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WAYS OF KNOWING:
EPISTEMOLOGY, ONTOLOGY, AND COMMUNITY AMONG ECOLOGISTS,
BIOLOGISTS AND FIRST NATIONS CLAM DIGGERS

By

CHANTELLE MARLOR

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ABSTRACT OF THE DISSERTATION

Ways of Knowing:

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Nations Clam Diggers

By CHANTELLE MARLOR

Dissertation Director: Dr. John L. Martin

This dissertation is an exploration of the various ways in which knowledge practitioners come to know about a subject. Using four case studies of marine experts—government-based invertebrate biologists, a university-based team of contaminant ecologists, Kwakiutl (or Kwakwaka'wakw) First Nations (Native American) clam diggers, and Nuuchah Nulth First Nations clam diggers—I explore the processes and practices by which these practitioners produced knowledge about clams. The case studies are based on ethnographic research I conducted between 2003 and 2005. Drawing on tenets espoused by the Strong Programme in the Sociology of Science, I use a balanced (symmetrical) framework to compare the 4 sets of knowledge practitioners' social relations with their peers, the signs they use as evidence, the methods by which they order and summarize observations, their relationship to what they come to know, their interests, and the assumptions they make when drawing inferences. My theoretical arguments build on literature drawn from a wide spectrum including works from the sociology of science, sociology of culture and cognition, cognitive anthropology, cognitive psychology, and human ecology. Themes running throughout the dissertation include standardization, precision, the situated body and cognition, community, temporality, and multiplicity.

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Chapter 1: On Ways Of Knowing: Comparing Scientists' and First Nations' Understanding of Clams

On Ways of Knowing

This dissertation is about ways in which knowledge practitioners come to know about the empirical world. Specifically, it is about some of the ways by which four sets of practitioners came to know about the same subject matter: clams. I start exploring this topic by describing the general process by which each set of practitioners came to know about clams. As my title suggests, though, my purpose encompasses more than this. In describing the thoughts and practices of these practitioners I explore how their knowing emerged in a *variety* of ways: through bodies and minds, through practices and conventions, through trust and sharing, through coordination of actions and thoughts, through observations and measurements, through the reduction and organization of variations within observations and measurements, through synthesis and comparison. In fact, each practitioner employed a variety of ways of knowing in their process of coming to know.¹ As such, this work represents not so much comparative ethnographies of four groups but an ethnography of *ways of knowing*.²

As the above paragraph suggests, this dissertation is about multiple ways of knowing; the phrase “ways of knowing” is intentionally pluralized. Consequently, I

¹ The ways I have identified are not the only ways by which knowledge practitioners come to know about the empirical world—there are undoubtedly many more. When choosing to title my dissertation “Ways of knowing” I decided to do a quick title search to see what books had previously been published under this title. In doing so I was struck by the wide variety of “ways” in which the authors have described coming to know: metaphorically, musically, scientifically, through literature, art, drama, and stories, as a Mayan, a woman, a designer, a Blackfoot, a social scientist, or a Dene Tha, to name a few.

² In making this claim I borrow phrasing from Annemarie Mol’s book *The Body Multiple: Ontology in Medical Practice* (2002).

spend time (space?) in this introduction exploring the phrase “ways of knowing” so as to orient the reader to the many ways in which I intend the phrase.

I begin with a story. When I told Dean what I would be doing for my new job as a research assistant on an ecology research project—collecting clam samples for analysis in the lab—the only question the long-time Kwakwaka’wakw³ clam digger asked me was why my boss was not doing it himself. He said, “If he wants to learn about clams he should go out and get them himself. He’s cheating if you do it for him.” I tried to explain that science was not like that—that scientists frequently hire assistants to help them collect their data—but he would have nothing to do with the idea. “Then they are all cheating,” was his response. Cheating, he said, just like someone in elementary school who gets another student to complete his homework for him. How could they learn anything without doing their own work?

Initially I was perplexed by (what I considered) this clam digger’s inability to appreciate how science was done. It was only after working with the Kwakwaka’wakw diggers for a longer period that I came to see the interaction in a new light; to see Dean’s objections as an epistemological stance as opposed to lack of understanding. For Dean, the scientist can not possibly know about clams if he has not *gone* to their natural habitat to collect them himself; to know something requires having studied or experienced it in a particular way, namely being in the context of where clams live. The biologist, in contrast, felt he did not need to experience the context from which the clams were collected as the most important information was to be obtained from analysis of the clams in the laboratory. The only information he absolutely required in terms of context was

³ A First Nations tribe situated on and near the north end of Vancouver Island. Also referred to as the Kwakiutl.

the beach from which each clam originated, a task he had entrusted to his research assistants who were instructed to record this information.

This short story illustrates at least two ways of knowing. The ways of knowing most clearly illustrated are epistemological in nature. Epistemology is about ways in which people come to know; it is the set of standards one employs in the creation of knowledge so as to be confident that one actually knows something (Landesman 1997). For the Kwakwaka'wakw digger in this story, learning was something that one gained from direct interaction and first hand experience with the phenomenon in question. This stands at odds with the approach taken by many scientists, including the ones with whom I worked, where the task of “data collection” is often left to fellow team members and/or research assistants.

Such an authoritarian system of knowledge relations is highly familiar to scientists; as students of science, they worked with books and under the authority of teachers likely for years before being taken to a beach, forest or other ecological terrain to directly interact with nature-in-context as opposed to truncated nature (to which students are exposed in the classroom and laboratory). Whether or not one is required to have first hand experience of a phenomenon in order to know something, and at what point one must begin accumulating this experience represent diverging epistemological standards employed by the clam digger and ecologist. Each standard represents one way by which these practitioners came to know about their empirical subject.

I have now identified two ways of knowing. They are ways that differ in terms of epistemology by practitioner. But these were just two particular epistemological standards they employed. Each also employed a large number of additional standards

addressing a wide variety of topics when they constructed new knowledge, such as what types of phenomena to observe, how to observe them, how to mentally or physically organize and summarize observations, and when (and how widely) to apply the new knowledge to new cases. Each of these epistemological standards can be considered another aspect or way of knowing that shaped how the practitioner learned about their subject. The pluralisation in “ways of knowing” thus takes on an additional meaning—practitioners employed more than one standard in their process of coming to know.

The above discussion is in need of some correction, though. These practitioners’ epistemological standards were more than just theirs; the knowledge practitioners were not acting in isolation. Both are members of a nested set of larger, inter-related communities. In the case of the Kwakwaka’wakw clam digger, he is part of a number of nested groups to which he orients. He is part of a temporary group of Kwakwaka’wakw clam diggers who he travels with to and from clam beaches. He is also a member of the village where he resides, a village that is in the geographic and social center of his tribe’s clam digging activities. He is also part of a larger group of Kwakwaka’wakw clam diggers. Furthermore, he is a part of the Kwakwaka’wakw community, a loosely affiliated group of friends and relatives. Finally, grudgingly though it may be, he is a member of Canadian society. As a member of each group he is exposed to their conversations, and, therefore their language and ways of expressing ideas. He is exposed to their practices or ways of doing things. He is also exposed to the tools, technology, and other material goods they use in their practices and to aid communication. The means by which he comes to develop knowledge, then, are not necessarily his own. Instead, he can be described as being part of a number of thought communities (Fleck

1979) or communities of practice (Wenger 1998) and his learning process is one of socially-mediated learning. The same can be said of the contaminant ecologist.⁴

By using the phrase *socially-mediated learning*, I am exposing my intellectual roots. The way I approach practitioners' knowledge construction is as a process by which the practitioner *learns* something new. Two important points can be drawn from this statement. The first is that I generally approach knowledge as something that is constructed. The second is that knowledge construction is akin to the process of learning. Consistent with Vygotsky (1978), I am firmly of the position that social interaction proceeds cognition, at least in terms of specific ways of thinking and specific conclusions; it is through a process of interaction with others that our thoughts and understanding are developed and become more complex. Vygotsky's orientation towards cognitive development forms the foundation of more recent anthropological and psychological work in the area of cognition. More specifically, his orientation is used in situated learning theory and situated cognition.

A good example of this work is Jean Lave's Adult Math Project, described in her book *Cognition in Practice* (1988). As with her other work, Lave is interested in how math is conducted *outside* the classroom, in "real life" as some may call it. She argues that the way in which one learned math in school is not the way in which one uses math outside of school, such as in a supermarket when grocery shopping. Instead, the context shaped the process by which her subjects, shoppers, went about calculating which of two

⁴ It should be noted that the structure of communities can differ substantially. For example, in the Kwakwaka'wakw community of clam diggers, the community includes fellow clam diggers who, for many, are also relatives and friends with whom they grew up and shared many types of experiences. For the contaminant ecologists, their community of ecologists represented peers, not relatives, and people with whom they may have only had contact by reading another's journal article, email correspondence, and meeting at professional association conferences.

products was a “better buy” or what the unit price was of a product. The shoppers did not use formal math, with pencil and paper, as a school teacher would instruct them, but a number of different forms of information they had available to them combined with individualized processes of analyzing the information (including approximating) to come up with an answer. As Lave argues, cognition is not something that occurs within the domain of one’s brain, but as an interaction between body and context or situation. Moreover, individuals may employ similar mathematical methods to one another, but these were by no means uniform or the specific skills individuals had once learned in school and conveniently transferred to a new problem-solving context.

Lave’s view of cognition, like others working in her field, creates new opportunities for theorizing about the social analysis of cognition. Previous social theorists of cognition have been working in this direction for generations—Durkheim pointed out how our world was composed of social facts and how our minds reflect those facts (Durkheim and Mauss 1963; Durkheim 1995), Mead (1934) and Blumer (1969) described the process by which the mind forms through social interaction and meaning becomes attached to objects, Schutz (1967) articulates the uniqueness of individuals’ perspectives in cognition and intersubjectivity, and Berger and Luckmann (1966) pull many of these arguments together to describe how actors navigate everyday life via knowledge they created about everyday life. All but Durkheim explicitly stipulate that actors have some form of agency and therefore view cognition is more than just a reflection of social life. What Lave and others do is to definitively widen the view of cognition from being one in which the brain is seen as an instrument through which social life is mediated to the entire context in which cognition occurs. In some ways, this

is similar to what Bruno Latour, Michel Callon and John Law do with Actor Network Theory (for example, see Latour 1987), where knowledge materializes not from a single actor applying and extending tools he or she previously learned to solve new problems, but as a result of interactions among an entire network of “actors,” many of which are not human.

Lave’s shoppers are clearly not “expert” mathematicians. This does not mean that situated learning is only undertaken by the novices. Instead, the argument is that all people—including “experts” as they develop new knowledge about their subject matter—learn and, as they learn, they do so as situated learners.

One connection left unclear by Lave is the role communities *do* play in influencing how we think. If we cannot transfer what we learned in math class to the supermarket, then what, if any, connection does cognition have to society other than being influenced by the immediate materials at hand? Wenger (1998) provides an answer to this in his concept *communities of practice* in which he describes *communities of practice* as composed of three elements: a domain, a community and shared practices. Domain is a shared domain of interest around which a practitioner is actively engaged, community is relationship-building activities that expose a practitioner to the thoughts, ideas, stories and language of other practitioners with the same set of shared interests, and practices are the shared activities and behaviors in which these practitioners engage. Much like Schutz (1967) would argue, cognition is therefore not a reflection of one’s social environment, but of one’s lived experiences and those experiences are shaped by the community to which one belongs. In other words, the shoppers were not transferring their math skills from school as their teachers never took them grocery shopping.

Instead, their individualized mathematical methods were perhaps a reflection of how their mothers (or fathers) went about deciding what was the “best buy” – cognitive norms (Zerubavel 1997) *particular* to shopping that he or she had directly learned from others and perhaps adapted to his or her liking.

So when I mention the communities to which the Kwakwaka’wakw clam digger or the contaminant ecologist belong—or anyone else, for that matter—and how these shape the epistemological standards they employ, I do not mean to suggest that they are merely reflecting what they have seen others do. They may accept these standards because they make sense given the particular set of practices they use or interests they have, or they may be required to adopt the standards if they want to continue being part of that community. But it is not a given that just because many people in their community adopt a standard that they will, too. Regardless, it is because members of communities are exposed to particular epistemological standard that these standards become a convenient tool to be used in the construction of knowledge.

From this initial discussion it would appear that I am tying my exploration of *ways of knowing* to epistemology. But the ways in which one comes to know extends beyond epistemology. How and what one comes to know is also shaped by the social relations within a community. For example, trust can take on a number of forms. Members of a community may feel comfortable trusting one another in terms of coordinating and accomplishing tasks but not in terms of what others claim. Members of another community, in contrast, may trust in a different sense; members may also believe what others claim they have done or observed, like how the contaminant ecologist trusted his research assistants to accurately report the origin of the collected clams. More than

epistemological standards, ways of knowing are bound up in different forms of social relations. Each formulation of trust results in different procedures by which knowledge is constructed or different ways of knowing.

Practitioners make assumptions about the world, about its existence, and the basic categories that form its foundation. Whether you think that knowledge is cognitive and something you can write in a linear format represents a different ontological approach but also a different way of knowing from one where you place little emphasis on verbal communication and instead emphasize embodied knowing.⁵ Similarly, one is employing different ontologies (and ways of knowing) when one sees the world as part of a system as opposed to territories and divisions, or when one's orientation is towards the world as static and thus knowable or dynamic and therefore never quite tractable (for example, see Paolisso 2002; Palsson 2000; Roepstorff 2000). Ways of knowing are thus also tied to ontological positions.

There are additional ways of knowing. What we use in our environment to signify the presence or existence of a particular concept and what we consider "evidence" can differ among knowledge practitioners. There are numerous potential signifiers a knowledge practitioner may employ to develop new knowledge (Liszka 1996). For example, a practitioner could assess the age of clams by the length of the clam's shell, by the number of rings on the shell or perhaps a number of other signifiers. The possibility of alternative indicators is germane to the positivist tradition of scientific research.⁶ Yet the implications of this—that different indicators can potentially result in different

⁵ For a good example of alternative ontological approaches to the same medical topic see Mol 2002.

⁶ What is not always acknowledged by positivist-oriented scientists is that not all representamens are obvious—at least not to knowledge practitioners working in different communities of practice. The result is that what is deemed as a good representamen to one group of knowledge practitioners may be "voodoo" or invalid to another group of practitioners.

insights *and* that practitioners from different knowledge traditions may not only use but identify signifiers that other practitioners may not even recognize as such—is not typically expressed by those working in the positivist tradition. Different signifiers thus represent different ways of knowing.

Another way of knowing is through the techniques and practices one uses to reduce, order and reason about the observations/measurements one has made. For example, practitioners can craft claims around central tendencies or, conversely, consistency and difference. They can combine observations from multiple geographic sources or isolate by geographic region. They can decontextualize and normalize or retain context and nuance. Each of these choices combines to form distinct ways of knowing.

Initially I struggled to identify a common underlying theme or statement I could make about this project. No matter how hard I tried I could never find a single statement that drew everything together. At one point I wanted to claim that all empirical knowledge can be explained in terms of culture. At the time I thought I would be “adding to the literature” because most scholarship in science studies has focused on how scientific knowledge is *social* (in the sense that it is influenced by social practices) but has yet to fully explore how it is *cultural*. For example, Michael Lynch (1991) described how “objective” measurement is really a matter of judgement on behalf of those making the measurements, Latour (1997) describes how scientific claims are negotiated among actors before they are settled upon or accepted, and Shapin (1995) and Dean (1979) describe how scientific practices are shaped by the social structure and interests of those involved. Studies such as these have effectively pointed out that what scientists know is

not based on pure, mechanically “objective” practices but practices that employ judgement and negotiation. These studies show human-ness and the inherent social nature of science, but they tend not to explore cross-cultural variations of how empirical knowledge practitioners make observations, judgments or craft conclusions.^{7, 8}

Karen Knorr-Cetina’s now-classic comparison of molecular biologists to high energy physicists (1999) begins to address the question of whether empirical knowledge is cultural. Her study shows that epistemologies differ by the substantive question being addressed. Not as clearly answered, however, is whether and, more specifically, *how* epistemologies might differ when addressing *the same* (or a similar) research question. My claim, then, was to be that epistemologies used to answer the same empirical question can differ and that these differences are due to culture. Yet I consistently ran up against the problem of “culture”—what was it and why could I say it was differing? As Douglas Clyde Wilson (2003) argues in relation to fishers and government biologists regarding bluefish management, arguments about cultural differences tend to miss the point that what differs in terms of knowing is the institutions to which these practitioners belong, not culture *per se*. Whether one can accept that institutions are distinct from culture (which Wilson himself is somewhat ambiguous about) is an open question, but his point is valid to the extent that researchers need to be specific about what they mean by “culture” if their arguments are to be insightful. My claim that “knowledge practitioners’ practices differ because they have different cultures” results in an argument that is more circular than directional.

⁷ I am using the term *culture* here quite generally to mean the specific sets of practices and associated mental life of group of people.

⁸ For exceptions see Asquith 1996 and Verran 2001

Other ideas I toyed with at various points included claiming that mental lenses and work practices, each in isolation, could explain how people come to know. But these did not provide a complete picture either; they, too, had shortcomings. Specifically, they were too limited in that by themselves they only provided a partial answer. In the end I decided I was trying to provide a single answer to a complex question; saying “it’s all about what one thinks” or “it’s all about the practices one employs” was not enough.

It then occurred to me that knowing was about more than just what one thought or did, the epistemology or ontology one used, minds or bodies, or how one related to others. It was about all these things. Moreover, it was only in combination that these elements resulted in new knowledge and it serves no particular interest to stress the importance of one over the others. This then brought me to framing this dissertation as *ways of knowing* with an emphasis on its pluralisation. It is through many ways of knowing that, combined, result in one’s ability to know: ontological, epistemological, relational, cultural, cognitive, social and praxial.

In sociology it is common to describe theories and perspectives as “lenses”—e.g. a dramaturgical lens, a macro lens, a conflict perspective lens, a functionalist perspective lens, a symbolic interactionist lens. Each lens results in a somewhat different analysis or insight about the social world. The implication of this metaphor, however, is that social analysis is somewhat instantaneous; one has but to look through an object, a lens, to gain a new view of the world. Yet coming to know about something rarely involves one step. Rarely do we gain knowledge from a single observation. Instead, coming to know often involves a series of steps or stages during which we make multiple observations and then craft these into conclusions. Knowing involves doing, thinking, generating ideas,

reasoning, and interacting with the world. Latour (1999) describes this evolving process as a chain of inter-locking steps.⁹ What we metaphorically depict as a single act is composed of a number of different acts and thoughts; we do not “see” directly, we “see” through a process—a process in which we as actors are actively involved.

Each stage or step does not stand in isolation; an indicator, epistemological standard, ontological classification, or patterned social interaction, when employed alone, cannot create new knowledge.¹⁰ In fact, in most cases, it is difficult or impossible to employ each of these ways of knowing in isolation. Each step—each way of knowing, as I have described it—by itself constitutes perhaps a partial way of knowing. These partial ways of knowing fold in on themselves to form a complete way of knowing—complete at least in the sense of moving a practitioner from the beginning to the end of an inquiry.

Such partial ways of knowing are not interchangeable pieces; one can not neatly switch one classification, indicator, standard, or interest for another (whether it be one classification for another classification or a classification for a standard). Each result is a step in a different direction to different ways of interacting with the world, different assumptions about how the world is organized, to a different way of ordering observations, and a different purpose in knowing it. The result is that some ways of knowing have natural affinities to one another (Abbott 2004). For example, it is far easier to use mathematical formulas on precise figures (numbers) than on qualitative

⁹ Latour also claims that this chain simultaneously moves away from particularity, locality, materiality continuity and multiplicity—which he generally refers to as reduction—and moves towards standardization, text, relative universality and compatibility—which he generally refers to as amplification. I hesitate to add this here as all the examples upon which he constructs his models were those found in Western science; the simultaneous movement towards amplification and away from reduction, as he describes it, may not be applicable to all processes of empirical knowledge production. His main point, though, that knowledge practitioners are involved in a series of transformations is sound.

¹⁰ Here, too, I pick up on Annemarie Mol’s phrasing related to wholes as found in her discussion about atherosclerosis (2002).

observations. Similarly, a division of labour among practitioners who aim to generate a single, cohesive conclusion is perhaps best suited to more verbal, less embodied knowledge. Some such ways of knowing seem to have more “natural” affiliations.

Ways of knowing can be community-specific. By this I mean they are associated with a specific community or set of inter-related communities. For example, the biologists with whom I worked adopted random selection strategies in their research. This approach is not unique to them; in fact, I would guess that many readers have, themselves, been exposed to the idea and even used the technique at some point in their life. Random selection techniques are broadly employed in both the natural and social sciences, especially by those who intend to use statistical techniques. Yet random selection is not only unheard of amongst the aboriginal clam diggers with whom I worked, but makes little sense to them, either. Instead, they have their own way by which to select “samples” for observation, a method they consider superior to that of random selection (see chapter five). The same can be argued for many of the practices, assumptions, interests, and classifications practitioners employed; ways of knowing can be associated with particular communities of practice.¹¹

Up to this point my discussion about *ways of knowing* has focused on *processes* of coming to know. But the phrase *ways of knowing*, surely must also encompass the meaning “ways of understanding”—*what* a practitioner comes to know. This, too, is relevant to my discussion. As Thomas Kuhn (1970) argued in relation to scientific paradigms, paradigms change over time, with each shift resulting in the gaining of some

¹¹ For this reason, this topic is particularly well suited for analysis by a *social* scientist; while individual and human-wide factors are undeniably involved in the production of knowledge (e.g. the way in which the human brain processes information; physiological limitations of the human body), ways of knowing are clearly linked to issues of community.

empirical explanations but the loss of others. By examining my case studies it becomes clear that practitioners make different types of explanations; one group of practitioners created general claims about the generic relationship between phenomena while another group made claims about specific locations. The difference resulted from their use of different ways of knowing, demonstrating that what we know is forever linked to how we know it.

In saying that knowledge is grounded in the process by which it was constructed I do not mean to suggest that empirical knowledge is completely artificial in the sense that it need not have any correspondence to the world “out there.” As Pickering suggests (1993), in coming to know about the empirical world the world resists and, through this resistance, we gain new insights. In fact, researchers who compare knowledge of practitioners from different cultural backgrounds studying similar empirical phenomena often find the practitioners’ knowledge complementary and even possessing considerable overlap. For example, Scott and Walter (1993) reported that, when scientific and indigenous knowledge about erosion interventions were jointly applied and tested for their effect, the multiple year average rates of downstream sedimentation were reduced by 92% over rates produced by conventional scientific methods alone (*ibid*: 62). In a study by Cools, DePauw and Decker (2003) the research team created two GPS-based “maps” of a farming region, one using land resource professionals (scientists) and the other indigenous knowledge of farmers in a northwestern village in Syria. The two completed maps were compared and the researchers found considerable agreement between the two sets of experts, and disagreement was often traced to discrepancies in scale in which the scientists were less attentive to microclimates and microhabitat than

the farmers.¹² Yet in both these examples, what each set of knowledge practitioners knew was conveyed using different language and in different ways than the other. My point then is that different processes of knowing can result in different forms of resistance and therefore in different forms of knowledge and understanding; *ways of knowing* can refer to what is known and there are different ways to know the same empirical phenomenon.

Let me try to summarize my argument regarding ways of knowing by contextualizing it in relation to various bodies of literature. What I am doing is similar to Actor Network Theory (ANT) (e.g. Latour 1987) in that I am interested in how a number of different “actors” of various forms and types (e.g. individuals, communities, institution, tools, clams, regulations, seasonality, etc.) influence what practitioners come to know and how they come to know it. I differ from ANT, though, in terms of the relative amounts of agency I credit all actors having in the process of “fixing” knowledge into a particular configuration. For example, while clams clearly play an important role for anyone learning about clams, the individual learning about the clams has a larger range of options or choices to make in terms of behaviour, including forms of interaction with the clam, than does the clam. It is also possible that the amount of agency actors within a network affect can differ. For example, in science there are higher levels of social consensus required among actors in regards to the construction of knowledge compared to the Kwakwaka’wakw clam diggers and therefore agency (in regards to settlement on a particular knowledge claim) is more evenly distributed among actors in

¹² Lack of precise agreement occurred for a variety of reasons including categories that did not map directly onto one another and differences in micro-climate that were captured by the farmers but not by the scientists due to the larger scale at which the scientists worked.

the former than the latter. So while I do adopt a similarly wide scope in terms of cognition, I do not adopt the exact same position as those employing ANT.

In fact, I believe I am closer to the approach taken by cognitive anthropologists and psychologists in theories of situated social learning than to ANT. I see myself suggesting a refinement on situated social learning theories—a refined description of the types of factors that come into play when developing situated knowledge, such as cognitive norms (including epistemological standards and ontological practices) that are consistent with the communities of practice to which a knowledge practitioner belongs, the configuration of social relations—especially those related to trust—that influence how knowledge practitioners socially organize their process of knowledge construction and dissemination, the set of signifiers that are used to construct new knowledge, and the processes by which the knowledge practitioners attempt to organize and summarize what they have observed/measured. Each of these different ways of knowing are required aspects of situated knowledge production and undertaken, in some form, by a knowledge practitioner when constructing new knowledge. The specific ways differ by practitioner and the community(s) of practice with which they are affiliated.

The refinements I suggest in terms of how situated cognition occurs—regardless of whether it is figuring out the “best buy” in a grocery store or why clams are (or are not) turning black—are not all of my own creation. As a member of a community of practices myself, I owe recognition to a number of scholars. I mention some of my key influencing sources here.

Cognitive norm is a term proposed by Eviatar Zerubavel (1997) to capture the idea of patterned ways in which we think that constrain our thoughts, such as rules of

focusing that lead us to see some things as foreground and others as background. This is clearly one possible way in which communities can influence the ways in which a practitioner goes about constructing new knowledge. In his book *Social Mindscapes*, Zerubavel talks about how everything from our classification schemes to the way in which we order our memories are socially mitigated. Zerubavel's work draws from a wide range of sources but is most deeply influenced by Durkheim (e.g. 1984; 1995; Durkheim and Mauss 1963), Mead (1934), Schutz (e.g. 1967) and Mannheim (1968). My own thinking in relation to this project has been largely shaped by Zerubavel's discussion about social optics, islands of meaning (or classification norms), cognitive traditions, mental focusing, attending, and cognitive bias. Each of these plays an important role in the process by which practitioners produce knowledge and are reflected in my various discussions about signs (attending, social optics), ontology (classification norms), and processes of crafting conclusions (cognitive norms).

My general orientation towards the relationship between practice and cognition is also influenced indirectly by Thomas Kuhn (1970) through the work on Barry Barnes (1982). Barnes broadened Kuhn's discussion about training to general learning about a category which he claimed to be achieved primarily through ostension (demonstration, illustration, pointing things out) and the learning of similarity relations as taught by an authority figure. According to Barnes, individuals use representatives of categories to mentally construct boundaries around categories. In other words, the similarities and differences among representative examples are used to learn how to apply a particular category and to understand the meaning inherent to that category. Like Kuhn, he uses the phrase *learned similarity relations* to refer to clusters of criteria associated with a

category. Such a system of relations, learned by the individual, is identified by a community so as to order nature, rather than being imposed by nature (*ibid*: 24)

Authority figures act as a guide regarding how perception should be organized and understood (*ibid*: 25).¹³ Barnes' description of categories is complementary to that of Zerubavel in that Barnes provides a detailed account of how categorical norms are learned through interaction with others. I used this type of thinking when writing about the techniques practitioners use to order and summarize data, such as the contaminant ecologist "knowing" how to statistically analyze a batch of data he had never seen before.

When studying knowledge, one cannot avoid the topic of interests. This is especially true when dealing with cross-cultural or cross-community situations where the topic of study may be identical but the purpose of study can be radically different. There is no one scholar to whom I can trace the origins of connecting interests to ways of knowing. In large part this is because so many people have written about it. Certainly the discussion can be traced back to Mannheim (1968) (and Marx through Mannheim), Polanyi (1958), Fleck (1979), Kuhn (1970) and Shapin (1995). Each of these scholars contributed to the larger discussion about the relationship between a practitioner's interests and her resulting knowledge by commenting on how interests shape the questions she asks, the types of materials she uses to explore the question, and the concepts she considers appropriate for answering the question.

¹³ An example originally taken from Kuhn serves to illustrate Barnes' argument well. It is a story about a father teaching his child how to distinguish among three types of birds: ducks, geese and swans. While on a walk, the father points to the three types of birds and calls them by name. As they walk further, the child tries to identify new birds they encounter as 'swan,' 'goose' or 'duck,' and the father confirms whether the child has identified the birds correctly. If the child makes an incorrect identification, the father corrects the child. After repeated instances such as these, the child masters identification of the three categories. Subsequent observations will lead the child to catalogue observations and insights about these three types of birds by type, allowing her to develop an understanding of each category. The father, an authority figure for the child, has served as a representative of a larger thought community and provided the child with training consistent with practices of these others.

I also draw on ideas and reasoning presented by Annemarie Mol's in her book *The Body Multiple*. Mol's project was to examine the various ways in which a particular disease, atherosclerosis, is "done" by hospital professionals. A number of practitioners are involved in the diagnosis and treatment of atherosclerosis. Each interacts with the disease in different ways. For example, a pathologist "does" atherosclerosis by examining cross-sections of an amputated lower leg whereas a clinician asks a patient if they have experienced any symptoms of atherosclerosis. Yet each is "doing" atherosclerosis.

Mol claims to be strictly presenting a philosophically-based empirical piece about ontology; she claims what she says has nothing to do with epistemology and therefore with knowing. Not being a philosopher, I am perhaps missing some of the nuances of her argument (namely the crystal-clear line she is drawing between ontology and epistemology), but it seems to me that what she was saying had a lot to do with how people come to know (about atherosclerosis). She adds new breath to Mannheim and others' arguments about how the questions one asks and the position from which one asks them impact one's perspective.

Finally, I draw on Knorr Cetina's work in my discussion of different ways of knowing in a number of ways. I identify two such ways below. Like others working in the sociology of science, Knorr-Cetina has drawn attention to the importance of locally-available materials to the scientific enterprise —the particular chemicals already on order by a lab, the breed of mice that had been used in a previous study, the pipe cleaners on hand in the storeroom (1983). This is known as *situational contingency* and reinforces ideas about situated learning in that the situated learner draws on materials available in

their respective situation (and not necessarily those which would be ideal or available elsewhere). The different ways in which social relations can be organized within a field and the relevance of this to the knowledge its practitioners produce is also a topic explored by Knorr Cetina. In *Epistemic Cultures* (1999), she described how experts and nonexperts alike are organized in their work on high energy physics research projects, where trust becomes an important currency and there is no clear leader. This is contrasted to molecular biologists who work in labs and are accountable to a lab leader. This is closely related to ideas I develop in this work about the organization of social relations among knowledge practitioners.

In general, in refining my arguments about situated social learning and the various ways by which practitioners come to know about an empirical subject I have drawn from three primary bodies of literature: cognitive anthropology, cognitive sociology and the sociology of science—an eclectic array even for a project in the sociology of science, a field known for its diversity. There should be no inherent problem with taking such an approach as all three fields aim to gain better understanding of cognition and knowledge. The difficulties that do exist are due to each taking a somewhat different vantage point¹⁴ for understanding the production of knowledge. Yet this can also be thought of in a positive sense; pulling together ideas from all three fields offers an exciting opportunity to create new theoretical arguments and draw new insights.

¹⁴ The sociology of culture and cognition is, in many ways, a direct descendent of the sociology of knowledge. One might think then that the sociology of knowledge and the sociology of scientific knowledge would not be distinct fields. Yet in many ways they have developed as two separate branches of sociology. Historically, those working in the sociology of culture and cognition have viewed scientific knowledge as distinct from social knowledge (e.g. Mannheim 1968) and have not tended to extend their work into the realm of topics covered by the “natural sciences.” Sociologists of science, on the other hand, made a conscious decision to break away from Mannheim and those who followed his lead such as Berger and Luckmann (Derksen 1996; Knorr Cetina 1995) and largely draw from other theoretical works.

Up to this point I have gone to great extents to explore my intended meaning behind the first three words of my title. I now turn to the subject matter studied by the knowledge practitioners with whom I worked: clams. All knowledge is about *something*—it has a topic. When selecting knowledge practitioners with whom to work, my goal was to select practitioners who studied the same empirical topic. This was important as it allowed me to explore how ways of knowing could differ when the subject matter was, to a large extent, the same. Studying the work of high energy physicists and molecular biologists provides insights into how scientific methods and cultures, as related to the study of different empirical domains, can differ. These, too, are ways of knowing. What such a study can not answer is whether these differences are due to differences in the domain of inquiry; whether it is something about the nature of physical particles or the molecular composition of cells that resulted in those *particular* ways of knowing. To answer questions of how *ways of knowing* can differ that are *indifferent* to empirical domain requires picking a domain of inquiry.

Focusing on a single domain of inquiry perhaps limits the range of ways of knowing to which I will have access; although ways of knowing about clams can differ from one practitioner to another, these particular ways of knowing are likely distinct from how one comes to know about minerals, solar systems or atoms. Yet one must pick a domain of inquiry if one is to explore how ways of knowing can differ within a domain. It is for this reason that I emphasize that the ways of knowing discussed in this dissertation are those related to clams—i.e. with clams as a referent.

The topic of my choice was clams. This is not because clams are inherently interesting. In fact, as I quickly found out, many biologists and clam diggers do not

consider clams a “hot” topic. Few scientists work on clams. When I asked one of the biologists why this was so, he suggested that clams are not exactly a sexy topic. Despite their (and perhaps my) lack of appreciation for these creatures, I picked clams for three reasons: (i) I had access to knowledge practitioners who worked with clams, (ii) gaining knowledge about clams, like many other empirical subjects, is not straight forward, and (iii) there was concern and some debate between the various knowledge practitioners as to whether fish farms were affecting clams and clam beaches which prompted the practitioners to generate new knowledge about clams.

Clams live inside shells and under the sand. Humans are considerably larger than clams and physically unable to position their heads under the sand. To learn about clams you pretty much have to dig them up. But there are only so many clams one can dig up on a given beach and it is impossible to know if you have dug them all up. Consequently, it is impossible to observe what *all* clams are doing at any point in time; “sampling” of one sort or another is required. The process of digging disturbs the clams, though, from behavior they might exhibit otherwise. While clams are not as sophisticated as chimpanzees or seals they do react to stimuli and these stimuli differ when resting under the sand compared to when sitting on the surface of a beach. For example, when disturbed they retract their soft body parts into their shell and “clam up” (clamp their shell shut). They may change their behavior in other ways, too. Clams are also affected by conditions undetectable to human senses, such as chemical changes in water quality.

Our inability to directly observe clams in their natural habitat means there is no clear way to know where they are under that sand, how many clams are there, what they are doing in their natural habitat, or what is happening to them (e.g. other species preying

on them or forming mutually-beneficial relationships with them, water dynamics shifting their position in the sand, etc.). Not being able to observe clams easily means that those who study clams are forced to create solutions to physical barriers, make assumptions and associations, conduct methodological manoeuvrings and provide theoretical “stuffing.” That said, clams are far easier to study than migratory fish that cannot always be seen and may not even be present for most months in a year. Clams, on the other hand, do not move very far. Clams therefore make an excellent topic for exploring how people engage with incomplete information to arrive at conclusions.

The knowledge practitioners

While clams are the referent in my case studies, it is the knowledge practitioners and their ways of knowing about clams that are central to this dissertation. My observations and accounts of ways of knowing are based on four ethnographic case studies of clam knowledge practitioners.¹⁵ The first two cases are scientists: a university-based team of contaminant ecologists and invertebrate biologists from Canada’s Department of Fisheries and Oceans (DFO). The second two cases are clam diggers from the Nuu-chah-nulth and Kwakwaka’wakw (or Kwakiutl) First Nations groups in coastal British Columbia.¹⁶ The four sets of practitioners¹⁷ are comparable in that they all study

¹⁵ I collected the ethnographic data for all four case studies through participation, observation, semi-structured and unstructured interviews, and existing documents. A complete description of the methodology used for collecting this data is described at the end of Chapter Two.

¹⁶ Those readers unfamiliar with non-scientific empirical knowledge practitioners should be directed to anthropological and other academic works describing the knowledge of indigenous and “local” peoples from around the globe. A vast number of practitioners’ empirical knowledge has been documented, including Tibetan Buddhist medicine (Barmark 1998), fishers’ weather-reading abilities (Daipha 2007), Burkina Faso hunters’ ability to forecast rainfall (Roncoli et al. 2002), Nigerian’s use of plants for veterinarian purposes (Adewumni 2001), and natural resource management practices (e.g. Baker 2001; Peterson and Rigsby 1998).

intertidal clams on Vancouver Island, British Columbia and work with at least two of the following three species of clam: butter (*Saxidomus giganteus*), little neck (*Protothaca staminea*), and manila (*Venerupis japonica*). Perhaps unsurprisingly, there are considerable differences amongst the sets of practitioners in terms of how they come to know and what they end up knowing. That said, there are also considerable similarities. It is for this reason that these sets provide an excellent source of material for teasing out issues related to ways of knowing.

My approach

It is my belief that the best way to learn about the social aspects of others (i.e. culture) requires a two-fold approach in which the researcher both looks in depth within cases while also looking critically across cases. Being an active participant observer allows the researcher to immerse herself in a particular culture and, by doing so, become intimately familiar with that culture. In fact, my ideal is to get as close to “going Native” as one can without losing one’s critical sensibility. Of course *what* she comes into contact with within a social group and *how* she does so is influenced by who she is and

¹⁷ Throughout the dissertation I refer to these four groups of knowledge practitioners as *sets* of practitioners. This phrasing is inconsistent, though, in the sense that the number and cohesiveness among practitioners differs. My ethnography of the Kwakwaka’wakw and Nuuchah-nulth clam diggers is of a community of diggers; diggers that work out of the same village but who do not necessarily work together. In contrast, the contaminant ecologists with whom I worked were members of a team working together on the same research project. I have had direct and indirect contact with others in their larger community, including having taken ecology courses at the undergraduate and graduate level, having attended an annual meeting of the Ecological Society of America and read numerous ecology journal articles, but this was outside the purview of this project. Similarly, my work with the government invertebrate biologists was focused on three individuals. In this case, their peer community was not academic biologists or ecologists, but other government biologists and a few applied academics conducting management-oriented research. The DFO biologists do work in coordination with one another in that they worked in the same small department and on many of the same projects, but they were not organized as a team *per se*. In using the phrase *sets of knowledge practitioners*, then, I am referring to the set of practitioners I included in my cases but the numbers and relationship of the practitioners to their community of peers is not necessarily the same among cases.

how others see her. Regardless, it is through such a process that she can develop an in-depth appreciation for the ways in which these individuals think and act. But traveling into the depths of one social world is not enough, even if it is one clearly “foreign” to you. A researcher also needs to have a basis of comparison. It is only by traveling outside the world of the familiar into the unknown and developing a new sense of familiarity there that one can develop meaningful, comparative insights. Familiarity with a second “world” and being able to mentally move between the two provides two vantage points—critical vantage points from which to examine what one also understands and sympathizes with as a (semi)insider. What I am advocating for then is the need to contrast one lived experience with another, thus moving us one step beyond Garfinkel’s (1967) vantage point of pretending to be a foreigner in one’s own (relatively) familiar terrain to becoming a foreigner and a native in two terrains.

In terms of studying ways of knowing, it would seem helpful to have access to as complete an experience of the process by which knowledge is constructed so as to be able to have “access” to as many aspects of the process as possible, thus allowing the researcher to analytically carve ways of knowing from a whole (or as close to a whole as possible) instead of from a pre-imposed analytical unit. For example, determining in advance that I was only interested in issues of sampling and thus limiting my investigation of practitioners to when they were working on the beaches (sampling clams), could lead to my missing out on other aspects of sampling at other points in their inquiry *or* missing being exposed to a more suitable analytical unit than “sampling.” Combining this approach with that of in-depth familiarity with two “worlds” i.e. processes of knowing, suggests a study of at least two complete processes.

In my comparison I attempt to be balanced or symmetrical. By this I mean that I follow assumptions proposed by the Strong Program from the Edinburgh School of the Sociology of Science (Barnes, Bloor and Henry 1996; Sismondo 2004), namely that: (i) I view knowledge as accepted belief, (ii) claim that knowledge arises from the practical activity of humans in groups, (iii) and that these humans are pursuing specific goals, or interests. I also follow the majority of their tenets as well—that sociological explanations of the content of science (and presumably other empirical knowledge) should be: (i) causal, or concerned with the conditions which bring about belief or states of knowledge, (ii) symmetrical, with the same types of causes explaining true or false, accepted or unaccepted beliefs, and (iii) reflexive, meaning the same type of sociological explanations can be used to study sociology itself. I break with the Edinburgh School of thought in one important regard, however. Barnes and Bloor strongly advocate that sociological explanations should be impartial or non-evaluative in regards to the beliefs held. While I have no fantasy of declaring what is truth or that one claim is superior to another (I do not follow Fairhead and Leach's (1996) lead and search for additional evidence to corroborate one side or the other), I do occasionally enter into a critical discussion about the strengths and shortcomings of the processes and practices employed by the practitioners. That said, I make every effort to make my critical gaze balanced; at no point do I claim one is, on balance, better or worse.

As I stated at the start of the chapter, this is an ethnography of ways of knowing. In writing an ethnography there are a set of concerns every writer must deal with. The specific ones I wish to address here are in relation to locality, first in terms of the degree to which I provide identifying features of people and place, specifically names, and the

second in terms of how general I mean to suggest the content of this ethnography applies beyond the particulars of people and place. The two are obviously connected, so I will address them together.

In writing this dissertation I had a difficult time deciding whether to include specific names, including those of individuals, organizations and places, or to obscure them in some way so as to mask their identity and then also, to some extent, reduce the local-ness of what I discussed. It is common practice in ethnographic work to obscure proper names. This can be for a number of reasons, a major one being the ethical implications of identifying the people and place of which intimate details are reported. I personally felt that obscuring names was important for several reasons. The first is that I did not (nor was I required to by ethics) get permission from every person, place and organization in which I came into contact during my research and made record of in my fieldnotes. For example, I had very little contact with one of the organizations involved in the contaminant ecologists' research project. Consequently I did not ask the organization for permission to mention their name or discuss them in the following pages. And, in fact, I make very little mention of them in the following pages as they were a minor (yet arguably important) player in the project. I do, however, allude to them as they were pivotal in terms of supplying one of the key actors to the team—the project coordinator. The second reason is that I am putting each knowledge practitioner (or team of practitioners) under an analytical microscope. As such, I am being critical of their work. As the knowledge practitioners I worked with were kind enough to let me work with them for my project, the last thing I want to do is be publically critical of them; their generosity deserves better than this. In saying this I do not mean to suggest that they did

their jobs “wrong” or in a way in which they should be embarrassed. Instead what I mean is that *anyone* many feel uncomfortable being exposed or held naked as these practitioners are in the following pages. My intent is not to create such a situation for these individuals. Consequently, I would prefer not to divulge their names, at least not within the main body of the dissertation.

On the other hand, though, these are knowledge practitioners and there is such a thing as intellectual property rights and proprietary rights over what one knows. In this regard, it is perhaps unjust of me to strip them of their identifying features, including their names. Moreover, some knowledge practitioners explicitly told me they wanted to be identified by their personal name in whatever I wrote.

Ultimately, I chose to take a combined approach. For those practitioners who explicitly told me to include their names I did so, at least in the junctures where it would not seem odd to identify some individuals’ identities while concealing others. For those who made no such explicit statement, I obscured their identities, including their names and other identifying features. Specifically, I identify the names of the two Nations, Nuu-chah-nulth and Kwakwaka’wakw, from which the aboriginals originated. I also identify specific villages in Chapter Two where I describe the historical context of these tribal groups. I did not identify the communities from which specific individuals originated, however. For the contaminant ecologists, who were easier to mask in terms of their identity, I concealed the names of the universities and/or organizations at which they were employed. The organization to which the government biologists belonged was harder to mask; anyone familiar with marine issues in Canada could instantly recognize that the government biologists worked for the DFO so it was pointless to cover up the

name of this institution. That said, the institution is large enough that the identities of the specific individuals within it could not be easily identified. For this reason, I refer to them as the DFO biologists, but provide minimal identifying features beyond this.

The majority of my reason for masking identities is ethical. Yet I am also driven by theoretical reasoning. Using a combination of specific and obscured names also allows me to enter a theoretical space in which I am simultaneously local and extra-local. This suits me fine, as it provides a degree of ambiguity in terms of how far beyond my domain of empirical work my claims can travel.¹⁸ In many ways, what I describe in this dissertation is about how specific knowledge practitioners conduct their work and organize their thoughts. Yet they are clearly not acting in isolation. This is especially true of the ecologists, who are participants in professional fields and coordinate many of their thoughts and actions with others in these fields. It is also true, though, of the First Nations in that these practitioners work among others, not all of whom I worked with. In many ways my discussions about ways of knowing extend to others in their respective communities. In fact, my discussion may, in many ways, extend beyond these communities. For example, there are additional natural resource harvesters, First Nations and otherwise, who harvest clams and other marine resources. Moving beyond the marine, there are other natural harvesters elsewhere who may encounter similar issues and adopt similar ways of knowing. Likewise, others working in the sciences have

¹⁸ In doing so I am following Mol's (2002) lead. Mol suggests that, as a researcher of localized cases, it is impossible to know how little or how much can be generalized to other cases, what aspects can be generalized, and to whom they can be applied. Her example is Dutch doctors who work at a specific hospital. They are Dutch doctors, so presumably the hospital she worked in will be similar to other Dutch hospitals. Yet no hospital is identical—individuals differ, physical space differs, local culture differs. So what can be generalized from one Dutch hospital to others? Certainly many things, but which? Moreover, can a statement be generalized to all Dutch hospitals or just some? How many? This type of reasoning leads her to leave the inferential work up to her reader, who may see how some things apply while others do not. This then allows the reader to make of her analysis what is useful to them.

adopted many of the same or similar ideas and practices as those in ecology. I did not work with these practitioners and therefore can not make any explicit claims about. Yet from previous and on-going experiences, such as by reading peer-reviewed ecology journal articles, reading about other natural resource harvesters, and meeting clam diggers from other areas, I can see that what I discuss in the following pages has parallel to others outside these immediate communities. That said, there are also often differences that are present as well. I am therefore content with leaving this local/ex-local ambiguity as it is then up to my readers to see, explore and decide for themselves whether, in what ways, and to what extent my analysis can be imported to new locales.

