



## **Clayoquot Alliance Working Paper Series**

### **Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report**

Stuart Lee

Environment Canada

Email: *Stuart.Lee@ec.gc.ca*

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## Summary

“Public participation” is becoming a mantra these days, being trumpeted by both right and left wing spokespeople as solutions to issues especially dealing with resource management. Yet despite public input on many land-use decisions made by government, there are palpable problems with this practice. This paper will focus on one of these problems involved in issues which are predominantly framed as technical, such as resource management. The point of this paper is to firmly establish the problematic *cultural* aspects of these technical or scientific-based negotiations, then to propose a vocabulary of reconciliation based on research undertaken in the social studies of science. The analytical frame developed in the introductory material will then be applied. to the Clayoquot Sound Scientific Panel. Based on this perspective, research directions will be proposed and potential problems will be highlighted.

The paper is structured in two major sections. The first is a review of relevant concepts taken from the literature. This review has nine sections that progress from theoretical foundations to more elaborated treatment of the issues. It aims to create an account of “science-based” procedures that delineates their contingent, culturally embedded nature. The second section uses the notion of hybridity to structure an analysis of key aspects of the Clayoquot Sound Scientific Panel report and to comment on barriers and opportunities encountered in its implementation.

## Introduction

As happens all too frequently, procedures of knowing the world based on Western scientific/bureaucratic traditions are given preferred status over other ways of knowing the world. Science and scientific work, is often considered the legitimate conduit to “reality” where other ways are considered “tacit” “local”, and “culturally influenced.” This introduction works to create a discursive space<sup>1</sup> where the two different cultural perspectives (scientific, non-scientific) can come together and can mutually transform each other, in an atmosphere where some of the major political and cultural assumptions are made explicit.

The point of this introduction is to show how knowing the world is *always* a political enterprise, and to argue that we do well to take a reflexive approach to our knowing practices in order to allow new ones to emerge from our encounters with other cultures. Following this exposition of science as a cultural pursuit, we propose a vocabulary of cultural “hybridity” to discuss a broad approach toward moving forward on resource management issues.

## Theoretical Bases

The task of this theoretical section is to make many of the assumptions necessary for an appreciation of the notion of “hybrid object” seem natural. To do this, we work through a progression of nine theoretical sections, from philosophical foundations to explicit examples of “hybridity” in the literature. In the first section, two philosophers, Michel Serres, and Gilles Deleuze, are introduced who argue against the Modern project of systematization, determination and transcendence. As an alternative, they struggle to provide narratives of relation, dispersion,

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<sup>1</sup> “Discursive space” refers to a way of discussing something that provides room for certain accounts. What is counted as real and un real, and what is counted, period, are two important features of a discursive space.

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report locality, heterogeneity. Though their work is difficult and a full appreciation of its implications is beyond the scope of this work (and this author!), their ontological<sup>2</sup> assumptions are key to the success of many of the narratives that follow.

The next three sections (sections 2, 3 and 4) can be considered to be working out specific applications of this non-unitary, decentered ontology. In section two, the ideas proposed by the leading philosophical scholars in Science and Technology Studies (STS) are discussed. They emphasize the role that non-human objects and entities (“actors”) have in forming the social world, and the importance of social arrangements in determining how the non-human world is conceived and treated. Section three introduces the “practice theorists,” who emphasize the importance of communities of practice – local assemblages of people, tools, and landscapes – in creating work that describes or delimits reality. The theorists presented in section 4 emphasize the co-production of specialized languages (linguistic registers, speech genres) within and between these communities of practice. Language and language use is situated in local groups, and seen as inextricably related to what the group is doing, and therefore to its material surroundings as well as social “norms.” Thus a research program that links language, sociality and materiality is justified.

Next, section 5 summarizes a literature which focuses on a narrative analysis of language’s relation to environment and practice. This is given its own section partially because it is distinct theoretically from many of the others, and partially because it is a special case that is relevant to our concerns, that of disparate narratives regarding “the same thing” – a resource or a landscape.

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<sup>2</sup> “Ontology” is the strain of philosophy concerned with questions of being. Although they consider themselves commentators on science, many of the philosophers that are encountered in this work consider their project not an epistemological (what it means to know) one, but rather an ontological one. This seems natural if you take a social approach to learning, as learning and knowing become located in social lived worlds rather than solely cognitive realms.

In section 6, we encounter another way of working with narrative, this one a material perspective. This section summarizes some central texts about the importance of physical representations, such as graphs, drawings and so on, in the politics surrounding science and technology. As one of the major entities that are circulated in the cultural knowledge practice of Western science, representations have a key place in the theoretical approach taken by science and technology studies. Some cultural and political aspects will be elucidated.

The final three sections (sections 7, 8 and 9) now take the core theoretical approaches laid out in the first six and apply them to studies of science in society. The first is a brief overview of the role of science in the very important social process of standardization – the “enforcement of sameness” that allows works of large scale to happen. Standardization is an excellent site for elucidating social aspects of science because standardization processes are always deeply embedded in political ambitions. The next section, section 8, explores the role that science plays in reconciling or delineating social disputes. This topic will be treated in detail, examining the social mechanisms that come into play as people use science to marginalize certain groups’ voices, prove a point in court, or confront uncertainty. These are all key issues that come out in struggles to define, manage, and harvest natural resources, so their insights will be very useful in later analyses.

The final section reviews the use of the word “hybrid” in this literature. It appears in multiple contexts, and a discussion of the way it is used throughout the literature will be helpful in guiding us to a fruitful, informed and well connected use of the word in our analyses.

