a, 2014b; Phillips 2015, 2017). This resonates with our present endeavor since all of our Salvadoran and Honduran informants are also individuals who experienced these realities first-hand and decided to leave their countries. Our research participants are individuals whose labor was chronically underpaid in Central America, and remains underpaid in the U.S.

Our ethnographic research among this group of transnational migrants in western Washington engages these broad anthropological questions about people’s reasons for, and experiences with, international migration. In so doing, however, we also move beyond causal frameworks about why individuals leave their “sending” country (the “push” and “pull” factors of migration), to ask what differently-positioned individuals actually do once they arrive in the “receiving” country: how migrants navigate daily life in the U.S. and develop their own short-term and long-term goals, and how events from their lives in El Salvador and Honduras influence people’s socially-constructed ambitions. We consider valid and important those studies that examine the human experience of crossing the southern U.S. border clandestinely, but we also advocate for additional research into understanding the various ways that Honduran and Salvadoran migrants continue to make history once in the U.S. We therefore join other scholars of the Central American diaspora in shifting our focus “beyond civil wars and political factions to community emplacement and social justice within the United States” (Alvarado et al. 2017:ix). This shift demands we study individual choices and experiences.

Our focus on the agency of migrants aims to uplift the creative and adept ways that Hondurans and Salvadorans have been reading the post-2017 political landscape—the changes to immigration policy in the U.S. since the inauguration of Donald Trump, and the

accompanying increase in xenophobia and racism directed toward Central Americans (regardless of paperwork status). Rather than see Honduran and Salvadoran migrants as mere victims in a global system of structural inequality, we consider the various ways that they actively construct their individual lives between more than one nation-state, and use their agency to make conscious efforts toward realizing their goals. A focus on the human ability to achieve diverse notions of “the good life” amidst adverse structural conditions has been especially productive for understanding the everyday experiences of working-class and vulnerable migrants in particular, and something we believe the ethnographic method is well positioned to study (see, for example, the seminal work of Gomberg-Muñoz (2010) on the utility of an agency framework for understanding the complexities of Mexican busboys in a Chicago diner). By studying how differently-positioned individuals read the contemporary U.S. political landscape and envision possibilities in both sending and receiving countries, we seek to contribute to scholarly debates on how migrants take concerted actions in everyday contexts to achieve their familial and economic aims. How then should migrants be conceived, theoretically?

We begin with the premise that contemporary immigrant communities—in the U.S. or elsewhere—can no longer be considered “uprooted,” or to have completely severed ties with their countries of origin. Rather, we assume that Salvadoran and Honduran migrants actively construct some kind of *transnational* ties, meaning that they “forge and sustain multi-stranded social relations that link together their societies of origin and settlement” (Basch et al. 1994:7; see also Schiller et al. 1995). Our project is thus an exercise in understanding the lived experience of sending remittances to, and remaining in contact with, family members in migrants’ countries of origin; becoming politically conscious about global processes that unfold in either or both countries; and developing individual goals that involve more than one nation-state. In sum, our objective is to study the various

ways that Salvadoran and Honduran migrants simultaneously build their lives in both North and Central American societies.

This focus on the agency of migrants in not severing ties and loyalties to either country moves beyond the assimilationist paradigm, once popular in anthropological studies of immigration. The present endeavor also represents a shift away from classic place-bound studies in the discipline as a whole (of, for instance, a village). Rather than study the dynamics of one location, our project aims to study a process—transnational migration—as it is experienced across space. In so doing we ask questions informed by economic and political processes in contemporary Honduran, Salvadoran, and U.S. societies, in order to study not just why people leave, but what characterizes their experiences with migration in the U.S., and the Pacific Northwest in particular. For instance, we see the phenomenon of gang violence in Central America as not just a reason why people leave, but part of how they envision the wellbeing of loved ones once in the U.S.—how this influences their decisions about remittance patterns, and any possible family visits. The global flow of policies and capital thus influences migrants’ living and employment conditions across national borders. At the same time we engage topics of long-standing and continued anthropological interest by considering variables to do with migrants’ identity, including how people’s varied gender roles and ethnic categories affect their experiences with migration—and in turn, how transnational migration itself influences these aspects of people’s identities.

In uplifting the agency migrants in choosing to move to and remain in the Pacific Northwest instead of other regions of the U.S., we learned that Hondurans and Salvadorans have a diverse set of reasons for settling in Washington state. Their reasons range from perceptions about the strong economy and job opportunities; to less competition with other undocumented migrants; to few encounters with Immigration and Customs Enforcement (ICE) officers; to the ability to get a driver’s license without legal residency

paperwork (which is now changing with the Real ID Act of 2005, see Goodell 2018); to simply the notion that “People from Washington are the most laid-back” (*La gente de Washington es la más tranquila*), as one Garifuna-Honduran restaurant owner in Burien so candidly captured [interview, September 5, 2018]. The Pacific Northwest region has a long-standing history as a “frontier space” of the U.S. nation. Newcomers and native-born residents alike tend to discourage government oversight in business and family affairs. At the same time, the perception that people from Washington state are less xenophobic and less overtly racist toward Central American migrants was commonly expressed among the majority of our research participants who had lived in than other parts of the U.S.

Research Sites, Positionality, and Methods

We spent the summer of 2018 conducting ethnographic research among Hondurans and Salvadorans who were already settled in the Pacific Northwest. All of our research participants had been in the U.S. since before 2017, which means they experienced living in the U.S. during previous periods of U.S. immigration policy. In 2018 our research participants were sharing with us their intimate stories of crossing the U.S.-Mexico border and settling in the Pacific Northwest during a moment of increased racism and xenophobia directed toward the Central American migrant community, much of which coincided with the first of several migrant “caravans” (already well underway by summer 2018). Our engaged research project also emerges from our positions as insider and semi-insider in this community, and our own long-term personal and professional commitments to Honduras, El Salvador, and their diasporic populations. Sandra Estrada was born in Olancho, Honduras, and migrated to Washington at a young age where she was raised in a Honduran-Salvadoran immigrant household. During the time of data collection Sandra was an undergraduate student; she regularly attends church and community

events within the Central American community in Tacoma. Her professor, Jordan Levy, has been doing engaged anthropological research in Valle and Choluteca, Honduras, on various political processes since the 2009 military coup. He is married to a Honduran woman from the region—now an immigrant to the U.S. That we as researchers each have family in Honduras, and Central American family members in Washington state, means that neither we nor our research participants approached these topics from a supposedly “objective” or completely “neutral” position. Rather, we believe it is our very positionality as members of this community and demonstrable dedication to justice for Central America and its diasporic populations, that facilitated our rapport and ability to interview Salvadoran and Honduran migrants during our contemporary historical conjuncture.

We are living in a moment of U.S. history characterized by heightened levels of nationalism and nativism; one cannot simply insert oneself into this migrant community and expect to collect ethnographic data without strong levels of trust. As others have convincingly argued, there are several theoretical and methodological advantages to engaged anthropology: the solidarity ethnographers demonstrate with their informants facilitates trust and access to ethnographic details they are not otherwise likely to obtain when dealing with controversial topics (cf. Nash 2007; see Kirsch 2018). In other words, we approached our fieldwork with explicit support for migrants.

Given the similar historical and contemporary political processes of both Central American countries that influence their out-migration patterns, and their people’s experiences in the diaspora, we decided to include both Salvadorans and Hondurans in a single study. In the Pacific Northwest, Hondurans and Salvadorans frequently eat at the same Central American food restaurants, attend the same churches, and social events. This allowed us to set up initial interviews through our pre-existing networks of friends and family, following which we received recommendations of who else to

interview as the news about our project began to “snowball.” Through our conversations in people’s homes, at restaurants, and other public spaces, we were able to ask how this migrant community manages livelihood strategies and navigates uncertainty—how they forge a life for themselves amidst increasing economic and political precarity.

While we didn’t directly ask about people’s immigration paperwork status, the topics that interviewees themselves brought up revealed to us that more than half of our participants were either undocumented, or were in some kind of “limbo status”—toward documentation, or toward having their legal status removed. To complement our semi-structured interviews we also attended migrant solidarity events outside the Northwest Detention Center in Tacoma, and different public celebrations of Central American culture in the greater Seattle area. In total we interviewed thirteen different migrants—nine Salvadorans and four Hondurans; six of whom were female, and seven male. The following three vignettes underscore our findings on how Honduran and Salvadoran migrants use their agency to navigate contemporary political processes beyond their individual control. Topics to be examined include migrants’ access to healthcare under a privatized system linked to employment; the shifting criteria for “nexus” to obtain asylum status in the post-2017 political environment; and the recent removal of the Temporary Protected Status program for Central Americans.

Findings: Domestic Violence, Brain Cancer, and Forced Divestment from Washington

While many people from Honduras and El Salvador flee state-sponsored violence, drug-trafficking violence, or gang-related violence, we also heard stories about people fleeing from domestic violence. The case of Laura is one such example. Her childhood in El Salvador consisted of never knowing her biological father, and her mother seeking out male partners who could support her financially, but ultimately abused

her physically, and kicked her children out of the house. Laura therefore spent her childhood moving between her aunts’ homes and the houses of the various abusive men with whom her mother had relations. Domestic violence was rampant during Laura’s childhood—she was threatened by several men, prompting her to flee home on more than one occasion; ultimately one of her mother’s boyfriends was murdered. Laura had hoped for a life without violence when she married and became financially independent from her mother. But Laura’s husband repeatedly beat her and raped her. She had attempted to leave him on several occasions but could not find adequate support for doing so successfully. Especially as neoliberal governments reduce expenditures for social services, no women’s shelters or social workers existed in the city where Laura lived, and she did not feel that law enforcement officials would step in to help a battered woman. Laura’s husband would not allow her to use contraceptives and she became pregnant after one such instance of rape. After her daughter was born, during one episode of alcohol and drug abuse, her husband attempted to murder both her and the baby. Laura managed to escape and sought refuge in a neighbor’s house.

With the help of her neighbor, Laura was able to call someone in the U.S. who arranged for a *coyote* (human trafficker) to bring her across the U.S. border. She had never before considered fleeing from her country, but after that incident felt that she had no other options. Given her socioeconomic background and lived experiences similar to those of her mother (of depending upon abusive men for economic support), Laura had no previous travel experience, much less internationally; she never desired to leave her home in order to work in the U.S. Yet she did not have anyone in El Salvador who was able to help her, or to physically defend against her abusive husband. He knew this, and would constantly tell her, “*no tienes ni perro que te ladra*” (an idiomatic phrase, which roughly translates to “you don’t even have a dog who will bark to defend you”)

[Interview, June 28, 2018]. Laura thus came to the United States fleeing for her life, and seeking asylum for both herself and her daughter. At the time of her departure from El Salvador, Laura was confident that once on U.S. soil her case would be successful. She knew of course that her declaration to U.S. immigration authorities was sincere—because she really was fleeing for her life. And she believed in the U.S. asylum system to defend her against the high likelihood of torture and death that she would endure if forced to return to El Salvador.

The clandestine journey itself took her through Mexico via land, which was very dangerous and difficult—especially since she was traveling with an infant. Other migrants along the way would tell her that she would surely be the first to get caught because of the baby. This, however, did not deter her in the least. Laura kept repeating to herself, “*mi hija va a tener un mejor futuro*” (my daughter will have a better future) [Interview, June 28, 2018]. She eventually made it to Washington, D.C. where she had an aunt who received her. Knowledgeable about the dynamics of femicide in her home country, Laura mentioned in her interview that at least one woman a day dies in El Salvador, and how she hoped that this statistic would help advance the credibility of her asylum case. In fact, the actual statistic is worse than what Laura imagines: According to the Institute of Legal Medicine, there were 468 femicides in 2017—that is one every 18 hours (Griffin 2018). Yet even once in the U.S., Laura was not entirely safe. Her ex-husband managed to find where she was; he would send her and her boyfriend death threats.

At one point, Laura’s ex-husband attempted to cross the U.S. border himself with the expressed intention of killing her, but he was detained at the Mexican border and was sent back to El Salvador. Anthropologist Lynn Stephen has demonstrated through her work serving as expert witness for asylum cases for Mexican migrants in Washington and Oregon, the intersectionality between institutional violence and domestic violence, and the extent to which international

migration does not necessarily solve domestic violence:

We need to explain both as expert witnesses and anthropologists how gendered violence is not simply violence that targets women because they are women and continues because of how men and others are socialized to treat women as disposable and unimportant. We also have to demonstrate the ways that states, police, local government, and justice officials perpetuate and sanction this violence. (2016:161)

This understanding resonates with Laura’s situation, since she fled from El Salvador not just because she was a woman, but because she lacked access to state institutions that could protect her, and financial capital to simply live on her own. We must therefore understand the attempted murder of Laura and her child in light of Laura being someone without a large kin support network; as someone who never graduated from high school; and, as mother in Salvadoran society, as someone who would have little chance of obtaining any kind of viable employment that could provide enough income to support her child.

At the time that Laura sought legal immigration paperwork by applying for asylum, these variables would have likely convinced a judge that she was worthy of asylum—or another kind of legal protection in the U.S. (such as the “withholding of removal and protection under the convention against torture”). At the time of our interview with Laura, however, Laura revealed to us that her asylum case was still in limbo. She paused our conversation to communicate that she is especially worried since her ex-husband might be able to find her—especially if she were deported to El Salvador. That her case is still pending is not surprising to us as researchers since Trump-appointed attorney general Jeff Sessions no longer considers individual household violence (and gang violence) as valid criteria for

asylum in the United States (see Benner and Dickerson 2018). This new legislation means that Laura may not receive asylum at all (unless her legal team can argue that she is subject to other, more systemic reasons for her persecution based on her “particular social group,” which is becoming increasingly difficult to prove).

For now, Laura remains in Washington state without legal immigration paperwork and is thus subject to deportation. Yet while the Trump Administration challenges the validity of Laura’s need for asylum, Laura herself emphasized in our interview how she does not want to “be a burden for the president” (*ser una carga para el presidente*) [Interview, June 28, 2018]. Laura’s comments go directly against much of the current rhetoric about migrants as “taking advantage of the U.S.” since she is suggesting she would continue to provide for herself and not depend upon any U.S. social service or welfare program. At the time of our interview, Laura spoke of how she only wants to depend upon herself, “*para seguir sacando adelante a mis hijos*” (in order to continue to bring her children forward) [Interview, June 28, 2018]. In order to do so, however, Laura will need domestic violence to once again be a criterion for asylum.

While not all women are persecuted in Salvadoran society, the high femicide rates and particulars of Laura’s situation make clear that she lacks institutional resources to escape the threat of violence. An intersectionality approach thus reminds us that Laura’s situation cannot be reduced to any one aspect of her identity, but rather, needs to be seen in conjunction with other interconnected forms of institutional injustice. In El Salvador, the police are unlikely to intervene to defend Laura and her daughter—even if they believed her story about abuse, which they may not. For all these reasons, Laura was indeed fleeing from structurally-imposed dangers to her life in El Salvador, where she had very limited access to public education, social services, or employment opportunities that could have helped to prevent, or at least alleviate, such threats to her life. So long as she is undocumented and thus lacks access to

basic public services in the U.S., however, such structural violence that disproportionately affects women will continue to pose a major challenge to Laura’s wellbeing.

If deported to El Salvador it is very likely that Laura’s husband would find her. This is because her resettlement would depend on her own pre-established network of family and friends and the news of her presence could easily reach her abusive husband. She told us during her interview that one of her goals is to get permanent residency paperwork, which would allow her to raise her children in the Pacific Northwest, where she believes she would have the best access to employment opportunities and a good quality of life—that is, once she has legal documents. Until she gets permanent residency, however, Laura continues to face an uncertain future while living in a transnational limbo space. Yet she remains resilient and continues to exhaust all resources in order to make a life for herself.

Such intersectionality about who is likely to be most affected by structural injustices in Central America can also be seen in the case of Mario. As discussed in the opening of this article, he did manage to graduate from high school but could not find viable employment. And he lived in a neighborhood affected by gang activity, where drug trafficking was already a common livelihood strategy when he came of age. He felt neoliberal gendered pressure to provide for his family—even when no viable employment opportunities existed. But thankfully for Mario, he had the foresight to understand that selling illicit drugs would have likely brought him more problems. He chose instead to reform his life. To do this, however, he knew he would have to migrate without paperwork to the U.S., and eventually to Washington.

In his interview Mario told us that upon arriving to Sea-Tac airport, he had three priorities: get a job, get a state-issued driver’s license, and find a church. Churches in western Washington that offer services in Spanish also perform an important social function for the Central American migrant community—providing resources for job trainings, immigration

“LA GENTE DE WASHINGTON ES LA MÁS TRANQUILA”

paperwork assistance, and English classes. At first Mario thought Washington state would be only a temporary home, somehow hoping that things in Florida would resolve themselves. He did not know anyone in Washington, and he thus spent his first few days living in a hotel while he looked for work with a bicycle. His determination and resiliency amidst structural difficulties meant that he would ride the bicycle to different warehouses throughout the Seattle-Tacoma area, and with his limited English, use hand signals to communicate to employers that he knew how to weld metal. But these welding jobs were occasional, temporary, and always under-the-table.

Los empleadores gringos me trabajaban mal (the American employers would work me poorly). *Yo rogaba a Dios que hubiera uno donde hablan español, y me respondió: hubo uno... ¡pero no había trabajo!* (I begged God that there would be one where they speak Spanish, and he answered me: there was one... but there was no work!) [Interview, July 12, 2018]

To make matters more difficult for Mario, in the midst of his struggles to find employment in western Washington state, he discovered that cancerous cells in his body he thought he had previously overcome did in fact return. In our interview, he told us about this additional difficulty he experienced in Florida, well before the incident with traffic police: he had been diagnosed with brain cancer. Getting access to proper healthcare in order to manage cancer was a real challenge; Mario had neither legal immigration paperwork, nor health insurance. Yet he was cognizant of how hospitals could not legally turn someone away because of inability to pay or because of their immigration status. He had therefore gone to a hospital in Florida and simply accrued the debt. By 2015 when he moved to Washington, apparently the cancer had returned.

Mario's story of surviving life-threatening illness in the U.S. illuminates his savvy ability to navigate the privatized healthcare system

that neoliberalism has produced for the U.S. population—where access to affordable health insurance is linked to employment (and permanent residency immigration paperwork). Upon learning that his cancer had returned, Mario went to a public library in south King County where he sought resources on how to obtain health insurance. He applied for and received COBRA insurance (through the Consolidated Omnibus Budget Reconciliation Act) in order to gain coverage and access to his medication. Striking is his ability to seek out resources on his own, and to adeptly navigate the U.S. healthcare system.

At the time of our interview he had been off chemotherapy for five months. He spoke about the difficulties of the U.S. privatized and for-profit health care system, and how having cancer also meant not being able to send remittances to his family in Honduras—especially his mother and his daughter who depend upon that income for their subsistence needs. The COBRA insurance does not cover all of his medications, which means that Mario often cannot send any money at all. He recalled one such occasion in our interview:

Mi hija ahora vive en San Pedro Sula. Yo siempre le he mandado dinero, pero una vez estaba cumpliendo años y yo no tenía para mandarle. (My daughter lives in San Pedro Sula now. I have always sent her money, but one time it was her birthday and I didn't have anything to send her.) [Interview, July 12, 2018]

Given his undocumented status, Mario cannot realistically visit family in Honduras since he would risk not being able to make it back to Washington. But he maintains in contact with his family on a regular basis—even now, 16 years since he left La Ceiba. Mario's dream is for his daughter to come visit—that is, if his ex in-laws would let her travel here, and if she could get a visa to do so. Although he does not have his daughter in Washington with him, Mario believes that he can still give her a better life in Honduras than the one she would have if she

were not receiving the remittance money that he sends back. While so much of the U.S. population struggles under a neoliberal healthcare system (as under-insured persons whose insurance plans have such high co-payments and charges it discourages doctor visits, or as simply people without insurance), Mario's story illuminates how much more difficult these situations are if you are undocumented. Yet rather than see Mario as a mere victim of globalized neoliberal structural variables in both Honduras and the United States, we can also understand how Mario has persistently and creatively used his agency to read this political landscape, and then make efforts to obtain his own socially-constructed goals with migration to Washington.

At age 34 Mario now has family obligations in Washington state, having married into a Mexican family. He also still supports his daughter in Honduras. During our interview he reflected on the extent to which his life has changed with migration, explaining that:

En Honduras no pasé ni un día con mi familia, sólo navidad... siempre andaba en la calle. Pero aquí no, siempre estoy o trabajando o con mi esposa y mi suegra. Y me siento que por fin tengo a una familia que me cuida. (In Honduras I didn't spend even a single day with my family, perhaps only Christmas... I was always in the streets. But here no. Here I'm either working, or with my wife and my mother-in-law. I finally feel as if I have a family to take care of me. [Interview, July 12, 2018])

The gendered practice of men loitering in the streets is no longer a part of Mario's routine. In Washington state, by contrast, he goes home to his family after work. In his interview, Mario told us that his ultimate goal is to someday save enough to money buy a plot of land in both Honduras and in Washington state, but that such a goal has become difficult because of his health and undocumented status. In summer of 2018, Mario had a deportation order—the result of having neglected to go to the traffic court

hearing in Florida. Yet neither the deportation order nor the brain cancer has stopped Mario from pursuing his dreams.

For now Mario views Washington as his new home, where he continues to work in welding. Moving from his troubles in La Ceiba to a more comfortable, albeit still difficult, family lifestyle in south King County should be seen as a function of Mario's agency and perseverance in overcoming the abandonment of youth by the neoliberal Honduran state (where educational opportunities and social programs are increasingly limited and difficult for the majority to access) and the neoliberal U.S. state (where access to healthcare is linked not only to having a job with good benefits, but also one's immigration status).

Such resiliency and desire to remain in Washington state can also be seen among Hondurans and Salvadorans who have been living in the U.S. with Temporary Protected Status (TPS)—a program established in 1990 for individuals whose lives are in danger in their home countries (due to either violence or natural disasters). Hundreds of thousands of individuals have been living in the U.S. with this legal status—some for well over a decade, and many now have U.S.-born children. Yet with the Trump administration's decision to remove TPS for Hondurans and Salvadorans, an estimated 86,000 Hondurans, and 200,000 Salvadorans registered with this program will lose their legal right to work and reside in a country they now call home. (At the time of our interview, the program was set to end for Hondurans in January 2020—a termination date which the government is now reassessing, see Jordan 2018a, 2018b; USCIS 2020.)

The case of Josué and Gloria highlights their agency as TPS-holding Hondurans in knowing how to navigate the exclusion and injustice in contemporary U.S. society that comes when the government attempts to dismantle a long-standing legal migration program. The couple moved to the U.S. from their homes just outside of El Progreso and La Ceiba, Honduras a little more than 20 years

ago. They have spent most of their time in the U.S. living in Washington state, where they are raising their son, who was born in south King County. At the time of our interview with Josué and Gloria they were forced to rearrange their long-term plans and negotiate uncertainty both in the U.S. and in Honduras, unsure of what they would do.

As researchers, we came to interview Josué and Gloria at their apartment. We arrived with coffee and pastries, which prompted a conversation about all the Honduran foods they long for, and what foods their families would prepare for them while growing up in the Honduran provinces of Yoro and Atlántida. It was Josué’s family relationships that brought him to Washington state. He had originally migrated to California, without paperwork, where he struggled to find dignified work. His brother told him about how much better things were in Washington state—how he had not experienced any encounters with ICE, experienced less competition with other undocumented migrants, and where the local economy seemed to be good. His brother was able to help Josué gain employment in a bakery, even though in Honduras Josué had worked in a carpentry workshop and had no prior experience baking. Josué spent six years working in this bakery, during which time he applied for and was granted TPS.

As his ties to the U.S. developed, his son was born in Washington state, and he and Gloria continued to make a life for themselves in Auburn—imagining their future in the Pacific Northwest. As Josué put it: “*Nunca nos sentimos más en casa que en los Estados Unidos, por los vínculos*” (We’ve never felt more at home than in the United States, because of our ties here) [Interview, July 27, 2018]. Josué emphasized to us that returning to Honduras right now could be very dangerous for him, telling us: “*No tengo miedo a la pobreza, sino a la violencia*” (I’m not afraid of poverty, but of violence) [Interview, July 27, 2018]. Josué’s comments prompted a conversation about the increase in gang activity in their hometowns of El Progreso and La Ceiba, realities that resonate with current

anthropological efforts to understand such everyday forms of violence (see, for instance, Wolseth 2005; Pine 2010; Phillips 2015, 2017).

At the time of our interview, Josué and Gloria continued to debate how to respond to the removal of TPS. They commented how their immigration lawyer reminds the couple that their son could soon sponsor their application for legal permanent residency; he was 19 years old at the time, and at 21 would become eligible. This resolution seemed more feasible to Josué than begging his employer to sponsor him to get a work visa; their main dilemma was thus in how to wait out the next two years [Interview, July 27, 2018]. Josué mentioned that if he were to be deported Gloria would stay with their son. Another option is simply to remain in Washington and “*vivir en las sombras*” (live in the shadows) as Josué put it [Interview, July 27, 2018].

During their interview the couple emphasized how they have never had any problems with the law or otherwise, and had never done anything that would bring any additional attention about their presence in the U.S. But they worry about the increase in xenophobia and vigilance over immigrants in general, especially Latinos with darker skin tones. In summer 2018 they were carefully thinking through potential worst-case scenarios, using their agency to read the political landscape and make decisions. If one of them were deported, this could negatively affect their eventual application for legal permanent residency. Despite the uncertainty that has overcome their future plans, Josué maintains a positive outlook on the situation. When he looked at their future he said, “*Yo no me veo deportado, yo me veo aquí. Nunca nos sentíamos mas en casa que en los Estados Unidos*” (I do not see myself deported, I see myself here. We will never feel more at home than in the United States) [Interview, July 27, 2018].

At the time of our interview, the couple had still not reached a decision about what to do. Josué talked with us about how all they could do right now was save up money. Josué’s goal was to therefore have at least two years-worth

of rent saved up for his wife and his son, and to simply spend as much time with his family as possible (fearing that he could be deported once his TPS was removed). Such precarity and uncertainty—of not knowing whether or not they should or would return to Honduras, or for how long—thus leads to less investment in Washington: Josué and Gloria no longer plan to buy a house, and their son's plans to go to college are now uncertain (despite his excellent grades and the likelihood he would get a scholarship and be academically successful in college). Paradoxically, they described a situation whereby Washington state has treated them well and allowed them to contribute to U.S. society as a whole. As Josué told us, "*Estamos en el lugar correcto. Nos sentimos parte de esta gran nación*" (We're in the right place. We feel that we are part of this great nation) [Interview, July 27, 2018].

Josué and Gloria continue to envision their future in Washington state and pursue goals toward that end, even though they understand that those plans are always tentative (just as the plans of so many Hondurans living in Honduras are tentative, albeit due to everyday violence and not the threat of deportation). They continue to paradoxically attempt to save for the unknown and also establish themselves in Washington state. As a result, the couple hasn't been able to send as much remittance money to Honduras as they once did—something that could complicate their possible forced return since their relationships there have changed over the years. Some people in Honduras have even blamed them for not helping out as much as they were expected to. In describing to us how they fear returning home empty-handed, Josué recalled a family member of theirs in Honduras whom he suspects would be upset with them if they were come back now. As Josué put it, they would tell them: "*Tanto tiempo allá y no me ayudaste... ¿ahora quieres venir aquí?*" (So much time over there and you didn't help me... now you want to come here?) [Interview, July 27, 2018].

Situations such as that of Josué and Gloria are illuminative of how the maintenance of some familial relationships are dependent upon

continued remittances that fulfill expectations of reciprocal relations. And yet, as the couple revealed to us, splitting their finances between Washington and Honduras has meant that there have been times in their lives when they had to "*vivir en lo escaso*" (live in scarcity), as Gloria put it [Interview, July 27, 2018]. They are already investing in a *potential* future in Honduras for themselves and their extended family members. Even though they would like to imagine their futures in the Pacific Northwest and have made concerted efforts to do so, they are now forced to divest from Washington state. The couple also fears returning to a context of everyday forms of violence and to a country they hadn't been living in for such a long time.

Political alliances and state practices continue to shift quite rapidly in Honduras, especially after the June 2009 military coup and the 10 years of post-coup militarized neoliberal governance (see Portillo-Villeda 2014a; Phillips 2015, 2017). Josué and Gloria have already been well established in the Pacific Northwest—having left Honduras in 1999. If deported, they would thus be forced to reintegrate into a society less familiar to them than when they left, where new social movements and struggle for change emerged in their absence. At the same time, post-coup policies of governance have significantly altered everyday life in Honduras. Josué and Gloria's story thus highlights how, while migrants do forge transnational ties of *some* kind, we would be incorrect to assume there is always a strong maintenance of family ties. Instead, their goals up until this point have always revolved around building a life for themselves in Washington state, and supporting their son's future in the Pacific Northwest.

The case of Josué and Gloria also highlights how while Central American transnational migrants may send remittances to help family members in their countries of origin achieve some kind of upward mobility (to open a business, or to remodel a house, for instance); they may also send remittances out of a sense of desperation for their loved ones' immediate wellbeing. Remit-

tances can also serve as a method for migrants to plan for their possible return by maintaining on good terms with friends and family in their countries of origin, thus strengthening their social safety-nets—a shortcoming that Josué and Gloria admitted to, because of their attempts to build a life for their families in the receiving country; they had hoped to send their son to college in Washington state. Other interviewees in our project also highlighted the paradox of on the one hand, feeling joy in being able to contribute to household expenses in their countries of origin, but also the burden of not having significant portions of their income for their increasingly costly living expenses in the U.S. Similar to Josué and Gloria, some interviewees also reflected on familial pressures they felt—the perceptions of their families about how “easy” life is in the U.S., and their own fears of negative repercussions if they do not meet expectations.

Discussion: Toward Ethnography of Central Americans Living in Post-2017 U.S.

The ethnographic stories recounted here add not only a voice to migrants but also a sense of their agency once living in U.S. society—perspectives that are often excluded from mainstream journalistic analyses of Central American “caravans,” detention centers, and deportations. Such processes are important to study, but we would be incorrect to assume the Central American migrant experience begins and ends with crossing an international border. Hondurans and Salvadorans are still making history once inside the U.S. The ethnographic record on their experiences is exceptionally limited to previous periods (exceptions include Coutin 2000, 2007; Schmalzbauer 2005; and Alvarado et al. 2017, but these studies either focus on Guatemalans, or date back to as early as the turn of the century). We continue to know even less about the everyday lived experiences of Central Americans in the Pacific Northwest. Contributions from political science and journalism provide valuable statistics about macro-level

political process, and news as stories unfold (e.g., Fabian 2018; PBS NewsHour 2018; TRNN 2018). We believe however that the ethnographic approach is best suited to say something of the lived experience of Central American migration to the U.S., and illuminate some of the complexities of everyday life for migrants who are already living in the U.S. While quantitative analyses are valuable, only the ethnographic method can relay the lived experiences of an undocumented person who navigates privatized healthcare to battle brain cancer; how a woman fleeing from domestic violence and entrusting the U.S. with her life and that of her daughter reacts to new criteria for asylum; or how a family is forced to divest from Washington with the news of the end to a long-standing legal migration program.

Our focus on the agency of Hondurans and Salvadorans illuminates how contemporary transnational migrants attempt to take control of their own futures through reflexive livelihood strategies as they strive to make a living and remain in the Pacific Northwest. Even though Laura is cognizant that there is no guarantee of approval for her asylum case, her persistence is demonstrative of her imagining of a future in the Pacific Northwest and her concerted actions toward achieving that goal. Yet if she doesn’t get her case approved then she will continue living “*en las sombras*” as Josué and Gloria are likely to do once their TPS is removed. They will continue to live in limbo status of “undocumented,” just like Mario, with an uncertain future.

Political crises and precarity continue in Central America as the neoliberal governments of Honduras and El Salvador reduce their expenditures in public services—and largely abandon the poor, government officials and elite members of the capitalist class alike are cognizant of the \$3.77 billion in remittance money that currently goes to Honduras and \$4.61 billion that goes to El Salvador every year (PEW Research Center 2019). In essence, we have a situation whereby Central American migrants in the U.S., often undocumented and working for meager wages, are subsidizing very basic public services in Honduras and El Salvador—by providing a steady

income via remittance money through which the poor in Central America can purchase their basic necessities. In this sense, it is doubtful that the Honduran or Salvadoran governments really want to stop all forms of out-migration—since it has become such a major source of revenue for Central American countries, just as it is doubtful that the U.S. government really wants to stop all undocumented migration as U.S. employers have seen advantages to hiring undocumented laborers—where they are often paid less, while laboring in undesirable or even dangerous working conditions.

Latin Americanist scholars have continually demonstrated the extent to which unemployment and precarious working conditions in Honduras and El Salvador are exacerbating problems of gang violence, and how state-sponsored repression has worsened in recent years (in Honduras, especially since the 2009 military coup which ousted the one government that advocated for reform programs for young men who resort to gangs). The increase in violence, unemployment, and political uncertainty in recent years has left so many Salvadorans and Hondurans in desperate conditions, while the various movements of popular resistance and struggles for social change (e.g., Portillo-Villeda 2014b; Phillips 2017) may very well seem foreign to someone like Josué or Gloria who has not been living through that rapidly shifting political environment. Work and social life in Central American societies thus revolve around the maintenance of relationships that in the case of long-term migration, are relations that can become altered—sometimes even severed when migrants do not remit as much money as expected. As the Trump administration continues to deport Central American migrants, more ethnographic research on the experiences of returnees is also needed to understand how exactly such individuals reintegrate into a society that in the eyes of the state is legally their country of citizenship, yet socially and culturally rather foreign after having lived in the U.S. for so long (see, for example, Golash-Boza (2013) on the alienation of returnees to El Salvador).

Hondurans and Salvadorans living in Central America are increasingly developing strategies for how to leave their country—amidst the U.S. response to militarize the border and separate families—and Salvadorans and Hondurans living in the U.S. are now developing adept strategies to remain here and successfully avoid their possible deportation. “*Se necesita ser astuto*” (one has to be astute), as Josué put it—a phrase that highlights the agency of the Central American migrant community. Amidst the violence and poverty augmented in Central America vis-à-vis neoliberalism, these individuals have used their agency to seek refuge not just anywhere in the United States, but in Washington state where they seek to establish themselves and invest their futures. Despite the constant threat of deportation for some, and the increase in xenophobia directed to all, Central American migrants are contributing to the economy of Washington state. Some are business owners who provide services and employment opportunities for the same Central American migrant community—while paying taxes, providing jobs, and contributing to long-standing traditions of becoming an entrepreneur in Washington state. Current attacks against this community hinder these efforts.

That the U.S. neglected asylum status to Salvadorans fleeing a civil war it contributed to, and that Honduran and Salvadoran citizens have been removed from TPS—a long-standing *legal* migration program—highlights how ruling elites in charge of governing can forge the conditions necessary to create what Nicholas De Genova has aptly calls, “the legal production of migrant ‘illegality’” (2002:429). The removal of this legal program, and the altering of qualifications for asylum, demonstrates how the state itself—as a set of governing institutions and as a culturally-produced idea—establishes what activities are considered within the realm of the “legal” or the “illicit,” highlighting how these very categories are subject to change:

“Illegality” is the product of immigration laws—not merely in the abstract sense that without the law,

nothing could be construed to be outside of the law; nor simply in the generic sense that immigration law constructs, differentiates, and ranks various categories of “aliens”—but in the more profound sense that the history of deliberate interventions that have revised and reformulated the law has entailed an active process of inclusions through “illegalization.” (De Genova 2002:439)

We are living in a moment of heightened nativism and increased xenophobia in U.S. society, which, while not limited to any one group is so often directed toward and experienced among the Central American community. U.S. state officials are thus currently forging the conditions of exclusions and “illegality” to which De Genova alludes, thereby criminalizing migration (cf. Heyman 1999). The irony is that the U.S. government has long been a proponent of open borders in Central America—that is, open to U.S. transnational corporations to do business abroad, and open to importing goods produced in Central America to be consumed in the U.S. It has thus been acceptable for capital and commodities to freely cross borders, but not for Central American people—to sell their labor in the U.S. or simply flee from violence.

At the same time that the U.S. forges conditions of illegality and deportability for Central American migrants, government officials in El Salvador and Honduras are aware of both the reasons why people are fleeing, and the high amount of remittance dollars that flow into the local economy. But with the Trump Administration, gone are the days when Central American governments can ask the U.S. to continue TPS for “development” or “humanitarian” reasons (see, for example, El Heraldito 2014, 2016a, 2016b, 2018, La Prensa 2014a, 2014b), as has consistently been the case since the programs’ passing in 1990. As self-serving governing officials in El Salvador and Honduras continue to be granted impunity for violent crimes (see, for instance, Phillips 2015), the individual migrants recounted here fled

a state unwilling to invest in social programs that could alleviate poverty, all while the Trump administration uses racist fear tactics to paint an image of a “crisis”—devoid of U.S. culpability in supporting neoliberal policies and the military industrial complex in Central America that has helped to create the very conditions from which people are fleeing. The discourse of migrants composing an “invasion” of the United States (Fabian 2019) is thus disconnected from the Salvadoran and Honduran realities of poverty and violence from which individuals are seeking refuge.

Engaged scholars of Central American migration must study these processes across space and be willing to take action, when asked to do so by the communities we work with. This may include serving as expert witnesses for asylum cases when migrants are faced with deportation orders, or becoming informed academic signatories for denunciation letters and reports that debunk the notion that the U.S. is unable to accept so many Central Americans, or that deterrence via the desert (or a wall) is an effective way to prevent migration (cf. De León 2015). Conditions in Central America would have to first improve in order to decrease the amount of people fleeing from violence and extreme poverty. Scholars of this process must therefore continue to make connections between the kinds of obstacles that the working class and peasantry face in Central America, and those that Salvadoran and Honduran migrants face once in route through Mexico. How these experiences may change under the government of Lopez-Obrador, and while crossing the actual U.S. border—amidst a fabricated “crisis” and proposed wall, will be a particularly fruitful area of research. But we also need more studies about the various obstacles Central Americans face once they are actually living and working here in the U.S., where they are still using their agency to make history.

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Salish Sea Islands Archaeology and Precontact History

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Abstract *Salish Sea islands archaeology and precontact history are reviewed in light of Coast Salish history and the archaeological record. Our examination shows that (1) the ancestors of Salish people have occupied and used the archipelago continuously since its formation approximately 14,000 years ago; (2) archaeological work has not been conducted uniformly across the archipelago, resulting in places, landforms and times being differentially represented; and (3) as an inland seaway the Salish Sea basin is more geographically similar to the Mediterranean than the Pacific.*

Keywords

Salish Sea, precontact, island and coastal archaeology

Introduction

In this article, the record of precontact human use and occupation of Salish Sea islands is examined in light of Coast Salish history and the archaeological record. We emphasize islands because they are culturally and ecologically unique places generally and because no regional synthesis has ever been undertaken of the Salish Sea archipelago. While we have chosen islands as our study universe, it is not the only one available and other prominent Salish Sea landscape features (e.g., mountains, foothills, rivers, deltas, inlets) would most assuredly tell different stories.

The largest marine embayment in western North America south of Cook Inlet, Alaska, and north of the Gulf of California, the transboundary Salish Sea encompasses portions of southwest British Columbia, Canada, and northwest Washington, United States (Figure 1). The sea is comprised of three distinct water bodies—the Strait of Georgia, Strait of Juan de Fuca and Puget Sound—that are variously protected from the open Pacific Ocean by the mountainous Olympic Peninsula and Vancouver Island. The inland sea connects to the Pacific Ocean via Johnstone Strait in the north and Strait of Juan de Fuca in the west.

The Salish Sea is ~450 km (280 mi) long and varies in width from >50 km (31 mi) in the north and <2 km (1.2 mi) in the south. It contains ~7,500 km (4,660 mi) of marine shoreline and hundreds of islands. The sea has a saltwater surface area of ~18,000 km² (7,000 mi²), with Puget Sound comprising ~2,500 km² (965 mi²) or 14% of the total area (Freelan 2009a; Quinn 2010). This means most of the sea (86%) occurs in the north (Strait of Georgia) and west (Strait of Juan de Fuca). For comparison, the Salish Sea is about four times larger than San Francisco Bay and about nine times smaller than the Gulf of California.

At ~110,000 km² (42,000 mi²), the Salish Sea basin—the watershed that surrounds and contains the Salish Sea—has approximately six times the surface area as the sea. For comparison, the Salish Sea basin is half the size of Great Britain (210,000 km² [81,000 mi²]) and three times larger than Vancouver Island (31,000 km² [12,100 mi²]), which defines the basin's northwestern boundary (Figure 1). Like its western boundary, the basin's eastern rim is mountainous, made up of the rugged Cascade Range and Coast Mountains.

The Salish Sea basin is unique for many reasons, including the degree to which its boundary corresponds with the historic dis-



Figure 1. The Salish Sea and its islands showing Northern, Central and Southern Salish Sea subdivisions and other localities mentioned in the text. Base map: ERMA 2015.

tribution of Indigenous Coast Salish speakers (compare Suttles 1990:ix and Freelan 2009b; see also Deloria 2012:5–7) for whom the sea is named (Tucker and Rose-Redwood 2015). In geographical terms, cultural boundaries are described as being “natural” or “geometric.” Natural boundaries are defined in relation to such prominent physical landscape features as rivers, mountains and watersheds and are commonly associated with Indigenous cultures (Berkes et al. 1998).

Alternatively, geometric boundaries, a European invention, follow the cardinal directions, running north-south or east-west (Harris 1997), those straight lines and rectilinear areas being the easiest for colonial states to survey and control. In this regard, modern political boundaries like the U.S./Canada border must be eschewed in studies of Coast Salish history. Rather, we consider the Salish Sea watershed an essential framework for the historical analysis of Salish Sea islands (Smith 1969; Berkes et al. 1998; Bentley 1999).

Archaeologically, the Salish Sea basin is the most intensively studied region in northwestern North America (Suttles 1990; Matson and Coupland 1995; Ames and Maschner 1999). This is, in part, a consequence of the basin's (1) uniqueness as a maritime cultural area, (2) high precontact population density, (3) high population density today (where more people = more construction = more sites discovered), and (4) high number of research institutions (there are four major research universities in the basin, all of which offer Ph.D.s in anthropology and/or archaeology). Although bisection of the Salish Sea by the U.S./Canada border has produced significant fragmentation of knowledge, researchers have in the past considered the basin a distinct geographical area (e.g., Smith 1907; Smith 1940; Mitchell 1971). Nevertheless, archaeological analysis today is rarely, if ever,

conducted at the Salish Sea basin-scale. However, a growing number of sub-regional areal studies undertaken in recent years suggests basin-scale analysis is both possible and warranted (e.g., Carlson 2008; Croes et al. 2008; Ames et al. 2010; Caldwell et al. 2012; Clark 2013; Croes 2015).

Orienting our island study are two Salish Sea chronologies (Figures 2 and 3). They are designed to situate islands within broader Salish Sea sequences and frameworks, not to replace other regional or local chronologies. They provide a framework for theorizing Salish Sea islands that reflects calls to move away from linear, progressive evolutionary models and toward identifying the "range of human behaviors as performed in time and space" (Davis 2011:18). In this case, the nexus of time and space is the island.

Salish Sea Chronologies					
This Study	Coast Salish Timeframe ^a	Geological Timeframe	Sea Level Position & Direction ^b	Archaeological Timeframe ^c	Date ^d
Salish Sea VI	Recent Time (Events easily remembered)	Late Holocene	Present Stable ^e	Contemporary	Present-1900 CE ^f
Salish Sea V			Present Stable	Historic	1900-1775 CE
Salish Sea IV	Age of Transformation (Transformers set things right and the world became stabilized)	Middle to Late Holocene	Present Stable	Gulf of Georgia Marpole Locarno St. Mungo/ Charles	1775 CE - 5 kya
Salish Sea III	Mythical Time (Time when things were in chaos)	Early to Middle Holocene	Lower Rising	Old Cordilleran Olcott-Casacade	5-10 kya
Salish Sea II		Holocene/ Pleistocene Boundary	Lower Dropping	Western Stemmed Point (WSP)	10-13 kya
Salish Sea I		Terminal Pleistocene Deglaciation	Higher Dropping	Clovis-WSP Pre-Clovis	13-17 kya
----- VASHON GLACIATION -----					16-19 kya
<p>a. Adapted from Reimer 2011:47 b. Position of sea level relative to present (Higher, Lower, Present) and direction of sea level change (Rising, Dropping, Stable) in the Central Salish Sea c. Archaeological units for the Central Salish Sea (after Mitchell 1990) d. kya = thousands of years ago; all dates and periods are approximated e. While sea levels have been stable for the past 5,000 years, they are now rising as a result of climate change. f. CE = Common Era (calendar years)</p>					

Figure 2. Salish Sea chronologies.

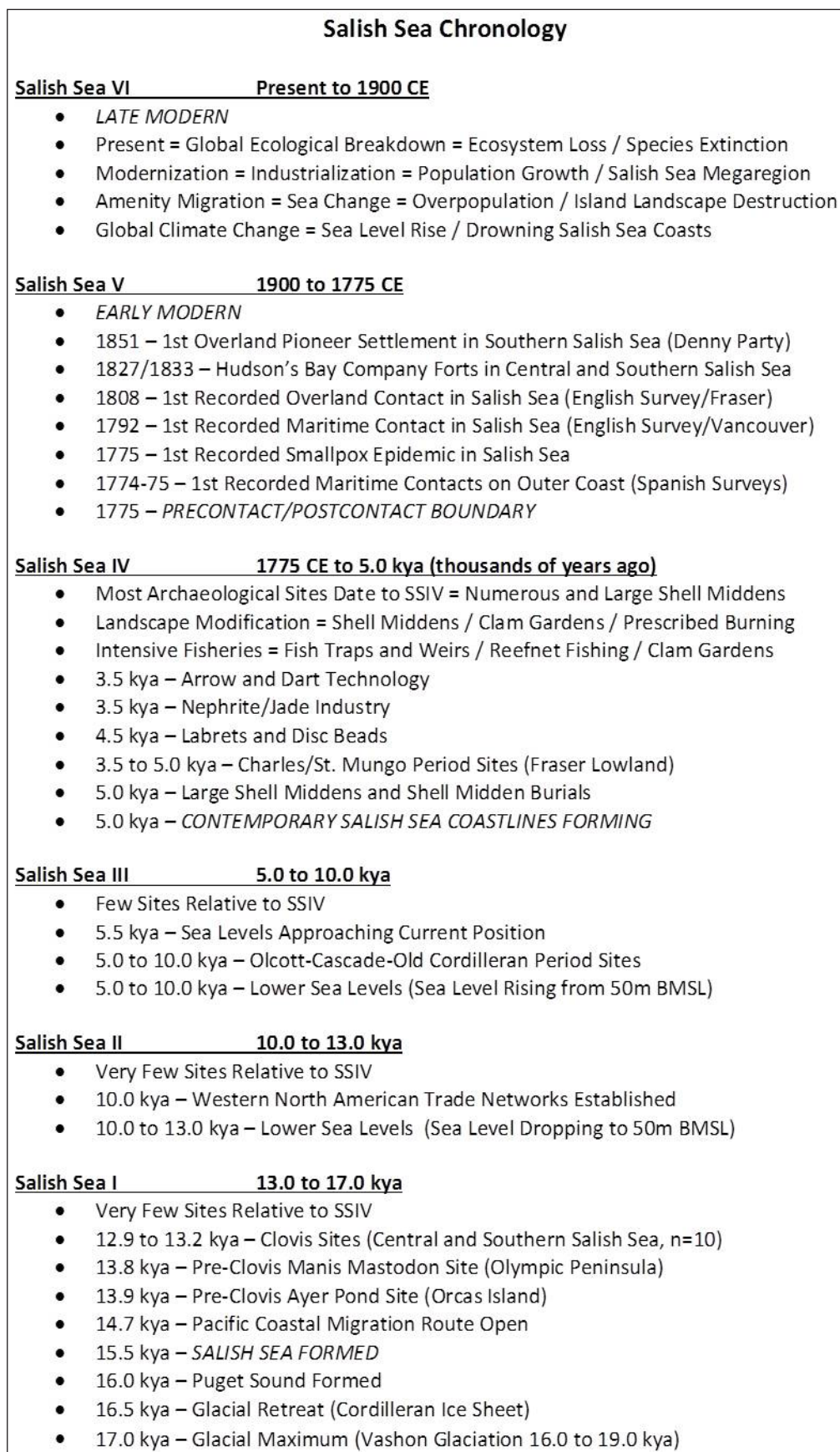


Figure 3. Salish Sea chronology.

The primary objective of this paper is to consider the temporal, spatial, and cultural range of precontact island use and occupation, recognizing that gaps in the region's maritime archaeological record exist (Moss and Erlandson 1998; Davis 2011; Elder et al. 2014; Wyatt 2015). In this light, our units of analysis (Salish Sea I, II, III, etc.) are best understood as analytic units rather than archaeological or culture history phases, although important correlations exist, as indicated.

We begin our survey by examining Indigenous perspectives of Salish Sea history. We then turn to the archaeological record. We conclude by synthesizing the two histories and considering the role of islands in the precontact Salish Sea basin.

Indigenous History of the Salish Sea

[E]xcept the tribes that lived far inland on the slopes of the mountains (called horse Indians by the water peoples), everyone lived by the water and used canoes to get around.
—Vine Deloria, Jr., 2012:5

The traditional histories of Indigenous Pacific Coast peoples document events from the recent or historic past to time immemorial. In addition to local and regional cultural information, traditional histories record such major geological events as earthquakes, tsunamis, landslides, floods, volcanic eruptions, sea level changes, glacial activity, and climate change, at a variety of temporal and spatial scales (McMillan and Hutchinson 2002; McLaren 2003; Thom 2003; Reimer 2011).