What follows

I now come to the obligatory part of an introduction: describing what is contained in the following pages and how it is organized. In chapter two I historically contextualize the four sets of clam practitioners as is relevant to clams and related issues in the coastal waters of British Columbia. More specifically, I provide a detailed description of both the Kwakwaka'wakw and the Nuuchahnulth and their relationship to the water, clams and non-aboriginals. I then describe the clam fishery in British Columbia and the Department of Fisheries and Ocean's (DFO's) role in regulating the fishery. Finally I move into a description of salmon fish farms, a highly controversial topic in British Columbia and one to which many coastal First Nations are opposed as they claim fish farms have a negative impact on clams and clam beaches. The contaminant ecology team with whom I worked are briefly introduced as public-minded academics who responded

to the call for more research to be conducted on the relationship between fish farms and neighbouring clam beaches.

In chapter three I provide a detailed description of the four sets of knowledge practitioners with whom I worked—their interests and goals (as related to clams), their motivation for learning specific types of information about clams, the types of decisions they made, others with whom they worked, and specifics of the processes by which they studied clams. Following this description I provide an initial, “quick” analytical comparison of the knowledge practitioners. I conclude the chapter with a description of the final knowledge practitioner involved in my study: me. At various junctures throughout the chapter I described my involvement with the various sets of knowledge practitioners. It is not until the end of the chapter, though, that I specifically address my “methods”—how and why I came to work with these particular sets of practitioners, what I did with each, when I did it, and how. The main purpose of the chapter is thus to contextualize the practitioners (including myself) and provide a comprehensive picture of the processes by which these practitioners developed new knowledge.

In chapter four I compare the systems of social relations in which the various clam knowledge practitioners are embedded. My main focus is on two of the four case studies, the Kwakwaka’wakw diggers and the contaminant ecologists. I describe these two cases as contrasting types, the first being one in which social relations are oriented towards creating private knowledge and the second in which the social relations are oriented towards the production of public knowledge. The discussion focuses on the ways in which social relations and interests, combined, resulted in ways of knowing and

how these ways of knowing can differ by the particular web of social relations and interests.

In chapter five I examine ways of knowing pertaining to the signs practitioners use to construct new knowledge. Specifically, I compare the different means by which fisheries biologists and traditional aboriginal marine harvesters make assessments about clam abundance on a beach. I start by showing how multiple alternative measures or *representamen* can exist for the same concept (*interpretant*) and that knowledge practitioners make selections among these representamens. I describe how the biologists and First Nations practitioners took opposing views regarding random selection, the use of expert knowledge in the selection of samples, and preference for domain-specific versus general measures. Their preference over specific practices was closely tied to respective underlying differences in epistemological standards and ontological assumptions. The biologists, who employed a select number of precise measures, emphasized the tractability and measurability of their concepts whereas the aboriginals, who employed multiple measures in an on-going assessment, considered precise measures problematic and unable to yield any special insight to the behaviour and presence of living organisms. Each set of practitioners' ways of knowing were similar in that all employed empirical signs, but the differences in their signs—and therefore their ways of knowing—were most notable.

In chapter six I switch my focus back to the team of contaminant ecologists and compare them to a select sub-group of Nuuchah-nulth clam harvesters in an examination of the ways of knowing by which these knowledge practitioners moved from multiple and varying observations to knowledge claims. In other words, the purpose of the

chapter is to explore how these knowledge practitioners crafted their knowledge claims. As I argue, the heart of crafting conclusions involves sorting, comparing, typifying and generalizing observations. The specific practices employed by these knowledge practitioners to move from multiple and varying observations to knowledge claims differs, however, by the knowledge tradition in which they work. For example, while all the knowledge practitioners sorted their observations by location, the biologists formalize their sorting so that single, isolated characteristics are compared whereas the aboriginals sorted in a much more fluid, nuanced way in that their sorting was according to temporary classifications and tended to incorporate multiple characteristics. The ecologists mathematically calculated measures of central tendency and analysis of variance to determine what was “typical” whereas the aboriginals were interested in the range of observations and how these differences could be explained by relationships to other characteristics or phenomena. Thus, despite some key similarities, the knowledge practitioners exhibited distinct ways of knowing in regards to crafting conclusions.

The final chapter, chapter seven, functions as a conclusion. In it I draw out two themes that run throughout the dissertation but which I do not directly address until this chapter. The first of these two themes is the importance of practices or, as I refer to them, “doings,” in terms of the practitioners ability to achieve individual conclusions as well as maintain a particular perspective or worldview. Throughout the dissertation I have discussed a number of practices as well as the roles and importance of embodied knowledge to the practitioners. In this section I build on these issues to explore the

importance of practices relative to mental lenses in terms of interpretation and understanding.

The second theme I build on is how ways of knowing are not treated equal. In fact, in many situations knowledge practitioners and their practices are treated as unequal and in a relatively static hierarchy with science at the top. Here I discuss the implications of this in term of how the respective sets of knowledge practitioners are treated and who has the authority to implement what they know. My main focus, though, is to make a suggestion as to how this hierarchy of ways of knowing seems to reflect structural features (as opposed to individual prejudices) inherent to our modern democratic world.

As a final point to end this chapter I would like to add that, in keeping with the sentiment of my dissertation, the analysis presented in the following pages is contingent on my social position vis-à-vis others with whom I worked, what I observed, my particular array of social lenses, reflections and style of analysis. Moreover, like others, I have limited capacity to enter the minds of the knowledge practitioners with whom I worked and therefore can not say to speak for them. While I made all attempts to make my analysis as accurate as possible, everything is filtered through my words and my perspective. In other words, like all other knowledge, what follows is based on my particular configuration of ways of knowing.

Chapter 2: The Practitioners in Context

This chapter serves to historically contextualize the four sets of knowledge practitioners as is relevant to clams and related issues in the coastal waters of British Columbia. More specifically, I provide a detailed description of both the Kwakwaka'wakw and the Nuu-chah-nulth and their relationship to the water, clams and non-aboriginals. I then describe the clam fishery in British Columbia and the DFO's role in regulating the fishery. Finally I move into a description of salmon fish farms, a highly controversial topic in British Columbia and one to which many coastal First Nations are opposed as they claim fish farms have a negative impact on clams and clam beaches. The contaminant ecology team with whom I worked are briefly introduced as public-minded academics who responded to the call for more research to be conducted on the relationship between fish farms and neighbouring clam beaches.

The Kwakiutl, As Boas Called Them¹⁹

Prior to the arrival of Europeans, the Kwakwaka'wakw inhabited the inner upper half of the coast and northern tip of what is now called Vancouver Island, the opposing coast on the mainland and the islands in between (Provincial Archives 1966: 9). Although they traveled inland, their lives were oriented towards the water; to them, the sea was a means of transportation, the front door to their village sites and the main source of food (Rohner and Rohner 1970: 3). Their neighbours were the Bella Coola to the

¹⁹ Unless otherwise noted, the information in this and the following section were drawn from interview material I obtained while working with members of the Kwakwaka'wakwa and Nuu-chah-nulth Nations.

north, the Coast Salish to the south and east, and the Nuuchah-nulth (also known as the Nootka) to the south and west.

The Kwakwaka'wakw are described as being of three dialect groups: the Haisla, the Heiltsuk and the Kwakiutl. As this list suggests, the name *Kwakiutl* is somewhat of a misnomer when applied to the entire group. Frans Boas, the first and most famous anthropologist to write about the Kwakwaka'wakw, worked with natives from a village now known as Fort Rupert. This group called (and still calls) themselves the Kwakiutl. Boas adopted and applied this name to all natives in the language group. More recently, political and cultural leaders from Alert Bay have suggested the term Kwakwaka'wakw to refer to the entire group as the word means "Kwakwa'la speakers" (Kirk 1986).

The Kwakwaka'wakw were not tightly bound in a single political order. Instead, they were loosely affiliated through family ties, language and tradition (Provincial Archives 1966). Clans inhabited villages or clusters of villages and small sets of more immediate families inhabited households. Houses took the form of long houses—semi-permanent square or rectangular structures with gabled roofs held up by long wooden cedar beams. The surrounding walls were formed from planks made of wood. There were no windows but they did use long poles to prop up roof boards to allow smoke out as cooking and heat was generated by open fire (BC Provincial Archives 1966). The size of the house differed by the rank of the family and, as the Kwakwaka'wakw had an extremely hierarchical social organization, this meant the size could be notably different.

Villages were not permanent. The Kwakwaka'wakw subsisted on fish, clams, deer, duck, seal, roots, and berries, among other things. As some still say, "when the tide goes out, our table is set." The foods they ate were only available seasonally; even the

clams, which are present all-year round, were primarily harvested during the winter months as they are said to “go milky” in the late spring. The Kwakwaka’wakw moved their settlements with the season. When they moved they took the house planks with them but left the frame behind.

The people I worked with described having four village sites and moving from one to another as one resource became seasonally scarce and another ready for harvest. Even today, when people are more settled, small groups still temporarily migrate to Kingcome Inlet to fish for eulachon, to Gilford or Village Island to dig for clams, and up and down the channel between Vancouver Island and the mainland to fish.

It is difficult to approximate the Kwakwaka’wakw population living in the Broughton Archipelago, the area of my study, at the time of European contact. It is known, though, that these numbers plummeted after 1860 when, like everywhere else in North America, smallpox and other diseases ravaged villages and killed indiscriminately (Kirk 1986). For example, the village of Fort Rupert was said to have decreased from 3,500 residents to 100 due to a smallpox epidemic (Healey: 20).²⁰

What is known is that there were more occupied villages in the past than there are today. Villages once existed at Hopetown, Village Island, Wakeman Sound, Turnour Island, Blunden Harbour and New Vancouver.²¹ New Vancouver and Turnour were reported to still have had residents in the 1960’s (Rohner and Rohner 1974). Today, one

²⁰ According to the government census from 1834, there were anywhere from 5 to 24 houses or 200 to 600 people at each of the Kwaiult villages in the vicinity of the Broughton Archipelago, south of Fort Rupert and the area in which I conducted my research (Rohner 1967: 36, 39). These numbers were more than double two years later in several villages and substantially lower again by 1885. The wild fluctuation in numbers between 1834 and 1836 is somewhat surprising and perhaps indicates poor census taking more than anything else. Given the Kwakiutl were seasonally nomadic, it is possible that the increase in numbers reflects census counts at times when the village was occupied for harvesting of a resource compared to times when it was not. Then again, it may represent actual growth.

²¹ Village sites changed over the years and there are a number of traditional villages I have seen listed in anthropological work such as Rohner (1967: 34) that I did not include here.

family is reported to live in Hopetown and the only other villages with residents are Kingcome Inlet and Gilford Island.²²

Today, the majority of Kwakwaka'wakw who live in the Broughton Archipelago live in Alert Bay on Cormorant Island. According to the 2006 Census, approximately 550 people live in Alert Bay, over half of which are native (Statistics Canada). Many of the native residents presently belong to the Nimpkish Band but originated from any number of villages in the area.

Alert Bay is a relatively new settlement. The area was uninhabited until 1885 when two white men leased the island from the Provincial government to set up a fish saltery (Healey 1971). To gain a labour force the men convinced a reverend to move to Alert Bay and set up a missionary school for native children. Other industry such as a saw mill started on the island. Not long after, the Province's Indian Agency office moved from Fort Rupert to Alert Bay. By 1894, the residential school that has caused so much hardship and disruption to Kwakwaka'wakw communities was established in the town (Healey 1971). At first, native children were forcibly removed from their homes in nearby villages to be brought to the residential school. The act was seen by the government and the church as a necessary and humanitarian effort to civilize the natives. The residential school has left a bitter impression on the First Nations, however, as the children were forced to grow up away from the older members of their families, brothers and sisters were separated, and all children were punished if they spoke in their native language. Stories I've heard from former students are about poor living conditions and

²² According to residents, the numbers of inhabitants in these villages used to be substantially larger than the current numbers living there today. Kingcome Inlet has approximately 100 residents today but they used to have over 200 people living there in the 1960's. Gilford's numbers have declined even since I first went there in 2004 when they were around 30. In 1935 and 1962, their numbers were said to have been up around 100 residents (Rohner and Rohner 1974:6).

mistreatment: being served breakfast porridge with maggots, cleaning whole stairwells with toothbrushes, being called “spawn of the devil,” sexually abused, locked in their dorm, the basement or a chicken pen outside for misbehaving. These schools are blamed for disrupting over two generations, during which time many did not learn their traditional language, cultural practices, or even how to act as parents. Today, Alert Bay and other Kwakwaka’wakw settlements are plagued with alcoholism, domestic abuse, sexual abuse, drug abuse and depression as they struggle to deal with their relationship with the “white” world and themselves.

Change has been a common part of recent Kwakwaka’wakw life. Adding to the momentum for change is the decline in fish stocks (especially the sockeye salmon which is a prized but historically plentiful food source), the arrival of television and the introduction of welfare. Several older individuals that I interviewed talked about the changes they had seen in their lifetimes in regards to the work ethic and lifestyle of their friends and relatives.

Their way of life was not completely disrupted, however. A number of children avoided being taken to residential school and, until recently, elders were still alive who grew up before the time of residential school. Although many of their cultural practices such as potlatches were outlawed for a long period of time, this did not stop them from performing these events. They did have to be altered, however, to avoid detection by the provincial authorities. Likewise, as many still lived in the villages they continued to live from the land and the sea. One of the clam diggers I worked with who was in his 40’s remembered staying with relatives in dirt-floor smoke houses (the more recent equivalent of a long house) as a child and said many elders his grandparents age never drank

alcohol. Many people who live in the villages and in Alert Bay still eat traditional foods, although fewer and fewer are involved in its harvest. Likewise, while many attend potlatches and feasts today despite large numbers of these people not knowing what is being said by the elders when they speak their native language or the significance of the dances and rituals taking place.

There is a strong sense of community among the Kwakwaka'wakw. Alert Bay and the smaller villages can easily be characterized as *gemeinschaft* communities where people not only know each other but where gossip flies faster than a person can walk home from the bar (as one informant told me). As suggested above, these communities can also be characterized as having a relatively high level of transience. Many family members have moved away from the area into larger urban centers and many of these return to Alert Bay or the villages a few months or years afterwards. Even in the short amount of time I have known people, the list of "permanent" residents in Gilford Village has changed dramatically and there is a persistent flow of temporary residents in and out of the village. These include grown children who move home to stay with their parents, aunts and uncles or grandparents and those who are working in the village on a temporary basis (e.g. clam digging, logging, tree planting). Many of those who move away from the region keep in close contact with relatives and travel to the area on a regular basis. They also serve as inn keepers for friends and relatives traveling or moving into the bigger urban centers or to locations where outside feasts and potlatches occur.

Due to the historical relationship between "whites" and natives there is ongoing tension between these two groups. Racism in Alert Bay on behalf of the white inhabitants is not as strong as it apparently once was. That said, a racial divide still

clearly exists and is a source of tension for many Kwakwaka'wakw when interacting with white people. This tension is magnified tenfold for many Kwakwaka'wakw when interacting with a white person in a position of authority, especially a representative of the government. DFO employees are particularly disliked and treated with caution. The reciprocal side to this is that many Kwakwaka'wakw either feel inferior to white people and what they know or feel angry towards white peoples' ways and their strange, short-term logic. Often these feelings co-exist within the same individual.

Disruption in their communities as well as becoming integrated into the Modern provincial- and now global market has undeniably affected clam digging. Boas provides us with scant information about clam harvesting. According to some anthropological accounts, though, clam digging used to be primarily the work of women and children. If this is true, it was before the days of the clam diggers with whom I spoke. With few exceptions, most present-day clam diggers talk about clam digging as a man's work. In fact, they talk about clam digging as being done by those who "have a strong back but a weak mind." By this they (and I) are not meaning to suggest that women are too smart to dig for clams. Instead, they are indicating that clam digging is (presently) a low-status occupation for men where all that is required is physical strength. Some women dig, but these women are described as "tough" women.

Older diggers had stories about whole families who went digging together in their younger days and being shown by their *grandparents* (as opposed to *grandfather*) how to dig for clams. One of the oldest diggers told me a story about traveling by canoe with his grandmother across the channel from their village to dig for clams. They filled the canoe bottom with clams and then rowed home.

My guess is that the transition of clam digging from a more gender-neutral activity to a male-dominated one was complete by the 1950's. I can not accurately trace out how this transition occurred, but I do know that there is a strong feeling among many Kwakiutl today that closely mirrors other Canadian's not-so-distant thoughts towards the gendered division of labour: men are supposed to be the breadwinners and women stay home to bake and attend to other things needing to be done around home. Given the hierarchical nature of their society and the recency of belonging to a cash economy, I think the association between men and clam digging (one of the first ways in which these communities were tied into the cash economy) emerged only when the larger Canadian society into which they were being integrated imposed a new status/reward structure on familiar tasks. I am sure other Eurocentric ideas of gender are part of the explanation as this could explain why women are, for the most part, not longer considered "tough" enough to dig for clams when women from older generations are described as walking around barefoot all year round. The disruption of family life through the educational system and DFO-imposed regulations on clam digging, discussed below, also likely played a role.

One interesting aspect of the Kwakwaka'wakw history in relation to clams that is definitively known is their use of aquaculture. A huge number of clam beaches have been engineered by the Kwakwaka'wakw. These beaches, referred to in Kwakwa'la as *lúxwxiwey* (place of rolling rocks together) (Natural Resources Canada 2009) and in English as clam terraces or clam gardens, are essentially rock walls composed of large rocks that have been rolled into place over generations of clam gardeners. These rock walls effectively extended the area of beach that was hospitable to clams by creating a

barrier behind which silt was deposited when waters passed over the wall. Using this method, small steep beaches could slowly be transformed into flatter, larger beaches with better clam habitat. I do not know anyone who still maintains the walls on these beaches but the practice is still within living memory of some of the older diggers. Although the practice is abandoned, many of the beaches are still good clam-producing beaches and therefore frequented by diggers.

Nuu-chah-nulth or Nootka

The Nuu-chah-nulth, formerly known as the Nootka (so labelled by Captain Cook (Efrat and Langlois 1978)), occupy the west coast of Vancouver Island. Today, many refer to this area of the island as “the wild west coast” as it is exposed to the open ocean. Consequently, most Nuu-chah-nulth villages—at least those used in winter—were (and continue to be) in sheltered areas such as inlets or “sounds” containing clusters of islands (Arima 1983). Like the Kwakwaka’wakw, the Nuu-chah-nulth were a people oriented towards the sea. Travel of any distance occurred over the water and food was obtained primarily from the sea or close to it. Also like the Kwakwaka’wakw, the Nuu-chah-nulth were not united as a single political entity. Instead, groups were organized by families with a chief as head, each of which is now commonly referred to as a tribe. Also like the Kwakwaka’wakw, the Nuu-chah-nulth territory is large. Those with whom I worked reside near the centre of this territory. This territory is divided even further, though, in the sense that single islands were associated with individual families or tribes (Arima 1983).

The west coast has an extremely rainy, windy climate. Many families had two or more settlement areas to which the family traveled when resources were available (Sproat 1987). Travel between camps was by canoe. Houses were semi-permanent and similar in structure to the Kwakwaka'wakw except that they had flat roofs. Feasting was a common activity in the winter months. Also like the Kwakwaka'wakw, their society was hierarchical in the sense that all individuals and families were ranked with the chief holding the highest position (Arima 1983). Not all goods were acquired through one's own work; bartering was conducted with neighbours as well as more distant tribes. In fact, one digger told me a story he had heard from his elders about how the Kwakwaka'wakw used to acquire butter clams from them previous to their building of clam gardens and for the exchange they received eulachon grease. Foods consumed included salmon, cod, halibut, ducks, clams, seal and a number of other seafoods, deer and elk, huckleberries, root crops and much more (Arima 1983; Marlor and Eyding 2005). Clams were said to be harvested all year round except when the herring were spawning. Potlatches were an important part of community life but they were somewhat different from those of the Kwakwaka'wakw in that they were not competitions but intended primarily as entertainment and education (Arima 1983).

Population figures for the Nuuchahnulth are not accurate for the time of contact with Europeans. Claims have been made that there were 15,000 Nuuchahnulth at the time when European explorers were passing through their waters and that this figure dropped to 2000 in the late 1800's due to smallpox and warring, both associated with Europeans (Arima 1983; Kirk 1986).

The Nuu-chah-nulth have a longer, more involved history with European explorers and settlers than the Kwakwaka'wakw. The west coast was the site of the Cariboo gold rush and the fur trade. Like the Kwakwaka'wakw, missionaries moved into the area towards the end of the 1800's. A residential school similar to that in Alert Bay was established in Port Alberni, a distance from the islands where children's families resided and to which Nuu-chah-nulth children were forcibly removed. Combined, smallpox, warring, missionizing, and the introduction of new trade items and a wage-based economy transformed traditional Nuu-chah-nulth society into something almost completely different than what it had once been (Kirk 1986). Old systems of rank were lost due to the great number of indiscriminate deaths and old traditions were exchanged for new forms of organization. Language and cultural ideas were prohibited by those who were attempting to "civilize" the natives. Traditions and practices that continued were often a combination of the old and the new.

In terms of clams, I have been told that, in previous times, their clams were known far and wide as being the biggest, whitest clams on the coast. Clam digging is reputed to have been a Nuu-chah-nulth activity for even longer than it was an activity of the Kwakwaka'wakw. In more recent decades, though, clam digging lost favour to fishing as the latter was a more lucrative business. But the fish stocks (and the fishing industry) have since declined. Today, the Ahousaht Village harbour is described as "empty" compared to the 1970's and early 1980's when it was filled with fishing boats. Clam digging has since become more popular, but has not replaced the large incomes fishers once had. Instead, clam digging does nothing more than supplement anyone's income. This is especially true given that commercial digging days are restricted to

twelve or fewer in a season—dramatically fewer than those open to the Kwakwaka'wakw. This is because the commercial species they dig for is manila clams, not butter clams. Manila clams fetch a higher price on the market but are also a more common global product. To prevent flooding the manila market, which would result in the lowered cost of manilas, the local clam management board decided to limit the number of digging days. Clam digging for home and ceremonial use can occur on any day of the year, though, and clams continue to be a major part of many households' diets (Marlor and Eyding 2005).

The Department of Fisheries and Oceans (DFO) and British Columbia's Clam Fishery²³

The origins of the commercial clam fishery in British Columbia date back to the end of the 1800's. The first commercial landings in Kwakwaka'wakw territory are recorded in 1883 in Fort Rupert. I am unsure of when the first commercial clam digging occurred in the Broughton Archipelago, but it was likely not long after this. I do know that by the 1930's chief William Scow was acting the local clam buyer for B.C. Packers. He and his family traveled by boat around to the various clam-digging villages to buy clams. In those days, clams were placed in boxes and payment was by box. The only species of clam being bought at the time was butter clams because they preserve well.

The DFO was legislated into being in 1868. The department, then called Department of Marine and Fisheries, was and still is responsible for programs and

²³ Unless otherwise noted, the information in this section originated from Quayle and Bourne (1972) or material from interviews with DFO biologists, a DFO wild stock invertebrate manager, or native clam diggers.

policies related to Canadian coastal and inland waters. Their mandate included economic development, conservation, and safety, the latter of which takes form as the Canadian Coast Guard. Although the provinces have jurisdiction over foreshore areas, oversight of marine organisms—including intertidal clams—are the responsibility of the DFO. DFO, in other words, is responsible for clam management.

It is unclear of how much federal fisheries officers were involved in clam management in the early years of the clam fishery. There is mention of fisheries officers being in the area in the late 1800's. The first commercial clam landings were recorded by the DFO in 1900. It is clear, though, that since 1900 the DFO have imposed increasing restrictions on this industry including minimal size limits, time restrictions for harvest, areas in which diggers are allowed to dig (both license-restricted fisheries areas and closed beaches), and, occasionally, quotas for how much they are allowed to dig.

The majority of DFO restrictions have been imposed in recent decades. In 1989 a special fishing license was created for the clam fishery. In 1991 a decision was made to “rationalize” the fishery (Gillespie 2000) and, in line with this, restrictions were put in place in 1997 to limit the number of licenses issued. This reduced the number of all license holders by 50% and to almost 0 in some native communities as most diggers had not bothered to obtain licenses previously and the new restrictions were affected diggers who had been licensed for the least amount of time. Not unsurprisingly, native communities reacted against the new regulations and began negotiating with the DFO for special licensing provisions. Shortly after, a new licensing system was created for First Nations called the Aboriginal Clam License (ACL).

From the First Nations perspective, DFO fisheries regulations have been impositions upon their way of life and traditional methods of harvesting. Older Nuu-chah-nulth and Kwakwaka'wakw diggers talked about traveling more widely to dig for clams in older days and Kwakwaka'wakw diggers talked about digging at more times of the year. But not all changes have resulted in a loss of power—at least, not for the Nuu-chah-nulth. A number of trial community management boards were established, including one in the Nuu-chah-nulth territory (Fisheries Area F). This board allowed greater involvement of communities in the management of clams. It is them who further restricted clam harvesting time from a number of months to 12 or fewer days per year.

From the DFO's perspective, increased regulations were required so as to ensure the sustainable harvest of clams. Clam landings reached a high in the 1980's. A number of beaches had been removed from the fishery around the same time due to contamination which resulted in fewer beaches available for harvest. Feeling within the DFO was that the clams were being over-exploited and restrictions needed to be put in place to prevent the collapse of the clam stocks (Gillespie 2000).

Consistent with the Aboriginal Clam Licenses and in recognition of their traditional occupation of the land and way of life, the DFO has formed a number of special arrangements with First Nations in regards to fisheries. In 1990, the Supreme Court of Canada released its decision in *R. vs. Sparrow* in which the court held that, after conservation and other “valid legislative objectives,” Aboriginal rights to fish for food, social and ceremonial purposes have priority over all other uses of a fishery (DFO website). In keeping with this, the DFO created an Aboriginal Fisheries Strategy that allows for the DFO and aboriginals to enter fisheries management relationships. In some

areas this has resulted in aboriginal communities having considerable autonomy over local fisheries management. In other areas, such as where I worked, it has meant the formal designation of locally-hired natives, hired through their tribal officers but funded by the DFO, to act as fisheries guardians. These positions are an intermediary between First Nations communities and the DFO. Guardians are responsible for issuing clam licenses and monitoring clam digging activities, among other things. They do not, however, have the same authority as fisheries officers; a guardian cannot charge someone for violating fisheries regulations but is obligated to report the breach to a fisheries officer.

One last topic needs to be mentioned in regards to the clam fishery as managed by the DFO. There are two branches of DFO clam management: the wild fishery and the depuration fishery. The wild fishery is managed out of a management office, separate from the science office, and managed by people with management but little, if any, science background. The majority of B.C. clam beaches are part of the wild fishery.

As the manager I interviewed described it, clam management is done “on the fly” or “by the seat of his pants.” There are no official formulas or quotas set for clams. Instead, clams are managed by openings and closures with the goal of obtaining the same amount of clams per area as previous years. He manages based on the information he collects from buyers, harvesters, fisheries officers and clam landings. In clam digging season he is in contact with people from the industry on a daily basis. He talked about management as dynamic—making adjustments as new information comes. When he first started managing, there used to be more fisheries officers who spent more time monitoring the beaches and clam digging activities. In recent year, severe budget cuts

have resulted in fewer fisheries officer positions and less time for monitoring. The result is he has had to rely more on commercial diggers and others' reports than he did previously.

The other DFO branch, the depuration clam fishery, controls the management of beaches that have been removed from the wild fishery due to being semi-contaminated from sewage pollution. Clams harvested from these beaches can be consumed but only if they have been placed in filtering systems for 48 hours. Due to the potential safety issues related to this fishery, each beach is individually managed.

Management of the depuration clam fishery is overseen by the science arm of the DFO. Biologists presently manage in accordance to a sliding scale they developed. The scale is based on research the conducted and happens to be the project I focus on in my analysis of the DFO in this dissertation. The scale is applied by determining the approximate abundance of clams on a beach, as assessed by a clam survey, which is then used to determine a harvest rate for the beach.

Salmon Farming in British Columbia²⁴

Canada's fishery is not known for clams. On the east coast, Canada was known for cod and on the west coast it was known for salmon. Recent years have witnessed a collapse of the cod stock on the east coast and a similar crash in salmon populations on the west coast. Coastal communities, First Nations and otherwise, have suffered

²⁴ Unless otherwise noted, the material in this section originates from Peter A. Robson's *Salmon Farming: The Whole Story* (2006) and interviews with native clam diggers.

accordingly. As a partial remedy, the Province of British Columbia has been promoting aquaculture, specifically salmon- or, as they prefer to call it, finfish farming.

The practice of raising commercial fish in captivity has a long history, but the history of raising fish through to maturity, at least in B.C. waters, is relatively new. As it is now practiced, juvenile fish are placed in large, floating pens anchored to the sea bottom in protected coastal waters and kept there until they are ready for commercial sale. Feed, hormones, medicine and anything else they require is distributed into the pens on a regular basis. Extra netting is placed around the edge of the pens to prevent predators (e.g. seals, sea lions) from attacking the fish.

Fish farms are a jurisdictional nightmare. Officially, the federal government is responsible for large bodies of water and the organisms in them and fish farms are clearly related to fisheries. The provinces, on the other hand, have jurisdiction over agriculture and the foreshore. Technically, fish farms are anchored to sub-tidal Crown land—too far out to be considered part of the foreshore. They are, however, an economic development initiative related to agriculture. The present administrative arrangement in regards to fish farms is a shared arrangement between a number of Provincial and Federal Ministries, with the Province's Ministry of Agriculture and Lands taking the lead.

Despite not being the lead agency for fish farms, DFO has a clear role to play in terms of these endeavours—at least, it is clear in terms of the legislation. The 1977 *Fisheries Act* stipulated that DFO has a mandate to protect against the harmful alteration, disruption or destruction (HADD) of fish habitat and the deposit of deleterious substances in water frequented by fish. Yet DFO has not always seemed to act in accordance with this legislation. For example, in April 2005, a government-appointed investigation into

Fraser River sockeye management concluded that DFO had not monitored the fishery in a meaningful way and was not properly enforcing the *Fisheries Act*. Rightly or wrongly, DFO has publically been held responsible for the East Coast cod stock collapse and is even seen by insiders to the organization as biased towards commercial and sport fishing. Until recently, DFO has put few resources into the “science” required to effectively determine whether fish farms were damaging the habitat of wild species and therefore seem as not uphold their role as enforcer of the *Fisheries Act*.

DFO’s initial lack of effort in terms of protecting habitat from the effects of fish farming should perhaps come as no surprise. Although DFO is also mandated as an environmental protection agency, the language used by DFO employees—including their scientists—is one of the market. Clams, for instance, are referred to as “product.” DFO has had an extremely difficult time developing reliable methods for predicting salmon stock returns. In a single year they can be out by millions of fish. Salmon farming represents an easier method by which to manage fish; the fish are contained in pens and one can count the number of fish in each pen. Even though epidemics have been known to wipe out the stock of an entire farm, this is still immediately knowable and potentially preventable. As one of the DFO biologists I interviewed said, one day people will realize that fish farms are the way of the future.

Fish farming has a relatively short and initially uneven history in British Columbia. Fish farming had a humble beginning in B.C. in 1971 when one experimental farm was opened. The same farmed closed in 1976. Ten more farms opened in various locations along the coast by the end of the decade. Among these was one near Alert Bay in the Broughton Archipelago. By 1988 there were 101 salmon farming companies with

118 active farms. Annual production of the farms was 6,600 tonnes (7,260 tons) with a landed value of almost \$40 million Canadian. 1989 was a bad year for the B.C. fish farm industry, though, as global salmon prices fell and many B.C. farms (approximately half) were forced out of business. After this, multinational companies began taking over the business. By the mid-1990's, the industry was controlled by 17 companies that owned 121 tenures. Production in 1995 was just over 27,000 tonnes (29,700 tons) and the harvest had a wholesale value of just over \$170 million. By 2005 there were 12 fish-farming companies operating in BC. More than 80% of the 128 tenures and 90% of the production of farmed salmon in BC was controlled by four foreign-owned companies. These companies were among the top salmon-farming companies in the world (Robson 2006).

At present, farmed salmon owns a bigger share of the market than wild salmon (\$202 million compared to \$52.5 million in 2004). BC produces about two-thirds of Canada's farmed salmon and about 5% of the world's annual production. Most of the farmed salmon from BC is exported (85%) and most of this is sold to the US (90%). Most is sold fresh as whole fish with the head on but gutted.

The above figures may represent financial success for a new-found industry but they are not without critique. Fish farming has raised considerable ongoing criticism by wild fishers and the BC public. Although fish farming was initially intended to complement wild fishing, wild fishers I spoke with claim fish farmers dumped their product on the market at the same time as wild salmon were being taken to market. The farmers sold their fish at incredibly low prices as so to out-compete the fishers and force the fishers to lower their prices. The result was that fishers did not profit from fishing for

that year and many could not pay what they owed for their boats, moorage, equipment and hired help. This is but one concern raised about BC's fish farming industry.

Another controversial topic in regards to fish farming in BC is Atlantic salmon. In 1984, DFO allowed Atlantic salmon eggs to be imported into Canada's Pacific waters. Atlantic salmon are better suited to farming than Pacific species and have since become farmers' species of choice. Atlantic salmon are not native to BC waters. The public have raised questions as to whether Atlantic salmon will disrupt wild salmon species stocks if they escape from their pens and reproduce.

The public and fishers' concern about fish farms also spawned from the type and amount of regulations government over fish farming. As it was a new industry, the government did not make an honest start at developing regulations for fish farming until the 1980's. Fishers felt the fish farm industry was being given preference over the wild fishery and that the wild stocks and their habitat were not being protected from fish farms. Among other things, many fish farms were situated in wild stock migratory routes, forcing the wild fish to make adjustments to their routing. In 1985, the United Fishermen and Allied Workers' Union called for a moratorium on new salmon farm sites. Additional public concern was being voiced in terms of the lack of research and monitoring of the environmental impacts of fish farms. This led to a short-lived moratorium on the approval of new farm sites in 1986.

The early years of the fish farm industry were plagued by poor management decisions, both on behalf of the industry and the government. These continued to fuel fishers and public anger against the farms. In the late 1980's the provincial government undertook a Coastal Resource Inventory Study in the Broughton Archipelago. The

Ministry of Environment and then-Ministry of Agriculture, Food and Fisheries held public consultations in which they asked commercial fishers, First Nations, tourism operators and local interest groups in the Broughton to identify areas they did not want to see farm sites established. These included productive commercial fishing areas for cod, halibut, salmon and prawns, as well as sensitive environmental areas. The government used this information to draw maps dividing the Broughton Archipelago into colour-coded zones: green indicated an acceptable area in which to establish a fish farm, yellow indicated a possible site but one requiring caution, and red indicated an area where no fish farms should be. Despite the time, effort and intent behind the maps, a number of farm sites were approved in “red” areas. This understandably led to feelings of distrust and frustration by Broughton Archipelago residents against the provincial government and negative attitudes towards fish farms.

Spearheaded by environmental groups, public opposition against the fish farms continued to grow in the 1990’s. In response, the provincial government imposed a second moratorium on new farming siting and commissioned the provincial Environmental Assessment Office to undertake a public review of the industry. Their report, *Salmon Aquaculture Review*, released in 1997, claimed that fish farming in BC represented a low overall risk to the coastal ecosystem but had some negative effects at select farm sites. It also stated that not enough research had been conducted on the impact of fish farming.

The seven-year moratorium was lifted in 2002. Since the moratorium, 14 new tenures have been granted. In 1999, the provincial government created more stringent guidelines for salmon farm siting. In 2003 these guidelines were updated. The granting

of any new tenure must now be approved by the *Canadian Environmental Assessment Act*. New regulations also made it mandatory that sites be “fallowed” when waste levels exceeded established guidelines.

The most recent debates around fish farms are in relation to farm waste and sea lice. Large numbers of sea lice, an external parasite that attaches to fish, are found near fish farms. First Nations, fishers and independent scientists have been raising the alarm about sea lice. Independent scientist Alexandria Morton claims sea lice explain the historically low return levels of pink salmon in 2003. She argues that sea lice are attaching themselves to salmon fry as they swim past farm pens on their way to the open ocean waters. While grown salmon are large enough to outlive the impact of sea lice, salmon fry are not. She is claiming that the reduced numbers of salmon returns are in part due to the effect of sea lice on salmon fry.

First Nations Responses to Fish Farms

As already mentioned, many fishers and the public have a negative attitude towards fish farms. Although it varies by region, many First Nations are adamantly opposed to fish farming. This is no more true than with the Kwakwaka’wakw in the Broughton Archipelago. As of 2007, the majority of operational fish farms in BC were sited in waters between the Broughton Archipelago and Queen Charlotte Strait, near Quadra Island, and in Clayoquot Sound (Robson 2006). The first two of these areas are Kwakwaka’wakw territory and the third is Nuuchah-nulth.

As suggested above, the Kwakwaka’wakw have particular reason to be opposed to fish farms. Not only were they one of the first sites for fish farming but the experience

over the Coastal Resource Inventory Study left a bad taste in the mouths of many Broughton Archipelago inhabitants. In addition, a large number of the Broughton farms were established early in the history of BC fish farming, during the experimental days when farmers still had much to learn about environmentally-appropriate siting. The result is that Kwakwaka'wakw almost unanimously want fish farms out of their territory. The Musgamagw Tsawataineuk Tribal Council, representing four of the Broughton Archipelago Kwakwaka'wakw tribes, has a full-time staff member dedicated to protecting the marine environment. One of his main tasks is to raise awareness about the negative impact of fish farming.

The Kwakwaka'wakw dislike of fish farms run deep. I know of at least two public protests they have held in opposition to fish farms in their area. They have voiced their desire to the government for fish farms to be removed from their waters on numerous occasions. In fact, the reason why the contaminant ecology team with whom I worked became involved in their project was due to the vociferousness of Kwakwaka'wakw about fish farms in their area. As scientists, the Principal Investigator and the scientist whose lab was used for the research were interested in doing science that had public utility. A project on fish farms was particularly appealing to them given the high level of public concern in British Columbia over fish farms. Although at that point the DFO and the province had done little research on the impact of fish farms themselves, the province dismissed Kwakwaka'wakw protests over fish farms by claiming that their evidence for the negative effect of fish farms on clam beaches was not scientific and therefore not valid. Kwakwaka'wakw administrators and scientists then decided the best

approach was to partner with scientists on research that they expected would clearly show that fish farms have a negative impact on clams and clam beaches.

In addition to their severe dislike of fish farms, many Kwakwaka'wakw fishers and clam diggers have a negative view of the DFO. As a retired digger who had been consulted for the Coastal Resource Inventory map said to me:

SB: Government doesn't know how to manage anything. They say they have no money, but they buy all sorts of machinery. Yet they don't have the money to go pull signs off beaches that state they are closed. Then they think they are looking after a beach by putting up a signing saying it is closed. There was a condemned float house that they dealt with by putting up a sign on. This didn't help solve anything. DFO is useless. They don't have any first hand anything [knowledge, experience].

This digger was not alone. Most diggers feel mistrust and anger towards the DFO.

Although united in their dislike over fish farms, they provide mixed reports regarding how the fish farms impact the clams. Those holding administrative and political positions tend to claim that the fish farms are turning the clams black. The clam diggers, on the other hand, will tell you that black clams result from dark mud or sand—there were black clams long before there were fish farms. Many also say, though, that the beaches have become muddier since the fish farms have been in the area (hence more black clams). All are concerned about the high volume of feed and other materials being deposited into their waters via the fish farms and say that the number of clams has decreased. They also say this is true for halibut, whales, ducks and a number of other species. The explanations for the declines differ from one digger to the next, but all argue that fish farms have a negative effect on the environment.

Perceptions of fish farms and fish farming are more mixed among the Nuu-chah-nulth. For one thing, the influx of fish farms into their area (Clayoquot Sound I particular) occurred more recently than in the Broughton after farmers had more knowledge about how to site, anchor and run farms. Moreover, jobs are scarce in the islands of Clayoquot Sound and fish farms provide jobs that a few highly value. When a research partner and I interviewed 34 Ahousaht diggers during a survey about clam consumption, less than half said they were concerned about fish farms affecting clam beaches. In fact, two diggers even claimed that fish farms enhanced the ecosystem through the feed being added to the sea water. The diggers who thought the fish farms had a negative impact on the clams were adamant about it and claimed that others were afraid to say anything negative about fish farms because fish farms provide jobs (Marlor and Eyding 2005).

One point that almost all diggers were in agreement over was that some beaches were now “dead.” The explanation diggers gave for why beaches were dead varied by beach; in some cases, near-by fish farms are the cause, for other beaches they talked about fish farm “morts” (fish that die due to disease or other causes while in the fish farm pens) being dumped nearby, at others they identified creeks on or nearby the beach with run-off from open pit mines, logging activity and/or sewage from houses. The main source of contamination for the majority of dead beaches was believed to be close proximity to fish farms.

Chapter 3: Introducing the Practitioners and Their Work

This chapter serves as an introduction to the various knowledge practitioners and their work with clams. In it I provide a detailed description of the type of work in which each set of knowledge practitioner is involved, including, if appropriate, the exact study I observed or partook in; the social organization of their work; and the form their eventual conclusions took. In many ways, the content of this chapter serves as the readers' most direct access to the underlying "data" upon which my analysis is based; the majority of analyses presented in this chapter is based on decisions I made regarding what to include, exclude, highlight and cluster from my ethnographic experiences and from resources provided to me by the knowledge practitioners such as research reports and computer analysis files. The chapter contextualizes the various knowledge practitioners and their work in a way that I do not repeat in my later analysis. This contextualization can help orient the reader to what I and the various knowledge practitioners were involved with, either independently or in conjunction with one another. Such information allows the reader to be more critical but also more informed about the analysis that follows. The descriptions can be thought of as more than just "data", though, as these detailed descriptions summarize four complete ways of knowing.

The chapter is organized into five main sections. The first four sections focus on the knowledge practitioners with whom I worked. The first two sections describe the university-based



Figure 3.1. Butter Clam

team of contaminant ecologists and the invertebrate biologists from Canada's Department of Fisheries and Oceans (DFO). The third and fourth sections provide a description of the two sets of First Nations clam diggers, the first being members of the Nuuchahnulth Nation and the second are members of the



Figure 3.2. Manila clam

Kwakwaka'wakw First Nation. Both sets of First Nations originate from and all knowledge practitioners reside in coastal British Columbia, Canada. All the practitioners study intertidal clams on Vancouver Island, British Columbia and work with at least two of the following three species: butter (*Saxidomus giganteus*) (Figure 3.1), little neck (*Protothaca staminea*), and manila (*Venerupis japonica*) (Figure 3.2). In the fifth section of this chapter I focus on the final knowledge practitioner involved in my project but not yet discussed—me, the author and the ethnographer. In this section I describe what others would call my “methodology”: the what, when, where, and why I did to go from observation to written report.

The Contaminant Ecologists

The Research Proposal

For many people, academic scientists are the prototypical model of the knowledge expert. Not only are they held out publicly as being empirical experts, but almost everything in their daily routine is somehow related to knowledge: they read the findings of others' research, they impart knowledge to students through teaching, they fill out payroll sheets for those who assist them in their research, they analyze data, and they

write grant proposals. The contaminant ecologists were no exception. They, too, focused their work around the creation, evaluation and transmission of knowledge. All senior members of the team had worked many years to receive their Ph.D.'s in biology or a closely related field and all continued to do research in biology as part of their employment. Like their colleagues, their main interest was in producing research articles that could be published in peer-reviewed journals. This particular set of ecologists was also interested in creating knowledge that would be helpful for local communities. Driven by a particular ethic related to the relevance of research, this latter interest was particularly important in regards to the type of knowledge they aimed to generate. Regardless of their ethics, their work—like their peers—was consistently oriented towards knowledge.

The faculty member who initiated this particular project research was a senior research chair at a major university in the region. Previously he had spearheaded previous research projects on the quality of their drinking water in a number of nearby communities. He, along with other academics, had been contacted by Kwakwaka'wakw political leaders and asked if he would be interested in conducting a community-based study about the impact of fish farms on clams on their beaches. According to the Kwakwaka'wakw political leaders, fish farms were causing clams to turn black and unhealthy to eat. The native band wanted scientists to test the scientific validity of the Kwakwaka'wakw claims. The B.C. government, the licensing body for the fish farms, said the First Nations had no scientific evidence that fish farms were damaging clams and clam beaches and therefore refused to make any changes to the fish farms. The senior

research chair was invited to attend a meeting to discuss research possibilities on the topic.

A number of researchers affiliated with clams and marine life were also present at the meeting which the senior research chair attended: a government biologist, a small number of current and former Kwakwaka'wakw clam diggers (one of whom was the chief of the territory in which the majority of clam digging activity occurred), Kwakwaka'wakw fisheries guardians (local managers and monitors of DFO fishing regulations), a biologist and his assistant representing a nearby band, a biologist working for a provincial Aboriginal fisheries organization, a second contaminant ecologist who was working as a post-doc at another major university in British Columbia, myself, and another social science graduate student who had just finished writing her thesis on fish farms in the area. From the meeting emerged a small team of researchers (the senior research chair and post-doc and the biologist from the Aboriginal fisheries organization) who agreed to write a research grant and to do some form of research on the relationship between clams and fish farms.

In developing their project, the contaminant ecologists wanted to include additional clam-harvesting First Nations communities who live in the vicinity of fish farms. Their reason to do so was to broaden the scope of their research to a wider geographical area. They began negotiating with two additional First Nations communities: the Nuu-chah-nulth and a band from the north coast of British Columbia. In conjunction with these two additional communities, the contaminant ecology team submitted a joint research proposal to two large Canadian organizations in which they proposed research exploring whether there was a connection between salmon fish farms

and contaminant levels in clams. The proposal included a list of the contaminants they would test for, the number of test sites they would use and the general procedure by which they would collect, transport, store and analyze the clams. The research proposal named the post-doc as the Principal Investigator (PI), meaning he would have ultimate responsibility for the project.