## ***I. A De-Centered Ontology***

The writings of the philosophers Michel Serres and Gilles Deleuze and Felix Guattari (Deleuze and Guattari wrote as co-authors) have proven pivotal in forming the style of research and research writing done by key thinkers in STS, particularly those from Europe and Britain. Both of these authors have huge bodies of work, and Deleuze particularly has influenced thinking in many different fields. Serres is less well known, but is coming to be recognized for his unique approach to the philosophy of complexity. In the following paragraphs, I focus on the aspects of their philosophy that have been taken up by STS researchers, emphasizing how these ideas can be helpful in developing better narratives about negotiated settlements to resource management.

But first, I want to emphasize the context for their work. They seek to challenge the “modern” notion of reality. By “modern” I mean the world view that the Enlightenment philosophers such as Bacon, Descartes, Newton and so on called modern. The notions they proposed about the world and about knowing the world are still central to the rhetoric of much of science, if not its practice. Features of the modern world that come under scrutiny include many of the standard assumptions articulated by practitioners of science. They include:

- The assumption of the possibility of omniscience – of determining an abstract reality, of obtaining a “god’s eye view”
- The drive for transcendence – discerning what is “behind” phenomenon, searching for “underlying structures”
- The creation of a-temporal models – systems that do not include irreversibility or temporal constraints

- Analysis, reductionism, determinism – the assumption that you can break a system down into its component parts, learn all there is to know about the components, and from that knowledge, know all there is to know about the system. Also included here is the assumption that you can know the “essential” parts of a system (for example, genes) and use them to predict outcomes of the system (what will happen to the organism).
- Unity – the use of “tree” structures in organizing knowledge, genealogies, systematics – implying that there was, or is, a single root, or a possibility of unitary ancestral entity.
- Purity – seeking to “distill” essential qualities from messy phenomena, as in social models
- Objectivity – that one can know about phenomena or entities independently of one’s subjectivity
- Scaling – principles learned about little things can be “scaled up” to big things
- Information, information transfer – information exists independently of its medium and its interlocutors – it is an entity that can be transferred in pure form.
- Mind/body dualism – the famous Cartesian dualism between thinking and sensing, putting thought primary.
- “readability” – one can “read” the “book of nature” and divine its secrets. Scientific works are directly about nature, they mirror reality.

The list can go on and on, but the way I see it is that a “modern” approach is one that seeks its true-ness or “ultimate reality” somewhere other than at the site of where things are actually



Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report going on. Reality is accorded to the immutable laws, social structures or economic markets, and (for example) the site where money is being changed is but one of many sites of *applications* of these laws. If the laws aren't working exactly right there, then there must be a problem with the site, or the researcher's work at the site. The laws are general and transcend any one site. To know the general is to master the particular. In the general case, contingencies, stresses either do not exist, or are represented in very simplified fashion.

In response to the epistemological and social problems with these modern stances toward reality, a number of writers in the last half of the twentieth century have proposed different ontological bases for confronting, organizing and accounting for the "real world." They have critiqued the modern project from numerous angles. Two, among many, have been especially influential in Science and Technology Studies.

My treatment of Deleuze is based entirely on my reading of the essay "Rhizome." In 1980 Gilles Deleuze and Felix Guattari, published a small essay named "Rhizome" in their collection "A Thousand Plateaus." The intellectual project which "Rhizome" sought to work out was a more accurate description of relations between things. Their essay listed two classically modern ways of looking at relations: the tree – as in genealogical, or biological or organizational –and the radicle-system or fascicular root, where there is still a unifying root, but no clear "top." Examples of the second form of ordering metaphor include James Joyce's writing, which "shatter[s] the unity of the word, even of language, only to posit a cyclic unity of the sentence, text or knowledge." (Deleuze and Guattari, 1987, 6). Unity here is in the cycle or the "big picture," which constrains multiplicity.

They propose the "rhizome" as a more appropriate metaphor. "The rhizome itself assumes very diverse forms, from ramified surface extension all directions to concretion into bulbs and

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report tubers... Burrows are [rhizomes] too, in all of their functions of shelter, supply, movement, evasion and breakout.” (Deleuze and Guattari, 1987, pp. 5-6). The rest of the essay is devoted to working out the metaphor of the rhizome. They propose and elaborate on six rhizomic principles: connection, heterogeneity, multiplicity and asignifying rupture (the ability to regenerate a new rhizome from fragments of the previous one – shattering the rhizome’s organization will not destroy it),<sup>3</sup> and its unamenability to tracing or having a deep underlying structure.

This essay relentlessly develops the rhizome metaphor, emphasizing growth, connections, transformations, modifications, multi-dimensionality, multi-functionality, territoriality and movement. “it [rhizome] is composed not of units but of dimensions, or rather directions in motion. It has neither beginning nor end, but always a middle (*milieu*) from which it grows and which it overflows.”(p. 21) “The rhizome is an acentred, non-hierarchical, nonsignifying system ... without an organizing memory or central automaton, defined solely by the circulation of states.”(p.??)

Important concepts also introduced in this essay are those of “trajectories” and “assemblages.” By using “trajectories,” they emphasize spatial, territorial and social groupings of ideas, signs, arguments, bodies and so on which are in motion, each in their own way; and by assemblages, they mean what we normally consider “identity.”<sup>4</sup> A book is a kind of assemblage. So is a computer, so is a person. They are various conjunctions of different types of signs, matter and energy, on different trajectories, which happen to be brought together (in rhizome-like fashion?) at a given time. Both these concepts appear later in STS theory, though in much simpler guise than that of Deleuze and Guattari’s dense text.

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<sup>3</sup> Sounds like bin Laden read Deleuze and Guattari when setting up the organization of Al-Qaeda!

<sup>4</sup> There is a likelihood that this simplification is not a correct interpretation, but it seems to me to make the point.