Anthropologists and archaeologists on the Pacific Northwest Coast have increasingly sought to align their work with traditional histories (Miller 2011; Gauvreau and McLaren 2016). For Squamish archaeologist Rudy Reimer (2011:18), incorporating oral history means accounting for an Indigenous perspective in archaeology, allowing for the possibility of an “integrated whole where oral history validates archaeology and archaeology validates oral history.” Studies of

Salish Sea oral histories demonstrate “an overall narrative that is consistent with [post-glacial] historical events described in paleoecological, geological, and archaeological studies” (McLaren 2003:201).

Our Salish Sea chronology (Figure 2) includes Reimer's tripartite Coast Salish (Squamish/*Skwxwú7mesh*) historical framework that includes “Mythical Time,” defined as an early period of chaos; a subsequent “Age of Transformation,” when contemporary Salish Sea landscapes formed; and postcontact or “recent” events (Reimer 2011:47). In Coast Salish histories, time beyond memory is “time immemorial.”

In Coast Salish histories, landscape features are linked to events related in historical narratives. These histories relate to a distant past, especially through the First Ancestors, the Transformer, and the Great Flood traditions; and to a recent past that links the present-day people to the First Ancestors. In Coast Salish histories the stories are evident in the landscape where various natural formations are reminders of the deeds and misdeeds of previous generations.

Coast Salish history is an oral history that distinguishes three types of historical accounts. Following Reimer, these histories include *sxwexwiyam* (mythical time), *xaay xets* (time of transformation), and *syets* (recent time) (Reimer 2011:46). The histories are told in the form of stories that relate a distant past that cannot necessarily be identified with a specific date in prehistory, but it is a past that was a time when the land and waters were populated, the waterscape and landscape created, and the first humans placed in their respective territories. From the perspective of the Coast Salish, these histories are evident from landmarks. Coast Salish histories speak of a way of life that explains and justifies their connection to their traditional homelands. To the Coast Salish these are true histories.

This view of history (or chronology) is consistent with the Coast Salish worldview insofar as it

encapsulates both time and place and considers the relations of humans to the non-human world, including real and mythical beings.... [T]his temporal arrangement meshes with previously defined archaeological time periods and geological events, such as late Pleistocene to early Holocene glaciations, subsequent sea level changes, river geomorphology, fluctuating tree line limits and changing climatic conditions. (Reimer 2011:46)

Reimer stresses that the eras described in these histories cannot be seen as depicting time in a linear fashion, rather the archaeological sites associated with these three times are still visited by *Skwxwú7mesh* and the historical narratives serve the purpose of “continual cyclical understanding, use and remembrance of these places.”

The three past events in Coast Salish histories overlap and interact outside of linear time. The first event was when the First Ancestors were placed at different locations by the Creator. The second era was the time of transformation, when *xeʔəl's* (the Transformer or Changer) made changes for the people. Coast Salish *xeʔəl's* histories are many and varied but they all relate to a time when humans and animals were indistinguishable. *xeʔəl's* taught the people certain skills, gave animals distinctive characteristics, turned evil people into stone, and made parts of the natural landscape useful for the people. *xeʔəl's* was usually benevolent but sometimes would punish those that he found offensive. *A Stó:lō Coast Salish Historical Atlas* identifies a number of Transformer sites (*Xá:ls* in *Halq'mélem*) in the Fraser River Valley, many of which are stone (McHalsie et al. 2001:6–7).

The second era concludes with the Great Flood. Great Flood stories are universal among the Coast Salish and can be found along the entirety of the North Pacific Coast. Most Great Flood events relate how some people survived by taking refuge in canoes that were tied to mountain tops. In some versions, when the flood receded the canoes settled in their traditional

homeland; in other versions, canoes drifted away and settled in new areas. The oral traditions of the Great Flood explain and validate Coast Salish connection with the land, explaining why the various First Nations are located where they are today and justify their claims as the original inhabitants of a territory. In a provocative article, McMillan and Hutchinson (2002) have related the Great Flood traditions to a 1700 tsunami; nevertheless, accurately dating the Great Flood is not important in Coast Salish world view. What is important is how the Great Flood event explains and validates claims to territory.

The final event is the period linking previous events with the present. The Coast Salish distinguish between narratives that speak of the earlier events, the “myth time,” and stories that speak of events that occurred in the time more customarily referred to as the “historic period.” Nevertheless these periods are a continuum in the history of the Coast Salish peoples. In relating the oral traditions referred to in *Skwxwú7mesh* as *syets*, or “recent time” the public recounting of the narrative requires that knowledgeable individuals be called as “witnesses” (the term for witnesses in *Skwxwú7mesh* is “*Ust'am*,” described as “history in action”) (Reimer 2011:28). *Syets* is called *sq'álq'əł* in Central Coast Salish languages. Customarily the speaker will preface the oration with validation like “the old people would say,” or “my grandmother told me,” or “this happened to me.” The purpose of the *sq'álq'əł* is to relate customs, referred to in English as “teachings,” “ways,” and family accounts that pass from generation to generation. It is these cultural traditions that constitute the essence of what it means to be Coast Salish. The witnesses verify and validate the narratives for accuracy and legitimize the telling of the history. The historical knowledge conveyed and passed on in oral traditions is critical to understanding the construction of a “narrative inheritance,” family histories that relate the times before and after living history.

Coast Salish histories do not distinguish between what Western science categorizes as “myth” and “history.” The assignment of “truth”

to one type of oral history as opposed to another is misleading. For Coast Salish people, all their histories are true histories and most features of the landscape are explained in these histories.

Consider, for example, this Great Flood story told by Ambrose Wilson about the Northern Salish Sea:

A man who was training for power dreamed that there was going to be a Great Flood. The man prepared for the Flood by making an anchor and a long cedar bark rope.

The Flood came. The man who had trained tied his canoe's anchor to the top of the high mountain on the mainland that is northwest of the entrance to Bute Inlet. *Pá7lhmin* (known in English as Estero Peak) is the name of this mountain. The entire area was covered in water and only this peak was still above the surface. Although other people had canoes, only this man had prepared an anchor and a rope that was long enough.

The people in the other canoes asked the man who had trained if they could tie their canoes to his, but only those who offered him wives or goods, such as mountain goat wool blankets, were allowed to tie up.

Finally, the flood waters went down. The only people who survived were those who were allowed to tie up to the trained man's canoe—the Homalco people are descended from them (others say that the Klahoose and Sliammon [*Tla'amin*] people are also descended from them).

Some of our people have seen the remains of the anchor and the anchor rope which have turned into rock and are lying on top of *Pá7lhmin* mountain. (Kennedy and Bouchard 1983:107)

Importantly, *xeʔəl's* (the Transformer) is involved in the task of island creation and transformation; for example,

A short distance from the coastline [*xeʔəl's*] anchored the former island of Tsawwassen to the bottom of the Strait of Georgia so that in time it would grow in size and join the mainland. Thus the world came to resemble its present order. (Oliver 2010:30)

Coast Salish histories are complex and imbued with local and regional meaning. We return to the subject in our synthesis, where we consider traditional and archaeological histories together.

Archaeology of Salish Sea Islands

Precontact Pacific Coast maritime cultures are characterized as being highly diverse and resilient (Turner et al. 2003; Deur and Turner 2005; Troster 2009; Mathews and Turner 2017), and islands are understood to play an important role (Erlandson 2001; Erlandson and Fitzpatrick 2006; Fitzpatrick et al. 2015). On the Pacific Northwest Coast between Alaska and California, islands are associated with boats (canoes), long-distance seafaring, migration, trade, fishing, and the development of so-called “complex” maritime cultures (Arnold 1995, 2005; Altschul and Grenda 2002; Ames 2002; Erlandson et al. 2007). Some of the oldest and most important archaeological sites and regions in western North America are islands and archipelagos, including On Your Knees Cave, Prince of Wales Island, Alaska, the Haida Gwaii archipelago, British Columbia (Fedje and Mathewes 2005); and the Channel Islands archipelago, California (Altschul and Grenda 2002; Jazwa and Perry 2013).

For such reasons, “island archaeology” is a distinct and well-developed archaeological subdiscipline globally (Fitzpatrick 2004; Rainbird 2007; Fitzpatrick et al. 2015; DiNapoli and Leppard 2018; Fitzpatrick and Erlandson 2018). One principle of island archaeology is

that small islands (Fitzpatrick et al. 2016) and submerged islands (Kealy et al. 2016) should be considered just as important as large and subaerially exposed ones. Another is that islands are culturally significant places, and not just for maritime cultures. The benefits of Salish Sea island use and occupation are numerous and significant:

Access to water for personal use and transportation is obviously a key factor in locating habitation sites. Equally important, however, is the flexibility provided for people situated near water bodies through ready access to different ecosystems, and therefore to a wider range of resources at various geographical scales than if they were situated within only one ecosystem. As well as providing opportunities to exploit resources from the juxtaposed major ecosystem types, ecological edges facilitate exploitation of a wide variety of microsites and habitat interfaces situated within or in association with the major ecosystems. For example, villages located at the ocean's edge will yield easy access to a diverse range of marine and shoreline habitats.

The epitome of this situation is the intertidal zone, renowned for the richness and productivity of its food resources. Immediately above and below the intertidal band are other ecological edge sites that are productive and culturally important, for example, sandy bays, lagoons, estuaries, tidal marshes, rocky headlands, and the like. On the landward side of the interface there is, almost invariably, proximity to freshwater wetlands of various types—creeks, rivers, marshes, fens, bogs, ponds, lakes, and sloughs—as well as to forests of various successional stages, depending upon the natural and

anthropogenic disturbance regimes, and on ecological composition, structure, and function based on topography, microclimate, and elevation. (Turner et al. 2003:444)

The location and composition of precontact island archaeological sites reflect peoples' preference for situating themselves on ecological edges—"In both ancient and modern times, it is these places that people are drawn to settle and make use of" (Turner et al. 2003:444). In terms of cultural identity and the development of place,

[p]eople of maritime circumstances engage with outsiders socially, and continuously incorporate elements of this contact into their own populace, and through contact and exchanges create a distinct community and identity located historically on sea and land. (Rainbird 2007:173)

History of Research

While a formal "island archaeology" research tradition has never been established for the Salish Sea, archaeologists have always been drawn to its islands (e.g., Smith 1907; Mitchell 1971, 1990; Nelson 1990; Stein and Phillips 2002; Taylor and Stein 2011). Consider, for example, San Juan Island, one of the earliest and most intensively studied Salish Sea islands. Archaeological research was first undertaken on San Juan Island in the late 1800s (Smith 1907:380–386), and in 1946 archaeologist Arden King excavated the Cattle Point shell midden on the island's south coast (King 1950), making it the first large-scale investigation of a single site on the Northwest Coast (Carlson 1990:108). Based on that work, King (1950) established a four-phase culture history of the region covering 9,000 years of human occupation and related his earliest component, the "Island" phase at Cattle Point, to the inland Archaic cultures of North America (Carlson 1979:5; Faith 2011:9). Adan Treganza led an archaeological survey of San Juan Island in 1947, identifying a total

of 52 sites, and in 1950 initiated excavation of the English Camp shell midden on the island's north coast (Faith 2011:10). In the 1950s, Roy Carlson expanded on King's culture history by comparing the San Juan Island record to the mainland Fraser River delta sequence (Carlson 1954, 1960, 1979). In the 1970s, large multi-season excavations were undertaken at English Camp, focusing on both pre- and post-contact components (Sprague 1973, 1976, 1983). Julie Stein revisited the English Camp shell midden throughout the 1980s and 1990s, resulting in the publication of three books: *Deciphering a Shell Midden* (Stein 1992), a technical text on shell midden archaeology; *Exploring Coast Salish Prehistory: The Archaeology of San Juan Island* (Stein 2000), an introduction to the history and meaning of Central Salish Sea archaeology; and *Is it a House? Archaeological Excavations at English Camp, San Juan Island, Washington*, an introduction to Salish Sea household archaeology (Taylor and Stein 2011).

Given there are hundreds of Salish Sea islands and thousands of island sites (see below), it is well beyond the scope of this paper to detail the research histories of specific islands or archipelagos. Those interested in Northern Salish Sea islands should consult Acheson and Riley (1979), Lepofsky and Caldwell (2013), and Lepofsky et al. (2015); for the Central Salish Sea, see Mitchell (1971, 1990), Blukis Onat (1987), Wessen (1986, 1988), Carlson (1990, 2008), Weiser and Lepofsky (2009), Ames et al. (2010), Taylor and Stein (2011), and Clark (2013); for the Southern Salish Sea, see Wessen and Stilson (1987); Nelson (1990), Stein and Phillips (2002), Croes et al. (2008), and Croes (2015).

The most influential publication on the archaeology of Salish Sea islands is Donald Mitchell's 1971 *Archaeology of the Gulf of Georgia Area, a Natural Region and its Culture Type*. To put his shell midden excavations at Montague Harbour, Galiano Island, Central Salish Sea, in regional context, Mitchell delineated the "Gulf of Georgia culture area" (Mitchell 1971:22, Figure 9) and its three archaeological subareas: Northern Gulf of Georgia, Southern Gulf of Georgia, and

Northern Puget Sound (1971:40–45). Mitchell compiled archaeological sequences from across the Salish Sea (1971:42, Table VI) and utilized a base map that included the entire sea and surrounding basin (1971:2–19), which he considered a "natural region." Mitchell can thus be considered one of the first archaeologists to define the Salish Sea basin as a framework of analysis. Nevertheless, he ultimately left southern Puget Sound out of his study as he deemed the area peripheral to his Montague Harbour site. For recent applications of Mitchell's ideas, see Grier (2003), Ames et al. (2010), and Clark (2013; cf. Morin 2014).

The most prolific author on the archaeology of Salish Sea islands is Julie Stein (Stein 1992, 1996, 2000, 2002; Stein and Phillips 2002; Stein et al. 2003; Deo et al. 2004; Taylor and Stein 2011). Her research covers islands in both the Central and Southern Salish Sea and examines in the greatest detail the most important, ubiquitous and visible type of island site—the coastal shell midden, discussed in detail below. Stein's work at the English Camp shell midden, for example, shows how the people living at this Salish Sea IV winter village harvested, processed and consumed salmon, flatfish (halibut or flounder), herring (or smelt), rockfish, dogfish, and ratfish, producing "millions of fish bones" (Stein 2000, 97). All local shellfish species were found, including mussels, cockles, horse clams, bentnose and sand clams, venus clams, and barnacles, demonstrating use of all available littoral ecosystems, from low energy, sandy beaches to high energy rocky beaches. Deer and elk were hunted extensively, as were ducks; herring and salmon were the dominant fish consumed. In the case of San Juan Island, Stein's work demonstrates clearly how every major island ecosystem, terrestrial and marine, was used.

Salish Sea Island Site Inventory

Given all this work, it is surprising how little is known about some key aspects of Salish Sea island archaeology, particularly at the basin scale. This includes, for example, the number

and distribution of precontact island sites. In response, and in addition to our Salish Sea chronologies, the Salish Sea island site inventory (Tables 1, 2 and 3) is an attempt to establish a baseline for Salish Sea island archaeology by quantifying (1) the number and distribution of Salish Sea islands and (2) the number of recorded precontact archaeological sites on each of those islands.

There are hundreds of islands in the Salish Sea. Most are uninhabitable, at least for any meaningful duration, and many are submerged twice daily by rising tides. To limit the scope of our inventory, we decided to count only “large” Salish Sea islands, defined as those ≥ 2 km long. Island length was confirmed manually and archaeological site data was obtained from British Columbia’s RAAD database and Washington state’s WISAARD database in late 2017 and early 2018, respectively. All site counts are considered best estimates. The northern half of Whidbey Island occurs in the Central Salish Sea while the southern half is in the Southern Salish Sea; for the purposes of our inventory calculations, the island is counted as occurring in the Southern Salish Sea. Vancouver Island was not included in the inventory. According to our inventory:

- There are 85 Salish Sea islands ≥ 2 km long, of which
 - 22% are in the Northern Salish Sea (19)
 - 60% are in the Central Salish Sea (51)
 - 18% are in the Southern Salish Sea (15)
- There are ~2,350 recorded precontact archaeological sites on those 85 islands, of which
 - 25% are in the Northern Salish Sea (599)
 - 61% are in the Central Salish Sea (1,437)
 - 14% are in the Southern Salish Sea (332)
- The islands with the most recorded precontact archaeological sites are

- 189 sites – Whidbey Island (437 km²), Central/Southern Salish Sea³
- 183 sites – Salt Spring Island (194 km²), Central Salish Sea
- 161 sites – Quadra Island (276 km²), Northern Salish Sea
- 143 sites – San Juan Island (143 km²), Central Salish Sea
- 98 sites – Gabriola Island (59 km²), Central Salish Sea
- 82 sites – Orcas Island (148 km²), Central Salish Sea
- 79 sites – Fidalgo Island (107 km²), Central Salish Sea
- 78 sites – Galiano Island (57 km²), Central Salish Sea
- 73 sites – Nelson Island (127 km²), Northern Salish Sea
- 62 sites – Cortes Island (125 km²), Northern Salish Sea / Lopez Island (77 km²), Central Salish Sea
- The islands with the most sites are all relatively large and occur in the Central and Northern Salish Sea; the largest Salish Sea island (Whidbey, 437 km²) has the most sites and the third largest Salish Sea island (Quadra, 276 km²) has the third most sites
- Of the 85 Salish Sea islands, five have no recorded precontact archaeological sites (Burrows, Hat, Ketron, Sinclair, Stretch), all of which occur in Washington state and three of which occur in the Southern Salish Sea
- On average, each Salish Sea island has 27 recorded precontact archaeological sites
 - The Northern Salish Sea average is 32 sites/island
 - The Central Salish Sea average is 28 sites/island
 - The Southern Salish Sea average is 22 sites/island

The appearance of a correlation between island size and number of sites is deceiving. Not all big islands have lots of sites and many small

Table 1. Northern Salish Sea Islands ≥ 2 km Long (n=19) and Their Number of Recorded Precontact Archaeological Sites

Island	# Sites
Quadra (CA)	161
Nelson (CA)	73
Cortes (CA)	62
Lasqueti (CA)	52
Hardy (CA)	45
Sonora (CA)	38
Denman (CA)	36
Hornby (CA)	19
Texada (CA)	19
Beaver (CA)	18
Read (CA)	15
Savary (CA)	14
West Redondo (CA)	11
Harwood (CA)	10
Stuart (CA)	7
South Thormanby (CA)	7
Redondo (East) (CA)	6
Maurelle (CA)	5
Raza (CA)	1
Total	599

Table 2. Central Salish Sea Islands ≥ 2 km Long (n=51) and Their Number of Recorded Precontact Archaeological Sites

Island	# Sites
Salt Spring (CA)	183
San Juan (U.S.)	143
Gabriola (CA)	98
Orcas (U.S.)	82
Fidalgo (U.S.)	79
Galiano (CA)	78
Lopez (U.S.)	62
Valdez (CA)	56
Portage (U.S.)	52
Prevost (CA)	41
North Pender (CA)	40
Gambier (CA)	37

Table 2. (cont.)

Saturna (CA)	33
Waldron (U.S.)	31
Shaw (U.S.)	30
Mayne (CA)	26
Thetis (CA)	26
Keats (CA)	24
Stuart (U.S.)	24
De Courcy (CA)	23
Cypress (U.S.)	20
Bowen (CA)	19
Moresby (CA)	18
South Pender (CA)	16
Henry (U.S.)	16
Sucia (U.S.)	15
Mudge (CA)	14
Newcastle (CA)	12
Sidney (CA)	11
Willy (CA)	11
Lummi (U.S.)	11
Patos (U.S.)	11
Decatur (U.S.)	10
Portland (CA)	9
Wallace (CA)	9
Reid (CA)	8
Tumbo (CA)	8
James (CA)	7
Parker (CA)	7
Samuel (CA)	7
Guemes (U.S.)	6
Blakely (U.S.)	5
Samish (U.S.)	4
Spieden (U.S.)	4
Penelakut (CA)	3
Johns (U.S.)	3
Anvil (CA)	2
Eliza (U.S.)	2
Protection (U.S.)	1
Sinclair (U.S.)	0
Burrows (U.S.)	0
Total	1,437

Table 3. Southern Salish Sea Islands ≥ 2 km Long (n=15) and Their Number of Recorded Precontact Archaeological Sites

Island	# Sites
Whidbey* (U.S.)	189
Camano (U.S.)	28
Bainbridge (U.S.)	26
Indian (U.S.)	24
McNeil (U.S.)	21
Vashon-Maury (U.S.)	19
Harstine (U.S.)	8
Squaxin (U.S.)	6
Anderson (U.S.)	5
Marrowstone (U.S.)	3
Blake (U.S.)	2
Fox (U.S.)	1
Ketron (U.S.)	0
Stretch (U.S.)	0
Hat (U.S.)	0
Total	332
*Whidbey Island is counted as occurring entirely in the Southern Salish Sea, even though the northern half of the island occurs in the Central Salish Sea	

islands have numerous sites. Camano Island, for example, is 246 km² and has 28 sites while Orcas Island is 148 km² and has 82 sites. Alternatively, the second largest Salish Sea island, Texada, is 287 km² and has 19 sites while Portage Island is 4 km² and has 52 sites. This shows how the Salish Sea archaeological record is as much a product of contemporary archaeological and heritage management practices (e.g., some islands have been more intensively documented, such as Portage Island) as it is a reflection of precontact human activity.

As shown in Figures 4 and 5, there does appear to be a strong correlation between the number of islands and the number of island sites, indicative of extensive precontact island use across the entire basin. What those sites are thought to represent is the subject of the next section.

The Archaeology of Salish Sea Islands

The basis for our treatment of Salish Sea island archaeology is our Salish Sea chronology (Figure 3), which begins with Salish Sea I.

Salish Sea I—17.0 to 13.0 kya. The earliest record of human activity in the Salish Sea basin comes from two ~13,900 year old sites in the Central Salish Sea: the pre-Clovis Ayer Pond site on San Juan Island and the pre-Clovis Manis Site on the Olympic Peninsula. Understanding these

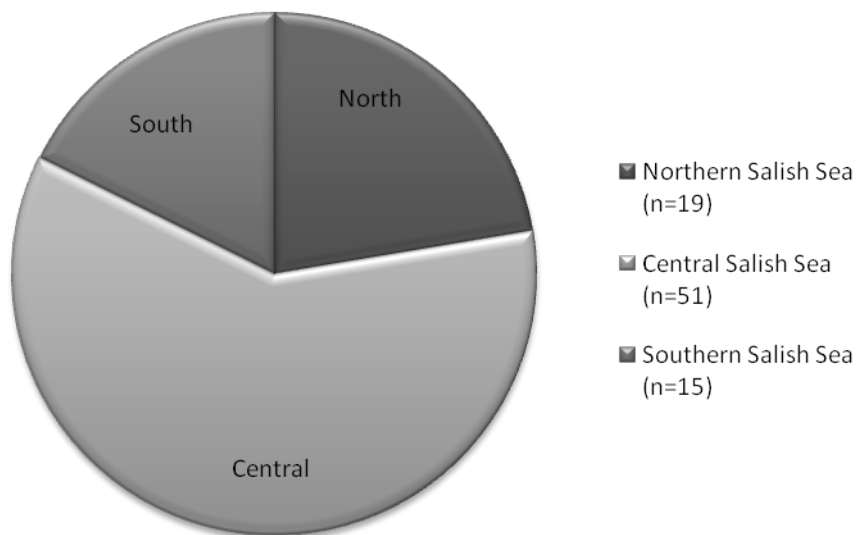


Figure 4. Number of Salish Sea islands ≥ 2 km long (n = 85).

Number of recorded island sites (n=2368)

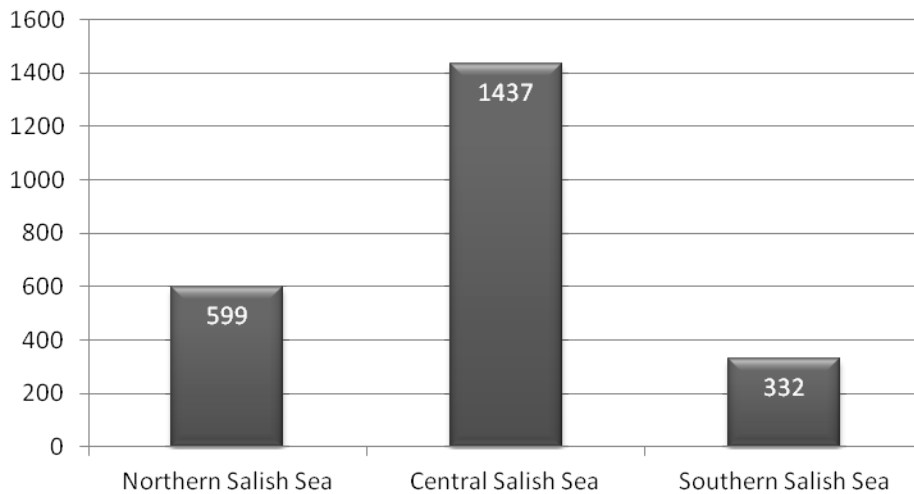


Figure 5. Number of recorded precontact archaeological sites on Salish Sea islands ≥ 2 km long.

early sites necessitates consideration of continental-scale socioenvironmental conditions, which is where we begin.

Discovery of $\sim 14,700$ and $\sim 14,100$ year old sites in Chile (Monte Verde) and Oregon State (Paisley Caves), respectively and in addition to other pre-Clovis sites, has forced archaeologists to reconsider the nature and timing of initial human occupation in the Americas (Pederson et al. 2016). Most archaeologists now recognize the continent's first occupants arrived not on foot via the interior "ice-free corridor" but by boat down the British Columbia coast (Erlandson et al. 2007; Sutton 2017), and that Clovis is not the oldest cultural horizon in the Americas, but is preceded by more than a thousand years of pre-Clovis occupation (Haynes 2015).

Although there is currently no physical evidence of pre-14,000 year old occupation in the Salish Sea basin, the upper limit of Salish Sea I is set at 17,000 years ago, at which point the Cordilleran ice sheet had reached its maximum extent in the basin. Access to the basin at this time was restricted by glacial ice (Booth et al. 2004), estimated to be 1.5 km thick in the Central Salish Sea and 1.0 km thick in the Southern Salish Sea, with the nearest ice-free land (tundra) located immediately south of the ice sheet. In light of Coast Salish traditional history, this could be

construed as time immemorial, for the Salish Sea and its islands had not yet been created.

Deglaciation was a relatively quick process, and Puget Sound was ice-free and inundated with marine water by $\sim 16,000$ years ago, followed by the entire Salish Sea a few hundred years later. There are thus four important basal dates for Salish Sea I: (1) 17,000 years ago, before which the basin was uninhabitable; (2) 15,500 years ago, before which there was no Salish Sea; (3) 14,700 years ago, at or before which people were able to move south down the British Columbia coast into the Americas; and (4) 13,900 years, the oldest site in the Salish Sea basin. As illustrated in Figure 6, the coastal or maritime entry route (Erlandson et al. 2007; Davis 2011; Erlandson and Braje 2011; Croes and Kucera 2017; Sutton 2017) makes the newly formed Salish Sea a maritime gateway to the continent's interior.

The earliest documented physical evidence of human activity in the Salish Sea basin is the $\sim 13,900$ year old Ayer Pond site on Orcas Island, Central Salish Sea, shown in Figure 7 (Kenady et al. 2010; Wilson et al. 2009). The site is noteworthy for two reasons, in addition to being the oldest Salish Sea island site. First, the site's age and location show Salish Sea islands were occupied immediately following their formation. Second, the earliest documented

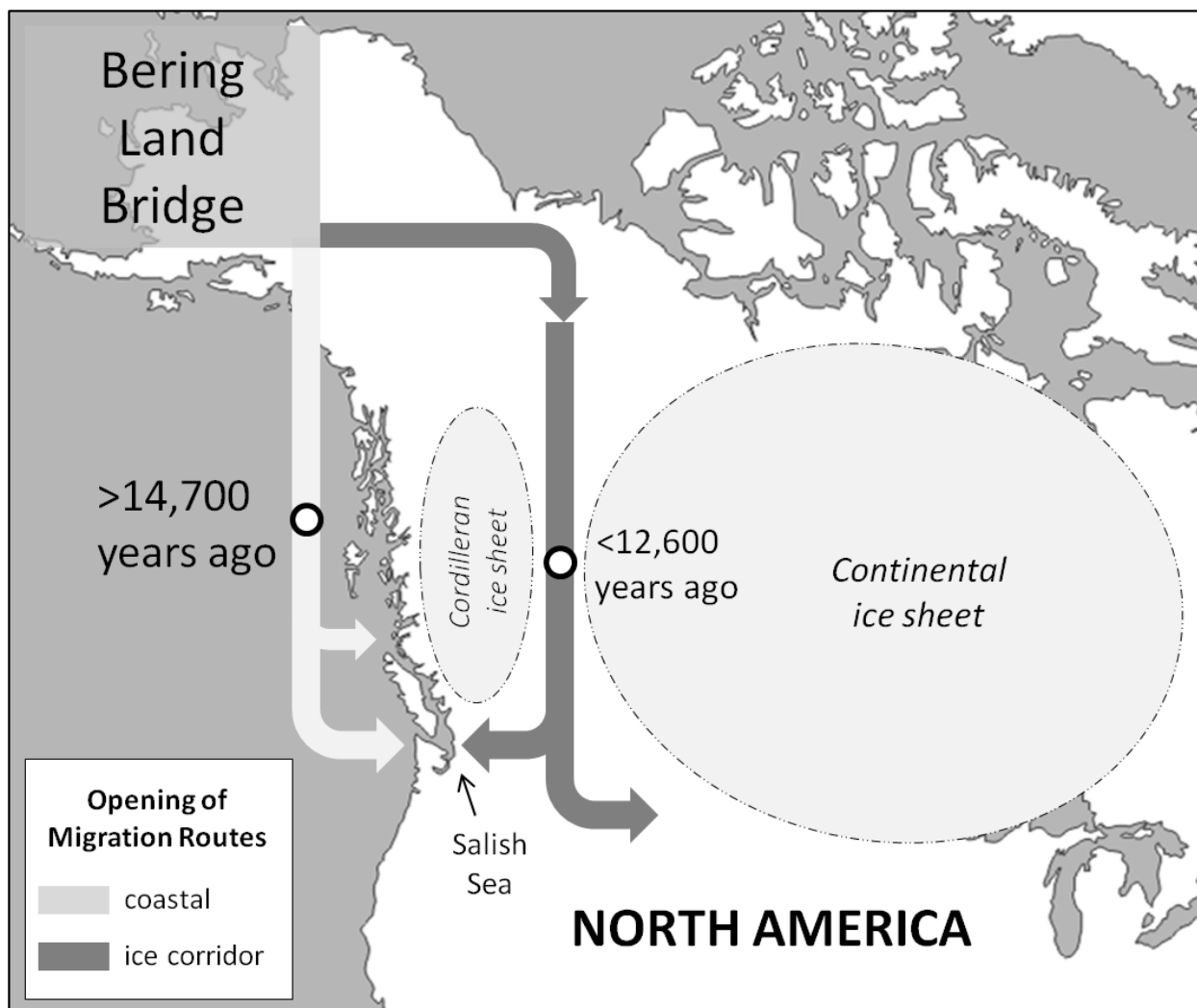


Figure 6. Opening of human migration routes into western North America (after Pederson et al. 2016).

evidence of seafaring in the Americas comes from southern California’s Channel Islands, with open water marine crossings occurring as early as 13,000 years ago (Erlandson et al. 2016). The Ayer Pond Site, almost 1,000 years older, thus represents the earliest evidence of New World seafaring (Erlandson 2018); the next oldest securely dated island site in the Americas is the ~13,000 year old Arlington Springs site—known for the “Arlington Springs Man”—located on Santa Rosa Island, Channel Islands.

Despite its island context, Ayer Pond is not maritime in the conventional sense. The site consists of an adult male *Bison antiquus* cranium and partial skeleton recovered from Ayer Pond wetland. Dating to ~13,900 years ago

(Kenady et al. 2010), the site is of paleontological interest because it provides further evidence of a postglacial tundra-like or meadow community and succeeding open pine parkland before ~13,000 years ago that supported bison in the basin (Wilson et al. 2009:49). When coupled with evidence of lower sea levels (Figure 7), these sites suggest Central Salish Sea islands acted as early postglacial land mammal dispersal corridors from the mainland to Vancouver Island, with reduced water barriers (crossings) between the mainland and islands. Dispersing ungulates such as bison, elk, and deer would have significantly influenced island vegetation establishment and early succession.

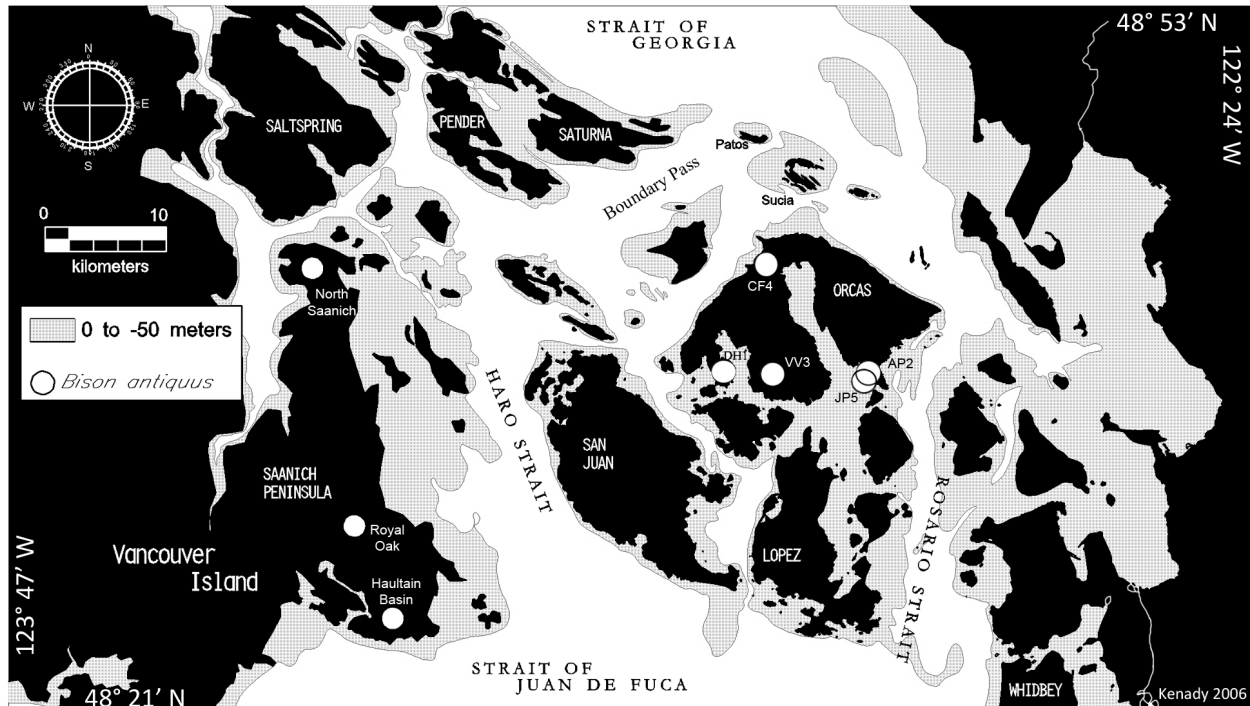


Figure 7. Generalized bathymetric map of -50 m contour for the Central Salish Sea indicating early postglacial viability for land mammal dispersal—thus availability for hunting—and known *Bison antiquus* localities, including the Ayer Pond archaeological site (AP2) on San Juan Island. Lower sea levels meant (1) there was significantly more coastal landmass dominated by open parkland, especially off the Saanich Peninsula (left), between Orcas and Lopez Islands (center), and between the Nooksack and Skagit River estuaries (right); (2) there were numerous landbridges connecting islands to the mainland (e.g., Lummi Island landbridge) and to each other (e.g., Orcas–Lopez landbridge, Pender–Saturna landbridge); and (3) an overall reduction in distance between islands. Source: Adapted from Wilson et al. 2009, 56, Figure 10; reproduced by permission.

The Ayer Pond bison is of archaeological interest because of evidence of human butchering, although no lithic artifacts or cultural features were found directly associated with the bison (Kenady et al. 2010:58). Evidence for human butchering includes fresh (“green” or “spiral”) bone fractures, percussion impact scars, cut-marks, and bone representation skewed toward less meaty cranial and distal limb elements, suggesting element selection. At least nine bison are documented from six Orcas Island localities (Wilson et al. 2009:50), with Ayer Pond being the only known archaeological site.

Co-occurring temporally and spatially with Ayer Pond is the non-island Manis Mastodon site, located $\sim 60\text{ km}$ (37 mi) southwest of Orcas Island on the northern Olympic Peninsula, Central Salish Sea. In the late 1970s, bones from

a single male mastodon (*Mammuth americanum*) with evidence of spiral fractures, cut marks, and other modification were recovered from a landowner-excavated backhoe trench (Gustafson et al. 1979). The only recorded artifact associated with the mastodon was a “foreign” bone fragment, interpreted as a “bone projectile point,” embedded in an ex situ rib fragment recovered from backhoe excavated sediments (Waters et al. 2011:351; cf. Haynes and Huckell 2016). In 2011, the bones were reanalyzed, including high-resolution CT scanning of the embedded bone “projectile point” and 13 AMS 14C dates, which returned an average of $\sim 13,800$ years old (Waters et al. 2011).

The identification of the “embedded bone projectile point” has since been called into question. According to C. Vance Haynes and Gary

Huckell's "backhoe hypothesis," the projectile may not be foreign but instead "the result of the backhoe mashing one piece of [mastodon] bone into another," reflecting how the "backhoe had cut right through the rib cage" (2016:190). Specifically, Haynes and Huckell question whether the embedded bone object shown in the CT scans reflects the morphology of known bone projectile points, noting how thin it appears to be (~2–3 mm), particularly relative to its depth of penetration (~22 mm) into vertebrate bone. The "backhoe hypothesis" only concerns the projectile point, not the ~14,000 year old date nor the evidence of butchering.

Other evidence of human activity in Salish Sea I comes from ten Clovis points found around the Central and Southern Salish Sea (Figure 1), some with little or no provenience, and two coming from islands: one from Whidbey Island (Croes et al. 2008:106, Figure 1) and one from Orcas Island (Kenady 2018). The highly distinctive fluted points do not occur in the Northern Salish Sea and are rare on the British Columbia Coast (Carlson 1990). In this regard, the Southern Salish Sea represents the northwestern limit of the Clovis tradition, which is securely dated to 13,000 to 12,600 years ago (Waters and Stafford 2014:543; see also Haynes 2015:134–135). Historically associated with big game hunting, particularly mammoth, the tradition is understood today to represent a diverse array of terrestrial and aquatic subsistence strategies, best understood not in terms of a carnivorous or vegetarian diet but as generalist omnivores (Haynes and Hutson 2014:305).

Salish Sea II—13.0 to 10.0 kya. We know of only one recorded island site that may date to the Salish Sea II period. The DeStaffany Site on San Juan Island is a lithic scatter located on a 25 m high bedrock outcrop overlooking what would have at the time been grasslands (Kenady et al. 2002, 2008). No faunal remains or features were recovered from the site, but it did contain a high concentration of finely made lanceolate projectile points and associated debitage. The site has not been radiometrically dated, but stylistically the

artifacts from the site are typical of the Western Stemmed Tradition (Davis et al. 2017), dated in other places at 13,000 to as late as 8,500 years ago. The site is thought to have been used by bison hunters who used the bedrock outcrop as a vantage point to look for game (Kenady et al. 2008). While there, they maintained and rejuvenated their hunting toolkits.

One recorded site from this period does not mean, however, that the islands were not used more extensively. Local landowners have found artifacts typical of this period on San Juan, Orcas, and Lopez Island as isolated finds in plowed fields and beaches (Kenady 2018). The formal recording of only a single site is likely the result of modern land ownership and cultural heritage management regulations rather than an actual lack of use during this period. The San Juan Islands are characterized by primarily rural residential and agricultural private properties, with no large parcels of federal lands and few large parcels of public land. Given this pattern of land ownership, the regulations that drive archaeological surveys in other parts of Washington state (due to either development or land management by public agencies) seldom apply to properties on the islands. If the San Juan Islands underwent the level of urbanization and land development as other areas around the Salish Sea, where sites from the Salish Sea II period are well represented (Chatters et al. 2011:29; Kopperl et al. 2015), it is likely that more sites would be encountered.

Given the artifact assemblage at the DeStaffany Site, which suggests big game hunting, and the occurrence of butchered *Bison antiquus* at Ayers Pond, it is likely that the peoples of the Salish Sea II period were utilizing the islands to hunt bison and possibly other large game, such as deer and elk. As sea levels continued to rise and islands shrank in size, this use of the islands may have diminished, particularly after the bison died off. Rising seas also had the effect of drowning Salish Sea II shoreline sites, as discussed in the next section.

While not apparent in the Salish Sea basin archaeological record, the Salish Sea II/III

transition coincides with the establishment of long-distance trade networks in western North America (Fitzgerald 2005). These networks, which connected Pacific Coast peoples with peoples living in the continent's interior, are known from inland sites containing *Callianax* (previously *Olivella*) *biplicata* shell beads procured along the Washington, Oregon, and northern California coasts (Smith 2016). Long-distance trade networks involving Salish Sea peoples were likely in place by the end of Salish Sea I, as indicated by the presence of Clovis artifacts in the basin (Haynes and Hutson 2014; Miller et al. 2014). Major corridors for trade and exchange between the Salish Sea basin and the interior include the Fraser River, located in the Central Salish Sea, and the Columbia River, located 110 km (68 mi) south of Puget Sound.

Salish Sea III—10.0 to 5.0 kya. Salish Sea III is represented by a handful of formally recorded island sites (Fedje et al. 2009:238), along with surface finds of artifacts known from private collections. The Bellevue Farm Site, also from San Juan Island, is a lithic scatter located both above and below the tidal zone at Westcott Bay. Much like the DeStaffany site, the Bellevue Farm site is an undated lithic scatter lacking features or faunal remains but containing large lanceolate bifaces and associated debitage. Stylistically, the bifaces from Bellevue Farm are later than those from the DeStaffany Site, and are more typical of the style known as Cascade or Olcott (see especially Stein 2000:16–19; see also Ozburn and Fagan 2010; Chatters et al. 2011).

An isolated lanceolate projectile point was found near Cascade Lake in Moran State Park in the interior of Orcas Island in 1980. In 1985, Gary Wessen examined the reported area and found no evidence for any other cultural materials. Stylistically, the projectile point is an Olcott/Cascade point and dates to Salish Sea III (Chatters et al. 2011). Such points are often found as isolates in interior areas, on raised terraces that were historically heavily forested (2011:30). These sites are thought to represent a hunting strategy that targeted large mammals

in the patchy ecosystems that were developing on glacial outwash deposits and hillslopes in the early to mid Holocene.

The low number of recorded Salish Sea III island sites is likely due to the same factors described for Salish Sea II, including the drowning of coastline sites by rising seas. Landscape submergence has long been offered as an explanation for the low number of early sites (Grebmeier 1983; Reinhardt et al. 1996), and the subject has received increased attention in recent years (Locher 2006; Fedje et al. 2009; Grier et al. 2009; Mackie et al. 2011; Mackie et al. 2014; Wyatt 2015).

Salish Sea IV—5.0 kya to 1775 CE. Salish Sea IV is by far the most intensively studied period. There are two main reasons for this. First, stable mid- to late Holocene sea levels mean coastal landscapes used and occupied during this time are more likely to be located at or near today's shoreline, not submerged offshore as drowned sites or stranded in the forest as elevated sites, as is the case with older sites. As such, Salish Sea IV sites are much easier to find and study.

Second, Salish Sea IV has more and larger sites than previous periods. While many explanations have been given for this difference—for example, environmental change, technological change, changes in settlement strategies, population growth, the development of “social complexity” (Croes and Hackenberger 1988; Matson and Coupland 1995:146–154; Ames et al. 2010; Clark 2013:41–73)—there is little to no agreement on which are correct or what they actually mean (Morin 2014). Nevertheless, the stabilization of previously dynamic sea levels at the onset of Salish Sea IV constitutes a significant moment in Salish Sea precontact environmental history, as it corresponds to the formation of biologically productive and diverse littoral and estuarine ecosystems (Hutchings and Campbell 2004).

Rather than approach Salish Sea IV chronologically, it will be more productive to take a thematic approach. The following seven themes are addressed here: shell midden, fauna, technology,

symbolism, constructed landscapes, managed landscapes, and households and villages.

Shell Midden

The most common and studied type of precontact Salish Sea island site is the shell midden (Stein 1992, 1996, 2000; Belcher 1998; McLay 1999; Stein and Phillips 2002; Stein et al. 2003; Mather 2009). Technically speaking,

Shell midden refers to anthropogenic deposits containing noticeable amounts of shell, that is, calcareous invertebrate tests. Such deposits are common in marine coastal areas from subarctic to tropical latitudes throughout the world.... Shell midden is not an analytically rigorous term in the sense of human activity, but a descriptive label that identifies the most superficially recognizable constituent of the deposit. Shell-bearing site is a more accurate label, although cumbersome and not likely to be adopted. (Campbell 2005:870)

Sarah Campbell notes the term midden originally meant domestic refuse deposited around a house, and the addition of shell-bearing to the concept “further obscures the importance of other constituents of the deposit” (2005:870):

These sites do not solely represent shell-gathering activities by people. Other food remains such as fish or plants may be very abundant and may, in fact, represent more significant economic activities at that location, but such remains generally require screening or microscopic analysis of samples to identify.

A unique quality of shell middens is the alkaline environment created by the leaching of calcium carbonate from the shell into the soil (Stein 1992). This acts to preserve organic materials such as antler, shell, and bone, which would otherwise disintegrate quickly in the acidic soils typical of Salish Sea islands.

This includes human remains (burials), which are commonly associated with shell middens (Wessen 1986, 1988). In this regard, many Salish Sea shell middens are Coast Salish cemeteries (McKay 2002).

A Salish Sea island shell midden is shown in Figure 8a. Located in the Canadian Gulf Islands, Central Salish Sea, this site contains highly distinct pockets of whole shell, partly crushed shell, and finely crushed shell, representing a wide variety of depositional and post-depositional processes (Figure 8b) (Stein 1992, 1996; Stein et al. 2003). Like at English Camp, this island site contains evidence of sea urchin harvesting, and the urchin spines are often found nested in whole clam shells, like those shown in Figure 8b. Presence of sea urchins is significant because they only occur on rocky coasts with high-energy waves, indicating human use of this particular ecological niche (Stein 2000:97).

Salish Sea island shell middens represent a wide variety of activities, ranging from temporary, seasonal procurement, and processing camps (meaning fewer people and days/weeks of activity) to semi-permanent villages (meaning more people and months of activity), and a midden's size and contents are understood to reflect these differences. Accumulation rates vary widely for shell middens, making them difficult to date and interpret (Stein 1992, 1996, 2003).

The ubiquity of island shell middens in the Salish Sea basin relative to other site types is demonstrated in a 1987 study of the archaeological sites of five northern Puget Sound counties. Of the recorded sites, 87% were shell midden sites, with 44% located on mainland coasts and 42% located on island coasts (Blukis Onat 1987:26–27, Table 7). In other words, as recorded at this time in this part of the Salish Sea basin, more than 85% of all sites were shell middens and more than 40% of all sites were located on island coasts.