Prior to the initial meeting in Kwakwaka'wakw territory, the contaminant ecologists had no particular interest in clams—they had not done any previous research on clams, they had not dug for clams, they did not eat clams (or if they did, they bought them from a store), and they did not live in the areas in which the clams and the farmed fish were located. At the point of submitting their research proposal, none of the scientists on the research team had physically stood on any of the clam beaches in question. Their only exposure to the beaches, clams and waters were boat and ferry trips to and from two of the First Nations communities (which were not in close proximity to the clam beaches) and viewing a few clams and beach sediment that had been collected by fisheries guardians immediately prior to the initial meeting. The research chair had, however, seen satellite images of the Broughton Archipelago (Kwakwaka'wakw territory) as part of the data he used in another research project. They chose to study clams for three reasons: (i) because First Nations consume them, making clams an important community interest; (ii) clams are considered good biomonitors in that they are relatively sedentary creatures and thus considered good indicators of the level of contaminants in local waters; and (iii) fish farms are a contentious issue in British Columbia and research in this area would be of considerable interest to the public, allowing the researchers' work to easily gain public attention.

Despite having no previous experience with clams and limited experience with fish farms the contaminant ecologists felt competent in undertaking a study on this topic. As ecologists they were trained to study the relationships among organisms and their environments; while knowledge about the specific organisms is important, their competence was rooted in (i) their understanding of the basic ways in which organisms are inter-related and (ii) how to study these relations. Their general approach can be described as a systems approach; beaches, waters, organisms and other marine phenomena are considered part of an ecological system. The system is conceived as a series of inter-linked components which act as “mechanisms” within the system, holding the system together and acting on the various parts of the system. Phenomena from outside “the system” are described as inputs—materials or organisms typically imported through anthropogenic means that originated from another system, such as manufactured fish feed deposited in the waters by fish farmers. In this line of thinking, all marine systems are conceived to be similar in design. All one needs to do to design a study is thus understand how marine systems operate and, in this specific case, identify mechanisms that could link the fish farms to a causal change in clams and clam beaches.

Perhaps unsurprising, the contaminant ecologists thought the best place to start looking for a mechanism that linked fish farms to clams was contaminants, the area of their expertise. They were interested in heavy metals (e.g. zinc, cadmium, copper, mercury, and lead), pesticides, anti-biotics, and persistent organic pollutants (POPs) (e.g., polychlorinated biphenyls (PCBs), dioxins, and polybrominated diphenyl ethers

(PBDEs)) contained within fish feed and the various stable isotope ratios found in the carbon and nitrogen in clams.²⁵

Their focus on contaminants as a mechanism made it even less relevant, in their eyes, to have had previous experience on the various First Nations' clam beaches. Contaminants of this sort are not visible to the human eye or detectable by other human senses. Moreover, the procedure by which contaminant ecologists isolate and identify contaminant levels in tissue is standardized for all organisms, regardless of species or origin. Instead, what they considered important was reviewing available academic literature on clams, on what is known about contaminants in shellfish, on the scientifically-documented impact of fish farms on other species, on human health risks from seafood consumption, and on coastal food webs²⁶.

Organizing the work

After the team had been awarded funding for the project, they gathered for a project planning meeting. Present at the meeting was a representative from the main funding agent to discuss the funding of the project and limitations on how the funds could be spent. Also present at the meeting were several Nuuchahnulth band council members, Nuuchahnulth fisheries guardians, an elder, and a non-native biologist hired by the one of the First Nations bands, a biologist working for the local fish farm company and two graduate students (myself included). Present by phone was a second non-native biologist, hired by the Northern coastal band involved in the project.

²⁵ Stable isotope ratios are affected by the type of food consumed by the organism.

²⁶ *Food web* is an ecology term used to describe the network of relations between species that prey on each other.

The goal of the meeting was to communicate information to all the parties involved, for the parties to discuss concerns about the research, to further develop the design and decide exactly what “deliverables” were to be produced by the project. The majority of the meeting focused on a discussion about the contents of a protocol agreement, an agreement between the various parties covering the rights and responsibilities and understandings of each party, such as who has the rights to data, who was responsible for what work, the work schedule, and the deliverables of the project. Early in the meeting one of the Nuu-chah-nulth band council members complained that the majority of extra or “matching” funds (additional funds required by the project that were not covered by the research grant) being supplied to the project were being provided by the First Nations communities *and* that, despite this, the discussion up to that point had mainly been about what everyone else, including graduate students, would get out of the research—not how the First Nations communities would benefit. At that point, discussion turned to deliverables for the community. Among possibilities discussed was a lay manual for how to conduct future contaminants research, “capacity building” for the communities (which included the hiring of one First Nations student and training band fisheries officers or guardians on how to collect samples), and the final report on contaminant levels in clams. The academics wanted everyone involved in the project to clearly understand that there was only a limited amount that could be learned in a year, the funding duration of the project, and that they would not be able to tell by the end of the year whether or not clams were healthy to eat. All they could tell was whether the clams did or did not have a particular type of problem i.e. whether certain types of

contaminants were present or not. This was less than what the members of the First Nations communities had hoped for, but all agreed it was a start in the right direction.²⁷

Included in the protocol agreement, finalized and ratified by all parties via email in the two months after the meeting, was a work plan and schedule in which tasks were assigned to particular individuals. The post-doc was the PI but he was not the coordinative administrator of the project. Instead, administrative responsibility was assigned to the biologist working for the provincial Aboriginal fisheries organization. She was to act as the central contact person and task master. With input from the PI, she had organized the project in a rational, bureaucratic manner. Stages in the process included designing the methodological details, sample collection, storage and transportation of samples, preparing samples for analysis, chemical analysis of samples, correcting the data, statistical analysis and interpretation of data, and writing of the report. The PI was responsible for several of these components. He was to finalize the specifics of the research design, conduct the fieldwork portion of the research (with assistance), and statistically analyze and interpret the data. A chemist permanently hired by the senior research chair for work in his laboratory was assigned to oversee the lab work on the samples. Members of the other parties were assigned smaller responsibilities such as assisting with sample collection. The senior research chair, the laboratory proprietor, was not involved in the research itself but was responsible for liaising with the funding agents and providing the equipped laboratory. Others were to be hired as needed: a graduate student from the lab, four Kwakwaka'wakw clam diggers, two Kwakwaka'wakw boat owners/operators and I were hired to assist with data collection

²⁷ The project was enough to answer a number of academic questions (i.e. enough information for several publications) but could not answer the question asked by the Nuuchahnulth project partners.

and three technicians with training and work experience in biochemistry were hired to prepare the samples. In addition, three Nuu-chah-nulth fisheries guardians provided transportation and digging as part of the Nuu-chah-nulth's in-kind contribution to the project.

Selecting beaches and finding clams

The first and second stages of the project involved finalizing the sampling design and collecting the samples. Samples were to be collected from beaches in all three First Nation communities. It was during travel to and the first day in the first community (a Nuu-chah-nulth community) that the PI made his decisions about how to collect the samples. Prior to his departure, he had decided what equipment he would require: five coolers for storing samples, dry ice, new plastic laboratory vials, a marine chart of the area, hexane-sprayed tin foil,²⁸ specially "cleaned" jars, and sealed plastic sampling collection bags. This equipment is relatively standard for all contaminant sample collection. A boat, gasoline and digging equipment were also supplied as an in-kind contribution by the Nuu-chah-nulth band. After arriving in the community, the PI used the marine chart and consulted with the Nuu-chah-nulth fisheries guardians, Nuu-chah-nulth biologist and the fish farm biologist to select the beaches from which he would collect the clam samples. Those he consulted were asked to identify two types of beaches: beaches they considered close to or affected by fish farms and those that were not, the latter of which he referred to as reference beaches.

²⁸ Hexane "cleans" the tinfoil so as to prevent cross-contamination from other sources such as grease or particles on the lab technicians' hands that transferred while he or she was cutting the foil into sheets.

The research budget was originally organized to allow clams from a total of six beaches at two different points in time in each of the three territories to be chemically analyzed. The fish farm biologist, a potential member of the project if the project was organized in a way that was acceptable to his company, pushed for collection of samples from additional beaches. He argued that the additional information would allow the First Nations communities to have a broad set of “baseline” data that they could then use in the future to assess whether contaminant levels had changed. The fish farm biologist suggested this was to the advantage of the fish farm company as well as it meant the fish farmers would not have to defend “unfounded” accusations that they were harming the clams. The PI was worried about budget constraints (lab analysis is costly and prevented extensive lab analysis), but he conceded to collect



Figure 3.3. Packaging a composite

clams from both “focal” and “screening” sites. Focal sites were beaches that were deemed either close to or far from fish farms where the full complement of clam samples would be collected: two composites²⁹ for each species of clam³⁰ for each size class (small and large). The “screening” sites also included beaches “close to” or “far from” fish farms but the samples collected from these beaches were to be of only one size class and of one clam species. These samples were to be stored in a freezer in the lab but to be analyzed at a later date. The PI thus had adjusted the sampling plan to fit the suggestions

²⁹ A composite was a single package of clam samples in which the clams collectively possessed a minimum of 150 grams of soft tissue.

³⁰ Manila and little neck clams in Nuuchahnulth territory and little neck and butter clams in Kwakwaka'wakw territory.

of the fish farm biologist. In large part this was an attempt to appease this potential research partner.

After the issue of focal and screening sites was settled, the PI, with the assistance of the Nuu-chah-nulth and fish farm biologists, made tentative decisions regarding which beaches to collect clam samples from, which ones were to be focal and screening sites, and which ones were deemed “close to” and “far from” fish farms. The following day, the plan was put into action. Early in the morning, while the tide was still dropping, the PI, the fisheries guardians and I traveled to the first beach to collect the clam samples. The PI organized and divided the labour involved such that the guardians and I dug for clams while the PI cleaned, sorted, selected and wrapped the clams as composites in foil and labelled each composite (Figure 3.3). During this process, the PI decided which clams were “small” and which were “large” and when we had enough clams for a composite. At any given beach we dug for clams until we had a full complement of samples or until the tide came in to wash over the beach. Upon returning to the village the PI placed the coolers with the samples inside a large walk-in refrigerator at the village grocery store (the only refrigeration unit nearby that was large enough), where he stored them until we left the village. Each day we would set out to a new set of beaches, collect a new set of samples and return to the village at which point the samples, in their coolers, would be added to those already in refrigerated storage. At the end of each sampling trip the PI transported the sample-filled coolers to the university lab for analysis. At this point, immediate responsibility for the samples was no longer his; upon his arrival at the lab he “signed in” the coolers and their contents. At this point the frozen clams, in their

shells, were placed in freezer storage until a designated lab technician prepared the clam for analysis.

Not everything went according to plan when collecting the samples. In the process of sampling on that first morning we encountered a stumbling block—one of the selected focal sites did not have many manila clams, making it too difficult to collect the composites required. The screening site we visited immediately after, however, had plenty of clams. As a consequence, the PI decided to switch the designation of these two beaches, thus making the second beach a focal beach. This allowed us to collect the required number of samples we needed.

This first sampling trip occurred in the early summer and encompassed a total of five days. The trip was somewhat costly as hotel accommodation, food and transport had to be provided for the PI and myself, in addition to equipment and other costs. Summer was purposefully selected as the first of two times to collect samples. At this time of year, the lowest tides occur in the daytime. This meant we were able to see what we were doing without much difficulty and in relatively pleasant weather.

The PI's plan was to collect samples from each beach in two distinct times of the year, summer and winter, to see if contaminant levels fluctuated by season. A second sampling trip of the same duration to this same region thus occurred approximately six months later, in the early winter when digging tides occur after the sun sets for the day. I did not accompany them on this trip but the other participants overlapped with the first sampling trip. Due to limitations in the amount of time they had in each community, the short duration of tidal exchanges, and the large number of samples they had to collect on each beach, however, they did not manage to collect the full complement of samples they

desired. As a consequence, they decided to combine the samples from the two time periods to create “complete” sets of samples and therefore were forced to eliminate seasonality as a “variable”. Once again, plans were changed to suit circumstances.

The contaminant ecologists undertook sampling trips in the two other regions as well. I joined them for the first of these trips in Kwakwaka’wakw territory, a few weeks after the initial Nuuchahnulth sampling trip, but not the second. During these trips they roughly followed the same sampling protocol as the PI had established for the Nuuchahnulth beaches.³¹ An exception, however, was in regards to the screening sites.

Somewhere between the first and second sampling trip, the PI changed his mind regarding the inclusion of screening sites. When collecting clams in the Kwakwaka’wakw territory, he collected clams from focal sites only. His decision to exclude screening sites was because he had such limited time/funds available for collecting the samples that he was forced to make some sort of cut in the protocol to have any hope of collecting the minimum number of samples he required. Weighing in on this decision was the limited amount of funds available in his budget combined with the fact that the fish farm company in the Nuuchahnulth region decided *not* to participate in the project.³² As no fish farm or designated fish farm biologist from the Kwakwaka’wakw area was ever involved in the project, this eliminated outside pressure to include screening sites.

³¹ A few problems were also encountered on these trips. For example, the outboard motor repeatedly died on the skiff (boat) being used to transport the team to the beaches to collect samples and, on one of the winter trips, their crew got lost in the fog.

³² This was despite the contaminant ecologists’ efforts to get them involved in the project.

Preparation of clams for analysis in the lab

The first step taken in the lab was preparation of the samples for analysis. The following describes the procedure employed by a single technician to prepare the clams for PCB/OCP analysis. Similar preparations were undertaken with additional clam tissue for detecting stable isotope ratios, fatty acids, and heavy metal concentrations. Two technicians were responsible for preparing the clams for PCB and OCP analysis throughout the course of the project, one having taken over the responsibility from the other part way through the project.

The technicians began the preparation by removing 20 grams of clam tissue from a single clam composite (Figure 3.4). The PI had initially asked that a mixture of clam flesh from a single composite be included in each preparation. His reasoning for that was that it meant he could get a more representative picture of the whole beach this way than if a single clam was used. The PI, however, worked at a different university than where the lab was housed and did not oversee the



Figure 3.4. Twenty grams of clam tissue

preparation of clam samples and his exact directions were lost or insufficiently communicated somewhere along the way. What actually happened was that the technicians simplified the procedure, selecting the largest clam from a composite to obtain the 20 grams required for a preparation. Only if required to do so to obtain the full 20 grams, did the technicians remove tissue from additional clams.

The preparation procedure was tedious and time consuming and involved a series of clearly-defined steps or procedures that the technician was required to follow. The technician prepared one batch of samples at a time. Each batch included a 20 gram subsample of clam tissue, in its own separate sample dish, from nine composites. An additional 40 grams of clam tissue was removed from one of the composites and divided into two 20 gram subsamples to be used as “replicates”—double checks on the preparation and analysis procedures. A “blank” was also prepared in which no clams were included but all other procedures were followed to produce a final “extract.” The blank was to be used to test whether any contaminants were being introduced from the preparation procedure. The nine prepared clam extracts along with the 2 replicates and 1 blank constituted a “batch.”

The technicians spent the following three to four days transforming the twelve 20-gram “samples” of clam tissue into clam extract, to which they added surrogates (known quantities of specific PCB congeners³³ and OCP’s or chlorinated pesticides) and other chemical compounds, some of which were subsequently removed when the extract was transformed into a solid or liquefied again (Figures 3.5-3.13). The purpose of adding surrogates was for comparison purposes; the surrogates were standardized measures of contaminants added at the beginning of the transformation of clam extract that could be used at the end of analysis to assess the impact of the laboratory process on the recorded level of contaminants. Different amounts of surrogate were added to the replicates for the same reason—to assess whether the preparatory procedure impacted the recorded levels of contaminants in the samples. In the second replicate, OCP was the only

³³ Elements from the same group in the periodic table.

surrogate added, and for the third, half the amount of PCBs was added (and no OCPs) compared to that used in the other subsamples.



Figure 3.5. Mushing clam sample

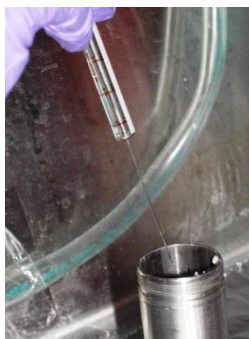


Figure 3.6. Adding surrogate



Figure 3.7. Creating clam extract



Figure 3.8. Clam extract



Figure 3.9. Extracting water from subsample



Figure 3.10 Distilling subsample into heavy and light PCBs



Figure 3.11. Removing lipids and separating



Figure 3.13. Reducing extract

Part of the technicians' job was to make sure each of the prepared samples could be identified with the particulars from which it originated. On the beaches, the PI had created and labelled each composite with codes identifying the beach, season and size classification of the clams. The PI's codes were the same ones as the technicians used in the lab for identification. At each new step of the preparation procedure the technicians labelled each new flask, test tube or vial in which the clam and its various incarnates of extract were placed with a code. The lab technicians worked "blind," however, in the sense that they did not know the meaning of the codes and therefore did not know the name of the beach or even the region from which the sample originated. Despite their "blindness," their diligent transference of codes from one vial to the next was the thread that held the project together despite the distance and context of the lab from the clam beaches.

Chemical and Statistical analysis of clam extract

The next stage of lab work was chemical analysis of the prepared samples. Once preparation was complete the technician handed the batch of subsamples over to the lab chemist, who was responsible for their chemical analysis. The chemist then placed the

batch in the lab's gas chromatography high resolution mass spectrometry machine—a large machine (see Figure 3.14) that transforms the various liquefied subsamples (Figure 3.15) into a gaseous state during an automated 24 hour analysis.³⁴ On the machine the chemist set parameters to test for the specific suite of PCB and pollutants in which the



Figure 3.14. Gas chromatograph with electron capture detector



Figure 3.15. Vials placed in gas chromatographer for analysis

contaminant ecologist team were interested, set other settings according to accepted protocols for ultra-trace analytical methods, and followed standard operation procedures developed and standardized by the US Environmental Protection Agency.

Analysis from a mass spectrometry machine produces a graphical read-out (Figure 3.16). The lab chemist examined the read-out for potential problems, such as unexpected and unexplainable spikes on the graph that could either indicate unusually high amounts of a particular pollutant in the samples or a problem with the machine in

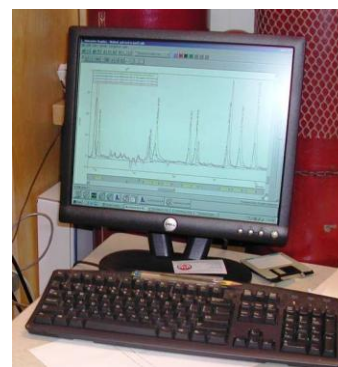


Figure 3.16. Digital display of analysis on computer screen

³⁴ The chemist also assessed concentration levels for heavy metals, stable isotopes, pigments and fatty acids using other machines and other clam preparations. Heavy metals were analyzed using an inductively-coupled plasma atomic absorbance spectrometry (ICP-AAS), fatty acids by gas chromatography mass spectrometer with flame ionization detector (GC-FID), stable isotopes by isotope ratio mass spectrometry (IRMS), and pigments by high performance liquid chromatography (HPLC) with diode array detector. Like with the PCB/OCP analysis, the chemist set the parameters on each machine according to acceptable standards in his field.

analysis. When an unexpected spike did occur, the chemist attempted to track down the cause of the spike to make sure it was not attributable to anything besides an increased level of contaminant in the clam itself. If required, the technicians were asked to prepare new clam extracts, the machinery was adjusted, and a new batch was analyzed. Once the read-out looked acceptable to the chemist, output from the analysis was exported as a set of numbers on a spreadsheet for statistical analysis.

When the time came closer to the deadline for the annual report to the funding agent and the chemists had enough initial batches of samples analyzed he forwarded his data spreadsheet to the PI for statistical analysis. What the PI received was “raw” data or data that was not yet ready for analysis. Included in the recorded concentration levels were the known quantities of the surrogates and the amounts present in the clams. To correct the data, the PI had to subtract the known amount of surrogate concentration from the figure to arrive at the level in the clam. The PI was then ready to begin data analysis.

This was the first time anyone had done research on contaminants from fish farms, so the PI was not sure what to expect in terms of the types of contaminants that he would find or their level of concentrations. His hope was to use the various concentration levels to create a continuous measure by which he could rank the beaches in terms of how much they were affected by fish farms. This proved problematic for all but the stable isotopes. The PI was expecting the concentration levels to differ among beaches, as each beach had different exposure to fish farms, but beaches deemed close to fish farms were expected to have higher levels of contaminants than beaches deemed far from fish farms.

He did not find such a distinction among beach concentration levels.³⁵ The PI was thus forced to revert to his second option for analysis in which he continued to use a dichotomous variable for the fish farms (i.e. close to or far from fish farm) and use a t-test to establish whether each farm site was statistically different from the average calculated for the reference sites in that region. In this way he could claim whether individual beaches “close to” farms were different (i.e. statistically significant from) the reference beaches.

While the PI was involved in statistically analyzing the initial set of data the lab chemist decided to switch methods he was using for OCP/PCP analysis. The chemist had been reading up on and talking with others in his field about recent methodological developments. As an outcome of these interactions he decided to switch to the new method. The justification was that this new method would produce more accurate results. The outcome of this decision was that the chemist had to re-analyze all previously-analyzed batches again using this new method. As chemical analysis took a considerable amount of time to conduct (i.e. months) it meant the PI was forced to submit the annual funding report based on the initial, incomplete data. It also meant, as far as the scientists on the team were concerned, that any results “found” in the data at that point were preliminary and subject to change. As the PI stated repeatedly, he needed *finalized* data to draw a final conclusion.

³⁵ Two potential reasons for this are that: (1) not all beaches deemed close to/far from fish farms fit their designated classification or (2) that one or more beaches deemed “far from” fish farms were exposed to some other form of contamination.

Meeting and report

Part way through the project year the project coordinator organized a team meeting for all parties. The meeting had several purposes, including the dissemination of preliminary results, the coordination of activities among parties, and an information session as to what the various parties had completed. In large part, the meeting seemed to be a vehicle for evening the playing field between the scientists and other party members, as the First Nations communities had, at that point, provided considerable in-kind funds, materials and effort without receiving anything in return.

Controversy erupted early in the meeting when the academics asked the other parties to sign a confidentiality waiver claiming they would not communicate the preliminary findings to anyone. The PI was concerned that the results could change when analysis was complete. Moreover, he preferred the findings to be peer reviewed before being made public. Given the topic was of political interest to many in British Columbia, he was worried that incorrect results could be given to the press or the government and thus jeopardize the research project and his credibility. The request to sign the confidentiality statement angered the First Nation's representatives as they had promised their communities to return with information. An agreement was finally struck among the team members when both sides conceded to the First Nations' representatives signing a revised and reduced statement.

The report submitted to the funding agent was written by the PI. Using the data he had available, the PI constructed and interpreted tables and a number of graphs. These were placed in the report alongside verbal accounts of the research objectives,

methodology, preliminary “findings,”³⁶ and his interpretation of these. The format of the report, including the type of content it required, was stipulated by the funding agency. As per the protocol agreement among the project team, all parties reviewed and ratified the report before it was submitted.

By the end of their year of funding, the contaminant ecologists had a considerable amount of data on PCBs and other pollutant levels in clams. Exactly how these levels related to the proximity of fish farms was not always clear to the contaminant ecologists, however, as there was considerable and unexplainable diversity in the data, unresolved questions about causality and additional questions about whether they had accurately assessed the “true” proximity of beaches relative to fish farms. The result was that only a few stories or clear patterns began to emerge for the PI from his analysis. For the most part, though, the scientists on the team decided they needed more data. Consequently, the PI and project coordinator decided to submit a follow-up research proposal to extend the project into a new funding year. This marked the end of my involvement in the project.

DFO Biologists

Request for working paper

At present there are three invertebrate biologists working out of this DFO branch office: one “emeritus” scientist, who is writing a book summarizing his life-long work on manila clams, a senior biologist and his assistant. They work (or had worked) full time on matters related to a number of invertebrate species harvested in B.C. coastal waters.

³⁶ For a discussion about the method by which the contaminant ecologists and other knowledge practitioners constructed their conclusions, see chapter six. As argued in chapter six, conclusions are not “found” but, instead, crafted using a variety of techniques and practices.

The DFO budget, particularly for invertebrates, has been dramatically cut in the last decade or so. The result is that the DFO biologists' work includes far more than research—they also attend community and government meetings, correspond with depuration harvesters, liaison with the public, and, as the senior biologists said, “fight fires” wherever they occur. In fact, the staff and equipment funding has been cut to the point that the biologists claim they can no longer do the types of long-term studies that *should* be done; according to them, any study should include data from at least 10 years from each study site, and at this point they had to be happy with data from three or four years.

As part of their work the DFO biologists are responsible for providing research reports (referred to as “working papers”) to a shellfish working group who recommend shellfish policies to the upper management at DFO. In fact, all research undertaken by the DFO biologists is at the request of this semi-autonomous group composed of DFO staff, industry representatives and select stakeholders. The research questions need not originate from this group, however; they can also originate from individuals within DFO who require the information for their work, such as the wild fisheries manager. The requesting individual makes the request to the semi-autonomous group, which in turn determines whether to make a formal request to the DFO biologists. If it does, a one page document is drafted containing information about who made the request, the rationale for the request, the date the report is due, and questions that need to be addressed in the paper.

Federal clam management in British Columbia is driven primarily by market forces and secondarily by the ecology of clams. As indicative of this, the DFO biologists

refer to clams as “product.” Manila clams are more desirable than other clam species in the market place, and thus commercial digging, management and therefore research is oriented towards manilas.³⁷ According to the DFO biologists, the ideal is to manage wild and farmed clams so there is a constant and consistent supply to the market. Beach openings are thus staggered, to the ecological extent possible, and limited to provide an adequate supply.

The DFO biologists are thus part of the governing infrastructure in that there is a direct correlation between the reports the DFO biologists produce and the policies that are implemented. Their research has very real implications—whether good or bad—for clam harvesters, clam consumers and clam beaches, and, as government biologists, they are accountable to the public for their policy recommendations.

In conducting their research, the DFO biologists build primarily on previous research that themselves and other government biologists have completed. Their knowledge claims are cumulative, with the majority of claims believed to remain true today. In discussing their work, the biologists frequently cite these reports and bulletins. A quick review of the bibliographies in these reports makes it clear that their citations are primarily in-house. The impression I received by talking to the two senior biologists was that the work of academics was often theory driven and not always applicable for management. Researchers to whom they did turn for resources were applied academics, such as those working at Rutgers’ Haskin Shellfish Research Laboratory.

³⁷ Butter clams used to be a major commercial clam species but now they are used more for canning than fresh clam consumption. The current market is oriented towards fresh products.

The study

Due to budget cuts within their institution the only clam research in which the DFO biologists were involved at the time of my research was routine annual surveys of depuration³⁸ clam beaches. Although these surveys used the same procedures as surveys conducted as part of policy research projects, at this point the policy had already been established and the purpose of these surveys was to make a management decision about the harvest rate for that particular beach for the next year in relation to the policy. I therefore decided to reconstruct the process by which they had completed their policy research a few years previous.³⁹ Given that their survey methods are consistent regardless of whether the survey was for creating new policy or implementing policy regulations on a given beach, such reconstruction was not difficult to do. To flesh out my reconstruction I used personal interviews with the DFO biologists, observations I made in the DFO lab during a routine clam survey, access to one of the biologist's Excel spreadsheet files he uses in analyzing data from clam surveys, a detailed manual written (Gillespie and Krunlund 1999) by the DFO biologists for communities conducting their own surveys under the purview of the DFO and their published research report.

The DFO biologists had completed two policy studies to establish clam harvest rates in the ten years preceding my arrival at their door. The second and more recent one was an elaboration on the first one, but had not lead to any changes in the policy that had

³⁸ "Depuration" beaches are clam beaches that have been declared semi-contaminated by the DFO. Any clams harvested from these beaches have to be placed in filtering tanks for 48 hours prior to their sale. These beaches are managed separately from "wild" beaches. Unlike the wild beaches, management is at the level of individual beaches (instead of by region) with harvest rates set for each beach.

³⁹ My work with these knowledge practitioners was thus somewhat distinct from that with the other knowledge practitioners in the sense that I did not directly witness the work involved in their study. To compensate for this I limit the use of this case study to information that I am confident about.

been implemented after the first study. Consequently, I decided to examine the first and more foundational of the two (Gillespie 2000).

The purpose of the first study was to establish general guidelines that would allow managers to stipulate harvest rates for a maximal harvest while also ensuring continued presence of clams. The report proposed “biologically-based reference points” or measurable attributes to determine the harvest rate on a given beach. Harvest rates were stipulated by a sliding scale. The sliding scale had already been implemented on the condition that it may be revised according to the findings of future research. At the time of my research it was the method by which harvest rates for depuration beaches were established.

The study targeted the depuration fishery, a fishery managed separately from the so-called wild fishery. The DFO classified beaches as depuration beaches if the beach had been closed due to high levels of fecal contamination (a different form of contamination than studied by the contaminant ecologists) and then reopened under the supervision of the DFO on the condition that the harvesters “clean” all clams before selling them. Cleaning was achieved by placing harvested clams in a tank with clean, filtered salt water for 48 hours in specialized, approved facilities. Depuration beach harvesting is restricted to staff members of these processing plants or to licensed diggers who sell their clams directly to these plants.

Unlike the “wild” beaches, the DFO individually manages each depuration beach. By this I mean they assign a harvest quota to each beach for each year. For obvious reasons, this form of management is time consuming and costly. Faced with budget cuts but still being required to follow management procedures, the DFO biologists had to find

ways to make their research efforts more cost effective. In the case of the depuration beaches, there were clearly-identifiable parties of interest—the processing plant owners/operators who requested access to the beaches for depuration harvest. It was in the processors' interest to have the research completed as it helped them gain licensing approval from the DFO for their harvests. The DFO used this situation to transfer some of the costs and workload of the research on to the harvesters.

The DFO biologists organized the research so that clam surveys were conducted every summer and harvests of a pre-determined rate were conducted every winter for three years. They collected information on each species of clam and for clams of legal (sexually mature) and sub-legal size. To determine the impact of particular harvest rates on clam populations they compared changes in clam density from one year to the next. They then used this information to decide what harvest rate was appropriate for a beach given the clam density on that beach.

Categorizing beach and counting clams

Clam surveys are an assessment of the abundance and density of clams on a given beach. They are the measurement technique most frequently used by the DFO in their research on clams. The procedure involves a division of labour in which there is a surveyor and a number of diggers. The survey involves mapping the perimeters of the area in which clams are expected or known to live and randomly selecting points within the perimeter to dig. The perimeter is composed of a series of reference lines, each of which is staked out by a minimum of two markers. Markers were composed of 2 m poles with fluorescent triangles on the top. A measuring tape or surveyor's chain was stretched across the beach and a compass bearing was taken to confirm the orientation. The second

marker was then driven into the beach 30 meters away from the first. The triangles on the top of the markers were used to maintain alignment for the remaining portion of the reference line. The goal was to make reference lines as straight as possible.

The majority of the leg-work of the surveying was completed by locals who were sponsored by the depuration facility. The volunteers were to follow the survey procedures described in a manual written and provided by DFO.⁴⁰ The DFO biologists recommend that surveyors lay out the reference lines as the tide begins to ebb. At this time the surveyors can also begin the task of randomly selecting points on the beach where they dig for clams. To select these points the surveyor designates one reference line as the x axis and a second reference line at the 90 degree angle to the first as the y axis. Surveyors randomly selected coordinates within the perimeter of the survey using a random numbers tables supplied in the DFO's surveying manual. To do this she first selects a number indicating the point on the reference line she designated as the x-axis. She then physically located this point on the reference line by use of a measuring tape or surveyors' chain. She then randomly selects a coordinate on the y-axis and locates its physical location on the associated reference line. The surveyor then stakes the intersection of these coordinates. This point will then be used as the corner of a sampling unit referred to as a *quadrat*.

In other ecology research in which I have been involved I have seen a square frame placed at the randomly selected point described as a quadrat. In this research no frames were used, but the terminology they used ("quadrat") and the idea behind their method was the same. The surveyor or her assistant placed stakes in the

⁴⁰ In addition to following the procedures described in the manual they also required to gain approval from a DFO biologist as to their specific plan for surveying the beach before they commence the initial survey.

ground at the randomly selected locations. Tied to the top of the stake was flagger's tape with the quadrat number and location written on it. Volunteers were instructed to face seaward and use the stake as the left origin point of an imaginary square that they were to dig by moving up and right from the flagged stake. The diggers' rakes were marked with tape part way up the handle to indicate how wide the quadrat should be. All diggers were instructed to dig to 20 cm deep or until they found no more clams. The diggers were to place all the clams they found in a sack which they then tied with the flaggers' tape originally tied to the stake. The tie identified where the clams had originated from on the beach and the sack indicated that all the clams had been dug from the same quadrat.

As the tide empties out of the beach, the diggers were to move down to the lowest exposed areas of the beach to complete the digging of randomly-placed quadrats in this area. Then, as the tide moved in, they moved up the beach to complete the quadrats in the middle and upper area of the beach. After the surveyor completed mapping the perimeter she calculated the area of the beach within the perimeter so as to determine the exact number of quadrats they were required to dig, as stipulated in the DFO manual. As the entire activity is limited by the action of the tides this meant that at some point the surveyor needs to switch roles and begin digging quadrats.⁴¹

After the survey is completed, the surveyor transports the clam sacks to a laboratory or facility in which she can process the clams for analysis. This work is done

⁴¹ Small beaches can be surveyed in a single tidal exchange but more than one day of work may be required on large beaches. Larger beaches are often divided into smaller sections, referred to as stratum, which can be surveyed on different days without creating too much complication or confusion.

with a partner. Labour is divided so that one person processes the clam samples while the other records data. The data recorder records the observations and measurements on the Intertidal Clam Number and Weight Data Sheet provided by the DFO. The data recorder is also responsible for watching that the sample processor does not make a mistake while handling, organizing and measuring the clams. This is especially important given that the clams will eventually be frozen and then the soft tissue will be discarded, after which point it will be impossible to re-check any observations or measurements.

The first step in processing the samples must be completed within 24 hours of the survey being completed while the clams are still alive. This is because the weight of a clam changes somewhat after it is frozen. This stage involves separating the clams from a quadrat by species and by whether the clams are of legal or sublegal size. Crushed clams are removed from analysis. The processor then weighs and counts the number of clams in each pile.

After all weight and count data is recorded for every quadrat for the entire beach the partners then take individual measures of 500 “randomly” selected manilas and little neck. Up to this point, weights have been recorded for the composite weight of each bag (or quadrat). Weighing every clam would be too time consuming. The task now is to individually weigh each of the 500 randomly selected clams’ weight and length. All other clams are thrown away. To randomly select these 500 clams, the sample processor randomly selects 10 quadrat sacks using random numbers generated by computer software or a random numbers table. Not all randomly selected bags are kept, however. The bags need to have the same ratio of legal to sublegal clams as the other bags from

that beach.⁴² In the case where they randomly picked a bag with a higher or lower proportion of legal (or sublegal) clams than that found on the beach, the bag is rejected and they randomly select another bag for use. This process continues until the partners have the 500 clams they need. At this point, each of the 500 clams is individually weighed (Figure 3.17) and measured (Figure 3.18) and the information is recorded on the data sheet.



Figure 3.16. Weighing clam



Figure 3.17. Measuring length of clam

As can be seen, the list of tasks for which the processing plant staff and volunteers was responsible was considerably long. Moreover, they conducted the surveys with minimal supervision by and training from the DFO. I did not witness these surveys but it is easy to imagine that surveying techniques were not always consistent with what was stipulated in the manual or up to the standards of the DFO. This was a concern of the DFO biologists and of the semi-autonomous board to whom they reported their research. As evidence of their need for concern, the DFO biologists found problems with the data, such as landings (i.e. the commercial clam harvests) being recorded in pounds instead of

⁴² They roughly estimate this ratio using the data they recorded on their data sheet during their initial measurements of all clams in all quadrats.

kilograms, and high volumes of little neck clams (and few or no manilas) being counted on beaches that had historically been manila-dominant on several occasions. The procedures outlined above should thus be thought of as the general guidelines surveyors were supposed to follow but not necessarily what was done. That said, the DFO biologists involved in the project were in frequent communication with the surveyors and made careful checks over the data with which they were supplied by the processors. They checked to make sure landings were measured using the same unit of measurement (e.g. kilograms or pounds) and that all data appeared probable. They would make inquiries to the processor regarding any data or survey procedures they considered suspect and, to the extent possible, correct the data.

While the clams from a beach were selected using random selection, the beaches were selected by a different method. Although the DFO considered it the responsibility of the processing plants to contribute the labour necessary for the surveys, the DFO did not want to over-burden the processing plants either. Likewise, the DFO biologists did not want to burden one processing plant more than another. Total and relative costs to the processing plants were thus the chief concern in the selection of beaches for inclusion in the study and for designation of harvest rates on each beach. The DFO biologists decided that each processing plant should have three beaches included in the study. Harvest rates have direct and indirect financial impacts on processing plants. A higher rate means they can harvest more clams and therefore have more immediate financial gain. At the same time, high harvest rates conceivably reduce the future clam productivity of a beach. To make sure no participant was unfairly impacted, each participant had one of their three beaches set at the harvest rates of 25%, 50%, and 0%

(an unharvested control). A total of eight beaches were included in the study (one beach was the shared responsibility of two processing plants). All beaches were located on the eastern side of Vancouver Island between the middle and southern tip, an area south of the Kwakwaka'wakw territory and east of the Nuuchahnulth territory in which I worked.

Additional concerns dictated the assignment of beaches to harvest rates. The beaches were not arbitrarily assigned to one of the three harvest rates. Instead, the assignment of a particular harvest rates to a particular beach was based on how recently the beach had been closed for harvest. Recently-closed beaches were presumed to have clams of more evenly-distributed ages whereas beaches that had been closed for several years had more mature clams, leaving less space for larval clams to settle. Larval production and settlement is sporadic, meaning that new larvae are not expected to settle each year on a beach. High harvest rates of legal-sized clams from these beaches was believed to potentially jeopardize the future presence of juveniles by reducing sexually mature adults. The DFO biologists were not sure whether this was accurate but they wanted to be careful so as not to destroy the clam population on any given beach. As a consequence, they decided to assign the higher harvest rates to beaches that had been closed more recently.

The surveyor was also instructed to record additional observations during the surveys, such as whether clams harvested during the survey were dying, and if they were, if it was due to damage consistent with signs of below-freezing winter conditions. This was supplemented by observations made by the DFO biologists when they visited the beaches. The goal of collecting this additional information was so the DFO biologists

could assess whether changes in clam density were attributable to mortality rates as opposed to harvest rates).

During the course of the study each beach was surveyed four times: the summers of 1996, 1997, 1998 and 1999. Between each survey commercial clam diggers associated with the processing plant harvested clams for the facility. The facilities oversaw the harvest, attempting to make sure the gross weight harvested was approximately the amount stipulated. The exact target weight for harvest was calculated by multiplying the experimental rate for the beach by the amount of legal-sized clams assessed to be on the beach according to the previous summer's clam survey. Achieving the target harvest rate was not always possible, however, for a number of reasons. For example, in some cases harvesters could not find enough clams to harvest and in other cases they accidentally over-harvested the beach.

Although the study had an a priori design (i.e. was determined in advance), the DFO biologists made adjustments to it while it proceeded. Each year, they examined figures from each beach and, in the case where unexpected results occurred they sought an explanation. For example, clam densities on one of the control beaches decreased. The DFO biologists found this surprising as the beach was a "control," meaning there beach had not been harvested that winter. They explored alternative explanations such as winter kill due to extreme weather conditions, but could not find an acceptable answer. As a consequence, this beach, along with others that had unexplainable drastic reductions in clam stocks, was removed from the study and, in some cases, from the fishery entirely.

Designing a policy and getting it approved

While the bulk of data collection was conducted by people associated with the various processing plants, the DFO biologists were directly responsible for statistical analysis of the data, authoring the working paper, attending the meeting at which the paper was reviewed, and making final revisions on the paper. The DFO biologists statistically analyzed the data using pre-determined mathematical formulas in which they assessed changes in density of legal sized clams from one clam survey to the next (as impacted by harvesting) for each beach. These figures were then used to create a regression model relating change in legal density to post-harvest legal biomass for all beaches. The regression line was used to derive reference points for the management framework—a simplified chart that stipulates harvest rates for a beach by the assessed density of clams found on that beach. Cut-off points for the various harvest rates were based on how many juvenile clams reached maturity in a square meter per year. A buffer was built in to the sliding harvest rate so that harvesting would not accidentally remove all “produced” clams.

When statistical analysis was complete, the lead DFO biologist on the project wrote a “working paper” that included a description of the methods used, a description of the known methodological problems and weaknesses in their study (including what measures had been taken to ensure the data was accurate), tables and charts summarizing the data, a list of policy recommendations, and the sliding chart stipulating harvest rates. The report was submitted to the semi-autonomous shellfish working group, who then found two external “experts” to review the report. At a subsequent meeting, with the DFO biologists present, the reviewers and group members critiqued the report, asked the

authors questions of clarification and, finally, made recommendations as to what changes were needed. After the authors made the required changes the report was passed to the regional and national DFO offices for approval. Only once it had been approved by the national level was the report made public and related policies implemented.

The DFO biologists' knowledge production process was thus largely organized around gaining acceptance from others as to the validity of their research conclusions. Writing a report that included empirical evidence as well as an honest discussion of potential methodological weaknesses was used by a committee and external reviews to assess the policy recommendations put forward by the biologists. The report and the authors were required to adjust this report until it was acceptable to the committee, at which point it was put forward within the larger organization to gain further approval. It was only once the report had gained approval at these higher levels of authority that the biologists' knowledge claims could be made public and endorsed by the institution.

Kwakwaka'wakw

The Kwakwaka'wakw have a long history with clams (Figure 3.19). Clams have formed a stable part of a core group of Kwakwaka'wakw clans' diet for as long as anyone remembers. In the past century clams have also taken on a new role in Kwakwaka'wakw society: they have become a means of access to the cash market, with Kwakwaka'wakw becoming commercial clam diggers, selling clams to wholesale buyers. But the Kwakwaka'wakw clam diggers do not focus on clam knowledge in the same way as the contaminant ecologists or DFO biologists. Kwakwaka'wakw clam diggers do not set out to test general claims such as whether clams close to fish farms contain more

contaminants than clams found on beaches further away from farms. They do not



Figure 3.19. Traditional preparation of clams for BBQ by a member of the Kwakwaka'wakw

develop a rationalized research design for obtaining new knowledge. They do, however, follow routine procedures to collect clams and, in the process, learn about clams. In fact, clam knowledge is central to the diggers' work as they use this knowledge to facilitate their work; without knowing where beaches are, which beaches likely have clams or where to dig on a beach, the amount of clams a digger can sell to the buyer is significantly diminished. As commercial clam diggers, these First Nations thus learn about clams so as to

develop more effective techniques for finding clams; the more successful they are at finding clams, the more money they make.

Many of the Kwakwaka'wakw diggers have dug for clams since they were children. As children they typically went digging with an older relative such as their father or a grandmother who showed them which beaches to dig on and, for some, where and how to dig on a beach. Specifics about where and how to dig on a beach were just as likely to be learned through a combination of trial and error and observing others, though; many diggers reported simply being encouraged to go dig clams without any formal instruction. Over the years they developed techniques and strategies for digging clams and learned where, when and how to dig by observing and working with more experienced diggers.

In developing knowledge about clams, the majority of Kwakwaka'wakw diggers did not focus on a single burning question to which they single-mindedly sought an

answer.⁴³ Instead, their approach was more consistent with that of many qualitative researchers in that they followed several lines of interest at one time, accumulated observations, experiences, and developed new questions as they worked. One issue to which all diggers did pay attention, though, was how the beaches were changing. Fish farms were of major public concern. According to the diggers, the beaches and clams have changed considerably since the 1980's when fish farms were first installed in their territory. Since then, many beaches are said to have become muddier and have far fewer clams. Other decreases in species presence and abundance, such as fewer whales, halibut and other select fish species were also associated with the fish farms. Changes such as these were of particular concern to diggers, both because they need to adapt to these changes but also because these changes represent a threat to their lifestyle.

The presence of fish farms in their territory was also of burning interest to the Kwakwaka'wakw diggers for an additional reason. To them it represented power the government forcefully wields over them. In the not-too-distant past, the First Nations were the only people populating the area and it was they who were responsible for the human management of beaches and waterways. Management authority has changed considerably since then, with the past few decades marking the most notable changes. Licensing has been imposed on them where licenses were never required before, such as with the clam fishery, and other fish licensing has become more restrictive. Increased regulations, limitations, and licensing have been correlated with decreasing ecological

⁴³ An exception to this trend was one older digger who was intently interested in knowing whether the number of clams had decreased and why. He repeatedly returned to this question every time we talked and pointed out evidence of change when I dug beside him on the beach. Other diggers expressed interest in some specific questions, like when one digger set out to confirm (or disconfirm) for himself what he had been told by another digger. But generally, the diggers did not select a particular question to focus on to answer in the process of their work with clams.

abundance and health. Fish farm licenses were extended to ecologically and culturally sensitive areas in Kwakwaka'wakw tribal territory. The fish farms are a sore reminder of the Kwakwaka'wakw's lack of legal authority over natural resource management in their own territory.

Where, what and when they dig

Like most aboriginals, the Kwakwaka'wakw are closely tied to particular tracts of land and waterways. These tracts are referred to in a number of ways but the one I heard the most frequently was *tribal territories*. A tribal territory demarcates boundaries within which a band, clan or community within a Nation has natural access rights. In some cases, though, some tribal members travel outside their territory to harvest natural resources. This can be based on old arrangements between tribes, marriage alignments, or kinship ties that provide an outside tribal member access to additional territorial rights, but just as often it is due to the breakdown of tribal alliances and the imposition of new territorial boundaries and regulations by the DFO. Diggers' options are somewhat restricted socially, however, in that others express resentment if outsiders take too much—especially those who gain access by DFO regulations.

The diggers travel to beaches by speedboat, row punt, or a combination of a gillnetter (a small fishing vessel) and punt. They dig on nearby beaches, which allows the diggers to return home after they finish digging. They travel in groups of two to eight diggers. Digging in groups serves several purposes: it is safer, as travelling alone on the water or digging on a beach where there could be bears, wolves or cougars is not safe; it is sociable, allowing people to visit while they travel to the beaches and when taking a

break from digging on a beach, and; not everyone owns a boat or has access to one, making it mandatory for some diggers to travel with others.