Michel Serres also writes a non-unitary philosophy – he calls it a philosophy of flux, a philosophy of prepositions, different from other philosophies that study nouns (properties) or verbs (processes). He can be properly called a philosopher of science, unlike Deleuze and Guattari. He writes in turbulence. He writes a philosophy of chaos. His writing style is chaotic, in the mathematical sense – rigorously focused islands of development which bifurcate fluidly into quite distinct arguments and topics. His logical style is iterative, though each iteration could be said to be fractal – never quite the same structure or argumentative framework. It is difficult to summarize his work, but it has some distinct properties that make it instantly recognizable, and which have shaped the approach of many STS scholars, whether they know it or not.

He works relentlessly on the theme of transformation, change, translation and movement. He invades the classic philosophers, renaissance angelology, nineteenth century authors, the Bible, mathematical developments in topology and geometry, French fables and scientific history with his analyses. He has studied deeply in a number of disciplines: philosophy, literature, thermodynamics, cybernetics, biology, geometry and mythology, and brings all this awareness to bear in each of his books. He is determined to demonstrate that not only is science laden with cultural, often mythological motifs, but that literature and poetry can be argued to be important exponents of scientific problems. But, as one reviewer notes: “His aim is not to establish immediate relations between different domains, to mix philosophical with scientific contents, or to discover farfetched analogies. Convergences and alliances take place not by similarity and analogy, but by a formal set of operations of interference, transformation and passage. Thus to speak of borrowing or of importing and exporting between domains is to miss Serres’ point. It is to confuse the common idea of a critical grid with a much more fundamental notion of identical structure.”(Hermes, p. xxxvi)

His project is massive, and I confess, a thorough description is well beyond the erudition of this humble author. But let us return to his contributions to STS. It is most apparent in the maxims of Actor Network Theory (ANT) (see section II below), which has been the major theoretical contribution of STS to sociology. Serres' contribution is to focus the analysts' attention on the positions of the different actors relative to each other, and relative to the information that is being circulated. To focus on positions, relations and circulations. By positions I mean where an actor (say a scientist) positions herself relative to the ongoing debate or development. Controversies are conceived of geometrically. By placing oneself in agreement with another, one may then receive money to do work, one may be able to move literally to a new location, or to move a colleague, or a strain of bacteria or a paper or protocol. Thus positions have strategic importance. And the act of positioning has great importance regarding the distortion or transformation of information that occurs. Scientific activity, rather than being the disinterested pursuit of knowledge, is transformed through his narrative into a chaos of statements, textual translations, and material translations (for instance from verbal messages to graphs to material experiments and back).

But that is not all. He make us cognizant of the mythological aspect of many of the contests, of the parasitic nature of much of the relations and of the asymmetrical exchanges that are going on as part and parcel of these circulations – basic relations in, for example, parasitology are juxtaposed in his texts with “isomorphic” moral fables about social parasites. Science is thus presented as part and parcel of the whole package of our culture's activity. It is not cordoned off into the “rational” space, exclusive of “artistic” endeavours.<sup>5</sup>

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<sup>5</sup> An example, from *The Parasite* “The intuition of the parasitologist makes him import a common relation of social manners to the habits of little animals, a relation so clear and distinct that we recognize it as being the simplest. Let's retrace our steps for a moment, going from these habits back to those manners, reversing anthropomorphism. We have made the louse in our image; let us see ourselves in his.

The way he analyzes interactions allows him to draw attention to bifurcations, surprises, breaks in the chain of logic or routine. He writes complexity theory. As I will outline in the next section, ANT has adopted this focus on messages, transformations, and rearrangements (for an example of this writing, see Appendix 1)

One last word about Serres is that he is a deeply moral philosopher. He is centrally concerned with violence, coercion and destruction – and centrally obsessed with science’s role in it. He once said in an interview “Since the atomic bomb, it had become urgent to rethink scientific optimism. I ask my readers to hear the explosion of this problem in every page of my books.” (Serres, 1995, p.15) He approaches science’s pact with war and destruction openly, frankly, and often. This moral stance is not taken up to as great an extent by the STS writers, but their insistence on close ties of knowledge, relationships and power echo his concerns.

The two philosophers Gilles Deleuze and Michel Serres have contributed to STS a focus on networks, on loose associations that change through time, on shifting phenomena that change under changing conditions. They give the STS scholars analytic tools to conceptualize the transformations that entities, theories, graduate students, grant funds, and instruments undergo as a scientific project progresses through time. Importantly to our work, they insist on attending to alliances, to the tangled assemblages that become more and more entangled as they gain

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The intuition of the fable of the rats [the country rat and the city rat] and that of the philosopher who wrote of the eagle and the lamb, makes them import a very common relation in the realm of mammals and of the vertebrates in general, the relation of the hunt and of predatory behaviour to human habits and customs. Man is a wolf for men, an eagle for sheep, a rat for rats....Quite curiously, the manners of this wolf, fox, lion, monkey, cat or rat are never, or seldom those of predators; in these stories, they are almost always those of parasites. In the guise of an attack, a theft, a power play, in the person of these animals, the simple relation of the abusive companion reappears. Beneath the apologist, the parasitologist. Quite simply, what is essential is neither the image nor the deep meaning, neither the representation nor its hall of mirrored reflections, but the system of relations. The relation is that of guest to host. Copying the relation of man to man brings us back to parasitism. Thus the writer agrees with the scientist and agrees with the intuition that makes the book enchanting. Of course, we may speak of rats, snakes or hares and none of them can be assimilated to the louse or the tapeworm, and yet, what is in question will be nothing but the Parasitic. The triangle is closed. At each of its points, through story or science, social science or biological science, just one relation appears, the simple, irreversible arrow.” [from host to guest]. Serres, 1982 p. 8

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report prominence or influence. They describe a world that is heterogeneous, a world of multiplicities. This is a ripe world in which to explore hybrid possibilities.