Although high, this distribution of shell-bearing versus non shell-bearing sites approximates a study of San Juan County (Orcas, San Juan, Lopez, and surrounding islands), where about 90% of all recorded sites were found to be shell

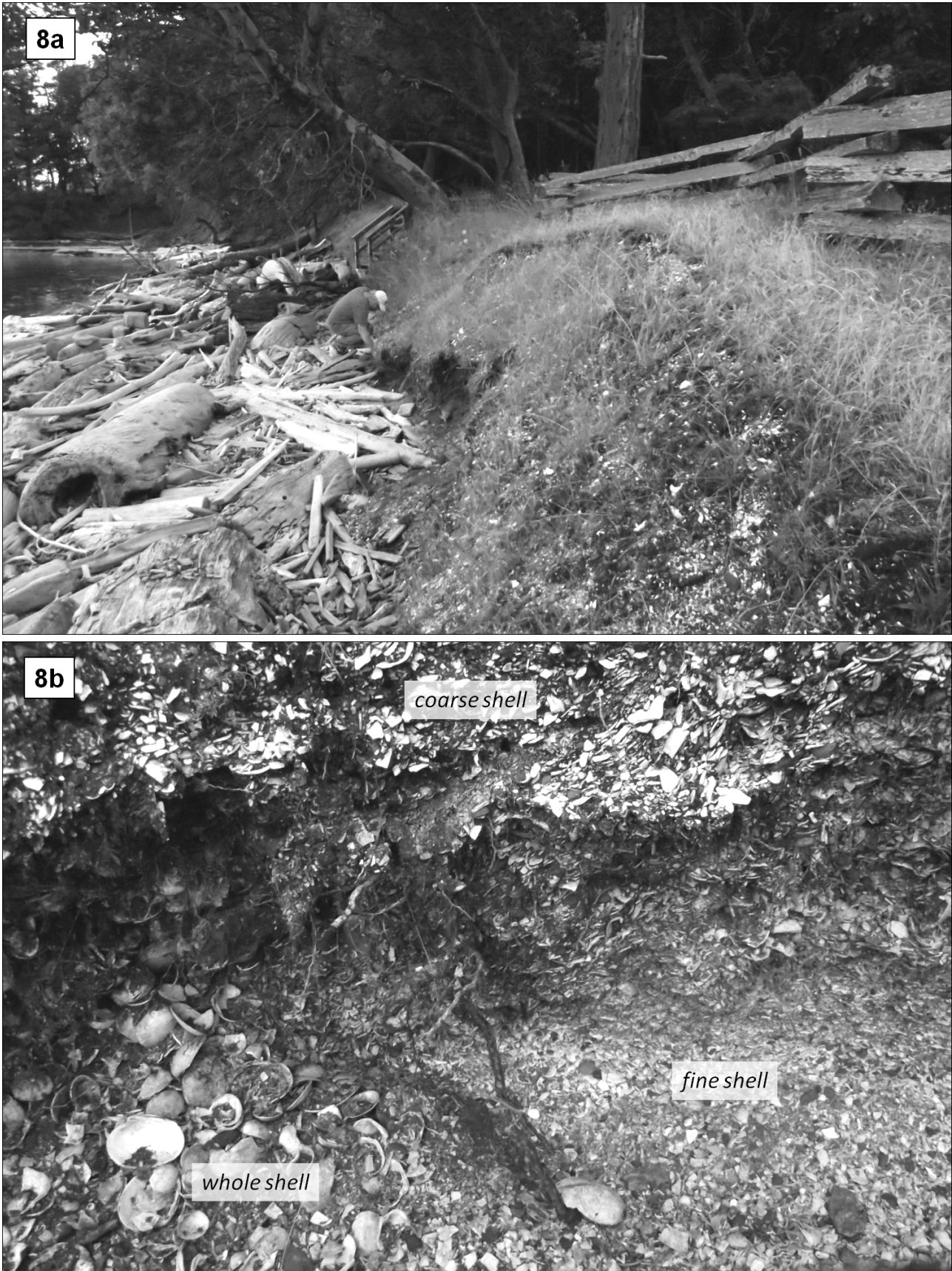


Figure 8. A Salish Sea shell midden in landscape view (8a) and close up (8b). Photos by Marina La Salle; reproduced by permission.

middens (Wessen 1986:40). A study of Island County (Whidbey Island, Camano Island) found about 80% of all recorded sites to be shell middens (Wessen 1988:64). A more recent study on the *shíshálh* Coast shows 76% of sites to be shell middens (Hutchings 2017). In terms of island shell midden, the conclusions of the Island County study are perhaps most telling:

The clearest pattern apparent in the reported data is that shell middens exhibit a strong association (70 out of 87) with protected shorelines. This condition is particularly apparent at Penn Cove on Whidbey Island, as this large protected harbor contains 30 of the 87 shell midden sites. Almost half (37) of the shell midden sites are associated with erosional beaches. Among depositional beaches, another 35 such sites are located on cusped forelands, near lagoons, or near extensive tideflats. There does not appear to be a strong association with deltas or stream mouths, but as these areas are uncommon in Island County this is not surprising. (Wessen 1988:35)

Fauna

A result of their unique geochemistry, island shell middens represent an important record of precontact economic strategies. As described earlier for English Camp, a wide range of resources and ecosystems can be represented at a single site. In addition to those resources already described (i.e., fish and shellfish), Salish Sea IV island sites regularly show evidence of terrestrial big game hunting, sea mammal hunting (McKechnie and Wigen 2011), and bird hunting (Bovy 2007). The following, for example, were recovered from a shell midden at the Dionosio Point locality, Galiano Island, Central Salish Sea (Hopt and Grier 2018:10–11, Table 1):

- Terrestrial mammal: deer, black bear, elk (wapiti)

- Sea mammal: pinniped, harbor seal
- Fish: herring, dogfish, salmon, rockfish, cod, hake, pollock, sculpin, perch, halibut, sole, flounder
- Bird: ducks, swan, geese, common murre, Canada goose, Brandt's cormorant

While all these resources (and more) were used at one time or another in previous periods (Ames 2005:99), what is unique about Salish Sea IV is that some came to be used more intensively, a process described as “intensification” (Ames 2005; cf. Smith 2005). Intensification refers to the intensification of food production, which involves increasing the amount of the food being produced (Ames 2005:75). To intensify salmon, for example, is to increase the amount of salmon being harvested relative to an earlier period. Numerous island resources appear to have been used more intensively during Salish Sea IV than previous periods, including fish, shellfish, and various plant species (Hewes 1973; Stewart 1977; Boxberger 2000; Butler and Campbell 2004; Deur and Turner 2005; Weiser and Lepofsky 2009; Caldwell et al. 2012; Lepofsky and Caldwell 2013; Greene et al. 2015; Lepofsky et al. 2015; see especially Mathews and Turner 2017:174, Figure 9.2). Nevertheless, while changes in animal use are evident during Salish Sea IV, “the overall record is characterized by stability rather than change” (Butler and Campbell 2004:327).

Technology

The constituents of Northwest Coast shell middens are often described in terms of “stone, bone, antler, and shell” (Stewart 1996). Stone, bone, and antler are particularly important because they are primary raw materials for tool-making, thus technology. Plants are another primary source of raw material (Gunther 1995; Stewart 1995; Turner 1998), but because they do not preserve well they rarely appear in the archaeological record, making such finds all the more significant (Bernick 1998; Croes 2015). In contrast, stone tools and the debitage resulting from their manufacture are the most common

artifacts found in Salish Sea sites due to their extreme durability in nearly all site conditions.

The most commonly studied stone tools in the Salish Sea basin are projectile points (e.g., Mitchell 1971; Close 2006; Carlson 2008; Croes et al. 2008; Eldridge and Stefen 2008; Keddie 2008), and recent analysis of Central Salish Sea island projectile points indicates “cultural and ethnic continuity” after 5,000 years ago (Carlson 2008:157). Another major study of Salish Sea IV technological change concludes that after 5,000 years ago sites in the southern Strait of Georgia

experience the beginnings of a major transition that see the gradual increase in the variability of artifact assemblage compositions. Also, the first major investment in ground stone technology at 4,500 [years ago] is through an abrupt introduction of beads into the archaeological record only to be followed by a gradual increase in the proportion of ground stone tools related to subsistence resource extraction. However, chipped stone continues throughout the entire time span, often in considerable proportions, and after 3,000 [years ago] the variability in assemblage compositions encompasses the entire range of chipped stone, ground stone, and faunal tools. (Ames et al. 2010:54)

According to the study’s authors, this evidence of “in situ cultural evolution” should not be surprising given “known ethnographic data demonstrating a seasonally variable resource strategy that involves considerable diversity in the technology used to carry out various activities” (Ames et al. 2010:54). The timing of the introduction of arrow technologies (bow and arrow) and ground stone celts (jade/nephrite adzes) in the Salish Sea basin is set at 3,500 years ago (Morin 2015, 2016; Rorabaugh and Fulkerson 2015).

Barbed points of bone and antler served multiple purposes, from waterfowl and marine mammal hunting to salmon fishing (Rorabaugh

2010). Recent analysis of 593 barbed bone and antler points from 56 dated archaeological sites in the Central Salish Sea show they range in age from 5,500 years old to contact, with most dating to the past 2,500 years (Rorabaugh 2012:19). Central Salish Sea barbed points show “continuity in this mode of learning over the past 5,000 years” (2012:17).

Symbolism

Because so many Salish Sea island shell middens are cemeteries, it should come as no surprise they contain cultural elements that are both sacred and symbolic (McKay 2002; Mathews 2014). Associated with some interred individuals—the ancestors of living Coast Salish peoples—are disc beads and labrets (lip plugs), both considered by archaeologists to be symbolic of social status (Ames and Maschner 1999:188–189; Coupland et al. 2016). Both appear in the archaeological record around 4,500 years ago, roughly contemporaneous with the appearance of large shell middens and shell midden burials.

Around 3,500 years ago, lip plugs (labrets) made of stone, bone, antler, and shell became prolific on the Northwest Coast, nowhere more so than the Central Salish Sea. A recent study of the symbolic nature of labrets and body modification found that over 30% of the Canadian Northwest Coast sample studied (n=220) came from the Central Salish Sea (La Salle 2013/14). The Pender Canal site on Pender Island, for example, has yielded both labrets and dental evidence of labret-wear on ancestral remains.

Certain forms such as the knob, disc, and pendant labrets, as well as those made of shell, coal, and soapstone/steatite, occur more frequently on the southern Pacific Northwest Coast, particularly in the Central Salish Sea (La Salle 2013/14). Soapstone/steatite knob and disc labrets are particularly indicative of Gulf and San Juan Island labrets, although more labrets overall have been recovered in the Canadian Gulf Islands than in the San Juan Islands in Washington state (Shantry 2014).

Labrets were used contextually to communicate social identity of various kinds including status, gender, and kin relations, at multiple scales such as household, village, and cultural-linguistic group (La Salle 2013/14). Communicating both solidarity and difference, labrets eventually fell out of use in the Central Salish Sea around the same time cranial modification became a widespread practice, approximately 2,500 years ago (Rorabaugh and Shantray 2017, Figure 1).

Perforated disc beads, like labrets, became prolific on the Northwest Coast around 3,500 years ago. While most disc beads are associated with burials, at some sites they have appeared in non-burial contexts. Study of the latter has led to the suggestion that early Salish Sea IV beads “sometimes adorned living bodies” (Coupland et al. 2016:310–311); indeed,

It may be useful to consider disc beads as bodily adornment in conjunction with labrets. Strung in necklaces or sewn on to garments, beads were worn on the body, while labrets were inserted in the body. Evidence for widespread use of both forms of adornment in the Salish Sea region, beginning about 4,000 [years ago] or soon thereafter, suggests growing concern with “body value” at this time, linked to emerging forms of personhood and social identity.

According to the study’s authors, the production and exchange of disc beads has implications for understanding the emergence of wealth-based inequality in the Salish Sea (2016:312).

Constructed Landscapes

Researchers are increasingly looking beyond artifacts and sites to consider constructed landscapes (Deur and Turner 2005; Thomas 2006; Weiser and Lepofsky 2009; Caldwell et al. 2012; Lepofsky and Caldwell 2013; Lepofsky et al. 2015; Mathews and Turner 2017). This means, for example, seeing a shell midden as

something more than an assemblage of artifacts (i.e., an archaeological site). We have already mentioned one way of seeing a shell midden as a landscape—that is, as a Coast Salish cemetery. Another way to see island shell middens is in terms of forest ecology:

Human occupation is usually associated with degraded landscapes but 13,000 years of repeated occupation by [Indigenous Pacific Coast peoples] has had the opposite effect, enhancing temperate rainforest productivity. This is particularly the case over the last 6,000 years when intensified intertidal shellfish usage resulted in the accumulation of substantial shell middens. [S]oils at habitation sites are higher in calcium and phosphorous. Both of these are limiting factors in coastal temperate rainforests. Western redcedar (*Thuja plicata*) trees growing on the middens were found to be taller, have higher wood calcium, greater radial growth and exhibit less top die-back. (Trant et al. 2017:1)

In a nutshell, “[d]isposal and stockpiling of shell, as well as the cultural use of fire, altered the species composition of the forest and understory in and around habitation sites” (Trant et al. 2017:2). The Pacific Northwest Coast is the first known example of long-term intertidal resource use enhancing forest productivity.

Another important constructed landscape is the clam garden (Williams 2006; Caldwell et al. 2012; Groesbeck et al. 2014; Lepofsky et al. 2015). These landscape features consist of rock boulder walls constructed near the zero tide line that create “a terrace on the landward side of the wall that significantly expands bivalve habitat and productivity through a variety of abiotic and biotic mechanisms” (Lepofsky et al. 2015:236).

Found along the Northwest Coast from Alaska to Washington, clam gardens are most numerous around northern Vancouver Island

and in the Northern Salish Sea (Lepofsky et al. 2015:243–244). On Quadra Island, Northern Salish Sea, 133 clam gardens have been recorded over 111 km of shoreline surveyed; in the Central Salish Sea there are 60 clam gardens recorded over ~650 km of shoreline; no clam gardens are recorded in the Southern Salish Sea (Lepofsky et al. 2015:259). Clam gardens are important because they reflect ancient maricultural practices that provide evidence of “habitat enhancement and creation to enhance bivalve production” (Lepofsky et al. 2015:253). More than just a technology or constructed landscape, clam gardens constitute a highly unique form of resource management.

Managed Landscapes

Concomitant to the landscape emphasis has been a focus on precontact island and marine ecological management strategies (Blukis Onat 2002; Mathews and Turner 2017), particularly in the Central and Northern Salish Sea (Thomas 2006; Weiser and Lepofsky 2009; Caldwell et al. 2012; Lepofsky and Caldwell 2013; Lepofsky et al. 2015; see also White 1980, 1999; Turner 1999; Turner et al. 2013). A recent review of these and other Northwest Coast studies identifies ten precontact landscape management strategies (Mathews and Turner 2017:175–176, Table 9.1):

- Landscape burning: Burning of prairies to promote growth of camas, deer forage, etc.;
- Clearing, cleaning: Manual removal of large rocks, driftwood, etc. from estuaries, clam beaches, canoe runs;
- Habitat creation, extension, or alteration: Creating new habitats through rock and log terracing and ditching in clam gardens;
- Bounding of resource areas: Laying of plot boundaries or establishing borders, in crabapple trees, edible red laver seaweed picking areas, clam gardens;
- Tilling soil (usually with digging stick): Tilling aerates soil, enhances moisture penetration, and helps recycle nutrients;

- Dissemination: Planting or scattering seeds, fruits, or other propagules;
- Transplanting: Moving young fish, larvae, root fragments, etc. from one location to another, including transplanting salmon eggs, herring eggs, spawning herring and eulachon, clover rhizomes, riceroor bulbs, and possibly seaweed;
- Selective, partial, rotational, or nondamaging harvesting: Taking only a portion of a plant, or only some individuals from a population, seaweed, kelp fronds, kelp stipes, shellfish;
- Fertilizing, mulching: Adding nutrients or moisture retaining materials to soil; and
- Feeding: Providing food for growing fish; putting fishguts, bones, and dead salmon back into the river to nourish young fish, crabs, etc.

Islands are believed to have played a unique role in the husbandry of Salish wool dogs, as represented at Ozette Island, Bainbridge Island, and probably Squaxin Island in the South Salish Sea (Croes 2014, 2015). On islands, the specially-bred wool dog would have been kept separate from larger “village” dogs and systematically sheared to create blankets made with wood, bone and stone spindle whorls and woven on looms. To make the best hair, wool dogs are said to have been fed the highest quality foods—i.e., fish.

These management strategies reflect “long-standing social–ecological systems in place for at least several millennia” that enhanced people’s dietary diversity and food security, provided products for trade and exchange, and underpinned the complex ceremonial and socio-economic systems that characterize Northwest Coast cultures (Mathews and Turner 2017:194).

Households and Villages

Arguably the most human aspect of precontact Salish Sea island life—the household—is the least studied archaeologically. This is due in

part to the complex and highly variable natural and cultural processes associated with shell midden formation (Stein 1992, 1996; Taylor and Stein 2011). Counter-intuitively, small, thin shell middens can represent long periods of human activity, while large, thick shell middens can represent very short periods, resulting in the “big sites–short time” dilemma (Stein et al. 2003). Problematically, the only solution to the accumulation rate problem—obtaining dozens of radiocarbon dates costing tens of thousands of dollars— is cost-prohibitive, meaning the large, complex shell middens that form the basis for Salish Sea archaeology “are often characterized by only a handful of radiocarbon dates” (Stein et al. 2003:297). Absent a secure chronology, “[i]ssues central to settlement pattern analysis, such as abandonment and reoccupation events, population fluctuations, building activities, and activity areas” are difficult if not impossible to delineate in a shell midden (Stein et al. 2003:297).

In part a consequence of these factors, focused archaeological study of the precontact Coast Salish house (thus village) has been limited (Matson 2003; Ewonus 2006, 2017; Taylor and Stein 2011). The centerpiece of the Coast Salish village is the Salish shed-roof house, which R. G. Matson has described as “simultaneously well known and undefined” (Matson 2003:76). According to Matson, ethnographic descriptions (e.g., Suttles 1990:6–7; 1991) make the shed-roof house’s general purpose and main structural features “well known”:

Suttles [1991] makes clear the many functions that are agreed to by most investigators—general purpose, winter dwelling, storage, and location of public ceremonies. The main structural features of this house form are also a point of general agreement. Beyond these general statements, such things as size, number of people per compartment, divisions between compartments, location of different people within the house, and amount of economic

specialization are subject to widely varying interpretations. (Matson 2003:76)

Archaeological features typically associated with Northwest Coast houses include floor deposits, hearths, structured living areas, and post holes (Taylor and Stein 2011:170). Non-domestic structures constructed from shell associated with houses and villages include defensive sites, storage, and water storage (2011:170). There are numerous descriptions of what are thought to be precontact Salish Sea island houses/villages, or elements thereof, especially in the Central Salish Sea, including on Gabriola Island, Galiano Island, Pender Island, Saltspring Island, San Juan Island, and Valdes Island. All date to Salish Sea IV.

The use of islands for seasonal resource gathering from Salish Sea IV onward means that actual plank houses might be rare, with temporary shelters being the norm at island sites. These large tent pole structures with sewn tule mat walls (Croes 1995) may be the main one used in spring/summer/fall island encampments and should be recognized as likely alternative to “houses.”

The shed roof house is probably not the oldest house type in the Salish Sea basin. Indeed, the oldest securely dated village in the Salish Sea basin—occupied at the end of Salish Sea III—is an assemblage of semi-subterranean pithouses located on the Nooksack River delta near Bellingham, Washington (Grabert 1983; Hutchings 2004). Like at English Camp on San Juan Island, the people living at this Central Salish Sea village utilized the full spectrum of available coastal resources, both terrestrial and marine (Hutchings and Campbell 2005; Nokes 2005).

Salish Sea IV ends abruptly with European contact in the latter half of the nineteenth century, with profound repercussions for all Coast Salish people. The two postcontact Salish Sea periods are useful to consider here, if only briefly, because Salish Sea V and VI provide important context for thinking about Salish Sea archaeology and Coast Salish heritage. Consider,

for example, that archaeologists interpret the past in the present, meaning the stories they tell may have as much—if not more—to do with modern history and the political present than the ancient past (Wilk 1985; see also Wobst 1978; K. T. Carlson 2007).

Salish Sea V—1775 to 1900 CE. Salish Sea V begins with the arrival of European surveying expeditions on the outer coasts of Vancouver Island and Washington state, starting with the Spanish in Nootka Sound in 1774 and on the Olympic Peninsula in 1775. At the same time, the Coast Salish experienced their first smallpox epidemic:

Of all introduced diseases on the Northwest Coast, smallpox caused the greatest mortality. The disease appeared in epidemic waves [and the] initial outbreak occurred sometime during the 1770s [and] seems to have affected the entire coastal region, and was (apparently) not witnessed by Euro-Americans. (Boyd 1990:137)

Evidence suggests a “minimal figure of one-third” as the approximate mortality rate on the Northwest Coast from the 1770s smallpox epidemic (Boyd 1990:138; see also Boyd 1994). Before the arrival of Europeans in 1774–1775, approximately 200,000 people lived on the Northwest Coast, “making it one of the most densely populated nonagricultural regions of the world. Within 100 years, the aboriginal population had declined by over 80%” (Boyd 1990:135; see also Harris 1997:3–30). As a result, most Salish Sea islands were more sparsely populated during Salish Sea V compared to earlier periods.

Between 1788 and 1792 the Salish Sea was surveyed by six European maritime expeditions, John Meares in 1788 (Great Britain); four Spanish expeditions between 1789 and 1792 (Narvaez, Quimper, Eliza, Galiano/Valdéz); and George Vancouver in 1792 (Great Britain), who named both Puget Sound and the Strait of Georgia (Blumenthal 2004). European colonization of the Salish Sea was a destructive process that served

to alienate Salish people from each other and their lands (Harris 1997; Lutz 1998; Arnett 1999; Flack 2006). In addition to smallpox, a major event affecting use and occupation of Salish Sea islands was the cooptation and commercialization of the salmon fishery by Europeans at the close of Salish Sea V (Boxberger 1989; Newell 1993). Another was the radical transformation of the basin’s transportation network, which fundamentally changed how people and their goods moved around the Salish Sea (Harris 1997:161–193; cf. Ames 2002). The establishment of the U.S./Canada border had—and continues to have—important consequences for Coast Salish peoples (Flack 2006).

Salish Sea VI—CE 1900 to Present. In the context of colonialism and globalization, Salish Sea VI is an extension of Salish Sea V. Salish Sea VI dates from 1900 to present and corresponds to the late modern or contemporary era, characterized by industrialization, urbanization, and the transformation of the Salish Sea basin into a megaregion of more than seven million people. Most of the built landscape visible in the Salish Sea basin today post dates 1950, and it is during this time the most significant physical harm has been done to Coast Salish landscapes (McLay et al. 2008; Mapes 2009; Stapp and Longenecker 2009; McLay 2011; Hutchings 2017), particularly at the shoreline (Shipman et al. 2010). Two major threats today are amenity migration (people moving to “rural” Salish Sea islands to escape “city life”), resulting in overpopulation and landscape degradation and destruction, and climate change, with wide-ranging and profound consequences for all Indigenous Pacific Coast peoples (Grossman and Parker 2012; Hutchings 2017).

Modern development has impacted Coast Salish cultural heritage landscapes on an industrial scale, and no part of the basin has been exempt (Acheson and Riley 1979; Wessen 1986, 1988; Hutchings 2017). A 1986 study of San Juan County, which includes Orcas, San Juan, and Lopez Islands, showed that 96% of recorded sites there had been disturbed (Wessen 1986:48). A 1988

study of Island County, which includes Whidbey Island and Camano Island, found that 100% of sites there “exhibit some degree of disturbance and that, in most cases, this disturbance has been significant” (Wessen 1988:65). A 2004 study of Pender Harbour found 100% of sites there had been disturbed, and that over two-thirds of each site had been destroyed—almost half were 90% destroyed (Merchant 2004). A 2017 study of the *shíshálh* (Sunshine) Coast found at least 75% of sites examined had been disturbed and while logging was once the primary cause of archaeological site disturbance and destruction, today it is home construction (residential development) driven by amenity migration (Hutchings 2017:76–87). These heritage landscapes are extremely important to Coast Salish people and their destruction can be linked to psychosocial dislocation (Alexander 2008) and solastalgia (Albrecht et al. 2007), the physical and emotional distress caused by the loss of place (Hutchings 2017:96–98). Nonetheless, Coast Salish people continue to use the same resources and places as their ancestors.

Synthesis

[The] study of islands is the study of movement.

—Paul Rainbird, 2007:1

Coast Salish traditional histories tell how Coast Salish peoples have used and occupied the Salish Sea and its islands since time immemorial, and the archaeological record reflects this. Of the 85 Salish Sea islands identified in our Salish Sea Island Site Inventory, 94% contain at least one recorded precontact archaeological site and every Salish Sea period is represented in the archaeological record. Of the three Salish Sea regions, the Central Salish Sea has the most islands, the most island sites, and the most archaeological research. On average, each Salish Sea island ≥ 2 km long contains 27 recorded archaeological sites.

The oldest securely dated site in the Salish Sea basin is an island site. The ~13,900 year old Ayer Pond site on San Juan Island in

the Central Salish Sea represents the earliest evidence of seafaring in the Americas by almost 1,000 years and shows Salish Sea islands were occupied immediately following their creation. While the Ayer Pond site and Clovis points are associated with big game hunting, overall Salish Sea I island use most likely reflects the needs and desires of generalist omnivores; that is, people used a wide variety of island resources and ecosystems.

The next oldest site in the Salish Sea basin after Clovis is probably the DeStaffany Site, also on San Juan Island. Although not securely dated, based on comparison of the artifact types to other dated sites in the foothills of the Cascade Mountains the DeStaffany Site likely dates between 13,000 and 10,000 years old. The absence of other island sites for this period is believed to reflect sampling bias and landscape submergence, not the absence of human activity. Due to the paucity of sites, little can be said about Salish Sea II island use and occupation, although it is probably similar to Salish Sea I insofar as it signifies use of the islands to hunt large fauna, in particular *Bison antiquus*.

As with the other periods corresponding to Coast Salish Mythical Time, little can be said archaeologically about the use of Salish Sea islands during Salish Sea III. The few sites that exist indicate continued hunting of large mammals (elk, deer) after the bison went extinct. Along with this change in game came a change in hunting technology and a switch from Western Stemmed Tradition projectile point technology to Cascade projectile point technology.

The vast majority of island archaeological sites date to Salish Sea IV, with most post-dating 3,500 years ago. These sites reflect use of all island ecosystems, from mountain top to seafloor. Coast Salish peoples are understood to have increasingly occupied islands and used island resources after 5,000 years ago, but the submergence of sites, particularly shell middens, makes interpreting the Salish Sea III/IV transition exceedingly difficult. The Salish Sea IV archaeological record demonstrates cultural continuity, corresponding to the Age of Trans-

formation (Figure 1) where “Transformers set things right in the world.”

Precontact use of Salish Sea islands was not a passive affair. Rather, Coast Salish peoples actively manipulated and managed their island resources and ecosystems (Mathews and Turner 2017). In some instances, these strategies were associated with large-scale food production (Boxberger 2000; Greene et al. 2015; Lepofsky et al. 2015).

Like all systems of traditional management in the Salish Sea basin, the management of island resources and ecosystems was nested within larger systems of Coast Salish tenure and governance (Blukis Onat 1984; Lepofsky et al. 2015), and it is within these larger systems that the island history presented here should be considered. This begins with recognizing the socioecological life of Salish Sea islands cannot be considered apart from the socioecological life of the surrounding basin. In this way the Salish Sea is perhaps more similar to the Mediterranean Sea (Braudel 2002; Rainbird 2007:68–89) than Oceania (Rainbird 2007:90–113; Cochrane and Hunt 2018) insofar as the former is defined—literally and figuratively—by its surrounding basin (Mediterranean comes from the Latin *mediterraneus* meaning “inland” [*medius* “in the middle of” + *terra* “land” + *aneus* “having the qualities of”]). In this regard, the Salish Sea is first and foremost an *inland sea*.

Taking this a step further, the Salish Sea is perhaps better thought of as the Salish Seaway, a term that simultaneously highlights the basin, the sea, movement, and connectedness—all of which are key features of the island archaeological and ethnographic record. In this regard, a useful conclusion and point of departure for discussion of precontact Salish Sea island use and occupation is Astrida Blukis Onat’s integrated Residence–Resource Model (Blukis Onat 1984:92–94; 1987:19–20), shown here as Figure 9.

While the Residence–Resource Model recognizes Salish Sea island use and occupation as unique, it remains nonetheless fully integrated into and subsumed by larger patterns of social

relations, trade, resource use, and ecology. As described by Blukis Onat, Figure 9 is “a static presentation of a spatially and temporally complex series of fluctuating interactions among residential groups, resource specialists, resource locations, resource ownership and kinship” (1987:19). Movement and connectedness, particularly in the form of travel and trade, are integral components of this dynamic system. The Residence–Resource Model is not based on archaeological knowledge; rather, it is derived from ethnographic sources and traditional Coast Salish history.

Although not without relevance, there are significant limitations to relying on the archaeological record to understand precontact Salish Sea history: there is scant information for the early period (Mythical Time), most archaeologists are not Coast Salish (La Salle and Hutchings 2016, 2018), and most archaeological research has focused on hunting and fishing and the attainment of “social complexity” (Matson and Coupland 1995; cf. Bernick 1999; Coupland et al. 2016). The vital cultural information transmitted in traditional knowledge about Coast Salish history and identity does not preserve in the ground. Instead, Coast Salish history provides the foundation for most archaeological interpretation, particularly as it relates to Salish Sea IV. Further, it is in oral history and traditional knowledge where the stories that make history meaningful are most vibrant. It is perhaps for this reason that many Indigenous people do not find much meaning or relevance in archaeological data alone (Yellowhorn 2002; see also Watkins 2005).

Conclusion

This article considered precontact Salish Sea island use and occupation in light of traditional Coast Salish history and the archaeological record. Coast Salish histories relate the use and occupation of Salish Sea islands since time immemorial, and the archaeological record reflects this. The earliest physical evidence of human activity in the Salish Sea basin is from

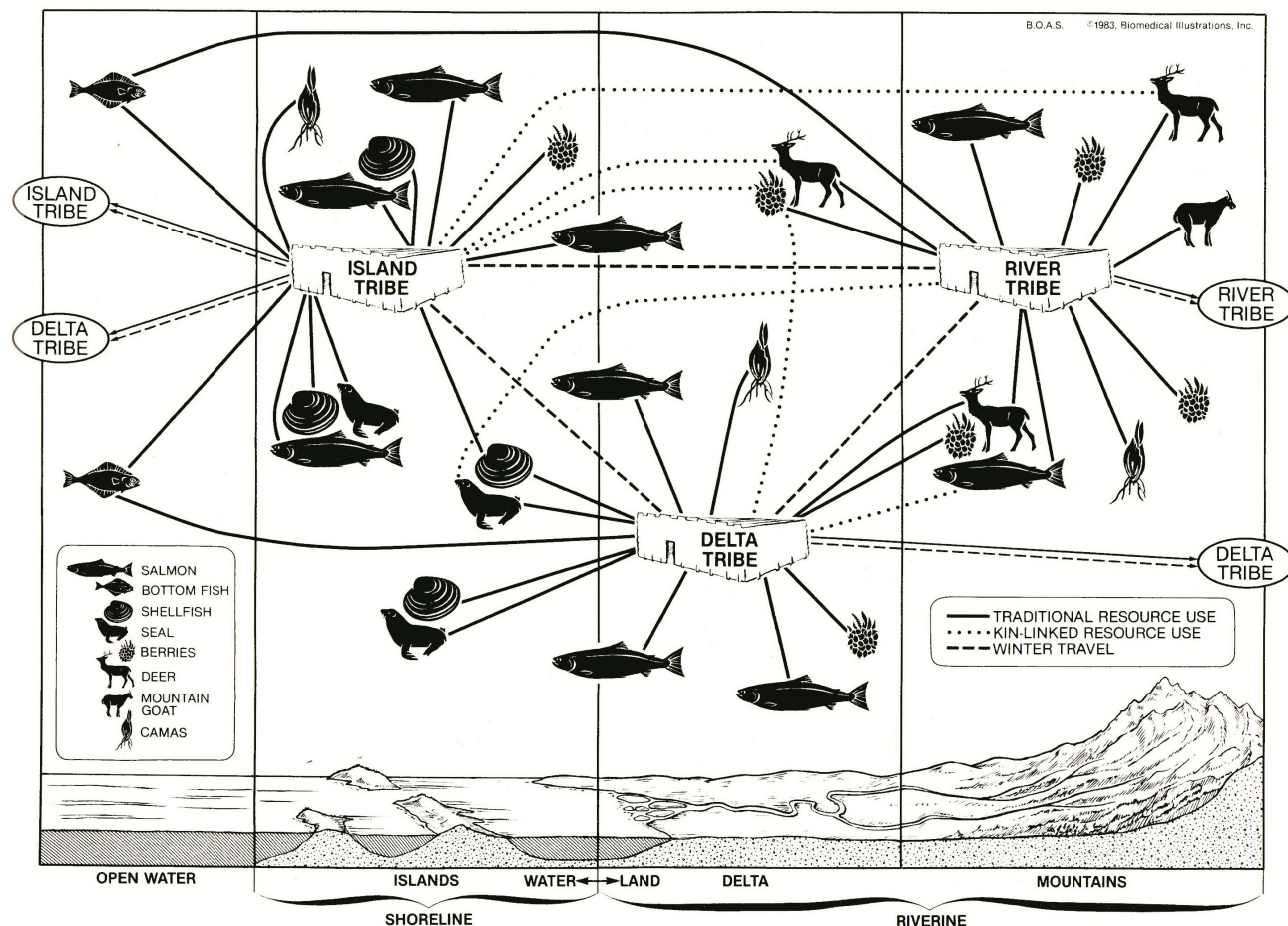


Figure 9. Southern Salish Sea Residence-Resource Model. Source: Blukis Onat 1984:93, Figure 1; reproduced by permission.

a Salish Sea island, as is the earliest evidence of seafaring and island occupation in the Americas.

While only a small number of island sites predate 5,000 years old, they show islands were occupied immediately after they were formed at the end of the last ice age, and continuously thereafter. Alternatively, there are thousands of island sites younger than 5,000 years old, indicating the use and management of all island ecosystems, many intensively. The paucity of early island sites is understood to reflect not the lack of early human use and occupation but rather landscape submergence and contemporary archaeological management and land use strategies. At the time of European contact in late 1700s, the Salish Sea basin was one of the most densely populated regions in North America, and the islands were used regularly

and intensively, as reflected in our Salish Sea Island Site Inventory.

In highlighting movement and connection (Rainbird 2007:167–172), the Salish Seaway concept is useful because it accommodates both intrabasin relations (within the basin) and interbasin relations (outside the basin). While the present study emphasizes the former, the latter is significant insofar as the Salish Sea is part of a larger terrestrial geography, namely the Puget-Willamette Trough (McKee 1972:50–51, Figure 5.1), the lowland corridor that connects Puget Sound to the Willamette Valley in Oregon. When linked, they form the Willamette Valley-Puget Trough-Georgia Basin Ecoregion (Floberg et al. 2004:15–16), the geo-historical linchpin of western Cascadia (Smith 2002; Barman 2008).

Islands are shown to be distinct and vital components of the precontact Coast Salish cultural landscape. While an island focus is certainly warranted in Salish Sea historical discourse, caution must be taken that such work does not obscure or erase the larger socioecological systems in which islands and island life are embedded. This lesson about the primacy of the watershed reflects the idea of the Salish Sea basin as the essential unit of analysis for the historical study Salish Sea islands (Berkes et al. 1998; Bentley 1999; Holling 2001; Berkes and Folke 2002; Gunderson and Holling 2002). The lesson, in a nutshell, is that Salish Sea islands do not exist in isolation, but are nested within larger dynamic socioecological systems.

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A Comment on Gary C. Wessen’s “Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington,” with a Reply from Gary C. Wessen

Abstract *In Journal of Northwest Anthropology Volume 53(1):1–54, archaeologist Gary Wessen published an article on the precontact settlement of the Olympic Peninsula. In that article, entitled, “Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington,” were many references to an article by anthropological linguists Dale Kinkade and Jay Powell, published in 1976, called “Language and the Prehistory of North America.” In that article, Kinkade and Powell discussed the utility of historical linguistics for archaeologists, with an example proposing the prehistoric occupation of the Olympic Peninsula based on linguistic evidence. Wessen claimed in his article that the available archaeological evidence contradicts, rather than supports, the arguments by Kinkade (now deceased) and Powell. Below, Powell responds to Wessen, who in turn replies to Powell—an uncommon scholarly dialog by recognized experts.*

Keywords

linguistics, Olympic Peninsula, Makah, Quileute, precontact, comment, response

Editor’s Note: While this exchange between Powell and Wessen stands on its own, readers will gain a better appreciation by first reading both Wessen’s recent *JONA* article (Wessen 2019) and the 1976 Kinkade and Powell article, which can be found at the *JONA* website, appended to the electronic version of this issue.

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A Comment on Gary C. Wessen’s “Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington”

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Introduction

I still have to think twice about the name *JONA* (*Journal of Northwest Anthropology*), because I first subscribed to it when it was *NARN*, (*Northwest Anthropological Research Notes*). The substantial doorstep volume of Fred Woodruff’s and my Quileute¹ dictionary, published as *NARN, Memoir 3* (Powell and Woodruff 1976) has been on my shelves for 43 years. Now I discover that

a recent article (Wessen 2019a) deserves my attention and has it.

That article by archaeologist Gary Wessen (2019a) is called “Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula of Washington.” His article takes issue with a publication by Dale Kinkade and me that appeared in *World Archaeology* in 1976 (Kinkade and Powell 1976). It was called “Language and the Prehistory of North America.” Kinkade died

¹ Note: the spelling of tribal names and locations often reflects the use of a quoted passage and results in variant spellings throughout: Quileute (Quillayute), Nitinat (Nittinat, Diitida’ath), Chemakum, Chimaicum (a tribe, language and town), Nuu-chah-nulth (Nuchanulth, etc.).

in 2004. We were both anthropological linguists, faculty members and colleagues at the University of British Columbia, and both committed to the study of the languages of the Northwest Coast culture area.

Prehistoric Quileute Occupation

This response to Wessen is primarily a discussion of Kinkade's and my basis for positing a prehistoric Quileute occupation of the northwestern Olympic Peninsula until they were wiped out or forced to vacate by the arriving Makah about a thousand years ago. I then reformulate that hypothesis based on evidence that the Makah arrived earlier or later than our original estimate. Finally, I briefly consider Wessen's newly developed claims. For instance, Wessen has regularly remarked and described Makah and Quileute cultural patterns as homogeneous. But now he often characterizes the Quileutes as an inland-focused tribe, which contradicts the factual record but may be useful in case of a future fishing rights legal action.

A Review of the Hypothesis in the Kinkade and Powell Article

Our article (Kinkade and Powell 1976) discussed the utility of historical linguistic data for archaeologists. As an example, we looked briefly at the prehistoric occupation of a site near the northwest corner of the Olympic Peninsula of Washington state. We chose the Cape Alava/Ozette example because it was, at that time, the site of one of the most interesting excavations on the continent. When we wrote our article in 1976, that excavation was in process, a dig that eventually spanned 11 years. Back then, news coverage of the excavation suggested a time depth of 450 BP, with some objects radio-carbon (¹⁴C) dated much earlier.

In our article, we introduced the Ozette example like this:

The original inhabitants of the Cape Alava area, known as the Ozettes, are extinct as a community (although there are people today who identify

themselves with Ozette). Extant neighboring tribes on the Olympic Peninsula are the Makah, a Wakashan (Nootkan) group located at Neah Bay to the north, and the Quileute, a Chimakuan tribe settled south of Cape Alava at La Push. The Clallam (now S'Klallam or Klallam) and the Quinault, Salishan groups, are more distant neighbors and were not contiguous with the Ozette. Common cultural patterns relate the Quileute and Makah, including secret ceremonial societies, material culture and economic practices including whaling and sealing. The Makah appear to have emphasized the halibut as a primary staple, while the Quileute exploited salmon runs as well as the halibut grounds around Tatoosh Island; but despite a few such distinctions the cultures of the Quileute and Makah were remarkably homogeneous...

Can language data be presented which shed light on the populations responsible for the deeper, earlier habitation strata at Cape Alava? A great deal of evidence suggests that the entire northern Olympic Peninsula was originally controlled by Chimakuan peoples. The time depth of Nootkan occupation of the northwest tip of the Peninsula cannot be determined with accuracy. But an estimate based on linguistic evidence places it at approximately a millennium. The language evidence, bearing importantly on the issue of Nootkan settlement, relates to the placenames, the mythic corpora of the Makah and Quileute, and Chimakuan comparative linguistics. Although we necessarily speak of estimates, data of several types complement each other in suggesting this figure. (Kinkade and Powell 1976:94-95).

That article was written more than four decades ago. So, Wessen's article (2019a), which was critical of our hypothesis, provided a stimulus for me to review Kinkade's and my theory regarding the prehistory of the north end of the Peninsula and, if warranted, to consider an alternative prehistorical formulation.

I admit that my first reaction to thinking about our version of prehistory was a little defensive. I thought, "Our version of history was published 43 years ago. Over the years a number of studies have assumed it is right." Of course, we were proposing pre-historic events, so we proposed a round figure (a thousand years) and used phrases such as "no specified timetable," "educated guess," "working hypothesis," and "the hypothesized homeland." But our proposed prehistoric construct was supported by a body of data, which is largely presented and discussed in this response.

Rethinking our proposed historical claim was a process of distinguishing what we now know from what we then hypothesized. Before I started writing, I spent several days, checking each of the "facts" on which our 40-year-old prehistoric construct was based.

Support for That Scenario

Indeed, there are a number of published statements that support the prehistoric scenario that we posited for the north shore of the Olympic Peninsula and especially for the northwest section of the Peninsula. Even the date of our hypothetical claim of the Makah movement to Cape Flattery is supported by the Makah in their 1990 tribal sketch in the Smithsonian *Handbook of North American Indians*, where Ann Renker and Erna Gunther state in the first paragraph: "The Makah language is the southernmost member of the Wakashan Family. It belongs to the Southern or Nootkan branch and is closest to Nitinaht, from which it separated about 1000 years ago" (Renker and Gunther 1990:422). Makah scholar Joshua Reid also states in his 2009 dissertation that "Linguistic evidence reveals that the Makah dialect separated from the Nitinat dialect just over 1,000 years ago." Both of these

citations reference W. H. Jacobsen's "Wakashan Comparative Studies" (1979).

Some Background for Readers

In our short article (Kinkade and Powell 1976), Kinkade and I discussed the nature of historical linguistic data and how it might be used to support hypotheses and conclusions regarding prehistory. So that readers can follow this comment on Wessen's article, the following information might be useful. My discussion focuses on tribal groups that spoke languages belonging to two language families: Chimakuan and Wakashan. The Chimakuan family included two languages: Quileute and Chemakum, both descended from an original language referred to as Proto Chimakuan. The Wakashan Family originated on Vancouver Island and will be discussed below.

The two known Chimakuan languages, both now extinct, are Quileute (spoken at La Push and upriver settlements on the west side of the Peninsula, 26 miles south of Cape Flattery) and Chemakum (spoken by a small tribe on the northeast corner of the peninsula near Port Townsend). During late prehistory these two tribes were separated by a distance of 100 miles of territory or 160 miles of shoreline occupied by the Makah and S'Klallam including the Elwha (Klallam). My Ph.D. dissertation (Powell 1975) was a reconstruction of Proto Chimakuan that included a discussion of the time degree of divergence between Quileute and Chemakum since they had clearly separated many hundreds, perhaps thousands of years earlier.

The Separation of Quileute and Chemakum

Knowing that the Quileute and Chemakum had been separated for many centuries and ended up on different sides of the Peninsula caused Kinkade and me to wonder what caused that separation.

The Quileutes have a narrative that provides a folkloric explanation for that separation. According to that story, a great flood (or tsunami) caused the tribe to take to their big freight

canoes in which the rising waters carried them up to the peaks of one of the highest Olympic mountains. At night, a few of the canoes broke loose and drifted away with members of several families in them. When the waters receded, those canoes had drifted to the northeast section of the Peninsula near present-day towns of Port Townsend-Chimacum. The people settled down and stayed there. As the flood receded, the Quileutes in the other canoes paddled back to their village site at the mouth of the Quillayute River. And that's how the Quileute and Chema-kum ended up located so far from each other according to Quileute folklore.

Increasingly, data regarding seismic events and sudden floodings in prehistoric times are being documented, validating such "great flood" stories. (Check out Ann Finkbeiner, *Earthquakes and Tsunamis in the Pacific Northwest*, 2015.) But back in the mid 1970s, it seemed to Kinkade and me that a less legendary and more defensible hypothesis for that separation was that in earlier times those two Chimakuan tribes (a) were the occupants of the whole northern Olympic Peninsula; (b) were separated by the arrival of the Makah moving across to Cape Flattery from Vancouver Island; and (c) at some point, one or more S'Klallam groups moved across from southern Vancouver Island and gradually expanded to occupy the Sequim, Port Angeles, Elwha, Lake Crescent and Clallam Bay areas. Other ethnographers had also suggested such a prehistoric formulation.

Prehistory, by definition, is the time before there is documentary evidence. But Kinkade and I thought that there were a number of conjecturable if not convincing issues to back up such a construct. However, Wessen, in his recent *JONA* article (Wessen 2019a) detailed archaeological evidence that he believes supports the hypothesis that the northwest Peninsula has been occupied for 2,000 to 5,000 years by the Makah. This claim presented an opportunity for me to review Kinkade's and my hypothesis in light of what Wessen has brought to our attention and proposed.

The History of Claims and Narratives About the Chimakuan Occupation of the Peninsula According to Albert Reagan

Kinkade and I weren't the first to mention our proposed prehistoric scenario. Albert Reagan published Quileute narratives that essentially described a prehistory of the Peninsula that is almost exactly as Kinkade and I proposed in our 1976 article. Reagan had mentioned the stories earlier and finally arranged for their publication in 1934, only two years before he died (Reagan 1934).

Wessen (2019a) described Reagan in his *JONA* article in a way that is strangely incomplete. Wessen is right when he tells us that Reagan was "a school teacher at La Push between 1905 to 1909" (p. 5). However, Reagan wasn't just teaching reading and writing in the Indian School. During that four years he was unstoppably interested in Quileute culture, stories, ceremony, and much else. For instance, he was remembered to have once halted a Quileute shamanic healing ritual in order to take the pulse of the participants! He observed and documented community life, and became arguably in four years the best-informed expert on the tribe of his time, even though anthropologist Livingston Farrand had spent a few weeks with the Quileutes earlier. Reagan's interests were encyclopedic. His 108-page description of the geological history of Quileute territory and the northern Peninsula, published three years after his arrival in the area (1908), is a remarkable achievement for an amateur (Reagan 1908).

Wessen points out that Reagan even investigated archaeological remains, devising distinct "temporal units" (Recent, Old, Very Old, and Ancient) and "regions" (Quillayute Region, Ozette-Makah (north) Regions, etc.) which strikes me as informed and sophisticated for that social-scientific era. What is most interesting about Reagan's archaeological observations is that he notes that the midden contents in both regions suggest that the Quileute didn't make or use stone objects, and the Very Old period in the Ozette-Makah region was characterized

by few objects of stone as were the entire Very Old strata in the north. Reagan assumed that the Very Old level of remains (without stone objects) indicates that the Quileute occupied the northwest corner of the Peninsula during that period, consistent with legendary Quileute narratives. And, Reagan noted that there was again evidence of some stone objects underlying the Very Old levels at northern sites, which he suggested was evidence of invaders from the north. Of course, Reagan had no way of detecting or deciding the date or time depth of the various periods of occupation. He certainly didn't have Carbon 14 or other test methods. Also, he was operating with no mentor or advice about excavation and no help. Yet, he traveled widely throughout the territory of the Quileute and the neighboring tribes at a time when there were still few roads. Considering that he had no libraries to consult and that he was also employed full time as a teacher, Reagan really deserves positive recognition. Like Wessen, I also visited the Brigham Young University (BYU) archives in the 1970s (Chad Flake was the archivist, as I remember). Going through Reagan's notes, I felt like I was watching a Northwest Coast ethnographic version of Heinrich Schliemann digging at Troy with a copy of the Iliad in Greek in one hand and a shovel in the other. Reagan was a true renaissance man.

Sure, Reagan was better at some things than others. More noticeable to me is that he had a "tin ear" and couldn't distinguish any of the twelve Quileute K-sounds from each other. More problematic is that during his early years in La Push, his documentation of tribal folkloric stories seems to have involved noting the details of the story line and then later fleshing out the narratives in a prose style appropriate to the dime novels of the time, e.g., references to scalping and other battle tactics that the Quileutes didn't do. But we have no evidence that he departed from the basic story line. To his credit, Reagan documented 38 Quileute and Hoh stories that aren't noted anywhere else.

It is disturbing to me that in his critique of Reagan, Wessen neglected to tell readers that

Reagan followed his teaching at La Push with a career in the Indian Service mostly outside the Northwest and that he earned a Ph.D. in anthropology at Stanford in 1925 and served as a full professor of anthropology at BYU for a decade. Reagan was a well-trained professional. In the opinion of many informed northwest community members, he is a paragon. This seemingly disrespectful omission of his experience, achievements and credentials aroused the indignation of several attendees at Wessen's June 10, 2019, presentation at the Makah Cultural Research Center (Wessen 2019b).

Quileute Narratives

Reagan's published tribal narratives validate his claim that the Quileute occupied the north end of the Peninsula as Kinkade and I claimed in 1976. In fact, there is a handwritten version of these narratives in the BYU archives in a notebook. It has a note by Reagan on the cover which really gives those stories a pedigree. He wrote, "Unless otherwise stated, my informants in obtaining the traditions of these Indians were Benjamin Hobucket (b. 1868) and his brother, Police Luke Hobucket (b. 1872), and my assistant in the government school and translator, Gordon B. Hobucket" (BYU Archives, Reagan fons, exhibit 09.01.251, parentheses added). That would appear to indicate that these stories were told between 1905–1909 in Quileute and then translated into English for copying down. Reagan is presenting these cultural narratives like a trained, careful ethnographer.

In discussing Reagan's Quileute folkloric narratives (1934), I will refer to and present excerpts from three of the 17 stories in this article: #2, The Battle of Nittinat (pp. 75–76); #3, The Battles of Neah Bay, Warm House, and Ozette (pp. 76–77); and #13, The Battle of Chimakum (pp. 90–91). As a group, they served as the basis for Kinkade's and my claim that the Quileute, Chimakum and possible other Chimakuan bands had occupied the whole of the northern Olympic Peninsula in precontact times. I will then discuss the basis for our now-contentious claim that the arrival in the area by the Makah

and S'Klallam and Quileute folk history of the events.

The beginning of the story below, entitled "Battle at Nittinat," is a statement that appears to be a prologue that may have been added by Reagan (1934) rather than having been stated by the Quileute storyteller. Nowadays, folklorists consider it an intrusion on the validity of the recorded version of a cultural narrative if the compiler adds such information other than as a footnote. Having read this story many, many times, I suspect that Reagan was presenting the story as he would have told it to a non-Quileute audience. That's because Quileute storytellers would assume that tribal listeners know the background information that Reagan included in the introductory section of the story below. Traditional Quileute storytellers start out with a statement such as "*Hikawolhxa'tila'li kixi' xwa' Bayak talhaykila*. I'm going to tell you a story about Raven a long time ago." They don't give listeners background information. I have come to assume that Reagan was presenting that story as if he were telling it to members of the Utah Academy of Sciences, Arts and Letters, who wouldn't know that important background information.

Battle at Nittinat

It then began to be rumored that the Indians from the northland, from the west coast of Vancouver Island as that island is now called, were fishing now and then on the fishing-grounds of the Quileute at Cape Flattery and Tatoosh Island, at the entrance to the Strait of Juan de Fuca. This was in the days when the Quileutes-Chemakums had complete control of the greater part of the Olympic Peninsula. Furthermore, is (sic) was also rumored that these northern people expressed a desire to take that part of the peninsula around the cape and Neah Bay as their own. This, of course, aroused the jealous ire of the Quileutes. Preparation

for a raid into the Makah country on Vancouver Island was at once begun, and in a few days more than a hundred war canoes set out for Nittinat, the old home of that tribe to the northward of the Strait of Juan de Fuca. (Reagan 1934:75-76)

The rest of that story details the Quileute raid on the Nitinat village that almost wiped out the Nitinat tribe, killing all the males who had been in the village and carrying off the women and children as slaves.