In choosing where to dig, the Kwakwaka'wakw diggers target beaches and spots on a beach they think will have a lot of clams—"hot spots," as they refer to them. Most diggers are commercial diggers who also dig an extra sack for home consumption. The main target species for the diggers I worked with was butter clams,⁴⁴ only occasionally switching to little necks. Little necks command a higher price but are scarcer and do not have the same meaning to diggers as they are not part of the traditional Kwakwaka'wakw diet.⁴⁵ That said, butter clams are not always purchased by the clam buyers. At least once a year the buyers reach their quota (i.e. the amount they think the market can bear) and the diggers have to turn to digging little neck clams.

The Kwakwaka'wakw beaches fall under the jurisdiction of the DFO's "wild" clam beach management, a different branch of the DFO than the depuration beaches. These beaches are managed as an area as opposed to as individual beaches. The DFO does stipulate which beaches are off limits or closed from clam digging, as listed in a booklet published by the DFO and available to all commercial diggers, but other than that there are no quotas set for any of the other beaches in the area. Instead, regulation of the fishery is through a general opening and closure of the clam digging season. The DFO-authorized digging season typically opens in November and closes at the end of March.

Openings and closures of the digging season is determined by test results for the

⁴⁴ Diggers based out of a different reserve also clam dig, but use a somewhat different approach digging. They tend to dig for little necks instead of butters and many do not have the same family history of clam digging. Their families were often involved in the salmon fishery, instead, and they turned to clam digging after fishing collapsed. Moreover, they tend to dig on different beaches and stay aboard gillnet boats for a week or longer while digging instead of returning home every night.

⁴⁵ As reported in Chapter 1, manila clams do not inhabit Kwakwaka'wakw territory. The only two commercial clam species in their territory are butter and little neck clams.

presence of PSP in clams, but the timing is not contentious with the Kwakwaka'wakw diggers as it coincides with the time when the Kwakwaka'wakw diggers say they have always stopped or decreased their digging. In the spring the clams go "milky," which is associated with reproduction but also with "bad" clams. Old time diggers are reputed to have dug in additional months of the year, but they knew how to detect and/or avoid unhealthy clams. There have been years in which the DFO chose not to open the clam fishery for the whole season, based on low levels of clam landings (commercial harvest rates) in the area, indicating the clam populations were reduced and in need of time for recovery.

The Digging Process

The Kwakwaka'wakw diggers have routines they follow for traveling to the beaches, but these routines undergo frequent adjustment. Minor but frequent problems such as their gill netter breaking down, stormy weather conditions, a beach not turning out to have many clams, or additional diggers joining them for the night leading (leading to re-organization in terms of who goes with whom) leads diggers to modify their routine. The lowest tides in the winter in this region of the world are in the evening. This means much of their digging is after the sun has set. All diggers carry with them the equipment they need for the night. This includes a gas lantern, a pitch fork (for butter clams) or a rake (for little necks), at least one bucket, and empty nylon mesh sacks. Equipment maintenance and work on the boats is often collective, though, in the sense a few diggers will fill up all the diggers' lanterns with gas for the evening while another set of diggers

cut the rolls of mesh sacks the diggers will need for the night, and others still are in charge of driving the boat to the beach.

Upon arriving at a beach the diggers make a quick assessment as to whether the beach has enough clams to make it worth their while to dig, or whether the beach is “dead.” Digging time is limited by the tide, and there is no point staying on a beach with few clams. Assessing a beach can be done by the water, looking for visual cues, or when on the beach, by digging test holes.

After deciding to stay on a beach, the diggers settle down to the business of harvesting clams (Figure 3.20). On the beach there is no division of labour—everyone is there to harvest his or her own clams. They move around the beach as individuals,



Figure 3.20.
Kwakwaka'wakw digger
digging for butter clams

making individual choices about where to dig and when to move. On the beaches, digging routines differ by digger, by the amount of daylight still available and by the rockiness of the beach. Each digger starts by selecting a spot at which to dig, putting down his⁴⁶ gas lantern, bucket and empty sacks and beginning to dig for clams. Some diggers prefer to dig a straight line across the beach (Figure 3.21). Other diggers

prefer to dig in short contiguous rows until they have dug

what is essentially a square or rectangular area. After the sun has set, many of these same diggers switch to a different pattern. Given the limited light, they often dig in a big circle around their lantern. Once this circle is complete, they may either move their lantern a short distance beyond their dug circle or, if the spot has not been particularly fruitful,

⁴⁶ I defer to the male pronoun because the majority of clam diggers were male.

relocate to a new, more promising, location. If they find a spot particularly poor, they will move before having completed the circle.

Clam digging in this region is hard physical work; the “sand” is considerably more densely packed and rocky than the beaches in Nuu-chah-nulth territory. Consequently, the Kwakwaka’wakw diggers tend to be strategic about how and where to dig and spend some time attending to the strategies. For example, some diggers will wait until the tide has emptied a large way down the beach before they start to dig at all, as they claim the choicest spots are located lower down; digging higher up the beach is considered a waste of effort. Others look for large rocks, mats of seaweed or specific types of formations in the sand. The diggers may develop these strategies for a specific beach or as a general practice for all beaches in their territory.



Figure 3.21. Digging in a straight line

Digging at this time of year occurs primarily in the twilight and dark as the lowest tides occur after the sun has set. Working in relative darkness means the diggers have learned how to use additional sensory cues than sight for working with clams. For example, some diggers can “feel” clams on their fork as they dig and can distinguish clam species by rubbing their fingers over the shell to detect the texture. That said, their visual acuity for clams is also well developed. For example, I was having a difficult time figuring out if the shell I was holding contained a live clam or just sand. The shell was clamped closed. As it turned out, it was filled with sand and clamped closed due to suction, not a clam muscle. As I struggled with this clam one of the diggers, who was

standing a good ten feet away from me, told me not to bother—that it was just an empty shell. How he could tell the difference from that distance when I could not tell when it was sitting in the palm of my hand and directly in front of my eyes I still do not know.

Assuming the beach has enough clams to make it worth their while, the diggers stay on a beach until the tide covers the viable digging spots on the beach. By this time, depending on how “good” the beach was, a single digger could have anywhere from 2 to 8 sacks of clams. Sack sizes (or, more accurately, how full a digger fills a sack) are not standardized, though. Sack size is based loosely around bucket size, but just how full a digger fills his bucket, what size of bucket is used, and whether the digger puts one or two buckets of clams in a sack differs by the digger.

When the tide begins to wash over the higher digging areas on the beach the diggers load their sacks of clams, buckets, and other equipment on their boat and head back to the village (Figure 3.22). The Kwakwaka’wakw diggers store their clams for up to five days before they sell transport and sell them to the buyer. This means they have to look after the clams for these days, making sure they do not die. Each digger has his own

way of dealing with this situation. Some keep only those clams with unbroken shells and that did not get speared in the process of being dug, or check their sacks daily to toss out any smelly clams to prevent further sickness and death among the remaining clams. Some are also worried about the weight of clams, as clams become lighter when they are stored out of water and thus will sell for less (as price is based on weight).



Figure 3.22.
Kwakwaka’wakw digger
unloading punt into
gillnetter after digging

Historical perspective

For the experienced Kwakwaka'wakw diggers, the land and waterways are a lived-in environment. It is not just abstract “people” who travel and work in the area, but friends and relatives as well as strangers. They can relate endless stories about what happened when they dug at a particular beach with their cousin, uncle or friend; point out bays that gave them shelter when the weather was too stormy for safe travel; where a boat sank or who bumped their boat against what rocks; or where a friend drowned.

In addition to their peopled associations with place, diggers can also describe how clams and sediment on particular beaches have changed over time. For example, one digger talked about the beach we were digging on as good the previous year but, by pointing to the half a bucket of clams he had found in two hours of work (a small harvest for that amount of time and effort), how the beach had changed. Like other diggers, he also talked about “hitting a hot spot” on another beach only to find that hot spot “dead” the next year. Likewise, he talked about how another beach had become “real muddy” in recent years. These types of comparisons are frequently made by the diggers.

The beaches are historically “peopled” in another way: as culturally modified artefacts. A large number of clam beaches in this region have been engineered to be clam habitat. “Clam gardens,” as they are called, are beaches where mid-sized rocks have been rolled to the low tide line to form a wall (Figure 3.23). These rock walls prevent sediment from moving outside and thereby extend the amount of beach exposed at low tide. These walls created better clam habitat as they resulted in larger beaches and water currents that encourage clam larvae to stay on the beach. Not all diggers have heard about these walls, however, and not all beaches have these walls. The practice of

building and maintaining them has been all but lost. Yet, whether the beach is engineered or whether the digger is aware of its engineering, the diggers are aware of the various clam beaches in their territory, the history of those beaches in terms of whether they have unusually large clams or consistently large amount of clams, whether they are “little neck beaches” or “good butter beaches.”



Figure 3.23. Kwakwaka’wakw-built rock wall for traditional aquaculture purposes

The form of diggers’ knowledge about clams

Kwakwaka’wakw knowledge about clams is both descriptive and prescriptive. In some cases, the diggers make statements about clams or clam beaches. For example, they might say “the clams move up in the sand when the tide starts coming in” or “you find little necks higher up the beach than butter clams.” In other cases, they talk about what *should* be done to look after clams. For example, “make sure you cover the clams you leave behind with sand so they do not freeze or get eaten.” Implicit to such statements as the latter are information about dangers to clams, but it is expressed in terms of how a digger should *interact* with the clams and the beaches instead of being expressed as a knowledge claim.

Regardless of whether it is descriptive or prescriptive, Kwakwaka’wakw knowledge about clams is always specific to the beaches about which the digger is familiar. Diggers prefer not to make broad, sweeping statements about clams and clam

beaches. They often suggest that clams can differ from one territory to another. For example, one digger said about a claim made by another digger “that may be true of clams in his territory, but that’s not what clams are like here in my territory.” In fact, some diggers prefer not to generalize beyond the specific beach on which they are digging.

Expertise is difficult to identify

As suggested above, there is considerable diversity in diggers’ practices. This is also true in terms of the level of their expertise. Unlike science, there is no credential or certification system among diggers used to clearly label who has expertise and of what sort; there is no exam one has to pass to prove what one knows about clams. Like science, however, those with the most clam expertise tend to be the diggers who have the most experience. They are also the ones who tend to have been trained by or at least witnessed the older generations in terms of how and where to dig. That said, not all diggers have the same intellectual curiosity, intellectual capacity or attention to detail. This makes identification of “experts” or those knowledgeable about clams difficult.

There is a hierarchy of expertise among diggers which is allegedly due to who is the better digger. Those who have the most experience in local waters and who are considered the best diggers are treated with higher status and are the ones who select which beach the crew will dig at on a given night. Yet some of these diggers are considered “good” not because they necessarily know a lot about clams but because they work harder than others. They are the diggers who are still digging hard while others are taking a break or slowing down for the night. Yet hard work does not indicate expertise;

diggers that know more often take their time and yet dig as many clams as the diggers who exert a lot of effort.

Despite varying levels of expertise, all experienced diggers will tell you they know a lot about clams. And they do. The experienced diggers have all learned as they dug, incorporating and critically analyzing their experiences and testing their ideas so as to find more clams. Those who do not do so do not make enough money to continue in the business.

Nuu-chah-nulth

Like the First Nations in Kwakwaka'wakw territory, members of the Nuuchah-nulth First Nation have been clam digging for hundreds if not thousands of years. Clams have historically constituted a major part of their diet and still represent a regular diet item and a way of life for a notable percentage of the population (Marlor and Eyding 2005).

Like the Kwakwaka'wakw, the Nuuchah-nulth diggers do not set out to answer specific research questions about clams. That said, they do actively ask themselves and others questions about changes they have observed. For example, one digger said to me: "I noticed the clams [on a particular beach] are dying off—last tide the top layer is a die off, but below it there's live clams. Why the die off?" Or, similarly,

I'm worried about the population health of urchins. Every weekend there are people going out and bringing back 40-50 urchins and giving them away. How long can this go on for? There are no big crabs now, either—only little ones. Are they overfished or is it sediment from the fish farms [that killed the big crabs off]? Some areas have lots of dead crabs on the beach. Why? There are fewer chitons on the rocks, too.

The marine environment is thus not something the Nuu-chah-nulth diggers just passively accept as dynamic and changing. Instead, the diggers mentally engage themselves in asking and, to some extent, answering questions about why changes were occurring.

The content of the Nuu-chah-nulth diggers' knowledge about clams is somewhat different from that of the Kwakwaka'wakw, but the general process by which they generate knowledge and its format is largely the same. There are three types of Nuu-chah-nulth digger: commercial, home and fisheries guardian. "Home" clam diggers dig for clams they will consume at home and may or may not dig commercially. Commercial diggers dig for manila clams and may or may not dig an extra sack of clams for home use. "Fisheries guardians" are band-hired fisheries officers who are responsible for ensuring that diggers follow DFO regulations but who also harvest sea foods for the community.⁴⁷

Commercial diggers

Like the Kwakwaka'wakw beaches, the Nuu-chah-nulth beaches are managed by the "wild" clam management branch at DFO. The DFO requires that all adult commercial diggers possess a digging licence. Obtaining the license does not require anything beyond proof of identity; no special training is required. When using the license, however, one must follow regulations stipulated by the DFO.

⁴⁷ The Kwakwaka'wakw also had fisheries guardians, but, unlike the Nuu-chah-nulth village in which I worked, the fisheries guardians played a relatively minor role in clam-related activities. They did not have the same relationship to their natural resources as the Nuu-chah-nulth guardians in that they did not make regular patrols of beaches and did not harvest any form of seafood for their community. Instead they spent the majority of their time in their office. Part of their duties was to manage and monitor licensing, but they did not monitor this very carefully. Eventually, the band I worked with opted out of participation in this office. The DFO were supposed to regulate them directly after this but, given the cut backs faced by the DFO, this led to even less of a regulatory presence on the beaches.

All commercial digging occurs in the winter between November and March, as regulated by the DFO and when there is no chance of PSP. Unlike the Kwakwaka'wakw, the Nuu-chah-nulth's digging season is further reduced to specific days during when they are allowed to harvest clams. This is because they commercially harvest manila clams. Due to market demand, availability of manilas from other areas, and their desire to maintain a high price for their clams, their commercial digging openings have been reduced to approximately 12 days per year. These opening days have progressively decreased in recent decades as more clams have become available on the market from other, more distant areas.

Commercial diggers typically dig on one of three beaches. These beaches are unusually large clam beaches for the British Columbia coastline and have sustained up to 30 diggers per night during openings without apparent detriment to the clam populations. Only a few diggers have their own boats, so most diggers pay a small fee to boat owners to be transported to one of the these main digging beaches. Only clams of legal size can be sold to the buyer for commercial sale. The diggers have various methods to determine whether a manila clam is of legal size, such as comparing the length of the clam to their thumb or finger or to the distance between prongs on their rake. All clams are sold to a single buyer on the night in which they were harvested.

Home diggers

Many home clam diggers dig for butter clams, the traditional clam in the Nuu-chah-nulth diet, but some dig for "steamers" (an assortment of manila and little neck clams). Many of the home diggers also harvest other seafoods such as sea urchins, chitons, barnacles, crabs and fish. Home diggers harvest clams from an array of beaches.

Those who dig for home clams are technically required to report to the fisheries guardians when they dig and how much they harvest, but this rarely happens. Home clam diggers either travel with the fisheries guardians to harvest clams (discussed below) or have their own boats to travel to more distant beaches. Some of these diggers harvest clams 12 months of the year. When asked if they are concerned about PSP poisoning, many of these diggers claim they are immune to PSP due to regular consumption of clams throughout the year.

Fisheries guardians

The fisheries guardians are equipped with boats and spend considerable time on the water. They are particularly committed to *sustainable* harvesting of clams and other seafood species and often find themselves at odds with others in their community over hard lines they have taken to protect the tribe's resources. For example, they have been instrumental in the closing of a few key beaches from commercial harvest so as to save the beach for home and ceremonial use. This has been at the protest of many commercial



Figure 3.24. Nuu-chah-nulth diggers digging for manila clams

diggers.⁴⁸ Similarly, they diligently patrol beaches on the nights when clam digging openings occur and have been known to call in the DFO when a regulation has been broken, like when sacks of clams were found hidden on a submerged rock shelf the day before the official opening.

The guardians also take it upon themselves to dig clams and harvest other sea foods for community feasts and for giving to elders and others who are not physically able to harvest their own seafoods (Figure 3.24). The result is that the

⁴⁸ Some of the protesting clam diggers claimed that the closed beaches are not being environmentally protected at all as clams need to be “turned” (much like garden soil) to stay alive.

guardians are among those who are the most experienced and knowledgeable about the beaches within the community.

The digging process

All Nuu-chah-nulth diggers use the same equipment regardless of whether they dig for commercial or home use clams—some sort of digging instrument (a rake or a pitch fork), a bucket, a gas lantern for night-time digging, and empty nylon mesh sacks. Most diggers work independently of the other diggers, moving to whatever area on the beach they think they can find clams. Some diggers do not have their own lantern, though, and end up sharing with others. The result is they end up digging together. The diggers also take breaks from their work and socialize with other diggers, either watching them work or all taking a break.

Clam expertise

As with the Kwakwaka'wakw, knowledge about clams facilitates the Nuu-chah-nulth diggers' harvesting of clams. Some diggers have considerably more experience, a longer history, and/or more training from their elders than others, though. Clam digging was less popular while fishing was strong, but now that fishing has died off more people are turning to digging. Diggers whose families dug continuously have considerably more long-term experience. Several of these diggers are those now employed as Fisheries Guardians. These diggers learned about clams from their elders, watching them and listening to them, as well as from their own experience.

Format of knowledge

The Nuu-chah-nulth clam diggers orient themselves towards knowledge in much the same way as Kwakwaka'wakw diggers. Like the Kwakwaka'wakw, Nuu-chah-nulth diggers' knowledge is tied to specific beaches and waterways in their territory and to theirs (or their grandparents' reported) experiences in their territory. In addition to changes on the beaches, they are aware of other changes that have occurred such as fish farms and logging activities. In fact, many of them currently work or have worked on the fish farms. Often the information they have about what happens at fish farms remains individual, though, in the sense that people have personal experience about what was done at a particular fish farm but they do not spend much effort making this information communal or public.

Like the Kwakwaka'wakw, changes to clam beaches are associated with other anthropogenic changes in the marine environment. One of the major changes the diggers talked about was the presence of "dead beaches," or beaches that no longer have any clams.⁴⁹ Those willing⁵⁰ to identify a cause of the dead beaches attributed it to the presence of the fish farms.

A Quick Comparison

In the remainder of this dissertation I make a number of comparisons among the various knowledge practitioners' work. At this point I will only make some initial

⁴⁹ The Nuu-chah-nulth diggers strictly associated the term "dead beach" with beaches where the clams had completely disappeared. The Kwakwaka'wakw also talked about "dead beaches," but used this term loosely in relation to any beach that had low levels of clams, like when the beaches were over-dug, where machinery had been used to dig up the beach, or fish farms had allegedly killed the clams.

⁵⁰ Not all diggers stated that fish farms were the cause of dead beaches. Jobs are not easy to find in this remote island community and several community members rely on the fish farms for employment. Perhaps unsurprisingly, fish farm employees were hesitant to implicate fish farms as the cause of dead beaches.

comparisons. I have already made some comparisons between the two First Nations communities but additional comparisons can be made. As stated, the Kwakwaka'wakw and Nuuchahnulth diggers' general reason for studying clams and their methods for doing so were consistent. One major difference between them, however, was in terms of the specific species they studied. Generally, the Kwakwaka'wakw diggers' clam knowledge was about butter and little neck clams whereas the Nuuchahnulth diggers' knowledge was about either manilas or butters, differing by whether they dig commercially or for home clams. The difference in expertise by species was due in part to the presence of certain species in their respective territories⁵¹ but also due to market demand and personal taste for clams.

The distribution of expertise differed somewhat between the two communities. Distinguishing levels of expertise was somewhat easier among the Nuuchahnulth than the Kwakwaka'wakw in that more Kwakwaka'wakw diggers have extensive experience digging for clams, making the level of general knowledge held by all but novice diggers higher. Amongst the Kwakwaka'wakw diggers, pretty much all of the diggers were or had been commercial diggers. Amongst the Nuuchahnulth, the pattern of experience differed; the most experienced and knowledgeable diggers were those who dug clams exclusively for home use or who were employed as fisheries guardians. In both communities, the more experienced diggers were the ones whom others turned to for recommendations or advice. For the Kwakwaka'wakw, the most experienced diggers were the ones who made decisions about which beaches to dig on a given night. For the Nuuchahnulth, more novice diggers turned to the fisheries guardians for deciding whether clams were currently safe to dig (and eat) due to PSP poisoning.

⁵¹ Manila clams do not live in Kwakwaka'wakw territory.

There are also notable differences between the contaminant ecologists and the DFO biologists. To begin with, there was a distinct difference in terms of the size and number of tools each employed. While both used tools for measurement, the largest tool used by the DFO biologists was a computer (for data analysis). The majority of their tools were small and relatively unspecialized—a scale for weighing clams, digging rakes, nylon sacks, and a ruler. The contaminant ecologists, on the other hand, used a variety of large, expensive, or/or specialized tools such as their mass spectrometry machine, fume vent, glass vials of various shapes and sizes, hexane rinse, and surrogates. This is important in terms of these knowledge practitioners' relative ability to correspond their measurements to their own sensory perceptions and level of trust required in their machinery, tools and procedures. The contaminant ecologists used more sophisticated machinery and subsequently were put in a position of having to trust more in this machinery than in their own sensory perceptions relative to the DFO biologists.

A second difference between the two sets of scientists was in terms of the amount of experience they had with clams. The DFO biologists had collectively accrued years of experience with clams. In contrast, the contaminant ecologists had no such experience to speak of. Moreover, the scientists' perspectives of clam beaches were considerably different in that the contaminant ecologists thought of beaches in terms of systems, with all systems having the same basic underlying structure, whereas the DFO biologists were much more attuned to specific intertidal species and how to regulate these species for harvest in relation to the market. This difference is important both in terms of the theoretical approach each set of practitioners brought with them to their studies but also how broadly they expected to be able to generalize their knowledge claims. The

contaminant ecologists did not consider it a problem that they did not have much prior knowledge about clams as clams were just one organism within marine systems and the ecologists intended to learn about the relationship of clams to other aspects of the system. The DFO biologists, on the other hand, were focused more on individual clam species and considered detailed information about the species highly important.

These two sets of practitioners also differed in terms of the type of knowledge they sought. The DFO biologists were interested in predicting clam population fluctuations relative to harvest rates whereas the contaminant ecologists were interested in the presence of pollutants in clams. As a consequence, many of the specific methods they used in their research were different. For example, the DFO biologists wanted to answer questions about the number of clams on a given beach, and thus opted to use random selection of quadrats along with counting numbers of clams. The contaminant ecologists, on the other hand, were not interested in the distribution of clams on a beach but were interested in the chemical content of the clam. Random selection of clams from the beaches was not of any particular concern for them.

A difference in interests also exists between the DFO biologists and contaminant ecologists. The DFO biologists are applied scientists whose goal is to create management policy whereas the contaminant ecologists are more basic academic researchers and interested in answering a general question about the relationship between fish farms and clam beaches. This affected the type of knowledge each aimed to create, with the DFO biologists developing a sliding scale for determining harvest rates for particular depuration beaches whereas the contaminant ecologists' knowledge—while it had

potential policy implications—was structured as general claims instead of a prescription for action.

Regardless of the differences between them, the two sets of scientists were similar in that they both set out to answer a specific research question, wrote reports, were funded to produce knowledge (as opposed to being paid by the amount of clams they collected), divided their labour in a rational-bureaucratic manner amongst a team, and organized their work in distinct stages so that they first collected samples, then made measurements and subsequently analyzed their measurements.

In comparing the scientists to the First Nations clam diggers, one can see a variety of differences. One difference lies in their respective motivations for generating knowledge. The former were interested in producing knowledge for others' consumption whereas the latter were interested in producing knowledge for their own use. A second difference was in terms of the division of labour and collective nature of their work. The biologists generated knowledge by working together as a team in which labour was bureaucratically organized and divided whereas the First Nations clam diggers generated knowledge as independent individuals who, at specific junctures, worked collectively. A third difference was that the biologists employed much more strict protocols in their work. For the DFO biologists this can be seen in terms of the many steps they articulate in their manual in regards to collecting clams from the beaches as well as with how they conduct themselves in the laboratory and during data analysis. For the contaminant ecologists, who were studying clams for the first time but who had extensive experience with contaminants research, their protocol for the beach was rigid in terms of cleanliness of equipment and the handling of samples. As can be seen from the large number of

photos depicting the preparation of clam samples in the lab, numerous distinct steps were involved. Moreover, their lab protocol for dealing with the clams was far more rigid still than their beach protocol in that every step in preparation and analysis had been identified and listed on a document to be carefully followed. The First Nations clam diggers, on the other hand, did not follow strict protocols but did have routines. These routines were frequently adjusted, however, to suit current circumstances, like when a digger changed digging patterns because there was more or less light or traveling with another crew because his outboard engine blew up.

That said, the biologists did not always follow strict scientific protocols. For example, the DFO biologists selected beaches for inclusion in their study based around financial considerations for the depuration facilities but they did not follow a strict set of rules or procedures to do so. Likewise, the contaminant ecologists selected the beaches for their study based on the advice they received from clam diggers and biologists who lived in those areas. Moreover, when collecting the clams on the beaches, the PI made quick judgements as to which clams were “small” and “large” and the method and location on the beach at which the diggers’ dug was based on individual decisions made by the digger.

Another difference of note between the First Nations and the scientists was that the scientists had far less personal attachment to the clam beaches than the First Nations. This was both in terms of whether they thought of the beaches as “theirs” and in terms of their personal history with the beaches. The DFO biologists had a longer historical connection with some of the beaches than the contaminant ecologists (although this, too, varied by beach and by individual), but the depth of the historical connection they

brought to each beach was considerably less than that of many First Nations diggers. That said, the DFO biologists had a broader general perspective of the history of clam beaches in British Columbia than did the First Nations, being aware of things such as when and how manila clams (an invasive species) first entered the B.C. coastal waters and the gross weight of commercial clam landings, by species, from each fisheries area in the province for the last forty years.

Similarities also exist between the four sets of knowledge practitioners. Given the prevalence of differences, it is important that these constants be stressed as, by doing so, the reader can see that I am not comparing apples to oranges. There are five important constants among the case studies. First, all four groups are studying inter-tidal clams or clams found between the low and high tide line on a beach. All experts worked with at least two of three species of clams. Second, all four sets of knowledge practitioners worked in the same geographic region, namely Vancouver Island in British Columbia, Canada. In fact, two of the three study sites in which the contaminant ecologists worked (and the only two I report) were in the Kwakwaka'wakw and Nuuchahnulth diggers' territories. The only group whose study sites did not overlap with those of the other practitioners was the DFO biologists. Third, all are knowledge communities, or at least communities in which knowledge is generated. Fourth, overlap exists among the four sets of knowledge practitioners in terms of the questions they were interested in answering. Specifically, the questions of interest to the First Nations paralleled those of the scientists: the DFO biologists and the First Nations (the Kwakwaka'wakw, in particular) were interested in assessing beaches for whether they were harvestable and the contaminant ecologists and First Nations were interested in how the fish farms were

affecting clams and the clam beaches.^{52,53} Finally, at least one person within each set of knowledge practitioners had direct experience digging for and collecting clams on a beach. Moreover, in doing so, these practitioners used the same basic set of tools to dig: a rake or pitch fork and a bucket. This might seem an inconsequential similarity, but it is important in that it means all practitioners were restricted in similar ways in terms of how they could potentially interact with clams on the beaches. For example, it meant that none of them were likely to dig up huge portions of the beach or to dig particularly deep. For all practitioners, considerable effort was required to interact with clams.

These are just some of the similarities and differences among the four sets of knowledge practitioners with whom I worked. Additional comparisons are made in the following chapters as I explore various ways of knowing. Specifically, in chapter four I examine differences between the contaminant ecologists and Kwakwaka'wakw diggers in regards to the social organization of their communities, their interests in clams, and how the former and latter are inter-related. In chapter five I compare the DFO biologists, the Nuuchahnulth and the Kwakwaka'wakw diggers in terms of what they attend to and how they attend to it in their ways of assessing clam abundance on a beach. In chapter six I compare the practices by which the contaminant ecologists' and Nuuchahnulth diggers craft a conclusion from the basis of their observations and measurements.

⁵² These questions are discussed in more detail in chapter four.

⁵³ The questions the contaminant ecologists and DFO biologists wanted to answer were considerably different to one another. The former wanted to better understand the relationship of fish farms to contaminant levels in clams whereas the latter wanted to know how to set sustainable harvest rates for beaches.

My research

I now turn my focus to the final knowledge practitioner involved in this study: me. I have already inserted myself into some of the description above particularly that of the contaminant ecologists' and DFO biologists' studies. Here I describe my activities pertaining to this project in more detail.

As should already be clear, I compiled four case studies of knowledge practitioners who work with clams. The majority of data collected about the cases is based on ethnographic material I acquired while working with the knowledge practitioners. I began this project with the idea of learning about alternative routes by which empirical knowledge practitioners create knowledge about a similar subject. My area of substantive interest was in ecological/biological knowledge as I have studied this topic in school and as an amateur naturalist. The exact subject and the origins of the knowledge practitioners was of no particular theoretical concern to me but, given my affinity for marine life—especially in the intertidal zone—and my lack of interest in spending several years living abroad to complete the amount of ethnographic work required, I decided to focus my efforts on locating practitioners who worked on the Pacific coast of North America.

My first priority was in contacting traditional knowledge holders as I anticipated this group of practitioners more difficult to locate. Through the assistance of Nancy Turner and Russel Barsh I made contact with a biologist by the name of Marty Weinstein working for one of the Kwakwaka'wakw bands. He was indirectly involved in organizing the initial community meeting from which the contaminant ecology research

project developed. Marty kindly invited me to the meeting without having even talked to me about my research plans.

At this initial meeting I made contacts with many of the people who turned out to be of key importance to three out of my four case studies. It was here that I met the chief of the Kwakwaka'wakw village in which I did the majority of my Kwakwaka'wakw-based research, the DFO biologist who, two years later, showed me how the lab work was done for clam surveys, the eventual PI and project coordinator for the contaminant ecology research project, a handful of the Kwakwaka'wakw diggers, a Kwakwaka'wakw fisheries guardian who provided me with an initial list of commercial clam diggers in the region, Marty's assistant who told me everything and anything she knew about the native clam fishery as well as kindly housed me during my initial visits to the area, and the owner of the boat on which I took my first trip to the Kwakwaka'wakw village where I worked.

My initial intention was to conduct two comparison ethnographies: one of scientists and the other of a group of First Nations traditional harvesters. This eventually blossomed into four as my project developed. I had remained in contact with the contaminant ecologists since the initial meeting and met with some of them on additional occasions to discuss the possibility of my working with them on whatever research project emerged. They had agreed. I had made similar arrangements with the chief of the Kwakwaka'wakw village in which the majority of clam digging occurs. I became interested in working with the Nuu-chah-nulth, though, shortly after the contaminant ecologists began talking to them about being partners in their study. At one point in the development of their project, the Kwakwaka'wakw fisheries commission dissolved and

the Kwakwaka'wakw (temporarily) withdrew from the contaminant ecologists' project. This presented a problem as I wanted to compare knowledge practitioners working on the same species in the exact same geographic region. A solution to this problem developed, though, as their project developed. They wanted a social scientist to conduct a clam consumption survey in one of the Nuu-chah-nulth villages. As they knew I was interested in the topic they approached me to do the work. I agreed to taking on this extra research as it meant it my project would once again allow me to compare knowledge practitioners from alternative traditions who were working with the same species and in the same geographic area. This additional research would also cover much of my expenses incurred while conducting field work with Nuu-chah-nulth clam diggers as the contaminant ecologists' project covered my travel expenses and much of my work with the Nuu-chah-nulth diggers. At this point I had already begun fieldwork with the Kwakwaka'wakw diggers, so it seemed pointless to exclude them from my project. In the end it turned out to be beneficial anyway because the Kwakwaka'wakw re-joined the project, allowing me to compare them to the Nuu-chah-nulth and to the scientists.

I decided to include the DFO biologists as a fourth case due to the lack of expertise the contaminant ecologists had with clams. Initially I described the people with whom I worked as "clam experts" instead of "knowledge practitioners." My shift in terminology is partly due to a revision in my theoretical approach to my project, but is also undeniably due to the fact that the contaminant ecologists were *not* clam experts. My goal was to include some sort of scientist that worked with clams. This proved incredibly difficult if for no other reason than clams are not a "sexy topic" (as one DFO biologist put it) and very few scientists study clams. In fact, the DFO biologists were the

only scientists I could find who studied clams in the same region as the contaminant ecologists and two First Nations communities. It was for this reason that I initiated contact with the DFO biologists.

My goal in working with the various knowledge practitioners was to gain as much direct, in-context experience as possible. With the Kwakwaka'wakw clam diggers, whom I worked with the most and for the longest, I stayed in the main village out of which clam harvesting occurs, hung around the dock so as to talk with clam diggers as they prepared for digging, traveled with the diggers to the beaches, dug for clams (which I gave to the diggers as in thanks for letting me go with them), helped move equipment onto and off the beaches from their boats, carried sacks of clams, traveled with the diggers to sell their clams to the buyer, and participated in anything else that allowed me to become involved in their activities. In the village I conducted interviews that ranged from being relatively formal to highly informal. I often brought marine charts of the area along with me so diggers could point out where they had dug and on which beaches they had noticed the most changes in recent years. In total, I worked with over twenty clam diggers, eight of whom I spent extensive time with.

My involvement with the Nuu-chah-nulth took a somewhat different form although the general method was the same: to get involved in any way I could. As stated, I was hired by the contaminant ecology team to conduct a clam consumption survey. For the survey I worked with a randomly-selected sample of village residents and as many clam diggers as possible to assess a variety of clam-related phenomena including the prevalence of clams in villagers' present-day diet, the source of these clams, any health issues they had experience in relation to eating clams, and observations the diggers

had of changes in the health of the clams or clam beaches. Interviewees were asked questions from one of two distinct interview schedules depending on whether they were considered a village resident or clam digger. It was from the interviews with clam diggers that I learned a lot about what they observe on a beach, their perceptions of beaches, and the process by which they reason about clams.

I was also hired by the contaminant ecologists to assist with the collection of clam samples during both initial sampling trips to Nuu-chah-nulth and Kwakwaka'wakw territory. In the Nuu-chah-nulth area, sampling was a group endeavour undertaken by the PI, myself, another biologist, and the Nuu-chah-nulth fisheries guardians. As already described, the guardians were among some of the most experienced clam diggers in the village. My job was to dig for clams, which I did, but I also took the opportunity to work alongside the guardians as they dug, asking them questions about clams and watching how and where they dug. The guardians facilitated my ethnographic work with the Nuu-chah-nulth in other ways as well. On other occasions I traveled with the guardians to dig clams for distribution to elders in the village and to monitor the activities of clam diggers on official DFO clam digging openings. During the latter of the two, the guardians landed their boat on shore to allow me and another member of the contaminant ecology team to wander around and talk to/observe the 30 plus commercial clam diggers digging on one of the beaches.

My work with the contaminant ecologists was consistent with that of the First Nations in that I became as involved as I could in as many ways as possible. As I already stated, I assisted the contaminant ecologists with the collection of their initial clam samples in two regions. On these trips I was the main company the PI had as we were the

only ones who traveled from outside the area and stayed in temporary accommodations in the villages while the samples were being collected. This meant I was the one available for the PI to communicate his thoughts and concerns aloud about the project, to give him feedback on alternative sampling strategies he was thinking of employing, and for answering any questions I had about his project. As the key researcher for the clam consumption survey, I was privy to project-related emails, was included in all the project meetings and co-authored one of the final reports submitted to the project funder. I also volunteered to write a manual, one of the “deliverables” from the project for the First Nations communities. The manual was a description and photo journal of how the contaminant research was conducted, thus providing the First Nations involved with the project a better understanding of how the research was conducted and directions for how to go about future research on contaminant testing in their area. As the lead author on the manual I was allowed one full day to follow the PCB sample preparation lab technician around and have her show and explain to me, step by step, how she prepared clams for analysis. On a subsequent occasion I interviewed the lab chemist and had him show me the mass spectrometry machine and the graphical read-out it creates. Finally, I interviewed both the PI and the senior research chair on several occasions throughout the project, including one wrap-up interview with the PI in which I probed extensively into the steps he took while conducting statistical analysis.

I have already described a lot of my work with the DFO biologists. My first hand experience with their research was minimal. The closest I came to experiencing it directly was to witness one of the DFO biologists process the clams after a routine clam survey. The same biologist also showed me the Excel data file template he used for entering and

analyzing his data, the official map he drew of the beach he had just surveyed, and the form letter he submits to the senior biologist containing the information he gleaned from the survey. I was not able to witness the collection of clams from the beach. That said, this part of the research had a design similar to that of several other ecological studies I had assisted with or conducted as a student and research assistant in the past, one of which was with randomly-placed quadrats in the intertidal zone, another with randomly-selected units in a forestry study and two employing transects (similar to quadrats with the exception that the quadrats are not randomly placed but arranged in a contiguous line). Moreover, I had training in probability and statistics and was familiar with the logic behind random selection. Combined, this meant that many of the procedures described in the clam survey manual were familiar to me in some capacity or another. All other data about the DFO study were obtained from interviews with the three DFO biologists, documents they provided me (such as the manual for clam surveying) and other material I downloaded from the DFO website.

All data was collected concurrently between July 2003 and June 2005. In total I spent approximately four and a half months in the field with the Kwakwaka'wakw clam diggers, six weeks with the Nuuchahnulth diggers, over a year and a half of work on and off with the contaminant ecologists which amounted to several months total at a minimum, and about a week in total with the DFO biologists—not to mention endless hours reading and re-reading their documents and other DFO material to which I had access.

My work on the cases involved considerable moving around. I conducted the majority of my work with the Kwakwaka'wakw diggers before starting work with the

Nuu-chah-nulth. My work with the contaminant ecologists was interspersed with my work with the two First Nations groups. I collected the majority of data on the DFO experts after completing my work with the other three sets of practitioners. The interspersed structure of my data collection was partly by choice and partly of necessity. I wanted to be able to move back and forth among the various experts' knowledge production processes to make sure I was thinking critically about all of them. This was not always possible as their schedules were organized according to their needs, not mine. Moreover, they had to organize their work around the timing of tides, fluctuations in the market, government regulations, funding restrictions, teaching schedules, and other constraints. As a consequence, I worked with them whenever it was possible.

The analysis of my four case studies is a story unto itself, but not worth describing here in any detail. Suffice it to say I transcribed, electronically recorded and/or downloaded all notes, recorded interviews, fieldnotes, research reports, computer files—anything and everything I had—into a format that could be used for analysis in Atlas.ti, a qualitative analysis software package. I then proceeded to read and re-read these imported documents, develop codes, reject codes, create different codes, link portions of text to codes, and write memos. I then created “data files” into which I dumped everything with a given code (e.g. everything coded as “cognized environment”). From here I organized these quotes into themes or sub-codes. Finally, I began writing—a process of analysis in itself.

Chapter 4: Social Relations and Interests

This chapter is about two aspects of knowledge production—social relations and interests—and how different types of relationships between them can constitute different ways of knowing. Knowledge practitioners rarely work in complete social isolation. Even the Kwakwaka'wakw diggers with whom I worked, who developed much of their knowledge about clams independently from others, were still embedded in a web of relations with other clam diggers. Moreover, like the other knowledge practitioners with whom I worked, the web of relations in which each set of knowledge practitioners were caught up shaped what they did and how they did it. The particular web of social relations in which they were embedded differed considerably, though, depending on their interests. As the following chapter illustrates, interests and social relations play an important role in regards to how one comes to know, with differences resulting in different ways of knowing.

My focus in this chapter is on two of the four sets of knowledge practitioners with whom I worked, the Kwakwaka'wakw diggers and the contaminant ecologists—and on one interest over which they diverge—whether or not they aim to make their knowledge known to others.⁵⁴ In many ways, these two sets of knowledge practitioners are polar opposites in regards to their interests. The Kwakwaka'wakw, for a variety of reasons, prefer to withhold their knowledge from others or create private knowledge. The main goal of the contaminant ecologists, on the other hand, is ultimately to communicate what they learn about clams to others or to make public knowledge. If one was to draw a

⁵⁴ The other two sets of knowledge practitioners, the DFO biologists and the Nuuchahnulth clam diggers, can also be discussed in terms of their interests in making their knowledge public (or keeping it private) and how this is related to the web of relations in which they were embedded with others. As I discuss at the end of the chapter, though, they do not fall at the same extreme ends of the public-private continuum that I describe here.

continuum in terms of interests, then, these two knowledge practitioners could be placed near opposite ends (see Figure 4.1).

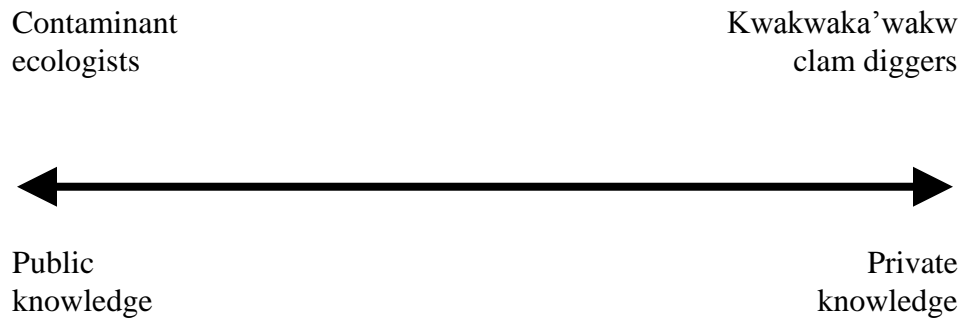


Figure 4.1: Public/private knowledge continuum

Interests, Interests, Everywhere

Interests are everywhere in human life and ways of knowing are no exception. Sociologists, historians and anthropologists of knowledge have spent considerable time documenting and analyzing ways in which interests influence the production of empirical knowledge. Susan Leigh Star (1983) listed a variety of ways in which scientists have adjusted their knowledge construction practices due to interests. These include the simplification of terminology and concepts used in research to facilitate communication when working jointly with specialists in other fields; simplification (and perhaps overgeneralization) of conclusions to aid communication when conveying knowledge claims to an agency or client that wants to apply the knowledge; the collection of data more quickly, from a smaller area or from a less-than-ideal source due to time and budget

restrictions; being required to provide conclusions on a pre-determined schedule that does not allow full exploration or analysis of the data; and ‘rules of thumb’ used in a field that limit how knowledge can be analyzed, organized and communicated to others. Knorr Cetina (1983) and others (e.g., Zenzen 1979, as cited in Knorr Cetina (1981) have noted how, in the interests of getting their job done (i.e. in constructing knowledge), many scientists have resorted to what or who is immediately or easily available in their work site, such as: tools, equipment, materials on hand, journals and books available through a library, funding for hiring a new technician, office space, computer software, services and companies already on contractual arrangement or locally available. Dean (1979) discusses how an unresolved debate over how best to classify species is grounded in practitioners’ competing interests over status, efficiency, and the use of different tools (DNA analysis compared to phenotypic comparison). Porter (1986) discusses how insurance tables, based on statistical averages for a given groups, were useful for insurance companies but of little value to individuals who wanted to know what would happen to themselves. Lloyd (1987) talks about how the ancient Greeks, whom he describes as egoists, were interested in making innovations (or at least claiming they were innovative) and therefore purposefully established their personal presence in texts and making extensive criticism of others. Palsson (2000), talks about how fishing skippers rarely mention the principles by which they make decisions because, at least in part, they are guided by practical results rather than an interest in theoretical advancement. This list of interests and how they affect knowledge practices is far from comprehensive but makes it clear that interests are present in a variety of capacities in knowledge practitioners’ production of knowledge.

The four sets of knowledge practitioners with whom I worked were no different from the knowledge practitioners described above; they, too, had interests that shaped the process by which they produced knowledge. To a certain extent, these interests overlapped, at least in the sense that the questions to which they sought answers overlapped.⁵⁵ Their motivation for answering these questions differed considerably, though. For the First Nations diggers, who were interested in clams for food and economic gain, answers to questions about clams helped them harvest larger quantities of clams. To be effective diggers they also needed to know which beaches to go to, what equipment to use, when to go (season and tidal exchange), how to get there (boat, safety issues about the beach), where to dig on the beach, how to dig (e.g. depth, motion of tool so as to maximize harvest and minimize shell breakage), how to identify clam species (including recognition in the dark), which clams were worth keeping (which are healthy), how to store clams, and where to sell them. Knowledge is the lubricant that greases the wheels of motion for any practitioner, and it was no different for the First Nations clam diggers.

The contaminant ecologists, like many other academic scientists but unlike the Kwakwaka'wakw diggers, were not going to use their conclusions about clams in any direct, applied sense. In fact, when asked what he thought about eating clams, the PI on the contaminant ecology project said he had “no interest what-so-ever [in eating them]. I don't know, they are just gross.” Instead, his goal was to have his research findings become part of public discourse, specifically the discourse among those in their academic field. The acceptance of their knowledge was not a given, though; as is well known,

⁵⁵ For example, the DFO biologists and the First Nations commercial diggers were interested in knowing the abundance of clams on a beach and the contaminant ecologists and First Nations were interested in how the fish farms were affecting clams and the clam beaches.

scientists do not simply conduct research, write their conclusions and distribute them to eager knowledge consumers (for example, see Knorr Cetina 1981). Instead, they must convince others of the validity of their conclusions. To do so, they have to become rhetorical salespeople of a sort, writing an article or other form of communication that convinces others of the validity of their conclusions, both at the level of the peer reviewer, who provides the initial seal of approval, and then at the level of the wider scientific public.