## ***II. Core “Science and Technology Studies” Theorists***

STS researchers such as Latour, Callon, and Law have worked to apply the often convoluted and abstruse writings of post-structuralist French authors in a straightforward and applied manner. They have taken the de-centered philosophy described above and mixed with some specific semiotics – Greimasian, which examines the transformations actors undergo as their meaning changes in a story relative to their position in its narrative structure. Two other prominent features of the theory are added insights from Foucault (1977; 1978), particularly on relations between material/spatial issues and social and cognitive ones, and an often ethnographic research approach, borrowed from anthropology (Geertz, 1973) and ethnomethodology (Suchman, 2000; Garfinkel, 1996).

Their accounts emphasize social:material linkages, assemblages, and the symmetry between knowledge and power. They focus on the substitutions, alliances, and delegations as information and materials circulate through a social and material landscape.

Particularly important for this work is the notion of non-irreducibility, that things become layered into new assemblages as they travel from one site to the next. This is a very empirical observation. For example, a scientific article can be associated with many different types of activities – rebuttal, deployment of resources, beginning of new work, and so on, depending on the milieu into which it is being incorporated. The artifact (or article) is thus altered through this new set of associations, as is the entity into which it travels. Often, in the case of many of the studies written by ANT scholars, these transformations may result in drastically changed social

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“positions” and “trajectories” of activity of social (and natural) entities. In this way, it is a theoretical tradition which uses topological metaphors to articulate social phenomena.

What are the consequences, the utility of adopting this approach? It gives us a vocabulary to discuss the simultaneously social and material natures of things, their material aspect, and the social aspect, and how they co-construct one another. It goes beyond that in that it allows us to clearly articulate the social changes brought about by the material entities. For example, let us take the newly-recognized “culturally modified trees” (CMT’s). They are certainly real physical entities. Without this designation as CMT’s, however, they are harvested, just like any other tree would be. Through the social work of the First Nations people, they become part of the standard procedures for measuring and inscribing (writing down) what a forest is. Thus they become re-materialized in the paper and computers of the Ministry of Forests. This first materialization is essential to their existence as material entities in the forest. But to be materialized on paper is not enough – they existence must be circulated to the proper individuals for this new categorization to have any meaning. And as a circulated entity, the CMT’s will have different consequences depending on where, in the social/physical world, they are located. What I mean by this is that the knowledge of CMT’s will generate different types of activity and have different types of meanings attributed to them (“consequences”) depending on where they find themselves. The logging foreman will have to take different action than the forest manager, than the Ministry forester. CMT’s will engender different activity in the First Nations Band office, and still different activity in the academics or activists’ office. A CMT, thus is not a passive entity, but an active one, a central player in many different types of activities at multiple sites.

So an actor-network approach sketches out a series of relations and transformations based on concrete descriptions of local material/social arrangements. In this way, it shows its strong links

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report with a type of anthropology called ethnography, and a type of sociology called ethnomethodology. It is a kind of biochemical account of the metabolism of information.

This framework is useful to us because it provides a rich vocabulary for articulating the mutually co-constructive relationship between “natural” and “social” entities. In fact, it specializes in how these categories are constructed, deployed and enforced throughout an analytic space. It is also crucial to our working notion of hybridity, as it is a type of inquiry that seeks to articulate the different strands and heterogeneity of social/material things, and as such has ample space for hybrid objects in its vocabulary.

As an example of a seminal theoretical work in STS, I want to take a moment to discuss some salient aspects of Bruno Latour’s *Irreductions*. This essay, which appears in English as the second half of a book entitled *The Pasteurization of France* (Latour, 1988), sets out Latour’s philosophical approach to doing STS research. I am giving it its own unique space because this is a clear explicit rendition of a way of doing and writing research that is implicit in many others’ work.

The essay is a rich polemic covering many aspects of doing social science, and I will not attempt to summarize or explicate it in a complete manner. I will focus on one aspect – irreducibility – found in the title. The position is laid out in the first sentence of the essay – “Nothing is, by itself, either reducible or irreducible to anything else.” (p. 158).

From this starting point, Latour goes on to replace reductions with “trials of strength” – “what is neither reducible nor irreducible has to be tested, counted, and measured. There is no other way.” His next proposition follows – that “everything may be made to be the measure of everything else” (p. 158). Reality is a quality given to those entities which, when subjected to a trial, resist.



For example, in early studies done to test whether DNA was the genetic material, mutant bacteria were given DNA from normal cells and then asked to grow on a mixture of nutrients (media) which normally they could not grow on, although non-mutated bacteria of the same species could. This is a trial the bacteria were subjected to. To those bacteria given a protein mixture instead of DNA, the trial was too much – they died. Among those bacteria that were given DNA from normal bacteria were those who could resist the trial and lived. The notion that DNA was the genetic material just became more real.

Latour then develops a framework where the struggle toward determining what is “really” going on is one of enrolling others (whether “others” are convinced colleagues, compliant bacteria or purified DNA molecules), becoming able to speak for others (“DNA is the molecule of heredity”), and developing asymmetrical circumstances where one outcome is more likely than another (laboratories and practices where the experiments can be performed reliably).

He replaces the notion of “reduction<sup>6</sup>” with a much more empirically sound one – “association.” Thus a given situation does not become “reduced” to a system, (I recently read a newspaper article where a researcher was quoted as saying about exercise that “essentially what you are doing is turning sets of genes on and off” this is an example of reductionism) but rather the researcher’s activity of aligning or associating their model with the situation is becomes salient – how do they do it? How well does it stand up to scrutiny of others? Who buys in and what does that mean for the future of the model – how is it transformed and so on...<sup>7</sup>

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<sup>6</sup> A typical “reductive” scheme would be the “paradigm” proposed by Kuhn as an explanatory measure for the progress of science.