The next story in that article tells us about the almost immediate retaliation of the Makahs and surviving Nitinats against the Quileute settlements on the northwest tip of the Peninsula.

Battles of Neah Bay, Warm House and Ozette

When the Quileutes attacked the Makahs at Nittinat, most of the Nittinat braves were away on a marauding expedition up the coast to the northward and on their return to find their village obliterated, their rage knew no bounds. So without stopping to camp, they set out in hot haste in pursuit of the southern enemy, erecting at the front of each advancing canoe a board-shake on which were carved and painted certain symbolic (sic) designs which were understood to mean a declaration of war.

On arriving at Neah Bay, they landed amid a shower of arrows from the shore and immediately laid that village in ashes, killing or capturing every living soul in the place. Then they hastily proceeded to Warm House between Cape Flattery and Neah Bay on the strait side, at which place there was quite a settlement of Chimakum-Quileute Indians. There another desperate battle was fought, for they surrounded the village, and death or worse to those shut in it was

inevitable. Yet the defenders fought with desperation.

...The Makah and Nittinat victors then carried all the dead foes out to the sea beach at low tide and laid them in a long line so that the flow tide would take them out to sea—you know all Indians whose bodies are claimed by the sea turn into owls, the worst thing that could happen to any one.

...Following up the victory at Warm House, the Makahs rounded the cape, captured the villages at the mouth of the Tsooez and Waatch rivers and then proceeded down the coast to the village of Ozette... Reaching Ozette, another fierce and terrible battle was fought and the Chimakum-Quileutes there were slaughtered till none were left. (Reagan 1934:76–77)

It is interesting to note that, according to that account, the Chimakuan (Quileute) bands who were occupying the northwest end of the Peninsula were wiped out in a single day. We have no evidence as to whether they were replaced immediately or over time by Wakashans, whether the Quileutes later returned to re-occupy the area for a time, or whether there was a period of serial dominance by both groups or even co-occupancy and use.

I am comfortable that Reagan is portraying the northwestern Peninsula as having earlier belonged to Quileutes and that at some point the Makah-speakers took over. Wessen argues in his article that the Cape area may have passed back and forth between Chimakuans and Wakashans several times...when he isn't concluding that the Quileutes probably were never resident on the north end of the Peninsula at all. It's progress to expand the prehistoric alternatives that we are considering but surprising when the conflicting alternatives appear in Wessen's article a few pages apart.

The third of the stories served as a basis for Kinkade's and my assumption that Chimakuan

peoples occupied the whole of the northern Peninsula in prehistoric times. It is called "The Battle of Chimakum," and mentions the S'Klallam.

We were once a powerful people and had possession of the Quillayute and Hoh rivers and all the rivers that flow into them. Our women also gathered fern roots from all the prairies of the region. Not only that but our possessions extended over the Clallam mountains to the north to the long water that goes out to meet the big water towards the setting sun. Moreover, along that water our possessions stretched from the mouth of the Hoko River to Chemakum, a distance of three long days' canoe journey.

Peaceably we lived in all this region and a happy people were we. The salmon came early in the years at Quileute, and we could always dig clams in abundance on the long water. There were also plenty of game in the woods and water birds in the rivers. We were happy, but an evil day came.

A certain woman, called Natankabos-tub, became a witch. With the glance of her evil eye she killed people. From place to place she went doing harm. She would keep the fish from "running" in the streams. She would keep the the hunters from killing the game in the woods. And when the people would go out whaling, she would cause the whales to destroy the boats and drown the whalers. (Reagan 1934:90–92)

The rest of that story describes the "evil day" that was the end of peace in the whole of the original Chimakuan territory across the north end of the Peninsula. It involved Thunderbird and powerful spirit beings with this sense of the final collapse of Chimakuan participation

in the central and eastern lands of the northern Peninsula.

...Onward with the blood-curdling yell came the evil spirits. Before them there was no mercy. All of the assembled Quileute-Chimakum tribe then and there perished and the demons (the Clallam Indians) held the land. (Reagan 1934:90)

Ethnographic Mention of the Quileute and Chemakum Occupying the Northern Peninsula

Over the years, numerous ethnographers and others have studied the Quileute. Some, specifically Livingston Farrand (from 1902–1904), Edward Curtis (in 1913), Leo Frachtenberg (in 1916), Manuel Andrade (from 1928–1931), George Pettitt (in 1951), Verne Ray (in 1953), and Ram Singh (in 1966). A few concentrated on their chosen area of academic study and, as a result, were focused on tribal territory at Treaty time or current claims.

However, all of the researchers above mentioned the popular Quileute corpus of origin myths featuring the “Changer,” called *K’wati* (QUAH-tee), a mythic character who traveled the Peninsula and adjacent areas transforming natural landscape features and living things at the Time of Beginnings. According to the early Quileute storytellers the world had always existed. *K’wati* just changed things into the way they are now. *K’wati* appears in origin stories among many Nootkan bands as well. But, although *K’wati*, *Bayak* the wily Raven, and other mythic characters appear in numerous stories, none of those mythic accounts discuss Quileute occupation or ownership of the northern shore of the Peninsula.

There are several Quileute legendary and folkloric stories of the Quileutes being raided, primarily by the Makah. The Quileutes built a fortress atop James Island to retreat to during raids and they also lived up there for periods of time. Led by heroic figures (e.g., *Kalatob* and

Wadswad) the Quileutes did their share of raiding against the Makah. But in all of those accounts the Makah and Clallam already occupied their “traditional territories.”

In any case, by 1976 only two ethnographers had mentioned the Chimakuan early occupation of the whole north end of the Peninsula in prehistory: Reagan and Farrand.

Farrand was an ethnographer who did limited fieldwork with the Quileutes in the late 1890s and wrote the short entries for the Quileute, Chimakum and Chimakuan Family in the two-volume encyclopedic *Handbook of American Indians* (Farrand 1907). About the time that Reagan was recording those and other stories (1905–1909), anthropologist Livingston Farrand projected in print that the Quileute and Chemakum may earlier have been the dominant tribes on the northern Peninsula. He wrote:

The situation (i.e. Quileutes and Chemakums separated by S’Klallams and Makahs) of these two tribes as well as certain traditions indicated that in former times the family may have been more powerful and occupied the entire region to the south of the Strait of Juan de Fuca, from which they were driven out by the Clallam and Makah. This however is uncertain. (Farrand 1907:269, parenthesis added)

Unfortunately, Farrand didn’t record for us the details of the special traditions or explain fully what were the details of the “certain” traditions that he alluded to in his note in the first *Handbook* (Farrand 1907). He was told things about Quileute prehistory that others who later spent months there never recorded.

So, Farrand and Reagan are the earliest and most probable sources of the various aspects of Peninsula prehistory that we are considering.

Reagan’s published stories (1934) include these features:

- 1) That the Quileutes occupied and controlled the upper end of the Peninsula,

- 2) That there were many Quileute villages in the greater Cape area,
- 3) That they were wiped out in a set of raids by the Makah, and
- 4) (In a different story in the same article) that the Quileutes living at the mouth of the Quillayute River, who were also attacked in that set of raids, survived by retreating to their fortress atop James Island.

Considering a Date

Reagan's narratives don't include a date for the end of Quileute occupation of the northwest part of the Peninsula. The date of about "1,000 years ago" for the arrival of the Makah to the Cape area of the Peninsula seems to have originated with Kinkade and me. It was a historical linguistic exercise—an attempt to calculate a date for a hypothetical prehistoric event. And we had to base the date on things that we knew at that time.

Wessen, at one point in his *JONA* article (2019a), claims that the arrival of the Makah probably happened at least 4,000 years ago. But, I still feel that the thousand year date that Kinkade and I calculated in 1976 is a more realistic estimate for the end of a Quileute occupation of the northwest tip of the Peninsula, for the separation of the Quileute and the Chemakum, and for the Makah to have taken up residence in the Cape area, whether exclusive or not. Here's how we arrived at that date.

The Calculation of the "About 1,000 years ago" Hypothesis for Final Settling of the Peninsula

Some Background on Glottochronology

During the 1950s and 1960s a major figure in historical and comparative linguistics was Morris Swadesh. He pioneered a process for calculating how long ago two related groups separated from each other. His approach is called glottochronology or lexicostatistics. At the beginning of our article (Kinkade and Powell

1976), Kinkade and I described this analytical model and our misgivings about the process.

Lexicostatistics is based on these three assumptions:

1. A basic core vocabulary of 200 (or 100) words is less subject to change than other parts of the language.
2. The rate of retention and loss of vocabulary items in the basic core vocabulary is constant through time.
3. The rate of loss is the same in all languages, and knowing the rate of loss, one can figure the time depth since the languages separated.

While the procedure strives to provide answers to the question of the time depth of descendant languages, many historical linguists question the validity of these assumptions. However, in the absence of all other indications of time depth, any evidence of prehistory becomes invaluable.

So, despite our reservations, Kinkade and I used lexicostatistics often and gratefully in that project, relying on it to compute some sense of the timing of prehistoric events. Back in the 1960s, the timing of almost every aspect of prehistory was "in the absence of all other indications."

Where to Start

The Makah arrival date on the Peninsula is a focal issue in Wessen's article (2019a). He uses archaeological excavation report info and various time depth test results to support his 3000–4000 (or sometimes even 5000) BP date for the arrival and presence of the Makah in the Cape area. Since our objective in our article (1976) was to give an example of the utility of language data for archaeologists, Kinkade and I focused on what historical linguistics could contribute to archaeological analysis. That being the case, we decided that, due to the limited information that we had access to, it seemed easier to figure the date of the separation of

Quileute and Chemakum rather than the separation of Nitinat from Makah. We didn't have comparable vocabulary for any of the three Nootkan languages (considering the various village usages a dialect group, and Nitinat and Makah as distinct languages). So, we decided to try to figure the time depth of the separation of Quileute and Chemakum.

Dating the Separation of the Chimakuan Tribes

We used the two main Chemakum word lists which had been collected by George Gibbs (1853) and Franz Boas (1890). I had used all of these data sets six years earlier working on my dissertation. Unfortunately, there wasn't even close to a perfect vocabulary of Chemakum to do a proper Swadesh core vocabulary comparison between Quileute and Chemakum. Andrade had discovered the same roadblock in 1953 and had made a different type of calculation. Looking at the same sentences in both Quileute and Chemakum, he did an impressionistic sense of the two languages (Andrade 1953). He characterized the difference between these two languages as comparable to the difference between German and English but not as great as the difference between Spanish and Italian. That type of reaction to the vocabulary of two related languages isn't science. But, as Kinkade said, "It's an indication." In 1953, Swadesh did a lexicostatistic analysis of Quileute and Chemakum using the paltry word lists available and decided that 59% of the vocabulary of the two languages had been replaced during the period of their separation (Swadesh 1953a, 1953b). That worked out to be 21 centuries.

That being the case, why did Kinkade and I suggest that the Quileute and Chemakum had separated a thousand years ago instead of two thousand? Well, we tinkered with the period of separation based on what we knew. For one thing, the date of separation of Quileute and Chemakum need not refer to the S'Klallam intrusion on the Peninsula. We reasoned, as Elmendorf later ratified in his 1990 tribal sketch of Chemakum in the *Handbook*, "...Quileute and

Chemakum may already have been linguistically separate when they became territorially separate" (Elmendorf 1990:440).

Also, Boas had said that when he attempted to find Chemakum informants in 1890, he was able to find only three speakers and "they spoke imperfectly." There was a good reason why "native speakers" were hard for Boas to find. Chemakum had been functionally extinct for 43 years when Boas took down his 1200 words of Chemakum. The whole tribe was essentially wiped out by a Suquamish raid led by Chief Seattle in 1847. Edward Curtis took down a description of that raid from "Whelchu" (probably Wahalchu, the Suquamish chief who took over after the death of Seattle). He was one of the raiders and described it like this:

The rapid rain of bullets mowed them down. Women and children were captured and taken away as slaves. The Suquamish paddled away, leaving the last Chemakum village in ruins and nearly all of the people either dead or captured. One of the few Suquamish who died in the encounter was Chief Seattle's eldest son. The few surviving Chemakum joined the Twana or Skokomish and, when they married, their children were raised speaking that [new Salishan] tribal language. (Elmendorf 1990:439)

It seemed clear to us that Swadesh's calculation based on Boas' notes wasn't a dependable test of the prehistoric situation on the Peninsula. By the time Boas tried to document Chemakum in 1890, the tribe was extinct as an ethnic group; the women and children were scattered and hadn't used the language for two plus generations. Furthermore, it seemed clear that the word list that Boas had taken down included a number of S'Klallam and Salishan loanwords that had been adopted into the survivors' usage of the moribund language during the decades after the annihilation of the tribe.

So Kinkade and I decided to go back and consider Andrade's (1953) description of the

difference between Quileute and Chimacum as comparable to the difference between English and German. Impressionistic or not, that date corresponded to the period since the Anglo-Saxons arrival in England about 450 CE (1525 BP in 1976). And since we still presumed that Andrade's estimate had been affected by the issue of recent loanwords into the moribund Chemakum usage, we settled on a purely putative date of about 1,000 years BP for the separation of the Chimakuan tribes.

Dating the Separation of the Makah and Nitinat

As linguists working with limited data, we thought that the date for separation of Quileute and Chemakum might be about the same as the date for the separation of Makah and Nitinat. In 1976, not a lot was known or available that would help with the prehistory of either tribe. There wasn't, as yet, compelling archaeological or linguistic (including lexicostatistic) information regarding the separation of Makah from Nitinat (Jacobsen 1979). Swadesh had done an analysis of the Wakashan family of languages in 1950, using available word lists (Swadesh 1950). He figured that Wakashan had split into Kwakiutlan and Nootkan subgroups 2,900 years ago (Swadesh 1953a, 1953b). Swadesh wasn't clear about the time depth of the separation within Nootkan, lacking information on both Makah and Nitinat. Kinkade and I found 2900 BP an unsurprising number since it allowed enough centuries for Nuuchahnulth to split from Nitinat-Makah, and then, later, for Makah to separate from Nitinat. Note that the illustration below (Figure 1), taken

from *Book 6*, p. 15, of the 1980 Quileute Culture Series (Powell and Jensen), uses the no-longer common geographical term Bella Bella instead of the tribal name Heiltsuk.

Fortunately, in the 40 years since 1976 we have accumulated a lot of evidence relating to Wakashan prehistory that we didn't have when Kinkade and I wrote the *World Archaeology* article (1976). I was involved with much of that development of new data. Vickie Jensen and I were invited to work for five years (1980–1984) with the Kwakwaka'wakw (Kwakiutl) doing a set of language books, a teachers' manual, and teacher training. Later I was asked to work with the Haisla over a period of 15 years (2000–2014) doing life histories of the Haisla elders and the Kitimat School language program. Those years of research on (two of the three) languages of the northern group of the Wakashan languages made it clear that the Wakashan tribal groups moved and settled far and wide over a period of about three millennia, Swadesh's 29 centuries time depth for Wakashan. They didn't just move into uninhabited areas. The Haisla pushed Tsimshian bands out of the way to inhabit the lower Kitimat River valley, not differently from what Kinkade and I suggested that the Makah did to the Quileute at about the same point in prehistory.

In 1989, Andrew Callicum, Language Programs Co-ordinator for the Nuuchahnulth Tribal Council (NTC) invited me to organize a multi-language/dialect Nootkan dictionary project. First, we devised a Nuuchahnulth orthography with a separate writing system for the quite-different Nitinat. Native-speaking

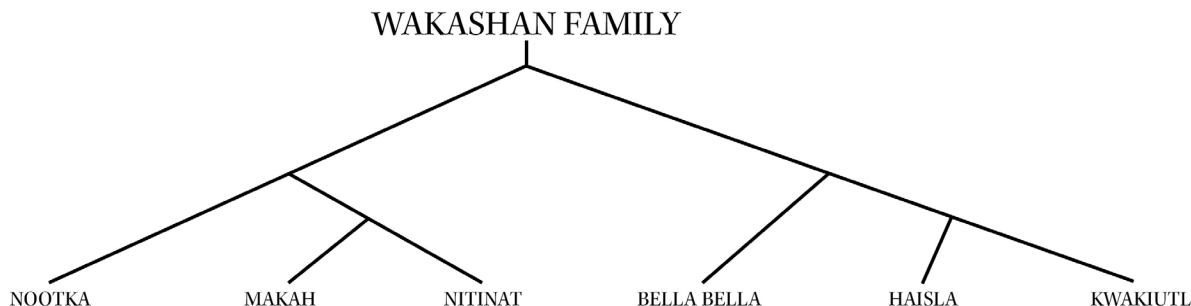


Figure 1. Wakashan language tree (adapted from Powell and Jensen 1980).

elders from eleven Nuu-chah-nulth bands met from May to September for three summers, 1990–1992. Besides Diitiidaath (Nitinat), the participating Nootka groups were the Tseshaht, Toqaht, Hesquiaht, Mowachaht, Kyukwot, Ahousaht, Tla-o-qui-aht, Hupacasaht, Ucluelet, Ehattesahat and Nuchatlaht. That three-year project gave me in-depth experience with all but one language (Makah) of the southern Wakashan group. Before beginning, the NTC contacted the Makah and asked them to join the effort, but in the end they didn't participate.

Actually, the lack of an available Makah dictionary has been an important issue over the years and in the current discussion. In fact, a few years ago I called and offered the Makah Cultural and Research Center a copy of the T'aa'taaqsapa (Nootka and Diitiidaath) Cultural Dictionary in exchange for their Makah dictionary. I sent them a copy of the Nootkan dictionary, but later when I called to inquire about the Makah dictionary, I was told that their dictionary "wasn't ready for viewing." This suggested to me that current research may involve politically-motivated withholding of data, which certainly impedes scholarly exchange and cooperation. The result is that we still don't know and can only wonder whether comparative linguistic wordlists would give us an answer to the timing of the separation of the Nitinat-Makah and the move of the Makah to the Peninsula. As a result, I don't have a lot of patience with guesses of up to 4,000+ years BP for the movement of the Makah.

I had expected that Bill (William H.) Jacobsen, would give us a definitive answer to the timing of the separation of the Southern Wakashan languages. He worked for decades with the Makah and produced a language course in Makah, along with his historical and descriptive linguistic reports and publications. But Jacobsen's final statement (2007) on the matter was simply this:

The Southern Wakashan (or Nootkan) languages exhibit a sort of chain relationship, from south to north: Makah, Nitinat, and Nootka.

Given the intermediate geographic position of Nitinat with respect to Makah (situated more to the south) and Nootka (situated more to the north), one can ask which of these languages is most closely related to Nitinat. At present, this question remains unresolved, as reflected by the disagreement in the literature. Relying primarily on lexical data, but also considering aspects of sound changes and grammatical criteria, it is proposed that the closer grouping of Nitinat is with Makah. (Jacobsen 2007:766)

In the end, back in 1976 Kinkade and I had nothing specific that we could use as a temporal yardstick for the separation of Makah and Nitinat, which was presumed to have resulted from migration of the Makah across to the Cape area. So we just went ahead with the estimate of about 1,000 years for both the time depth of Chimakuan and the Makah-Nitinat separation, which was also presumed to be the date for the move of the Makah to the Peninsula and the Quileute retirement.

Skipping ahead to 1985, I admit that I was, frankly, shocked when I read Sheila Embleton's (1985) figure of 5500 BP for the time depth of the Wakashan split into northern and southern Wakashan (Embleton 1985). My first thought was that 5500 BP is one of the common estimates for Proto Indo-European, the language that split repeatedly over millennia into (now) 445 living languages spoken by 3.2 billion people including English and such different tongues as Hindi, Persian, and the great languages of the past like Hittite and Tocharian in China. Compare that with Wakashan, which has only two subgroups of languages that, if comparing written texts with a little imagination, are still somewhat mutually intelligible. Swadesh's figure of 2900 BP for the Wakashan split seems more accurate to me, considering that the six daughter languages are geographically as well as phonologically and grammatically close.

Relevant to the 5500 BP date of Embleton, in the same year as Kinkade and I did the *World Archaeology* article, I gave a conference paper called “Proto Chimakuan-Wakashan: Intriguing Similarities between Quileute, Nuu-chah-nulth, Kwak’wala and Haisla” (Powell 1976). It suggested a coastal proto super-stock that would date back to about 5000 BP. But, the distinctions and sound correspondences in “Chimkashan” were vastly more distant than any that I could imagine involved within the two documented Wakashan sub-groups.

Wessen’s Take on the Rest of the Kinkade and Powell, *World Archaeology* Article

There was more to our (Kinkade and Powell 1976) example than simply positing Chimakuan occupation of the Peninsula until about 1000 BP. And Wessen in his recent *JONA* article critiqued those issues as well. I always appreciate an informed researcher discussing my ideas. And I would like to respond to his comments.

Myth and Legend

Wessen discussed our treatment of myth and legend. We had suggested that Makah folkloric narratives might profitably be studied as possibly offering a clue to prehistoric tribal movements. That was certainly the case with the Quileute legendary accounts that were told to Reagan. First of all, on the lighter side, Wessen suggests that it was unfair of us to refer to Makah stories as myth (2019a:9), which suggests that the issue is untrue (it’s a myth that Jay is smart...) as does calling something a legend (the great Jay legend will live on for minutes). Wessen is, of course, using the popular sense of these terms to suggest that the Makah folktales are untrue and that we were impugning Makah folklore by using those terms. In fact, myth and legend are anthropological concepts and, if I can suggest in good humor, no student could pass my Anthropology 101 course without knowing

that a myth is a cultural story with a character (at least one) who has supernatural powers and a legend is a story with a character who has superhuman powers.

We only mentioned the Makah tale of the dog children with relation to Makah myth because it is not uncommon to find mythic and legendary reference to actual events lingering in a tribe’s myth-memory. Sometimes events from the tribal past (centuries ago) are retained, whether those accounts disagree with other tribal narratives or not. As an example, the myth of the dog children was first told to me by Hal George, a Makah-Quileute half-blood born in 1894 (pc, 1978). The story, which was mentioned by Wessen (2019a), involves a Nitinat chief’s daughter who bore a batch of puppies. The chief told the mother to get rid of those puppies. His daughter refused, so the mother and dog children were taken across to Cape Flattery and dropped off. The mother worked hard each day to hunt and gather food for the puppies. One day she sneaked back to watch the puppies and saw them take off their dog blanket costumes and inside they were ordinary Indians. So, when the children went off to play, she collected the dog outfits and put them in the fire. And those children were ordinary Indians ever after and they grew up to be the ancestors of the Makah.

I was also told that story by Eleanor Wheeler Coe of Hoh River and by John Thomas, the respected Nitinat speaker and informant for the Nuu-chah-nulth Dictionary project. He told me that the story exists among the Nitinat, as well. It is relevant to this conversation that the shared myths of neighboring tribes may also retain memory of the Makah move across to the Cape. However, it is improbable that such myth memory would have persisted over a period of three to five millennia as Wessen’s proposed timing might suggest.

The Makahs’ own oral histories align with the Quileute oral traditions. Joshua Reid (2015) describes family tradition and mythic narrative among the Makah elders that derive from tribal folklore from the 1500s (500 BP) to mythic

exploits of 3,500 years ago (Reid 2015:89). In an interesting analytical tactic of cross-referral to information in James Swan's diary, Reid similarly dates another Makah oral tradition of the return of the Makah families to their territory in the Cape area after they had been driven out by a Quileute raid.

James Swan was the teacher at Neah Bay from 1862 to 1866, and while there, he recorded Chief Kal-chote who stated that

Neah Bay was named after an ancestor, Deeah, who had lived twelve generations earlier. Deeah had expelled Ditidahts from Neah Bay shortly after a combined Makah-Ditidaht force fought back against the Quilleutes (sic). (Swan 1870:58; refer to Reid 2015:89, footnote 16)

Finally, Albert Irvine's story, "How the Makah Obtained Possession of Cape Flattery," relates a battle between the Nitinats and Makahs for Cape Flattery that suggests ownership of the region changed (possibly several times) through internecine warfare (Irvine 1921).

The S'Klallam Tribes also maintain oral traditions of having arrived on the Peninsula from the north. In 1917, Reagan relates that the "Clallam traditions that they came from the north" support the conclusion that they migrated southward to the Peninsula. The tradition states:

The Clallams state that before they had moved to the mainland, and while they were yet on Whidbey Island, their braves, by surprise, fell upon the Quillayute-Chemakum Indians at a time of a Devil's dance on the spit and massacred the whole population attending the ceremony, but the victim to be sacrificed was a woman, whom they saved and who afterwards became the wife of their chief. This defeat of the Quillayute-Chemakums gave the Clallams a lodgment on the mainland, which they still maintain. (Reagan 1917:90-92)

Thus, the S'Klallam tradition further supports the evidence that the Quileutes and Chemakums once had control of a large portion of the Olympic Peninsula.

One Quileute oral tradition indicates that the S'Klallam moved to the Peninsula after the Makahs. In "The Battle of Chimakum," the Quileutes describe their territory as extending from the Hoko River to Chimakum before the S'Klallams arrived:

We were once a powerful people and ... our possessions extended over the Clallam mountains to the north to the long water that goes out to meet the big water towards the setting sun. Moreover, along that water our possessions stretched from the mouth of the Hoko river to Chimakum, a distance of a three long days' canoe journey. (Reagan 1929)

The tale goes on to describe the "evil day" when a witch caused the S'Klallams to take control of a portion of the Peninsula.

Placenames

On the other hand, Wessen is right that our discussion of placenames with Chimakuan roots that came to be used in Makah traditional territory is the least developed points made in what Kinkade and I were presenting as a mini-example of the utility of historical linguistic data. It would've taken several pages of rather dense discussion of proto-language discovery procedures with complete examples to explain how placenames might provide an indication of earlier occupancy by Chimakuan tribes. In fact, though, placenames can sometimes be shown to have been used by earlier occupants and then continue to be used by the new occupiers of the land.

Kinkade and I suggested that there might be placenames composed of Chimakuan roots and affixes that were still being used for locations in Makah traditional territory after generations of Makah occupation (Kinkade and Powell 1976).

Such relic placenames would have to be proven to have derived from the language of the original occupiers of the territory. We proposed several possible examples of such retention.

The best example of such an intriguing possible relic of Chimakuan occupation is Archawat, the current name of a beach area about seven miles south of Neah Bay. The name is clearly the Chimakuan root **hac'h* (meaning “good”) with the lexical suffix meaning “beach” **-awat*. (An asterisk marks reconstructed proto-language forms.) That lexical suffix is still used in the Quileute word for beach, *lawáwat*. So, the old-time Chimakuan name for that beach was **hác'hawat*. The name descended into Chemakum as *hác'hakwat*. The fact that Archawat has the accent on the second A shows that the name was Quileute rather than Chemakum, because Quileute moved the accent to the next-to-the-last syllable in most words and Chemakum didn't. And the fact of that clearly Chimakuan name in the heart of Makah territory suggests the possibility that when the Makah arrived and took over, they may have continued to use the Chimakuan name for that beach.

And, to bring the example up to the present, why does the name appear on maps as Archawat? Well, linguists can account for those changes, too. When the name was recorded by the early cartographers, they were apparently speakers of British English, who commonly lived on both the Canadian and American sides of the Strait of Juan de Fuca during the late nineteenth century. Speakers of British English don't pronounce R after a vowel. Thus, for example, they pronounce farther as “fah-thuh,” leaving out both R-sounds. But they put in R-sounds after A-vowels where they don't belong, like pronouncing Cuba and Africa as “Cuber and Afriker.” So, when they heard an A-sound as in the Quileute word for good (*hac'h*), they pronounce it “harch.” Finally, the H at the beginning of the name got left out, which is also something that speakers of British English do. They leave out H where it's supposed to be and put it in where there isn't one, so they order ham and eggs by asking for “am and heggs.” Hence, Archawat.

I apologize for imposing on readers' patience with such an extended example, but it explains how Kinkade and I relied on historical linguistic analysis to explain how an old Chimakuan placename could have continued to be used long after the Quileutes were displaced or wiped out from the northwest end of the Peninsula and even gotten on the maps of the area. It is particularly mentionable as an example of how placenames can suggest that the Chimakuans were there before the Makah.

A Revised Estimate of the Time Depth of Makah Arrival and Occupancy of the Northwestern Olympic Peninsula

Given Wessen's own repeated admissions regarding the archaeological similarities at midden sites throughout our part of the coast, it seems clear to me that more than just archaeology is required to identify the cultural groups that occupied the area. That is where cultural and linguistic evidence are crucial—neither of which Wessen, admittedly, has.

Despite his prior reports, Wessen makes clear that the source for the standoff between his and my conclusions about Peninsular prehistory is based primarily on archaeology. He states, “Currently, there is no archaeological reason to suggest that Chimakuan speakers were ever widespread on the Olympic Peninsula or elsewhere on the Northwest Coast” (Wessen 2019a:42); and, “Preliminary findings suggest that Makahs and/or other Wakashan speakers have been present for at least 3,000 to 4,000 years and that there is no credible evidence for an earlier presence of Quileutes and/or other Chimakuan speakers” (Wessen 2019a:1).

In juxtaposition, let me provide an updated version of Kinkade's and my conclusion about that prehistory.

Based on the evidence currently available, it is now my opinion that the Makah occupancy of the northwestern Olympic Peninsula occurred sometime between about 650 and 2000 BP.

The Reason for the More Recent Date: About 650 Years Ago

The lower date, about (but probably less than) 650 BP, has to do with Quileute tribal memory of events. In pre-literate societies such as pre-contact Quileute society, memory of incidents is transmitted orally from generation to generation. Such recalled folk history events are generally considered to be retained for hundreds of years rather than thousands. Happenings earlier than five or six hundred years ago tend to become myths and legends or simply be forgotten. It's an anthropological presumption (pers. Comm. Del Hymes, 1997 ICSL panel on "Timing the Legendizing Process"). Elmendorf reflects that presumption in his 1990 *Handbook* article, referring to persistent Quileute folk history when he wrote that "...it does appear that Chimakuan may once, *some centuries ago*, have been the linguistic family of northwest Washington" (Elmendorf 1990:440, italics added).

This anthropological presumption of folk history versus myth or legend applies to dating the arrival of the Makah and the routing of the Quileute ancestors from the Northwestern Peninsula. Here are two examples of statements by Quileute elders in the late 1960s: (a) Chief Charles Howeattle (*Xáwiyat*, b. 1884), was trained as a youth by his father Albert Howeattle (b. 1839). Back then, part of the preparation for a prospective chief was learning the details of tribal history. During one of our conversations, Charlie made the statement, "In the old days we had no boundaries and didn't need any borders because we owned everything." Later this statement came to mind when Lillian Pullen (b. 1911) told me that she was trained by her grandfather, Chief Tommy Payne (*Taxáwit*, b. 1870) to be a rememberer of family and tribal history. She recalled Tommy describing for her how the Quileutes established and enforced a tribal boundary above (north of) Dickey Lake to keep poachers, trespassers, and raiding parties from entering what was known to be Quileute territory.

What is interesting is that oral folk history does, in fact, erode after several centuries, if it doesn't morph into myth or legend. That suggests that Quileute boundaries may only have existed during the last half a millennium or a little more. It also may explain why early ethnographers like Reagan and Farrand heard accounts about Chemakuan occupation of the north end of the Peninsula and later ethnographers did not.

The Reason for the New Proposed Date: Up to 2000 BP...Rather than "About 1,000 Years"

The new hypothetical date for arrival of the Makahs, up to as much as 2000 BP, is largely based on the archaeological site reports discussed by Wessen in his *JONA* article (2019a) and his 1990 Smithsonian *Handbook* article (1990). The 2000 BP date also reflects the various glottochronological dates which Kinkade and I originally disagreed with.

Even so, 2000 BP may not accommodate the time depth that Wessen suggests for the beginning of Makah exclusive occupation of the Cape area. My new proposed earlier date for Makah takeover of the Cape area would allow for early back and forth Makah-Quileute serial occupation. I posit this date because we now have a more organized picture of the languages and cultures of the area than Kinkade and I did half a century ago.

Of course, Wessen was discussing broader issues than whether the Quileutes were in what is now traditional Makah territory before the Makah took up residence and when they made the move. Nonetheless, the title of his article (2019a) was "Makahs, Quileutes, and the Pre-contact History of the Northwestern Olympic Peninsula, Washington." So, we haven't strayed from the topic at hand.

Well, What IS the Topic at Hand?

This is a good time for me to mention that I have occasionally been an admirer of Wessen's archaeological work. But this isn't one of them. I found his argument regarding Peninsular

prehistory in the first part of that *JONA* article occasionally took him beyond his expertise. Furthermore, it all seemed quite unnecessary. As he points out, after 43 years of being “ignored by most archaeologists” (Wessen 2019a:12), he could have simply dismissed our hypothesis in a paragraph and gotten on with what he does well. However, while the archaeologists were largely ignoring our claims and dates, anthropologists considered the evidence for earlier Chimakuan ownership of the northern Peninsula to be convincing.

In his article Wessen (2019a) included a whole section called “Current Status and Possible Future Directions.” It opens the door for me to bring up an issue which appears to be a “future direction” that he seems to have already punched into his GPS. The issue is that for 35 years Wessen and I collegially disagreed about when the Makah moved across from Vancouver Island to take up residence on the Peninsula. But we AGREED, along with other scholars, that the Quileutes and Makahs had homogeneous cultural-economic patterns (give or take a few stone objects) as cited below.

Homogenous Culture Patterns

Kinkade and I wrote in 1976:

A common cultural pattern relates the Quileute and Makah, including secret ceremonial societies, material culture and economic practices including whaling and sealing. The Makah appear to have emphasized the halibut as a primary staple, while the Quileute exploited salmon runs as well as the halibut grounds around Tatoosh Island; **but despite a few such distinctions the cultures of the Quileute and Makah were remarkably homogeneous.** (Kinkade and Powell 1976:94, boldface added here and below)

And Wessen has repeatedly stated that in most respects, the Quileute and Makah material

cultures were nearly identical. To mention a few of these citations, his 1990 Smithsonian *Handbook* article says “Most sites of the northern coastal region are late prehistoric shell midden deposits; **overall they are quite similar**, and much like late prehistoric shell middens from elsewhere on the Northwest Coast.” The same article states that the Quileute and the Makah and the other northwest coastal tribes “**used a similar range of natural resources and applied technologies**” (Wessen 1990:412).

And in Wessen’s 1993 report on the shell midden site near Sand Point, he again asserts, “In late prehistoric and early historic times, **both the Makah and Quileute were well known for their prowess in offshore fishing and marine mammal hunting....** The Makah and Quileute had similar traditional economies. They were skilled fishermen, marine and terrestrial hunters, and plant material gathers who possessed a great deal of knowledge about the resources available in their environment” (Wessen 1993:9). This echoed what he said in 1984 in another report on Sand Point, “**The material cultures of these two groups (Makahs and Quileutes) appear to have been quite similar.** Historically, both were skilled offshore hunters and fishermen and they exhibited sophisticated cultural adaptation to the maritime environment” (Wessen 1984:4, 7).

This observation of similarity is also noted and documented by others with regard to the marine-based subsistence of the White Rock and Toleak sites, both Quileute sites. Archaeologist Mary Ann Duncan’s 1977 report states that **findings of harpoon valves, bone points, and bipoints at La Push were similar to the implements found at Toleak.** And then Wessen’s 2006 report on the La Push midden emphasized that **the La Push site “closely resembles” the Ozette site** in that it contained an “overall dominance of marine mammals, and [a] prominent place of fur seals among them” (Wessen 2006:29).

Thus, we have all that archaeological and ethnographic agreement that traditional Makah and Quileute cultural patterns were homoge-

neous and largely identical. Why, then, don't we wonder whether uninterrupted archaeological sites in the Neah Bay area might indicate that more than one ethnic group occupied that site over time? It seems perfectly consistent with what we know about the Makahs and Quileutes that a settlement site on the north end of the Peninsula could have been home to different warring factions who had similar material cultures (and thus shouldn't produce differing midden assemblages over an extended period of time).

Not Just Archaeology

I'm not the first one to note that where tribes share materially similar cultures, "it is not possible to infer from the archaeological record alone which tribe occupied this coastal area prior to treaty time." This most recent statement is from a federal court judge looking at the same data. [*United States v. Washington*, 129 F. Supp. 3d 1069, 1109 n.4 (W.D. Wash. 2015), *aff'd in relevant part sub nom. Makah Indian Tribe v. Quileute Indian Tribe*, 873 F.3d 1157 (9th Cir. 2017), *cert. denied*, 139 S. Ct. 106 (2018).]

So, as I read through Wessen's *JONA* article yet again, it is clear to me that despite his previous statements of the homogeneity of Makah and Quileute culture patterns, he now regularly depicts the Quileutes as a largely inland-focused tribe, contrary to the facts. This is a newly developed Wessen claim. And I am

led to wonder whether this is footwork leading up to yet another fishing rights legal action (in which the Makah previously did not prevail).

Once I start to consider the possibility of partiality, I note issues such as the map depicting "relatively recent descriptions of the western boundary between Makah and Quileute territory" (Wessen 2019a:2). I note that Wessen, archaeological expert for the Makah in a recent fishing rights case, has left out relevant context that discredits Wessen's delineations between the two tribes' territories. And I smile that my map from my 1990 Quileute tribal sketch in the Smithsonian *Handbook* (Powell 1990:432) has been discredited as well. But I gave the Makahs four extra miles of Quileute coastline. Wessen, on the other hand, gave the Makahs six miles of Quileute shorelands. So, do readers and I have to worry about analytical bias here and elsewhere in Wessen's presentation of data? By the way, the Quileute-Makah tribal boundary is now mandated by a federal court to be Cape Alava. Having been a social scientist for a long time, I now read many cultural opinions with a nostalgia for the academic era before decisions about prehistory started to be made in courtrooms.

Of course, it should go without saying that this public exchange of opinions is scholarship. I am pleased to join Wessen in that effort. We should have coffee and talk this over. Our next article could be co-authored.

Reply to Powell

Gary C. Wessen

Introduction

I would like to begin by thanking the *JONA* editors for affording me an opportunity to reply to Jay Powell's recent remarks about my article *Makahs, Quileutes, and the Precontact History of the Northwestern Olympic Peninsula, Washington* (Wessen 2019a). Here, I will first offer some general comments about Powell's (2020)

remarks, turn to more specific discussion of some of the issues he raises, and then conclude by addressing some broader issues which are directly relevant to the subject.

General Comments

I have 50 years of archaeological experience; 47 of them working on various parts of the

Northwest Coast. Relatively early on during this period, I came to believe that archaeologists and linguists live in different worlds. The two cohorts are trained very differently, work with very different types of data, and—apparently—believe very different things. I acknowledge that I have taken only a single introductory linguistics class after I already had considerable archaeological experience. I got a “B” in the class and left with serious doubts about some of the things I was told. Similarly, while I know nothing of the details of Powell’s training, it is clear that he has only a very limited knowledge of the archaeology of the Northwest Coast, or even archaeology in general. I don’t think either of these limitations are disqualifying. Both Powell and I have Ph.D.s in Anthropology. Thus, I think that both of us are qualified to make judgements about whether the results of analyses from other disciplines appear to make sense in anthropological contexts. Archaeologists do this regularly and—I assume—linguists do too. These differences create significant barriers for Powell and me. In this particular case, I believe there are even bigger barriers. For two native-speakers of the English language, there seem to be moments when we don’t even agree on the meaning of single English words or sentences.

In my article, I presented some examples of Powell citing a source about something that, upon examination, proves to be an inaccurate or misleading account (e.g., Swan’s 1870 Makah genealogy, Irving’s 1921 account of how the Makah obtained Tatoosh Island, and Jacobsen’s 1979 discussion of the separation of the Makah and Nitinat Languages). Powell’s recent remarks continue to exhibit this characteristic. They contain inaccurate descriptions of both things I have said and things that he and Kinkade said in their 1976 paper. Examples of each are considered in this reply.

The first time I read Powell’s response to my article, I was disturbed that he included personal remarks about my motivations and also chose to include a similar attack directed at the Makah Cultural and Research Center (MCRC) in Neah Bay. Upon reflection, I’m actually pleased that

he did, as it gives me a clear opening to address an aspect of this matter that I had wanted to raise earlier. Powell’s personal remarks about me and the MCRC are the first real appearance of the “elephant in the room.” However, Powell only offers the reader very limited peeks at the elephant; he never really explains the elephant clearly or considers it in context. I will address this issue in some detail shortly, but let me begin here by offering the reader a fuller introduction to the elephant.

While most of Powell’s comments are framed as an academic debate between two scholars, that’s not the only thing happening here. In fact, the issues being debated actually have some real world consequences that have largely been ignored. I have a very long association with the Makah Tribe and have acted as an advocate for their position in some judicial and regulatory matters. Powell has an even longer association with the Quileute Tribe and also acts as an advocate for their position in some judicial and regulatory matters. Kinkade and Powell’s 1976 claim that Makahs arrived on the Olympic Peninsula relatively recently and that the lands (and waters) they controlled in the nineteenth century were a part of Quileute territory prior to the Makah arrival has been repeatedly used by the Quileute’s attorneys and their expert witnesses—including Powell himself—in support of that Tribe’s position in legal proceedings regarding hunting and fishing rights. Rulings in some of these cases have significant economic implications. My article may undermine the influence of their argument and so it is potentially a threat to a tool they like to use.

An important background detail in this regard is a trial conducted in Federal Court in 2015 regarding the offshore fishing rights of Makahs and Quileutes; a case which addressed who was where when. The official designation of the case is: *United States v. Washington, Sub-proceeding 09-01* (U.S. District Court for Western Washington, Case No. C70-9213) and I will here refer to it simply as the “Offshore Fishing Case.” Powell did not appear as an expert witness in

this case, but Kinkade and Powell's 1976 claims were offered to the court by other witnesses for the Quileute Tribe.

Albert Reagan: His Training and Work on the Olympic Peninsula

Powell criticizes me for my treatment of Albert Reagan, his data, and what he thought about it. I'm actually an admirer of Reagan in many ways, but, as scientists trying to learn things, we have to be both aware of and honest about what we're working with. I find Powell's description of Reagan to be both contradictory and, occasionally, inaccurate. Powell conflates Reagan's time at La Push (1905–1909) with his entire professional career. Thus, while he described Reagan's archeological work on the Olympic Peninsula as being conducted with “no mentor or advice about excavation and no help” [2020:66], on the following page he tells us that Reagan was “a well-trained professional.” In defense of this claim, Powell says that Reagan “earned a Ph.D. in Anthropology at Stanford in 1925.” Note that the degree was earned 16 years after his time at La Push. Of greater significance, Reagan's Ph.D. from Stanford wasn't in Anthropology; it was in Geology (Tanner 1939; Bratt and Stavast 2013). In fact, Bratt and Stavast (2013:74) specifically say: “Although highly educated for the time, there is no evidence that he ever received any formal archaeological training.”

My discussion focused largely on Reagan's archaeological work, although I also briefly addressed his accounts of oral history. Both are worthy of comment in light of Powell's remarks.

Reagan's 1917 archaeological report was a ground-breaking document. It contains numerous interesting ideas, but it also lacks many important details which limit our ability to evaluate them. The excavations it reports were likely among the first Reagan ever conducted and Powell is probably correct that he had “no mentor or advice about excavation and no help.” As I earlier noted, we don't know which sites he investigated (other than La Push), his assemblage descriptions are extremely minimal, and he

sometimes reported taxonomic identifications which he was likely unqualified to make. With respect to his culture history reconstructions—a key point of focus for both Powell and I—we don't know whether he actually sampled sites with multiple stratified components at the same location or if his chronology represents assemblages from different sites for which he assumed a temporal sequence. If the latter is the case, then the basis for his assumed temporal sequence was never explained. I have personal knowledge of at least 40 archaeological sites on the northwestern Olympic Peninsula and I don't know of any with the range of stratified components described by Reagan.

Beyond quibbling over academic qualifications and missing relevant details, I suggest that the most useful measure of the value of Reagan's archaeological observations is to what degree they are consistent with more recent and much better documented archaeological studies conducted by people who are/were professionally-trained archaeologists. In this regard, as described earlier, Reagan's ideas about the archaeological deposits in this region fail dramatically in two critical ways. First, the idea that Quileute people neither made nor used stone tools is contradicted by both Reagan himself in the 1917 paper and subsequent archaeological studies at both La Push and Toleak Point. Second, the only older component identified in Makah Territory contains a far higher density of stone artifacts—rather than no stone artifacts—as Reagan claimed.

My consideration of the stories recorded by Reagan was admittedly limited and I acknowledge that my review of his unpublished documents focused largely on trying to learn more about his archaeological activities and findings. I was unable to locate anything resembling excavation field notes nor any account of anything he collected. It is not even clear that he actually collected materials for study. As such, I'm not at all surprised to learn from Powell [2020:66] that Reagan's notes on the stories he collected are brief and that he “fleshed out” the details of them many years later. I suspect that his

1917 archaeology paper may be the result of a similar process.

Powell presents three stories which are a part of a larger collection in Reagan's 1934 publication: *Some Traditions of the West Coast Indians*.¹ I was unaware of the first two of these stories (i.e., Battles of Neah Bay, Warm House, and Ozette and Battle at Nittinat). I was familiar with the third story (the Battle of Chimakum) as it was also included in a collection of Quileute stories Reagan published in 1929 and I discussed it in my article. The first two stories contain clear claims of Quileute settlements in the Cape Flattery area. However, the third story makes no such claim. It explicitly states that Quileutes formerly held the lands along the Strait of Juan de Fuca from the Hoko River eastward and makes no mention of a possible former Quileute presence in the Cape Flattery area. Thus, the third story clearly contradicts the claim in the first two. Why are they different? I don't have an answer to this question. Given Powell's comments regarding how Reagan worked, it is tempting to suggest that his methods are a part of the answer. It also raises questions about Reagan's intentions and his attention to detail, as it is difficult to imagine that the contradiction was not apparent to him as well. All we can really say with certainty is that—at least 20 years after he collected them—Reagan published a collection of stories which contain some contradictory details. And finally, given the dramatic scale of their reported recent victories over the Quileutes in the Cape Flattery area and at Ozette, isn't it strange that the Makahs don't seem to have any stories celebrating these events?

Glottochronology and Linguistic Measures of Time

This is a central issue for Powell's claims and I would like to address two different aspects of it: (1) how Powell now describes what he and Kinkade had to say in 1976, and (2) the new information he provides about where the

1976 age estimates actually came from. I should begin by noting that Powell uses the terms "glottochronology" and "lexicostatistics" as though they were synonymous. They are not. Foster (1996:65) explains: "lexicostatistical procedures yield measures of distance among genetically related languages, whereas glottochronological procedures purport to translate measures of relative distance into actual dates."

In his recent remarks on this subject, Powell [2020:70] presents text which he says appeared in their 1976 paper. The language he cites appears on p. 84 of the 1976 paper. Careful comparison reveals that the two statements are very similar, but his recent presentation has altered the earlier one in a few important ways. The citation, as he offers it now, presents three basic assumptions underlying "lexicostatistics" followed by the statement:

While the procedure strives to provide answers to the question of time depth of descendent languages, many historical linguists question the validity of these assumptions. However, in the absence of all other indications of time depth, any evidence of pre-history becomes invaluable.

Immediately following this quotation, Powell comments: "**So, despite our reservations, Kinkade and I used lexicostatistics often and gratefully in the project**, relying on it to compute some sense of the timing of prehistoric events" (emphasis added). The text on p. 84 of the 1976 paper is somewhat different. While it offers the same three basic assumptions underlying "lexicostatistics," they are immediately preceded by the statement: "lexicostatistics is based on three assumptions, all of which are, unfortunately, invalid." The paragraph immediately following the three assumptions in 1976 is not repeated by Powell now. It reads:

These generalizations obstruct the most basic intuitions of historical linguistics. Thus, the generalization

¹ This collection of 17 stories—all recorded more than 20 years earlier—includes 14 from Quileute and three from Lummi. It does not include any Makah or other Wakashan accounts of the events Powell focuses on.

based on these assumptions—that, knowing the percentage of cognates, one can compute the time depth of divergent languages—must also be emphatically rejected. **Many linguists have used lexicostatistics in the absence of any other dating procedure; however, we feel that it is improper to use the methods of lexicostatistics when it suits our purposes, in view of our misgivings about all of its premises**” (emphasis added).

To be clear, while the 1976 paper explicitly implies that the technique was not used, Powell now admits that it was. My initial reaction to this new information was that it is important and helpful, as the 1976 language suggesting that lexicostatistics (glottochronology) was not used left me at a complete loss as to where the reported temporal estimates came from.

Powell’s additional remarks on where their temporal estimate for the separation of Quileute and Chimakum languages came from are also important [2020:71]. After acknowledging all of the flaws in “lexicostatistics,” then admitting that they used it anyway, Powell next acknowledges that the available language data for Chimakum is deeply flawed and of questionable value. Given these conditions, Powell finally explains that their temporal estimate is actually based upon Manual Andrade’s “impressionistic sense” that the difference between Quileute and Chimakum is comparable to the difference between German and English and so a “lexicostatistical” estimate of the time separation of the latter was applied to the former. To this, Powell adds the comment: “That type of reaction to the vocabulary of two related languages isn’t science.” But, as Kinkade said, “It’s an indication.” I completely agree. It isn’t science. To the extent that it is an “indication,” it is fair to ask: “an indication of what?” I accept that they felt it was an indication of the likely date of the separation of the Quileute and Chimakum languages. To me, it is more likely an indication of their need to have something coherent to say about the subject. While Powell

now admits that they used a temporal estimate for the separation of German and English for Quileute and Chimakum, their published discussion in 1976 clearly stated that the assumption that rate of language change is the same for all languages is “invalid.”