In contributing to public discourse, the contaminant ecologists were not acting with benevolence. These scientists also had personal interests at stake. The ecologists want credit for the knowledge claims they generated. A peer-reviewed article has currency in the academic field. Ecologists, like many other university faculty, are hired, promoted and granted research funding, in large part, on the quality and number of their publications. Quality is measured by whether or not the publication was peer-reviewed. One of the contaminant ecologists' main goals was thus to produce as many peer-reviewed articles as possible. As a result, each of the lead scientists was continuously figuring out what they could publish in relation to the research conducted. Even the junior scientists were encouraged to think along these lines.^{56, 57}

⁵⁶ For example, I was highly encouraged to publish anything in peer-reviewed journals I could from the clam consumer survey for which I was responsible. This was beneficial to the senior members of the team in two ways: (1) unlike most social science articles, science articles have long authorship lists, and being the PI or a senior member of the team ensured they would be listed; (2) any publications I made would have to give credit to the funding agent and the research project which would bring more recognition to the project and hopefully more funding—which would eventually lead to more publications.

⁵⁷ The DFO biologists, like the contaminant ecologists, were also creating knowledge for an audience other than themselves. As part of the bureaucratic machinery of the Federal Government, their role was to provide the scientific assistance required for establishing harvest rate regulations. The process by which their knowledge becomes public was somewhat different than the contaminant ecologists, however: peer review was conducted by a single committee (a semi-independent committee) instead of individual reviewers associated with the journal or conference of one's choice, and knowledge and policy were approved jointly and made public by upper Federal fisheries administrators.

This stands in contrast to the Kwakwaka'wakw clam diggers' knowledge goals of producing knowledge that they themselves will directly employ. Clam sales are between an individual harvester and the buyer; diggers did not sell as a household or a community. Moreover, the Kwakwaka'wakw diggers were not accountable to anyone in terms of how they harvested clams, with the exception that they were expected to act in a sustainably-responsible manner on the beaches and to help others load and unload the transport boat. The only incentive they had to make their knowledge public was to help others, as novice diggers had much to learn from experienced diggers. Generalized reciprocity is a common practice among the Kwakwaka'wakw and many other First Nations. As described below, however, there are good reasons why one would not want to help others—good clams and clam beaches are in limited supply, and healthy competition is definitely in the commercial diggers' interest. Unlike the contaminant ecologists, the harvesters did not have incentives or social pressure to make their knowledge claims believable or convincing to others. They may experience social pressures within their community to be honest, but, as already described, others were often treated with an air of suspicion as well. For most diggers, they felt their responsibility was to themselves and their future work.

For the DFO biologists, the research report was published for alternative reasons to that of the academic scientist; the “working paper” was published to justify the policy or regulations implemented by DFO. Regulations have consequences for clams, clam harvesters, and the ecological health of the coast. If there is public outcry that the DFO has mismanaged the resource, the DFO scientists will be put in the position of having to justify their management decisions. Although clams are not a big-ticket harvest, public accountability is a very real issue; the East Coast Canadian cod stock collapse and ensuing investigations into the reasons for its collapse made it clear that government scientists can not hide. The review process and subsequent administrative stages before the recommendations become policy are designed in the way they are *because* the DFO is accountable for their actions. The scientists thus do not implement policies and regulations without peer-accepted evidence that their knowledge and policies are good (by scientific standards), thus protecting the stakeholders from the DFO scientists implementing whatever policy they want and the DFO scientists from being accused of doing so. As can be seen, there are differences between the two groups of scientists as to why and how their knowledge is made public. That said, the two groups are the same in the sense that their ultimate goal in producing knowledge is for its use by others.

The knowledge goals of the set of knowledge practitioners had implications for how they organized social relations amongst themselves and their peers. Differences in regards to their interest in private and public knowledge and how this affected their respective ways of knowing pivoted around four key points: the division of labour, trust, embodiment of knowledge, and standardization. These issues are inter-connected, as will be seen below.

Division of Labour

I use the term *division of labour* in the classic sociological sense to mean the division of tasks involved in the production of a good or service into specialized tasks in which different individuals are responsible for fulfilling each task. In this section I outline how the two sets of knowledge practitioners organized their labour. Further discussion of the division of labour continues later in relation to other key issues about public and private knowledge production.

The two sets of knowledge practitioners worked in different types of group settings. The contaminant ecologists worked on a “project” as a team of scientists with community partners. The research project was too cumbersome and required too many types of expertise and/or too much time for one person to complete. Consequently, labour was divided into distinct components—research design, sample collection (plus data storage and transport), sample preparation, sample analysis and raw data adjustment, interpretation, and the drawing and write-up of conclusions—and each component was further divided by task. For example, 5 to 6 people participated in sample collection trips

in which tasks were assigned by the PI to each of us⁵⁸: the boat driver was responsible for transporting us to the beaches; the Kwakwaka'wakw members and I were responsible for digging clams; I was also responsible for collecting clams from the diggers and placing them in the water bucket (to get rid of the dirt); the PI was responsible for sorting the clams by species and size class, and then packaging them in specially-treated tin foil.

Labour was assigned by expertise.⁵⁹ The commercial clam diggers were responsible for digging clams; the principal investigator—trained in research design, methods by which to separate contamination from other source, and data analysis—was responsible for designing the research methodology, wrapping the samples in a way that prevented post-harvest contamination, and analyzing the research data. The lab technicians, responsible for preparing the samples for chemical analysis, were educated as bio-chemists and had previous experience preparing samples in a bio-chemistry lab.

In contrast, the Kwakwaka'wakw diggers worked collectively but as individuals. They dug in groups of anywhere from two or eight or more. The diggers did not work together, however—their process of learning was not a “project” as it was with the contaminant ecologists. Collective travel and work was for convenience (not everyone has a boat), safety, and socializing, and their division of labour was minimal. Moreover, the Kwakwaka'wakw diggers did not divide the task of digging amongst themselves;

⁵⁸ I use the personal pronoun “us” here because I was one of the individuals assigned this task by the PI.

⁵⁹ For the DFO project, severe restrictions in terms of budget/funding necessitated reliance on others' voluntary labour contributions. As a result, beach surveys methods (collection of the clams) along with clam measurement were approved by the lead DFO biologist but undertaken by depuration staff members and volunteers. DFO staff members were responsible for entering these measurements and a DFO scientist was responsible for assessing the likelihood of accuracy or problems with the recorded data. Division of labour for the DFO project was therefore not necessarily according to expertise, as the volunteers did not necessarily have expertise. In fact, it was an open question by the semi-autonomous review board as to the quality of work provided by industry (depuration facilities) in regards to following scientific procedures of with which they had little training or experience. Lack of funding, however, necessitated reliance on these untrained individuals.

everyone dug for his or her own clams. Even couples who dug together did not pool their harvest. As a result, the Kwakwaka'wakw diggers do not specialize in a particular type of expertise, such as one person knowing how to set up the equipment and another knowing how to store the clams until delivery day. All diggers were independent agents working collectively to facilitate their own harvesting of clams and all develop the same set(s) of expertise.⁶⁰ Both the contaminant ecologists and Kwakwaka'wakw diggers thus coordinated activity with others, but the type of coordination differed: those interested in producing public knowledge were involved in a multi-person team project involving community partners in which labour was divided whereas those interested in private knowledge worked in collectives of independently-working individuals.

Embodied knowledge

In many ways, the Kwakwaka'wakw's private knowledge was also embodied knowledge: they knew when a clam was present underground by the way the clam fork moved through the sediment; they could distinguish between alive clams and empty clamped shells by visual appearance; they knew how to distinguish little neck clams from small butter neck clams in the dark by the texture of the shell under their finger; they knew how to push their digging fork into the sand so that it did not break clams; they knew how to tell (without using a ruler) whether a clam was big enough to be legally harvested; they knew how to look after the clams for several days before they were sold

⁶⁰ The harvesters did have additional chores to that of digging, such as refilling their gas lanterns with fuel, making sure the clam sacks were watered down (if stored on the docks) and cleaning up the boat(s) after digging for the night. These tasks were typically distributed among the Kwakwaka'wakw diggers on the crew. Some do try to duck out of their responsibility, but those who consistently did so were kicked off the crew. (The Nuw-chah-nulth diggers, in contrast, tended not to work as collectively as the Kwakwaka'wakw and each digger was responsible for his/her own equipment.)

to the buyer; and they knew where to stick their fork in the sand to find clams on the beach. This type of knowledge is embodied in that it is knowledge that can not adequately be captured by words. Instead, it is knowledge that is housed, at least in part, in one's body and actions.⁶¹ Embodied knowledge is more than what one knows through verbal description. It is what one knows in a deeper, fuller sense and can be implemented in one's activities in a way that exclusively verbal knowledge can not.

In contrast to the Kwakwaka'wakw diggers, the contaminant ecologists' knowledge about clams represents a combination of embodied and disembodied knowledge. In the past, sociologists of science (e.g. Knorr Cetina 1999) have argued that scientists' knowledge is embodied.⁶² I am not attempting to argue the opposite. The question is *what* knowledge is embodied. As many recent cognitive theorists have argued, we learn in contexts and by doing (e.g. Chaiklin and Lave 1993; Lave 1991). To figure out what someone knows (in an embodied sense) we have to figure out what they do or did previously, such as which empirical phenomena they were exposed to or interacted with, when they were exposed to it, for how long, and what equipment they used to interact with it. As knowledge is modulated through experience, *what* knowledge

⁶¹ Gatewood (1985) captures the idea well in terms of his experience with fishing. In response to overhearing a tourist who had never been a commercial fisher explain how a "set" (the practice of casting and retracting the net) worked:

His account differed in no significant way from the story I would have told, yet I submit that he did not know what he was talking about....After a couple of openings a rookie not only can talk a good set but he knows what it means to make a set. As he relates to specifics of his job routine to the operation as a whole, the jargon takes on deeper meaning. With experience, hauling gear, for example, becomes more than just a colourful addition to one's vocabulary and acquires all sorts of connotations....Its meaning is lodged in muscles as well as words.

(Gatewood 1985: 208-9)

⁶² For example, Knorr Cetina (1999) argues that molecular biologists need to be skilled workers and that not all scientists are equally skilled or capable. She relates a story about one particular postdoctoral student who was incapable of mastering the skills required of him in the lab, eventually leading him to quit research and take up teaching full time.

one comes to embody is influenced by the tasks one is responsible for. The PI on the contaminant ecology team was involved in work on the clam beaches during which time he developed embodied knowledge about, for example, the best way to wash and wrap clams to prevent the clams from being exposed to other contaminating materials. The chemist and lab technicians, in contrast, may not know what a clam beach looks like, much less what the specific beach was like from which the clams were harvested. In fact, the one question I was asked repeatedly by the laboratory technicians was exactly this—what were the clam beaches like. They had no contact with this aspect of the research and were curious about the origin of the tissue with which they worked. The point at which these workers first encountered clams was as a frozen lump of flesh in a freezer. They developed embodied knowledge about things such as the way to mush the clams and diatomaceous earth together or how to add PCB surrogates to the clam extract. What they learned about clams, at least in an embodied sense, was then limited to what can be learned about clams after they are dead and frozen.

Moreover, no one person on the contaminant ecology team was involved in the entire process from the inception of the research question to the crafting of the final conclusions. Even the PI, who came closest to this, did not participate in the lab portion of the research. As a consequence, the final conclusions he drafted were based on externalized data that the PI cognitively assimilated and assembled at the end of the research project. These conclusions were generated through a process by which labour was divided among a group to produce knowledge from their pooled collective experiences and the outcomes of those experiences. No one individual embodied the knowledge upon which the final knowledge claim was made. It is in this way that I claim

that the contaminant ecologists have only partially-embodied knowledge about the knowledge they generated about clam contaminants.

Whether or not the knowledge practitioners created embodied knowledge was not an arbitrary decision. Attitudes towards embodiment by the knowledge practitioners represented deeper philosophical positions. The fact that the final conclusions drawn by the contaminant ecologists were largely disembodied did not present a problem for them. In fact, there are philosophical reasons expressed in science giving reason to doubt one's sensory experiences (Descartes 2003; Knorr Cetina 1999: 94). How much this influenced the thoughts of the contaminant ecologists I do not know. I do know, though, that embodied knowledge is often difficult or even impossible to verbally communicate. As the main goal of the contaminant ecologists was to communicate their knowledge to others, having embodied knowledge was not particularly important. Moreover, it is common practice in ecology, as well as in other fields of science, to present "findings" that are based on the collective endeavours of team members. Moreover, the usual goal in scientific research publications is to present knowledge based on "objective" observations and measurements. Embodied knowledge thus served the contaminant ecologists to the extent that it facilitated their work but it did not figure largely in the conclusions they drew about contaminant levels in clams.

Although it was never expressed in quite this way by the Kwakwaka'wakw diggers, their feeling was that verbalizing knowledge flattened and distorted what they knew. What the body knows can not always be put into words and, when it is put into words, something is left out.⁶³ For example, when I asked several of the diggers what

⁶³ Jan Douwe Van der Ploeg (1993: 212, 215) describes embodied knowledge such as that produced by the Kwakwaka'wakw diggers or, in his case, potato farmers, as an attachment and connection to that which is

they looked for on a beach to locate clams the majority of them could not tell me. Most shrugged their shoulders or gave me a perplexed look. A couple diggers tried to explain how they look for rocks or formations in the sand, but also said there was more to it than just that and that they could not explain it well in words—that I was better off watching them to see where they dug.

The idea of words flattening and distorting understanding has been expressed elsewhere. For example, in his comparison of ancient Greek and Chinese medicine Shigehisa Kuriyama (1999) describes this in relation to the pulse. In Greek medicine, the pulse was (and still is) a measure of frequency of pulsing in the veins. In Chinese medicine, the *mo* was the “strength” as well as the frequency of the pulse. The strength of the pulse was extremely difficult to verbalize, however, and in their attempts to put the sensation into words these practitioners used a variety of terms such as “anting,” “worming,” “hard,” and “soft.” The Greek-trained practitioners, in contrast, preferred more standardized descriptions and sought ways in which to make the pulse consistently verbalizable to others. Ultimately, this meant these practitioners limited their use of the pulse to its frequency and ignored its other aspects. As Kuriyama insightfully asks, however:

Why did pulse takers keep blaming language for the uncertainties of the fingers and the mind? ..[N]othing more distinctly characterized the history of discourse on the pulse than this nervousness about words. We encounter it time and again—the haunting sense that vague terms are blunting, distorting, and misrepresenting what the

studied. In quoting Meendras (1970: 47), he says *the* farmer “felt as if he had ‘made’ his field and knew it as the creator knows his creation, since the soil was the product of his constant care: plowing, fertilizing, rotating crops, maintenance of fallow ground, and so on.” Van der Ploeg goes on to state that “it should be pointed out clearly that no plot, understood as a specific set of phenotypical conditions, can be interpreted as a static unity. In the medium and long run they can be improved, precisely because they are the subjects of the farm labour process itself. So progress can be made and new experiences can be gained: through cycles of observation, interpretation, evaluation and manipulation, the scope of the *art de la localité* is enlarged, which enables the farmer to obtain new insights” (P. 215).

fingers feel, the restless urge to rename and redefine, the ever-renewed hope that this time one might get it right. As if the failures securely to grasp the pulse were really just failures properly to name and describe it. As if the problem of knowledge was, at its heart, a problem of words.

(P.70)

Emphasis on embodiment versus words is thus about more than just about whether one wants to make knowledge public or keep it private; it is about what one wants to gain when one says one “knows” something.

Trust

As Durkheim argues (1984), division of labour produces solidarity in the sense that group members come to rely on one another for whatever they are producing, be it shoes, knowledge or access to goods in society. This can be clearly seen in the case of the contaminant ecology team in that their labour was divided and they were therefore required to rely on one another. The Kwakwaka’wakw diggers, on the other hand, do not rely on others in the same way. It is a different form of solidarity that holds these knowledge practitioners together. Yet their cohesion is not clearly based in a shared mechanical-like collective consciousness, at least not in the sense that their understanding of clams is identical.⁶⁴ In this story, the cohesive glue that holds the former together is trust whereas for the latter, trust only extends to the physical activities that facilitate their coordinated movement to and from the beaches; a clear lack of trust exists beyond this. Although they rely on each other for safety and companionship, the Kwakwaka’wakw diggers, for the most part, exhibited a lack of trust for one another for three reasons: (i) in their view, experience, not the words of others, is the best teacher, (ii) others can not be

⁶⁴ As I describe at the end of this chapter the particular configuration of social relations and practices amongst the Kwakiult clam diggers resulted in low inter-subjective agreement in terms of many of their ideas and theories related to clams.

trusted to tell the truth, and (iii) they can not trust how others will use the knowledge they pass along to them.

The Kwakwaka'wakw diggers talked about experience being the best teacher.⁶⁵ This is consistent with the idea of embodied knowledge being, in their view, a superior form of knowledge. While they talked about learning by watching other diggers, few talked about learning by being told what to do. Elders are reported to have invited youngsters to join them in their activities such as hunting, cedar bark harvesting and clam digging. The elders answer a few questions, but the majority of learning occurred through observation and a person trying it himself. The elder may then follow up by making a few observations or recommendations.⁶⁶ An underlying sentiment was that others can teach you, but who is to know if the information will work for you. There may be something that they did not explain (or were not able to explain) that makes the information less than accurate or helpful.

⁶⁵ This epistemological stance, in which experience is the best teacher, is apparently common with a number of knowledge communities. Blurton Jones and Kronner (1998) describe how !Kung hunters relate hunting stories to one another for entertainment, not instruction. Instruction is gained by young !Kung men about animals and technology by watching and listening to others and then trying it for themselves instead of direct, verbal instruction. Similarly, Polynesians (in this case, those from Pakaouka and Tikopia) are reported to emphasize learning-by-doing. For example, place names on a reef and the names and characteristics of reef fishes are gradually acquired as boys accompany their fathers on fishing trips (Ruddle 1994). Likewise, according to Scollon and Scollon, the Chipewyan from the Arctic regard the written word as hearsay. "Knowledge that has been mediated is regarded with doubt. True knowledge is considered to be that which one derives from experience" (1979: 185, as cited in Ridington 1988). As the authors report "far from rejecting order," such an approach to knowledge "seeks a fully integrated view of world order in which there are no elements felt as foreign" (*ibid.*)

⁶⁶ This style of training is likely associated with a lack of emphasis on verbalization in general among many Kwakwaka'wakw. Many consider it rude to ask too many questions. Moreover, it is inappropriate to talk unless you have something to say. Gossiping aside, small talk is considered unnecessary. Instead, people feel comfortable sitting in the company of others with long, drawn out silence; while considered awkward in Euro-Canadian social settings, in Kwakwaka'wakw circles silence is an acceptable pattern of interaction. Given this social norm, it would make sense that the emphasis in learning would be similar—that knowledge is something observed, internalized and acted upon as opposed to something expressed verbally to others.

Trust is also an issue for the Kwakwaka'wakw diggers in that others' knowledge claims can not be trusted; how do you know if others are telling you the truth?⁶⁷ Among the Kwakwaka'wakw, it is not uncommon for someone to be called a “bullshitter” or *klex'sum*. While some individuals are better known for their “bullshitting” than others, everyone is suspect. This is true regardless of the topic—whether people are talking about clams, family or what they did last night. Consequently, when someone says they heard (for example) that clams are black when found near fresh water streams, the instant response is “who told you that?” As most diggers know one another, the answer to this question is extremely important—the knowledge needs to have come from someone who is both potentially knowledgeable and who is considered less of a bullshitter than others. If the source of information is *not* acceptable on both levels, the information will be dismissed. If it passes on both levels, it will be accepted as potentially true but still in need of personal verification.

There are situations in which the Kwakwaka'wakw diggers are asked to share or demonstrate their knowledge to others. For example, by traveling together to beaches, the more experienced diggers end up showing novice diggers which beaches to dig at. In the past, experienced diggers thought nothing of going to good beaches to dig when they had novice diggers on their crew. The experienced diggers have since learned to keep their knowledge to themselves, though, even if it means they have to dig at mediocre beaches on the nights when novice diggers are with them. Some experienced diggers claim that novices have returned to these beaches later and over-harvested these beaches, leading to their “death” (i.e. not having enough clams on to make it worth digging). But

⁶⁷ Such suspicion of others was also reported by Blurton Jones and Kronner (1998: 331) of the !Kung hunters, who rarely discuss hunting with one another beyond general conversation and expected skepticism from others when asked to do so.

even experienced diggers do not want other experienced diggers to know their digging secrets. Diggers are in competition with one another. Competition can be overt in that some diggers were driven to dig faster so as to harvest more sacks of clams than others. Competition was also indirect and covert. Competition arises because all diggers, especially long-term diggers, want to make sure they harvest enough clams to make a living. Withholding good digging spots (“hot spots”) and tricks they have for finding clams from others are par for the course among all but close relatives and trusted friends.^{68,69} For example, after telling me her strategy for locating large butter clams on beaches, one female digger turned to me and said “but I shouldn’t be telling you my secrets. If the other diggers know how I do it then there won’t be any clams for me.”

The contaminant ecologists stand in clear contrast to the Kwakwaka’wakw diggers on issues of trust. Trust was a prerequisite if labour was to be divided among project team members; dividing labour required the PI and other senior team members to trust the lower-level workers on the team, such as the lab technicians and those digging for clams—individuals with no vested interest in the outcome of the project—completed their work properly. They needed to trust that the junior employees prepared the equipment/materials correctly and completely and collected and prepared samples in the way they were told to do so. While technically the senior scientists oversaw the work of

⁶⁸ I suspect the secretiveness about beaches is relatively new. One of the oldest diggers told me that diggers used to communicate more amongst themselves regarding where they had already dug that season. Ironically, their reason for doing so was to prevent fellow-diggers from over-harvesting beaches. (This is ironic because their present reason for secrecy is to prevent novice diggers from knowing about good beaches and then over-digging these beaches). Consistent with this, another Kwakwaka’wakw man told me he thought the mind-set of diggers had changed since he dug as a child. He felt the increasing commercialization and government regulations around clam digging had changed the way in which diggers treated the activity and altered their relation to other clam diggers.

⁶⁹ Although most diggers try to keep their knowledge secretive, there are “sayings,” “hypotheses” or “theories” that are commonly stated by diggers, such as butter clams organizing vertically in three layers under the sand, that manila clams found near freshwater streams have rust coloured shells, or that running fresh water cleans the toxins out of clams. These tend to be more abstract statements than information about specific sites.

others, in practical terms this was not always possible. For example, the PI wanted the sample clams to be collected far from any source of fresh water (e.g. streams on the clam beaches). The diggers (myself included) had limited time to dig clams on any given beach and not every beach had a lot of clams. Moreover, manila clams (the target species for work in the Nuu-chah-nulth area) prefer beach areas with fresh water run-off, making this the likely place in which to find these clams. Confronted with the choice between not finding enough clams or finding clams close to fresh water (or being so desperate to find clams in a limited amount of time that we forgot to check whether we were close to fresh water!) meant the diggers did not always follow the principal investigator's directions. Moreover, the PI was too busy with his own activities to watch where the diggers were finding their clams. Similarly, the lab work was conducted on Vancouver Island but the PI lived on the mainland. Traveling between the two locations was time consuming and costly. As a consequence, the PI did not make regular trips to the lab. Instead, he explained to technicians what he wanted done near the beginning of the project and left them to do their work. It was not until later in the project, when I was working with the PI on a manual describing the lab work, that the PI found out the technicians were not following all of his initial instructions.⁷⁰

A similar issue of trust arose among the senior scientists. As stated, each scientist had a distinct set of expertise. For example, the principal investigator was a field ecologist and the lab scientist was a chemist. Neither of them could have switched roles, as neither had the expertise to perform the role of the other and both readily admitted

⁷⁰ Instead of compiling 20 grams of clam flesh from a number of clams in a composite (their sample unit), the technicians were using the largest clam to obtain this amount of flesh.

such. This meant that they both had to rely on each others' respective expertise—they had to trust one another.

It is true that their trust was not absolute; others were trusted until there was reason to doubt such trust. The PI had confidence in us clam diggers because he did not witness us digging for clams near fresh water run off. Similarly, he had confidence in the technicians' clam preparations until he was informed of deviation from his desired protocol. He checked the data he was supplied from the lab to see if the figures looked plausible and that all sample codes he had established were present. He also checked to make sure the lab log books containing records for the processing and analysis of each sample were kept up-to-date. For him, trust remained as long as superficial appearances justified that trust.

But trust had a much further reach than just within the contaminant ecologists' team. Trust was the basis of relations the scientists had with their peers. This extended from the ecologists' initial schooling in which they were told to trust authority figures, namely their instructors and textbooks, to the trust they were hoped to be extended by other practitioners regarding their new-found knowledge claims. In the case of the latter, trust was not expected to be extended blindly; one could not simply pronounce new knowledge claims and be believed by one's peers. Instead, trust was expected to be extended only under particular conditions. Specifically, the contaminant ecologists expected they could convince others of their knowledge claims if they used methodology they thought would be acceptable to their peers. The best way to ensure this was to use methods and materials similar to those others had used previously. For example, the mass spectrometer was purchased from one of the major suppliers of such products and

the settings on the mass spectrometer were set according to standards used by other labs. These settings were clearly recorded and specified in their reports. The surrogate added to the clam extract in the preparation procedure was purchased from the same company as other contaminant ecologists purchase their surrogates. In the report, the name of the company along with the city in which the branch was located from which their surrogate originated was reported. Even in problem-solving situations, these practitioners chose among existing conventions used by others in their field. For example, it was not initially clear to the PI how he should analyze the project's data as he was not sure what to expect. If the beaches showed a clear ranking of contaminant levels in clams then he could analyze it differently than if there was no clear hierarchy. He figured out the exact analysis procedure he could use as he explored the data. Yet, in doing so, he did not construct a method of analysis from scratch. Instead, he explored the data using a variety of existing, accepted, standardized statistical methods. To borrow Swidler's metaphor (1986), he solved his problems using tools from a ready-made tool box. His purpose in using these ready-made tools and techniques was, in large part, because he believed the team would be extended trust by their peers if the procedures they used to generate their knowledge claims were standardized, acceptable and therefore trust-worthy.⁷¹ In other words, trust from peers was based in both the techniques, materials, tools and use of

⁷¹ Karin Knorr Cetina (1981) described this well in terms of the scientists with whom she worked:

If we look at the process of knowledge production in sufficient detail, it turns out that scientists constantly relate their decisions and selections to the expected response of specific members of this community of "validators", or to the dictates of the journal in which they wish to publish. Decisions are based on what is "hot" and what is "out", on what one "can" or "cannot" do, on whom they will come up against and with whom they will have to associate by making a specific points. In short, the discoveries of the laboratory are made, as part and parcel of their substance, *with a view toward* potential criticism or acceptance (as well as with respect to potential allies and enemies!).

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standards by the knowledge practitioners *and* in the honesty of these practitioners to have actually and carefully followed the methodological procedures they described.

According to Lorraine Daston (1992), the use of conventional procedures in science developed as a response to the changing nature of the scientific community. Specifically, when the number of scientists from an increasing number of nations began participating in any given field this led to a decreasing number of personal relations among the scientists and a corresponding lack of trust. To compensate, scientists increasingly turned to statistics and other conventional, standardized methods—the more mechanical the better. Trust through personal relations was thus replaced by trust in specified procedures. This form of trust still required some form of personal trust, though. Shapin argues (1995) that a culture of honour among scientists developed around this time as well in which it was considered dishonourable for one to falsify their data or intentionally lie about their knowledge claims. Combined, trust in conventions and trust in the honesty of those who reported using those conventions replaced interpersonal trust. Scientific conventions serve the same purpose today. As Theodore M. Porter frequently points out in his work (1992a, 1992b), rigid protocols such as those used in science are for political purposes, for knowledge that is to be made public; they lend credibility to the knowledge practitioner and convince others of the soundness of the work.

The contaminant ecologists were no different from the scientists described by other sociologists (e.g. Knorr-Cetina 1981; Lynch 1983; Daston 1992) in that they were constantly oriented towards making their work acceptable to their peers. They were concerned about the review process in almost everything they did; they knew that for others to accept their work they had to follow acceptable conventions. Trust was

therefore a central organizing principle in terms of the contaminant ecologists' relationships to others in their field, influencing not only their work amongst members of their team but also how they related to other scientists outside their team. In fact, the specific relations that emerged due to issues of trust were at the very heart of the process by which the contaminant ecologists organized their work.

Trust thus represents an important difference between the Kwakwaka'wakw diggers and the contaminant ecologists. Trust had a major impact on their relationships with others which, in turn, impacted their ways of knowing. The Kwakwaka'wakw were wary of trusting others as it is difficult to know whether others can be trusted—they may provide you with false information. Moreover, telling others what you know can be to your detriment, as the knowledge can be used destructively or to one's competitive disadvantage. If knowledge is the best teacher and embodied knowledge largely develops from experience, then there is no particular reason to share one's knowledge. The contaminant ecologists, on the other hand, worked on the assumption that the ability to accumulate knowledge and build on what others have learned requires them to trust accepted procedures and accredited others' honesty and ability.

Standardization

Above I stated that the contaminant ecologists employed standardized, conventional practices employed in science as a means by which to try to convince their peers to trust their knowledge claims. Standardization was important in several other regards. Anyone on the contaminant ecology team performing the same task more than once or by more than one individual was encouraged to perform the task in a similar

manner.⁷² Standardization was encouraged as it increased consistency of actions among individuals over time and among actors. For example, the lab technician that prepared the clam extract for analysis followed the same routine each time she worked with clam samples. This included adding the same amount of surrogate each time, keeping the setting on the extractor at the same levels throughout the project and using the same grade of solvent in all her preparations. In the case that a technician other than herself was to eventually take over the preparatory work from her, she had documented her routine in written step-by-step instructions.⁷³ This was considered important because consistency created clarity in terms of what was done and therefore what could be reported to peers in reports, articles and conference proceedings. It also served to minimize variation within the project. As already described, the contaminant ecologists standardized their materials, techniques, actions, procedures and settings with those of other scientists working on comparable projects.

Standardization served two important purposes beyond those of trust. First, it coordinated the contaminant ecologists' actions with the larger knowledge community to which they belonged by minimizing variation between theirs and others' projects. If every project used different settings, new equipment, or different materials, it would be difficult to know what led to the particular outcome—was it the equipment, the setting on the equipment, the order in which an activity was undertaken, or something about the

⁷² This is not to say that all activities were standardized within the project. For example, on the DFO project the depuration facilities used one of two methods for selecting quadrats, depending on the expertise and comfort level of the staff.

⁷³ The DFO's procedures for clam surveys were similar in that they were carefully and comprehensively documented in a manual so as to coordinate the actions of the various depuration facility staff members with how DFO biologists conduct their surveys. This included step-by-step instructions on measurement procedures such as weighing all clams from the same quadrat (i.e. in a single sack), then sorting the clams by species so as weigh each individual manila clam which is then placed in a carefully-arranged row on a clean table.

phenomenon in question that resulted in a difference? By reducing the amount of variation within and between projects, the knowledge practitioner creates what is referred to in science as “control.” Control, or reduced variation, allows knowledge practitioners to identify the few things that changed between studies as the cause of any identified differences.⁷⁴ Standardization across projects thus increases the ability for knowledge practitioners to know that they are talking about the same thing. Coordination across projects allowed the contaminant ecologists to build on the work of others and have others potentially build on their work. For example, if another contaminant ecology team wanted to compare contaminant levels in store-bought beef and chicken to that of wild clams, they could use the same brand and model of analysis equipment, follow the same protocols and obtain their materials from the same source and, by doing so, they could compare their results to the results of this project. In other words, building their study on that of the contaminant ecologists and allowing knowledge to accumulate.

Secondly, as Porter (1992a) and others have pointed out in regards to other scientists, standardized rhetoric was employed to facilitate communication between the contaminant ecologists and their peers.⁷⁵ For example, the contaminant ecologists employed terms such as replicates, control, random selection (in comparison to their

⁷⁴ Not unrelated, scientists are prone to moving as much of their studies as possible into the indoors where the capriciousness of Mother Nature’s variations can be reduced. This relates well to Knorr Cetina’s discussion in *Epistemic Cultures* (1999: 25) on this topic.

⁷⁵ Collins (1974) has made the point that coordinated action does not occur through words alone. For example, proper building of the TEA laser never occurred without the builder traveling to the site of the original TEA laser and being shown how it as built; verbal communication among practitioners and the passing of immutable mobiles such as diagrams and manuals were not adequate. My intention is not to negate the importance of learning-by-doing; in fact, this topic is discussed in detail in chapter 6. In less technical situations as the TEA-laser, though, learning-by-doing ceased to be required as the skill and accuracy required in replication was considerably less and could be communicated through alternative means. This is the same as what Barnes (1982) meant when he claimed that all verbal understanding is based on ostensible experiences affiliated by an authority figure with terms at some time in one’s past; that there is no need to carry a swan around (or better yet, a collection of swans) at all times to communicate with others what one means by “swan.”

method of beach selection), sample, PCBs, analysis of variance, and bio-indicator which are terms known to others in their profession. As Porter claims in regards to statistics, “quantification is a form of rhetoric that is especially effective for diffusing research findings to other laboratories, languages, countries and continents” (1992b: 644).

Porter’s point can be broadened to include more realms than just statistics; attaching standardized parlance to specific actions, tools, materials and practices facilitates the standardization of those actions, tools, materials and practices across settings.

Standardized rhetoric allows for transmission of information about what they did as well as enabling them to know what others had already done.

But standardized rhetoric did more than facilitate communication. It also fostered inter-subjective understanding. Members of the contaminant ecology team, separated by geography, expertise and task, stayed in contact primarily through email and, to a lesser extent, phone calls. On three occasions the senior scientists and community partners of the team met face-to-face. These meetings were in large rooms with few windows and out of range of the clam beaches and laboratory. With the exception of when two or more team members worked together (e.g. on the beach collecting clams or on the lab preparing and analyzing samples), there was little opportunity to communicate in a non-ostensive fashion through symbols, photos and standardized rhetoric. The implication of this was that the team members’ thought processes also became coordinated, as all reasoning, description, and discussion occurred out of context of the references to which they referred. In fact, some team members never entered some of these contexts (i.e. clam beaches) and the understanding they developed was *exclusively* based on others’

interpretations and style of communication about an object. Their form of coordination also became a means of creating inter-subjective understanding.

Added to this was the process by which they generated their final knowledge claims. The PI, who wrote the report, spent considerable effort ratifying the report contents by all project team members. All those with their name listed as author were required to read and approve of its contents. Symbolic coordination also occurred between the contaminant ecologists and their peers through their reading of journal articles, talking with others, and listening to the conference presentations of others in their field. In all these settings, rhetoric acted to reinforce the consistency among how these practitioners talked and thought about their subject matter.

In contrast, the Kwakwaka'wakw diggers standardized very little of their work. They coordinated their actions with other diggers in terms of travel to and from the beach and to the clam buyer together but they each had their own routine for digging clams and looking after their clams until sold. These routines might initially appear the same, but this is only true at a coarse scale. For example, although all the First Nations harvesters first placed their clams in buckets and then transferred the clams to nylon mesh sacks, the specifics of how this was done differed from one digger to another. The most common way was to fill a bucket with clams and pour the clams from the bucket into the sack. At what point a bucket was considered full was a matter of preference; some diggers decided their bucket was "full" when it hit the three-quarter mark while others waited until they had to stop because the bucket was overflowing. Bucket sizes differed, moreover, as the harvesters used whatever buckets they had available. If it was a big bucket (e.g. an industrial size), then they may have tied off the sack after that one bucket. If it was a

smaller bucket, they may have poured two buckets into the sack. Some diggers, however, prefer to make their sacks bigger (fewer sacks to deal with but heavier sacks to move) and put two industrial-sized buckets full of clams in a single sack. As a consequence of all this, a “sack of clams” as a unit of measure was far from a homogeneous unit.

Similarly, the routine a digger employed for digging for clams was adjusted to suit the weather conditions, tidal level, and amount of light. Thus, while they often followed routines and coordinated their actions with others, the degree to which these routines and coordinated behaviors were consistently applied was only at a coarse-grain scale; none of them employed a protocol in which they consistently followed step-by-step procedures.⁷⁶

The Kwakwaka'wakw diggers were also unlike the contaminant ecologists in that, for the most part, they did not standardized the rhetoric they used with other diggers. For example, the Kwakwaka'wakw diggers expressed the amount of clams that could be found on a particular beach using a wide variety of terms including the number of diggers that could dig that beach, the number of boxes that could be harvested, the poundage a digger (or group of diggers) could take, the number of sacks that a digger (or group of diggers) could get or the number of boats filled with diggers that could be on a beach in a given night.⁷⁷ In fact, not only was rhetoric unstandardized, but the meaning affiliated

⁷⁶ One way in which the First Nations harvesters were relatively consistent was in terms of the tools and supplies they used. Almost all harvesters digging for butter clams used pitch forks and those digging for little necks and manila clams used rakes. All commercial diggers used mesh nylon sacks for transporting their clams.

⁷⁷ The choice of these specific terms largely reflected historical changes in methods of transport and containers for clams. In earlier years of commercial harvesting, clams were carried, unbound, by canoe to the buyer, at which point they were placed in boxes. This is the origin of the description of clam abundance in terms of boxes. Later, when clams were still being bought (based on weight) immediately after harvest and harvesters were more aware of how many pounds they had harvested each night, harvesters started referring to clam abundance in terms of poundage. The change in terminology can be seen even in the speech of a single harvester:

One of the best beaches is Deep Harbour. It's one of the biggest producers, year after year. Deep Harbour came back thankfully this year. I got 10 sacks a night there this year. I was afraid the fish

with the rhetoric—the practice or phenomenon affiliated with the rhetoric—was often inconsistent. For example, although most diggers talked about “black clams,” when I pressed the diggers for a more specific description of what this meant—was it the shell or body tissue that is black? Is it s true black or a blue-black colour, or a hue of grey?—I was provided with a number of answers, even from the same digger; I found diggers used the term “black clams” to refer to clams with black meat, clams with grey shells and clams with charcoal-coloured shells.

The objects and activities for which the Kwakwaka’wakw diggers did establish relatively standardized rhetoric were not about clams, per se, but objects around which they coordinated their actions, such as the names for equipment they passed to and from one another when loading and unloading the boat or when working together in preparation for digging, such as digging forks, gas lamps, and sacks, and the terms they used to describe their territory, such as the names of islands or waterways, to enable their discussion of where they would dig that night.

The result was that the Kwakwaka’wakw diggers did not develop high levels of inter-subjective understanding. Their primary form of coordination was through activities. Although they did not talk about clams with other diggers in any depth, the

farm would finally hit it [would kill it off], but it hasn't. Dad says can remember seven boats being there.

Note how the harvester used number of sacks and number of boats in the same sentence to describe the number of clams removed from a beach. Perhaps more importantly, note how the digger sees no problem in comparing clams using two different units of measurement: sacks and number of diggers (or boats with a family of diggers).

Yet different terminology can not be explained exclusively by historical changes in equipment and practices. Some harvesters have personal preferences in terms of how to describe amounts of clams. For example, one digger consistently described clams in terms of poundage, even though daily purchase of clams ceased over a decade before. Other diggers switched from one descriptor to another, depending on what suited their purposes. For example, another digger talked about the number of sacks he harvested in a single night and also talked about the number of diggers that could dig on a particular beach all night without the clams running out.

diggers were united by their collective activity of going together to the beaches and being able to experience first hand what others were also experiencing. What was lacking in these experiences was a strong collective interpretation or meaning beyond the basic level required for coordinating actions. For example, several diggers I interviewed talked about finding higher densities of clams close to small boulders (six 6 inches to five feet in diameter). Their explanation for why clams were found there could differ radically, however. For example, one digger claimed the clams lived there because the rocks acted as collection agents for the food clams consume. In contrast, another digger described clams using boulders as their castles (family homes) because they are a good location to reproduce. The larvae then move away and find their own castle. The former digger was surprised to hear the latter's description of why clams lived near rocks; it was something he had never heard or thought before. Yet the two diggers had dug along side each other on many occasions, unaware that their working theories differed.⁷⁸

Unlike the contaminant ecologists who actively worked on increasing their level of standardization, the Kwakuitl diggers considered their lack of standardization a positive feature of their work practice; while they coordinated the movement of themselves and clams to and from the beaches, they saw no particular need to standardize their routine. In fact, standardization at any fine-grain scale would present a problem as flexibility was often the key to adapting to situations or adjusting procedures to suit the needs and abilities of an individual. As one digger was fond of saying when asked about his plans: "we'll see when the time comes." Given that conditions on a beach are largely

⁷⁸ The same situation was found by Blurton Jones and Kronner (1998: 338) in their work with the !Kung hunters who are reported to have seemingly gain a lot of new information, or at least heard about observations and generalizations concerning animal behaviour which were quite new to them, when asked to participate in a group discussion about animal behavior.

unknown until one arrives, that the weather (and therefore the water) conditions are unpredictable, and that one only finds out shortly in advance of digging whether the buyer will be buying your preferred clam species, there is no point making hard and fast plans. The harvesters' work was conducted under circumstances of considerable uncertainty, making everything but the most general and immediate plans largely a waste of time.^{79, 80}

The Kwakwaka'wakw's lack of standardization in their clam-related knowledge work does pose a limitation for them in some regards. For political reasons, it is becoming increasingly important that the Kwakwaka'wakw digger's knowledge claims be accepted by others as valid. For example, in regards to the relationship between fish

⁷⁹ The ecologists were also subject to the capriciousness of Nature, even if to a lesser extent due to their bringing many aspects of their research indoors. Both the contaminant ecologists' and DFO biologists' research designs were modified over the course of the projects after they encountered difficulties and/or found better ways to analyze clam tissue. For example, the DFO biologists excluded beaches mid-way through the study that were negatively impacted by the study. Unlike the First Nations diggers, though, being required to change their plans was not seen as positive. In fact, it was considered a potential problem to the validity of their research findings.

⁸⁰ This idea of not wanting to be too prepared due to the capriciousness of Nature has resonance with other knowledge practitioners. For example, Gladwin (1964: 174) compared the strategies used by European navigators and Trukese sailors when travelling from one island to another over miles of ocean. The Europeans planned before they set off for their destination whereas the Trukese were involved in continuous decision-making, accommodating the winds, tides, ocean currents, and other shifting factors. In describing Andean farming, Jan Douwe Van der Ploeg (1993) talks about how the farmers choose not to make plans too far in advance as there are too many uncontrollable conditions. As Van der Ploeg describes it:

The [farming] labour process is essentially a craft. In the first place it entails a permanent interaction between 'mental' and 'manual' labour, and in the second it presupposes a continuous interpretation and evaluation of the ongoing process of production so as to enable interventions (which, for evident reasons are hard to predict exactly) the magnitude of the harvest and the quality of the final products are to a great extent determined. Thus the labour process does not lend itself easily to any standardization or exact planning. Diversity both permeates and is created by the process itself. Thus, the decisions taken during the labour process indeed determine the results and, when evaluated in connection with the results, this decision-making also leads to the generation of new or more detailed knowledge.

(P. 209-210)

In fact, Van der Ploeg's description is not unlike that of the obstetrician's in regards to childbirth, in which the medical practitioner is always ready to make interventions that will ensure the birthing process stays on a "normal" track (Marlor, in preparation). The knowledge thus gained, at least in part, is in relation to how one creates a desired outcome as opposed to understanding "how things are"—the latter of which is a much more static approach.

farms and clams, the provincial government was not willing to accept the Kwakwaka'wakw diggers' claims that the fish farms were negatively impacting the clam beaches. In the present Kwakwaka'wakw diggers' epistemic culture, what any one individual knows remains just that—what one individual knows. Even in the case where a digger tells someone else what he knows, there is still the issue of whether or not the other person believes (or can believe) what he says. There is the possibility that a digger can show someone else evidence of his or her assertion (e.g. by taking them to a beach), but this is only possible when entering into a one-on-one relationship with the digger; in situations where relationships become more distant the digger finds himself in a situation similar to that in which the contaminant ecologists find themselves—having to use standardized, conventional methods that justify *how* someone knows what they know. The ecologists embed themselves in a system of relations which not only helped them achieve their goal of selling their knowledge to others but also of participating as full members in a global discussion about the environment. Instead, the Kwakwaka'wakw embed themselves in a system of relations which a Modern, mass-scale society is unsure how to deal with.⁸¹

As can be seen from the discussion above, standardization is, at its foundation, an issue about social relationships with others. Specifically, it is about the degree to which individuals attempt to do things in a similar manner and/or use the same tools and materials as others. In saying something is standardized one must pay attention to exactly what is “the same” and to what degree it is comparable, though. The extent to which something is standardized across time, place and person can vary—it can be the routine behaviour of a single individual, consistent across a pair of individuals, the shared

⁸¹ For a further discussion on this topic see chapter seven.

practices of a small group or people, or duplicated by a community of individuals; it can be the pattern of behaviour in one setting (e.g., in one lab) or multiple settings (e.g., all contaminant ecology labs). It can differ in terms of the extent to which an entire process is replicated.⁸² This is important in the context of comparing the contaminant ecologists to the Kwakwaka'wakw digger in that the contaminant ecologists are far more standardized than the Kwakwaka'wakw diggers. For the contaminant ecologists, standardization is both essential and occurs at a multitude of levels. They are far more attuned to coordinating their ways of knowing with their peers than are the Kwakwaka'wakw diggers. Specifically, the contaminant ecologists standardized to aid their communication and coordinate the transfer of their knowledge amongst themselves and with others. The division of labour coupled with the standardization of their rhetoric stands to increase the level of shared or inter-subjective meaning held among the team and their fellow peers. But this does not mean the Kwakwaka'wakw diggers never standardized; they did follow patterned behaviors and routines. For the most part, though, the Kwakwaka'wakw diggers had minimal concerns about creating inter-subjective meaning. For them, standardization was important only to the extent required for coordination of their actions and the informal discussion in which they engaged; coordination of social relations as a means to facilitate their understanding of clams was not deemed necessary.

⁸² Timmermans and Berg (1997) make a detailed study of this point in their examination of CPR, a technique used to resuscitate individuals whose hearts have stopped. They found that CPR is a general framework into which variations are added. The medical practitioners Timmerman and Berg observed performing CPR followed the standard protocol and its order, but also individualized it by making minor adjustments and additions to the procedures. Yet they also completed the protocol.

Conclusion

Above I have described and compared two “real types,” the contaminant ecologists and the Kwakiult clam diggers with whom I worked, and described how the divergence of their interests in regard to whether they make their knowledge public or private was associated with the social relations particular to their knowledge community. Whether their knowledge was for public or private use has implications for how these knowledge practitioners relate to one another. The practitioners strongly believed knowledge should be based on personal experience and did not rely on others’ observation reports. For these knowledge practitioners, working alongside others may be an option but working with fellow practitioners to produce collective knowledge is not. In contrast, the practitioners who wanted to create knowledge for others used methods and techniques that others would (hopefully) consider acceptable. This also meant being trained in a highly authoritative educational system with credentials to establish that they were capable of producing knowledge, the use of standardized procedures, and frequent, on-going rhetorical contact with their peers.