<sup>7</sup> This approach has theoretical brethren with *ethnomethodology*, a form of sociology that seeks to describe *how* the “norms” “values” and “beliefs” reported by other sociologists are constructed, negotiated and enforced in everyday life. It has influenced STS scholarship insofar as STS researchers can be distinguished by their preference for investigating and articulating the mixtures, movements and transformations in which science participates.

One other unique controversial feature of this approach is the symmetrical sociological treatment given to non-humans – both instruments, infrastructure and the entities being researched. From Latour’s perspective, the scientists mentioned above need the bacteria and DNA to be “on-side” for their success as scientists. Without these non-human entities’ cooperation, the scientists’ theories and careers go nowhere. He emphasizes the considerable effort expended to secure and maintain their support. In this case, laboratories are built, incubators designed or purchased, media are specially tailor-made to the bacteria’s needs and so on.

He also emphasizes that the “truth” is invariably the *result* of all this activity. It does not passively “reveal itself” to an investigator’s keen eye, but is the result of a lot of work and depends on a lot of work to maintain its status as “truth.” He notes that while in the heat of research, scientists never refer to nature as an explanatory resource, but instead to their tools and procedures. It is only after their controversy has been settled do they claim to be subordinate to nature, or to have revealed what is “natural.” Note that he does not hold that everything is *merely* socially constructed<sup>8</sup>, but notes the extensive social and material interactions necessary to maintain a knowledge claim or point of view.

The point that I think bears recognizing is that with this approach to research, things-in-themselves do not disappear under the analysts’ lens, but are considered to become embroiled in increasing (or decreasing) networks of associations. Thus rather than being reduced to some single substance or concept (for example, gene) that can “explain” it all, a researched entity is treated as a an actor-network – an entity always bound up and connected with others (in our example, the DNA is bound up with the American science funding regime, the specific bacteria

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<sup>8</sup> To those who may take the claim that reality is socially constructed to be a claim that it is therefore illusory, I might suggest that a house is socially constructed, but is also undeniably “real.”

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report with which it was tested, the presence of the specialized media and so forth), and through these connections *gains* its attributable qualities (molecule of heredity).

The other point worth drawing out of this approach is that the social world and the material world, the world of the human and that of the non-human are considered to be inextricably combined. Thus, no “truth” about DNA exists without a society to support it, and similarly, society, indeed humanity is considered to be deeply intertwined and living intimately with tools and technologies. Though this sounds rather obvious, it is worth bearing in mind that much of our sociology and science work to split these two worlds. Scientists claim to seek self-evident or transcendental truth about the natural world and make claims that minimize or ignore the social and material contingencies involved in coming to the claims. Sociologists often speak of entities such as “kinship systems” or “norms” which have no material aspect. So in relation to these ways of examining, the philosophy/sociology of Latour and others in STS is different from the conventional approaches.

### **III. Practice Theorists**

The “practice theorists” have many aspects in common with those whom I call the STS theorists (see section II above). Both groups take embodied, material experience as their data set. Both assume a coproductive<sup>9</sup> relationship between the material and social worlds, and both focus on the importance of the environment in the shaping of an individual. They come from different origins, however, and have slightly different conceptual perspectives. In the common task of articulating a social/material world however, I find that these differences become inconsequential.

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<sup>9</sup> This is a term introduced by Jasanoff (1996, 397), which emphasizes the mutually constitutive relationship between scientific and social worlds.

The predominant difference, in my opinion, stems from differences in their lineage. The STS scholars mentioned above are working to incorporate post-structuralist insights, predominantly from the French tradition, into a coherent framework, while the practice theorists are composed of two groups, one with its roots in Marx's material dialectic and the other in grounded theory of the American sociologist Anselm Strauss. I am familiar with those who are grounded in the work of Marx, and the "critical psychology" and "activity theory" schools, based predominantly in the work of Russian psychologists, and now in the recent follow up by American and Finnish scholars. The main difference between the "STS scholars" and the practice theorists is that the practice theorists put more emphasis on social categories, the concept of production, and subject/object relations. They also have a more concrete and unitary concept of the individual, and though they acknowledge the importance of tools in forming human relations and individuality, do not grant these non-human entities agency.

Both the grounded theory and neo-Marxist schools analyze phenomena in terms of work activity. They ground their analyses in attempts to articulate how any individual's contribution is contributing to the overall goal of their community of practice, the group of individuals, tools and rules that structures any one individual's activities.

From them, we get a strong and compelling picture of the social and collective nature of cognition, activity and representation. A seminal book on the topic of social cognition, Jean Lave's *Cognition in Practice* (Lave, 1988), details how the mathematical practices of "just plain folks," which, though much disparaged by experts, can be shown to have accuracy levels of around 95% (Stu –check this), is a consequence of relations between the physical layout of, for example, grocery stores, the demands of the family unit, and economic considerations. In an extremely detailed analysis of navigation practices as social cognition, Edwin Hutchins

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report (Hutchins, 1995) demonstrates the historical, spatial, tool-based and socially determined nature of cognition and activity among the staff on a large naval vessel who are responsible for its navigation. Other writers point out how doctors' framing of illnesses to patients can be conceptualized as largely determined by the social setting of the hospital clinic beset with funding cuts (Engestrom, 1993), how cognition and interpersonal interactions are constrained and shaped by architecture and instrumentation (Goodwin, 1995) and so on.

Recent work in the so-called "third generation" activity theory attempts to broach the rapid and mutual transformations involved in practice, through introduction of the term "knot-working" (Engestrom, 1999). I find, however, that these theorists' analytical distinctions such as "rules, tools, community" make it difficult for them to trace the delicate changes and interlinkages that Actor Network theory can articulate as material entities take on or influence social roles or activities.