Let me be very clear. I certainly understand the limitations of questionable analytical techniques and flawed data sets and I appreciate that, sometimes, that’s all we have to work with. I also acknowledge that analytical exercises conducted under these circumstances sometimes produce interesting insights which may then inspire additional valuable research. This is an important dynamic common to many types of scientific enquiry. I don’t fault Kinkade and Powell for doing so. However, a major problem I have with their 1976 paper is that they didn’t explain the background details Powell provides now and they then summarized their discussion with the statement: “Thus, three types of language data allow us to conclude that Chimakuan peoples originally controlled the northern end of the Olympic Peninsula” [1976:98]. In my view, a much more accurate summary would say that their analyses suggest that Chimakuan peoples may have controlled the northern end of the Olympic Peninsula at some time in the past, but much stronger evidence is required before this can be assumed.

Before leaving this subject I would like to briefly re-visit a related point which I raised in my article and think is important, but Powell has chosen to ignore. Powell conflates evidence that a language has changed with evidence that the group of people speaking that language has moved. This can clearly be seen in his statement [2020:64] that

Even the date of our hypothetical claim of the Makah movement to Cape Flattery is supported by the Makah in their 1990 tribal sketch in the *Smithsonian Handbook of North American Indians*, where Ann Renker and Erna Gunther state in the first paragraph: “The Makah language is the southernmost member of the

Wakashan Family. It belongs to the Southern or Nootkan branch and is closest to Nitinaht, from which it separated about 1000 years ago.”

Renker and Gunther cited Jacobsen (1979:776); Powell made the same argument himself in a legal filing (2015:18). Beyond the fact that this is another glottochronological estimate, Jacobsen was clearly speaking about when a discernable difference between two related languages could first be detected. He was not talking about the movement of people. While I am not an expert in linguistic theory, it is very clear that languages can, and do, change without the speakers moving to a new location. Powell consistently implies that the date for the split in the languages can be assumed to date the movement of people, but he offers no justification for this conflation. As I noted earlier, the suggestion that people began to speak a discernably different language shortly after their arrival in a new land strikes me as unlikely. In my judgement, it is far more likely that the language began to evolve in a different directions after Makahs and Nitinats had been separated for a while. Thus, if the suggested 1,000 year estimate is accurate (something which should not simply be assumed), then I think it is better regarded as evidence suggesting that their arrival in Washington is at least somewhat older.

Powell’s Understanding of Archaeology

To me, Powell’s response contains considerable evidence that he doesn’t understand much about archaeology. This is completely consistent with my earlier stated view that archaeologists and linguists really live in different worlds. I’m willing to let most of these remarks go without comment, but I do want to explore one of them here. At the beginning of his explanation for revising the 1976 temporal estimate (2020:76), Powell says:

Given Wessen’s own repeated admissions regarding the archaeological similarities at midden sites throughout

our part of the coast, it seems clear to me that more than just archaeology is required to identify the cultural groups that occupied the area. That is where cultural and linguistic evidence are crucial—neither of which Wessen, admittedly, has.

Before turning to the core of this matter, I need to note a few other things about this statement. First, I’m assuming that when Powell refers to “cultural evidence” he means ethnographic evidence. Hopefully we agree that both linguistic evidence and archaeological evidence are types of “cultural evidence.” Second, Powell’s repeated claims about my previous position on the relationship between Makah and Quileute cultures and archaeological assemblages is not accurate and I will address this shortly. Third, Powell suggests that I have admitted that I didn’t use cultural (ethnographic?) and linguistic evidence. This is simply not true. In fact, I did consider all of the cultural and linguistic arguments offered by Kinkade and Powell in 1976, more recent cultural and linguistic arguments by Powell in a legal filing in 2015, and a range of traditional Quileute stories collected by Reagan, Frachtenberg, and others. I felt that none of them appeared to offer much support for their ideas. I then turned to the archaeological data in order to explore what it might have to offer.

I certainly agree that a reconstruction of precontact ethnic identities which used ethnographic, linguistic, and archaeological evidence would be better than one using only one or two of these sources. In this regard, note that Kinkade and Powell largely ignored archaeological data in 1976 and Powell does so again now. Their decision to do so in 1976 was not unreasonable as very little relevant archaeological data was available then. Powell’s decision to avoid considering it now is unfortunate as a considerable amount of relevant archaeological data is available today.

Having acknowledged that using all three types of evidence would be best, it is important to add that archaeologists rarely have the opportunity to do so. North American archaeologists must address an interval of ~15,000 years. On

the Northwest Coast, ethnographic data has a time depth of a few hundred years to perhaps a few thousand years. Linguists using glottochronology have proposed relationships going back as much as ~8,500 years (e.g., Mosan [Swadesh 1954]). From my perspective, however, their ideas employ a highly suspect dating technique in the service of ideas generated by linguistic theories. While I have already acknowledged my limited knowledge of linguistic theories, what I do know of this subject suggests that they are largely a body of potentially interesting ideas which are assumed to be correct, but lack any real independent corroboration. As such, a great majority of the existing ethnographic and linguistic data is of limited use to Northwest Coast archaeologists trying to identify ethnic groups in the past.

The message clearly implied by Powell's remark is that, in the absence of supporting ethnographic and linguistic data, archaeologists simply cannot credibly make such interpretations. I suggest that this is nonsense, both theoretically and practically. I would argue that nearly all efforts by anthropologists to recognize ethnic groups rely—in large part—on recognizing cultural traditions. This seems to be the underlying basis for all linguistically-generated claims of ethnic identification and I don't disagree; languages are very important cultural traditions. Precontact languages, however, are not preserved in archaeological sites on the Northwest Coast. Nevertheless, a number of other important cultural traditions (e.g., technology, economy, and art) often are. In fact, it is difficult to make ethnic inferences from the archaeological record, but, as anthropologists, there is no fundamental theoretical problem with attempting to do so (Jones 1997). It is much more the practical obstacles that have limited study of this type on the Northwest Coast so far.

Ten pages of my article were devoted to addressing the likely ethnic identity of the people represented by shell midden deposits on the northwestern Olympic Peninsula. I noted earlier efforts by both Donald Mitchell (1971 and 1990) and Dale Croes (1989) and suggested

a few new possible archaeological signatures for Wakashans. I feel pretty good about the new ones, but I didn't use phrases like "strong evidence" or "allow me to conclude." I acknowledged that some were the results of preliminary study of flawed data sets. Powell dismisses all of it with the two sentences cited at the beginning of this section and moves on. Apparently he didn't like any of it, and by "it" I have to assume that he is including the work of Mitchell and Croes as well.

For the record, one of the possible new ethnic signatures I discussed using was the vertebrate faunal assemblages from Makah and Quileute shell middens. Ethnographic accounts report some points of difference in the two economies and I suggested some possible faunal candidates (2019a:37). Since the publication of my article, Steve Samuels and I have been doing more work with these assemblages and we presented a paper describing additional analyses supporting this idea at the 2019 Northwest Anthropological Conference in Kennewick. Our work indicates that both absolute bone densities and the relative proportions of some animals—or groups of animals—appear to be significantly different between Makah and Quileute sites. We are currently doing more with these faunal assemblages and have also begun to look at the artifacts, where we also think differences may exist. No publication is in preparation at this time, but we think we are seeing some interesting things and hope to share more about this soon.

Competing Models and Their Implications

A central premise underlying my discussion here is fundamental and I'm going to assume that Powell agrees with it: *if the techniques employed by linguists—properly applied—and the techniques employed by archaeologists—properly applied—both have the potential to inform us about the precontact history of the Northwest Coast, then the results of both types of studies should be telling essentially the same story.* After all, there is only one real precontact history of

the Northwest Coast (or anywhere else, for that matter). In this case, Powell and I offer very different reconstructions of the precontact history of the northwestern Olympic Peninsula. Thus, something is wrong here. Neither of us offers a complete or highly detailed model and the scales of our discussions are different. Mine is heavily focused on two related details of the northwestern Olympic Peninsula: when did Wakashans arrive and were Chimakuans displaced by their arrival? Powell takes a wider view of the entire northern Olympic Peninsula over a much broader interval of time. Each of us drifts a little into Vancouver Island as well, that being the agreed upon point of origin for Wakashan speakers. Given our differences, it is useful to summarize the main points of each model in order to see where they differ or, possibly, agree.

My model has Wakashan speakers crossing to the northwestern Olympic Peninsula at least 4,000 years ago. Powell [2020:70] says that I said “Makahs came across.” This is not accurate. While I sometimes used the phrase “Makahs and/or their ancestors,” I explicitly stated in my Summary discussion [2019a:42] that “the inferred ethnic identification is for Wakashan speakers in a general sense, not for Makahs specifically.” In fact, my model rejects the idea that there was ever a self-identified “Makah” group residing on Vancouver Island. Rather, I think that the “Makah” identity emerged after their ancestors arrived in the Cape Flattery area.² My model doesn’t really address who occupied the northwestern Olympic Peninsula prior to the Wakashan arrival, beyond the observation that I see no credible evidence for an earlier presence of Chimakuan speakers. This is not an emphatic statement that Chemakuan speakers were never present in these lands, but I am unaware of any credible evidence that they were. In this regard, I note that Powell [2020:68] says: “Wessen argues in his article that the Cape area

may have passed back and forth between Chimakuans and Wakashans several times.” Powell makes this claim without explanation and I have no idea what he is talking about. I certainly noted that oral histories suggest that control of the Cape area alternated among Wakashan speakers in the past (e.g., Irving 1921), but I never expanded it to include Chimakuans. Not only did I never suggest that Chemakuan speakers were previously here, I also acknowledged that the currently available archaeological data for them is so limited that I cannot even suggest possible markers of their presence.

The original Kinkade and Powell (1976) model is much broader and remains largely intact in Powell’s comments now. This model claims that “the entire northern Olympic Peninsula was originally controlled by Chimakuan peoples” (1976:95). Powell argues that an already existing self-identified “Makah” group from Vancouver Island moved to the Cape Flattery area, replacing Quileutes formerly living there [2020:75]. Their model also argues that the S’Klallam people arrived on the northern Olympic Peninsula sometime after the Makahs established themselves here, further displacing Chimakuans and “explaining” why Quileutes and Chimakums were widely separated in the early nineteenth century (1976:97). The original Kinkade and Powell argument was that the Makahs arrived approximately 1,000 years ago. Powell has now modified the temporal estimate for the Makah arrival to “sometime between about 650 and 2,000 BP” [2020:76]. (Note that this is the only detail of their original model which he felt needed modification.) He justifies the 650 year estimate by referral to Quileute oral history, stating: “Such recalled folk history events are generally considered to be retained for hundreds of years rather than thousands. Happenings earlier than five to six hundred years ago tend to become myths and legends or simply be forgotten. It’s an anthropological

² While “Makah” is the historically accepted name for this group, it is the S’Klallam (i.e., Salish) name for them. Their own Makah language name for themselves is: “*q’idičča?á-tx*.” The English translation of this term is: “People of the Cape;” clearly a reference to Cape Flattery.

presumption.” No citation is offered in support of this claim.³ Powell [2020:77] justifies the 2,000 year estimate by saying it is “largely based on the archaeological site reports” I discussed in my article, but he doesn’t explain how.

So, we have two very different models which share little beyond that both agree that Wakashan speakers came from Vancouver Island to the northwestern Olympic Peninsula at some time in the past. I don’t believe that either model is sufficiently well documented to simply be accepted at this time. However, given the premise I identified at the beginning of this section, I do think that it is useful to consider what the real world implications of each model are for the data and ideas of the competing one. That is, what are the implications of my model for the linguistically-generated ideas and what are the implications of the Kinkade and Powell model (as now modified by Powell) for archaeologically-generated ideas? In the following paragraphs, I will consider the implications of the Kinkade and Powell model (as modified by Powell) for archaeologically-generated ideas. I won’t do the same for my model and while this may at first seem unfair, there are two good reasons for this approach. First, much of my recent article already does this. Secondly, there has been only very limited additional argument or data in support of the original 1976 claims. The original argument is quite brief (less than 2,000 words) and, to my knowledge, no other linguist or cultural anthropologist has conducted any additional analysis which actually supports or advances their ideas. As Powell himself says of their 1976 paper: “Over the years, a number of studies **have assumed it is right**” (emphasis added) [2020:64]. While it is fair to say that no linguist or cultural anthropologist has actually challenged them either, I think that is hardly evidence of strong support. Equally plausible, it is evidence that the issue is obscure and only of interest to a relatively small number of researchers. Powell’s much more lengthy defense of it now

contains the only new information offered in support of their ideas. In sharp contrast, neither the original 1976 discussion nor Powell’s recent remarks make any attempt to consider what their ideas imply for the archaeological reconstructions and there is now a substantially larger and much better documented archaeological data base for the entire northern Olympic Peninsula.

Taking a chronological approach, the earliest part of the Kinkade and Powell model argues that the entire northern Olympic Peninsula was originally controlled by Chimakuan people. The oldest archaeological site on the northern Olympic Peninsula is the ca. 13,800 year old Manis Mastodon Site [45CA218] (Waters et al. 2011). This very important site has been the subject of only limited study thus far and it currently offers no basis to suggest the ethnic identity of its occupants. While it is merely speculation on my part, I think it is extremely unlikely that they spoke a language which can be traced to Chimakuan.

Moving up several thousand years, there are a number of sites on the northern Olympic Peninsula which contain lithic assemblages dominated by leaf-shaped bifaces and small unifacial tools made of basalt or vitrophyric dacite. Washington archaeologists usually call these “Olcott” sites (after Kidd 1964). Excavated Olcott assemblages on the northern Olympic Peninsula include those from Slab Camp [45CA580] (Gallison 1994), the earlier component at one of the Sequim By-Pass sites [45CA426] (Morgan et al. 1990), and from an unnamed site [45CA432] close to the northern end of Lake Ozette (Conca 2000). Numerous other recorded sites and unrecorded sites represented in private collections in this area are also known to contain artifacts that can be attributed to Olcott assemblages. Moreover, Olcott sites are widely accepted to be a local expression of a much broader group of sites with very similar materials which also occur elsewhere in western Washington, in eastern Washington, Oregon, Idaho, and British

³ I find this statement remarkable. I agree that it is a presumption; that is, an idea that is assumed to be true, and used as the basis for other ideas, although it is not known for certain. To me, Powell is pointing to a presumption as “evidence” here.

Columbia (Chatters et al. 2011). Elsewhere, they have been referred to by such names as the Old Cordilleran Culture, the Pebble Tool Tradition, and the Cascade Phase. By whatever name, it is widely accepted to represent the Early to Middle Holocene. Making ethnic assignments this far back in time is tenuous but, as I noted in my article, Carlson (1990) has suggested that these sites represent the ancestors of Salishan speakers and I believe that many Northwest Coast archaeologists consider that this is a reasonable suggestion. While Carlson's association of Olcott assemblages with ancestral Salishan speakers remains only an interesting idea, the Kinkade and Powell model argues Carlson is wrong and that they actually represent ancestral Chimakuan speakers. Note that if Kinkade and Powell are correct about this, then Chimakuan speakers not only once controlled the entire northern Olympic Peninsula, they also controlled all of Washington and parts of Oregon, Idaho, and British Columbia. I suggest that this is unlikely.

Moving up in time again, the Kinkade and Powell model says that Wakashans arrived sometime between 650 and 2,000 years ago and that the S'Klallams arrived here sometime *after* the Wakashans. This would mean that the S'Klallam presence on the Olympic Peninsula is no older than ~1,950 years and could be less than ~600 years. There are relatively few well-sampled sites on the northern Olympic Peninsula that contain an extensive record of deposits falling within this interval. Two that do, however, are 45CA426 (Morgan et al. 1990), and the Číxwícən site [45CA523] (Larson et al. 2006). The younger component at 45CA426 represents the interval between 2,700 and 1,000 years BP. The occupation at 45CA523 extends from 2,700 BP into the early historic period. The Kinkade and Powell model argues that a potentially large part of the occupation at both of these sites represent Chimakuan speakers. The artifact assemblage from the younger component at 45CA426 has been identified as representing the Marpole Phase. While the artifact assemblage from 45CA523 has yet to be described, my impression—admittedly very

limited and based upon a few publically available photographs—is that Marpole Phase materials are likely present here as well. Marpole Phase assemblages occur widely in the northern Puget Sound and adjacent Strait of Georgia regions and have long been considered to represent pre-contact Salishan speakers (Mitchell 1970, Burley 1980, and Carlson and Hobler 1993). If Kinkade and Powell are correct and the Marpole Phase really represents Chimakuans, then Chimakuans not only dominated a vast part of the southern Northwest Coast and interior Pacific Northwest for much of the Early and Middle Holocene, they still held an area substantially greater than the northern Olympic Peninsula—including the Fraser River Delta—as recently as 1,500 years ago. I suggest that this is also unlikely.

Finally, returning back to the Northwestern Olympic Peninsula, Kinkade and Powell said that Wakashan speakers arrived approximately 1,000 years ago and Powell now accepts that it could have been as much as 2,000 years ago. My article reported that there is no archaeological evidence of a new arrival of people or any real change in material culture or economic orientation approximately 1,000 years ago. There is, however, a change in the archaeological assemblages in the Cape Flattery area around 2,000 years ago (or slightly more recently). This is the distinction between the older and younger Makah sites. Thus, I infer that Powell believes that the older Makah sites actually represent Chimakuans. Ten pages of my article were devoted to addressing the likely ethnic identity of the people represented by these deposits and concluded that it is far more likely that they were Wakashans; I won't repeat my case here. Powell doesn't believe that archaeologists can identify ethnic groups in the past.

In sum, the Kinkade and Powell model's implications are dramatically incompatible with archaeological findings from not only the northwestern Olympic Peninsula, but from a much larger area in northwestern North America. Further, the model's implications beg new questions, particularly: If Chimakuans really were such a large and widespread group for much of

the Holocene, why did they have such a limited presence in the early nineteenth century? Clearly, the arrival of Wakashan and Salish speakers on the Olympic Peninsula cannot account for the much larger loss of territory their model implies. Given both the very large body of archaeological research that would need to be rejected and what Powell has now admitted about how he and Kinkade actually obtained their temporal estimates, I do not expect his recent comments to change many minds.

The Elephant in the Room

As I mentioned earlier, I take issue with the tone of much of Powell's remarks suggesting that the Kinkade and Powell 1976 paper was essentially a hypothetical academic exercise. In support of this claim, he says that they used phrases like "educated guess," "working hypothesis," and "hypothesized homeland" [2020:64]. While this is technically correct, a little deeper look is worthwhile. Their 1976 paper is not very long (slightly less than 7,000 words⁴) and the portion of it in which they make claims about Makahs and Quileutes is only about 1,900 words. A word search of their article shows that the phrase "educated guess" appears twice, "working hypothesis" once, and "hypothesized homeland" once. Moreover, all four of these appearances are within the approximately 5,000 words of more general discussion. None of them are used in the text addressing Makahs and Quileutes. Rather, the latter uses statements like "A great deal of evidence suggests that the entire northern Olympic Peninsula was originally controlled by Chimakuan peoples" (1976:95) and "Thus, three types of language data allow us to conclude that Chimakuan peoples originally controlled the northern end of the Olympic Peninsula" (1976:98). In contrast, Powell's recent defense of their ideas includes six examples of describing their ideas as a "hypothesis" [2020:63, 64, 65, 70,

and 78] and three additional examples where the word "hypothetical" is used in the same context [2020:64, 70, and 77]. Powell now even describes their thought process by saying: "there were a number of conjecturable if not convincing issues to back up such a construct" [2020:65]. Thus, in their 1976 discussion of Makahs and Quileutes (presented in an anthropological forum), statements acknowledging that this is just a hypothesis do not appear, while Powell's defense now (also in an anthropological forum) acknowledges this fact at least 10 times. The reader should note, however, that when Powell, other expert witnesses, and Quileute attorneys present the 1976 discussion in judicial and regulatory forums, they never describe it as merely a hypothetical academic exercise.⁵ They offer it as a credible scientific analysis and urge judges and resource regulators to use it in their rulings. Powell wants it both ways, depending on the context of the discussion.

I will now turn to the specific charges Powell makes. While his claims and innuendos regarding "politically-motivated" behavior are largely directed at me, he also brings up a recent episode involving himself and the MCRC in Neah Bay. The episode has nothing to do with me or my article, yet Powell offers it as evidence of politically-motivated behavior "which certainly impedes scholarly exchange and cooperation" [2020:73] that has something to do with me. I was unfamiliar with this episode when I first read his remarks and, though I still don't know all the details, I think I now know enough to provide some useful background information. I don't speak for the MCRC, or anyone else in Neah Bay, but I think that it is accurate to say that Powell is widely viewed there as a Quileute partisan. While I do not know the specific timing, my understanding is that his request for the data came before the Offshore Fishing Case had actually reached a court room, but pre-trial maneuvering by both sides was already in progress.

⁴ Not counting the bibliography and figure captions.

⁵ Powell's 2015 document prepared for Quileute attorneys in a case addressing S'Klallam vs. Quileute hunting territories (briefly discussed in my article) is a good example. Expert witness testimony offered in the recent Offshore Fishing case is another.

People at the MCRC were quite familiar with his earlier claims and were concerned that the results of what he was proposing to do might soon be submitted as “evidence” against them in the upcoming trial. I don’t think that this was an unreasonable reaction. As such, I suggest that his remarks about “scholarly exchange and cooperation” need to be considered in this light.

Powell’s attacks against me are well summarized on the first page of his remarks where he says:

Wessen has regularly remarked and described Makah and Quileute cultural patterns as homogeneous. But now he often characterizes the Quileutes as an inland-focused tribe, which contradicts the factual record but may be useful in case of a future fishing rights legal action.

There’s actually quite a bit to address here.

Powell’s first claim, that I have regularly described Makah and Quileute cultural patterns as homogeneous also appears in longer form at the end of his comments. There [2020:78], he defends this assertion by citing several things I have written about Makah and Quileute culture. The reader will note first that none of the cited remarks actually show me saying that Makah and Quileute cultural patterns were “homogeneous.” The cited remarks document me saying that they were “similar” or “very similar.” The words “homogeneous” and “similar,” or even the phrase “very similar,” are not synonymous. Homogeneous means: “Of the same kind; alike” while similar means: “Having a resemblance in appearance, character, or quantity, without being identical” (WNWDAL 1960).

In fact, I have never said that Makah and Quileute cultures were “homogeneous.” Kinkade and Powell (1976) did and Powell makes the claim again now. Also, while he claims that I have said this regularly, most of his evidence is from documents written in the 1980s and 1990s. The most recent statement he cites is from a 15-year-old report. Some further context

is helpful here as well. Every statement I made about Quileute technology and economy prior to 1995 relied heavily on ethnographic accounts as the available archaeological data from Quileute sites before this time was extremely limited. It consisted of an aggregate sample of 228 stone and bone artifacts and no quantified faunal assemblages from controlled excavations. I mark 1995 as a turning point because this is when the first quantified faunal assemblage data from a Quileute site became available. My limited work at 45JE8 provided the first quantitative faunal data for a sample of 707 bone fragments and increased the aggregate artifact sample size to 248. The aggregate sample available for Makah sites in 1995 was approximately 47,700 bone and stone artifacts and 92,500 quantified bones. In the 24 years since 1995, the aggregate sample for the Quileute sites has increased a little (now, 284 artifacts⁶ and 3,900 quantified bones). In contrast, substantially more information became available for Makah sites during this period. By 2017, the aggregate sample sizes were approximately 50,500 bone and stone artifacts and 154,700 quantified bones. Thus, a few things should be clear. First, statements I made about Makah and Quileute technology and economy prior to 1995 were based upon my assumption that they were similar—even very similar in some respects—because I had only very limited archaeological data. After that time, with both the appearance of some better Quileute data and an increasingly large body of Makah data, it became more and more clear that better opportunities to examine such questions were available and I began to think more about them. Beyond better opportunities, I believe that scientists actually have an obligation to re-evaluate their ideas as new relevant data becomes available.

The second part of Powell’s charge is also worthy of comment. That is, that I now “... characterize the Quileutes as an inland-focused tribe, which contradicts the factual record.” In my article, I noted that descriptions of the Quileutes as a group with a strong orientation to interior

⁶With reference to earlier discussion of Albert Reagan’s claim that the Quileutes did not make or use stone tools, note that 149 of the 284 specimens are chipped or ground stone artifacts.

and riverine resources is neither unique nor original to me. Frachtenberg (1921) said so and Singh's (1956) account of their economy makes the same case. Powell himself (1997:29–30) said that as much as 70% of their late-precontact population was associated with interior rather than coastal settlements. As such, I don't agree that the claim "contradicts the factual record." Of more immediate relevance, it is not accurate to say that I now characterize them as an inland-focused tribe. I noted that ethnographic materials indicate that interior and riverine resources were important to them, and added that—in the absence of faunal assemblages from Quileute sites in interior settings—it is difficult to say much about the details of their diet. To the extent that I offered an opinion about precontact Quileute use of maritime resources, I suggested that their use of the marine environment probably did predate the arrival of Wakashans, but that their earlier use was probably less intensive and less sophisticated. The character and many of the details of late-precontact and early historic Quileute maritime activities appear to have been adopted from Wakashan ideas and technologies after the latter's arrival.

Finally, Powell suggests that my article may have been written to influence "a future fishing rights legal action" [2020:63]. So, what was behind my decision to write it? First, nobody with the Makah Tribe or its attorneys asked me to do it. No one in either group was even aware that I was working on the article until the first draft of it was completed. I had several reasons for writing the article. I had published an article with Dave Huelsbeck in 2015 which described the older sites near Cape Flattery and focused on their changing environmental contexts. That article, however, did not really explore the implications of the new data for interpreting culture history. I have had an interest in this subject for a long time and I have increasingly focused on how faunal assemblages might contribute to this conversation. This would be in about the 2014–2015 time frame and—full-disclosure—I was also preparing a report and other materials for the Makah's attorney's use in the Offshore

Fishing Case. In that context, the Kinkade and Powell (1976) ideas are relevant to the Quileute's claim that they formerly controlled Tatoosh Island, and I expected that they would make an appearance in the proceedings. They did.

When the Offshore Fishing Case and its associated appeal were over, I continued to think about these issues and decided that further discussion was warranted. My major goals were to consider the recently described older sites near Cape Flattery in a broader regional context and examine to what extent they are consistent with the Kinkade and Powell model. In doing so, I introduced some new ideas about how Wakashans might be recognized in the archaeological record. I didn't claim that I had proven Kinkade and Powell to be wrong nor that I could unequivocally recognize Wakashans. In fact, I acknowledged the importance of further study of my ideas.

Having said this, was I also aware that my article might be useful to Makah attorneys in future cases? Of course I was. I considered that it might help give the court a more balanced and up-to-date view of the contention that all of the northwestern Olympic Peninsula was held by Chimakuans until 1,000 years ago. I didn't think that this was likely to be powerful tool, but I thought it could provide an informed response to ideas that have long been assumed to be correct. This possibility did not upset me. In fact, it pleases me, but it was not an important consideration in choosing to write the article, nor did it influence what I had to say. It's a part of the context in which this discussion occurs. In fact, I briefly mentioned it in the introduction to my article. It is therefore not surprising that comments regarding possible impacts to legal proceedings have become increasingly common in this exchange of views.

For the record, I briefed *JONA* editor Darby Stapp about this dimension of my article when I first approached him about submitting it for publication. I advised him about the history of the issue, the Offshore Fishing Case, and how the article might be viewed as partisan in some places. Neither of the peer-review archaeologists

for my article expressed a concern about politics influencing my discussion and I am grateful to *JONA* for being willing to publish the work, despite this aspect of it.

Some Final Thoughts

So, I write my article, *JONA* publishes it, Powell comments on it, and I reply back to Powell. Where are we now and what might we say we have learned? For myself, the experience has certainly reinforced my long-held belief that archaeologists and linguists live in different worlds. In Powell's response and my reply to it, each of us has offered new perspectives on the ideas we came with, but I doubt that he has changed his opinions very much, and I haven't either. This is not, however, to say that our exchange has been without interesting and potentially important moments. For me, Powell's explanation of where the temporal estimates in the Kinkade and Powell 1976 paper actually came from is the most important new insight he offers. It explains an important detail of their 1976 discussion which was never clear before. I admire Powell's candor in this regard, but his explanation does not increase my confidence in their conclusions. Hopefully, the reader has also gained important new insights regarding scholars serving as expert witnesses in legal and regulatory contexts and how these roles can be complicated. And no one should assume that—now exposed—the elephant has left the room; he's still here and is unlikely to be going anywhere anytime soon.

The experience of preparing this reply to Powell has caused me to revisit the whole matter of how archaeologists and linguists relate to each other and just how different their worlds really are. The truth is, however, that I don't really know any linguists, including Powell. It has therefore been important for me not to conflate Powell with linguists in general. My impression is that—broadly at least—Powell is similar to other linguists who have written about Northwest Coast prehistory. In this regard, I suspect that our differences of opinion reflect

conditions and issues which extend well beyond when Wakashans arrive on the northwestern Olympic Peninsula.

Ideas about Northwest Coast prehistory generated by linguists did not begin with the appearance of glottochronology in the early 1950s, but the claim that changes between related languages could be dated stimulated considerable such analyses in the 1950s, 1960s, and 1970s (Foster 1996). Note that this was occurring at a time when there was relatively little archaeological data from the Northwest Coast. Linguists were free to propose whatever they wished. By the 1980s, however, increasing doubts about the reliability of glottochronology had caused it to fall out of favor and there have been very few, if any, glottochronological reconstructions of Northwest Coast prehistory to appear in the last 30 years. Against this backdrop, the pace of archaeological research on the Northwest Coast has accelerated considerably since the 1970s. To my knowledge, there is no place on the Northwest Coast where the currently available archaeological data offers more than extremely vague support for the temporal estimates generated earlier by linguists using glottochronology. Apparent misfits are common. As an archaeologist working on the Northwest Coast, I don't take such estimates very seriously, nor do I know any archaeologists who do.

For me, the experience has also underscored how much such historical linguistic research appears to be a closed system. Beyond lacking a credible way to estimate time depth, historical linguists working with Northwest Coast languages appear to be tied to data sets (i.e., vocabularies and grammars) which are unlikely to get much larger or much better in the future. That is, it is difficult to imagine that there are many previously unknown high quality vocabularies and grammars yet to be discovered. Thus, historical linguists appear to be increasingly restricted to re-examining the same data as their theoretical assumptions change. As I observed earlier, linguistic theories—as they relate to historical linguistics—appear to be largely a body of potentially interesting

ideas that are assumed to be true, but lack any real independent corroboration. This is not to suggest that archaeological research is free from problems. It certainly is not. However, archaeological research benefits from a data set that continues to both expand in size and improve in quality, as well as making analytical and theoretical advancements.

While it is certainly not my intention to evaluate Powell's career, a few observations from what he has shared are worth considering in this light. As I have already shown, while he and Kinkade denied using glottochronology in 1976, Powell now acknowledges that, in fact, they did. Powell now justifies their decision by acknowledging that, as linguists, they had no other way to estimate time depth. Basically: "the tool is of questionable value, but since it's the only one we have, we're using it anyway." Note also that Powell was prepared just "a few years ago" to conduct another of these questionable analyses in order to date the timing of the Makah-Nitinat separation (in the context of the Offshore Fishing Case where the results of such an analysis might have been introduced as evidence). Thus, while the use of this technique in 1976 was not unusual; Powell's willingness to use it again recently was. This point is also clear in his remarks about my article. Neither Kinkade and Powell, nor any subsequent linguist, has elaborated on their initial analysis and claims. Embleton's (1985) work with Wakashan languages comes closest to being such an elaboration, but it doesn't really address when Wakashan speakers arrived in Washington and Powell rejects it anyway [2020:73]. After Embleton, new linguistically-based temporal estimates for historical events on the Northwest Coast become increasingly rare. Indeed, Powell's only subsequent actions to follow-up on their 1976 ideas was his recent approach to the Makah Cultural and Research Center to try to date the Makah-Nitinat split and his response to me now. The former was not successful and the latter offered no new time estimates based on glottochronology (although it offers support for earlier glottochronological estimates he and

Kinkade previously rejected). Powell appears to be outside the mainstream in this regard, and it is clear that he has few—if any—real options to expand or reinforce his claims. In sum, I think that historical linguistic temporal estimates based upon the glottochronological techniques developed in the 1950s will remain largely unrelated to the continuing archaeological research on the Northwest Coast. Linguists may continue to talk about them, but they will be mostly talking to themselves.

Finally, I would like to close with a few thoughts about the personal tone of both Powell's response and my reply. As I stated at the beginning of these remarks, I was initially put off by Powell's use of both direct attacks and innuendos aimed at me. They have no place in a scholarly exchange of views, but, as I hope I have made clear, this exchange is more than simply a scholarly exchange of views. I have tried to set the record straight and explain the context clearly and honestly.

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COMMENT AND REPLY

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“Notes Regarding my Adventures in Anthropology and with Anthropologists” by John Swanton with an Introduction by Jay Miller

Abstract *The Journal of Northwest Anthropology is making available the unpublished 1944 autobiography of John Reed Swanton, in keeping with its commitment to help preserve and make available information important to the history of Northwest History. Swanton, mentored by Frederick Ward Putnam at Harvard and Franz Boas at Columbia, was involved in major Archaeological and linguistic projects while at Harvard then spent his long career at the Bureau of American Ethnology. He conducted important fieldwork with the Jesup North Pacific Expedition in the Northwest before moving on to all the major tribes of the southeastern U.S, both there and exiled to Oklahoma. Associate JONA Editor Jay Miller introduces the autobiography with an overview of Swanton’s life and a listing of his major publications.*

Keywords

Swanton, linguistics, Haida, Tlingit, biography

Introduction

Jay Miller

Raised by matriline Yankee women, faithful to the direct communication with spirits by Baron Emmanuel Swedenborg to interpret the Bible, John Swanton, not surprisingly, found rapport with native matrilineal societies, first in the Northwest among Haida and Tlingit, then life-long with the populous farming nations of the Southeast.^a While a student at Harvard, maestro Frederick Ward Putnam involved Swanton in all the major research ventures of his time, including the Trenton gravels, Ohio earthworks, and Chaco Canyon. [His subsequent major publications are listed in the bibliography at the end herein.]

He also developed an interest in linguistics, which came to include the Bushotter Lakota texts. Using Chinook linguistic materials recently recorded by Franz Boas for his Harvard Ph.D. under Putnam, Swanton passed his U.S. civil service, which featured Lakota, to become the first on the Bureau of American Ethnology (BAE) staff with specialized university training.

Swanton’s long career began problematically when he was sent to the Haida and Tlingit

with complex funding: his staff salary was paid by the BAE under W J McGee but his field expenses came from the Morris Jessup Fund at the American Museum of Natural History in New York City via Franz Boas, who supervised Swanton’s fieldwork through to publications by the BAE. Boas, also a F. W. Putnam protégé, worked with Swanton when he provided his own Chinookan fieldnotes dictated by Henry Cultee at Bay Center, WA.

[Unstated in his autobiography, Swanton married (16 December 1903) after the Haida research and then took his bride (Alice M. Barnard) with him to the Tlingit. In time they had three children: Mary Alice, John Jr., and Henry Allen.]

Even as he did fieldwork and published on Creeks, Choctaws, Chickasaws, and Southeastern Siouans, his contributions to the *Handbook of North American Indians* kept him involved in the Northwest. Unlike Boas’s then dominant idea of a migration from the interior, Swanton correctly looked for the origins of the Salishan family on the coast. He also drew parallels between the

^a Swanton, John R.

1944 *Notes Regarding my Adventures in Anthropology and with Anthropologists*. Manuscript no. 4651, National Anthropological Archives, Washington, D.C.

Northwest and Southeast in terms of the importance of wealthy and warfare in these nations.

He was very active professionally as a founder of the American Anthropological Association, serving as its president for one year and as editor [1911, 1921–1923 as an acceptable compromise for Boasians]. He was president of the Anthropological Society of Washington and the American Folklore Society, vice-president of Section H of the AAAS, served on the Social Science Research Council and the National Research Council, and was elected to

the Washington Academy of Sciences and the National Academy of Sciences (1932). Combining all of his life's work, he chaired the De Soto Commission and wrote the massive final report without naming his own authorship.

Swanton died in Newton, Massachusetts on 2 May 1958, at the age of 85. In all, his following memoir (Swanton 1944) provides personal glimpses of the great and near great anthropologists active during the late 1800s and the first half of the 1900s.

Notes Regarding my Adventures in Anthropology and with Anthropologists

John R. Swanton

When I was still in my teens in Gardiner, Maine, where I was born, I instituted a system of self-elevation consisting of a course in the reading of miscellaneous volumes on human history. I had an American history of course, and I had a history of Greece, and a universal history. I saved up my various earnings and donations for a three volume work entitled "The Seven Great Monarchies of the Ancient Eastern World" by George Rawlinson, brother I believe of the celebrated Henry who opened up Assyrian archeology. [I still have it.] Later I possessed myself of a copy of Labberton's "Historical Atlas," and at a much later period was reminded of this by being presented with another copy by my associate in the Bureau of American ethnology, Mr. J. N. B. Hewitt. This being the age of simplicity and naive credulity, I accepted everything I read as gospel truth. The serpent of adverse criticism had not yet entered into my peaceful Garden of Eden. Besides the books I owned I drew rather heavily on the small but excellent library of my native city, and among its volumes fate led me to Prescott's "Conquest of Mexico," the reading of which marks a definite turning point in my career. I may be more specific and say that the turning point came when I read those passages of Prescott's magical prose in which he describes

the pyramids of Teotihuacán and speculates upon their origin. That directed my interests from history in general to ancient history in particular and still more to the ancient history of our own continent. Full of that enthusiasm I followed Prescott's Mexico up with his "Conquest of Peru," and then went on to H. H. Bancroft's monumental work, confining myself to the first three volumes it is true, and I also added "Footprints of Vanished Races," and several others dealing with the "mysterious Mound Builders," among which I remember a most unsound, but therefore highly interesting, volume entitled "Ancient America" by one Baldwin who I believe was a former congressman. Perhaps that accounts for his ability to make myths attractive.

In the fall of 1890 I accompanied my chum Alexander Forsyth and his family to Chelsea, Massachusetts, where Alex and I entered the high school to prepare for Harvard. Our reason for selecting Chelsea as a spring-board undeterred by the current by-word "as dead as Chelsea" was the fact that my chum had relatives there. At the end of that year we took our entrance examinations, and Alex and his family moved to a house in Somerville just over the Cambridge line so that he could attend college. In the meantime, however, I had decided that, as I should be

obliged to enter with conditions, it would be best to take one more year at Chelsea, and this time my family took a house in that city, one on the slope of Bellingham Hill and just around the corner from the school. My brothers were then at the M.I.T.

Our family was increased, nevertheless, by a cousin from Bath, Maine, with the same first and last names as myself. He entered the same school to prepare for "Tech," where his brother Fred had been a senior, in the class of 1890. Fred and John were almost diametrically opposites, different in character, Fred being studious and aloof, while John C. was athletically inclined and a social favorite with both boys and girls. John C. played first base on the Chelsea team that year and his home run brought in the winning runs in the game against Lynn. We drilled together in what was called the Second Massachusetts School regiment and were in the competing company in the annual field day at Brookline where our school landed about in the middle. Next year we all moved over to Somerville to live with Alex's family, and I entered Harvard in the class of 1896 while John C. entered "Tech." After a time, however, my brothers found the distance from the Institute too great and left us.

My decision to take a second year at Chelsea was vigorously opposed by my chum but proved, unknown to either, of us, to mark another turning point in my career, for it was in the Harvard catalogue published ... that instruction was indicated for the first time in "American Archaeology and Ethnology," though only to "properly equipped graduates." When I saw that I remembered Teotihuacán and decided that American archeology and ethnology was what I was going to study. In order to prepare for the "graduate work" offered, I called at the home of Prof. Frederick W. Putnam who had succeeded Jeffries Wyman¹ in charge of the Museum of Archaeology and Ethnology and asked him to suggest suitable preparatory courses. His suggestions were decidedly variegated and may be set down for the edification of present day Americanists. Besides the three,

or rather one and two half, prescribed courses in English, I added an advanced course in English composition under Professor Barrett Wendell which proved of utility later as I knew little about English in spite of having spoken what went by that name in New England, and having written a history of the world on a series of composition books. In this connection I may add as one of my youthful enterprises that I set down long lists of monarchs, presidents, and other heads of states, even including, I believe, the monarchs of Ashantee or Dahomey, I forget which. A contemporary interest in geography was evinced by another set of blank books in which I set down the names of every town and city in the six New England states together with the latitude and longitude of each—somewhat liberally construed. Prof. Barrett Wendell had a red beard and a pronounced English accent but professed to be something of an anglophobe on account of some Dutch ancestry. We had to present daily compositions in his course and these were afterwards exchanged and we were asked to correct one another's. I remember to have handled most severely the three best compositions handed to me because I could not seem to find anything the matter with them and thought it was my duty to confirm. One of these compositions was by a youth who afterwards became well known. I was rather good in expositions, not so good in forensics, and distressingly medium in imaginative work though one of my compositions was spoken well of and another was rather good except a bit too moral. I believe I shocked Prof. Wendell when the question of seventh-commandment stories came up by saying that, as to that commandment, all I wanted of my heroines was to keep it. Such a sentiment was not so heretical then as it is today.

There was one course prescribed for freshmen besides English, Chemistry A, which consisted in one lecture a week in chemistry for the first half year. This was in part to acquaint budding intellects with a fundamental subject which they might have escaped in their preparatory

schools, and in great part it would seem to give a somewhat mellow professor something to do instead of retiring him. He died a year later, I believe, but I do not know whether my class was to blame or not. Our mental labors were shortened in this course from the very beginning since we were told that everything that was to be on the examination paper would be told us at the last lecture. This has a kind comic opera suggestion, but Prof. Josiah Cook was no comic opera performer. He had been one of the great chemists of his time and he made one remark which has been with me through the years and that is more than many of my other teachers can say. This was at his very first lecture when he said something like this, "During my life I have had to learn two different systems of chemistry, and, I do not believe that the one I am about to explain to you is final, but it has been found extremely useful." It is only this small man who gives you to understand that he has the last word on any subject.

The remainder of my courses as they were suggested by Prof. Putnam, are as follows: One course in general history, two courses in Fine Arts, including Prof. Horton's famous Fine Arts 3, Davis's courses in Meteorology and Physical Geography, Shaler's introductory course, Geology 4, and his advanced course in Palaeontology, Introductory courses in Zoology and Botany, one and a half courses in French, three in Spanish, and Philosophy 1. In this last we had three of the great men of our time as lecturers, Palmer in Logic, James in Psychology, and Santayana in Philosophy proper. Palmer had to me the atmosphere of a literary surgeon. If I had been brought into direct personal contact with him, I should have expected to be laid wide open by one stroke of his incisive intellect. James would probably have been just as capable of doing so but he seemed to be far too kind-hearted to do it without the most liberal use of anaesthetics. It was perhaps a slight foreign accent that made me think Santana a bit affected. I should have feared to approach him, just as I should have appeared to approach Professor Charles Eliot Norton,

for fear of shocking his aesthetic feelings. Only James carried about him the atmosphere of "a classless society," but I never had any intimate dealings with him.

William Morris Davis and Nathaniel Southgate Shaler I would link up as in some measure parallel to Palmer and James, Davis being the surgeon and Shaler the human being. Nevertheless, I derived enormous advantage from Davis. He was rigidly scientific and taught one to weigh the last grain of evidence before announcing that a fact was indeed a fact. On one occasion, I thought I would interest him by telling of an old sea beach I had discovered near the Kennebec River. "Not a sea beach," he said, "not a sea beach. It may be a sea beach, but that is to be demonstrated." Such a reply was a bit shooting but good for early training in scientific method and good also for the soul. However, I never had a feeling of affection for him as I did for Shaler. To be sure I was a member of a class which the latter declared had treated him with the most contumely he had every experienced, but in his advanced course I learned more of the Doctrine of Evolution than from the volumes I subsequently read. He was a pronounced Lamarckian and enjoyed nothing so much as to point to certain organs or certain characteristics of an animal and say, "that would be useless until fully developed, and how could it have come into existence through the selection of slight variations?" Or, he would say, "That is a very, beautiful feature, but it happens to be hidden entirely out of sight, and how could it have played any part in sexual selection?" On one occasion I asked him "What do you mean by accidental variations?" "By an accidental variation," he said, "we connote—and denote—our ignorance. We do not know what causes them." And as he said so he smiled all over. While Lamarckianism is, I believe, out of fashion in biological circles, but the points Shaler raised still fit into the selectionist pattern.

Prof. Mark's lectures in Zoology demanded an attention conducive of headaches since he rarely repeated himself, unless one bought a

set of notes, and I seemed to be one of the few who didn't. It was in Zoology 2, I think, that my afterward famous classmate Walter Cannon acted as a laboratory assistant. One course in Zoology recommended by Prof. Putnam I fell down on. Many in my family are susceptible to the sight of blood and even cannot bear to hear stories of accidents. In spite of that I valiantly dissected everything presented to us in the laboratory up to and including a dogfish, but when we began Zoology 3 and were presented with a cat to pull apart, I nearly passed out and dropped the course. That ended my career in the Zoological department except for a half course in Osteology which I took as follows. The course was then under an old gentleman named Slade whose health was fragile. His instruction, which had once been somewhat formal, was now reduced, or it was reduced in any case at least, I being incidentally his only student, to perusing a textbook on osteology by a scientist named Flower, and going over trays containing the bones of various animals. This examination took place in Dr. Slade's office in the Agassiz Museum. Midway of the year, however, Dr. Slade fell sick and was confined to his home. When he left, he made arrangements with the janitor to let me into his office and continue osteologizing all by myself. For a few days that worked very well, but presently, while I was busy with my (or rather the) bones, a sharp-featured gentleman let himself through the office, eyed me suspiciously, and then passed on without saying anything. However, the next time I repaired to the Yorick chamber, I found the door locked. I hunted up the janitor, but he said that a day or two before Agassiz (Alexander of course) had come through and found someone whom he thought had no business to be there. Hence the lockout, the "someone" being of course me. That was the first time. Many years afterward I was suspected by the good people of South Carolina of being a German spy, and on another occasion in Texas I was taken for a man advertised in the local paper who had fled from his family. All of which goes to support Mr. J. N. B. Hewitt's

aphorism regarding "anthropologists and other suspicious characters." The dangerous nature of my profession seems, indeed, to have been of long-standing, since I am told that gendarmes were present at all the early meetings of the Anthropological Society of Paris.

I presume the incident above narrated shows the difference between Alexander and his father, but I entertained no ill-will towards the former who was presumably acting in what he conceived to be the best interests of his Museum, though I am somewhat puzzled to know just what damage my examination of bones was supposed to produce. The rest of my course of study in osteology consisted in studying the skeletons in the exhibition halls, and perusing Flower. All of this yielded me a C which was, indeed, all that could reasonably be granted though I was hardly to blame for the chaotic nature of my introduction to the study of bones. Poor Dr. Slade died, I think, the following year.

This C [grade] was one of four others. It was the best I could get in English C though I might put up a claim that the gentleman who rated me was over influenced by the title of the course. I got a B, in fact, in English B, but here the hypothesis seems to break down for I did not get A in English A.

The Cs which I particularly regretted were two in Spanish 1 and Spanish 2 which were in charge of a very precise and aesthetically sensitive professor named Nash. Professor Nash (I would not dare call him Prof. even now) was very particular regarding the exact wording of one's translation. I have seen him begin to wriggle in agony when some student not similarly endowed emitted a rendering which, although it might convey the sense of the original, was in English of questionable validity from the point of view of construction. Although I sometimes produced this reaction in our instructor, I did not commit the sin of translating *malos fisicos*, "bad physics" like one of the sports. That I was not a complete dunce in my knowledge of Spanish was proven when Nash was retired and his courses given

to an Assistant Professor whose name, Marsh, rhymed with that of his predecessor, but whose method of rating students was preferable. From him I got an A- during the first half year, but for reasons in no way connected with the instruction decided to drop it then. Perhaps I delude myself, but I have always believed that the two Cs I got in Spanish were mainly responsible for knocking me out of a Phi Beta Kappa. As it was I graduated magna cum laude.

Nothing more is to be said regarding the general courses I took preparatory to plunging into anthropology except that I believe Prof. Putnam made a serious blunder in advising me to drop German. At that time he was mainly interested in archeology, and in that of the New World, and since Germany played no part in settling it and had never any American colonies, the professor thought I should confine my linguistic studies to the nations that had played a part there. Hence the advice to keep on only with French and Spanish. But as my interest expanded into ethnology, I found myself cut off from a great deal of valuable literature and always regretted it though I had had two years of German in the high school and entered Harvard on Advanced German. The rest of my courses I feel were of utility both on the ground of general culture and in forming a background to my special work. I would, however, except Fine Arts I which, while interesting and important to men who intended to devote their future career to fine arts in general, was of very little service in my case.

By the time I reached my senior year The Archaeological and Ethnological department had descended out of graduate work and offered Anthropology L, a general introductory course on the subject. I believe I took this course the second year it was offered though it may have been the first. It was in immediate charge of George A. Dorsey² who had assisted Prof. Putnam in preparing the anthropological exhibit for the World's Columbian Fair at Chicago in 1893. He received, I think, the first doctorate in Anthropology given by Harvard. Owens, who conducted the first Peabody Museum expedition to Copan,

Honduras, might have been the first, and he was spoken highly of by all who knew him, but the poor fellow died of a fever in Honduras, and his work was carried on by George Byron Gordon³ who had been in charge of the engineering end of the problem. During my two years, 1896–1898, which I spent at Harvard as a graduate student, Gordon devoted all his time to the copying of materials collected on the Copan expedition for future publication and he did so I believe until he was called to be Director of the University Museum in Philadelphia, his qualifications for that job having been confined apparently to the above work. As I recall he drifted into the Peabody Museum late, and worked entirely apart from everyone else.