In relating the above I have also discussed some of the implications this had in terms of their ways of knowing. Specifically, the divergence between them resulted in two distinct ways of knowing. The Kwakwaka’wakw clam diggers’ general orientation towards knowledge with the marine environment and organisms was in terms of knowing how to interact with (or intervene in) the environment so as to get a desired outcome; a digger had no real intention to share his knowledge. The diggers strongly believed that to know something one must experience it directly. As one digger frequently said, “Experience is the best teacher.” Moreover, many of the Kwakwaka’wakw diggers’

treated their knowledge as a secret to be withheld from others. The combined result is that these diggers kept their knowledge about clams private. The contaminant ecologist, in contrast, has no personal use for clam knowledge beyond how it served him professionally. For the contaminant ecologist, the knowledge they produced about clams was intended to be public knowledge, disseminated through the media, journal articles and conference presentations to fellow scientists and an interested public.

These extreme types are not meant to describe all knowledge communities. The DFO biologists and Nuu-chah-nulth clam diggers differ in some important ways. For example, the DFO biologists' main goal is to produce knowledge that can be used as the basis of public management policy. In fact, the knowledge they generate is not particularly relevant to the public except to the extent that it justifies the government agent's policy. Their interest in public policy affects how they organize social relations with others and, as a consequence, how they come to know about clams. Similarly, the are more interested in having knowledge which allows them to successfully find clams rather than producing knowledge for the benefit of others. But they are not as extreme as the Kwakwaka'wakw diggers in their secretiveness and distrust of others in relation to their knowledge about clams. The Nu-chah-nulth diggers appear to talk somewhat openly with one another about clams. This is perhaps due to structural reasons; the Kwakiult use over thirty beaches for commercial and home use whereas most Nuu-chah-nulth digging (home and commercial) occurs on six beaches. Consequently, there are fewer secrets about which beaches they go to. That said, there may be secrets about where to dig on these beaches.⁸³ The DFO biologists and Nuu-chah-nulth therefore do

⁸³ If there are, these secrets were carefully kept because they were not mentioned to me.

not fall onto the same end points of the public-private knowledge continuum as do the contaminant ecologists and Kwakwaka'wakw diggers.

I also do not mean to suggest that all scientists need share the majority of traits I have associated with the contaminant ecologists or that all aboriginal/ alternative knowledge practitioners share the majority of traits exhibited by the Kwakwaka'wakw diggers. Combinations of traits from both real types can be seen in scientists and alternative knowledge practitioners alike. For example, some alternative knowledge practitioners such as fishers employ a division of labour. In the work John B. Gatewood (1985) did on a seine boat, members of his crew were assigned different positions with a corresponding set of tasks for the season. More experienced fishers worked in positions that required more expertise including knowledge of what other positions entailed. Novices, placed in easier positions, were allowed the liberty to slowly piece together what the others were doing after they had mastered their own set of tasks. The crew simultaneously worked together—almost as a super-organism with the skipper orchestrating the proceedings. The division of their labour necessitated more finely-tuned coordination than that found among the clam harvesters, thus requiring a higher level of trust. That said, their form of divided labour was distinct from that of the contaminant ecologists in the sense that they were within close contact of one another, able to physically detect what the other was doing, and the tasks to which they were assigned were no where near as specialized as those of the contaminant ecology team members⁸⁴; the same sense of independence, coordination-at-a-distance and extreme

⁸⁴ Ultimately, like the Kwakwaka'wakw digger, the knowledge of these fishers appears to have been largely private. This can be seen in Gatewood's description of the process by which he learned how to seine fish:

cognitive divisions of labour did not exist for the fishers as it did for the contaminant ecologists.

Similarly, the Cha-Cha of the Virgin Islands in the Caribbean appear to have public discussions about fish. As reported in Ruddle (1994) in regards to work by Morrill (1967), “conversational monomaniacs fixated on fish, for whom fish behaviour was a subject of supreme interest, ‘taking the form of descriptions of observed behaviour, proffered explanations for the behaviour, and debate over the soundness of the observations and the conclusions reached from them,’ and particularly of fish feeding behaviour (which extends even to species that they do not target), and to a lesser extent reproduction, territoriality, and the ‘personality’ of species and individuals.’ ” (*ibid*: 177). In other words, these traditional fishers, like the contaminant ecologists, publicly vetted their observations, methods and conclusions amongst peers in their process of developing fish knowledge.

Finally, according to Knorr Cetina (1999: 98), molecular biologists use standardized protocols like the contaminant ecologists but also place great emphasis on the embodied knowledge they develop in their work. Given that the bodily tasks they perform overlap with the knowledge claims they produce, they can use their bodies as “truth-detectors” for assessing the work of others. As she explains it: “they can use their bodies as a truth-detector in the sense that one can feel with one’s body what to do or whether something is correct” (*ibid*: 99). She describes their bodies as having a

As I reflected upon my work, struggling to improve my mastery, I constructed a personal representation of my routine. This personal mode of portraying seining to myself became linguistically formulated (primarily in inner speech) and even incorporated some of the collective representations. But it remained a *personal* construct, not equivalent to the collective mode of representing the work when conversing with others. Because the cognitive organization of work may differ from its collective representations, inter-individual differences are free to arise. (1985: 215)

“witnessing presence” and that, as a result, “one could ‘break one’s neck’ by announcing a result when the relevant work was not performed or witnessed in person.” As a consequence, trust between co-workers or between researcher and assistant in terms of performing the work properly, making accurate observations, or supported inferences is not necessary as one *can* and *should* witness it for oneself. This places the molecular biologists at both ends of the continuum simultaneously in that they create both public and private knowledge.

All the above-reported cases diverge from the types I describe here. Yet, I would like to suggest that, in all these cases, examining the association between the practitioners’ interests and their web of social relations would provide valuable insights into their respective ways of knowing.⁸⁵

⁸⁵ In stating this, I do not mean to imply that interests dictate exactly how social relations will be organized. I refer to Karin Knorr-Cetina’s (1983: 134) argument on this point:

Strictly speaking, at least two sources of unpredictability can be differentiated. First, social action appears to be *underdetermined* by antecedent constraints (such as goals, rules or ‘structural’ conditions). I take this to imply that antecedent constraints become practically meaningful only when interested in context, and hence their specific enactment (including their neglect, perversion, or substitution) changes with time- and space-bound conditions. Second, in the given situation social action may be *overdetermined* in that different, interpreted constraints (such as personal goals and technical preferences) exist and are at variance with one another, in which case the outcome of the situation seems to depend on the (unpredictable) dynamics of the clash. Both features account for the negotiated aspects of social life, though conflict is endemic only to the latter condition. Both features describe the essential indeterminacy of social practice in the sense that practice can no longer be conceived as the mere execution of a predetermined order of things, but rather this order is itself a function of local closure in practical action.

Chapter 5: Selecting Signs

Selections can be called into question precisely because they *are* selections; that is, precisely because they involve the possibility of alternative selections.

(Knorr Cetina 1981: 12, italics in original)

All measurement is limited. When measuring length, one temporarily flattens the world into two-dimensions. When declaring someone's weight, we ignore that there are daily, if not hourly, fluctuations. When determining the size of a tree grove by counting the number of trees, one not only has to make decisions about whether to include saplings and new growth around the edges of the grove but also, more fundamentally, whether to use the number of trees in a grove to represent grove size. There are no perfect measures or means by which to determine what is empirically "out there"—if there were, researchers and philosophers of science would have finished arguing long ago.

The basics of observation and measurement are such that to observe or measure something one must first select a feature, characteristic, organism, event or phenomenon and identify it as meaningful or relevant to whatever idea or concept one is attempting to measure. As with the example above for measuring the size of a tree grove, one selects empirical features to represent the concept "size of tree grove." One not only selects what one wants to observe or measure, though, but one also makes selective choices when deciding *how* to do so. For example, in measuring the length of a snake with a tape measure, one needs to select particular points on the snake (presumably the tip of the "nose" on the head and the tip of the tail) between which to measure and therefore to represent "length" of the snake. All aspects of observation and measurement involve some aspect of choice or selection.

Following this, as Knorr Cetina suggests in the introductory quotation, observations and measures can be called into question precisely because they are selections—choices have been made and different alternatives could have been chosen; as measurement involves selection, measurement techniques can be questioned. The purpose of this chapter is to explore some of the ways in which the clam knowledge practitioners selected and assigned significance to aspects of their environments. Specifically, I compare three of my four case studies—the two sets of First Nations and the DFO biologists—in terms of how they went about assessing the abundance of clams on a beach. In all three cases the practitioners employed signs or attached significance to empirical objects. Even though they were assessing what was essentially the same concept, though, the specific objects they incorporated into their signs differed. In other words, each set of knowledge practitioners had their own complex of signs—their own ways of making observation and measurements—which allowed them to assess clam abundance. This chapter identifies and compares their respective methods of observation and measurement—their signs—as comparable yet distinct ways of knowing.

Clam Surveying

When I first started my fieldwork I heard a story⁸⁶ about a biologist who conducted a clam survey on a First Nations' clam beach. The goal of any clam survey is to estimate the abundance of clams on a given beach. This information is often used to

⁸⁶ I was told this story by the biologist. I state the story somewhat tentatively, though, as two years after her telling me the story, when I asked her for further details about the incident, she had little to no recall of the incident in the story at all. I chose to use the story despite this because it frames the contrast between two ways of assessing clam abundance on a beach well. Moreover, I independently heard all information contained in the story regarding signs used by the knowledge practitioners for assessing clam abundance from at least two practitioners with whom I worked, so I am confident that these signs are genuinely employed by at least a selection of these diggers.

establish harvest rates or management plans. The biologist in the story used a similar survey method to that of the DFO biologists with whom I worked in which a perimeter is marked around the edge of the clam-producing area on a beach (see Figure 5.1).

Perimeters can be circular, but typically straight lines are used because it is easier to calculate the area within the perimeter if the edges are straight. The biologist then randomly selects a pre-defined number⁸⁷ of quadrats (squares of a specific size) to be dug. The diggers, usually volunteers, dig up and put aside any and all clams they find within the quadrat, placing them first in a bucket and then a sack with a label affixed to it indicating the coordinates of the quadrat on the beach. When all quadrats have been dug, the sacks of clams are taken to a laboratory (or, in the case of many surveys, temporary pseudo-labs) for analysis. In the lab, the number of clams in each quadrat is counted and the average number of clams in all quadrats is calculated. The average is then multiplied by the area of beach included within the perimeters to estimate the number of clams on the beach. Clam density is calculated by dividing the total estimated abundance of clams on a given beach by the area (in square meters) of the beach. These two figures are the principal ones used as estimates of clam abundance on a particular beach.

⁸⁷ The specific number of quadrats is based on the area within the perimeter.

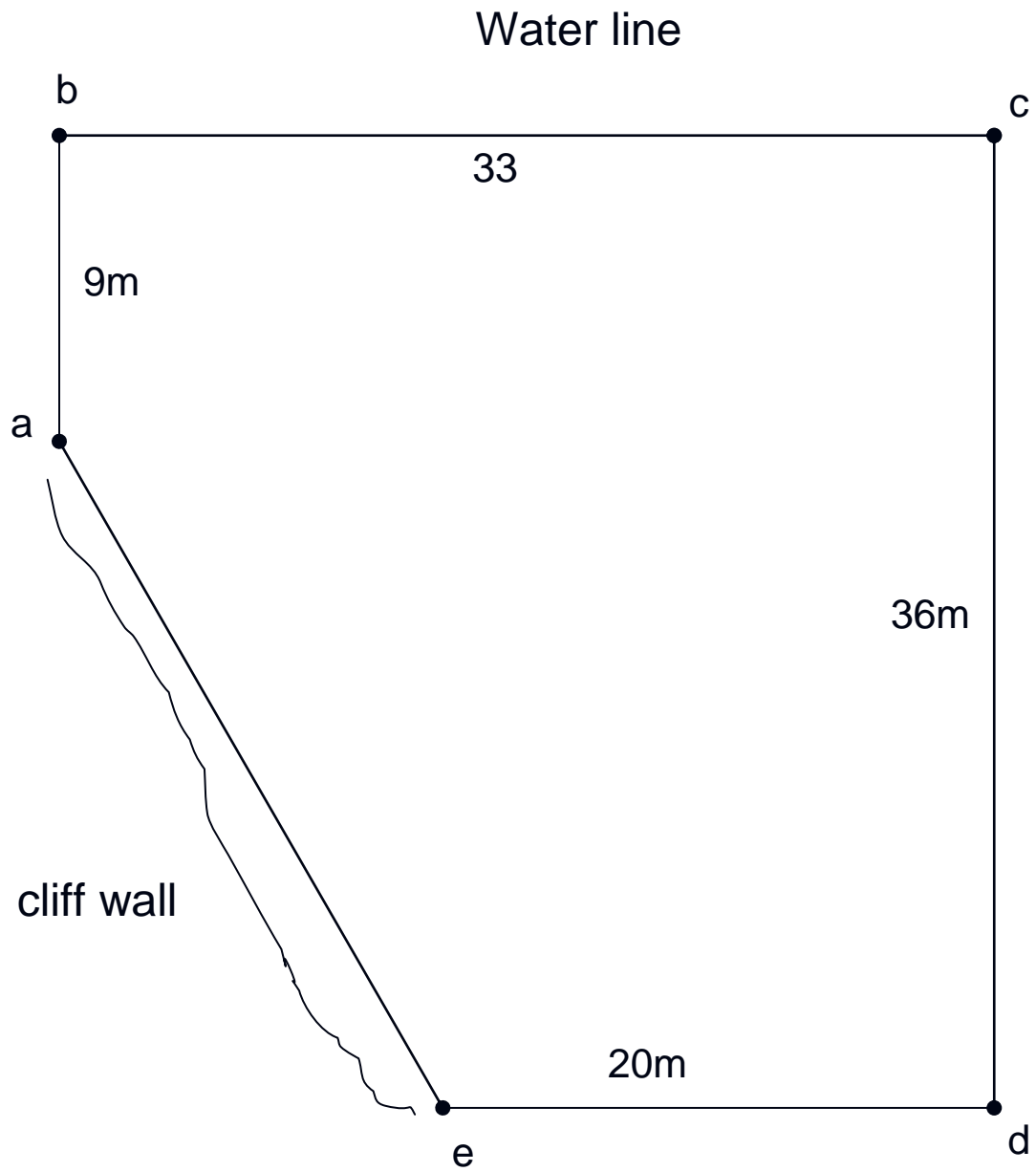


Figure 5.1: Mapped Perimeter of a Beach for Surveying

Please note: This map was drawn for the purposes of this dissertation. It is not a map used by the DFO biologists, who preferred to keep their maps unpublished.

Accompanying the biologist on her survey were several experienced Nuu chah nulth clam diggers. These diggers objected to the clam survey methods. To begin with, they said the best way to initially estimate the number of clams on a beach is to smell the beach. Clams smell and the more clams present the stronger the smell. This suggestion surprised the biologist as she had never heard of smelling clams before, much less using their smell as a means to estimate the number of clams on a beach.

Peirce's Sign: A Neutral Language

What I like about the biologist's story is that it shows a clear difference between the ways in which two groups of experts can assess the same phenomenon. But before I can analyze this story I need to introduce some language and briefly provide the theoretical approach I take in the following discussion. I have chosen to use terms borrowed from Charles Sanders Peirce's semiotics (1992; Atkin 2006): *sign*, *object*, *interpretant* and *representamen*. Peirce conceived of all thought as being in the form of signs. We form representations of phenomena which our minds can then manipulate and to which we attach labels (words) so as to communicate. These representations are the foundations of signs.

The aspects of Peirce's discussion about signs to which I pay the most attention are those he and others developed the least. Peirce described three types of sign: the index, the icon and the symbol. Peirce's icon and symbol, also relevant to discussions about knowledge construction, have been explored in considerable detail by linguists, cultural analysts and sociologists of science. Index signs, however, have largely been

marginalized in discussions about science but are of central importance as they form the basis of measures.

An index sign is a sign in which two empirical or “natural” phenomena become associated, such as smoke with fire, meowing with cats, a whistling sound with wind, and mold with dampness. To be clear, the association is empirical; it is not something created in people’s minds. What does need to be created—or noticed—by the human mind is that such an association exists. If one does not make a mental connection between smoke and fire, meowing and cats, or mold and dampness then it does not stop the association from existing but it does prevent the index sign from existing—at least within the mind(s) of that particular individual or thought community.

According to Peirce, all signs are constituted by a tripartite set of associations or relations among: (i) an object, (ii) the representamen, and (iii) the interpretant. An *object* is an empirical phenomenon—something empirically “out there.” In this parlance, an object need not be a tangible solid such as a house, mouse or tree but can be a fluid, continuous phenomenon such as water or air. A *representamen* is the perception, register or measurement we use to detect the presence of the empirical “thing,” the object. This can be anything from a flavour, odour, sound, observed behaviour, way in which the object interacts with a tool, reaction, or a visual cue.⁸⁸ The *interpretant* is the mental significance or representation of the object in relation to the representamen; it is the idea or concept we hold in our head about something we think exists “out there” in some empirical form and our understanding of how the representamen is associated with it.

⁸⁸ This is a modified version of how Pierce described a representamen.

Two examples will hopefully make the components of the index sign clear.

Imagine in front of yourself is some empirical “thing”. By what you can visually detect, smell and feel, you have decided this thing is a “tree.” “Tree” is your interpretant—the meaning or significance you attach to the empirical “thing,” an object. The visual, tactile and olfactory cues you use to identify this object are representamens.

A second example involves a measurement tool, the thermometer. To tell what temperature it is we often use a thermometer. The thermometer contains mercury placed in a narrow glass tube. Mercury expands when heated, causing the mercury to rise in the column. Markings beside the column indicate the temperature in terms of a number, a social convention we refer to as degrees (either Celsius or Fahrenheit). We detect the state of our interpretant, temperature, by observing the state of mercury in a tube, the representamen.

One important point to note is that more than one representamen can be associated with the presence, quality or abundance of a given object and interpretant. The tree example points to this but I will use an example adapted from Liszka (1996) to illustrate it more clearly. The interpretant “level of gasoline in a tank” can be indicated by a number of means: (i) by reading the gauge on the side of the tank in which a needle fluctuates in the gauge due to pressure exerted by the gasoline on the gauge; (ii) knocking on the side of the tank and listening to how the tank resonates; (iii) opening the lid on the top of the tank and, using a flashlight, looking inside to see how high the gasoline sits in the tank; and (iv) opening the lid on the top of the tank and drop a rope into the tank until it hits the surface of the gasoline, then removing the rope to measure the length dropped

into the tank. All four methods are representamens that can be used for detecting the level of gasoline in the tank.

In the process of measurement, not all possible representamens are used, though. In the case of the gasoline tank, we would likely be content to just check the gauge and not bother with the other methods. Or we could use a few representamen, a method referred to in science as triangulation, such as checking the gauge and then knocking the side of the tank to double check the gauge reading. Using all representamen, however, is too time consuming, involves too many materials, and requires considerably more effort than likely deemed necessary.

Moreover, not all representamens are obvious to knowledge practitioners. For example, there are undoubtedly more representamens than the four listed for detecting the level of gasoline. Using a particular representamen then requires, at minimum, noticing an association exists between the representamen and the object. For example, one needs to make a mental connection between the sound emitted from knocking on the side of a tank with the volume of fluid in the tank to use the sound as a representamen.

Choosing to use a particular representamen may also involve having a working theory as to how the representamen and object are related.⁸⁹ For example, having an explanation for why the sound emitted from knocking differs when the volume of liquid in the container differs.

Finally, in selecting a representamen, one picks what is “do-able.” It is fine and dandy if one considers the best method of assessing clam abundance to excavate an entire

⁸⁹ Many measures assess the presence of phenomena associated with the object of interest, not the object itself. For example, assessing the recentness of fire in an area by looking for certain types of plants that are known to colonize an area shortly after a fire has occurred. In these cases, there is an additional layer of theorizing or theoretical associations drawn.

beach using large-scale machinery and filtering the sand from the clams, but this is not only an ecologically unsound method but expensive, time consuming and difficult to do. Much more “do-able” are the clam surveys currently employed by the DFO biologists.

One consequence of multiple possible representamens is that knowledge practitioners can select different representamens than those selected by other practitioners. In fact, given that the association between interpretant and representamen is as much mental as empirical, a knowledge practitioner may consider the unfamiliar representamen employed by another practitioner as inappropriate, problematic, wrong or just downright strange. Also possible is that a knowledge practitioner mentally connects a representamen to the interpretant but does not consider the representamen consistent or accurate enough to select as the representamen she uses.

Different Types of Representamen

Using Pierce’s terminology, clam abundance is the interpretant of interest to both the Nuuchah-nulth clam diggers’ and the fisheries biologists. The representamens they use differ, however. For the Nuuchah-nulth diggers, the initial representamen they used was intensity of clam smell whereas for the biologists it was the abundance of clams found within the various quadrats.

Perhaps unsurprising to the reader, the biologist in the story did not initially consider the smell of clams a useful representamen for assessing clam abundance. In fact, the Nuuchah-nulth representamen is domain-specific in that it is specifically about clams; using this representamen requires knowledge about and experience and skill with clams. For example, one needs to know which additional factors to take into

considerations when smelling clams (e.g. rain, wind) and how these affect the intensity of the smell. It also requires a good sense of smell to be perceived effectively, and some people are better at smelling clams than others.⁹⁰ Smelling clams is an assessment based on the odour of clams—a specific clam quality.

The biologist's preference was for quantitative measures that reduced the amount of idiosyncratic judgement required by the assessor.⁹¹ The techniques she had been taught for conducting clam surveys involved the use of standardized measures that employed homogeneous units allowing for any trained practitioner to presumably make a similar assessment as another trained practitioner. Smelling clam abundance is clearly not a standardizable procedure in that it relies on personal judgement; agreement may be made among two experts as to whether they consider a given smell to signify the same amount of clams but they do not (nor would it be easy to figure out how to) use standardized units or explicit criteria by which to assess the strength of the smell. The process of assessment is a black-boxed event that is not easily unpacked by others. In contrast, the biologist and her colleagues at DFO considered the procedure of randomly selecting locations on the beach, digging up all clams within those locations, counting them and converting these figures into a measure of clam density and abundance reliable

⁹⁰ Nuu-chah-nulth clam diggers are not the only ones to employ domain-specific representaments. For example, in *Measures and Men*, Kula (1986) tells us that farmers from pre-industrial Russia to Spain measured the size of land based on the amount of land they could personally care for in a day. This type of measure is clearly related to specific knowledge, skills and abilities of the assessor as related to the quality of land in question, familiarity with the type of care required (e.g. crops versus animal husbandry), and knowledge of approximately how fast one works under those conditions. Similarly, European sowers of seed measured land in terms of the fertility and topography of the land. Sowers threw down more seed and took smaller steps on more fertile than unfertile soils or soils interrupted by topography. In terms of the sale of grain, grain was assessed by the quality rather than the quantity of grain, which required knowledge about grain quality. In all three examples, assessment was based on a representamen that was closely tied to unique aspects of the object and domain-specific knowledge/ experience possessed by the assessor.

⁹¹ To the credit of the biologist in our story, she did modify her survey protocol so as to make it more acceptable to the First Nations diggers with whom she worked while still remaining acceptable to the scientific community.

because the clam survey used a general protocol which can be used on any beach by any practitioner with the proper training. Even the procedure for randomly selecting quadrats, which is somewhat complicated, involved procedures that can be clearly communicated through diagrams and verbal step-by-step description. Other practitioners can thus repeat the procedure in the same manner and will presumably arrive at the same assessment.

Of course, as Wittgenstein pointed out (1958), all rules are subject to interpretation; the way in which a protocol is implemented can differ from one practitioner to the next. One way to increase the likelihood that practitioners employ a protocol consistently is to base the procedure on skills and practices in common usage. This approach can be seen in the clam survey. For example, random selection procedures are used throughout the natural and social sciences and quadrats are used by many ecologists. The clam survey procedure was designed so that practitioners only needed minimal knowledge about clams, the object being sampled, but considerable knowledge about counting, math and the logic of random selection. Counting is a skill that most individuals learn at a young age and use in a wide variety of contexts. These practitioners need to have perfected the ability to count and to take steps to prevent inaccurate counts (e.g. counting the clams from each sack twice or ordering the clams from a single sack on a table in a grid-like fashion so that every clam is clearly accounted for). In other words, the skills and knowledge required in the use of the biologists' representamen were borrowed from a general pool of procedures and common teachings rather than domain-specific knowledge about clams.

In addition, the representamens a knowledge practitioner uses in a clam survey are standardized, homogeneous units such as the quadrat, kilogram and meter. While quadrat sizes vary from one biological study to another, the dimensions of the quadrats for DFO-based clam studies are identical within a study and across studies of the same species. The result is that all DFO-related clam surveys are based on the same basic homogeneous unit. The kilogram and meter are even more standardized in the sense that these units are used widely by many people in- and outside science. The result is that the knowledge required about the specific substance being assessed (i.e. clams) is minimized whereas the majority of knowledge is based in abstract, quantifiable qualities that are shared by a broad spectrum of phenomena (e.g. weight, length) that have been organized as highly standardized units.

As Porter (1992, 1995) and Daston (1992) have argued in regards to other areas of science, the use of standardized homogeneous units increased “objectivity” and decreased the need to rely on personal judgement during the clam survey.⁹² In doing so, it also

⁹² As argued in chapter four, interests have a large effect on how knowledge practitioners approach their work. The clam knowledge practitioners’ observations and measurement were no exception; the methods described for assessing clam abundance reflected differences in the various knowledge practitioners’ interests. The First Nations commercial diggers were interested in harvesting clams. Using expert opinion about where to find clams makes more sense than digging haphazardly at any spot on a beach to see how many clams are present. The biologists, on the other hand, were first and foremost interested in making their knowledge public, thus making random selection preferable because, by using random selection, the biologists do not have to convince others that they knew enough about clam ecology to know where to locate clams. Instead all they needed to do was convince others that they had followed acceptable procedures.

A further indication of how interests affected which representamens were used is captured by a comment in a DFO report in which it is stated that observing the type of substrate, the presence or absence of siphon holes, clam squirting and test digging—some of the methods employed by the First Nations for “reading” the beach—can be used as a rough indication of clam density, but that these methods are not precise or transparent enough for DFO management decisions.

Perhaps an interesting note is how interests affected the timing of these practitioners’ observations and measurements. The First Nations had an immediate use for knowing whether there were a lot of clams on a given beach or not; when they first arrived at a beach, they needed to know if they should stay on that beach or move to another beach to dig. Similarly, after they have dug they needed to know whether and when it is worth returning to that beach to dig. The biologists, on the other hand, had no immediate time constraints. Their main constraint was convincing others that their assessment was correct. The biologists’

reduced the need to use domain-specific knowledge, such as how strong clams smell when buried under sand and how the strength of clam smell is influenced by the rain.

The representamens employed by the Nuu-chah-nulth and biologists thus represent two distinct approaches to assessment, one based on domain-specific knowledge and practices which may be idiosyncratic to the knowledge practitioner while the other is grounded in common knowledge and shared practices.⁹³

The Nuu chah nulth diggers had parallel concerns about the biologist's representamen for clam abundance. These diggers, like many of those with whom I worked, asked "why randomly select?"⁹⁴ Random selection is based on the premise that

lack of short-term time constraint provided them freedom in terms of when and how quickly they needed to assess clam abundance. The fact that it took days to count all the clams from the various quadrats or that they conducted their surveys in the summer when days are longer but the clams are unsafe to eat was not a problem. The representamens each set of practitioners chose was clearly shaped by their interests.

⁹³ Those of us trained to use standardized, homogeneous units may reflexively think that such measures are a superior form of representamen when this is not necessarily so. I return here to Kula (1986), who makes an excellent point about the metric system. His example is the hectare, a measure of land. All hectares are equal in terms of geometric area. This, however, is the only dimension in which they are equivalent. Anyone offered a choice between a hectare of ocean-front property or a hectare in the middle of a desert will quickly point out that the same sized parcel of land can be vastly different. This is true even in regards to how one counts. As Kula states:

Technically then, as well as economically, adding up hectares whose value and profitability vary does not mean adding like to like, and there is nothing here that makes valid the assumption that the owner of ten hectares of land is twice as rich as the owner of five hectares. We must, therefore, conclude that the old measures based on labour-time and the amounts of seed are definitely more commensurable and 'addable.'

(1986: 35).

In other words, what is standardized in units such as the meter is a quality that can be assessed in a consistent manner by a large number of people. What is not held constant are a multitude of other qualities that may be more relevant to know but can not easily be standardized as they require the use of judgment, such as how fast an individual can work combined with the soil condition of a particular tract of land.

⁹⁴ A similar objection has been voiced by fishers towards the accuracy of scientific selection methods, but this time not in terms of random selection. Palsson (2000) described a scientific survey conducted at the same time, with the same fishing vessels, the same fishing gear, and in the same locations every year. For the scientists, their consistency allowed them to collect data that could be compared from one year to the next. The survey was conducted with the assistance of local fishers, who had a different view of the scientists' consistency. They considered the methods too inflexible to assess fluctuations and what was going on as a whole in the waters of that region. In their view, using different sized vessels, types of fishing gear, survey routes and timing of surveys would result in a less biased assessment. In this case, the fishers are making a similar objection to the First Nations clam diggers in the sense that their local

chance alone dictates where to dig for clams. Yet, as both the biologists and the First Nations were aware, clams are not distributed evenly on a beach. Random selection will lead one to dig in areas where there are no clams. But whether clams are present can, to a large extent, be accurately predicted before one digs—at least by some people. Expert First Nation's clam diggers know how to “read” the beaches in terms of micro-habitats in which clams prefer to live. The First Nation's diggers therefore ask: “Why dig for clams in an area where you know that there will be no clams? Why not dig in areas where there are clams? That way you get a better idea of exactly how many clams are on the beach.”

In fact, this is what Kwakwaka'wakw clam diggers do on a regular basis when harvesting clams from a beach. In Kwakiult territory, typically 2 to 8 diggers go to dig on a beach in a night, with smaller groups tending to dig on smaller beaches and larger groups on larger beaches. They dig from the time when the tide is low enough to expose clam substrate until the tide covers up this area again. Most diggers dig at several locations on a beach in a single night. When they walk around the beach they ask the other diggers how many sacks they have dug (or look at their number of sacks to find out) and ask how the digging is at that spot. At the end of the night the diggers, who are somewhat competitive and somewhat suspicious of other diggers, count the number of sacks they themselves harvested and the number harvested by others. This gives them a total number of sacks harvested from the beach that night and an estimate of the abundance of clams on the beach. They considered this a superior representamen for assessing clam abundance than one that employed random selection.

knowledge about where and how to assess what is going on in the ocean is rejected for a representamen that reduces the need for local expertise and judgement.

The Importance of Precision

While the DFO biologists agreed that the Kwakwaka'wakw clam diggers gain a good sense of how many clams are on a beach by harvesting the beach, it is not a method they themselves could ever use. The DFO biologists were extremely concerned with issues of accuracy or replication. By this I mean being able to calculate a precise quantitative amount that would be comparable to an amount others could come to had they used the same representamen on the same beach. Specifically, the DFO biologists came up with precise numbers—say a clam density of 12.4 per meter or 34,769 clams on a beach—from each of their clam surveys. In calculating these numbers, the biologists were careful to double-check their figures. To do this they reviewed the figures they were provided by the depuration facilities. Figures that did not correspond to expectations were identified as suspect, were inquired into and, where possible, corrected. For example, measurements reported in pounds were converted to kilograms, figures that were larger than expected were re-calculated using raw data recorded in earlier steps, and conversion rates were standardized.⁹⁵ The biologists then bracketed their figures in a possible range of error or confidence interval that mathematically suggests the likelihood of their figure being correct.⁹⁶ Finally, in their written reports they described any measurement problems they and their partners encountered in data collection, recording and analysis. For example, the DFO devoted several pages to describing potential and realized problems such as how one group of surveyors reported the bulk of clams as little necks when no little necks had previously been reported on that

⁹⁵ The same was true for the contaminant ecologists who routinely repeated their chemical analysis on a select number of samples. They safe-guarded their figures by bracketing them by the likelihood of occurrence (*p*-value).

⁹⁶ A confidence interval is a standardized distance away from the mean. Specifically, a 95% confidence interval is 2 standard deviations away from the mean in both directions (above and below the mean).

beach, with the result that the biologists decided to treat these figures as manila clams instead. It was due to procedures such as this that the biologists felt they could claim their representamens were more accurate representamen.

Precision has been an appealing concept to many in science. As Heberden, an early modern physician argued, the pulse was a superior method of assessing the patient to more qualitative approaches such as the Chinese *mo*⁹⁷ because the pulse remained identical regardless of who counted it or which artery on the body one used to count (Kuriyama 1999). Precise figures allow for (supposedly) unambiguous comparisons—something that can not be achieved when concepts and methods are left unstandardized and qualitative. For example, one can clearly see if the density of clams on a beach increased or decreased from one year to the next by comparing the numbers calculated for those years or see if clams from beach *x* live in a higher density than beach *y* by comparing the calculated densities for each beach. Using standardized, quantified representamens allowed the biologists to compare their figures to other figures they themselves had generated and to those generated by other biologists employing similar methods (Porter 1992).

The biologists' emphasis on accuracy was only applied to a limited realm of the phenomenon they studied, however. For example, while the DFO were careful to randomly select quadrats on the beach, their choice of beach sites was based on economic and political reasons (see chapter 2).⁹⁸ Moreover, precision did not mean comprehensiveness or an accounting of all things that vary. For example, precision was

⁹⁷ The Chinese version of the pulse in which the quality as well as the frequency of the pulse is assessed.

⁹⁸ Similarly, the contaminant ecologists argued that their selection of beaches was a close proximate to random selection but in fact it was a combination of purposive and convenience sampling based on quickly-sought advice from local biologists and First Nations.

not seen to relate to how many characteristics were included for observation, the breadth of sites, or the historical trajectory of cases. Although the DFO biologists emphasized the importance of longitudinal studies, their research findings were based on data collected over a relatively short time span: three years of harvesting sandwiched by four annual surveys.⁹⁹ Even the preferred length of study by the DFO, 10 years, is short relative to the history of the beaches and the time over which the First Nations diggers worked on their beaches. Combine this with the small number of beaches they studied and the small range of representamens they employed (as opposed to the number of cases to which the representamens are applied) one can see that accuracy did not mean comprehensiveness.

To compensate for lacking more comprehensive information about a site, the biologists might ignore or assume constant some of the details they considered potentially relevant. For example, the DFO biologists said that clam population predictions are more complicated than their simple model of harvest rates and survey figures encompass.¹⁰⁰ Two of their chief concerns are mortality, especially the death of clams due to cold winter weather, and recruitment (clam larvae settlement and survival), neither of which they know how to effectively monitor. The biologists had toyed with including mortality and recruitment in their formulas, but doing so by including numerical hypothetical figures or constants. By doing so they included more information in their models but this information was theoretical and not based on any additional observations or measurements. In fact, one can say generally that what the biologists lacked in terms of empirical observations and measures they compensated for using theory (“theoretical stuffing” as I referred to it at one point earlier).

⁹⁹ The contaminant ecologists were similar in that they completed two return visits to each of the beaches within the space of a year.

¹⁰⁰ This is a common—perhaps even defining—feature of biological models. For example, see Giere 1999.

The Kwakwaka'wakw diggers, on the other hand, were not interested in precise measures. Although they frequently used arithmetic, such as when they counted the number of sacks removed from a beach, they did not calculate precise amounts. For example, the clam sacks they counted were not standardized in size or weight (see chapter three). Arithmetic contributed towards assessments of their representamens, but the final form their assessments took was more likely to be an informal classification, such as whether a particular beach is not worth returning to, should be tried again in a couple of months, or returned to next year, instead of a precise number.

Moreover, the Kwakwaka'wakw diggers were more interested in multiple and repeated representamens. The single harvest of a beach was only one of the representamens they used to decide when or whether to return to a beach. Many of the diggers lived in the territory they dug and had dug many of the beaches numerous times over decades. As inhabitants of these territories, they also had other occasions to make observations and measurements that helped them predict the state of clams and beaches: many diggers also fished, crabbed, prawned, and some collected seaweed from the same waters and beaches; those who lived in remote villages passed by several clam beaches when traveling to the grocery store or other villages and they are aware of activities others partook of in their waters, such as the presence and location of fish farms and trawlers. Finally, the diggers conducted an initial assessment when they return to a beach to dig, similar to the way in which the Nuuchahnulth diggers smelled for clams. Their decision to return to and dig a beach was thus based on a variety of representamens assessed over a considerable length of time—up to 60 years in one case.

A quote from a Kwakwaka'wakw digger captures some of the diversity involved in their making decisions whether to dig on a beach:

BB: The clams.....could be affected by something. The thing you watch out for is...you check the clam: if they are big shells but only little ones inside it we don't touch the beach. Because they're not eating, they're not feeding, so, there is something wrong with that beach. Notice one area in [X] where there's a fish farm. There used to be a really good clam beach there, at the pass. Used to be really big clams in there. Now there is only come down to half a size of a big one. Used to be...used to go there all the time with my uncle. His trap line is in that area, and we used to go and trap there. And when it's good tide, we'd dig big clams there. BBQ it. Now you only get the small little clams for every something like 3 empties.

In this quote the digger does not only consider previous harvest outcomes from the beach, but its proximity to fish farms, the size of clams and the density of clams while digging. These are only a few of the factors diggers might take into consideration.

Some Kwakwaka'wakw diggers questioned the wisdom of the DFO biologists in creating precise numbers.¹⁰¹ Precise numbers about, for example, the state of the beach, suggested Nature was somehow fixed or unchanging instead of dynamic and fluctuating. Several of the First Nations diggers I worked with were quick to point out that clams and beaches change over time. This meant that knowing the precise state of anything at any given time was difficult and not necessarily worth extensive effort to accurately calculate as it would change. This did not mean the diggers did not consider it worth their time to note the current state of things, it was just that such a statement did not represent anything more than that and, as a consequence, undergoing intense work to generate a

¹⁰¹ Precise quantitative representaments also arguably flatten and restrict what one comes to know. For example, the ancient Chinese doctors would likely feel at a loss if forced to reduce the flow of blood in a patients' body to a single number. Dimensions such as length, weight, speed, tensile strength, pulse and abundance are employed frequently in science because they are relatively easy to quantify. Qualitative dimensions such as smell, taste, tactile sensations, colour, emotions, inter-personal and inter-species dynamics and how these change over time are far more difficult to place a number on. Restricting oneself to quantitative-oriented representaments prevents one from attaining a more rounded, complete view.

number such as the precise number of clams on a beach (held to be *the* clam abundance until the commercial harvest) was problematic.¹⁰²

Similarities and Differences

To summarize, the three sets of knowledge practitioners were similar in that all selected and employed index signs and, more specifically, empirical representamens, to form impressions and draw conclusions about the world in which they lived. Moreover, all used representamens that they considered consistent and accurate¹⁰³ in terms of assessing the interpretant in question.¹⁰⁴ As was seen by way of comparison between their respective approaches to assessing clam abundance, though, representamens used to assess the same interpretant can differ considerably: different knowledge practitioners can have different ways of knowing. The representamens were not only unique but differed in systematic ways. For example, the First Nations preferred representamens that utilized their specific knowledge about beaches and clams, whereas the biologists

¹⁰² Roepstorff (2000) encountered a similar critique of other biologists by Greenland fishers. The biologists took the approach that catching a fish meant one less fish in the ocean. The fishers, in contrast, talked about fish in terms of their presence and non-presence. “It is well known that, at times, the fish are there, while at other times they may disappear” (*ibid*: 180). Through the lens of Western biologists’, the fishers’ assessment appears vague and uninformative. Transposing the example into one about mosquitoes at a marsh perhaps makes the fishers assessment more sensible, though. Imaging killing one mosquito while standing outdoors and thinking “ah-hah, one less mosquito to bite me!”—a strange thought if one knows anything about the amount of mosquitoes. While fish do not reproduce as quickly as mosquitoes, pinning an exact number on a set of fish for which the exact boundaries and fluctuations in their numbers were unknown seemed strange to the fishers; it is a bit like painting the exact population of a large city on a sign, even though residents are immigrating, emigrating, dying and being born every day. In the case of fish, juveniles hatch, predators (including fishers) consume them, and individuals become unhealthy and die. Likewise, it is still open for debate as to the extent to which fish migrate among “stocks”.

¹⁰³ Their representamens are consistent and accurate but they are always limited by financial and other constraints. For example, the contaminant ecologists would love to use a more sophisticated chemical analysis machine but do not have the budget to purchase such a machine.

¹⁰⁴ In a number of ways the First Nations clam diggers’ representamens are comparable to those employed by qualitative researchers such as ethnographers and naturalists. Moreover, much of their justification for using their methods over the quantitative methods of the biologists is comparable to Charles Ragin discussion comparing quantitative and qualitative methods (2004).

preferred representamens grounded in more general, standardized knowledge such as millimetres and kilograms that reduced the need to use domain-specific knowledge. They also differed in terms of whether they sought precise numbers, the number of representamens they employed and the duration and frequency of their employment.

Beneath these differences lay more fundamental differences in epistemology. In choosing whether to use a general procedure such as random selection or domain-specific knowledge about clams during one's assessment of clam abundance, the knowledge practitioners' choices differed by their interest in producing knowledge. The DFO biologists, who wanted their claims to be accepted by others, chose representamens that were commonly used by others and which did not require them to justify the accuracy of their knowledge about clams. In contrast, the Kwakwaka'wakw diggers had no need to make their methods transparent to others and therefore could rely on their personal knowledge about clams as part of their method for assessing clam abundance on a beach. Likewise, the DFO biologists emphasized the tractability and measurability of their concepts using precise measures whereas the Kwakwaka'wakw diggers considered such precise measures problematic as they were unable to yield any special insight to the behaviour and presence of living organisms. Instead, the Kwakwaka'wakw diggers preferred using multiple representamens that allowed them to conduct on-going monitoring of the beaches.¹⁰⁵

¹⁰⁵ An interesting note to make is that the Kwakwaka'wakw and DFO biologists' post-harvest representamens for clam abundance can not be compared to each other to determine whether the two obtain consistent results. To assess the representamen both representamen have to be used on the same beach. But both representamen involve the removal of clams from the beach; the beach is altered in the process of assessment. Simultaneous or sequential assessments would never produce comparable results for very practical reasons.

Consistent with Knorr Cetina's description of research as a selective process, the knowledge practitioners with whom I worked had clearly selected representamens as a means to know about clams. By comparing the representamens these knowledge practitioners employed for assessing objects of the same class, namely the abundance of clams on a given beach, it becomes clear how these representamens *are* selections. Moreover, each set of representamens employed by a knowledge practitioner can be thought of as one way of knowing about clam abundance and, examined collectively, various ways of knowing.

Chapter 6: Crafting Conclusions

Drawing conclusions, discovering patterns, developing findings, constructing knowledge claims—whatever one prefers to call it—requires making sense out of observed variation. In the construction of knowledge, knowledge practitioners do not just observe a single object at a single instance in time. They make a variety of observations and measurements which undoubtedly exhibit variation (as one of the fisheries biologists put it, he gets worried if there is little or no variation in his data). As Susan Leigh Star says, “scientific work involves the representation of chaos in an orderly fashion...scientists’... descriptions make the complicated turbulence of the world appear, at least in part, orderly, predictable, and bounded” (1983: 205). How, then, do knowledge practitioners move from varying and multiple observations and measurements to knowledge claims—generalizations that extend beyond the immediate time or geography that the knowledge practitioner witnessed? This process is *prescribed* in detail in multiple data analysis and statistics textbooks, but *descriptions* of how this process differs from one thought community to another are in shorter supply. The purpose of this chapter is to explore processes of conclusion crafting so as to better understand such ways of knowing and some similarities and differences between them.

In “The Externalized Retina,” Michael Lynch (1990) described the process by which the biologists transformed microscope-generated photographs into diagrams. This process involved transforming what was visible in the photograph into a diagram that filtered out that which was considered uninteresting or unimportant, created uniformity amongst objects considered to be of the same theoretical class (such as the size of spots or the colour of an organ), upgraded borders to make them more distinct,

and defined entities considered *theoretically* distinct so that they *appeared* distinct. In his words (*ibid*: 163), “Order is not simply constituted, it is *exposed, seized upon, extended, coded, compared, measured, and subjected to mathematical operations....*” (italics in original).

The process by which the knowledge practitioners I observed crafted conclusions was no different from that Lynch observed in that the clam specialists used a variety of techniques to organize and transform their observations into more general claims. Each sorted and categorized, reasoned, and made comparisons. The exact practices each employed to do so were different, however. To a large extent this can be explained by the types of observations with which they started: the scientists’ representamens were primarily quantitative whereas the First Nations’ were primarily qualitative. It can also be partially explained by whether their goal was to make public or private knowledge, leading the scientists to prefer standardized, “transparent” practices whereas the First Nations had no particular concern about documenting the process by which they construct their conclusions. Yet differences in the practitioners’ practices were about more than observational styles; they indicated opposing preferences for typicality versus difference, nuance versus general pattern, and localized versus “universalized” knowledge. Moreover, their distinct practices and overall processes had important implications for what they came to know and how they understood it.

One of the key questions of interest to both the First Nations clam diggers and the contaminant ecologists was whether salmon aquaculture farms had a negative impact on clams and clam beaches. Members from both the Nuuchahnulth and Kwakwaka’wakw First Nations communities had been noticing changes in their clams and clam beaches for

at least a decade and associated these changes with the nearby aquaculture finfish farms. The provincial and federal government, largely in support of finfish farms, denied the possibility of the farms having a negative impact on clams and denounced the clam diggers' claims as unsubstantiated. It was for this reason that the First Nations communities approached the team of contaminant ecologists to see if they could scientifically demonstrate whether a relationship existed between the two and for this reason that the contaminant ecologists began seeking an answer to this question.