However, these self-same distinctions do provide a vocabulary to talk about people and things as members of groups, and they have spawned a psychological approach, called situated cognition, that is a useful link between the mind "in there" and the social/material hybrids "out there."

Because they have a less problematized notion of an agential individual, practice theorists give us a means to understand how personal relations, representations and even perception are deeply influenced by social and material context. From their work, we have the theoretical background necessary to consider scientific and other ways of articulating the world as always cultural products – tools which function in a specific community of practice. When the groups come together to problem solve, an important analytic question then becomes – how do these representative from different social/cognitive and material worlds negotiate this new shared

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report space? What is the nature of the new arrangements? Power issues can be articulated very clearly in answering these questions as we trace the rules, tools and languages of the new emerging community with those of the ones whose members form it.

This is a key insight when analyzing the different contributions of different parties to agreements like the Clayoquot Sound Scientific Panel. The concepts of “social worlds” (from the grounded theory people) or “community of practice” (from the practice theorists) give us the second essential tool in our discussion of hybridity – a coherent point of difference between entities. Thus, we can consider the Scientific Panel to be hybrid because it is incorporating people from different cultural and linguistic worlds.

#### ***IV. Social Worlds/Linguistic Worlds***

This section and the two following it elaborate on the relationship between social worlds/communities of practice and communication. This section, section 4, briefly outlines the contributions of thinkers who link language, from a Marxist materialist perspective, with social groups or communities. Section 5 summarizes some American essentialist work that analyzes the relation of embodied experience to language in the context of resource management disputes. Section 6 explicates the role of material representations in communications within organizations and society.

Mikhail Bakhtin is the predominant theorist in the Marxist group. I introduce three features of Bakhtin’s work that apply to our future study: the dialogic nature of communication, the inescapable obligate relationship between language and a speaker’s social and cultural siting, and the struggles between languages as a part of greater social struggles.

For Bakhtin, language, as such, is not a system, but is always something enacted.

“As distinct from the signifying units of a language – words and sentences – that are impersonal, belonging to nobody and addressed to nobody, the utterance<sup>10</sup> has both an author ... and an addressee.” (Bakhtin, 1986, p. 95)

“The speaker is not the biblical Adam, dealing only with virgin and still unnamed objects, giving them names for the first time....the subject of his speech itself inevitably becomes the arena where his opinions meet those of his partners.” (pp.93-94).

People perform speech acts as important social/political and practical accomplishment in always situated and local settings. For Bakhtin, language is necessarily *dialogic*, it is enacted simultaneously in response and in anticipation to other speech acts or events. To engage with language is always to be participating in chains of dialogue with others.

The next notion of central importance is Bakhtin’s elaboration of the notion of a plurality of languages within a community. His world of *enacted* language is populated by a heterogeneous set of languages. Each language arises out of a different community of practice’s encounters with their lifeworlds, including tools, instruments, professional jargon, social standing and so on. “All words have the “taste” of a profession, a generation, an age group, the day and hour. Each word tastes of the context and contexts in which it has lived its socially charged life” (Bakhtin, 1981a, p.293). From this perspective language is wrapped up in political, class, ethnic, and professional issues. Therefore utterances are irredeemingly situated. This situated-ness allows the literary theorist to discuss not only language per se, but language in the context of its utterance, both within the story (as in the case of the novel) and within the cultural context as a whole. Bakhtin wrote several works that elucidated historical social and linguistic trends implicit in novels.

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<sup>10</sup> “The utterance” was Bakhtin’s preferred unit of literary analysis, emphasis the relational aspect of communication.

As a final elaboration of his situated model of language, Bakhtin located language within a pluralistic society, which necessarily contains many different non-overlapping languages “all languages ... are specific points of view on the world, forms for conceptualizing the world in words, specific world views, each characterized by its own objects, meanings and values. As such they may all be juxtaposed to one another, mutually supplement one another, contradict one another and be interrelated dialogically” (Bakhtin, 1981a, pp.291-292).

In such a pluralistic setting, power struggles between languages arise. Which language will become the dominant conceptualizing discourse? Who gets to set the terms of reference for what exists, what is a problem, and whose strategies are taken for granted as common sense? He specifically spoke of different languages from different classes and professional groups “quarreling” or struggling with each other. “the languages ... relate to each other as do rejoinders in a dialogue; there is an argument between languages, an argument between styles of language...it is a dialogue between points of view, each with its own concrete language that cannot be translated into the other” (Bakhtin, 1981c, p.76). Events where languages meet and quarrel with each other can result in the formation of irreducible hybrids – new unions. Here we see the notion of a “hybrid discourse” articulated.

This analysis has profound implications for doing qualitative research. Bakhtin urges us to do a type of interpretation that takes the context of a text into consideration. We are challenged with the charge of delineating – who spoke it? To whom? From what particular political position? During which controversy? How did the utterance constrain or afford the parties in the dispute differential opportunity? And so on.<sup>11</sup> As analysts, we are forced to abandon the hope of “value neutral” language, and instead attend to the sociology inherent in how we talk in the

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<sup>11</sup> This type of analysis is typified by the maxim arising from public administration research “it’s not where you stand, but where you sit” that counts in terms of putting forth rational ideas.



Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report world and about the world. This theoretical sensitivity is especially fruitful when describing the politically charged landscape of admitting other cultures' knowledge practices into regimes that are designed on the scientific metaphor, which many of us have naturalized as the bottom line of reality. And finally, it gives us a way to articulate what is new that arises from the confluence of the two cultures, a detailed theoretical meditation on socio-linguistic hybrids.