In my senior year, if I remember rightly, I took the new general course in anthropology under Dr. Dorsey. Shortly after the completion of this course, Dr. Dorsey became Director of the Anthropological Department of the Field Museum of Natural History, then known as the Field-Columbian Museum, and I think he did no more teaching. Indeed, his strength did not lie in that direction. His nature was too restless and he was at his best when on the move as a collector and promoter. His lectures were little more than a replica of the text book which he recommended to us, "*Precis d'Anthropologie generale*," by Hovelacque and Herve. Although the course was supposed to be conducted by Professor Putnam as well as by Dr. Dorsey, Putnam's work was confined to two weeks devoted mainly to the story of how he found a fiber moccasin in the Great Cave of Kentucky and one or two other similar enterprizes. He was, it must be confessed, no more a natural teacher than was Dorsey, and his field work was limited in character, his principal contribution to American archeology being in advertising the subject and building up the Peabody Museum. Dr. Frank Russell⁴ who succeeded Dorsey in the conduct of Anthropology 1, was the first real teacher of the subject at Harvard. He came from the University of Iowa, had traveled to the Arctic Ocean down Mackenzie River, was

western in mentality, as forthright as his name, thorough in his work, and honest to the core. Unfortunately, his work at Harvard was cut off by the incidence of tuberculosis, a result I imagine from the hardships of his Arctic expedition. In hopes that this disease might be stayed, an assignment to visit the Pima [*Tohono O'otam*] Indians of Arizona under the Bureau of American Ethnology⁵ was secured for him. Unfortunately for him, he carried this on with his usual industry exposed much of the time to the inhalation of alkali dust, and although he returned to the east and prepared his report on the Indians, he passed away shortly afterwards.

In organizing the collections of the Peabody Museum Prof. Putnam had another outstanding assistant in Charles C. Willoughby,⁶ a man without college training but with natural artistic ability and an enthusiasm for order very much needed in the Museum at that time. Under him the labeling of the collections probably became better than in any other similar museum in the country.

It so happened that my first contact with real anthropological work, in this case archaeological, was under Willoughby during the summer of 1894 when I accompanied him to Maine whether he went in continuance of his work on the mysterious Red Paint⁷ people whose remains he had located at Bucksport and Orland the year before. Another site was located on this trip on the banks of a tidal river south of Ellsworth. We roomed in an Ellsworth hotel and drove down to the site every day. One day, as I was sitting on the hotel porch, a young man drove past at high speed in a light rig, whereupon a gentleman on the porch near me said, "That is Senator Hale's son. He kills horses." This "son" was a member of my class at Harvard and afterwards became in succession representative from the First District of Maine and U.S. senator. He was one of those who, in political ideology, "advance rapidly toward the rear."

Our Red Paint site showed on the surface as three depressions, and on digging into these we found a layer of red paint at the bottom of

each overlain with beds of ashes which showed that great fires had been built there. This was apparently to consume, or at least pulverize, bodies of the dead, traces of which we found. The only actual bone, however, was the fragment of a skull on the edge of one of the pits, near the surface, and associated with some copper beads. This Mr. Willoughby considered an intrusive burial by more modern Indians. The important finds were in the layers of red ochre and consisted of stone plummets, beautiful long spears of slate, and other stone objects. But the find Willoughby prized most highly was what seemed to be the handle of a stone knife at the bottom of the largest and deepest pit. On this were rude scratchings which Mr. Willoughby believed intended to represent a settlement of very obtusely roofed houses located at the falls of a stream.

These notes are drawn merely from my memory and may not altogether agree with his published account. Later we went to Damariscotta and visited what Mr. Willoughby believed had been another site of the same people, but he concluded that all that was worth finding had been dug out by collectors. Here Gordon joined us and he and I waited at the hotel while Willoughby went scouting. Later we all went to Augusta, the State Capital, and, while Mr. Willoughby proceeded on to Riverside where a possible site had been reported, Gordon and I put up at the Coney House. For one night only, as it proved, for we then discovered our beds were populated and we forthwith shifted to the Augusta House near the capitol building. The wilds of Honduras apparently had not hardened Gordon against discomforts, but he conceded that the Augusta House was "passable." He arrived in Damariscotta with some Honduras cigars but they gave out early and he afterwards found it difficult to get anything decent to smoke, except the highest grade of fifteen-cent-ers in Augusta, presumably those used by the state solons.

A few days later Mr. Willoughby returned and took us to Riverside from which little village we were driven several miles into the country

and lodged in a farm house. Here we were royally entertained although the only item of fare that has impressed itself upon my memory was raised biscuit soaked in rich, fresh cream. One day I remember that Gordon fell to descanting upon the difficulties he had experienced in finding anything decent to eat upon this trip, whereupon our hostess said, "If what we have doesn't suit you, we shall have to ask you to leave." This solar pluxus [plexus] brought forth an instant's avowal from Gordon that everything there was very nice indeed which was the highest compliment he paid to anything during the trips though with a little of the air of approval of one accustomed to dine with royalty. Poor Gordon, I fear his fondness for the trencher was largely responsible for the sudden attack of apoplexy which finally, carried him off. Willoughby and Gordon were about as ill-assorted a couple as could have been found, but they maintained what is called "a correct attitude" toward each other during the expedition. Our new site was on a long knoll where there had seemingly been a number of pit burials, but these had been lined with bark instead of ochre and Willoughby thought they belonged to another people. Although the pits were thought to represent so many burials, we failed to locate anything but the bark, soon gave up work, and all returned to Cambridge.

During my next summer vacation, that of 1895, Prof. Putnam sent me to join Mr. Volk at Trenton, NJ.⁸ Mr. Volk was an old German from the Black Forest region who, however, had been in America a long time. Every morning he would set out with a good sized basket under his arm in which were brought home the findings of the day. He had only one or two men under him, and I used the pick and shovel myself to the extent I was able, long enough to enjoy the sound of noon whistles and appreciate the taste of cold spring water. Volk's scene of action was along the edge of the bluff overlooking the bottom lands of the Delaware River on a property owned by two old ladies named Laylor. The estate was an old one and there were said to be in possession of this family letters by some of the great men

of Revolutionary times, but I saw none of these. Our interests lay farther back still. The populated part of Trenton was growing in this direction, and a less desirable part indeed occupied by Slavic immigrants of miscellaneous types who were wont to appropriate outlying parts of the Laylor crops and other possessions. The finds here were of two kinds. In the black soil at the top were flint articles in whole or in part and pot sherds. In red or yellowish soil beneath we came upon a sparing number of pieces of argillite some of which seemed to have been worked—at least Mr. Volk thought they had—while others seemed to have been mere products of nature. We used the usual Putnam method of procedure, laying out the ground in squares, establishing a vertical front and, gradually undermining it, generally with the trowel, in order not to injure possible specimens. But after one had proceeded in this manner for some time without results, it was a temptation, and indeed allowable, to bring down a large amount of material by one or two blows with the pickax. On one such attempt, however, the virtue of the trowel method came out clearly, I used my pick twice on this occasion, the first time with the small end. At the second blow the earth came down as desired, but on going over it with the trowel I discovered an earthenware pipe which had been broken into three pieces by my first attempt with the pick. To hit such a small object as this with a so much smaller point by intention would have demanded extreme skill, yet the miracle, and a very much undesired one, had taken place by accident. This is an episode of the expedition that I presume Volk left unpublished but there was little germane to science that escaped him. Indeed, I rather suspect that he overdid his record after the German type of error. Two years later, when I was working in Ohio, we had an old German working for us who was sometimes left to make records of his findings, and it was said of him that he recorded the width of opening of the mouths of skeletons although the lower jaw was disarticulated and half a foot away. However, this is a kind of error of which field

investigators have less to complain ordinarily than of another type.

I have to confess that the scientific finds which turned up in our excavations do not stand out more clearly in my mind than the thought of a luscious watermelon which a neighboring farmer brought down one morning and placed in a neighboring spring so that it would be cold by lunch time. Lunches then had an aura about them which they have since lost. I also remember with unarchaeological, but possibly highly anthropological, intensity, my visits to the home of a college friend at Penn Valley a few miles to the westward. This was a man named Buckman who used to describe himself as substitute pitcher on the worst baseball nine Harvard ever had. The record in that direction may have been broken since. He was a splendid fellow belonging to an old Quaker family and was not the only child belonging to it. Along with J. C. L. Clark '97, and David Gibbs '98 we had a little literary circle which met once a week, one contributing a short story, another part of a continued story, a third play perhaps, and myself a poem. No, indeed, none of these were original. Later Buckman became a highly successful business man, I have been told and opened an office in New York, but suffered deterioration in his fortunes, perhaps at the time of time Great Depression, and the last heard of him he had gone to Alaska.

But to return to the subject, one week, I think it was when our expedition was near an end, Buckman and I paid a visit to Washington, and that was my first view of the nation's capital. As I remember, we went down the Delaware to Philadelphia by steamer and the rest of the way by rail, but I may have confused the first part with an earlier venture of mine to Philadelphia alone. In Washington we put up at the old Willard Hotel before it was rebuilt. The front of this building then set back farther from the street line of Pennsylvania Ave, and had four huge round columns. We took most of our meals at a little lunch room opposite the Treasury

building. Our week included Labor Day of '95, and I remember watching the procession from our hotel. The great avenue itself reminded one still of the main street of a city of the third class, and, as I remember, the Willard was not the only building which did not extend to the sidewalk. On the train back we bought fried oysters put up in little card board boxes.

I received my A.M. in June of '97, and that summer was sent along with Rowland Dixon '97 {Roland Burrage Dixon November 6, 1875—December 19, 1934}^b and Ingersoll Bowditch by means of a fund contributed by Bowditch's father, to assist Dr. Metz in the exploration of a village site at Madisonville, Ohio.⁹ It was here that the meticulous German was employed, and the man in charge of the whole operation, in theory, that is, Dr. Metz, was also a German. He was a local physician and of excellent ability, one who had previously assisted Prof. Putnam, but unfortunately addicted to the liquid that, like woman, "seduces all mankind," and the result was that he was seldom in condition to supervise adequately the work of three green undergraduates. His supervision consisted in little more than an evening visit to see what we had found during the day. He would stand on the pile of earth thrown from a trench in a very warbly manner and gaze abstractedly at anything to which we called his attention.

Dixon had preceded me by about a week, and when he left turned over to me the camera he had used, one of his own, with careful directions as to its use. I feel that a great part of the worthwhile results of this expedition were due to him. This was the richest archaeological site I had worked in up to that time, and it is unfortunate that a first-class expert was not in immediate charge. However, the excavated area was carefully laid out into sections, the stakes properly set, and we took pictures of practically every skeleton after it had been exposed and cleaned by means of trowel and brush. In only one case did I slip up I believe, when we cleared a skeleton late one afternoon

^b All material in {brackets} has been added to the original text by Jay Miller.

intending to photograph it next morning, but removed it then under the impression we had done so. The great damage done here lay in the fact that we were exploring in very hard soil and skeletons were broken-up in bringing them out through our crude methods which modern techniques would undoubtedly have saved. Besides the skeletons there was another type of find, "ash pits," deep circular holes which usually contained a miscellaneous assortment of bones among which those of deer predominated. One or two were filled with carbonized corn, and it is provident that some of them were originally storage pits, but almost all had ultimately been used for the disposition of garbage. The Madisonville site has been reported upon and I need not detail our findings. We made some discoveries, however, innocently enough, which have deeply annoyed later archaeologists. These were evidences of contact with Europeans in the shape of a fragment or two of iron and a number of blue glass beads. Most of the latter were in the neighborhood of a child's wrists and had seemingly been bracelets, but they were also found in three or four ash pits.

After my return to Cambridge I was set at the job of working up the results of our exploration for future publication and that consisted largely in washing bones, a technique of which Prof. Putnam was very fond but which destroyed the interest of some of his students. It was not really attractive to me but I worked manfully at it all winter, and was still doing so in April or early May when Prof. Putnam descended to the vault where I was engaged and asked me if I would like to go to the Pueblo country with Mr. Pepper. To a young man with my limited travel experience this was like striking the pot of gold at the rainbow's end and it did not take me long to accept the proposition, leaving the results of the Madisonville expedition to gather dust until a very much later period. In due course I joined Pepper in New York and traveled with him to the Chaco Canyon in New Mexico where the American Museum of Natural History was exploring the great ruins of Pueblo Bonito.¹⁰ We

had passes signed by George Gould over the Wabash and Missouri Pacific Railroads, changed at Pueblo to the Denver and Rio Grande, and went south from Mancos on horseback. The term "on horseback" applies to myself, however, only in part. By the end of the second day I had slid forward and back on the saddle of my pacer so many times that I was about worn to the bone. Very fortunately for me we there caught up with the wagon that was bringing along our equipment and the goods for the store we were to open, so I shifted to a seat beside the driver and continued for the rest of the way in a less heroic but more comfortable fashion. It is a strange fact that, although I was on horseback only once between this time and my departure about six weeks later, when I returned I rode all the way without the least inconvenience, and on one day we covered fifty miles.

Mr. Pepper was an agreeable young gentleman to work with. He had come to the attention of Prof. Putnam through the discovery of an Indian site on Staten Island and without a college education. He was short and dark and through and through a product of Manhattan, the dialect of which was as natural to him as cockney to a Londonese. When he first went west his ideas of western life were of the Kit Carson pattern as understood by writers of the Harry Castleman school, and he accoutered himself accordingly, in particular allowing his raven locks to grow down over the back of his neck much to the admiration I gathered of a circle of admiring damsels to whom he recounted his adventures in the "wild and wooly west" after his return. When I went west with him, however, much of this naivety had worn away and his locks had been sheared. I cannot say that he was devoted to the monotonous work of sitting upon the edges of Pueblo rooms in the alkali dust to see what turned up and prevent the wily Navaho working man from concealing turquoise beads in his headband. Some of that work fell to me but I was run down nervously at that time and quite unable to keep from falling asleep at intervals so that the Navaho nation was

no doubt rich in turquoise in consequence. The tall beau and comedian of the Navaho, *Tomasito* (pronounced *Tomasitah*) aroused considerable mirth by imitating my intermittent noddings and sudden recovery. They called me by several names of which the only one I remember was *Hastin Hazho*, *Hastin* being some sort of equivalent for our Mr. and *Hazho* signifying something like "Look out!" because I was always using it when I thought an excavator was careless. The Indians were summoned to work morning and noon by a yell at which Mr. Pepper was very good, but my voice was apt to crack in the middle. Therefore, when it was possible, I shoved that duty off on Pepper or Richard Wetherell who was in charge of the outfitting and with his wife attended to the store. At noon I was so tired that I took a sheepskin up to a crack in the overhanging cliff and lay down there until work recommenced. The Indians timed themselves by the sun, and I was put to shame on one occasion when I attempted to work them overtime because of the failure of my Waterbury {watch}. Work was terminated by a general strike. We were in some danger from the collapse of walls while we were clearing a room, and if I were disposed to exit the gallery, I might enlarge upon the occasion when a wall fell in and half refilled a narrow room that an old Navaho and I had been at work upon. Had we been at work, we might have been incommoded by a half ton of rock but the catastrophe occurred during the lunch hour and a shovel or two were the only casualties. Our store was housed in a oblong building made of stone set in adobe. It had a flat roof with a stone parapet all the way round and I slept on top under Navaho blankets along with a cousin of Mr. Hyde, sponsor of the expedition, a young fellow with whom I associate the name Lawrence though I won't answer for its accuracy. Those were really grand nights under the gorgeous starry skies, made doubly so by the clear dry air of New Mexico. It was equally grand when the moon was near the full. When we went to bed it was so warm that we hardly needed covers at all, but presently the regular down-canyon wind set in and before

morning we were glad to pull all the blankets back. "Lawrence" was a splendid companion and I wish that my acquaintance with him might have been prolonged. Mr. Pepper had a tent by himself in which some of the better finds were preserved. As I have said, he was not fond of routine, but he would work indefatigably on any sticking job that took his fancy.

The year before he had nearly made himself sick digging out an underground room in the alkali dust. He began the year on which I accompanied him by polishing up his turquoise rings, of Navaho make. Later, however, we came upon an apartment in which the women of the Pueblo had evidently been grinding corn while they exchanged gossip, for there was a row of metates down the center. Pepper took a fancy to prepare for an accurate reproduction of this room, using a rough-and-ready type of papier-mâché of his own devising with which to model the metates. What ultimately became of this I do not know. After his return to New York he devoted himself to the preparation of a case containing choice specimens of turquoise. I do not wish to seem too critical of Mr. Pepper. We all have our separate abilities and his was an interest in special exhibits involving an aesthetic sense far above mine, while I am rather a plugger who attends to the run-of-mine material. Mr. Pepper was easy to work with, perhaps too easy. I returned to Boston with "Lawrence" and the journey was pleasant.

Shortly after I reported to Prof. Putnam, he said to me that he gathered from what I told him that I did not care so much for archeology. "I guess you always did hate a bone," he said, although I do not think I had expressed a very strong antipathy. Anyhow, he concluded that I had better go to New York and study under Dr. Boas who then had a position under him at the American Museum of Natural History. In the fall of 1898, therefore, I ended my eight years of life in and around Boston including six at Harvard and went on to New York where I engaged a hall bedroom on West 85th St., within easy walking distance of the Museum and only two or three

streets up town from the one on which Dr. Boas and his family were then living.

Shortly before Dr. Boas had returned from a visit to the Columbia River region and had obtained there a quantity of texts and supplementary material in the nearly extinct language of the Lower Chinook Indians. The work on which he set me was the extraction of material from his notebook in order to determine the grammar of the language. This proved to be as interesting as, and rather more important than, a crossword puzzle, and after I grasped the proposition I got along very well, so that the results of my work were accepted as a thesis for the degree of Ph.D. at Harvard which was entitled "The Morphology of the Chinook Verb."

The circumstances connected with my first linguistic discovery, or rather supposed discovery, are important on account of the light they throw on the character of my teacher and the strong hold he had on the affections of his pupils. He had noticed that certain sounds in Chinook tended to change in consonance with changes in the sounds preceding, and he suggested that I make lists of the words in which such changes were exhibited. One morning, after I had been engaged in this work for a while, Dr. Boas looked over my material and told me that it indicated a certain law to exist. At that moment his secretary happened to come in and Dr. Boas said to her, "Miss Andrews, Mr. Swanton has just discovered an interesting law." The discovery was his but the credit was given to me. Dr. Boas did not even say "we have discovered?" He was too much interested in the discovery to care who made it. Later I made sufficient finds in my own right I am sure, but this one particularly impressed itself upon me and stamped upon my mind an assurance of the high-mindedness and disinterested devotion to truth of the man under whom I was working. Later I could recognize that he had his weaknesses like the rest of us, but the incident above related has had an enduring effect upon me. At that time I was in a highly nervous condition so that isolation in a hall bedroom which is no doubt a horror to

many new arrivals in the metropolis was just what I needed and my health improved.

In the summer of 1899 I was sent to South Dakota with some of the texts which George Bushotter had written out for J. O. Dorsey some years before. These I was to go over with Dakota informants to see what I could make out of them and correct the phonetics if such corrections were found necessary. My first objective was the agency of the Rosebud Dakota Reserve, but, not knowing how to get there, I arrived at Pierre, S.D., and found that I was on the wrong side of the reservation. A few days later, however, I learned that the postmaster of a place on the northern edge of the reservation was about to return and would take me with him. This was by horse and buggy, of course, or rather horses and buggy for spans were the rule throughout the western country. On the far side of the Missouri we seemed to pass into the kingdom of Beelzebub, since his name signifies, I believe, "the god of flies." These were the common type which do not sting, but their persistence when we stopped for a noon siesta drove one nearly wild. The first night we camped on the open prairie but my sleep was abbreviated by a thunder storm on the wake of which mosquitoes presented themselves out of nowhere. So I climbed up on the buggy wet and cold and fought mosquitoes until morning. The postmaster was a Jew who refused to take anything, kept me over night, and next morning found an Indian bound for the agency who carried me there in safety.

During this trip and subsequent residence on the reservation I saw more of rattlesnakes than anywhere before or since except in the snake pit at Crystal Lake, Florida. The first rattler I ever saw was a baby specimen that we nearly ran over during my wagon trip into Chaco Canyon in 1898. There were plenty of them not far from our camp but I did not happen to "meet up with" any others until I went to South Dakota. During our first day out from Pierre the smoothness of our trip across the prairie was suddenly broken by the horses who stood suddenly up on their hind legs, jumped to one side of the road and

then lit out across country. My companion said that this was due to a snake lying coiled on the side of the road but I didn't happen to catch sight of him. My second experience on this trip was in the company of the Indian just mentioned. This time the man caught sight of the reptile first, said "snake," and pulled his horses to one side of the road, and took the really profound risk of entrusting me with the reins while he got out and broke the back of his snakeship with one or two well directed blows of his buggy whip. The reptile was a big fellow just crossing the road in front of us and it was as well that the horses did not catch sight of him. The episode also shows that my Indian was not an old timer for in that case he would as soon thought of demolishing his grandmother.

At the Rosebud Agency where I was put up in the home of the Agent himself and very kindly treated, my stay was enjoyable but unproductive until I learned of a Yankton Dakota Indian named Joseph who conducted a school in the eastern part of the reservation and would put me up and help me with the language, his school not then being in session. During the few weeks I spent with him, I covered considerable ground and was able to introduce some necessary changes into the first hundred of the Bushotter texts though it was by no means a complete job. The winter of 1899-1900 I continued work under Dr. Boas, this time in connection with the Bushotter texts. As in the year preceding I was also entered at Columbia University where I took linguistic courses under Dr. Boas, and in 1898-1899 had a general course in anthropology under Dr. Livingston Farrand later head of the Red Cross and still later President of Cornell.

In June of 1900 I went to Cambridge to take my final examination for the doctorate in anthropology. My examiners consisted of the Prof. Putnam, Dr. Boas, Dr. Russell, and Mr. Bowditch, and the examination was a curious affair but as good as could be offered in anthropology probably in 1900. Putnam began by asking a few questions in general anthropology but more particularly in archeology which I was able to

answer, in part no doubt because I happened to know his particular prejudices. Dr. Boas, the only one present who knew anything about the special subject which I had worked up for my thesis and understood my qualifications already, asked a few things about the Chinook verb. Mr. Bowditch, as I remember, rather asked advice of me than made an examination, not I am sure because he expected to learn any more than he knew already but because he was feeling his way toward further work in the Mayan field which he had already promoted. When the examination was turned over to Russell something unique happened. He spoke something like this, "It is foolish to hold an examination of this character and we shall not give anthropology a respectable status until we specify a major and minors as in other departments." I may add that he was kind enough to say that he did not intend this as an adverse criticism of the candidate, because, he said, "I admire the man" (though I confess I am not sure that he knew me well enough to admire me which was perhaps why he could say it), finally, he said, "No, I have no questions to ask."

Rowland Dixon got his degree the same year although, having taken his A.B. one year later than myself, he had gained a year on me. During our undergraduate course we were members of a small folklore society which he organized and of which he was the moving spirit. He developed into an outstanding anthropologist and was always a good friend to me. His comparatively early death was a great loss to the science, and to all who knew him.

My Dakota work had behind it the purpose of fitting me to take up the work and editing the manuscripts of James Owen Dorsey {October 31, 1848-February 4, 1895} who had collected a vast amount of material from the Siouan group of languages. Dr. Dorsey and Dr. A. S. Gatschet shared between them the greater part of the linguistic work in the early days of the Bureau. Except when forced to undertake an expedition to Oregon—to his utter disgust as H. W. Henshaw told me, Henshaw being the man to do the ordering—Dorsey confined his

work in the manner just indicated. His records were made with meticulous care and have been highly commended by all later students who have handled his material. He began work with the Omaha among whom he had been a missionary and extended it to Osage, Iowa, Oto, Winnebago, Quapaw, Kansa, and finally to the Biloxi after Gatschet discovered that language. For Dakota, however, he depended on the records of the missionary Riggs, and for Hidatsa on those of Matthews. His principal omissions were Crow and Catawba, and the last-mentioned occupies such a peculiar position that it is unfortunate he did not take it up when more speakers of that language were available. The records of Dakota and Hidatsa would also have been more satisfactory had he done the work upon them himself.

Dr. Gatschet was a Swiss and the only university man in the original Bureau group. Like Dorsey he spread his attention over a number of very distinct languages. In spite of his educational advantage, he was very far below Dorsey in his recordings and his ear was by no means as keen. But what was lost in one way was compensated in another since through his willingness to spread his activities we are indebted to the rescue of material from some languages now extinct. In spite of the time he spent among Indians, Gatschet never really understood the mentality of his subjects. He was no ethnologist, but a linguist pure and simple, and singularly lacking in logical faculty when confronted with the problem of interpreting place or personal names. Thus he derived Tulane from the Choctaw word for a certain bird, and that of the Biloxi Indians from a creek in Oklahoma with which they could not have been acquainted for a hundred and fifty years after their name first appears in history. Gatschet's head was of a peculiar shape and to this we may charitably attribute some of his eccentricities, especially a salacious slant of his mind. Two men could hardly have been more unlike than the two linguists of the early Bureau.

In furtherance of the plan to have me take up Dorsey's work, Dorsey having passed away

in his prime in 1896, I was given a civil service examination in the summer of 1900. I spent several weeks in Washington at this time and was introduced to the extent to which Washington climate can go in making one uncomfortable in summer. My previous experience had been at a later time in the year and under more favorable climatic conditions, I had a small second-floor hall bedroom in a block of stone houses which still exists I believe, on the south side of L St. between 15th and 14th. It was a front room but that made conditions all the worse, because almost every afternoon there was a thunder shower so close to the termination of office hours that it was called "the clerks' storm" I believe. Anyhow, instead of cooling the air, these storms would drop just enough rain to turn to steam which rose into my windows, or rather window, and changed the baking process we had suffered all day into a steaming one. I came to know two other young fellows rooming in the same house, and almost every evening the three of us would walk round to a drug store on the corner of 14th and K Sts, to imbibe cool drinks or ice cream. Sometimes we would go up 14th to a little out-of-doors ice cream parlor—now long ago abandoned and the property probably built over. On one occasion a larger crowd, including the three of us, went to a beer garden on the other side of 14th street, east that is, somewhat higher up. Here I was introduced to the vicious treating system then in vogue, since each of the six or seven people felt it incumbent upon him to supply drinks all around. To be sure soft drinks were served as well as beer but no one, not even in a Washington midsummer, needs six bottles of cold liquid. However, some went the limit, and very probably out of courtesy to each other, and when the six were not soft drinks the effect was likely to be still less salutary. In particular there was one alcoholic in the party who did not require any extra stimulation to his besetting sin. Even soft drinks are not good for my own digestion, and the only beer I ever took was on a doctor's prescription with the idea of building up my waist girth. But my waist refused to respond and I was changed over to prunes. Of

my two summer companions, one dropped out of my life completely, a tall and rather elegant youth. After I returned to Washington to live permanently, the other, a short and dark man from up New York State named Seaman was still there and I kept in touch with him for a few years, until a tragic event befell him. One day, in trying to board a street car, he was thrown back and fell on his head on the pavement. He recovered from the accident after a time and went back to the boarding house where I had met him, but presently he began showing signs of a mania of persecution, believed that a young lady in the same house intended to kill him, presently talked of arming himself, and was sent to St. Elizabeth's for examination, he had hoped that he would get over this delusion but it stuck to him and he remained there till his death.

I do not think that I shone particularly in my examination and I am under the impression that, if my rendering of the Ten Commandments into Sioux had been followed, that tribe would have become extinct. However, I gave an account of myself sufficiently good to be passed and given an appointment to the Bureau taking effect September 1, 1900 upon which I got out of Washington and returned to my mother's home in Boston with the utmost speed.

Shortly before this, Morris K. Jesup, Director of the American Museum of Natural History had made a very large grant of money for anthropological work on and near the North Pacific coast and Dr. Boas had been placed in charge of this. Anthropologists being scarce in that epoch, Dr. Boas made an arrangement with the Bureau of American Ethnology by which I was to conduct an investigation among the Haida Indians during the winter of 1900–1901, the American Museum paying my field expenses and the Bureau my salary. My salary being fifty dollars a month, that made no great drain on the latter. In September 1900, I reached Skidegate via an old side-wheel steamer named, if I recollect correctly, The Princess Louise. Actually she should not have been allowed out of sight of land, but the sea happened to be smooth. At that time there were but two permanent settlements on

the Queen Charlotte Islands, Skidegate which was connected with the outside world by one steamer a month out of Victoria, and Masset on the north coast of Graham Island, which enjoyed one steamer annually bringing supplies for the Hudson Bay Post. There were but two white families at Skidegate at the time of my visit, that of the Methodist missionary in the Indian village, and that of Mr. Robert Tennant a mile higher up the inlet. Mr. Tennant was an old miner formerly in the Cassiar country on the mainland, and his wife an Indian from the Fraser River region. The latter had been brought up by an English woman and was white in everything but birth and color. She was a meticulous housekeeper and I was very comfortable there during my stay.

To one used to the Atlantic seaboard, the North Pacific coast surprises one by being so much warmer in winter and so much colder in summer than he expected. This climatic difference is, however, confined to a comparatively narrow strip of land between the coast and the mountains. The clouds lie low, and in the fall, winter, and spring there is a rainfall about every day. One takes one's slicker and hip boots—if one is to travel inland that is—as a matter of course, wherever one goes. Hip boots are needed, because just back from the high tide mark there begins a jungle of bushes and behind it a dank and lofty forest and underneath the latter a mattress often of fallen monarchs of the forest, so that one may sometimes climb many feet off of the ground in trying to cross such an obstruction.

During spare moments, when I was not working with informants [speakers], I used to climb to a high rock not far from the house, sit down there, and watch the changes of weather on the ocean and the movements of bird life. Although it rains so much, it is usually in showers with intervals of sunshine between. One can see a storm some little distance at sea and watch the changes it undergoes. Flocks of sea fowl float about or take off in long lines, flap, flap, flap along the surface of the water and then gradually into the air like so many aeroplanes.

There was scarcely a sign of civilization anywhere. Mr. Tennant's home was on the site of an old story town, and my mind was full of the stories, so that I almost expected to see a killer whale come to shore and dissolve itself into a manlike supernatural being or be a witness to some similar prodigy. It was to me a weird and beautiful country and remains so in recollection.

In the spring I hired an Indian to take me to Masset in his canoe, but like most of the commercial minded Indians of this region, he allowed me to outfit with provisions for several days and set me ashore on the near side of Rose Spit from which I had to walk into Massett. I don't altogether wonder that he did not want to round that long and shallow spit in a heavy sea, and I have fully forgiven him for the deception, because I thoroughly enjoyed my walk. The north shore of this, Graham Island, is one long beach at low or half tide and not very difficult to negotiate except when the tide comes way in. That happened after I had walked some hours and drove me up into the bushes. At one point, in order to make a short cut to *Hi-ellen* (or rather, *Thlielung*) River, I turned inland and was forced to climb over a natural fortification made by huge trees fallen and lying in all directions. Every time I jumped from one trunk to another, the pack I carried on my back came thump against me and nearly knocked me from my perch. In due course, however, I reached the river, worked my way up it till I found a tree I could cross upon and a few minutes later found an Indian shack where I prepared to spend the night. A short time later two or three Indians presented themselves there and built a fire in the middle. I disposed myself for slumber as well as I could but it was a cold night, and the airs of heaven came in *ad libitum* through the door and the smoke-hole so that I got little sleep. In the morning, however, we got something to eat, and I started out with revived courage, I do not remember just when I reached Massett but it happened. There I found it necessary to choose between staying with the white man who had charge of the Hudson Bay Post or going to the home of an Indian, Henry Edenshaw. I made the mistake of choosing the

white man until Monday; it was Saturday when I arrived. This white man had roughed it on the coast for many years and gotten roughed in the process. The Massett Post did practically no business and this gentleman was left in charge of the property awaiting its final disposition. He treated my coming considerately but not cordially, and it soon appeared that his consideration for visitors meant just that. His cooking was one of the greatest hazards I had to face during my visit to the islands. Ultimately this gentleman was taken from the island insane and no wonder.

It was interesting to compare the Christian missionaries at Skidegate and Masset. The former was a Methodist whose other name was "Thorough." If his flock did not take to religion, he took them to it in the spirit of the church militant. Religion to him appeared to consist in the number of religious services that one attends whether voluntarily or by compulsion, and he used every means to herd the sinners and the saved into church. His one great regret was that he was not clothed with the civil as well as the religious authority, and bitterly resented the invasion of his bailiwick by the Salvation Army. He also maintained a feud with Mr. Tennant, and had set up a dogfish oil factory in competition with the latter and for the supposed benefit of the Indians. This would have in itself been an admirable institution if he had had the genius of Dr. Duncan of Metlakatla but that is doubtful. I was not on the islands long enough to know how the business turned out.

The missionary at Massett was of a different stripe altogether. His mission was supported by the Church of England. He was the son of a missionary among the Tsimshian Indians of Nass River where he had grown up, and he spoke the Tsimshian dialect fluently. He apparently maintained the rites of his church with due order, and forestalled competition from the Salvation Army by organizing a Church Army along similar lines. Whether from native disposition or early association with Indians, he was tolerant to the point of indifference and during the two months or more that I lived in Massett I saw him but twice in any of the Indian houses.

While Skidegate was seething with religion and scandal, Masset seethed not at all, although no doubt indulging in the gossip of an isolated and self-supporting community with few outside contacts. The principal outside contacts were in summer when both Skidegate and Masset were practically abandoned, the inhabitants of both settlements betaking themselves family by family to their Columbia River {Chinookan style} boats and crossing to the mainland to work in the canneries and incidentally exchange news with the mainland Indians. No compensation could induce any of them to remain at home, and, no wonder, considering the isolation of their lives during such a large part of the year.

During most of my stay at Massett I lived with Henry Edenshaw who was a splendid cook and kept a neat house. I had a large front room with a wooden bedstead with high head and footboards. One morning as I lay awake in this just beginning to open my eyes, the bedstead was shaken violently as if some giant had seized the headboard and pulled it back and forth. I realized almost instantly that it was an earthquake and bounced out of bed in hopes that I might escape through the window if the house came down about my ears. Needless to say, it did not.

Later in the spring, Henry took me across to the Haida (*Kaigani*) towns in Alaska, all of which except Kasaan I visited. Dixon Entrance is sometimes turbulent, but we made the passage successfully, and when nearing Howkan, the first objective, Henry shot a deer which was trying to swim across the channel. After our return to Masset, we voyaged back to Skidegate, and at this time the summer was so far advanced that it was light all night. While the sun passed below the horizon, its course was marked by a great yellow patch of light on the southern horizon. I returned to the east as I had come except that on recrossing the Rocky Mountains, I made the mistake of side-routing through the Kootenay country instead of remaining on the main line and enjoying again the gorgeous scenery of the Kicking Horse Pass. I also shifted off at Lake Superior and went down by steamer through that great sea of clear blue water into

Huron and to Port Huron. I reached Toronto at night, but at my hotel the accommodations had been so far absorbed through the attractions of the nearby Buffalo fair that I had to sleep on a billiard table. That was almost my sole contact with the game.

Before reporting at the Bureau I spent some time with my mother in Roxbury, Boston, and made my final transfer to Washington in September about the time when our country was shocked and saddened by the assassination of President McKinley.

My only contacts with members of the Bureau of Ethnology before I took my examinations were during some meetings of Section K of the A.A.A.S. In particular, I remember a meeting at Columbia attended by William H. Holmes and W J McGee {October 31, 1848–February 4, 1895, married to anthropologist Anita Newcomb McGee in 1888, three children, died of cancer}. At that time there was a somewhat furious feud between those Washington gentlemen on one side and Prof. Putnam, and Prof. Wright of Oberlin on the other. Putnam and Wright believed that the occupancy of America by human beings went back to a remote period, and accepted the antiquity of flint objects found at Trenton and the Calaveras skull, while McGee and Holmes were in pronounced opposition. As in the case of the famous six men of Hindustan and their elephant, each party proved to be “partially in the right and partly in the wrong” [JRS: correction needed]. Putnam and Wright as to Trenton and Holmes and McGee as to the Calaveras skull {human, found 25 February 1866 by miners 130 feet (40 m) deep, confirmed by Josiah Whitney, California State Geologist and Harvard Professor, later by Putnam.}. I remember that Hrdlička testified that a thigh bone found in the Trenton gravels was human, though I do not know whether he, at that time, sided with Putnam and Wright or not. At the Columbia meeting Dr. Wright showed a small stone object with the figure of a mastodon (or elephant) scratched upon it {Davenport stone tablets hoax}.

The demeanor of Holmes and McGee at these meetings was quite different. Holmes was, as usual, quiet and reserved, but McGee asserted his beliefs in the hammer and tongs fashion with which his earlier occupation as blacksmith had made him familiar. At an earlier period his hair is said to have been jet black but when I first met him it was threaded with patches of gray. However, he still shook his “mane” violently in argument, and made up in aggressiveness what Holmes lacked. The argument on the other side was maintained with no such vim.

Of course I saw little of the Bureau of Ethnology until I returned from the Queen Charlotte Islands in September of 1901. The Bureau was then housed in the Adams Building on the north side of F St, N.W., opposite the building in which the Ideological Survey was then located. The main offices were on the top floor and the Bureau library two flights below. Dr. Hodge had been the librarian but was at that time in the Smithsonian Building assisting Dr. Langley. The librarian at that time and for many years afterward was Miss Leary who was assisted by a girl, part Negro I think and part Indian, named Ella. For a time the library force was augmented by the youngest daughter of Dr. Cyrus Thomas, a splendid girl who lost her life shortly afterwards by drowning during a skating party.

Upstairs at the front was the office of Major Powell, and between it and the head of the stairs where was also the head of the elevator, was the office of Mr. McGee and the head clerk. The rear of this floor had originally been a single room but was cut up by temporary partitions, which did not reach the ceiling, into several “cubby-holes” where most of the scientific staff did their work. I believe that the illustrator had an office at one end but have almost forgotten. Between this “cubby-hole” section and Major Powell’s office there was a passageway which led through a small room where the members of the staff often took their lunches together. This was a very pleasant feature of Bureau life at that time.

Major Powell lived only a year after my return to Washington, and I had but one interview with him, all business being then conducted through Mr. McGee. Seeing that a new man had arrived, he asked me one day to come into his office. He began, as I remember, by saying that he wanted to tell me something of what they “were trying to do,” but his intention was perhaps rather to draw me out, although his increasing infirmity might explain why he appeared to wander from the subject. At any rate, he seemed, to me to be buried in thought most of the time, and I do not know but he might have forgotten my presence. A few months later he passed away.

McGee had been Powell’s right-hand man during the last years of his life and fully expected to succeed him, but in some way or other he had gotten into the bad graces of Dr. Langley, then Secretary ~~head~~ of the Smithsonian Institution, and Langley appointed Holmes instead. Holmes and McGee had before this time been on the most intimate terms, and there was some surprise that the former should accept the headship of the Bureau. Holmes, however, though an excellent archaeologist and a consummate artist, his specialty being water colors, was highly sensitive and very timid. A row of any kind would occasion in him a nervous upset, and those who knew this weakness could impose upon him very readily. It also happened that Holmes was the only man whom Langley could place in charge of the Bureau with any justification, and, although I am sure that the whole thing was distasteful to Holmes, he yielded to the compulsion. Indeed, it was reported that he and McGee went out to dinner together and after a discussion of the whole problem agreed that it was best for Holmes to take the position. While such a meeting may have adjusted matters as between McGee and Holmes, it did not alter the feelings of the former regarding the action taken. Along with several other members of the staff—all those then in Washington—I was present when Langley brought Holmes over and introduced him to us as our new chief. In the course of his remarks Langley spoke of his

high esteem and great love for Major Powell, and upon this McGee was moved to remark that he was glad to hear of Langley's appreciation of the Major "in view of the reproach which is now being cast upon his memory." I think that these were the exact words.

Then the position was offered to Holmes. Dr. Langley put it up to him as to what title he would choose and, not feeling himself endowed with Powell's executive gifts, he chose that of "Chief" instead of "Director." For some time longer McGee continued in his position as assistant chief but presently resigned under the following circumstances. I am under the impression that McGee talked of taking some legal steps to secure the position he coveted, but whether that was actually the case or whether Langley felt that he must holster up his own case, an investigation of the affairs of the Bureau was instituted. I feel quite sure that nothing like a misappropriation of funds, certainly not an intentional misappropriation, had taken place under McGee but he was careless in the conduct of his affairs and it was probably not difficult to point to irregularities. There was the greatest difference between the appearance of Prof. Holmes's desk—we always called him "Professor Homes"—and that of McGee. The former was in scrupulous order, nothing upon it not in current use, and everything else in meticulously exact files. McGee's desk on the contrary was covered with a miscellaneous assortment of books, papers, separates, and so on to a depth of several inches and many of those on the edges were falling off on the floor.

One of the questions at issue, as it happened, was the status of the funds under which I had been working before entering the Bureau. There was a provision at that time under which the Bureau was empowered to buy certain completed manuscripts and I think it still exists. I had been working on Chinook and Dakota with the understanding that my manuscript would be paid for at the end of the year and that was done, although Professor Boas advanced the money out of his own pocket from time to time before

being reimbursed. There were other angles to the question, as to whether the payments had not been excessive, and so on. A similar question was raised regarding the purchase of certain photographs from Prof. Frederick Starr {first anthropologist at University of Chicago, famous for serving ice cream in the colors of the marjor races at his final exam}. I was called down to the Smithsonian to testify along with others, and from a transcript of my testimony which came up later I judge that my replies were not in Addisonian¹¹ English. I really do not know what the final report was, or even whether one was made, but it is probable that irregularities of one sort or another could be made out. At any rate, McGee gave up the struggle and resigned. Afterwards, I believe, he did work under the Geological Survey with which he had had earlier experience, and not many years later he died {and won a bet with Major J. W. Powell as to which had the bigger brain}. He did not impress me as a profound thinker but as intensely desirous to win scientific consideration and while aping originality desperately rather by means of unusual verbiage than new ideas feared to depart from the scientific "party line" of his day. Knowing about his previous work in geology, I once asked a very able geologist I happened to know about McGee's work in that field, and he said that he thought there was little of value except for one short article which he highly praised. I think he was earnest in his desire to be a true scientist and was warm hearted in his relations with others. He always treated me with the utmost consideration and only differed from me when I expressed my belief that the matrilineal system of the Northwest Coast Indians was not primitive. At that time [Lewis Henry] Morgan was the presiding genius of Bureau thought in sociological lines. The significant dissenter in the older group was Mooney who declared that the Kiowa Indians had not matrilineally organized clans and he could find no evidence that they ever did have any.

Since Boas had dealt through McGee and the transactions between them were in part the

occasion for the investigation just mentioned, Prof. Holmes was not prepossessed in my favor, but he always treated me courteously. I happened to share with him a sensitive make up but that was our only point of resemblance. I am not artistic in any sense of the word; my sensitiveness is crudely physical and not connected with any appreciation of color, form, or tone. As a "Boas man" I shared {in} the opposition to Boas entertained by many, if not most, of the Washington anthropologists at that time. More than in his later life, Boas was inclined to lay about him with his rapier intellect, and while he was more often right than not, only the very great or the very unselfish can accept a rebuke to their self-esteem without entertaining a dislike of the source from which it comes. And while in my experience of Boas I have always found him eager to accept truth whether or not it supported a private theory, he was not always right nor always just in his estimate of men. The affection which most of his students had for him was based on their admiration of his ability and respect for his scientific honesty as illustrated in my own case as given above. He had not that sympathy for the man underlying the error he entertained which drew everyone to Lincoln. But how many scientists have?

When I entered the Bureau I was the only member of it who had even that purported to be an anthropological education and goodness knows that was thin enough. Powell, as I have said, was soon to pass off the scene. In spite of his paper on Wyandot social organization in the very first volume of the Bureau reports, and some notes on the Ute Indians, he contributed comparatively little to anthropological research,¹² but through his ability in handling successive generations of congressmen he performed a major service to all anthropologists in securing the foundation of the Bureau of Ethnology, just as his work in the establishment of the Geological survey has placed all in geology in his debt. As I have intimated, Holmes was of an altogether different type. By himself he would have been utterly incompetent to secure the foundation of

the Bureau, and he lacked entirely that mixture of tolerance and firmness demanded of an executive. To be sure he had tolerance enough. That was his bane. Firmness was the element wanting. This was due evidently to his highly sensitive artistic nature. He could not bear rows, and was I know injected into that at the very beginning of his service as Chief wholly against his will. He could be imposed upon easily by anyone who understood his weakness, and I have seen him tremble all over at the mere suggestion of any serious difference either within or without the Bureau. He usually gave in at once if any of his staff chose a different line of action from the one he apparently thought should have been taken. I have known him to take an apparently firm stand in favor of a certain measure, and give in completely when the issue actually came to a head. He was thoroughly unhappy as an executive, but thoroughly contented and entirely competent when left to himself. His papers on archaeological subjects were composed with meticulous care and thoroughly illustrated. Nevertheless, it was the general feeling among his associates that he was rather an artist than a scientist and they believed it would have been better for him to have devoted himself entirely to his art in which there is no question as to his transcendent ability. He was, I believe, regarded as the greatest water colorist of his time, and the sketches with which he accompanied some of his scientific papers, notably one on the ruins of Maya cities, are inimitable. His sketch of the Grand Canyon of the Colorado, made to illustrate a geological paper before he came into the Bureau, has been copied over and over, and I well remember the enthusiastic praise bestowed upon it by as exacting a scientist as William Morris Davis. Holmes's sensitivity led him to lean upon others and during the latter part of his life, both before and after his resignation from the headship of the Bureau the particular "other" was Aleš Hrdlička who had been appointed Curator of the new department of Physical Anthropology in the National Museum. Dr. Hrdlička was the outstanding physical anthropologist of his

time and not only built up the collections in his department to become one of the greatest in the world but founded the central organ of the science, the *Journal of Physical Anthropology*. There is no doubt as to his ability, his energy and his devotion to his subject, and also as to his honesty, though it must be added that he honestly believed that he had all of the answers. His prejudices were so much a part of him that he did not realize he had any. Whether he possessed any sense of humor may be questioned. Anyhow, I do not think that he considered that there could be any joke connected with science or that a scientific man could be the victim of one. And as to a joke on himself, one might as well make merry with the Ten Commandments.

I do not know what Hrdlička's private opinion was as to the antiquity of man in America before he came to Washington although he did on one occasion back up a contention of Prof. Putnam's regarding the human character of a thigh bone unearthed at Trenton. This did not, however, involve anything conclusive in the matter of age, and from what I knew of Hrdlička later, I do not feel that he lacked honesty in the expression of any opinion. After he came to Washington, however, he sided immediately with the Holmes-McGee view in denying any great antiquity to man in America. This was, as I think I have said, a very useful corrective to such an extreme view as that involved in the assumption that the Calaveras skull belonged to the stratum in which it was found, but Hrdlička carried his opposition to such extremes that he was wholly in error regarding finds of real antiquity in Florida, and certain parts of the west. In his picture of Hrdlička like a modern Horatio, defending the continental bridge from too early intrusion, Hooton has beautifully characterized the situation. In defending almost alone the claims of Neanderthal man to a place in the ancestry of *Homo sapiens* Hrdlička was somewhat more fortunate as later discoveries indicate. Right or wrong, he did not have a character likely to make him widely popular. It is said that an Indian in the Southwest once

shot at him, but fortunately missed. Except for Holmes, Hough, and one or two more of such an easy and genial make up as to have gotten along with the devil, most of Hrdlička's confreres were alternately moved to wring his neck or shout with laughter at the Hrdličkaesqueness of him. Fortunately American scientists generally have a saving sense of humor. Had Hrdlička flourished in pre-war Europe, I fear he would have been involved in more than one duel. From this point of view one can understand how Hooton got along with Hrdlička so well. The one could indulge his sense of humor, and the other would never take it home to himself.

One of the happiest days of Holmes's life, it seemed to me, was in 1910 when he retired from the headship of the Bureau and returned to head the Division of Anthropology in the National Museum and become Curator of the American Gallery of Art. The staff of the former was small and its appropriations taken care of along with those of the Museum as a whole, and the latter was in Holmes's own special line in which he had no peer. Still later he confined himself to the latter entirely and his Head Curatorship of Anthropology was given to Dr. Walter Hough, a long standing friend. As I have already intimated, Hough came to understudy him as a companion of Hrdlička. Hough was a very genial, lovable person, and I shall always remember with gratitude the kind things he said to me from time to time when I was the kid member of the Bureau and none too sure of myself. It was his misfortune to share some of Holmes's weakness as an executive. But I have more than a little sympathy with that sort of defect, and my consciousness of the lack was partly responsible for my decision to decline the headship of the Bureau at a later date. The proper association of strength and sweetness and of executive with intellectual ability seems relatively uncommon.

One of the oldest members of the Bureau of Ethnology in years when I entered was Professor Cyrus Thomas. Previous to his appointment here he had been engaged in work in Entomology. He

came from that part of southern Illinois locally know as Egypt, presumably on account of the name of its chief city Cairo. Although from a section in which there had been much sympathy with the Southern cause during the Civil War, he was strongly Unionist in his beliefs and related to the Logans. On coming to the Bureau he had been a pronounced strong believer in the existence of race of Mound Builders distinct from the American Indians, and Major Powell gave him the commission to put this theory to the test. Offhand one might suppose that the investigation would have been prejudiced in advance but evidently the Major knew his man, for, while Thomas certainly was "sot" in his opinions, his other name was honesty and, when he came back with his opinion on this matter entirely altered, the case for the negative acquired additional strength. Whether he would have changed all of his opinions as readily I do not know, but he did abandon another view, an attempt to relate the Polynesian tongues to some American languages. His natural conservatism came out, however, when I mentioned simplified spelling then being pushed by Andrew Carnegie and some other reformers. Like the old "party line" linguists he thought that if we dropped *ugh* from *through* and *although* and spelt *trough* thru we would destroy the etymology of our speech, and he also animadverted against such an attempt by "an unauthorized committee," though what he would have considered authorized I do not know. Whether the Thomases were in the majority at this time I do not know but the attempt at reform failed although it almost succeeded in dropping the E out of whiskey. On examining the advertisements of those distilleries which seem to be the main support of our periodic literature, I find that one out of seven drops the E. It is too bad that the elimination stops there and that so many people choose to drug their way through life. However, the history of condiments and of spelling alike show the strength of habits, particularly bad ones. The fact is that an entrenched defect is the very devil to get rid of—and no doubt for that reason. We

are thus condemned to the indefinite typing of "GH's" because it takes too much mental effort to eliminate them. In the same way, I suppose our standard typewriter key board does not have the best possible arrangement of letters as one is aware every time he has to use "also" or try to work in both hands on words ending in "-erty." But it is standardized and that settles it, I rather admire the old days when editors were non-existent and spelling eclectic. After all, you generally know what Shakespeare meant. The only way to reform anything that has become "standardized" seems to be to revolutionize the whole thing. I mean to cut under the entire process that has been frozen in this way. Thus, if we could talk our letters and our books into something that would talk back to us, the use of letters might be abolished and the supernumerary letters along with them. I do not know just how that is to be done but I hope it will be by some system unlike the present radio which can't keep anything to itself but must spill it over the entire neighborhood.