Members within the two First Nations communities differed in their views over how the fish farms had impacted the clams, with the Nuu-chah-nulth community being more divided than the Kwakwaka'wakw. Divisions within the Nuu-chah-nulth largely fell along the lines of whether the member stood to profit from the fish farms, as several community members were employed by these facilities. The Kwakwaka'wakw, on the other hand, were largely opposed to the fish farms. Their opposition had become politicized, however, to the extent that discussion with many diggers on the topic almost always verged on, if it did not fall into, a polemical discussion. For this reason I focus on a few key members of the experienced Nuu-chah-nulth clam digging community who did not work for the fish farms to explore the process by which they moved from observations to conclusions about the relationship between fish farms and clams. I then contrast their process to the one by which the contaminant ecologists moved from observations to conclusions on the same topic.

Although the two sets of knowledge practitioners were interested in answering the same question, they made remarkably different types of observations or, using the language I adopted in chapter 4, they employed different representaments. The Nuu-chah-

nulth made multiple observations of the shape, colour, texture, smell, size, weight, abundance and location of clams, among other things, on the various beaches on which they dug over many years. They were also familiar with the comings and goings of people and commercial operations within their territory. Their observations remained largely qualitative in format and were rarely documented as text. The contaminant ecologists, on the other hand, collected samples of clams from the two regions over the course of a few weeks within a single year. The clams were frozen and transported to a laboratory where they were converted into a fluid extract into which technicians injected additional contaminants. The extract was then chemically analyzed to assess the level of various contaminants within it. These levels were recorded in a spreadsheet as numbers. The representamens the First Nations diggers and contaminant ecologists used, and therefore the set of observations and measures they drew on so as to create ordered understanding, were thus considerably different. In many ways this required that the practices they used to create ordered understanding be different. But a complete explanation needs to look beyond differences in data to see how the techniques by which they crafted their conclusions were combined as ways of knowing.

The Nuu-chah-nulth digger's Crafting Techniques

Over their years of digging, the Nuu-chah-nulth diggers have visited a collection of beaches and, as they dug, they made observations on those beaches such as which beaches reliably produced clams, the location in which specific clam species were found on a beach, and differences in the thickness, weight and colour of clam shells by beach and location on the beach. The diggers were also aware of how many clams they

harvested, either in terms of poundage or sacks from a given beach on a single digging trip.

The Nuu-chah-nulth diggers presently dig on a limited number of beaches but this was not always the case; in previous decades they used to dig on considerably more beaches. Many of these others beaches are now “dead” meaning they have very few, if any, clams living on them. The diggers have been very cognizant of these changes; many talked about “dead” beaches and most can easily list a number of beaches that used to be harvestable but are no longer so. In the words of one digger:

There's no real changes in clam abundance [on the beaches compared to earlier times]—there are plenty of clams still there [on the various beaches]. Just those beaches that are now dead [do not have clams]. On the beaches where the clams have died, the worms, starfish and mussels are all still there—they haven't changed, just the clams have changed.

For the most part the diggers claim that the fish farms killed the clams on the now-dead beaches. I am not sure when they first noted a dead beach, or who first made the connection between dead beaches and fish farms. One thing that is clear is that the association between dead beaches and fish farms is a public topic of discussion; there is an on-going discussion among clam diggers as to the state of beaches and part of this discussion revolves around which beaches are now dead and why they are dead. That said, the discussion tends not to be particularly in-depth; beyond the exchange of a few sentences, most diggers appear to keep their thoughts to themselves. In fact, most diggers appear to have drawn their own conclusions independently of others, at least to the extent that the evidence they use to justify their conclusion is based primarily on their own personal observations. The exact process by which the diggers drew the conclusions was largely internal, meaning it largely occurred in their heads (and bodies)

as opposed to being something they discussed in detail with others or analyzed through an explicit, externalized process such as written mathematics, documented “proofs,” or computer analysis. Instead, their conclusions appear¹⁰⁶ to be based on a series of qualitative and quantitative observations they have made over a period of decades (including before and after the installation of the fish farms) which they then sorted, compared, and carefully reasoned about.

The process of crafting their conclusion about the relationship between fish farms and clam beaches was far from linear. The possibility (i.e. their “hypothesis”) that the fish farms were negatively affecting clams was something that occurred to them a while ago and that they have continued to re-affirm as time goes by as they make new observations. In doing so, though, the Nuu-chah-nulth diggers typically did not have a clear plan regarding what to observe or measure. Moreover, while the diggers attend to a wide variety of characteristics and attributes, they rarely document these observations; at best they are mentally “recorded” in their memory.¹⁰⁷ Consistent with this, their observations are predominantly qualitative. Even representamens that could easily be quantified, such as clam size, tended to be measured by comparison against, for example, the distance between tongs on a digging rake, so no precise number was recorded or noted. One consequence of this style of observation is that they do not know in advance what they will be analyzing or even whether an observation will be used as evidence to draw a conclusion. It also means the practices they employ to draw

¹⁰⁶ I use the term “appear” here because I can not be sure exactly how their conclusions were drawn as the diggers had already formed their conclusions by the time I met them and they had not documented the process by which they formed their conclusions.

¹⁰⁷ Like everyone else, these memories are subject to recall and the diggers likely have selective memory in terms of what they recall.

conclusions are largely (and need only be) practices that can deal with large amounts of undocumented qualitative data.

As a general practice, the Nuu-chah-nulth diggers mentally organized and compared observations they made within and/or between beaches. For example, comparisons are often made in terms of how clam abundance at a given beach has changed over the years, where a particular clam species can be found on a given beach, how the size of clam “meat” (the soft tissue or body of the clam) has changed over time at a given beach, or how the weight of a bucketful of clams is lighter than a similar sized bucket from another beach. The diggers were constantly organizing and analyzing their observations by beach in ways such as these.

This basic comparative structure served as the foundation upon which the diggers built other conclusions, including their conclusion that fish farms “killed” clam beaches. An example from an interview with one of the more experienced commercial diggers illustrates this:

Beaches have a different smell around fish farm sites. In [X] Inlet—all along there it smells different. It's a noticeable change. There is a noticeable stench on big tides. There is green algae with a noticeable scent around. It's the same behind [Y] Passage (behind [Z] Island). There is also a noticeable change in the mud/sand there too.

Given the diggers' propensity to make incessant comparisons within and by beach, it is relatively safe to assume that this digger first made a series of comparisons between beaches in terms of their smells. After this he could then look for a single causal factor that explained the changed smell on these beaches by comparing the set of beaches he had identified as having this smell to their proximity to fish farms. But in saying this I

am already getting ahead of myself, so let me slow down and flesh out the process in more detail.

The following is my reconstruction of how the various Nuu-chah-nulth clam diggers formed their conclusion about the relationship between fish farmers and dead clam beaches.¹⁰⁸ It is based on how the various diggers explained their claim as well as on how they answered other questions I asked them, including questions for which they apparently had not already formed conclusions.

The first step in the drawing of a clam diggers' conclusion was for the digger to notice a definitive change that he wanted to explain—dead beaches. But it was not just one beach or the general state of “dead” beaches to which the digger wanted an answer; he sought an answer to why all the dead beaches he knew were dead. In doing so, he could list all the dead beaches. He then mentally searched for possible explanations. These explanations mainly included local anthropogenic changes such as the presence of fish farms, logging and mining activities. Each of these possibilities was assessed by drawing on the observations and comparisons he had made in regards to dead beaches and whether these beaches were in close proximity to the localized changes. The reverse was also done in which the diggers considered locations where these anthropogenic activities occurred and whether clams are still present on nearby beaches.¹⁰⁹ For example, as one digger explained:

¹⁰⁸ In stating that the diggers claim the fish farms are the cause of dead beaches I do not mean to infer that the diggers do not think logging and mining have a negative impact on clam beaches. In fact, the opposite is true—at least for some diggers; several diggers were concerned about the effect of logging and mining on clams. According to these diggers, though, these activities have a different type of impact on clams than the fish farms.

¹⁰⁹ Morrill reported an expert Cha-Cha fisher that used a similar comparative reasoning process (1967: 413, as quoted in Ruddle 1994):

I logged near the mouth of [the village] area and clams are still there and still OK. When [M] Cove was logged, they dumped a lot of logs there and there were still clams around. The same is true for [N beach]. I still see logs on the beaches there.

To assess proximity between dead beaches and other activities the diggers seem to employ a mental map they had in their mind that included information about currents, waterways and landforms. These assessments of proximity are not based on linear distance from one location to the other, however; proximity also took into account the strength of currents, the direction of currents, the “mixing” of the waters, and other factors. The result was that a digger may decide a beach that appears close to a fish farm in linear distance was not actually close at all. For example, all diggers adamantly denied that a fish farm on the opposite side of a channel from one of the main commercial digging beaches—two locations close in linear proximity—was “close” in terms of the flow of water from one to the other. In fact, they all insisted that the waters in the channel undergo such intensive “mixing” that there was no concern about the fish farm affecting this beach. Likewise, when I asked a digger about another popular commercial digging beach I was given a similar explanation for why it was not “close” to the fish farm: “no, the current doesn’t go to this beach from the fish farm.”

In addition to proximity, the diggers considered timing of the anthropogenic activities. Dead beaches, they all definitively stated, did not exist before fish farms were brought into the area. Before and after comparison were prevalent. For example:

He reasons that it is impossible that manicheel berries are the cause because highly territorial fish which prove toxic are taken in areas where there are no manicheel. Marine worms are unlikely because some fish which are toxic never eat marine worms or other fish which do not eat them. Copper makes some sense as a source because ciguatera is common around wrecks, but not all wrecks have copper bottoms, and some ciguatera is found far from any known wrecks. Algae are the most probably cause because not many species of fish eat algae, only a few of these are taken as food, and these few are heavily implicated.

[C] Sound beaches are now dead. In fact, from [D] River to [C] the clams that are not dead are bad and stinky. When the fish farmers came everything died. The beaches are stinky and the clams are no good there. You can smell it even before you hit the beach. The beach itself is muddy and black. They used to be nice, good beaches. It was about 1 year after the fish farms came around that the beaches started getting bad.

Some diggers' even provided exact accounting for how many years a beach had been dead (which could then be compared to the length of time specific fish farms had been present). For example:

The current runs strong from the fish farm to [F], and this is why these beaches are now all dead. There is a sand bar near [F] where it has been 9 years since there has been any good digging there. It used to be a good beach, though—one that could be worked by commercial diggers from early September to the end of March. Now there are no clams there, though—it's not even good for digging at for one night.

Some diggers were content to claim that fish farms were the reason why some clam beaches were “dead.” Others, however, reasoned further about the relationship between fish farms and clams. For example, one of the diggers said he had not noticed any particular changes on the beaches (other than the “death” of some beach), but that he has been worried about local clams ever since he worked at one of the local fish farms. On the label of a substance called SLICE it stated that the substance should not be fed to animals or put on or in water yet, according to this clam digger and former fish farm employee, it was being dumped into the water by fish farm employees. As he said: “This can't be good for the fish or the clams.”

Other diggers talked about “fish morts” (the dead fish found at the bottom of fish farm pens) being dumped into and therefore polluting the tidal waters. Still other diggers talked about fish feed and a thick dust that comes off fish feed settling below the fish pens and being moved by the daily tidal exchanges onto the beaches. For example, one

digger described how fish farm workers had to spray down the floors of feed barges due to the amount of feed dust in the air. He said that some fish farm "feeder" employees got sick from breathing in fish feed dust so that their throats closed and for one this led to strep throat. He then went on to say:

Dust from the feed settles over the sand where the clams are in.....The feed may not be dissolving right away. If it doesn't dissolve right away (and when it's on the barge it doesn't), then it will affect the beach and cover it with crap.

As can be seen from these examples, all these diggers developed some sort of further reasoned explanation as to why or how the fish farms were affecting clams. But this theoretical reasoning did not form the basis of the Nuu-chah-nulth diggers' claims that fish farms were the cause of "dead" beaches. If anything, this reasoning was in addition to their conclusions about why there were dead beaches.

As stated, the Nuu-chah-nulth diggers' conclusions were based on a reiterative process that extended over a large number of years. Familiarity and repeated reflection or pointed new observations led to their gradual understanding and the drawing of their conclusion. During this time, the diggers returned to dig non-dead beaches and occasionally tested dead beaches to see if they had begun to recover. Over time, new beaches have died, and when and where these beaches are is taken into further consideration in relation to the puzzle of dead beaches. By doing so, the diggers compiled numerous observations of the same beach over different seasons and years, all in the context of their mental map of the geographic area. By comparing these observations in terms of geography, proximity and timing, the diggers were able to craft a conclusion.

The Contaminant Ecologists' Crafting Techniques

At the point of my writing the contaminant ecologists have not published the results of their research on clams as an article in a peer reviewed journal. They have, however, submitted a final report to their funding agent in which they reported their preliminary “findings.” It is the work done towards this report that I draw on in the following discussion.

The contaminant ecologists do not have (and therefore can not draw on) intimate historical knowledge about the clam beaches. Instead their study is an ahistorical cross-section in that their observations and measurements are based on clams collected within the span of a single year. Instead of using observations about clams and beaches over time, the contaminant ecologists used chemical analysis as a means to answer the same question as the Nuu-chah-nulth asked. The pattern in contaminant levels they expected to detect within clams was based on a hypothesized history, though, in that higher concentrations of contaminants originating from fish farms were predicted to have traveled to beaches and been taken up by clams closer to rather than further from fish farms.

In the same way that the exact representamen to be used in data analysis were determined before a team member ever arrived at a sampling site, the contaminant ecologists' research was conducted in distinct steps or stages with each one having to be largely completed before the next stage could begin. As the PI repeatedly stressed, he could only draw final conclusions when he had finalized data for analysis. This meant that the crafting of conclusions occurred as a distinct stage in the process of creating new

knowledge. The clams upon which they made their observations and measures had been collected in a series of brief sampling trips. In each area they had selected six beaches from which to collect clams, three of which were deemed “close to” and three “far from” fish farms (reference beaches). In each region they collected two species of clam with the clams divided into two size classes. A subgroup of the ecologists visited each region twice in order to collect a complete complement of samples.¹¹⁰ After each sample collection trip these contaminant ecologists transported the clams to the university lab where, over a number of months, the different subgroup of the contaminant ecologists prepared and analyzed the clams for their chemical content. Their end product was a series of numbers, entered into a spreadsheet, representing the level of 37 contaminants in each composite¹¹¹ with repeated analysis of a subset of composites as a check on the analysis procedure.¹¹² The data created by the lab chemist through chemical analysis was thus quantitative and arranged on the spreadsheet in clear, mutually exclusive categories representing, for example, “small manila clams from beach #1 in region #1”. Columns on the spreadsheet were headed with codes indicating the beach of origin, species and size class of clam. The rows corresponded to the type of contaminant.

This spreadsheet, once finalized, was handed over to the principal investigator for analysis and interpretation—i.e. for the crafting of conclusions. In analyzing the data, the PI’s goal was to identify patterns in the numbers. Patterns were said to exist if there was

¹¹⁰ The original plan was to compare samples from the same beach in different seasons to see if contaminant levels differed by the season. They were not always able to collect a full complement of samples during each visit, however, so the samples from the different seasons were combined to create a full complement of samples from the beach.

¹¹¹ A composite was a sample of clams from the same beach and in the same size category packaged together in a hexane-foil “burrito.”

¹¹² The contaminant ecologists also analyzed the clam composites in terms of their stable isotope ratios for Carbon and Nitrogen. This analysis was similar to that which the contaminant ecologists undertook for PCBs, as described here.

a clear difference between, say, the contaminant levels in clams on beaches close to fish farms and those far from them. To determine if such patterns existed in the data the PI sorted specific columns on the spreadsheet and isolated them in a table or in a graph.

Given that each beach was just one of three beaches from one of three regions and the contaminant levels for each beach could differ considerably, patterns were difficult to detect. Patterns are easiest to detect if all figures with a classification are clearly larger (or smaller) than an opposing classification. This was not the case. Part of the PI's difficulty in detecting patterns was that he had too many numbers to examine. For example, if he wanted to compare clams by beach classification he first had to decide whether to focus on small or large clams and decide whether to focus on manila, little neck or butter clams (thus reducing the number of columns he examined to a minimum of 4 for each beach).¹¹³ But this still left him having to compare three beaches to three other beaches in three regions. If he wanted to see if there was a pattern across species regardless of size he had to compare a minimum of 8 columns for each of the 3 reference and fish farm beaches in each region. If this was not difficult enough, he had to do this for 37 different PCB congeners.

The PI deemed this large amount of numbers too much to manage at once. To make it more manageable, he reduced the contaminant levels data from 37 individual PCBs congeners to 8 homolog groups.¹¹⁴ To do so he used the mean or average to mathematically calculate a single number for each homolog. The contaminant ecologist then again tried to compare the various figures to detect a pattern. This time the task of comparison was easier, but the numbers in the columns he isolated on tables still did not

¹¹³ The numbers presented here are actually fewer than the number of columns the PI actually had to compare as I have not included the replicates in the discussion above.

¹¹⁴ A homolog group is a set of PCBs that are chemically similar to one another.

show clear patterns—there was “cross-over” in the figures among theoretically opposed classifications meaning that the range of numbers overlapped for groups that were hypothesized to differ.¹¹⁵

The question for the contaminant ecologist then became one of how to determine whether patterns exist despite overlap in the range of figures theoretically expected to be distinct. His answer was to further reduce his numbers. Specifically, he performed t-tests—a statistical test in which a group of numbers is averaged¹¹⁶ and compared to a second figure to see if this second figure falls within an acceptable distance¹¹⁷ from the variance found within the averaged numbers. For example, the figures for reference beaches or beaches “far from” fish farm in a given region were statistically compared to an individual figure from a beach “close to” a fish farm from the same region. Numbers were thus transformed and reduced once again. The final product of each t-test was two numbers, only one of which he focused upon: the p value. This value indicated whether the difference between the numbers fell within a pre-determined acceptable range. In doing so, the PI considerably reduced the number of figures he had to evaluate. As the PI computed one p value for each of the three farm beaches for each region for each homolog group it meant that, (for example) for the homolog group of TriCB PCB contaminants, 64 figures were reduced to three p values. Multiply these reductions for all 37 contaminants and it is easy to see how drastically the PI had reduced the number of comparisons he needed to make.

¹¹⁵ During this process the PI decided to eliminate clam size as a distinct classification. This was because there was no notable pattern between size groups for contaminant levels. This allowed the biologist to combine all clams of one species from a single beach together instead of having to deal with them as separate groups for comparison.

¹¹⁶ This time, the contaminant ecologists calculated the geometric mean, a similar but not identical type of mean.

¹¹⁷ As specified by the student’s curve, which is similar to the normal curve.

Yet still the pattern was not clear. If the pattern were clear, the p values for all three beaches for all contaminants (or at least the suite of contaminants expected to be associated with farm sites) would be below an arbitrarily-designated cut-off of .01. But they were not—only some of them were. In some cases, all three p values were below the cut-off, suggesting that that contaminant level for that homolog is notably different between reference and farm beaches in that region. In other cases, though, only one, two or none of the p values were below the cut-off.

At this point the PI could have made another arbitrary cut-off such as two out of three p values needed to be below .01 for him to consider the difference between these beaches distinct. Instead, he decided to assess each grouping of p values on a number of factors (such as how *much* above or below the .01 cut-off the various p values fell), in addition to the number of beaches falling below the .01 cut-off. He also decided to focus on a sub-set of the homologs and to ignore or de-emphasize others. For this assessment he did not follow any particular protocol but individually weighed each decision. At this point, the PI was finally able to make statements regarding patterns he had found.

In total, the PI used a number of practices to convert his spreadsheet full of numbers into a small group of numbers from which he identified a pattern including comparison of “pertinent” categories (e.g. individual beaches, beach close to or far from fish farm, size of clam, region, type of contaminant, etc.) in terms of specific numbers and overlap in ranges of theoretically-distinct figures, isolated some categories in tables and graphs, reduced the number of figures by mathematical averaging and additional statistical tests, conducted individualized assessments, and reasoned about how to proceed at each juncture of analysis. His goal was to evaluate whether his hypotheses

regarding the relationship between clams and fish farms were correct. The process he used was linear (at least in comparison to that of the Nuu-chah-nulth diggers') in that he proceeded from one clearly- articulated step to another. He was not always certain what is "next step" would be, as steps were adjusted to suit whatever he found at each juncture, but the options he chose among were largely shaped by practices commonly used by ecologists such as calculating averages and using statistical tests. His process was also distinct from the process by which his team had made observations and measurements; it was made in a physical environment (his office at the university) distinct and at a distance from where the clams originated. The measurements he analyzed were recorded and manipulated in computer software—a device clearly outside the contaminant ecologist's body. Moreover, the steps he used in analysis could be (with the exception of his final assessment) explained to and repeated by others.

Comparing processes and practices

Some general consistencies between the Nuu-chah-nulth diggers and contaminant ecologists existed in terms of the practices they employed to craft their conclusions. Both had a hypothesis or plausible explanations as to why a particular phenomenon occurred and drew on observations and measurements to assess the hypothesis. They both had practices by which they ordered their observations/measurement, made comparisons, and reasoned. In both cases their conclusions were crafted by a process of comparison within and across beaches – in one case, dead vs. alive beaches, in the other, beaches classified as "close to" and "far from" fish farms.

Yet, while they employed many of the same basic manipulations the specific practices were considerably different. For example, although sorting of observations and measurements was undertaken by both the Nuuchahnulth and the contaminant ecologists, the contaminant ecologists sorted their measurements during their process of data collection¹¹⁸ whereas the Nuuchahnulth did so while crafting their conclusion. The contaminant ecologists had formally sorted their measurements in the sense that measurements for key characteristics of interest were explicitly listed in rows and columns. Each number represented a single characteristic—a contaminant level—of a composite of clams. This characteristic was decontextualized in the sense that, although the recorded characteristics about the composite were grouped together and identified as originating from the same composite, at this point in the project it was impossible for the PI to experience these characteristics as parts of the larger whole from which they originated. In fact, during most steps of his analysis, he did not even pay attention to all the information he did have about the composite. Instead, his attention focused on a specific characteristic as it related across composites.

As many sociologists of science have pointed out, this type of decontextualized, variable-oriented analysis “normalizes” observations/measurements so as to make them equivalent (Lynch 1985); no longer are other characteristics of the sample or the whole sample itself relevant. Porter (1986) discussed the underlying logic of this approach in relation to the European history of census taking and why this approach did not always make sense. As can be seen in the following quotation, normalizing comparable

¹¹⁸ As many sociologists of science have noted before me, data is not “collected” in the sense that it is passively “picked” and “placed” into a data table or respective receptacle. Instead, data results from a process of one or more knowledge practitioners interacting with, and in the process, making note of, an empirical phenomenon.

attributes requires assuming that the bodies from which the attributes originate are equivalent, irrespective of their origin and history:

Implicitly, at least, statistics tended to equalize subjects. It makes no sense to count people if their common personhood is not seen as somehow more significant than their differences. The Old Regime saw not autonomous persons, but members of estates. They possessed not individual rights, but a maze of privileges, given by history, identified with nature, and inherited through birth. The social world was too intricately differentiated for a mere census to tell much about what really mattered. (Porter 1986: 25)

The numbers in the contaminant ecologists' spreadsheet were treated the same as the individuals are in modern states when being enumerated in that they, too, assumed the composites were equivalent enough that unmeasured attributes could be ignored.

But normalizing measures had implications. While it facilitated comparisons in that specific attributes could be isolated and placed in a table for comparison to other composites, their lack of knowledge beyond the measured aspects of the composite limited the PI's ability to assess anomalies. For example, he had no way of interpreting the significance of a beach considered "far from" fish farms that had a high level of contaminants—higher than other reference beaches and as high as those from beaches "close to" fish farms. All he could do with this type of data was either exclude it as an "outlier" or include it, unexplained, in his analysis.

In contrast, the Nuu-chah-nulth diggers' sorting procedures were informal in the sense that they did not list their observations of specific attributes for all clams they had encountered. They did not form mental or physical tables with discreet rows and columns. Instead, their observations about specific characteristics were held in context of the whole from which they originated and their sorting process was nuanced. For example, in some cases only one end of a beach was considered "dead." A digger could

explain this as being due to currents reaching the beach from different directions, only some of which were from fish farms. The dead end of the beach was described as having received considerably more water from the fish farm. In such an analysis, the beach was not placed in either the category of “dead” beach or “non-dead” beach. Instead, the whole of the beach (and its various “parts”) were considered simultaneously in relation to other factors, with one end being considered distinct yet still a part of the whole beach. Similarly, anomalies were assessed in terms of additional information available. For example, in some cases dead beaches occurred in locations that, for all intents and purposes, could not be considered close to a fish farm. In these cases other explanations were sought such as whether a fish farm had once been placed there but had since been removed, whether fish farm nets were known to be “washed” (with strong chemicals) in a nearby area or the beach was dead for another reason such as experimental mechanical clam harvesting on the beach some years previous that wiped out the clams on that beach. Such nuanced sorting thus allowed for qualitative differences and historical changes to be taken into consideration in a way that fixed or formal sorting of observations like that employed by the contaminant ecologists would not allow.¹¹⁹

¹¹⁹ In many ways the differences between the two sets of knowledge practitioners’ sorting styles are comparable to differences between qualitative and quantitative social science researchers. As Charles Ragin (2004: 137) has described it:

Case-oriented investigators try to account for every case in their attempt to uncover patterned diversity. Cases often deviate from common patterns, but these deviations are identified and addressed. Investigators make every effort to identify the factors at work in nonconforming cases, even when these factors are outside the frameworks they bring to the research. In variable-oriented research, by contrast, the “error” that remains at the end of an investigation may embrace much more than it does in qualitative research. It includes randomness, omitted variables, poor measurement, model misspecification, and other factors, including ignorance of these cases studied.

In the case of the clam knowledge practitioners, the Nu-u-chah-nulth are comparable to what Ragin describes as “case-oriented investigators” and the contaminant ecologists are comparable to the “variable-oriented investigators.”

The contaminant ecologist's and Nuu-chah-nulth clam diggers' crafting process differed in other ways. The contaminant ecologists' process was notably more articulated in both the sense of being verbalized and expanded than the Nuu-chah-nulth diggers; their procedure involved many clearly articulated, documented steps. Similarly, the contaminant ecologist's process incorporated standard analytical practices such as those related to statistics and was temporally and behaviorally distinct from his process of observation/measurement. The Nuu-chah-nulth diggers' process, on the other hand, was iterative, occurred over a long period of time, drew on observations that were not necessarily established in advance as being relevant to future knowledge,¹²⁰ and used practices that often remained internal and implicit to the knower.

The contaminant ecologist's process was not always as articulated as it could have been, though. In his final step of pattern-detection, the PI resorted to an unarticulated assessment of his figures in which he decided whether a contaminant was *generally* higher on beaches closer to fish farms than beaches farther from fish farms by drawing on a variety of information but not explaining the exact weighting he gave to this information in his evaluation. In this way, the contaminant ecologist also used a nuanced, "black boxed" form of assessment.

Another way in which the two sets of knowledge practitioners differed was in terms of their emphasis on synthesis and partition. The contaminant ecologist's practices heavily focused on synthesis in that he frequently calculated averages and, in doing so, synthesized his measurements. This form of synthesis resulted in a composite figure, a

¹²⁰ Although this was not always the case as some observations were clearly sought out as a means to assess the situation.

single figure that represented “clams,” despite the figure originating from a number of clams from potentially different beaches, time periods, and of different ages. The effect was that measured attributes of individual clams were used to claim, for example, what “contaminant levels in clams far from fish farms” are like. As Roth and Bowen describe it in their study of ecologists working with lizards (1999: 723):

What mathematization achieves is more than just seeing, for it is no longer individuals that are seen but a conglomerate of indistinguishable, replaceable individuals. It is a move from ‘this lizard is doing X’ to ‘lizards do X’, from the psychology of the individual to the sociology of the masses.....

For the contaminant ecologist, the practice of calculating a mean is used to transform empirical measurements of the “same” class of phenomena into a composite that is said to represent that class but which may not correspond to any specific clam. At best, the mean represents a set or class of cases. As nineteenth-century statisticians argued, the average eliminates “everything contingent, accidental, inexplicable or personal, and left only large-scale regularities” (Porter 1992b: 645; also see Porter 1986: 5).¹²¹

The PI’s rational for calculating means differed by the application. Generally, the PI was interested in finding the “central tendency” of his figures. Why the central tendency was important differed, though. For example, one reason he calculated means was because he had replicates—the same sample prepared and analyzed twice as a double check on the lab process. Rarely did the replicated sample measures yield identical

¹²¹ My favourite depiction of how the mean transforms data was stated by Sir Francis Galton in the 19th century (as quoted in Porter 1986: 129):

It is difficult to understand why statisticians commonly limit their inquiries to Averages, and do not revel in more comprehensive views. Their souls seem as dull to the charm of variety as that of the native of one of our flat English countries, whose retrospect of Switzerland was that, if its mountains could be thrown into its lakes, two nuisances would be got rid of at once.

As Galton’s analogy suggests, averaging deals with variation by reducing or flattening differences.

figures. The variation found within these figures was expected, though, as this is what many researchers have found when conducting this type of chemical analysis. Moreover, the variation was not assumed to be due to variation within the sample itself (e.g. due to having used different clams from the same composite in the replicate). Instead, it was assumed that the differences between numbers reflected error or variation in the sample preparation and analysis procedure. Given the lengthy and convoluted chemical procedure involved in preparing the samples for analysis the possibility of producing different figures does seem highly possible so their assumption is not unfounded. The PI's purpose in calculating means, then, was to (hopefully) neutralize these measurement errors and identify the "true" value or level of contaminant. Averaging was used to eliminate "unnecessary noise" in measurements.

Means were calculated in other situations as well, like when the PI grouped different contaminants into homolog groups and in the t-tests when the PI calculated the mean for figures from reference beaches. In situations such as these, differences were believed to *not* be due—at least not exclusively—to preparation and measurement error; different levels of contaminants were expected to occur in different types of contaminants¹²² and at different beaches.

Calculating central tendencies is a common practice in ecology in cases where differences are not expected to be due to measurement error. In fact, when I asked the PI why he calculated means in this case he initially gave me a strange look as if I had perhaps missed out on some basic education in my life. Theodore Porter's (1986) history of the rise of statistical thinking perhaps supplies the best answer to my question: the use of the mean and other statistical procedures originated from a concern

¹²² This was true even for those contaminants very similar in chemical structure.

with the likelihood of chance variation being attributable to error, but continued to be used even in situations where measurement error was only one possible explanation for differences in data because it was an efficient method for dealing with (i.e. reducing) large sets of numbers.

The Nuu-chah-nulth diggers, in contrast, did not synthesize their observations—at least not in the same way. They were not worried about “noise” in their observations, as all observed differences were taken to be real differences that could be explained by nuances in their subject matter. Their approach was to confirm or disconfirm whether a case was consistent with other cases they had deemed similar. For example, when they encountered (or thought about) a new dead beach they would then evaluate whether it, too, was close to a fish farm or other anthropogenic sites of activity. Any inconsistencies found among similarly-classified cases (e.g. dead beaches) were dealt with by looking for additional explanations. In doing so, the Nuu-chah-nulth maintained their focus on specific, concrete observations they had made; unlike the contaminant ecologist, they did not transform their observations into a new, synthesized product.

Above I described how the contaminant ecologists were limited in their ability to explain observed anomalies due to their practice of decontextualizing and normalizing their data. This was not the case with the Nuu-chah-nulth diggers. In fact, maintaining context was of central importance in their analysis. The Nuu-chah-nulth therefore did not have the problem of being unable to explain anomalies. Yet their approach potentially has its own weakness: their nuanced sorting perhaps allowed them to excuse away any and all cases that did not fit their larger claim.¹²³

¹²³ This is also a common argument made when comparing quantitative to qualitative social science research. For example, see Babbie 2007.

There is a second potential weakness in the Nuu-chah-nulth diggers' style of analysis. Even though the Nuu-chah-nulth diggers had made considerably more comprehensive observations than the contaminant ecologists and maintained their observations in context, they too were potentially limited by their "data" in that they were not omnipotent and had not witnessed everything there was to experience. My best example of this is from my work with the Kwakwaka'wakw diggers, but the point transfers well to the Nuu-chah-nulth diggers. The story is about two Kwakiult diggers who disagreed over why clam abundance had declined on their beaches over the years. The first digger, who was considerably older, claimed the lack of clams must have something to do with the quality of the water changing. The second digger claimed it was because native diggers were over-digging the beaches (i.e. removing too many clams for the beaches for the clams to reproduce at a sustainable rate). The first digger adamantly denied over-digging as a possibility; he described how he had witnessed up to 26 boats with five people on each out digging in their territory in the 1940's and 50's and that they never run out of clams then. There are far fewer diggers on the beaches now, so, he asked, how can it be that these few diggers are over-digging the beaches? He said the number of clams started decreasing in the early 1960's, and has continued to decline ever since.

The second digger was equally adamant in his reasoning; less than a decade before I began my research, the DFO had closed down the commercial butter clam harvest for a period of years. After this period, the second digger claimed, the number of butter clams on the beaches had dramatically increased. But since the commercial diggers returned to clam harvesting, clam abundance had once again declined.

The first digger, being older, drew on a longer history of the area. He not only experienced a deeper historical sense of place for many locations within their tribal territory, but, consequently, interpreted the lack of clams differently. Interestingly, though, he had also stopped digging for several years. Included among the times he had not dug was the period immediately after the beaches were re-opened by the DFO. In contrast, the second digger had dug every year for the proceeding 25 years including some of the years during the closure (when he commercially harvested little neck clams) and immediately after the butter clam fishery was re-opened.

Their differences in exposure to the clams, specifically the years in which they had dug, was the best explanation I found for their diverging explanations for the lower clam abundance. In other words, their lived experience of the region and therefore the contextual framework within which they reasoned had implications for what they “knew.” The same type of selective exposure is also possible in terms of the Nuuchah-nulth and the data they drew on for crafting their conclusions.

In terms of other differences between the contaminant ecologists and the Nuuchah-nulth diggers, both sets of practitioners reasoned, but what they reasoned about differed somewhat. The majority of reasoning demonstrated by the contaminant ecologist was reasoning over which practices he should employ in his analysis, like whether to use ANOVA (analysis of variance) or a t-test and whether he could justify to his peers why he had reduced the data into the groups (e.g. homologs) that he had. He also reasoned about whether the numbers he had were “good enough” to show that a pattern existed. It was only at the final stages of his drawing conclusions (in fact, a stage after that which I described above) that he reasoned about the pattern he had “detected”

in relation to what he knew about the original context from which the samples were collected. The Nuu-chah-nulth, on the other hand, reasoned as they performed their nuanced sorting. In fact, their reasoning could be described as replacing what the scientists' used mathematics and statistical procedures for; drawing on observations they could recall, they reasoned through a series of hypotheses to evaluate how consistent their observations were with these hypotheses.

Another major difference between the knowledge practitioners was in regards to their underlying approach to comparison. More specifically, they differed in terms of what they considered combinable. In my above description of how the contaminant ecologist crafted his conclusions I only described the process up to the point where he felt he had detected a pattern in regards to contaminant levels on beaches deemed close to and far from fish farms. After this step the PI undertook a series of additional steps before he made final claims. One of these steps involved comparing the level of contaminants he found in his samples to the level of contaminants another group of researchers had found in wild Pacific salmon.

The contaminant ecologist makes no mention of where the wild Pacific salmon used for the other study were caught. According to the authors of the original work (Easton et al 2002), the wild fish were purchased from a cold storage facility of a fish company in Vancouver, British Columbia. No further information about the origin of the fish was mentioned. In all likelihood, the fish were caught somewhere along the coastal waters of British Columbia, although they might have come from the neighbouring American states of Washington or Alaska. This places their capture somewhere nearby but not from the same location as the clams included in the contaminant ecology project.

The uncertainty about the origin of the fish was not a concern to the contaminant ecologists. In comparing the wild salmon to the clams, the assumption was that such information was combinable. Potential differences in location and how this affected the seafoods were not considered. What made the measured wild Pacific salmon and the clam contaminant levels comparable, in the eyes of the contaminant ecologist, was that the procedure by which both had been measured was highly similar.

Another example makes my point clearer. The clam study I have reported here was part of a larger study in which the contaminant ecologists were involved. In another part of their larger study they had harvested and chemically analyzed edible portions of rockfish found close to and far from fish farms. In the quotation below, the PI describes to me one of his conclusions from this part of the project:

We know from other studies that fish farms attract lots and lots of little fish. Because they are producing lots and lots of food for them, right? They are dumping fish food and poop on the bottom and bugs love that, and so there are lots of bugs. And there is always lights and there's nets and there's....structure. And so farms attract lots and lots of little fish. According to some studies, in, I think like the Mediterranean, tens of thousands more fish on a comparable area without a farm. Ludicrous densities of little fish around these farms. And that's not surprising. I mean, there's lots of food, right? And structure and lights, and stuff. And fish like that. And they accumulate under docks, and things too, but this is like the best dock in the world for them. Plus there's antipredation, which protects them for being eaten by seals and stuff. So, that has got to change the foodweb structure, right? And predatory fish, or let's say fish that could eat fish or could eat bugs, like rockfish, will end up eating more fish. 'Cause there is more fish around. And that bumps up their trophic level, and exposes them to higher concentrations of mercury, just through the, you know, simple fact of them being the higher trophic level. So, the neat angle here is that the rockfish are more contaminated NOT because the fish is putting more contaminants into the water, but because the farm is changing the ecosystem.

As can be seen in this quote, the PI assumed what others had learned about fish under fish farms in the Mediterranean—a geographic region far removed from the waters off the coast of British Columbia—held for B.C. fish farms and therefore combined what he

knew about contaminant levels in rockfish with what these other researchers reported about fish farms. Like with the Pacific salmon described above, what made them combinable was that the Mediterranean research was conducted on a similar topic and according to scientific standards.

The Nuu-chah-nulth diggers' practices were distinct from the contaminant ecologists in these regards. Instead of creating a composite picture in which material was cobbled together from a variety of wide-spread geographic origins, the Nuu-chah-nulth assumed that clams and beaches outside their territory were potentially distinct from those inside their territory and therefore not combinable. Combining, for example, information about the ecological structure of a few fish farms in the Mediterranean with the stable isotope levels of fish found near some fish farms in British Columbia would be considered like mixing apples with oranges and calling them the same thing. In fact, the diggers tended to keep their observations about one beach distinct even from other beaches in the territory and some diggers preferred not to generalize about clam beaches at all.¹²⁴ Even their claims that the fish farms were the cause of "dead" beaches were meant to explain what had occurred in their area; the impact of fish farms on other clam beaches in other areas was open for debate.

I return to the topic of averages briefly now to further examine ideas about measurement accuracy and exactness. Specifically, the practices of averaging and combining data from different geographic origins seem somewhat strange when considering the importance the contaminant ecologists' placed on these. The act of averaging takes numbers the contaminant ecologists attempted with painstaking accuracy to achieve only to calculate an average which effectively meant nothing at all in that it no

¹²⁴ This was even more common among the Kwakwaka'wakw than the Nuu-chah-nulth diggers.

longer corresponded to any particular clam or even clam composite. These numbers were further transformed (through additional averaging) when individual contaminant levels were converted into an average for homolog groups, which were then used to calculate the average homolog level for reference beaches. At this point, the figures are several steps removed from representing any one measure (e.g. the contaminant level of any specific contaminant from any specific clam) but multiple blendings of numbers. Regardless, the calculated figures continue to be reported at two decimal places for “accuracy.”

Yet the practice the PI used for “being accurate” did not always reflect his true feelings about the figures he generated. Given that both the chemical and data analysis processes they employed had transformed their measurements beyond anything any team member (much less any other human being) could directly experience, the PI was in a position of having to trust the procedures he used as there was no way to verify the outcome of the measurements or his analysis in any direct sense. Despite this, he was also aware that the procedures he employed could result in incorrect answers.¹²⁵ This idea is captured in a quotation from one of my interviews with him:

CM: ...didn't you say there was a lot of variability between the replicates?

PI: Yah, but that is difficult to escape. I mean, it just...even the best methods for analyzing organic contaminants have a lot of variability, which is why you have to do a lot of analyses and trust that on average you get about the right answer. But the variability is hard to escape.

The quotation is about variance in replicates, but it bears a larger message. As the PI makes clear, considerable variation can be found in any contaminant data and that,

¹²⁵ For example, something could have gone wrong in the procedure by which the clams were prepared for chemical analysis or during their chemical analysis in the various analysis machines. Likewise, the codes could be switched on the sample vials so that the data was incorrectly labelled.

because of this, clear patterns in the data need not exist. Instead, considerable analysis is required to find such patterns. Due to the high amount of variability and the ensuing need for extensive analyses, he is put in the position of having to trust these analytical practices. In doing so, he also recognized that not all his answers will be correct. Interestingly, he uses the term “average” in a different sense than discussed above; what one must trust is that the figures produced by various statistical techniques are correct *on average*.¹²⁶

But trust originates from another source as well—one that has already been mentioned above. The contaminant ecologist not only needed to trust the practices he used to craft his conclusions, but also needed to trust the entire process by which he constructed his knowledge. Specifically, he needed to trust that the representamens he employed capture the most relevant pieces of information for answering his question and that all other information (e.g. additional characteristics about clams or the beaches) is also “noise.” As mentioned, the contaminant ecologists did not have strong historical knowledge of the areas in which they collected their clam samples. This not only created difficulties in terms of assessing anomalies; it also made it difficult for them to adequately assess whether the lack of a strong pattern found between clams from reference beaches versus beaches close to fish farms was due to fish farms not having a strong negative impact on clam beaches, contaminants originating from a wider variety of sources than just fish farms, measurement error, poor selection and categorization of beaches or “natural” variation in the uptake of contaminants in clams. Limited information by which to contextualize their observations meant the contaminant

¹²⁶ This use of “average” is similar to the idea of confidence intervals that the sample parameter may or may not reflect the actual characteristic of the population.

ecologists had to place faith in the few representamens they had selected as meaningful for generating their knowledge.

A final point of difference between the two sets of knowledge practitioners is in terms of the degree to which they qualify their claims. Perhaps surprising, given their affinity for nuanced sorting comparison and reasoning, the Nuu-chah-nulth diggers' claim that dead beaches were caused by fish farms was not qualified in any way; like the Kwakwaka'wakw,¹²⁷ who also made such definitive statements, their conclusions were not nuanced or hedged. On the other hand, despite (or perhaps because) the fact that the contaminant ecologists combined information from different geographic sources, it was their practice to phrase their conclusions with carefully-worded qualifications. This can be seen in their claim that "Biota near fish farms show clear changes in contaminant profiles, *sometimes including large increases* in concentrations of PCBs and organochlorine pesticides, *but often including decreases*," as well as their statement that "Biota near fish farms *show variable changes* in stable isotope signatures, but overall these changes support the hypothesis that biota at the base of the food web are consuming an appreciable amount of farm waste" (deBruyn et al. 2004; emphasis added). As can be seen, they were careful to indicate when variation disrupted the overall pattern described.

Before concluding I need to return to the issues of comparability and synthesis once again as, combined, they have broader implications than already discussed. Scientific knowledge has often been described as "universal knowledge," especially in relation to traditional ecological knowledge, which is often referred to as "local." What exactly these terms mean, however, is not always clear (Radder 1992). To confuse the

¹²⁷ As several Kwakwaka'wakw diggers said to me when I re-phrased their statement as "so you think that..." (for example, "so you think that the fish farms are killing the clams on the beaches?") they would respond by saying, "no, I don't *think*, I *know*."

issue more, some sociologists of science have claimed that scientific knowledge is also local (e.g. Latour 1988, 1999). In the case of the contaminant ecologists it becomes relatively clear why *their* knowledge can be thought of as universal. Not only were their conclusions based on means (averages) which resulted in figures that represented a synthesis of cases from multiple *local* geographic and temporal settings (e.g. within a relatively small geographic area and within the time frame of a year), but they then combined this information with material from a variety of geographic origins. The result was that their conclusions, as Nagel so eloquently phrased it (1986), constituted “a view from nowhere.” By stating this, though, I do not mean to suggest that their knowledge *applies* universally—that it reflects *all* cases, in all circumstances. The practice of averaging, alone, makes this point clear. In fact, we can not even be sure exactly *which* cases it does apply to, as it is meant to be a statement about general (central) tendencies or higher-scale order. For example, even if, on average, clams near fish farms have higher levels of pollutants, the strength of the currents, a protective barrier, an inordinate amount of mixing with fresh waters or a number of other factors may result in some clams having lower levels of pollutants at beaches close to fish farms than other beaches at a similar distance or even further from fish farms. Just because their conclusions were phrased in the form of general propositions does not mean they can necessarily predict or describe specific local conditions much less the local conditions of another geographic location.

The contaminant ecologists’ preference for such “universal” knowledge, despite its shortcomings, should not be surprising, though, given their interests in making public knowledge (see chapter four). In the case of the contaminant ecologists, there is

little to gain by knowing specifics about any given beach, time period, or clam on a beach. In contrast, there is much to gain if they can make a general statement about the relationship between clams and fish farms or how something emanating from the fish farms negatively affects clams. This is because the majority of the consumers of their knowledge are, for the most part, not interested in the specific beaches and fish farms included in the study. In fact, many of the contaminant ecologists and citizens of British Columbia who are concerned about the impact of fish farms will never place foot on any one of the First Nations' beaches. What *they* want to know is something more general and at a higher scale of order—something that can potentially apply to a number of locations. The fact that their product is intended for public consumption therefore has important implications for why the contaminant ecologists' do not consider the practice by which they craft their conclusions as problematic.

The Nuu-chah-nulth conclusions, in contrast, were clearly local in that they did not synthesize or combine their observations to form a composite picture. Like the contaminant ecologists, though, the Nuu-chah-nulth diggers' interests in clams affected the way in which they chose to craft their conclusions. They crafted conclusions about clams in a way that allowed them to be better clam diggers. By this I mean they created location-specific knowledge. They wanted to answer questions such as whether they will find clams on *this* beach, if there are certain spots on this beach that usually have a lot of clams, if other diggers have recently dug at this beach, or whether the clams from this beach have a good flavour. Whether fish farms were negatively impacting clams outside their territory was someone else's concern. Their conclusions were indisputably local in the sense the answers were about local conditions; they were not crafted from

information originating outside the local nor was it meant to explain anything beyond the local. What becomes known may not be defensible in terms of the knower being able to clearly demonstrate how he crafted his conclusion, but it was directly linked to observed representamen.