Lev Vygotsky was a Russian doctor turned experimental psychologist working in the twenties and thirties. He had a unique conception of psychology – as arising from child's interactions with the world around it (Vygotsky, 1978). This point may seem obvious until it is contrasted with developmental claims of internally directed 'programs' that drive an organisms' development, which were and are still, the dominant theoretical approaches. Vygotsky would claim, rather, that psychological phenomena arise from an interaction with both tools and objects, and an appropriate human contact that enculturates them with the proper knowledge. Thus a child's mind develops through interaction. This conception of human development as one of interaction between an organism and its environment is also gaining support from numerous neurological studies, which are increasingly describing a brain whose development is profoundly influenced by activity (Bates, 1992).

The importance of this theoretical approach to our case studies is that once again, it allows us to articulate difference in terms of different conditions and lifeworlds and sidestep entirely assumptions based on notions about inherent intelligence. It is also useful idea to add to our stable because it emphasizes the social arising of speech about the world, and helps to place discourse in a social context. Note that we are not trying to discredit or dismiss the importance of biological phenomena, such as inherited difference. But we are trying to weaken their hold on causality and determinism.

Lemke has applied Halliday's "functional linguistics," which have developed the ideas of Bakhtin and Vygotsky, to different aspects of science writing, particularly with respect to scientific education (Lemke, 1995; 1999). He analyzes different aspects of science communication, focusing both on political aspects of science rhetoric as used in the United States, and of the different symbolic/semiotic repertoires that science students must master in order to become competent (across the modalities) (Lemke, 1990). He is part of this review paper because he reminds us of the different socially-grounded symbol systems that scientists use. His work provides us with some rich detailed and applied looks at the phenomenon, unlike Vygotsky (1978) and Bakhtin (1986), who were not interested in analyzing scientific communication.

The authors reviewed in this section ground language and communication solidly in the cultural, material and political realities of those who are speaking or communicating. Their contributions give us leverage to consider texts in terms of other cultural issues. This is an especially important leverage because much of the innovation and conflict in Clayoquot Sound arises out of deeply divergent cultural ways of being in a society and in the world. I suspect it will prove very helpful to be able to talk about issues around implementation of the scientific panel in terms of political and cultural struggles as well as epistemological issues.

## ***V. Narrative Approaches***

There are a number of social scientists whom have sought to understand and explicate the ways that different people talk about natural resources. For heuristic purposes, I group these authors into two broad categories – a) the "narrative" constructivists – whose research centers on how people construct their world through discourse and/or negotiation and b) the social/natural

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constructivists, who write research that includes accounts of biophysical entities. This is a definition based solely on research writing, and is not meant to reflect disciplinary genres or any other kind of distinction.

These authors share with those mentioned previously the premise that there is not a single, stable world “out there,” that should be the ground for sensible, rational decisions made through objective observation. They agree that scientific and non-scientific perspectives of the world, are formed by one’s lifeworld – the cultural settings in which one finds oneself. There is broad agreement between all authors reviewed so far, then, that a process for understanding and working with heterogeneous points of view, knowledges, access to resources, and so on, is necessary for appropriate political action on natural resource management issues.

What places these authors in a different category from those of sections 2 and 3 is that these authors tend to write their accounts with a more pronounced anthropocentric basis. People are people, who act on, discuss and manipulate the environment. There is less emphasis placed on co-production (or co-arising) of agency, identity and so on as a result of interactions between humans and non-humans. There is less emphasis on what is called the “dialectical relationship” between the human and the biophysical world. Some authors go so far as to claim that our reality is based on our values and beliefs<sup>12</sup>. Personally, I do not agree with this contention, and prefer a story of the world that is more physical and embodied. Nonetheless these authors have contributed to our understanding of how different people, or groups of people, talk so differently about the environment.

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<sup>12</sup> My perspective on that is “values” and “beliefs” are products of talk about values and beliefs, and as occasions for social interaction, discussion and forming groups, they are fine. I have an analytic suspicion that their prominence in social sciences is an artifact of the interview and survey research methods, which necessarily are limited to narrative-style data.

**a) “Narrative” constructivists**

In this sub-section, I will review two groups of authors. The first group details and relates issues of identity, understanding and meaning to differing narratives about the landscape and environment, and the second group examines values and metaphors implicit in scientific participation in resource management.

The methodology employed by these writers is primarily the semi-structured interview. In this situation, interviewers normally have a few questions or prompts, but respondents have the opportunity to take the conversation where they want it to go. The analysts then transcribe the interviews and work to create<sup>13</sup> simplified “themes” from the stories people tell. From these themes, they build theory on how their informants construct their world.

**i) Varieties of “indigenous” perspectives**

The three papers I review show that “nature” is a word that has many different meanings.

Two by, Bruce Hull et al. (2001) and Peterson and Horton (1995) involve interviews with local community members from the Appalachians in southwest Virginia and Texan ranchers, respectively. Both showed, first of all, the complexity of people’s talk about the landscape. Both papers argued that the “environment” for these people was a deeply cultured one. It had multiple meanings that arose from their activity on it, struggles with it and shared stories about it. The Hull et al. study made the point that landscape features, such as fields that have been farmed for generations by a certain family, which are very important to local people, may be seen as “un-natural” by outsiders and scientists. This point becomes important when environmental organizations purchase large tracts of land in order to maintain the land’s natural state, or science-driven government policies attempt to bring about changes in land use management in order to increase “wilderness values.” Often these actions are blind to histories and land uses that are part of local peoples’ relationship to the land. For the authors, the clashes of local and

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<sup>13</sup> I use the active verb “create” rather than ones such as “uncover” or “discover” to emphasize that noticing themes in someone’s story is an act of creation in and of itself.

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scientific narratives often negatively affect the residents, and are a result of not taking different stories into account when deciding land-use policies.