While Dr. Thomas's ideas were to some extent standardized, he compromised not at all with the truth. He was not case hardened. You knew where he stood at any moment but you might not find him there when you came back. In that last particular he differed from Hrdlička. When you came back to Hrdlička he was always there, just where the Lord created him, on the rock of ultimate Hrdličkian knowledge.

Another of the older men with pronounced opinions, and who, though not a member of our staff, spent most of his time with us, was Dr. J. D. McGuire. He was a man of means who had owned a large farm in Howard county and been much involved in Maryland politics but had sold his property there and moved into the District. He was thick set, unlike Dr. Thomas, and with a florid face which turned purple in some of the heated arguments he had with the latter. He had two anthropological interests which amounted to obsessions. One of these was tobacco on which he had written an excellent report, and he was pursuing this subject during

the last years of his work among one us. This consisted in setting down the words for tobacco and pipe in all of the Indian languages in which he could find the terms, and his material was afterwards profitably consulted by Dixon. His other obsession was the recent and Old World origin of Indian arts and industries. On certain smoothed objects he thought he could distinguish parallel marks that must of necessity have been made by an iron, and therefore European, file. American work in copper was also supposed to be, if not European in origin, at least European in motivation. In this he represented an extreme reaction from the old Mound Builder theory, and it was in line with the sentiments of Holmes and McGee. One day he came to me with a book on the antiquities of Scotland, and pointed triumphantly to some illustrations of copper circlets resembling in a measure the copper collars made on the North Pacific Coast. He thought that the latter had undoubtedly been copied from the old Scottish specimens, until I called his attention to the fact that the Old World specimens antedated Christianity while those of the Northwest Indians could have come into existence only during the last few centuries, and that Scotland and Alaska are not very close neighbors. "Evidently very late" was his comment when almost any artifact of relatively high quality was under discussion. On these matters he and Dr. Thomas frequently locked horns, especially in their discussion of copper. At times Thomas would contradict him flatly, and to contradict a southern gentleman of north of Ireland ancestry is not to be lightly undertaken. As the two gentlemen often occupied one of our cubicles together, they suggested somewhat the saw-pits in which the famous Kilkenny cats settled their differences, and the heat of the discussions made us wonder whether the end of the encounter might not be the same. Mr. Hewitt used to tell with his rare gusto how McGuire on one occasion came out to the elevator purple with rage and said "I could kill that man." The next day they were probably at it again with no blood shed. In fact,

I am sure that they had a real regard for each other, but they were at odds so much that it was considered a triumph when I obtained a snap shot of the two sitting side by side and smiling. Both passed away in the early days of my association with the Bureau, Dr. McGuire was stricken with some nervous affection which began to show itself in his left arm, causing it to shake continually, finally he got so feeble that he had to remain at home and I called upon him once finding him sitting at the top of the front steps of his house on Sixteenth Street. Not long afterward he passed away. However, some time elapsed after Thomas left us before Dr. McGuire was forced to remain at home, and during that period the latter was, in constant association with Dr. {Jesse Walter} Fewkes, later Chief of the Bureau. After that there were no more Donneybrook Fairs, Fewkes being more of the type of Holmes and Hough.

Mooney and Mrs. Stevenson were in the field when I settled down in Washington permanently after returning from the Queen Charlotte Islands. "Tilly" (Matilda Coxe) Stevenson was the widow of James Stevenson, an intimate friend of Major Powell and his right hand man in securing appropriations for the Bureau. He died before my time, but was said on all hands to have been possessed of a delightful personality which assured him a wide circle of friends. His wife was of another order.¹³ She obtained her effects by going after what she wanted and taking it. She shared so little in the esteem her husband had enjoyed with Powell that he wished to drop her from the staff of his organization after having appointed her to it. However, during her husband's lifetime she had secured a rather large circle of friends in the political world, and he found it advisable to reinstate her. It is quite likely that most of these gentlemen knew her husband rather than herself. All this was under the bridge in my time, and she was generally in the Southwest making that study of the Zuni Indians afterward published by the Bureau. I was appraised of her first return to Washington by the irruption of the chief clerk,

Mr. Clayton, into my room and appropriation by him of my favorite chair which, as it seemed, Mrs. Stevenson had used before she went west and which she regarded as personal property. In 1910, when we moved down to the Smithsonian building, she was assigned a room in the north tower on the second floor which it was thought would be more convenient for her than one higher up. She was then in the field, and on her return demurred somewhat at the assignment and fell in with the plan only after the room had been thoroughly clean and reconditioned while she stood on a chair in the middle of the floor superintending the operation. She was motivated largely, it seemed, by the fact that that room had been occupied by the fish specialist of the Institution for I do not know how many years without the admission of a renovator. She worked away from the office, however, a great deal of the time, and ultimately formed a partnership with another lady and together they bought a ranch in New Mexico. Probably the ladies were too much alike for they presently fell out and the other being younger outlasted and outlived the subject of this sketch, who, I think, died in that country. Mrs. Stevenson was related to the famous "Fighting Bob" Evans of Spanish War fame, and claimed that her devotion to cleanliness as exemplified in the above instance was due to her experience with "Cousin Bob's ship," though it was rumored that her affection for "Cousin Bob" was not mutual. Well! as Miss Clark, secretary to our Chief at the time, remarked, "Something was always doing" when Mrs. Stevenson was about. She was able to appropriate to herself more personal service than the rest of the Bureau combined.

Mooney did not return from the field much before our removal to the Smithsonian building, but after that I saw more of him than of any other of the staff until his untimely death, except possibly for Dr. Fewkes. Like some of the other members Mooney was "set" in his opinions, and he had the courage of them. The peyote question came to the fore about this time. Some wished to prohibit its use altogether asserting that the Indians were being demoralized by it, but

Mooney held that the use of it, like that of wine in the Christian communion, was an essential element of a native cult, and that those who used it uniformly gave up the much more damaging use of alcoholic spirits. In this he was supported by the Omaha Indian, Francis LaFlesche, who had been added to the Bureau's force in 1910. I attended a congressional committee investigation on this matter at which Mooney testified strongly in favor of peyote and in opposition to his co-religionist, Father {William} Ketcham.¹⁴ Mooney, like most members of south Ireland families, had been brought up in the Roman Catholic faith, one of his sisters was a mother superior, and practically all of his connections had strong Catholic affiliations. He told me that he believed Catholicism was best suited to the Irish temperament and that he should always be of that faith. In this particular, however, the very strong political views he held clashed rather sharply with strict Catholic doctrine. For Irish Nationalism was, it seemed, even dearer to his heart than religion. It came near being his religion. Naturally, he entertained a distinct dislike of England and this made his position a difficult one during the first World War. But his nationalistic faith involved an even more pronounced antipathy to the higher clergy in his own denomination which he accused of being responsible for smothering every effort of the Irish to obtain their independence. He claimed, and quoted expressions to prove it, that the higher clergy was willing to sacrifice Ireland in the interests of the Church in the British Empire as a whole and that they were afraid that much of that stream of young men which had been pouring into the Catholic priesthood would be deflected into purely political life if Ireland had her own government. He commended the French clergy because the Catholic priesthood in other lands accused them of being "too French," and expressed the opinion that the Church in each country should be autonomous. I imagine this view could hardly have received the approbation of many of Mooney's Catholic friends if he ever expressed it to them as he was quite capable of doing. His study of the Irish situation made

him liberal and favorable to reform in other lines as well, even to socialism though that was anathema in Catholic circles. As I was myself interested in the subject at the time, we found ground of common interest in this as well as in many other matters.

Some time after Fewkes became chief of the Bureau, Mooney had a falling out with him due in considerable measure to a clash of temperaments. Mooney, when this break took place, was in the Kiowa country, I think, at least in the far west studying certain Plains Indian ceremonies, and complaint was sent to the Office of Indian Affairs by some field agent that Mooney was encouraging practices inimical to the best interests of the Indians and which the Office was trying to stamp out. On receiving this complaint, Fewkes ordered Mooney to come home and report. If I have gotten the story straight, I believe this to have been a mistake. The head of an office should be prepared to back up his men until an accusation leveled against any of them is substantiated, or at least until he has had a chance to reply. But Dr. Fewkes resembled Holmes to some extent in hating a row and being too sensitive to hostile criticism. Therefore, he reacted immediately by calling Mooney home to report and thereby interrupting his work, instead of presenting the complaint to him in a written form to be answered by mail. That is in accordance with my understanding of what took place, but I have not, and never had, access to the details.

This accusation, that the Bureau was encouraging the perpetuation of "heathenish ceremonies," was not a new one. At a much earlier period when Mooney and George A. Dorsey, then of the Field Museum, were studying the Cheyenne, it was claimed by the agent of the latter, a man who indeed had an excellent record in dealing with the Indians, that Dorsey and Mooney had paid the Indians to stage a Sun Dance. So far as Mooney was concerned there was nothing whatever in this nor do I think Dorsey would have needed to be at any expense to secure such a ceremony which was given from time to time until a much later

date. Agent Sage's attitude reflected probably the missionary influence of the time which was directed to the total suppression of native Indian ceremonies, since then the churches have learned something of that toleration inculcated by the founder of Christianity and missionaries take courses in anthropology before entering upon their work, but it was not so when I first entered the Bureau. The shift from hostility to tolerance and later to appreciation, study, record and even preservation of ceremonies has been rapid since then, reaching its apex with the Collier administration.

Mooney's health had suffered greatly from the privations necessitated by his many years of fieldwork, and it was not long after the difference of which I have spoken that he was taken sick and died. We all felt his loss keenly and perhaps no one more than myself.

Once I met Washington Matthews in Mooney's office. That was before 1910 when we were still on F St. and my memory of the event is rather dim, but the impression left on my mind is of a vigorous, large-framed man with a very active mind and it seems to me that he had lost an arm or at least was one sided.

John Napoleon Bonaparte Hewitt was a kind of Bureau institution. He was appointed back in the 1880's I think and died in office in his 80's. He had been brought in in 1886 to complete the work of Mrs. Erminnie Smith on a dictionary and grammar of the Tuscarora language in which he had been acting as her assistant.

Those acquainted with Mr. Hewitt will be somewhat amused to read in the report announcing his appointment that the works in question were "soon to appear." Hewitt was descended, as he told me, from the Bear clan of the Tuscarora and that was founded by a captive white woman. He had lived away from his people for some time before his appointment and was obliged to learn Tuscarora anew. He was, he has told me, a conductor on a street railway system in northern New Jersey. The wages were low but in spite of that and a long day's work, each conductor had to clean out his own car or pay a small sum to an assistant to

do so. Generally they were so tired as to choose the latter alternative, and as illustrating the corporation morality of the period he told me that his own superintendent was surprised to learn that, instead of embezzling this money out of the day's take in, the men paid it out of their own pockets. The super's comment was "They're fools if they do." If that was typical of the principles prevalent among the officials, one wonders how much money ever got through to the stockholders. We all liked Hewitt, and his conversation, which generally turned upon Indian ceremonies, was always interesting, but 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 next day, next week, or next year. This was perhaps a holdover from his Indian ancestry. At any rate, he would procrastinate, even if it was against his own interest. For instance, he was promised a substantial rise in salary if he would catalogue the manuscripts in the vault of which he had been given charge. In vain was the completion of that work urged upon him and urged repeatedly. He never got round to doing it, and in despair it was turned over to the then chief clerk. The result was, not unnaturally, defective in many ways and there were not a few errors which Hewitt enjoyed calling one's attention to. He did not develop a profound dislike of the individual in question, however, as might have been expected. He criticized us all more or less, but I think his peculiarities arose from a realization of his inferiority in education to most of his associates and his inability to concentrate on a problem and see it through to its completion. It was a kind of defense mechanism. 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 had to do the best he could because of Hewitt's only 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. The volcano relapsed into slumber. Hewitt did indeed publish some Iroquois myths and edited at the same time some others by Curtin but not until the manuscript was fairly torn from him. Part of his procrastination was indeed a commendable desire for accuracy, but perhaps still more a fear of inaccuracy and consequent criticism. The less one publishes, the less one has to fear hostile, or any other, criticism of what he does. He has taken to heart the well known old exclamation "Oh!, that mine enemy would write a book!" There is, however, one way to get material from the confirmed introvert, or shall we say "introscript" and that is by printing something adverse to his prejudices or indeed downright wrong. In an old Haida legend the trickster and clown Raven obtains a much-desired diet of sea urchins which the ducks were gathering by standing on the shore where they were fishing and insulting them. The result was, of course, that they could not resist the temptation to bombard their assailant with sea urchins and he profited accordingly. Some of Hewitt's smaller contributions were teased out of him in a similar manner. 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." 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. He was given a great deal of work within the office answering queries, and to one of these he might devote a month or more, branching off during his quest into no end of country lanes, so that he acquired in that way a great fund of

information. How to bait your hook so as to get it out of him was the problem.

Hewitt came to the Bureau when Powell was constructing an anthropological philosophy and this included the then ruling cult of Morganism with proper regard to the Frazer, Lubbock, and McLennan theory of an original matriarchate and systematic change to our patriarchal society. Hewitt had absorbed this view and the McGee terminology which went with it as part of the necessary equipment of every Bureau member, and I got in bad with him in consequence. Nevertheless, my relations with Hewitt were quite uniformly cordial, and what I am saying now about him is not in an unfriendly spirit. It is characterization, not criticism. I am sure we all came to be very fond of the old fellow and his failings rather amused than irritated us. If he did not publish much, he left Iroquois manuscript material of value to future workers.

When the Bureau was established an impression existed in Congress that its work would be temporary, and we had to answer the question to every new session of the national legislature when we would be through. It also wanted to see results, and fortunately it did in the very solid series of volumes which constituted the annual reports and the bulletins. However, in my time it was thought that something more appealing to the untechnical minds of legislators ought to be provided, and we resumed with vigor an undertaking started some years before independently by Prof. Otis T. Mason of the National Museum and Mooney under the name "Synonymy of the Indian Tribes." To take over all editorship of this work, the name of which was changed to "Handbook of the American Indians," Mr. Frederick W. Hodge was recalled to the Bureau where he had formerly served as librarian. The editorship could not have fallen into more competent hands as I am sure everyone who has had occasion to use that extensive publication will agree. The work of preparing special articles was assigned to the several members of the Bureau staff, to those anthropologists connected with the National Museum and to others both in and

out of Washington. We also held meetings to discuss the assignation of articles and matters of general policy and these were very interesting to me. Whatever contributions any of us made, however, are incidental to a work which remains a great monument to the industry and intelligent handling of its editor.

A few other men were brought into our circle to work at that time. One of these was a physician named McCormick, a friend of Powell and McGee, I believe, who was a very enthusiastic Mason and when he left us took up work for his order in Mobile. Another was Frank Huntington who occupied the same cubicle as myself. These were retained to do editorial work. Huntington was, I regret to say, somewhat addicted to the flowing—or rather the overflowing—bowl, and that may be why, when I returned from one field trip, I discovered that he had been using my office coat as a penwiper.

Mention of the flowing bowl reminds one not unnaturally of the then head of the illustration department, Delancey Gill, about whose artistic abilities and convivial propensities there could be no doubt. He was very friendly with everyone, not merely between times, and I must add that I never saw him deflected from the straight line of march or of thought in consequence of his habits. He had been with the Bureau from very early times and remained with it until a few years before his death.

John Swanton Major Publications

- 1900 Morphology of the Chinook Verb. *American Anthropologist* n.s. 2(2):199–237.
- 1902 Notes on the Haida Language. *American Anthropologist* n.s. 4(3):392–403.
- 1904a The Development of the Clan System and of Secret Societies Among the Northwestern Tribes. *American Anthropologist* n.s. 6(4):477–485.
- 1904b Origin of the Bellacoola. *American Anthropologist* n.s. 6(5):743–744.
- 1905 Contributions to the Ethnology of the Haida. *Publications of the Jesup North Pacific Expedition 5; Memoirs of the American Museum of Natural History*, 8(1):1–300.
- 1905 Haida Texts and Myths, Skidegate Dialect. Bureau of American Ethnology, *Bulletin*, 29.
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- 1911 *Indian Tribes of the Lower Mississippi Valley and Adjacent Coast of the Gulf of Mexico*. Bureau of American Ethnology, *Bulletin*, 43.
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- 1922 Tokuli of Tulsa. *American Indian Life*, by Several of Its Students. Elsie Clews Parsons, ed. New York: B.W. Huebsch, Inc.
- 1922 Early History of the Creek Indians and Their Neighbors. *Bureau of American Ethnology Bulletin*, 73. Washington, D.C.
- 1928a *Social Organization and Social Usages of the Indians of the Creek Confederacy*. Bureau of American Ethnology, Annual Report 42 for 1924–1925:23–472.
- 1928b *Religious Beliefs and Medical Practices of the Creek Indians*. Bureau of American Ethnology, Annual Report 42 for 1924–1925:473–672.
- 1928c *Aboriginal Culture of the Southeast*. Bureau of American Ethnology, Annual Report 42:673–726.
- 1928d *Chickasaw*. Bureau of American Ethnology, Annual Report 44:169–273.
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- 1929 *Myths and Tales of the Southeastern Indians*. Bureau of American Ethnology, *Bulletin*, 88.
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- 1931 Modern Square Grounds of the Creek Indians. *Smithsonian Miscellaneous Collections*, 85(8):1–46. Washington, D.C.
- 1932 *Green Corn Dance*. *Chronicles of Oklahoma* 10:170–195.
- 1944 *Notes Regarding my Adventures in Anthropology and with Anthropologists*. Manuscript no. 4651, National Anthropological Archives, Washington, D.C.
- 1998 *Early History of the Creek Indians and Their Neighbors*. Gainesville: University Press of Florida. [Bureau of American Ethnology, *Bulletin* 73, 1922]

Supporting References Sequential with the Swanton Text

1. {Jeffries Wyman inaugurated archaeological study of sites, especially mounds, in both Maine and Florida.}
2. {George Amos Dorsey (February 6, 1868–March 29, 1931) famously wrote a best seller titled *Why We Behave Like Human Beings*, as well as a novel *Young Low*. He became a popular lecturer after he left the Field Museum, and had a reputation as a womanizer, traveling around the world with his “secretary.”}
3. {George Byron Gordon (1870–1927) took over after the sudden death of John Owens (27 September 1865–18 February 1893) at Copan. At Penn Gordon famously hired the Tlingit noble George Shotridge to bolster their Northwest Collection at the University Museum. Many of these crest and clan artifacts have since been

- rematriated to Tlingits, Tsmshians, Haidas. Gordon also locked horns with Frank Speck, who was able to leave this conflict at the museum and assume the chair of the new Anthropology department.}
4. {In 1997, John B. Zoe, Elizabeth Mackenzie, Mary Siemens, and Tom Andrews accepted from University of Iowa on behalf of the Northwest Territories, the Dogrib caribou skin lodge collected in Rae by Frank Russell in 1894 and now in Yellowknife.}
 5. {Neil Judd *The Bureau of American Ethnology—A Partial History*. Norman: University of Oklahoma Press 1967.}
 6. {Charles C Willoughby *The Turner Group of Earthworks, Hamilton County, Ohio*. Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University VIII(3):1–132, 1922. Willoughby succeeded Frederick Ward Putnam at Harvard and continued its Ohio excavations, including the spectacularly detailed human clay figures at Turner.}
 7. {Bruce J Bourque, With contributions by Steven L Cox and Ruth H Whitehead *Twelve Thousand Years—American Indians in Maine*. Lincoln: Bison Books 2001:51–66. Now known as Moorehead Phase (5000–4500 BP) of Late Archaic, it relied on swordfish and cod along the coast, and made red ocher offerings in huge cemeteries.}
 8. {Ernest Volk *The Archaeology of the Delaware Valley*. Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University V:1–258, CXXV plates 1911.}
 9. {Penelope Ballard Drooker *The View from Madisonville*. Ann Arbor: University of Michigan: Memoirs of the Museum of Anthropology #31 1997. This huge site, now encroached on by Cincinnati, marks the departure of peoples from the Ohio to the Missouri to take advantage of Plains bison herds.}
 10. {George Pepper *Pueblo Bonito*. Anthropological Papers of the American Museum of Natural History XXVII:1–398 1920 Reprinted UNM Press 1996.}
 11. {Joseph Addison (1672–1719) and Richard Steele (1672–1729) published early London newspapers, *Tatler* and *Spectator*.}
 12. {Don and Catherine Fowler, *Anthropology of the Numa: John Wesley Powell's Manuscripts on the Numic Peoples of Western North America, 1868–1880*, Smithsonian Contributions to Anthropology #14, 1971, compiles his massive notes. See also Don Fowler and John Matley, *Material Culture of the Numa: The John Wesley Powell Collection, 1867–1880*, Smithsonian Contributions to Anthropology #26, 1979, DOI: <https://doi.org/10.5479/si.00810223.26.1>
 13. {Nancy Oestreich Lurie, *Women and the Invention of American Anthropology*, University of Washington Press, 1966; Waveland Press 1999:18–23.}
 14. {Bureau of Catholic Indian Missions, DC. When its records were set to be discarded, they were rescued by Fr Frances Prucha, SJ, and sent to Marquette in Milwaukee, Wisconsin.}

ACKNOWLEDGMENTS

This effort is dedicated to the memory of Raymond D. Fogelson, who first showed me this memoir and promised to proofread my digitalization of it.

“The Haida” by Adolf Bastian with an Introduction by Richard L. Bland

Abstract *In 1882, Adolf Bastian, a German scholar, wrote an article on the Haida people living on the west coast of Canada. The focus of the article is aimed at ethnological information relevant to the Haida. Bastian’s effort was in response to the then widespread recognitions that Native cultures were rapidly disappearing and should be documented before they vanished. The original article, which was written in German, with quotes in English, French, and Latin, has been translated by Richard L. Bland. Bland also provides an introduction to provide the context of Bastian’s work to the readers of the Journal of Northwest Anthropology. All footnotes in this collection are added by Bland.*

Keywords

Haida, Canada, disappearing cultures, translation

Introduction

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When Europeans arrived in the New World the focus was on commerce.¹ What did the New World have that could be turned into cash? Many examples could be given. Here we will use the example of the Russians, British, and Americans acquiring furs on the Northwest Coast North America for sale in China, Russia, and other places. There was no thought of the Indigenous cultures that were being expunged. In fact, every effort was made to turn the Indigenous peoples into “Europeans,” ostensibly for their own good, but in fact to exploit their land and labor.² By the mid-1800s scholars began to realize that the vast, interesting, and important Indigenous cultures were rapidly disappearing and quick steps needed to be taken in order to preserve the knowledge they contained.³ Thus, collectors were sent out to retrieve as much physical⁴ and

non-physical culture as possible.⁵ One of those urging the collectors on was Adolf Bastian.

Adolf Bastian (1806–1905) was a polymath whose ideas influenced scholars far and wide, including those such as Franz Boas and Carl Jung. He is known particularly for his contributions to ethnology and was one of the founders of the Ethnological Museum of Berlin. He was acutely aware that Native cultures were rapidly vanishing, thus made every effort to collect as much information on them as possible. One of his efforts to do this was the publication of the article below. The article is specifically aimed at ethnological information, though the principle behind it—the collection or preservation of as much information as possible—is relevant to all areas where the materials are vanishing forever, for example, archaeological and historical.

¹ Gibson, James. *Otter Skins, Boston Ships, and China Goods: The Maritime Fur Trade of the Northwest Coast, 1785–1841*. Montreal: McGill-Queens University Press, 1992.

² Grinëv, Andrei V. *Russian Colonization of Alaska*. Lincoln: University of Nebraska Press, 2018.

³ Cole, Douglas. *Captured Heritage: The Scramble for Northwest Coast Artifacts*. Seattle: University of Washington Press, 1985.

⁴ For example, see Jacobsen, Johan Adrian. *Alaskan Voyage 1881–1883: An Expedition to the Northwest Coast of America*. E. Gunther (trans.). University of Chicago Press, 1977.

⁵ Boas, Franz. *Indian Myths & Legends from the North Pacific Coast of America*, D. Bertz (trans.), R. Bouchard and D. Kennedy (eds.). Vancouver, B.C.: Talonbooks, 2006.

Bastian was a person of another time and place, thus his article needs a bit of explanation. Being a polymath he wrote, quoting various authors from different countries, with different languages, which he assumed his audience knew as well as he himself. Therefore his quotations are in the language of the original author. Bastian's article was written in German with numerous quotations, many of which are in English, French, Latin, and other. I have elected to do two things in translating the article: first, to leave Bastian's style as much like the original as possible, and second, to distinguish the different languages he quotes by underlining and identifying the language, giving an abbreviation of the language at the end of the sentence. This will no doubt jolt the reader, though I believe it will be little different from reading the original where the reader has to change from German to English, then back, then to French, etc. In a word, I am trying to present as much as possible the feeling of reading Adolf Bastian. Nevertheless, the bottom line is the need to act quickly with regard to saving vanishing cultural resources.

A person might ask why this topic is necessary in this day and age. Don't we know that saving cultural resources is a good thing? Perhaps an example will suffice. In 1899, seventeen years after Bastian published his article, a group of scholars, including John Muir, William Healy Dall, and other notables from the scientific world and the Smithsonian Institution, accepted an invitation from Edward Harriman, a self-made millionaire, to take a cruise up the Northwest Coast. On the way, they stopped at Native villages. When the ship stopped the passengers would rush ashore, take what they like, then pose for photos with their trophies. The deplorable action of scholars is recorded in prose, poem, and photograph as they pillaged a Native village.⁶ This was seventeen years after Bastian was pleading for conservation. My point: I believe it is good to be constantly reminded that we need to have respect for the cultures of others and that we need to protect those that are disappearing.

The Haida⁷

Adolf Bastian

Translated by Richard L. Bland

How much the critical state of affairs in which ethnology (concerning its establishment as a science generally) finds itself at each moment of the present, so to say, to be a more precarious one, can be presented here by a striking example at this offered opportunity.

How much has already been irretrievably lost is recognized, and any possibility of an eventual future restitution will never occur for the pure ephemerality of psychological institutions among

simple native tribes who do not have writing nor lasting monuments that can be brought to light again in later excavations.⁸ For them it can be said: Now, in the moment of fleeting existence—or at no time and never again—for when perished before their types are fixed in the ethnological museums an unfillable hole will then gape forever in the statistical overview of the world and make the work of induction more difficult. For this the data are needed

⁶ Goetzmann, William H., and Kay Sloan. *Looking Far North: The Harriman Expedition to Alaska, 1899*. Princeton University Press, 1983. Pp. 161 ff.

⁷ This is a translation of "Die Haida's" from *Die Zeitschrift für Ethnologie* XIV:278–298 (632–652), 1882. Berlin.

⁸ Bastian often uses the word *psychisch* which generally translates as "psychic." This word has acquired a rather specialized meaning in English. I think Bastian is pointing here and throughout toward institutions in the realm of the spiritual, psychological, religious, and the like, as a contrast to social, economic, technological, etc.—*Trans.*

as the first material for laying the foundation for nonliterate peoples, thus the ethnological collections. Insofar, the ethnological museums are to be placed with those of the natural sciences—not so much with the art historical ones in which archaeological collections to a certain extent offer only a helping device to the texts of the classics on which the study is based in full scope; and thus, the *Monumenta Germanica* is to be sought less in the museums than in the libraries.⁹ However, in ethnology by contrast, collections of which yield the actual documents themselves for the entire number of native tribes so that their abolition would also involve that of scientific study, since such study has only rarely been thoroughly dealt with on the spot, and with isolated exceptions the world view propagated in hereditary tradition was scarcely to be obtained intact, even under the chances of favorable combinations. And the study of occasional superficial travel notes which may be brought back is all too frequently done up in such a way as to create greater seductions, entangling more and more in erroneous ways the obvious inclination toward theoretical illuminations.

Thus, each short moment shapes itself to the most urgent duty, to be used as much as

possible to save that which might be left, and bring without hesitation to safety that which anywhere in the worldly sphere may yet be remaining in the swiftly (and with the increased commerce of the last decade, swifter and swifter) leveling of the lives of native peoples. We have to reconstruct the psychological world, as it is reflected in the organic growth of the spirit, for each ethnic center,¹⁰ from the milieu. The impression of the milieu will vary accordingly. There are often found, as in zoological and botanical provinces, broad expanses uniformly covered with monotonous monotheism, perhaps in the religious domain, then again, a wealth of variations pressed together in narrowly limited terrain. The most complete productions must be found, as everywhere in organic development, where in increased majority the accumulating attractions meet, and therein the historical localities on the earth are marked as favored, similar to those of the Mediterranean Sea by the land of origin of our own culture.

In this regard now there is scarcely a topographically more significant area on the earth than that where the two great continents themselves—out of the eastern and western hemispheres—meet at their closest approach. Following the American Northwest Coast, the

⁹ *Monumenta Germaniae Historica* [Historical Monuments of the Germans] is a “voluminous, comprehensive, and critically edited collection of sources pertaining to German history from about ad 500 to 1500. The work was begun by German scholars in the early nineteenth century as a result of rising nationalistic feeling, and it gave impetus to similar endeavours by historians in other European countries” (Encyclopaedia Britannica; <https://www.britannica.com/topic/Monumenta-Germaniae-Historica>; accessed 1/2/2019).—*Trans.*

¹⁰ The significance of each of its areas is stipulated for ethnology toward the geographic configuration, and the richer, and accordingly, greater and more abundant the coincidence of various kinds of incentives in a concentrating middle point, the richer and more multiply-formed the psychological life developed. This however forms the authoritative point of view, for while in so-called world history (which from its standpoint will in certain measure also always remain) practical interest in the participant native groups is still a factor. This in itself continues for the majority of native tribes (apart from colonial interests and international connections), and the significance of ethnology as a science lies precisely in the investigation of the psychological growth processes of the society as such and in the thoughts of the people. Numerical figures of the spatial extent therefore have only a relative appraisal of value since the course of development even in the smallest circle, when through favorable circumstances it is precisely the most clearly recognizable, then can also often lay claim to similar importance there in comparison with far more brilliant phenomena (especially when reflected more sharply in the full light of the same paling pre-stage). A colorfully broken terrain, split into a multitude of small groups of peoples, often offers, in differences of local variations, the richest harvest for ethnology, against which greater multitudes of peoples wandering over equally broad expanses quickly, for the most part, show exhaustive uniformity. While again among them, as among larger groups, the results of the climatic environs reveal on average the physical appearance. From all this, anthropology will collect more solidly secure pieces of evidence for the establishment of primary laws than under the introduced conditions of transition already in a state of continuing mixing.

eye will, with some ethnological instruction, at once feel affected by the favorable conditions for development of a rich human experience on this coast cut by bays and points, its straits and sounds with islands of many forms, and on the mainland encounter at the same time four of the greatest families of peoples: the Athapaskan, spread throughout the entire breadth of the continent (through the Dene in the branches of the Tacullies), the Blackfeet coming together with the Flatheads and kindred from the other side of the Rocky Mountains (among the connections stretching from the Mississippi as far the East Coast), as well as contacts with the Shoshones extending farther through the Sonora in the Aztec intermediate stages to Central America, while on the isolated coastal rim through the Cascade Mountains to the Thlinkithen [Tlingit] follow the tribes designated under the name of Haidah or toward Nutka, Puget, and so on, in changing indefinite and uncertain divisions, which no one is able to make better, since up until now it is a matter of almost nothing yet is known. The little that here and there became known out of this region was always striking through curious surprises, in strange incongruence with other regions, so that—with the disconnected abruptness of these notes—a somewhat satisfactory overall view could not have been formed.

Also, this part of the earth belongs geo-

graphically to those that are the last to enter into our knowledge.¹¹ The ships sent out shortly after the conquest of Cortez reached only as far as California, and after Drake's voyage along the coast, called by him New Albion, it would be more than a century, including de Fuca's case, that only sailors' stories were told about the Straits of Anian and so on; even Bering's discoveries (from 1728) were so slow in becoming known in their complete importance that even after Juan Perez's voyage (1774), Cook, upon landing on Cape Flattery (1778), was still permitted the glory of discovery. After agreements with Spain on Nutka by Vancouver, the circumnavigator for whom the island was named, and the passage of Mackenzie over the Rocky Mountains, follows the expedition of Lewis and Clark (1805), which can to a certain extent be viewed as discovery, in ethnological regard, of a new world, with rich multiplicity of the iridescent splitting up of tribes into individual colors, amidst a region most favorable for that, and differentiated into colorful numbers of independent individualizations. From all this we have been able to add nothing, or only the most minimal, to the realm of knowledge, for here it is Oregon, where, although the establishment of Astoria occurred rather without leaving its mark and colonization began only in the 1840s, its rapid swelling with the discovery of gold in California sufficed within scarcely 30 years to wipe out

¹¹ Of the few ships that up to the end of the previous century had generally visited the coast, almost every one is known, and known mostly because nothing substantial became known through them. After Ulloa (1537) followed Alarçon (1540), then Corillo (1542), Valerianos (1552), Juan de Funca [Fuca?] (1596), and Maldonado (1588), Drake (1578) in New Albion, Cavendish (1587), Viscaino (1607) in Monterey, de Fuente or de Fonte (1640) and his publication (1708). Then with Bering (1728) the advance began from the other side, whereupon, after Krupishoff or Krupischew (1732) encountered the "Greenlanders" of the opposite coast, Steller (1741) was able to describe Mt. Saint Elias at the same time as Tschirikow's [Chirikoff] approach, while Dauerkin traveled over the ice and Hedenström searched for the new Siberia. Also Schelikoff's [Shelikoff] company (1785) had royal endorsement (for its settlement on Kodiak Island) as far as Sitka (1804). After Vila (1769), Ayala (with Bodega y Quadra) or Agnilar (1775), Arteaga (1779), the scientific investigation expedition began with Cook (1778), Martinez and Hara (1788), Malespina (1790) and so on. And soon there was also trade out of Canton by Hanna (1785), from Macao by Peters (1786), from Boston by Gray (1788), Meares (1789), Marchand and so on, then Juan Perez (1774), and Lowrie. And Guise from out of Bombay (1786) brought information about the Queen Charlotte Islands, as well as (1787) Portland and Dixon (then Funter, Douglas, Ingraham, Caamano, and so on) who visited the Queen Charlotte coast, as an island, since Colnett and Duncan (1788). This short period of discovery came to an end approximately with Vancouver (1792), since from that time there are scarcely any reports until Mitchell's (1852) visit, stirred up by the California gold rush, the search for coal by Downie, or Poole's expedition for copper mines in Lyndon's edition (1872), as well as Swan's reports from Fort Simpson (on the mainland), and now recently Dawson's geological report.

all trace, so to speak, of the Indians under the mighty blossoming of a foreign culture, before whose powerful force the native “plants of the land” were destroyed¹² or forced in the last vestiges to distant reservations.

With the introduction by Gray (1792) of the mentioned Columbia into geographical knowledge, its river region appears precisely as a soil¹³ on which Indian life¹⁴ appeared more richly developed than anywhere else, where for assessment the numerical alone does not give the decisive weight, and a small population in number of members of small and weak ethnological tribes *a priori* can be little contrasted, just as it would not be admissible for the spatial extent of their region to be used as an effective argument. An object characterized in zoology

or botany marked by specific features can have for physiological observations the same value,¹⁵ whether isolated or occurring in groups. In many cases the study will only have solid ground under its feet with penetration into the details—with local¹⁶ variations under differential comparisons (as among the investigations on the Galapagos and other suitable localities they provided the most recent reform in natural science)—in view of which, at least the “essential” still seems not to have been grasped in generally indefinite features (as about the Oregon Indians, for example, drawn in the textbooks in approximate outlines).

In addition to Vancouver’s coastal voyage, the occasional observations of Puget, Whidley, and Broughton, also to be mentioned would be Franchère, Cox, and de Smet, up to Gibbs,

¹² “In 1810 the first house was built in Oregon by Capt. Winship (Eng),” at that time still without a significant following, for it was not until 1838 that “the first printing press arrived at Oregon (Eng)” and only “1846 the first newspaper was started (Eng).” The newspaper indicates the first heart beat of American life, which thus began in 1846, but by 1869, with the laying of the railroad, a rather clean course had been made of the Indians. Now nothing more remains of it.

¹³ Appropriately named “the garden of the North-west.” None of the famous valleys of the Old or New World, not even that of the Sacramento, San Joaquin, or Santa Clara Valleys of California surpass it, in fertility and salubrity; in beauty of scenery its equal is not to be found anywhere (the Willamette-Valley). “The acknowledged garden of the Pacific coast (Eng).”

¹⁴ The nations, who inhabit this fertile neighborhood are very numerous (Eng), note Lewis and Clark on “Willamette Valley (Eng)” about “the great Multnomah nation (Eng)” (on Wappatoo Island) with the Clackstas, Cathlacumup, Cathlanahquiah, Cathlacomatup, Clannahminamun, Clahnaquah, Towahnahook, Quathlapatle, Shoto, then (at the confluence of the Clackamas) the Clackamos and then there are the Cushook and Chaheowahs as far as the Calahpoewah, “a very numerous people (Eng)” (1806). Since that time: The race of the Chinook is nearly run (Eng) (Swan). The Willopah may be considered as extinct (Eng) (Stevens). And so with all the others: Whole tribes have been found exterminated (Eng) (on the Columbia) and in the few miserable remnants collected on the reservations or straggling about the Oregon towns, no trace is apparent of the independent, easy living bands of the remote past (Eng) (see Bancroft). Their country has been settled by white much more thickly, than regions farther north (Eng) (1875), but since that time the settlements have taken effect as well on Puget Sound, even more the discovery of gold on the Frazer, and especially since the most recent session of the Union, with annual increase in trade everywhere. The Indians of this country are wasting away and that the time is probably not very far remote, when they will be extinct (Eng), was (according to de Harley) to be said of Oregon in the year 1855. Almost everything is now gone, and precisely there where, most of all, far-reaching insights might have been won. There are no two nations in Europe so dissimilar as the tribes to the north and those to the south of the Columbia (Eng) (see Domenech). Sproat divides nations (in Vancouver) “almost as distinct as the nations of Europe (Eng)” (fourfold). What a field for differential comparison has thus been lost here!

¹⁵ The natural scientist presupposes no rank order among the creatures that he observes, all being equally desirable and valuable to him. So also the natural scientist of humanity (see Herder). And practical success is just as likely, whether by microscopic investigations of parasites for agriculture or medicine, or by ethnic studies of thoughts of peoples for the healing of social damages.

¹⁶ In their own homeland “a description of the manners and customs of the Highlanders of Scotland would not be correct of the inhabitants of Kent, nor would it be right to use a description of either as applicable to the people of Great Britain generally. This has, however, often been done with regard to New Guinea (Eng) (Turner), and everywhere in ethnology, in localities, already long since known and nearer (nearest itself). Twenty five different languages are certainly spoken on the 300 miles of coast, extending from Yule Island to China Straits (Eng) (see Lawes). And then in America (by Adams) the number of languages are absolutely and radically different (Fr) (see Vinson).

who besides his own vocabularies gives those of Tolmie and Mengarini, as well as the revision by Gallatin in connection with that material collected by Hale and then Gatschet's still further corresponding contributions.

In Lewis and Clark's enumeration are found:

The Clatsop (in Oregon) reside on the southern side of the bay and along the seacoast, on both sides of Point Adams (Eng); the Killamuck on (Killamuck Bay or) Nielee River neighboring on the Lucton with the same language, as well as the related Lickawis, Youkone, Necketo, Ulseah, Youitt, Shiastuckle, and Killawat. The Cookoosen stretch from the coast to the mountains, neighbor on the Shalalah, and then come the Luckasos, Hannakalal, and so on. North of the Columbia the Chinook come into contact with "the Killaxthokle (on the coast). To these succeed: the Chilts (above Point Lewis), the Clamoitomish, the Potoashees, the Quinults, the Chillates, the Calasthorte, the Quinnechant etc. A particular detail of the characters[,] manners and habits of the tribes, must be left to some future adventurers, who may have more leisure and a better opportunity than we had to accomplish this object. Those who first visit the ground, can only be expected to furnish sketches rude and imperfect (Eng)."

¹⁷ What inestimable value the sole monograph about the necessary destruction of the originality of the tribes of people in Europe with the spread of the Roman Empire has, especially that about the Germans, and this itself only a small fragment. "O, dass wir den Tacitus ganz hätten" [Oh, that we had the entire Tacitus] (calls Herder), and what other views about the past of our part of the earth, if Illyria, Sarmatia, Celtiberia, Lusitania, and so on, still received equal special consideration. Similar losses are in the "immediate vicinity of the borderlands and Pomerania to be lamented, where the names of the land, the individual communities, the families, in addition to much else linguistic and technical, stem from a Slavic speaking tribe, without whose composition, as well as the time of its appearance and disappearance, would be ascertainable. We perceive these deplorable gaps in our ethnological knowledge, evident through the guilt of the south German proselytizers of these tribes, who as early as 1124 left everything found there, as of heathenish nature, equally unnoticed and undescribed and only tried to exterminate it, as later the Spanish all the Moorish customs in another part of the earth" (see Erman). And one "deplorable gap in our ethnographic knowledge," with regard to the total overview of the globe required for inductive treatment, will (by continued indifference toward the remains of the native tribes disappearing in these last hours before our eyes) be "evident through the guilt" of the present perceived by the descendants, that is, primarily ours, if one thinks that "everything essential" is already assured while we are scarcely just beginning to understand what is generally considered essential, and would have only just begun with the truly first collections after the scattering of the hitherto existing theories. If not indeed already to late! In the sagas of Palau (see Semper) "such a treasure of memories lies represented, that a truly precise study of them (no mere interpretation, but genuine explanation through the information of the natives) would deliver us a wealth of the most interesting psychological and mythological materials. Unfortunately not even the slightest prospect of encouraging it is present" (1873).

¹⁸ "For the purpose of the unfolding of the tragedy (Lat)," as for Ekkehard, monk of St. Gallen, his in a fateful time (eleventh century) for the history of the monasteries.

The hope spoken here has not been fulfilled. All the named tribes, and still many others in this connection, belong to an irretrievable past, exterminated from existence on the face of Mother Earth. Their trace is no more, and they allow themselves just as little to be fixed for identification of the individual befitting physiognomies, as the name lists, which are registered by the classic story tellers¹⁷ of the tribes of Thrace and Illyria. While we now have to satisfy and soothe ourselves with that which was granted us, we can not be absolved of accountability in the other case, since the native people are vanishing before our eyes, and even now, day in-day out, each hour, each moment even, ever more rapidly becoming torn away to the place from where no one returns. Whoever begins to sense the tragic¹⁸ of this type of catastrophe will, since language scarcely possesses terms (of equal value with that which is expressed), be far less anxious because of oratorical employment of these than because future mourning over the loss, then recognized as too late, especially if it, as by chance among Polynesians, concerns a highly gifted part of the human family, on a surface of about 70 degrees of latitude, or as here on the Northwest Coast of America, through the geographic configuration a preferably significant

province of ethnography, in a size of 20,000 to 30,000 □ miles or more. And nevertheless, a year more or less, a month perhaps, can, as repeatedly mentioned, in the present crisis, often stipulate the entire difference between existence and non-existence, a rescue of material, for its last remnants at least, or, as already communicated before, a loss of the factors from determination of induction, forever without compensation, with corresponding damage to acquiring results from statistical review.

In the book published (1880) on returning from the last journey there is, in reference to a trip to Oregon,¹⁹ mention of my experiences there, and these, as I can not conceal, since then allowed me no rest, anxious about it, since it may occur with the bordering tribes farther north. They must—after all that is known of them, which is actually all too little—as at all times, directly arouse curiosity, captivate reflective attention, and therefore the blank spot in the ethnological collection at the place they should be represented is perceived as the more painful. That which by chance is found in American museums caused the poverty of the European ones to emerge so much more deplorable, and the sporadic pieces, as they are encountered, came more from the peripheral neighborhood of the region than from the actual heart itself. A basis for excuse lay in the difficult accessibility, which on the other hand to be sure also appeared to promise better protection against premature damage.

¹⁹ Oregon and even more the territory of Washington, but above all the Alaska region purchased only in the year 1867 from Russia served and serves in part even today in Europe as the Ultima Thule of the New World; steam, the great spreader of culture of the nineteenth century, is also likely to further intervene (H. G. Müller). In what at that time was known as the “Far West (Eng),” Nesmith heard (according to his observation in the year 1876) of Oregon (1840) as a “terra Incognita,” somewhere upon the western slope of the continent (Eng). For the rapidity of the change, as here and from other circles on hand at the present time, there is no parallel in world history, and also there can be none such, as from the circumstances proves self-evident. The rapid growth of the colonies of British Columbia and Vancouver’s Island (Eng) (see Barrett-Lenard) “will ever be remarkable” among the achievements of our ages (Eng) (1862). Since Alaska became an American possession, the traceless disappearance of all ethnographic-anthropological phenomena originally there is inevitable (see Erman). In the age of steam and electricity time is reckoned however according to the reduction introduced thereby, so that now in one year is completed what often earlier would have taken a century to complete.

²⁰ On the Frazer River the disintegration began with the official report (1856) on the discovery of gold (1853) and then “the progress of the Gold fever (Eng)” (1858). In Mitchell Harbour the visit of the gold miners was only a temporary one (1862).

Meanwhile in this age of steamships and telegrams nowhere more is to be trusted, and it could be seen that this strip of land after the ceding of the Russians to the Go-ahead Yankee (Eng) would not long remain in an up-till-now isolated untouched state.²⁰ And so it happened. Already as now the news sounds. Tourists flow into there who buy up the last original items of the natives in order to disperse and waste them as “curios (Eng)” before they can be installed in museums as a building stone of future science. The natives, as always, are struck in the moment of contact with civilization by the breath of death, and melt quickly away—their psychological peculiarities in any case, as well as that of native manufacture, which because of items now prepared upon request, begin to become blurred and disintegrated through the foreign ideas and concepts. In this, all new publications sound out in agreement, and so do the answers, that I have received in my private correspondence from the introspection of most respected authorities. Also, here again the being or not being of scientific existence for a part of the human family would be decided within a few years, whose spacious terrain could be estimated at about 9000 □ miles or, in broader connections, at 30,000 □ miles and more.

Such catastrophes may, because of the remoteness and thus far unusual nature of the ethnological observations, initially leave one cold. Later, when these must come to realization as irreparably suffered losses in their complete

obliteration, a tragedy, as already noted, will be portrayed in them that could not be exaggerated since words are too weak to him who considers borrowing expressions in order to accurately express his feelings—the feelings of complete powerlessness to prevent the threatening dangers approaching here.

For those who by management of ethnological museums stand in the middle of the burning questions, who trace this burning almost daily in the most sensitive way from the universally necessary correspondence, the similar sounding reports of lamentation arriving from all parts of the earth, each citation from them of late particularly precipitating aggravation, by comparison with the assessments taken from the records of the “good old” (and in any case less exciting) times (of old ethnology)—for those then who, if in their wanderings over the immensity of the ethnological territories met by the already inestimability of the present tasks, must soon recognize that by the penetration in detail each further step forwards will first only bring further work. For us (few and weak), we who stand prepared for actively practical auxiliary service in ethnology, nothing will be more welcome than cordially granted support by scholarly schoolmates in processing of the materials brought home, but nothing more astonishing at the same time than the view that we already have to know²¹ something before

such material²² can be collected (comic almost, if not disquietingly sad for the momentary delay perhaps, where no moment more in hesitation can be lost, since each counts).

It could sound like ill-humored indignation to wish for the sad renown of a Herostratus²³ and live on in the annals of ethnology if, after years of effort, finally the aroused sympathy is again to be reduced a little: since ethnology is already in the old situation, what more does it need? In any case it will scarcely acquire too much more, since the most has long been destroyed. But in remote parts of the building of the ethnological realm, burning at all four corners, here and there are still valuable treasures, and if out of them the complete life of a people can be drawn, or can be snatched away from the devastating destruction in the last moment, in order for future studies to remain preserved, so the individual trouble will indeed be worth while.

Certainly lavished with ideal wishes, with fine phrases, as here, and indeed on many a printed page already, yet not much has happened in our practical sober world. Without the well-known *nervus rerum* [motive] (Lat), as everyone knows, no hand is stirred, and in spite of the pleasantly begun furtherance of the ethnological museum, with this beginning the same still cannot be provided simultaneously for the totality of the demands from all sides so suddenly and abruptly rushing on.²⁴

²¹ Whoever prefers nourishment from his own brain will have to be satisfied with the role of monophage, for our contemporaries are much too accustomed to substantial fare, to theoretical tumult and other publicly executed sentences, to have much taste.

²² With that kind of utterance right now, where in a couple of years more with opening the new Museum for Ethnology the significance of this science through visual demonstration will be clear to everyone who has the eyes to see—with this utterance now, in the year 1882, the irony in the history of ethnology will be repeated, as it stands registered in astronomy, with the rising of that star on the 1st of January 1801, precisely to the answering of the dissertation *de orbitis planetarum* [orbits of the planets] (Lat), of the inaugural dissertation for the star in the heaven of philosophy. But what does it help? In spite of all appeals to Prof. Titius and so on, at that time in the climax of Schröter's efforts Bode's predictions proved right, and even now we stand there in the middle, in the increase of material (one asteroid after the other). [This probably refers to the work *Dissertatio philosophica de orbitis planetarum* by Georg Wilhelm Friedrich Hegel (1770–1831).—*Trans.*].