In this chapter I have described various techniques and practices the knowledge practitioners employed in their process of crafting a conclusion about whether fish farms are negatively affecting clam beaches. While the underlying methods are similar in that both sets of practitioners organize or sort their observations/measures, make comparisons and reason, the specific practices they employed were considerably different: one used formal while the other used nuanced sorting; one synthesized measurements while the other built on consistencies and inconsistencies; one primarily reasoned in regards to making decisions about which techniques to employ while the other primarily reasoned by drawing inferences from compared observations. Each employed a variety of ways to craft their conclusions resulting in two distinct processes by which they moved from varied observations and measures to a cohesive (or set of cohesive) claims. Neither set of practices or ways of knowing, though, was ideal in the sense that there are known shortcomings associated with each. Moreover, the reason they favoured some practices over others was due to differences in interests. The result was that one set of knowledge practitioners' way of knowing consistently moved towards creating "universal" knowledge whereas the other way of knowing created knowledge focused at the level of the local.

Chapter 7: The Knowing is in the Doing

What can be known and translated into the forms of thought is already given in the conditions of experience created by the practical activities of people.

Smith 1990: 40

In this final chapter I draw the discussion of ways of knowing to a temporary closure by following up on two underlying themes that that I have yet to discuss in any definitive way. The first theme is the importance of practices in relation to ways of knowing. As I argue here, ways of knowing are anchored in the practices one employs while coming to know. All knowledge has a temporal and corporal aspect to it and develops through an iterative process.¹²⁸ This iterative process is intertwined with cognitive processes of classification and reasoning. Combined, they ground the knowledge practitioner in particular perspectives. By drawing on examples for my case studies I show how this argument is relevant to both the immediate conclusions drawn by knowledge practitioners and the larger knowledge tradition in which they work.

The second theme is the social inequity between ways of knowing. Epistemic hierarchies exist in which some ways of knowing are privileged over others. Specifically, science is often treated as superior to other empirical ways of knowing.

¹²⁸ This section resonates well with John Gatewood's criticism of cognitive anthropology in which he states:

The disregard of action in cognitive anthropology is symptomatic of the more fundamental lack of concern with the temporal dimension of knowledge. Because we tend to focus on human beings as *understanding*-systems to the exclusion of human beings as *acting*-systems, we lose sight of the fact that cognizing, thinking, and knowing take time just as much as do easily observable actions such as running, winking and fidgeting.

(1985: 199-200)

This section also resonates well with a lot of the discussion by Annemarie Mol in her book *The Body Multiple*.

This is even in the case of aboriginal knowledge, for which there is international and national legislation requiring scientists to work with aboriginal knowledge practitioners. In large part this hierarchy can be explained in terms of power differentials. Yet, as I argue here, resistance to accept other ways of knowing runs deeper than just this; resistance can be also traced to the structure of modern mass society relative to the particulars of ways of knowing. Specifically, scientific ways of knowing are particularly well suited to mass democratic society in a way that other ways of knowing may not be.

The Knowing is in the Doing

The first time I went clam digging I dug a lot but found few clams. This was not because there were no clams on the beach; the experienced Kwakwaka'wakw diggers found clams on almost every turn of their clam fork. It was only me who had difficulties. I was obviously doing something wrong. The question was what. I had two main challenges when digging for clams: (i) figuring out where to dig, and (ii) figuring out how deep to dig. If I could not find clams at a particular spot I had to make a choice as to whether to stay at that spot or move. Each option had costs. Staying in one spot could be a waste of time, as there may not be any more clams at that spot. Moving to a different spot was time-consuming as it required moving yourself, your raingear (which you have inevitably taken off because, even if it is raining, you are too hot from digging), your clam bucket(s) (which are heavy if you *have* found clams), your lantern (because you are digging at night), your digging fork, and perhaps an additional bucket that you have flipped upside down to use as a lantern stand. Moreover, as I soon found out, moving to

a new spot was no guarantee of finding clams. Digging deeper could also be a waste of time, though, if I picked the wrong spot on the beach to dig in the first place.

Initially, my decisions about whether to dig deeper or to move were based on whether my pitch fork hit rock when I jabbed it into the sand. Beaches in the Broughton Archipelago are extremely rocky, so rocks were an expected feature under the sand. Having dug up several rocks (with considerable effort), I decided it was too tiring and too time consuming to spend my time digging up rocks. I therefore made it a rule to always move to a new spot after hitting a rock. It took me a while to figure out my rock-avoidance rule was not necessarily a good strategy, though. On a few occasions I discovered that the “rocks” I hit were actually clams—they were just big clams that were solid enough that they *felt* like rock. The problem then was figuring out how to tell the difference between hitting a rock and hitting a clam.

After a while, I learned to dig clams of about 4 or 5 inches in diameter without (always) spearing them and without leaving them buried. I had to modify my rule in the process, though, to being “hit bedrock, move on.” It took me a while to have my second revelation about “rocks.” What with it being dark and seawater rushing into the holes I dug in the sand, it took me a while to realize that the “bedrock” was actually a layer of butter clams clustered together at the same depth under the sand. The representamen I had been using to indicate that I should give up digging at a particular spot was actually a representamen that I had found what I was looking for!

I start with this story about my initial learning process for how to dig clams as it captures the main point I want to make in this section: that knowing is deeply associated

with doing.¹²⁹ In this case, my experiences were what helped me form a cognitive/embodied model of where and how to dig for butter clams. As this story also shows, though, actions are not the only means by which I learned to dig for clams. Instead, it was an iterative process in which cognition and action were inextricably linked: my doing led to the development of new ideas, which in turn led to the development of new practices, which led to my mentally reviewing those practices. In the words of Keller and Keller in their discussion of blacksmithing (1993: 127): “Action has an emergent quality, which results from the continual feedback from external events to internal representations and from internal representations back to enactment.”

In chapter 4 I described how the representamens used for assessing abundance of clams on a beach differed between the First Nations and the DFO biologists. The representamens were described as selections made by the knowledge practitioners. Representamen are more than just selected features from the environment a knowledge practitioner passively observes or measures, though; representamens incorporate both selection *and* active engagement so as to perceive, record or measure it. In other words, representamens are products produced via interaction between the observer/measurer and the object; representamens include or incorporate practices.

¹²⁹ It perhaps sounds colloquial to talk in terms of “doing” (and “doings”) instead of *action* or *behavior*. I prefer “doing” for one simple reason, though; one need not be active to be doing something. For example, during a recent encounter I had with a shy raccoon family I decided the best thing for me to *do* was keep still. At first the family was unaware of my presence. Eventually, I was detected. I remained still however, and even though all their subsequent activities were geared around my presence (e.g. they hid in a short isolated tree, then eventually descended the tree and cautiously picked their way across the patio so as to escape into a forested area), my stillness likely prevented them from acting more defensively or running as quickly as they could, thus allowing me to watch them for a longer period of time. In this situation I was not taking any particular action. In fact, it would be better to say that I took no action or as little action as possible. Yet in *doing* what I did, I could clearly answer someone’s question: what did you *do* when you encountered the raccoon family? I sat still and watched.

Science studies scholars have long argued that what becomes known is mitigated by practices. In fact, “doing” is a central aspect of what many science studies scholars write about. For example, Lynch, Livingston and Garfinkel (1980), citing research by Friedrich (1982), describe how the actions taken in chemistry experiments highly influence the outcomes of research. They describe how things can go wrong in an experiment and the difficulty in determining whether and if it did go wrong—whether an outcome be due to “fact” or an artifact of the experiment. If artifact, the question needs to be answered of what went wrong and why. This procedure is made more difficult in the context of developing new techniques and exploratory research where there is no clear way to know what a “correct value” looks like.

In this depiction of laboratory work, it is clear that the researchers are not only interacting with but manipulating the empirical world and that their representamens are not mere selections but the result of an interaction. Laboratory ways of knowing are clearly anchored in doings. Knorr-Cetina (1999) pushes this point even further by reminding us that many of the materials used in lab experiments are not “pure” objects that occur in Nature but creations of the lab. For example, the mice and flies used in biological research are specially bred for genetic consistency. This additional layer of interaction anchors their knowing further in their doings.

It is easy to see how the contaminant ecologists fit into this type of analysis: through a number of transformative processes they transformed living clams into a fluid so as to achieve their desired representamens. The question is whether the same holds for other knowledge practitioners’ representamens; how much are these representamens products or constructions as opposed to selections? A quick examination of the First

Nations' clam diggers—the least manipulative of all the practitioners with whom I worked—show that their representamens, too, are the result of their interacting with their subject matter. The diggers walked on the beach and, in doing so, figured out just how muddy the sediment was. A beach was too muddy if they walked to the middle of the beach and sunk in “muck” up to their ankle. They dig for clams, and, in the process, potentially encounter a number of other things that need to be moved while digging. In previous years they had not encountered mussels while digging for clams but, in recent years on some beaches, the holdfasts of mussels had been found attached to buried clams. The diggers noticed this change as they were now required to physically remove the mussel holdfasts from the clams whereas they never had to previously. Those who only dig for little necks and/or manilas need only dig a maximum of 4 inches into the sediment. This is too shallow for butter and horse clams to live. As a consequence, only those who dig for butter clams will encounter and therefore observe butter and horse clams. Similarly, it is by physically digging at different spots on the beach that the diggers observe how clam abundance differs from one location to another. In all these cases, their representamens involved active “doings.”¹³⁰

Each of the “doings” of representamens is not conducted in isolation from cognition, though; selection and interaction are not without their cognitive counterpart. As many sociologists have argued, we use theories to “see.” As Zerubavel describes it (1997), our minds are socialized. We learn cognitive norms or rules that affect and constrain the way we think. For example, we learn conventions from others regarding what types of things are “important” when relating the day's events, what types of things

¹³⁰ While all practitioners' representamens are the result of an interaction with the empirical, it is also true that the scientists, particularly the contaminant biologists, processed their representamens to a much larger extent than did the First Nations.

are similar (e.g. wine and beer but not wine and grape juice) or different (e.g. mittens and socks), and what to focus on or attend to (e.g. people's eyes but not their elbows). These not only influence how we discuss or analyze a situation but they act as lenses for perceiving situations.

Similar discussions of mind exist in science studies. For example, Hanson (1958) strongly argues that the theories we bring to a situation highly influence what we observe. In Hanson's well known introductory chapter to *Patterns of Discovery*, he provides examples illustrating how the way in which one perceives is not inherent to the object, but born from the theory one employs while viewing it. In one example Hanson describes two scientists looking through a microscope at a slide preparation. On the slide, one of the scientists sees a mess—the contents of a cell artificially distorted by inadequate laboratory staining techniques—the other, an organ or component of the cell, a 'Golgi body.' Both observe something, but the significance of what they observe drastically differs.

In another example, Hanson conducts an imaginary contrast between the 17th Century historical figures of Tycho Brahe and Johannes Kepler who, when looking at the sun setting on the horizon, either see the Earth revolving around the sun (Kepler) or the sun revolving around the Earth (Tycho). Hanson argues that people observe the same (ambiguous) empirical phenomenon as being different – how Tycho and Kepler interpret the sunset depends on the hypotheses, theories, meaning, significance, or cognitive lenses they associate with the phenomenon. As Hanson argues, Tycho and Kepler may draw the same diagram to show a sunrise, but even the way in which they see the diagram will differ in the sense that when they “see” the sun they are also, simultaneously, “seeing”

things like the curve of the Earth, “the horizon dipping or turning away, from our fixed star” (*ibid*: 23). In other words, they are also “seeing” what they *know* (or believe) about the sun and the Earth in relation to it, not just lines on the page or the object in the sky. Hanson argues their seeing is not interpretation in that the acts are not distinct but simultaneous—seeing involves interpretation. Put differently, seeing incorporates knowing. All seeing involves interpretation, thus what is seen may be “seen” differently.¹³¹

Hanson’s examples illustrate how viewers are tied to a particular perspective; to “see” we employ working theories or, in Zerubavel’s terms, mental lenses. What Hanson does not emphasize but is also of central importance is that the viewers are actively engaged in *doing*. The viewers’ mental lenses did not spontaneously generate but were grounded in some form of direct or indirect experience. Kepler was privy to different, more accurate data (a series of observations recorded over a number of years by Galileo Galilei) than was Brahe and, for a number of reasons, had come to the conclusion that the Earth revolved around the sun. We know less history in the case of the two scientists looking through the microscope, but one can easily imagine that the biologist claiming the slide was just a poor preparation had considerable previous experience with the preparation of slides and the difficulties involved. It is also not difficult to imagine the other biologist, the one who “saw” the golgi body, to perhaps have had considerable experience looking through a number of slides in which he thought he saw more than just a problem with staining technique but a distinct shape that was relatively consistent

¹³¹ In both cases, the sensory apparatus employed are eyes, but any other number of senses or forms of interaction (and combinations therein) could have been employed.

across all slides. By including action and time into the mix we can see how mental lenses are more than fixed instruments but contextualized, evolving cognitive tools.

Different mental lenses employed by the clam knowledge practitioners regarding the presence and abundance of clams were also rooted in the practitioners' previous interactions with clams and beaches over time. A number of Nuu-chah-nulth and Kwakwaka'wakw diggers claim that clams move on the beach and from one beach to another. There are several reasons for this. Some talked about how beaches can change from one season to the next or even within a season. Tides and storms move sediment from one area on a beach to another or off the beach entirely. One digger told me how she watched a gravel point be completely swept away by winter storms and a new point develop in subsequent months.¹³² As some diggers argue, if sediment is moving, then the clams must be moving, too. Some also say that clams move to different areas on the beach and to different beaches of their own free will. If something happens to the beaches or the water, making the beach a less desirable habitat, the clams may choose to move somewhere else. Their evidence of this is based on their experiences visiting a beach one year and finding no clams but a large number of clams the following year, and the reverse—lots on clams on a beach one year and none the next. They also described experiences in which they returned to a beach three or four months later to dig in the same spots as they had previously and finding as many adult or legal-sized clams as there were the last time they dug—despite their having removed all the legal-sized clams they found the first time they were there.

¹³² This clam digger was actually not Kwakwaka'wakw or Nuu-chah-nulth. She originates from a different First Nations group that lives south and west of the other two Nations.

Their choice of actions and the experiences they encountered when acting led to them forming their particular mental lenses for interpreting the presence and absence of clams. They learned as they “bumped up against” the empirical world. What one bumps up against, in what manner one bumps up against it, and the mental lenses one employs while bumping up against it shape what one comes to know, which in turn influence how one continues to act.

It is interesting to note that the DFO biologists interpreted this same situation of clam abundance at given locations differently. The biologists claim that clam larvae settle in one spot where they mature and remain for the duration of their adult lives; that the only movement clams are involved in is vertical movement, up and down in the sand. Their response to why First Nations diggers find more clams when returning to the same spot on a beach in subsequent months is because the First Nations are really just digging in spots they had not dug before and therefore seeing clams they had not previously seen (1972).

I can not make any definitive claim one way or the other as to whether or how much clams move.¹³³ I can speculate, however, as to why the DFO biologists’ mental lens differed from many First Nations’ diggers. To begin with, the DFO biologists (or any biologists with whom they communicate, for that matter) have not done research on the horizontal movement of clams. When they study beaches, the biologists study multiple beaches and typically use research assistants or volunteers to dig for clams on a

¹³³ I *can* say that shellfish aquaculturalists, like the First Nations, claim clams move horizontally as well as vertically. They claim they are losing clams from their shellfish tenured areas to surrounding areas, and thus losing harvesting rights over their clams. Some of these aquaculturalists have asked an academic biologist at a nearby university to scientifically verify their claim to help them in their attempts to get the DFO to alter the structure of aquaculturalists’ harvesting rights (personal communication, Stephanie Duff 2007).

beach. As a consequence, they do not consistently spend time at any one beach. This limits the amount of observations they can make of small, dynamic changes in beach formation. Moreover, when they do dig on a beach they do not pay much attention to the locations where they dig as quadrats are randomly selected. This means they have little experience comparing the amount of clams in one spot over time. Likewise, the morphology of a clam (studied in great detail by biologists) is said to be inconsistent with horizontal movement. Reinforcing their views is their interest in repeatable measures, like using the same boundaries in subsequent surveys, which perhaps encourages them to view beaches as relatively stable. The way in which they interact with their subject matter, the length and timing of those interactions, the breadth of their gaze, and the tools and techniques they use exposed the DFO biologists to different empirical stimuli than those experienced by the First Nations diggers and, possibly, explains why they have different mental lenses.¹³⁴

As Kuhn (1970) and Barnes (1982) suggest, the relationship between socially-mediated cognition and doings can be more subtle than I have already suggested. Category recognition is arguably one of the most basic forms of cognition in that it is identification of what “is,” either analytically or empirically; it is only after we recognize categories that we can see relationships between categories and begin to interpret situations. Categories are typically described as cognitive structures (e.g. Zerubavel 1996; Murphy 2002) but they involve more than just mental activity; they also involve

¹³⁴ This explanation for why the two sets of knowledge practitioners employ different mental lenses for this empirical situation is consistent with what cognitive psychologists arguing for situated cognition are saying: that we learn in contexts and by doing (Lave 1991). To figure out what experts know we then have to look towards what they do or did previously—find out which empirical phenomena they were exposed to, when they were exposed to it, for how long, what equipment they used to interact with it and who they talked to.

physical “doings.” Individuals come to mentally associate objects with categories through a process. Specifically, the adoption of categories occurs through a process of ostension—learning through demonstration, illustration, and by pointing things out. While being guided by an authority figure, the learner learns to identify similarities and differences among representative examples and to associate particular sets of characteristics with a given category.

This process is captured well in a story first told by Thomas Kuhn and repeated by Barnes (1982) in which a father teaches his child how to distinguish among three types of birds: ducks, geese and swans. While on a walk, the father points to the three types of birds and calls them by name. As they walk further, the child tries to identify new birds they encounter as ‘swan,’ ‘goose’ or ‘duck,’ and the father confirms whether the child has identified the birds correctly. If the child makes an incorrect identification, the father corrects the child. After repeated instances such as these, the child masters identification of the three categories. When the child makes subsequent observations she does so by cataloguing these observations and insights by the type of bird, allowing her to develop an understanding for each category. As Barnes and Kuhn argue, all categories we employ can be traced back to some form of ostensive learning—learning through a series of guided doings.

As this story shows, there is a third element of importance to category formation. Cognition and activity do not exist in social isolation. Central to the child’s learning process about swans, geese and ducks is the role played by her father. He is the authority figure who guides her through the ostensive learning process. His authority comes from being a representamen of a larger thought community; he provides the child with training

consistent with the practices and thoughts of others.¹³⁵ Thus, albeit indirectly, it is the thought community that guides how the child comes to order her perceptions (*ibid*: 25).

As has been seen in previous chapters, thought communities play a major role in practitioners' ways of knowing. In this case, where knowing is through doing *as combined with cognition*, thought communities are important in several regards. As already suggested, the way in which one "bumps up against" the empirical is not random or haphazard. As with the swan story, the practitioners' interactions with the empirical were guided by existing ideas held by others in their thought community. But communities also play another important role. What we do is also often learned from a community and what we do determines the empirical phenomena to which we will be exposed. This can be in terms of preferences for types of tools and methods of detection. For example, the First Nations diggers have a preference for using their body as an instrument. The Nuu-chah-nulth diggers in particular explicitly employ all their senses when working with clams. I have already described how some smell the beach to detect the presence of clams. Diggers also described the smell of "dying" clams versus those that were healthy and the smell of certain beaches. Diggers described how some clams have a better taste than others depending on which beach they originated from. Diggers also talked about using their hands to try to swivel the two halves of the clam shell apart or noting the texture of "thick green slime". In contrast, both the contaminant ecologists and the DFO biologists—consistent with others in their respective communities—preferred to use tools and machines, such as electronic calipers and the gas chromatography mass spectrometry machine. These tools created capabilities for the

¹³⁵ Barnes argues that all learning, including that of verbal description and abstract categories, is based on initial learning through similarity relations transmitted by practical demonstration.

practitioner that extended beyond what was available to him through his senses, either in terms of precision or ability, creating a particular cultural style of interaction between the practitioner and the object.

Laws and institutional rules can also impact that to which a practitioner is exposed. Which beaches a knowledge practitioner visits can determine which species, how many clams, the degree to which the clams have been exposed to fish farm waste, the seasonal condition of the clams, or the type of sediment on a beach to which the practitioner will (potentially) be exposed. Some beaches are not accessible by law or institutional rules and who can access what beach can differ by one's social position. For example, the DFO stipulates that certain beaches are closed to commercial clam harvesters during the regular clam digging seasons, and all beaches are closed to commercial digging in the off-season. The Nuu-chah-nulth tribal office has also closed some beaches from commercial diggers so that only those who dig for home use clams visit these beaches. The DFO biologists were not free to choose any beach to be included in their study, either. They, too, were restricted by DFO regulations in the sense that only beaches classified as depuration-harvest beaches were allowed in their selection pool. The result was that one practitioner may have experience with summer clams that had been exposed to considerable farm waste whereas another practitioner rarely experienced such clams, instead being much more familiar with winter clams from beaches at a minimal distance from fish farms. In all these cases, institutional rules restricted, shaped, limited or guided the way in which the practitioners interacted with the empirical world and, as a consequence, that which they "bumped up against."

But exposure alone does not dictate ideas. For example, Kwakwaka'wakw and Nuuchah-nulth diggers undertook similar activities in a similar environment, yet they possessed somewhat different mental lenses. For example, both talked about vertical layers of clams under the sand. Only the Nuuchah-nulth diggers associated layers of clams with clam health, though. They claimed the layers of clams were of different ages, and for the clams to be healthy the older clams had to be removed so that the younger clams could grow. Otherwise, they said, the clams would start to die off. The Kwakwaka'wakw diggers, in contrast, did not associate layers of clams with clam health. What they talked about was turning over the beach, much in the same way as a farmer would turn over soil to loosen it up and oxygenate it. Each thus had a distinct view of the clam layers.

The difference in their lenses can not be explained in terms of how the diggers interacted with the clams and beaches as their digging styles were very similar. The difference could perhaps result from regional differences in ecology. A more likely explanation, however, is that they developed different ideas (or models) about the relationship between clam distribution and clam health. Experience ignites the practitioners' imagination but does not dictate the location to which the imagination travels.

Up to this point I have focused on the relationship between doing, thought communities, and cognition as related to classifications and specific conclusions. At this point I want to broaden the discussion to include the larger tradition in which the practitioners worked. Each of the knowledge practitioners tended to limit himself to a relatively consistent pattern of interaction with the empirical, whether it be in terms of

clam surveys, clam digging, or sampling and tissue analysis. All four sets of practitioners with whom I worked were consistent in terms of the types of tasks in which they were involved. For example, the First Nations clam diggers organized themselves for trip to clam beaches, went to the beaches and dug, and brought the clams to market. The DFO biologists attended meetings, wrote reports, read reports, and conducted formal and informal surveys of intertidal species. The contaminant ecologists attended meetings, developed research proposals, conducted research related to contaminants, wrote reports and journal articles. Their work involved more variation than the First Nations' work but was still relatively consistent.

Moreover, the basic thought patterns (mental lenses, classifications, ontological and epistemological assumptions about the universe) remained relatively constant within each set of knowledge practitioners. For example, the contaminant ecologists consistently employ the idea of *systems* wherever they work, the DFO biologists consistently think in terms of density, populations, and abundance and the Kwakwaka'wakw consistently think in terms of hot spots and aim to generate embodied knowledge.

While they did learn, the new pieces of information the practitioners added to their repertoire of clam knowledge was relatively small; one could think of their new knowledge as being added on to an existing structure as opposed to fundamentally altering that which they already knew. This seemed largely due to the reciprocal nature of their ways of knowing—their interests and mental lenses reinforced their ways of doing and their ways of doing reinforced their mental lenses. The consistent use of similar ways of doing resulted in exposure to similar types of empirical experiences; they

continued to “bump up against” the empirical in a way similar to how they had previously. For example, in walking the beach the DFO scientists measured the beach, divided it into sections and then set out stakes to demarcate sections and quadrat locations. The diggers, on the other hand, walked the beach looking to find ridges, holes in the sand and rocks. Each had their own distinct walking pattern that was tightly tied to their respective mental lenses. Their relationship to the empirical remained relatively stable, and thus so did the experiences they gained from those interactions.

As these case studies suggest, one does not simply “see” what is there, but constructs a lens for seeing through a series of dialectical interactions and interpretations and, in doing so, learns about clams. Each practitioners’ practices, cognitive style, interests and set of social relations bound him to a given realm of empirical experience which, in consequence, shapes his ecological imagination. Their repertoire of doings and mental lenses underpinned a worldview in which they largely remained. What became known about clams need not reflect clams but something about the entire relationship between clams and the practitioners studying them.

This is not to say that knowledge practitioners are forever rooted in one direction of knowledge acquisition. The PI on the contaminant ecology team was a good example of this. He was the only senior researcher on the project to visit the clam beaches. I was part of the clam collection team for the first two trips but could not join them for later trips. In these first few trips the PI had stayed near the boat to clean and sort the clams we brought to him. In my absence, the PI began to do some of the clam digging himself. Like me, he initially found it difficult to find clams. Consequently, he decided to talk with and watch the native clam diggers he had hired to see where and how they found

clams. In my final interview with him, he told me about his experience learning how to dig for clams:

PI: Yah, I love knowing how to dig clams. It's fun. And I have a sense for where to find them. Like when I say I know how to dig, I know how to use the rake and when I go to a beach and I know more-or-less where to look on a beach to find them.

CM: Why? How do you know?

PI: From talking to the clam diggers when we were digging.

CM: What did they tell you?

PI: The little necks like to be a little higher up and they like a bit of sun. So we looked a little higher up and we found scads of little necks. And you know, the butters are the squirters and you look in little depressions and things where its not too rocky.

CM: Who told you this?

PI: This year we were out digging with...what was his name...John

CM: So he said in the depressions and such to look?

PI: Well no, but you know, when we would go looking I would-this is something I would just sort out for myself. When we come to a beach, I would look around the beach, I would look where he would head to, and I would say "OK, he's going to the little sort of flat areas that are, you know, not on slopes but on little flat areas, like little raised flat areas, where there's a bit of sand maybe..."

And a little bit later:

PI: Yeah, well some of it I sorted out for myself by just trying it, so that was cool. Just digging in a bunch of places. And I would figure out where I got clams and where I got nothing.

CM: OK, so now you know where to find clams, so that is good. So what else do you feel you know about clams after this?

PI: I don't know. Just, uh.....I don't know. A lot more. I really knew very little about clams before. Now I have a sense for how they move and what they do, just from watching them, just from digging them up.

CM: Yah?

PI: And, you know, throw them in a pile and watch them try to bury themselves again. You know, and watching their siphons, and having opened them up and looked inside. And a better sense of how they are put together. You know, I never really paid much attention to clams before.

From these two segments of interview it can be seen how the PI's involvement in a new type of behavior, namely clam digging (as opposed to just supervising the collection of clam tissue from a beach), resulted in his development of new ideas about clams. The new activity captured not just his interest but ignited his imagination. In fact, while some of the observations he made about how to find clams were consistent with what I had heard other clam diggers talk about and do, several of his comments were unique. Trying new activities allowed the PI to break out of his existing view to develop a new set of mental lenses and a new set of empirical experiences. It was by trying something new—a new activity—that the PI acquired new mental lenses and was exposed to new empirical experiences, all of which occurred over time and space. From this story and others it becomes clearer how ways of knowing are grounded in ways of doing.

Epistemic Hierarchies as Rooted in Modernity

Throughout this dissertation I have described a variety of ways of knowing. At points I have talked about how one set of practitioners viewed other practitioners' ways of knowing, like when discussing Kwakwaka'wakw and DFO biologists' representaments for clam abundance. What I have not explicitly discussed, however, is how outsiders treat these respective ways of knowing. It will likely come to no surprise to the reader to learn, though, that these ways of knowing are frequently treated unequally, with the scientific ways of knowing typically treated with more esteem than the indigenous ways

of knowing. For example, the contaminant ecologists were asked by a Kwakwaka'wakw fisheries organization to study the relationship between fish farms and clam health because the Provincial government representatives had told them that their knowledge was not reliable and only scientific studies would convince them that fish farms had a negative impact on clams. Without question, science has become the dominant empirical knowledge system in our modern world. In fact, some have gone as far as to say that science is the modern religion. As one student of science studies wrote:

“Science” often stands metonymically for credibility, for legitimate knowledge, for reliable and useful predictions, for a trustable reality: it commands assent in public debate. If “science” says so, we are more often than not inclined to believe it or act on it—and to prefer it over claims lacking this epistemic seal of approval.

(Gieryn 1999: 1)

The dominant status of science over aboriginal ways of knowing has a long history. The ethnocentric views of Europeans towards alternative empirical knowledge are historically contiguous with the development of Modern science and are closely tied to colonization and imperial power struggles. Europeans, when entering the native lands of what we now call North, Central and South America, Africa, Asia, Oceania, and other regions, were colonizers who considered their worldviews far superior to those of those they came to dominate. Their goal was to conquer foreign lands and, while there, perhaps save the savage, soulless natives from their pagan and barbarian ways. European explorers were largely closed to the possibility that natives held their own sophisticated understandings of nature and the universe. This was despite explorers and settlers often relying on native knowledge to safely navigate the lands and waters and to survive cold, barren winters.

To this day, indigenous and other alternative knowledge practitioners often find themselves frustrated and powerless. For some it means not being given the respect they deserve, the authority to practice their trade, or the right to participate in legitimating institutions. For others, it means an inability to manage their lands and seas and the gradual death of their cultures (e.g. Robinson and Munungguritj 2001). For example, the Kwakwaka'wakw diggers talk about how they are no longer the masters of their own territory and how this has negatively impacted not only the environment but their culture and way of life. These and other indigenous knowledge practitioners have often had to stand idly by while scientists and policy-makers dictate the “proper” way to manage lands, diagnose the sick and forecast environmental change.

In Canada there is presently a new federal regulation, Bill C-51, proposed which would require all natural medicine and herbs to pass scientific testing before being approved for sale in Canada (Ministry of Health 2008). The biggest community lobbying against the bill is the Chinese community, who would no longer be able to legally import much of their traditional medicine. In proposing such a bill it can be seen how science is placed at the top of an epistemic hierarchy. Not unsurprisingly, arguments against the bill are being voiced in terms of the validity of alternative ways of knowing.

Specifically, Peter Wood, president of the Traditional Chinese Medicine Association of B.C., is reported saying that,

Traditional medicine focuses on the whole person, rather than a single symptom, to determine a course of treatment that will be effective. If a herb is scientifically tested for its effect on one symptom across a large cross-section of people [the typical scientific approach in these types of inquiries], it may not prove effective for everyone. Traditional medicine can not be tested the same way as pharmaceuticals.

(Luymes 2008: A10)

As the quote suggests, the use of averages and other forms of statistics in which the practitioner searches for central tendencies and statistical differences among comparable categories, as discussed in chapter 5, is but one way of coming to know. Other ways of knowing can take different approaches, such as examining how a change fits within the context of a whole or among interrelated subgroups.

The First Nations clam diggers find themselves in a somewhat different position than the Chinese doctors, however. Instead of federal regulations working in opposition to them, international and federal regulations are actually in their favour. The United Nations' Conference on Environment and Development (UNCED), also known as the Earth Summit, in Rio de Janeiro in 1992 created two documents that, in part, stipulated a particular type of relationship between federal governments and indigenous peoples. The Conference recommended involving indigenous people at national and local levels in resource management, conservation strategies and planning processes (article 26p), and developed national governmental arrangements for consultation with indigenous peoples to reflect indigenous knowledge and other knowledge in resource management, conservation and development programs (article 26q) (Flahr 2002). The second document, the UN Convention on Biological Diversity (CBD), which is binding under international law for those countries who signed and ratified it, states:

Each contracting Party shall, as far as possible and as appropriate:
Subject to national legislation, respect, preserve and maintain knowledge, innovations and practices of indigenous and local communities embodying traditional lifestyles relevant for the conservation and sustainable use of biological diversity and promote their wider application with the approval and involvement of the holders of such knowledge, innovations and practices and encourage the equitable sharing of the benefits arising from the utilization of such knowledge innovations and practices.

(Article 8(j): Traditional Knowledge, Innovations and Practices, from the United Nation's Convention on Biological Diversity)

In other words, involving indigenous knowledge practitioners in natural resource management decisions is not only recommended by the United Nations but in some cases is legally required.

In Canada, scientists and policy makers have additional legal motivation for consulting indigenous peoples. Although the Indian Act, drawn up by the Federal government without input from First Nations in 1876, makes it clear that the Canadian government is in charge of overseeing indigenous interests, a series of relatively recent court cases established that Canada's indigenous people have the right to choose how land to which they have Aboriginal title can be used (Flahr 2002:31). This means the various forms of Canadian government are now legally required to consult with indigenous peoples *before* implementing new legislation, policies and management regimes that impact Aboriginal title. Despite the present lack of any official federal policy (which has led to considerable differences among departments and agencies in terms of how they have incorporated/addressed consultation issues), it is at least commonly acknowledged in Canada that indigenous experts should be consulted, in one fashion or another. Similar legal structures have emerged in other countries as well, including Australia and New Zealand.¹³⁶

¹³⁶ Like with other forms of social inequality, legislation is not enough to eliminate prejudice. Even with political incentive, the worlds of alternative knowledge and science still remain apart. To date, there has been a proliferation of conferences, meetings, articles, and books describing indigenous knowledge and introducing indigenous peoples to policy-makers and scientists alike, but with low rates of satisfaction on behalf of both indigenous experts and scientists (Nadasdy 2000). Backroom conversations documented by Nadasdy, not unlike ones I have heard myself, suggest that many indigenous people involved in consultation feel that scientists and resource managers are only marginally interested in what they have to say, and many scientists are still at a loss as to what is indigenous knowledge, with some suspecting that it is "a political ploy invented by aboriginal people to wrest control of wildlife from "qualified" scientific managers" (*ibid*: 3).

A number of journal articles have been written explaining the gap between indigenous and scientific knowledge and proposing ways in which to overcome the inequality. The main explanation provided is that differences between the two types of knowledge, both in terms of form and the process by

Despite this legislation, a wide gap still remains between scientists and indigenous knowledge practitioners. A variety of reasons have been proposed as to why this gap exists, including discussions about epistemology and power relations (Nadasdy 2000). These discussions largely focus on why the scientists hesitate or resist accepting their indigenous counterparts. I would like to suggest an alternative answer—one that looks at scientific and indigenous ways of knowing in a larger context.

In chapter 4 I described how the interests and social relations in which the various knowledge practitioners were embedded shaped their ways of knowing about clams. Specifically, I focused on the contaminant ecologists and Kwakwaka'wakw clam diggers' as aiming to create public and private knowledge, respectively. One of the pivotal aspects of knowledge construction that increased the likelihood that others would accept the contaminant ecologists' knowledge claims was if the scientists had used standardized methods and terminology to generate their claims. This meant the contaminant ecologists frequently employed conventional practices and standardized procedures that others in their field had employed. Standardization also involved the use of coordinated rhetoric, which improved their ability to communicate with their peers, thus increasing the chance that their claims will be accepted. Moreover, it allowed the practitioners to reduce the amount of variation between their and others' research, thus making the ecologists' knowledge claims comparable to those of others.

which claims were generated, are too big to overcome, with the result that scientists do not know how to incorporate indigenous expert knowledge into what they know. Proposed solutions include creating databases with indigenous knowledge expressed in terms utilizable by scientists, creating a mental and rhetorical space for alternative knowledge (Turnbull 2000) and; acknowledging the power inequalities and examining how that has impeded current efforts (Nadasdy 2000). As Nadasdy and others have pointed out, the database approach is problematic at best; it decontextualizes and segments indigenous knowledge into "datapoints" which can then be used or ignored at the wont of scientist. The latter two approaches are useful but not sufficient. No clear answer (or combination of answers) presently exists in regards to how other ways of knowing can increase their epistemic authority relative to science.

Like the contaminant ecologists, the DFO biologists were also concerned with using standardized, replicable procedures that would be acceptable to others. In assessing the abundance of clams on a beach they used methods of random selection and, in the lab and on the computer, standardized procedures and formulas for counting, measuring and weighing the clams and producing figures which any trained surveyor could reproduce in a comparable manner.

For the DFO biologists, acceptance implied even more than endorsement by their peers—it meant their policy recommendations were bureaucratically safe from reproach. As their conclusions were implemented as policy guidelines for fisheries management, their research had very real implications for clam diggers and clam beaches. This put the DFO biologists, and the DFO more broadly, in a position of being accountable to the Canadian public. The review process was as much about safeguarding their management decisions so as to protect their actions as it was about coming to understand clams. If everyone involved in the review came to a consensus that the research conclusion was the best that could be done, given the circumstances, then they could feel confident in taking management action.

The rationale behind Bill C-51 is comparable to the concerns of the DFO; politicians argue that the Bill is for the safety of Canadians—that substances used for medical purposes should be regulated. In present Canadian society, the government is responsible for monitoring, regulating and promoting the safety of its citizens. Politicians have already created numerous regulations to increase workplace safety, to protect children from harm, to discourage discrimination based on gender, race, disability, or

sexual preference, and to regulate potentially harmful drugs and other substances. This bill would be just one more step in this same direction.

This issue of accountability is likely what creates a barrier between scientists and indigenous knowledge practitioners involved in developing natural resource management policies. Like the DFO biologists, these scientists are acutely aware that their claims will be judged by whether they used acceptable methods. Their own biases aside, they are put in a difficult position when told to work with indigenous knowledge practitioners to develop policies that reflect both scientific and indigenous knowledge. For, as I found with the Nuu-chah-nulth and Kwakiult clam diggers, indigenous ways of knowing are often distinct from scientific ways of knowing (Berkes 1993; Barsh 2000). As this dissertation suggests, it is not just the methods of measurement that are distinct; their distinctiveness is also tied up in different sets of social relations, ontologies, and practices that results in knowledge that is not open to critical appraisal. The scientists are thus in a bureaucratic bind as they likely do not wish to be accountable for proposed policies based on foreign ways of knowing. The result is an epistemic hierarchy between scientists and indigenous knowledge practitioners due not just to the convictions of many scientists that science is superior but that science suits the current structural relations of bureaucratic, democratic society.

Yet, as the content of this dissertation suggests, just because some ways of knowing suit current political structures does not necessitate that other ways of knowing are inferior. In fact, in the same way that the PI from the contaminant ecology team benefited from working with the First Nations clam diggers for learning how to dig clams, all knowledge practitioners can gain from working with each other. Moreover, all

ways of knowing are arguably partial in that no one practitioner is likely to ever have a “complete” understanding of that which she studies. The relationship between fish farms and “dead” clam beaches is a good example of this. As described in chapter five, the Nuu-chah-nulth and contaminant ecologists both developed answers to the question about whether fish farms were negatively affecting clam beaches. In developing their answer, the contaminant ecologists were *not* able to identify a chemical with unique origins to fish farms that could be found in clams. Their back-up method for testing whether clams originating from beaches close to fish farms had higher levels of contaminants did not show as clear a pattern as they had hoped to find. What they could provide, however, was a list of precise figures that represented contaminant levels in various classes of clams. This was something the Nuu-chah-nulth could not make any statement about at all; they could not say whether the level of contaminants in clams had been altered by the presence of fish farms. What the Nuu-chah-nulth diggers could make a statement about was that some clam beaches were now “dead” and that these beaches were in close proximity to fish farms. This is also a partial account in that they can not explain how the fish farms and clam beaches are related. It is entirely plausible that contaminants were not the cause of dead beaches. Instead, their “death” could have been due to the high volume of silty material in the fish feed washing up on beaches and suffocating the clams, the vast quantities of additional food (nutrients) being dumped in the ocean having unpredictable implications for local food webs or a number of other reasons. The Nuu-chah-nulth diggers’ explanations were also partial. A thoughtful discussion among these practitioners (and perhaps the inclusion of additional practitioners with different suites of ways of knowing) could perhaps result in a better, more complete answer than either of

these knowledge practitioners came to during the course of my research. Or, even better—practitioners cross-training, learning about how each understands and interacts with the world so as to develop familiarity and appreciation of their respective ways of knowing.¹³⁷

¹³⁷ For authors making a similar argument see Murray, Neis and Palmer (forthcoming).

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Zerubavel, E. 1997. *Social Mindscapes: An Invitation to Cognitive Sociology*.
Cambridge, MA, Harvard University Press.

Curriculum Vita

Chantelle Marlor

EDUCATION

2009	Rutgers University, Ph.D., Sociology
2000	University of British Columbia, M.A., Sociology
1994	University of British Columbia, B.A., Sociology

EMPLOYMENT

2006-09	Instructor in the Sociology and Criminology Departments at Vancouver Island University (formerly Malaspina University-College), Nanaimo, British Columbia, Canada
2007	Instructor in Sociology and Anthropology at Simon Fraser University, Vancouver, British Columbia, Canada
2002-06	Instructor and Teaching Assistant in Sociology at Rutgers University, New Brunswick, New Jersey
2004-05	Research associate for Asit Mazumder, seafood consumption survey, University of Victoria, British Columbia, Canada
1999, 1994-96	Research Assistant for Dr. David Tindall, <i>Trees vs. Jobs</i> SSHRC Project, Anthropology and Sociology, University of British Columbia, Vancouver, Canada
1998 and 1999	Instructor at Red Crow Community College, Blood Reserve, Alberta
1998	Research Consultant for International Labour Organization, Geneva Office (Research Consultant, assisting indigenous peoples in the Philippines to develop their own development criteria)
1997-98	Research Consultant for Office of the Treaty Commissioner, Saskatchewan (Strategic and Evaluation Researcher)
1997-98	Research Assistant for Dianna Tindall, evaluation research consultant
1994-97	Research Assistant for Dr. Martha Foschi, various social psychology experiments, Anthropology and Sociology, UBC
1996	Research Assistant for Dr. Fiona Kay, <i>Gender and a Changing Legal Profession</i> , Anthropology and Sociology, UBC
1996-98	Teaching Assistant in the Department of Anthropology and Sociology at UBC
1995-96	Research Assistant for Dr. Neil Guppy, various Canadian demography and social inequality projects, Anthropology and Sociology, UBC
1995	Research Assistant for Forestry Curriculum Task Force, Forestry, UBC

PUBLICATIONS

- 2005 Marlor, C. and N. Eyding. *Clam Consumption and Harvester Surveys in Ahousaht, B.C.: Estimating Contaminant Exposure Levels and Perceptions of Contaminant Risk to Health*. Report for Ahousaht Band Council, University of Victoria, B.C. Aboriginal Fisheries Commission, Health Canada and Assembly of First Nations. 29 pages.
- 2004 Marlor, C. Retrenchment and Regeneration in Rural Newfoundland (book review). *Contemporary Sociology: A Journal of Reviews*, 33(6), 696-697.
- 2003 Barsh, R. and C. Marlor. Buffalo Driving and Blackfoot Science. *Human Ecology* 31(4), 571-593.
- 2000 Barsh, R and C. Marlor. Grounded, Experiential Teaching About Violence; Creating Change By Giving Students Voice And Power, *Violence Matters* the research magazine of the Harry Frank Guggenheim Foundation. 10 pages.
- 1999 Barsh, R. and C. Marlor. Alternative Paradigms: Law As Power, Law As Process, in *Law in Society: A Canadian Reader*, edited by Nick Larsen and Brian Burtch. Harcourt Brace. 1st and 2nd editions.
- 1999 Marlor, C., R. Barsh and L. Duhaylungsod. Ethnocentric about Ethnoscience: Further Comments on Nazarea et al., *Human Organization*, vol. 58, p 347.
- 1999 Marlor, C., R. Barsh and L. Duhaylungsod. Comment on 'Defining Indicators Which Make Sense to Local People: Intra-Cultural Variation in Perceptions of Natural Resources' (Virginia Nazarea et al.), *Human Organization* , vol. 58, p. 216-219.
- 1998 Barsh, R., L. Duhaylungsod and C. Marlor. Culturally-Specific Concepts and Indicators of 'Development' For Indigenous Peoples in the Philippines, report for the International Labour Office.
- 1997 Kay, F., N. Dautovich and C. Marlor. *Barriers and Opportunities Within Law: Women in a Changing Legal Profession*, a report for the Law Society of Upper Canada. 137 pages. (Principal data analyst, report co-writer)

CONFERENCE PAPERS

- 2007 Marlor, C. "Clams and Measurement: A Cross-Cultural Comparison of Experts Coming to Know," *Ways of Knowing, 2007 Annual Conference for Society for Social Studies of Science*. Oct 27, Montreal, Quebec, Canada.
- 2006 Marlor, C. "Cultural Models of Knowledge: A Comparison of Kwakwaka'wakw Clam Digger and Contaminant Ecologists' Ways of Knowing About Clams," Regular session in Sociology of Culture, panel on Fields and Practices of Knowledge Production, *American Sociological Association's 2006 Annual Conference* August 11 in Montreal, Quebec.

- 2006 Marlor, C. "What is new in...studies of epistemology," presented at the *Eastern Sociological Society's Annual Conference*, Feb. 25, Boston, MA.
- 1998 Barsh, R. and C. Marlor. "Renewal or Rhetoric? Treaty Renewal on the Prairies," presented at *Futures and Identities: Aboriginal Peoples in Canada*, a conference convened by the Association for Canadian Studies, June 1, Ottawa, ON.

INVITED PRESENTATIONS

- 2009 "Bureaucracy, Democracy and *Exclusion*: Why Indigenous Knowledge Holders Have a Hard Time Being Taken Seriously," January 30, University of Mary Washington, Fredericksburg, VI
- 2008 "Signs, Measures and Methods: Comparing Indigenous and Biologists' Ways of Knowing about Clams," December 17, University of Calgary, Calgary, Alberta, Canada
- 2008 "Counting Clams: Differences between Kwakwaka'wakw and Marine Biologists' Ways of Knowing," November 25, Willamette University, Salem, OR
- 2008 "Bureaucracy, Democracy and *Exclusion*: Why Indigenous Knowledge Holders Have a Hard Time Being Taken Seriously," November 21, California State University—LA, Los Angeles, CA
- 2007 "Digging for Numbers: Debates between First Nations and Biologists over Observation and Measurement," October 13, University of Victoria, Victoria, B.C., Canada