²³ One who commits a crime to gain fame. After Heróstratos of Ephesus who set the temple of Diana on fire in order to make his name famous.—*Trans.*

²⁴ With overfilling the spaces, which scarcely permits recording the material, the end of acquisitions would lie near until the completion of the new museum, if every year did not count more or less for the existence of the native tribes.—One might deplore (especially for the volcanic phenomena) the lack of natural science studies (by Veniaminov), notes von Baer, “but one would be wrong” since he “brought information from that which changes the most rapidly” (the

All the more worthy of thanks therefore is the help which was not denied in the need of this stage of transition. The trusted were already known from earlier experiences with the efforts of scientific societies, which may be permitted to praise great-minded men here in Berlin, who to justified complaints (as announced recently in the “Prehistory of Ethnology”) do not close their ears, and so have now come together again in order to complete the limited means of the Ethnological Department with the capital established through private contributions of an Ethnological Committee. In the fall of the last year our agent was sent out, and here you see now before you what, through his stirring activity, was possible for him to save of this people, already standing with one foot in the grave, just before the close²⁵ of the door. The distinctiveness of the type that comes forth does not first need to be accounted for since the first appearance reveals it. A new world of thought lies revealed before us, a new problem, which

indeed has been framed for a long time, and now receives for the first time the first evidence. It concerns, in addition to the tribes from the coast of British Columbia and Vancouver, especially the Haida of the groups of the Queen Charlotte Islands, named by Dixon (1775) and then recognized in their insular character by Duncan, whose villages left, during Lord Dufferin’s trip (see St. John), the impression of a “Niniveh and Babylon,” though also certain is the conviction that the days of this race, favored by nature in many respects, were numbered, the once feared Vikings of those waters who, in the chief city of an English colony, dared to defy²⁶ the governor himself until the summons of a war ship, and the fright of the coast all around because of the slave trade, as before—prominent but at the same time through ingenious skill, for which in northern America no equivalent parallel is offered, either for the mechanical skill (up to the attempted production of a steamship),²⁷ or for stylistic completion in the execution of the

state of affairs of people). “Very soon this will be wiped away. Nature however changes very much more slowly” (1839). The rapid diminution and threatened extinction of the primitive inhabitants of the American continent and the islands of the Pacific is a fact of melancholy interest to the Christian philanthropist and the man of science (Eng) (see Macfie). The entire tribe of Casenove (the chief of the Klackatack tribe) (Eng) perished (in Vancouver) (Eng) (in Wilkes’ time), “they all died within three weeks” (many others have been swept off entirely) (Eng), the dying out of the Willopah, Klatskanai, Staktamish (see Gibbs), the Mamnit near Kaltaminim (in Gardner’s time). The Indians of British Columbia and Vancouver will be numbered by as many dozens, as they counted thousands (Eng) (see Poole) and so on. Jomard (in favor of his ethno-geographic conception) joins with geography, and no longer with archaeology (Fr) (see Hamy), the ethnological collections, which have now produced the “*Dépôt ethnographique*” that was already demanded by Abel-Retnusat in 1831 in the Trocadero, in one of their great tasks, worthy of its surrounding and administration.

²⁵ With Lord Dufferin’s visit to the Haida villages (with “huge carved images, such as those which one associates with Niniveh and Babylon [Eng]”), St. John (1877) “can hardly exaggerate the surprise which was generally felt at this unexpected spectacle” (strange, that a place of so much interest should have escaped notice). The early visits of the Queen Charlotte-Islanders to Victoria gave them a taste for the debauchery of civilization, to which they have yielded themselves unreservedly, and before which they will go down like withered reeds (1877). In a few years little of the original aspect of these villages will remain (Eng) (among the Haida) 1878 (Dawson). Already Swan saw the beginning destruction, and Sproat describes the psychological dying out, even if physically many of the more strongly established tribes should possess sufficient resistance to endure (at least in mixing).

²⁶ Threatened to take the colony (Eng) (see Molyneux). It became necessary to send down to Esquimalt for a detachment of Marines (Eng) (and then for “the steamfrigate Tribune [Eng]”). The Hydahs of Queen Charlotte-Islands are a fierce and turbulent nation and the most feared of any natives on those coasts, they build enormous sea canoes, capable of holding 30–40 warriors, and with their fleets used to sweep all the North Pacific coasts as far as Vancouver’s Island and the posts of the Hudsons Bay Company (Eng) (1879).

²⁷ The Belbellah (in Millbank Sound). In this splendid small steamship in miniature everything (according to an eye-witness) is supposed to have been perfect, except for the steam boiler itself. But, they thought, heartened, that they would also even learn that (with time and patience). The Tshimshian (in former days) have even got up such things as an artificial whale, in some way formed on a canoe. This appeared suddenly on the bay, seemingly swimming along with a little child on its back (Eng) (see Dawson). Then “a peculiar and very ingenious speaking doll” (at Skidegate) (Eng), thus already “l’homme machine” (in beginning of the enlightenment).

objects presented here. At the same time ghostly demon worship is conspicuous, which touches on the polar tribes, connections to secret orders and their mysteries among the Aht,²⁸ and so on, up to the eccentricities of religious madness in the Tyas,²⁹ the priest confederations of human- and dog-eaters, to whom some of the masks presented here belong, the mythical ancestral trees, as is represented in the poles, and so on. Indeed, further comment on that will be held off up until the arrival³⁰ of the remaining boxes, whose dispatch has already been announced. This most worthwhile enrichment of the Royal Collection, as they appear in this dispatch, cannot be valued highly enough. But at the same time one must consider whether similar very high interests may yet remain contingent upon pure accident, or if not rather without any hesitation

the necessary measures may perhaps still be met for that of ethnology, but later no longer (and then never) procurable.

Also, without special direction the attention will already have been directed by itself to a point that again makes evident how quickly with a single deed a decision is often carried out in questions that in endless theoretical discussion often become more en- than un-tangled. You see here with a glance that it is basically less a question about America than about Polynesia, that features from this Oceanic continent lie embedded deep in the borders of the coast³¹ of the mainland, and indeed in precisely such forms as they are to be recognized, after this provisional impression, among these labyrinthian threads, which from Mexican prehistory, as the materials laid down before our eyes in

²⁸ See Jacobsen, J. Adrian. Geheimbünde der Küstenbewohner Nordwest-America's. *Zeitschrift für Ethnologie* 23:385. 1891. [See: Jacobsen, Johan A. 2010. "Secret Societies of the Coastal Inhabitants of Northwest America." *European Journal of American Studies*. <http://ejas.revues.org/7803>.]—*Trans*.

²⁹ See footnote 22.—*Trans*.

³⁰ Up to now three shipments have been added, a new one recently arrived, and the largest on the way. Instead of some pieces of the stock, up until now ours, lost to the oldest of the ethnological museums, we now have hundreds of items (soon as many as 1,000) to survey, and with almost every one of them a long series of new concatenations of ideas is connected, which gradually have to come to a systematic process, one after another (in the next 100 years or so).

³¹ Gibbs offers the signs of distinction of the Tilamuk, as of their Indian neighbors, in connection with the shipwreck handed down in the traditions of the Nehalem, as a Japanese junk (probably a Japanese junk, several of which have from time to time been cast away on the coast [Eng]). Considering the recentness of European notice of the coast of northwestern America, the lists of positive established cases result in an already sizable number (as variously mentioned, compare *Culturländer des Alten Amerika*, Vol. II, p. 446). It seemed as if the parentage of the carving may have been in China (Eng) (St. John) among the Haidah (1876). In attempting from any parts of Polynesia to reach America, a canoe would naturally and almost necessarily be conveyed to the northern extreme of California (Eng) (Pickering) under further research of the currents of the hydrography. A distinct correspondence in style of art is traceable, between the ancient paintings and sculptures of Mexico and Yucatan, and the carved stone pipes of Northwest-America (Eng) (see Pickering). The Tsimshian (unlike Chippewas or Indians of the plains) have more the appearance of the Islanders in the South-Seas, than that which is generally supposed to distinguish the Indians (see Molyneux, St. John). "How like the South-Sea islanders" (Eng.), was the impression made by the Natives north of California on the Brackenridge Expedition to Oregon, and also "Polynesian analogies" (Eng.) (see Pickering). In the language from Mount Elias as far as the Strait of Juan de Fuca one encounters similarity with the Mexican (according to Galatin). Anderson finds in the speech of Nutka Sound "the most obvious agreement (Eng)" (with Mexican). As a concluding result of investigations (Vater's) there seems, "on the still little known Northwest Coast of America, to be before us a central point of at least very many of the peoples of America, from which the population of many lands of this part of the world apparently came" (1810). On the Northwest Coast of America old pictures have been seen, which "recall those great tableaux, those emblematic paintings, those hieroglyphics which served as the written history of the people of Mexico (Fr)" (then multi-storied buildings, temples, sculptures, tombs, and so on). "The transmigration should have begun to operate on the western parts of North America (Fr)" (as far as Mexico) and "as the terror, which went before Cortez, came from the east, drove the Mexicans from the center of the empire toward points around the circumference (Fr), the return migration should have come (by Marchand) (along previously indicated routes), in preservation of speech diversity (among the Tchinkitane, Nutka, on Queen Charlotte Island, and so on). Vater found Mexican connections (as in the speech of the Tlinkits) in the Nutka language, and Humboldt's attention was also directed to it. Indeed, the similarity with the Aztec or Mexican ("in a significant part" the Nutka speech) is a deceptive one (according to Buschmann), although "the attention devoted to it earlier" is "completely justified" in such case.

our museums produce it, wind to the east coast of the continent. The consequent effect of what has now been additionally won will therefore presumably soon throw unforeseen light on much darkness, perhaps as far as the opposite coasts, which are bound fast with definite landing spots to the American ones by the sea currents.

With regard to this, right now, each further word would certainly be too much, for at most (and nothing more about it) may be said—nothing more than that in the things now gone to the museum lie the first building blocks, the first and (it can be said) the only reliable ones, which unfortunately perhaps also (it can be further added) will be the last—but at least now building stones, with which an earnestly intended construction can begin its foundation, a base that previously had to be spread out for

later continued construction. And in coming times they will be more and more honored as inestimably most valuable relics, and the later, the more, because then they belong to a past expired completely to destruction—as last and only witnesses of their bearers which long ago were struck from the book of the living (for their psychological originality, on which spiritual knowledge depends in order to write the story of mankind).

When marking such enhancements of science, the best thanks³² will be the furthering of ethnology set in motion by these shipments, and if the latter (as hopefully soon now) attain their full significance in the exhibition in the new Museum, then the names of those to whom the procuring was owed will be forever retained in the enduring memory in the history

³² Regarding the even more cordial acknowledgment for this first answer to the complaint, “that no hand stirs”: This, a private initiative, is all the more worthy of acknowledgment as such, but that organization which would somehow correspond to the extent of the realm of work present here is still entirely lacking. That in the last ten years it has become continually better, that we ourselves in ethnological and anthropological circles have lived through. However, he who is of the opinion that as a result of this, sufficient has already happened, this person lacks even the weakest glimmer of an idea of what ethnology wants and must want, lacks the concept of the extent of the materials that it needs for the inductive completion for laying only the first foundations. The United States now begins to systematically examine its broad terrain, and in the last five years worthwhile advances have been made in certain areas for ethnology, in the monographs of Matthew, Powers, Gibbs, Powell, and so on. Similarly Australia has, just in the eleventh hour, also fortunately saved much, and perhaps there is hope (according to the most recent reports) that from Brazil, the great imperial state, there may be similar to obtain (as well as in Siberia where the memories of Pallas and Gmelin through Middendorf, Schrenck, and so on were brought to life). But up to now, due to the shortness of time, everything can naturally only be considered a drop in the sea. And so it is with the results of travelers, among whom precisely those with a well earned luster of praise radiating forth could often provide the least for ethnology, since they, as trail-breaking pioneers on the broad plains, who, over a very limited time span, were to wander through, having other more urgent affairs to pursue than studying the particulars of those details, as statistical survey demands. With regard to this, what particularly has happened in Germany is that the best survey is found here in Berlin, in whose museum the collection of objects are deposited from the majority of the voyages of epoch-making type carried out during recent years, and makes every word said about it be demonstrated with dates and numbers, and, in particular, scarcely any of the new procurers has gone out from here without instructions. If it was indeed precisely one of the standard points of view on the establishment of the African Society to likewise help ethnology, and the results, even if (as always at each beginning) not yet entirely as desired, have not been lacking. Comparative summary then must be drawn from such secondary sources, from active colleagues, as well as from theoreticians at home, to whom when the opportunity has been lacking to collect elementary material themselves no reproach can be made, only when the supposition consists of ignorance of the complicated details, to be shoved aside by a couple of unfounded generalizations. How many ground-laying enduring works, which will last as producers of norms in ethnology, are found up to now in the literature? They can almost be counted on the fingers. The most valuable assets are the journals appearing in the disciplines themselves, the *Journal of the As. Soc. Beng.*, the *batavische Tijdschrift*, and several others, because often through acquired experience, detailed over a period of time, exhausting a definitely circumscribed locale (but even here the lists would soon end), then might the scrutinized addressed places be counted on the map at the appropriately marked points with those still remaining blank. How often has it already been said, we stand just on the threshold of the conspicuous investigation of the hoped-for domain, which however prior to the investigation needs the combined details before it can be thought that ethnology is sufficiently equipped to be considered a completed science of man.

of ethnology.³³ Presumably now, through the stimulation provided by our collectors, much of the remainder of the original life of the Indians will come from there to Europe—just as the opening up by *S. M. S. Gazelle*, granted by the admiralty to ethnological interests, of the (until then avoided as dangerous) island groups of New Britain and New Ireland was effected, and the amazing collections (spreading unexpected light over the ethnic circumstances of entire Oceania) that at that time, as generally the first in the museums, reached ours, and the many subsequent ones that have since been received. The more material on hand then, the better for comparative study, although from the first original shipment onward they should be used with increasing caution, since through the trade itself and the imitations resulting thereby they are soon shot through with foreign features that could lead one astray if useful examples for correction are not present. Loud enough is the warning call that seems to forbid every hesitation, and is sufficiently loud from those pieces already here, which already clearly prove

influence, such that the first representation that comes over to us as genuine and true from that human world, already announces its death with its own mouth.³⁴ Fortunately, on the voyage Jacobsen was the right man at the right place. Without stopping in the vicinity of European settlements, where we know of the adulteration of the market from earlier reports, he visited the remote Indian tribes in their villages and entrusted himself to the Haidah and their boats in order to enter the coast of that island which, with the exception of visitors known only in the smallest number in the literature, could almost be considered still virginal (up until now, according to Poole, described by Dawson). Our collector³⁵ has in every way proved good and able, and that this genuine striving leads to success is also shown here.

To do justice to the magnitude of the danger that threatens here is not the work of individuals, since the rescue work must be taken in hand at all points simultaneously, because it is needed at all points.

³³ It gives me special happiness here to be able to add a second acknowledgment to our countryman, Mr. P. Schulz in Portland, who, not only according to the promises given to me on being present there, has enriched the museum with a valuable collection, but also has granted most important support to the expedition sent out through the meritorious deed of the geographic society in Bremen, on my spoken request, in order to ease the investigations of the Mr. Krauses, and the acquired results of these scholars will, with the approaching assimilation of the arriving collections, form a most valuable aid, the coinciding union of which offers here no more favorable circumstances than could be hoped for.

³⁴ The rapid process of dying out in Oregon surpasses everything similar, and although the first colonization began there scarcely earlier than in California, it has indeed already permitted the smallest remains to disappear, such material as Powers at the last minute has pulled from destruction in the neighboring state. In the year 1863 Poole prophesied that 20 to 30 years later, "the Indians of British Columbia and Vancouver will be numbered by as many dozens, as they counted thousands (Eng)" (when he first saw them). In general meanwhile the more favored tribes of the north appear to be capable of maintaining their physical existence, however not their psychological, since this, under the effect of the stronger ideas through contact with civilization, is impossible, and permits tracing the continuance of the process of decomposition in the exposition given through thorough observations of the Aht. Among the Tschimsian at Port Simpson most of the original carved posts have been cut down, as missionary influence was spreading among the people (Eng), Dawson notes in his work on the Queen Charlotte Islands from the year 1878 (which only now, in belated publication, has become accessible in Europe).

³⁵ The present results offer visible proof of what can happen even now in the last moment when it is genuinely a matter of the true end. The inventory of our ethnological museum (which extends farther back than others) of the enormous area which is dealt with here, and is one of the ethnologically most important (from California to Bering Strait), was represented by a few sporadic items. Now, through an energetic attack we are provided with a long and in part methodically pursued range, through the activity of a few months, and just before the close of the door, since various of the pieces are already inscribed with the mark of destruction. Here then it serves as the last rescue from the fire, for otherwise nothing remains, since the other museums, as their catalogues show, could offer no replacement from the present inventory, and only from America could a couple of collections be named (among the prominent, the one from Dall's trip in the Smithsonian Museum in Washington).

Those familiar with the aspects of ethnological survey have for a long time foreseen this catastrophe, its sinister approach, for the signs of the same loomed all too clearly to be misunderstood. Unfortunately, they resounded in vain, as voices in the wilderness, the repeated alarms³⁶ of Jomard, von Baer, Middendorf, and others, at a time³⁷ when rich and abundant harvests could still have been brought home with comparative ease.

My own experiences on these points acquired practically deplorable acknowledgments

when, for the administration of the collections, the task of increasing it from year to year became more difficult even though, with the clearly approaching demand of time, many forms of relief were correspondingly created. With the survey of the voyages undertaken from 1850 to 1880, in approximate intervals of every 10 years, the accumulatively-increased progressions of continuing destruction, with painful surprise especially among the last received impressions,³⁸ in addition to those already familiar from California,³⁹ are added those from Oregon. And it

³⁶ There will come a time, and it is no longer far off, in which of all the wealth of literature that will appear as the most precious, will be that which portrays for us the human conditions before the implanting of general civilization, with impartiality and after long exposure. We still (1839) have very little of this. It appears inconceivable “that governments, academies, and individuals are not more zealous to introduce such circumstances in order to collect richer materials for the history of humanity.” (Ships are sent out at great costs in order to record some unknown coast. That is very praiseworthy—but these coasts, if they otherwise do not concern us more, could just as well be investigated later, for they remain. However, the people who inhabit the coasts will soon be another [people]). In contrast to the circumnavigations of the world (“which everywhere take samples of stone, dried plants, and animal skins”) “how small the basic investigation they dedicate to the first steps of the development of humanity” (von Baer). Even in a region lying next to the study site it must be all too astonishingly noted that up to that point we knew the mice better there than the people who inhabited it (see Middendorf). And [this is not to mention] the Samoyed still farther through broad Siberia.

³⁷ When (as soon) the entire world will be traversed by steam and other mechanical crafts (as “a large workhouse”), “then one must look around with diligence for a true and genuine portrayal of the comfortable lightheartedness of the so-called savages, as our philologists and historians now do to examine how an Athenian or Roman citizen rose from his bed, ate and drank, carried out his domestic and public affairs of the day, and through this view would acquire more than through a dozen descriptions of battles and spectacular festivals” (“neither the spectacles needed up to the end of the previous century, which found in non-civilized peoples only decadence,” nor “the mode of glasses developed during the French revolution, which found man only in natural circumstances worthy of attention,” suffices). With the insight “that the realization of the human condition is just as worthy as useful a task of investigation,” science has to store up in its supply rooms that of which inevitable necessity more and more deprives it” (von Baer). Upon intercourse with civilized men (Eng) (Sproat) “the unfortunate savage becomes more, than ever, a creature of instinct, and approaches the condition of an animal (Eng)” (among the Aht).

³⁸ “Since the grandfathers of these deteriorated original inhabitants of Oregon came into closer contact with whites for the first time at the mouth of the Columbia, at Astoria, which was founded by our great countryman John Jacob Astor in the year 1811, scarcely two generations have passed, and after but two generations not another person will still remember the name of the tribe which once on the Columbia numbered its warriors in the thousands, while of Astor many a stone testimony will speak in New York, and his name will still be remembered on the Pacific for centuries” (thus the expression of recent times in the gold country of California). *Vae Victis* [woe to the conquered] (Lat), according to the passage of history, which has to take its course, but all the more reminiscent of science, at least here once, for the study of human development to establish in advance the early stages through which one could remember the people of history everywhere when they were already overpowered, and thus perished, therewith lost. In Oregon the “Pioneer Association (Eng)” has been formed (1872), for “facts relating to the Pioneers and history of the Territory of Oregon (Eng)” (in the year 1848, then 1859 “admitted into the Union as a sovereign State [Eng]”), but without a connecting member with Indian antiquity, or at most through Mr. Mc. Kinzie (one of the Astor partner’s), who, with so much pomp, took for his wife the Princess Chowwa, daughter of old king Comcomby, the celebrated Chinook chief (Eng) (see Rees).

³⁹ Still at the last minute Powers has sent us a small treasure of the last relics from prehistoric California, one of those valuable and fruitful gifts which, from governmental recognition of the Union, in most recent times have come especially to ethnological good and all the more thoroughly cause the realization of the wish that the last moments, which for ethnological collections could still be granted, might be utilized as much as possible. To Swan’s valuable monograph

was those from there as the last brought which caused me to think, as already mentioned (in this case acquired by chance), above all of the Haidah. But many another expedition should still be sent out, and without loss of time. It is a question here of the old or of the new ethnology, whether we, as up to now, want to be satisfied with those supplements to geographical and historical disciplines, which one is accustomed to designate as ethnographic, or whether we will be permitted to dare to think of the goal, shining at all times out in the distance, of the science of man. If, to the measure of mankind in all of its changes on the surface of the earth, with the materials delivered from ethnology, it should come to completion through comparative psychology, then this, in accordance with the anthropological-natural science flow of time, can only happen in the inductive way, only through induction.

And with this the fissure has entered that which from now on separates the earlier ethnology and must separate it from the one aspired for, a fissure just as formidable as that which physiology led into the inductive field, then suddenly radically separated from the older, as the summons is plainly represented “with the revolution in German medicine” (1841), “that it is broken with the current idea, and through another, a method joined by physiology, a purified base for experience, must be won,” and, “understood or not understood, the feeling is spread that one has entered into a new time”—at that time physiology for medicine, now ethnology on a psychological base. “As chemistry from alchemy, from the art of the

apothecary, has had to hoist itself, so also the same has been ordered of anthropology” (Nasse) and with “total transformation” (Wunderlich) “the entire terminology of science is formed anew” (in biology), so that it may be the same for ethnologists who follow the natural science direction of anthropological studies hesitatingly as for the “doctors of the old school” who were often no longer capable, “even with all insight, with all knowledge, and with the best will, to find their way in the new direction.” For them, it was just as difficult “to learn to think anatomically” as for many ethnologists to become familiar with the ideas of peoples.

Ethnology can say of the decade since 1870 what physiology said 20 to 30 years earlier:

“These ten years form an epoch in our science as scarcely another discipline has to show in retrospect on history. Physiology during this space of time brings before us a series of such powerful and pressing transformations, such brilliant new discoveries and doctrines, so decisive a victory over regressive false doctrines and institutions of antiquity, that we may stand in awe with proud veneration of the author of the new physiognomy, the new spirit of our present-day science, without doing wrong to the authorities of the earlier classic foundation which we owe to them” (1854). And still there is “no halt, no remission, still physiology drives restlessly forward” (Funke).

Indeed, restlessly forward, above all in ethnology, with the still completely inconceivable extent of its work, for which we scarcely begin to bring the first materials⁴⁰ together—restlessly forward above all, and especially with the brief-

on Cape Flattery (and the Makah), and, to name all beforehand, Sproat’s Vancouver [Island], now Dawson’s report as the first information on the Queen Charlotte Islands has come out in the literature (apart from Poole’s stories about the copper mines), as the first, and until then the only, in the year 1880.

⁴⁰ The collected work of Rudolphi and of Burdach had to precede before Joh. Müller’s *Handbuch der Physiologie* could mark the “beginning of a new epoch in German physiology.” The first requirement (in pragmatic psychology) “is the acquisition of the facts, which in every case must form the most profound foundation that can be replaced by nothing” (Beneke). This prerequisite, already recognized by individual psychology, will now, with the exit of the *zoon politikon* [the political animal] (Gr), be able to be fulfilled through ethnology, in the concept of society. “It was necessary that one became acquainted with a large number of individual illnesses before one could make a clear definition of the nature of illness” (with the diagnosing of an illness like the recognition of a plant type), in the progress of a special pathology to a general pathology, while earlier a speculative system was set up over the last, “which self-evidently had to collapse, as soon as the special pathology received an entirely altered appearance in the

light of the new facts" (Cohnheim). That, as far as an inductive construction is intended, the ethnic psychology also has for the present compiled its materials in solid building stones, which it, if tired of wandering in airy chimeras, must first of all collect and store—that then should indeed permit claiming from the facts themselves some excuse for such books which, with striving to justify such a goal, must bring to view the technical difficulties, which, with the production [of such goal], must be struggled with. Furthermore, it would be desirable if those who are constantly injured by the prodding of vexing books, instead of repetition of the somewhat monotonous complaints thereover, wanted to give practical advice for improvement, each guideline from them would be most thankfully accepted. The fact is that induction is also used in psychology, on the basis of the ethnological facts, in the progress implied in the progress of the natural sciences, and the prior accumulation of material constitutes an indispensable precondition for the statistical overview, must be presupposed as a question of principle. If this is the case, how must we proceed? Ethnology, with the mark of a future science, was born only in our generation, and we are thus still standing in the middle of the process of development, and (as with every development) in successively continual accumulation. Consequently, as lies expressed in the word itself, the accumulation of the material could also be only a successive one, and what, since it began in the '50s, should it have given (which since then arrives in such quantity and is still added to annually)—those who interpret the riddle of the sphinx or are otherwise gifted with the keenness of intellect of an Oedipus may better know. In my head nothing wants to allow itself to be resolved, especially since to German scholars the means are usually not at our disposal, as often in England. When Herbart Spencer, about 10 years later (1867), recognized the same necessity and sought to cope with the urgent task of time through comprehensive organization, it cost him, as he unfortunately had to explain with the business of his plans, a loss of 3000 £ (60,000 *M*). "In going through his accounts, Mr. Spencer finds that during the fourteen years, which have elapsed since the undertaking was commenced, the payments to compilers, added to the costs of printed, have amounted to £ 4425, 15 s., and while up to the present time the returns (including those from America) have been £ 1054, 12 s., 1 d.—returns which, when they have been increased by the amount derived from the first sales of the part now issued will leave a deficit of about £ 3250 (Eng)" (1881). Since similar means, and at the same time sufficient time, are not at my disposal, I will thus have to persevere for my part in the way undertaken for the common goal, with the hope, that through the production of a general index the use of books published (1859–1881) one after another (the lack of which in the meantime is regretted most by me) can then be made handier and more accessible. Whoever meanwhile understands in some measure how to read between the lines will find much already complete, more firmly established from the uninfluenced facts of the circumstances, than through subjectively acquired opinions. Only in such consequences of natural affinity does the control for assured results follow. The difficulties, grasped in the new interpretation of ethnology, lie especially in the fact that those experienced earlier, than (as all sciences) the natural sciences, a deductive treatment, as a result particular ease and enticement offered a generally more interesting light reading. Although it may be more appealing to read Schulze's enchanted rose or other ingenious flower poems, the botanist directed to scientific study will in fact have to work through thick volumes of the systematizers (as Dietrich, Hill, Buchoz, and so on), and as little accustomed as he is to be frightened away by their tediousness, so little as well in ethnology, no longer only a sideline but advanced to a study of life, when, by present unaccustomedness, endlessly appearing collected works cannot be spared. Now there is not a *ὁδὸς βασιλική* [royal road] in induction, rather only sour work, which is sweetened in the hopes of success. As botany before C. Gessner was viewed only from a medical point of view, such is ethnology preponderantly in its connections to geography and history, but although these still remain important for it, as the medical in botany, it will indeed at the same time have been constructed as an independent science, when generally striving for the character of one such, and when we make comparisons at the time of Meiner with the history of botany found in the epoch of Brunfels († 1534), a time we still have not reached sufficient clarification for the establishment of a system like that of Linnaeus (1732). Only at that time however physiological work could begin, with Treviranus, Rudolphi, and so on, until microscopic physiology. As in botany the flora had to precede, Germany's (through Roth, Schrader), Switzerland (through Haller), the Netherlands (through Kops), France (through Bulliard), England (through Smith, Curtis, and so on), so monographs in ethnology [must precede], and as *Herberia viva*, in the botanical gardens of Padua (1533), Bologna (1547), Zurich (1566), Montpellier (1568), Leiden (1577), Kew (1612), Paris (1626), Jena (1631), Berlin (1715), and so on were needed, so the museums in advance for ethnology. That which in related sciences demands centuries to mature is just as unlikely to be produced momentarily by magic in ethnology, and if in our more hastily accelerated present it should go more rapidly, the first decades are indeed still all too short for imperfection not to be permitted to claim leniency. Only after prior vigorous scientific development of botany could the practical aids be granted as assured, for which medicine or agriculture now have to thank it, and so at some time ethnology will intervene in most significant manner in the social life of the people toward the inductive construction of people's thoughts, but certainly only after the same natural development, in natural science fashion. And since the seduction to hasty immature hypotheses lies all too near, which would bring everything into

ness of the work time yet remaining to us. Each minute, each moment is valuable, and therefore not another can be spared for polemics with attacks on the questions of principles, which must be decided in the history of the science according to the organic development of the same, and in the interim already actually have decided, so that now no further counsel is needed.

It could be said that never in cultural history has a more critical crisis occurred than at this moment, since a decision must come within a few years, whether it will be possible for a future study of people, according to the principles of induction, which have the requisite materials, or whether by chance it is to be forever abandoned. Meanwhile the statistical survey of mankind in his distribution through the geographic provinces, especially the best part, the psychological half, will have to be determined, so that among the non-literate peoples, with the often discussed difficulties of objectively pure classification,⁴¹ there needs first of all to

be at least those embodiments of thought that can be localized in the ethnological museum. Whether according to the aesthetic canon they are ugly or beautiful of course comes just as little scientifically into question as in general the crookedness of a beetle's legs for the entomologist, which he nevertheless preserves and cares for with equal love in his collections. His [the beetle's] claim on the little place due him is one claimed in the interest of science, when he also might appear as a caricature⁴² in comparison with stags and proud stallions, as many a poor aboriginal tribe with old cultures in the light of our beaming civilization. Since such a powerful ray of light must at once necessarily decompose or in its peculiarity destroy everything it falls on that is deeper and lower, an impression of the latter should be deposited with its characteristic forms in a secure depository, before it expires for eternity into nonexistence, in order to provide in the course of studies approaching later generations that reflection in which the

the most dangerous confusion on this slippery field of speculation, the curbing by sobering self-control of hotheads and hotspurs remains to be desired, with reference to the toilsomely long way that is still before us. Until recently we knew in the museums from American antiquity only Mexico and Peru. Now divisions, known until the present only literarily, have also been added for the first time for the typical styles, chronologically after the Toltecs, Chichimecs, Aztecs, spatially toward the Totonacs, Zapotecs, Tarascos, and so on, the Quichés with substrate in Guatemala, Mayas in Honduras, Nicaragua, and so on (in long lists of tribes), as in South America not only the differentiations of the cultural centers of the Chimus, Cunchucos (and related ones in Huaraz), of the Huancas, Aymaras, etc., absorbed at the time of the conquest in the Inca realm, but, near the Scyri of Quito, the uninfluenced retained culture of the Chibchas, and that which refers from special finds to names in the Caucathal retained here and there. Each one of these tasks will have to undergo in the course of time the same painfully careful handling we are accustomed to in the classic studies, and as far as these themselves still removed from completely solid clues regarding the Rhodian or Cyprian problems in Greece, or in Italy for concrete signs of separation between what would typically be considered the Samnites, or the Volsker, Sabines, Etruscans with predecessors, and so on. *Ars longa, vita brevis* [art is long, life short] (Lat), but undiscouraged at least a beginning must be made. All such questions in the ethnological museums, since the preparations for systematic plans abruptly and suddenly emerged only in the last couple of years, and when those standing farther away, who have no knowledge of that type of detail (and could not have, because it has only recently become known), nevertheless are permitted an adverse judgement, cynics could think the intent hidden therein was to want to annul the approaching workload, although that, after the pieces in the cabinets of the Royal Museum are once recorded, indeed cannot be permitted to prove practicable.

⁴¹ See *Holy Myths of the Polynesians* p. 13 on the difficulties that are opposed to a deeper understanding (and especially for Polynesia where one was most referred to superficial folk gossip in the attempt to produce a mythology) and similar observations by Gibbs. On the externals of savage life on the Oregon coast, there are many graphic and full accounts, but insight into their minds is not so easy to reach, and those who have most carefully sought it, are likely to be most doubtful of their success (Eng) (Gibbs). Concerning the religious performances (of the Haidah) "it is difficult to ascertain exactly what they are, owing to the reticence observed by natives in speaking to whites of those of their customs or beliefs, which they fear may be ridiculed (Eng)" (Dawson). And so everywhere.

⁴² The prototype of humanity lies not in one nation of our region; it is the candid comprehension of all exemplars of human nature in both hemispheres (see Herder).

human spirit is manifested as the original for the respective geographic province⁴³ before it is absorbed into the current of our own generation, which in a short time will have overflowed the entire globe. So long (but only so long) will the material⁴⁴ be offered for comparative treatment, for those comparisons that provide the inductive method with its fundamental stage for the construction of a system in itself controlled and controllable. One stipulates the other, and if the evidence disappears in the creation of the primary materials, so with that the hopes would again be sacrificed, as they have become awakened to the view of scientific education in psychology.

That we know absolutely nothing of ethnology in advance, and are not able or permitted to know in the sense of scientific induction, this indeed is taught by a single glance at the map and the weighing of ethnological dilettantism against classic learning (which here is to be pursued as an illuminating example) with its millennia of research work,⁴⁵ compared with the decade in ethnology. He who already supposes to know has with the formulation of such wisdom the choice of writing his name on such a list as one may find occasion now and then to create from those [names] of *virorum obscurorum* [of obscure men] (Lat) of the respective century

—such namely, to whom the half darkness until now was indigenous, although the light of new epochs had already begun to dawn. Over that kind of anachronistic protest then history (which here could reasonably still be considered a world history) is freely accustomed to yield to the order of the day, and loud enough, even for deaf ears (one should think), is the answer given in the formation of anthropological societies (since the first in the year 1869), 22 in number. For ethnology, which until then was scarcely considered in chambers of curiosities,⁴⁶ now proud splendid buildings begin to be raised in [the form of] the museums dedicated to it. We might wish, in order to fill them, that many collections may yet follow as rich and valuable as the one presented here [to the museum].

Not everyone would be permitted to dare, as Aeschylus (at Chamäleon), to dedicate his work to Kronos (*Krononoo* or *chronoo*), for forcefully the stream of time rushes on, swollen by new ideas and, carried away, becomes unseen, even much of the already fixated, when not gigantically towering up in the isolated hero figures of spiritual history. Only the tangible material may remain as decisive building stone when joined to the correct place, against which the pretentious theories⁴⁷ (older ethnology and other *logoi*) be fizzled out in mere air, like the *Astralgeist* [star

⁴³ The hypothesis, which is offered in explanation of facts, must of course be considered a mere speculation (Eng) (up to the winning of more secure materials), notes Hales (over the distribution of Indian tribes on the Northwest Coast of America). To the Kygahni (Haidah) Dall refers “with doubt” (only provisionally), and “the Nasses and adjacent Chimsyan and other tribes are in so much confusion, from an ethnological point of view (Eng),” that they are better off to remain aside (1877).

⁴⁴ Each nation must be observed alone in its place, with everything that it is and has (see Herder). “Our European culture cannot be the measurement” in those theoretical discussions by which one has wanted to put in order the primitive religious representations (even before the material for objective understanding was present). Each people has its peculiar motion, but the continuation of all peoples lies under certain general laws of development (see Rossbach), and it serves best to study these by comparative survey first of all (in the organic growth of the thought of nations).

⁴⁵ In such cases each must know what he has to do, and he who writes his own conclusion is exempt from the often painful duty of expressing it, especially so when, by saving the polemics, at the same time the saving valuable time-loss can be recognized in the avoidance of personal encounter.

⁴⁶ As thaumatanthropology, *vera pariter atque ficta tractatus historico-physicus* [a historic-scientific tract is truth and fiction in equal parts] (Lat) (Calovius) had more charm (in the seventeenth century) than dry science, so it needed a transition stage in the cabinet of curiosities, with gradual accumulation of material in order to lead to the possibility of ethnological collections (and then museums).

⁴⁷ In ethnology at present all the less can be directly taught because we have not once thought to try to understand the thought process of the native tribes, and therefore have patched up from their mythology only wonderfully monstrous bugbears which, seen under the glasses of cultured people, appear so much more grotesque.

and wind spirit] (by Paracelsus), or somewhat like astrology before astronomy⁴⁸ radiating here in the luster of the heaven of fixed stars (also alchemy before inductively clarified chemistry, stabilized in its elements, like the future science of people once in primary thoughts).

Indeed, enough words. Let us be glad first of all about what has actually been obtained, as it lies before us, fruitfully swelling to give information. With regard to the religious ideas, which are basic here, so they may not, as up to now too often with the native people, under the prism of our conceptions of culture be distorted, rather are to be observed like the peculiarity of the land of origin on which they grew, since the leading thought process can be pondered and correctly thought out only when we have uninfluencedly tried to understand it ourselves. In addition to the mystery cults already mentioned above and sacramental meals, now a number of actual clarifications are granted,

even about, regarding the “light swallowing” of the Aleuts, the subsequent sun impregnation of the priest-prince tarrying in loneliness with regard to the rejuvenation of nature, about the snatching and patching of the soul and all the most colorfully emerging games of the masks in colorfully confused crossings [probably meaning both intersections and hybridizations] of the demonic world, in the battle or atonement.

In the remarkable eye (everywhere in the style of decorations) lies, as in “the eye of Osiris or symbolic eye (Eng) (*uta*),” the thousand-fold guarding represented by an Argus, against the “evil eye (Eng)” (of the Scots) or the evil look, against which (as *mal occhio* [evil eye] (It) in Italian) the Spanish are protected by the *higa* [method of pointing derisively at someone] (Sp), and others through other signs (of the phallic type and otherwise).

All this will find broader treatment in a publication prepared with illustrations.

⁴⁸ On Bode’s suggestion 24 astronomers had bound themselves under Schröter’s chairmanship, to seek the new star of promise—but the same was neither required nor expected, as the philosopher clarified *ex cathedra* [from the chair] (Lat). Whether meanwhile desirable or not, on the first day of the new year it climbed above the horizon. So also appearing to our good considerations, which are ready now with their theories, is new material, which, because it becomes disturbing through new reworking, seems uncomfortable and better rejected. Such wishes will clearly not help much now, for here that organic development reigns in the historical process that in any case would have remained to be decreed. It will preserve its own right, even without counsel, the office of which there is scarcely an individual worthy of holding.

Newest Release!

EXCAVATIONS AT GIVENS HOT SPRINGS

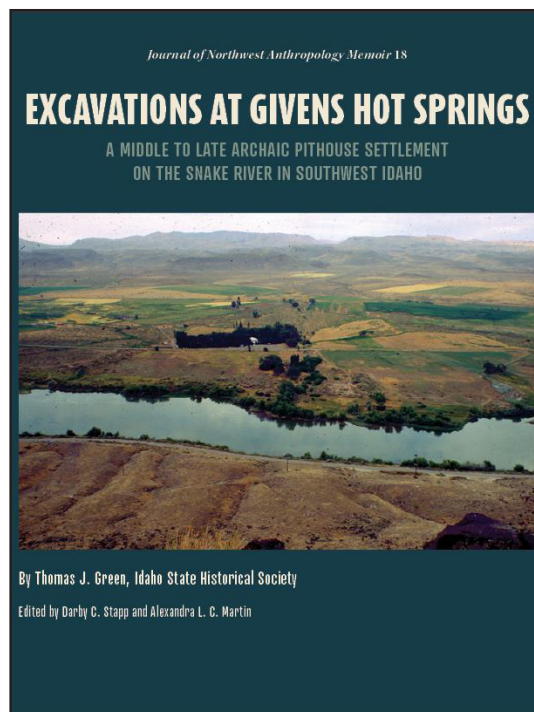
A MIDDLE TO LATE ARCHAIC PITHOUSE SETTLEMENT ON THE SNAKE RIVER IN SOUTHWEST IDAHO

By Thomas J. Green

Edited by Darby C. Stapp and Alexandra L. C. Martin

This monograph reports on four seasons of archaeological excavation at three separate localities at Givens Hot Springs. Givens Hot Springs is located on the south bank of the Snake River in Owyhee County in southwest Idaho between the modern towns of Murphy and Marsing. Map Rock, one of Idaho's most famous petroglyphs, is located directly across the Snake River from Givens. The area was also a preferred camping spot for emigrants traveling the southern route of the Oregon Trail.

The excavations at Givens were an outgrowth of a project started in 1975 by Dr. Peter Schmidt, the first Idaho State Archaeologist. In conjunction with the Great Basin Chapter of the Idaho Archaeological Society, Schmidt began a project to record archaeological sites in western Owyhee County and to document collections from the area. The initial goal of the project was to gather general information so that detailed archaeological projects could be planned. The project continued under Thomas J. Green's supervision, as the second Idaho State Archaeologist, after Schmidt left Idaho in 1976 to conduct field work in East Africa. The formal sponsor of the project was the Idaho State Historical Society.



Journal of Northwest Anthropology Memoir 18

Between 1975 and 1978 a number of sites and collections were recorded. Everett Clark, member of the Idaho Archaeological Society, former stockman, and a local public official in Owyhee County, reported the owners of Givens Hot Springs planned to subdivide the land and develop it. Knowing the importance of the sites around the springs, Mr. Clark was concerned that important information would be lost if they were destroyed. For these reasons, further survey and testing in the Owyhee Mountains was abandoned and plans were made to work at Givens.

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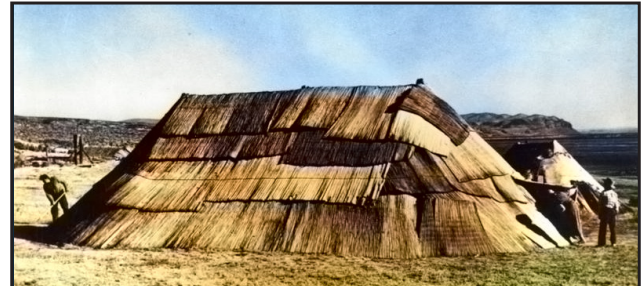
OF HOUSEPITS AND HOMES

Twenty-First Century Perspectives on Houses and Settlements in the Columbia-Fraser Plateau

By Molly Carney, James W. Brown, and Dakota E. Wallen
Edited by Darby C. Stapp and Alexandra Martin

What does “home” mean? Cultural connotations of home, habitation, and residence vary but usually encompass a physical place where individuals live, with accompanying notions of comfort, security, and attachment to place. This concept is wide; for many, the idea of home operates at multiple scales and could refer to a physical house, a particular piece of land, drainage or valley, topographic landmarks, or even a specific room or landscape feature. What constitutes home might not even be a set physical space but could shift throughout the year or an individual’s lifetime. Given this variability, there is one constant: home is usually the place that is returned to, and the place that is the center of daily life for an individual or group. For this reason, archaeologists have long sought these loci as a means of understanding the economic, adaptive, social, and ritual elements of past human lifeways.

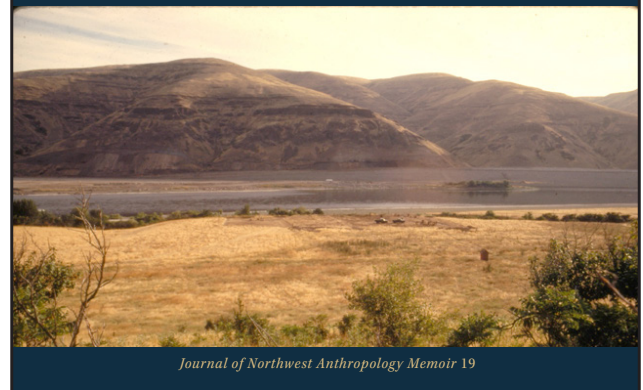
This volume focuses on archaeological houses, features, and places which may have constituted “home” to the inhabitants of the Columbia-Fraser Plateau. While boundaries of this cultural and physiographic area vary according to author, it generally encompasses the area drained by the Fraser and Columbia rivers and their tributaries. The Cascade Range bounds this region to the west, the Blue Mountains and central Idaho ranges to the south, and to the north and the east by the Rocky Mountain range. The Canadian Plateau consists of the Fraser, Thompson, and Okanagan drainages and adjacent mountains and features north-south trending narrow valleys and steep mountains. To the south, much of the interior Columbia Plateau consists of rolling hills, broad plains, buttes, mesas, and deeply dissected canyons. This region is home to diverse groups of people who have lived here since time immemorial, and who continue to live, use, and create spaces and places today. This region is variously referred to as the intermontane west, the interior northwest, or even simply the Plateau. We rely on the terms Columbia-Fraser Plateau or simply Plateau to refer to the general region, acknowledging that the area spans modern political borders between Canada and the United States. We further acknowledge that past peoples ascribed to their own spatial conceptions and that modern political boundaries between the United States and Canada did not exist until 1818 and were not established nor enforced for many decades after that date.



OF HOUSEPITS AND HOMES

Twenty-First Century Perspectives on Houses and Settlements in the Columbia-Fraser Plateau

Molly Carney, James W. Brown, and Dakota E. Wallen
Edited by Darby C. Stapp and Alexandra Martin



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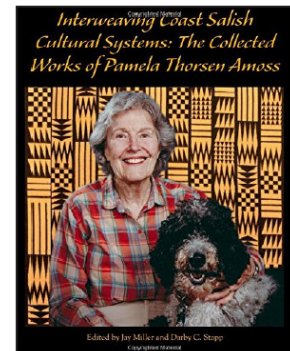
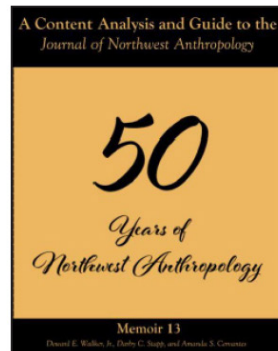
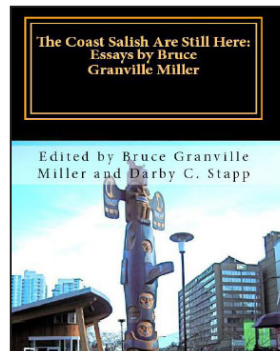
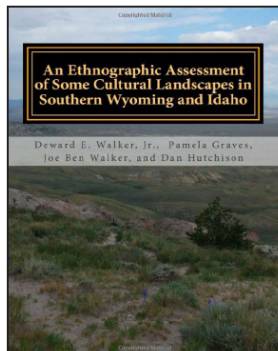
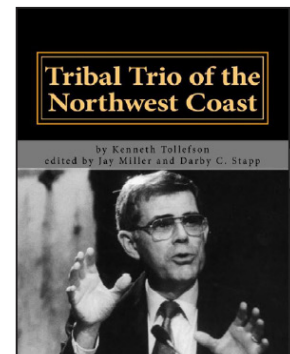
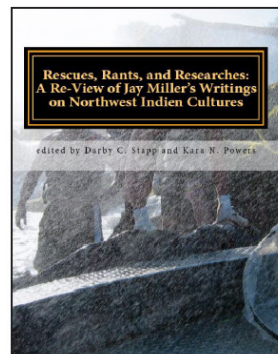
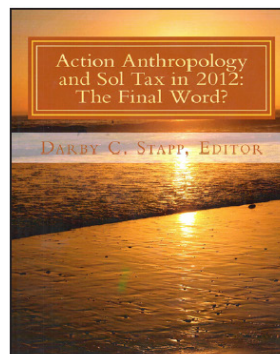
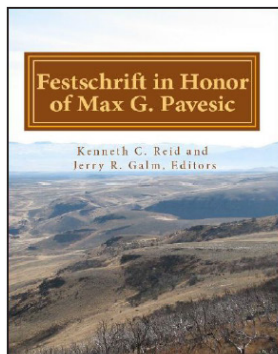
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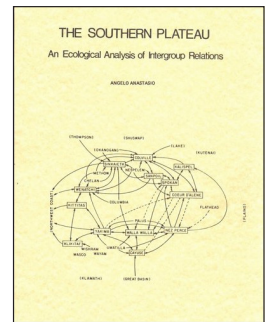
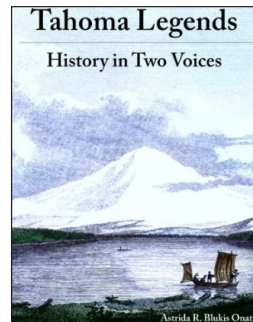
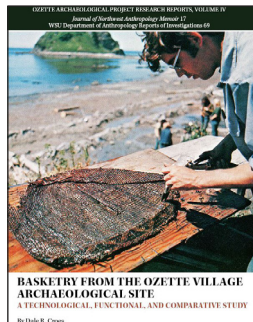
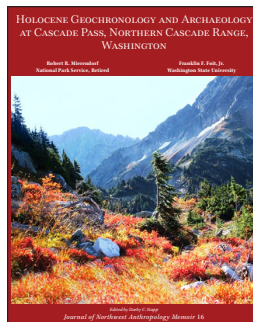
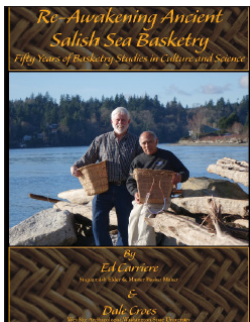
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