Peterson and Horton (1995) wrote their paper as a comment on Endangered Species Act-justified land takings by the US federal government in Texas. They were critical of the approach taken by the Fish and Wildlife Service (FWS), which involved only scientific assessment of the situation followed by brief public consultation that had no mandate to substantively affect the outcome – a massive appropriation of ranchers’ lands. The ranchers for the most part did not participate in these consultations, believing they were not being carried out in good faith. The researchers, through an analysis of 28 interviews, argued that they found “mythemes” within the transcripts of the talk. A “mytheme” is a theme, as identified through usual qualitative research textual analysis, which has mythic properties. That is, it “provides a rhetorical structure within which people continually recreate themselves and their situation” (Peterson and Horton 1995, p. 147). Through a detailed analysis of the qualities associated with ranchers’ “stewardship” mytheme, the authors suggested that the ranchers had deep and abiding interest in a healthy land and would have made better allies than enemies. They suggested that instead of ignoring the ranchers in public lands takings, the FWS should instead consult deeply and find common ground between the mythemes of, for example, landscape stewardship that both wildlife scientists and ranchers share.

The final paper in this cluster is more theoretical, and argued that landscapes are “symbolic environments created by human acts of conferring meaning to nature and the environment” (Greider and Garkovich 1994, p. 1). Developing this perspective, they propose policy implications that involve appreciating that changes to landscapes can be actually changes to people’s identity. From this perspective, landscape changes need to be understood and discussed

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report in policy forums as important changes to a community's identity. They claim that the introduction of landscape changes can both

- fracture previous community consensus on the *meaning* of certain landscape features – think of how the meaning a certain stream or meadow can be de-stabilized by an outside party preparing to develop it
- bring about significant community change before the change in the biophysical landscape – think about how fractured the communities in Clayoquot Sound became before any logging was done.

This work urges us to consider how deeply social the landscape is, and confronts resource managers who may want to do things from a strictly scientific approach with a deeply social and political landscape to negotiate as well.

## ***ii) Issues for scientific narratives***

The second grouping of papers focuses on values inherent or missing from scientists' discourse. In an excellent and challenging paper, Bryan Norton (1998) addresses the issue of ecologists' participation and influence (specifically, the lack thereof) in environmental policy making. He suggests that new language needs to be created, incorporating "indicator terms" that have instrumental scientific meaning, but also explicitly contain *evaluative* content, which will help relate the science to the value issues that concern policy makers.

He argues that there are a number of intellectual problems underlying the current approach to resource management. The first is one of "serial narratives." Under this approach scientists first describe the reality of a landscape in terms explicitly designed to avoid evaluative judgements. Then economists (or policy makers) put value on it. This serial approach to environmental

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report management is intellectually deficient because it does not provide for an integrative description and valuation of a landscape. He challenges scientists to develop a language that includes indicators of the values of what they describe in order to allow their work to become relevant to policy makers. He uses the example of the economic indicator term “GDP” as an example of a term that has both empirical rigour and a well-established valuative component.

His second suggestion is that scientists need to become aware of, and importantly, *responsive* to issues that are important to policy makers, and therefore the general public, when they design research projects. He uses the case study of the United States policy on “wetlands banking,” which assumes (with no scientific backing) that it is only the total acreage of land present as wetland that is important, and that developers can create wetlands elsewhere to compensate for the extant ones that they fill in. He challenges the scientific community with the question of (sic) “why hasn’t there been any follow up studies done on even one of the hundreds of these “banking” projects?” He also suggests that the choice of scale of most ecological studies is not useful to people needing to make decisions about environmental policy – the timelines are too short and often the physical scale is too small. As a positive contribution, he lists some suggestions as to what a communication system that would effectively link policy and science would look like.

Though I find his contribution very interesting and potentially fruitful, a serious theoretical problem with the paper is that it fails to acknowledge the already highly politicized nature of scientific activity and discourse. For example, there are many statements such as “global warming,” “genetic pollution,” and “toxic waste” that are both empirically measurable and which have substantial valuative components. The major blockages to bringing effective policy and research to bear on these issues is that there are political forces which are opposed to in-depth,

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long-term research on these topics, or who contest the valuative aspects of the statements in the political arena. I would suggest that the success of a term like “GDP” would be far less if it wasn’t aligned with prevailing narratives of legitimization of the current means of economic production. So although Norton’s contribution is welcome and a useful tool to think deeply on, we must acknowledge that, to use his example, an in-depth appreciation of the subtleties of hydrographic cycles is unlikely to support the approach to development of the people who introduced the idea of “wetlands banking.” Therefore, it is unlikely to result in a release of funding. Lack of scientific follow-up is not necessarily due to scientists’ deficiencies, but can be also seen as an act of intentional ignorance by those responsible for funding the science.

The final paper in this group is that by a group of Canadian geographers exploring the various historical and intra-disciplinary meanings of the metaphors “ecosystem” “health” and “ecosystem health” (Ross et al. 1997). They conclude that these metaphors’ meanings are not straightforward, but that they hold promise as organizing elements for social activity in that they bring us back into an organismal point of view with respect to nature, and that they encourage people to think about the environment in terms of their own embodied experiences of health.

In summary, these authors share a commitment to the centrality of language and verbal discourse in constructing environments. Their research highlights the considerable diversity of talk about the environment, and all call for political solutions that pay more attention to these different ways people have of creating their identities and environments through talking. In particular, Norton (1998) suggests the development of a new way of speaking, one that combines scientific with evaluative forms, in order to foster a discourse that is relevant to the new field of environmental politics. In the following section, I review the contributions of authors who treat the construction of environments as a consequence of larger and more material contexts.



**b) “Social/Natural” constructivists**

In this section, I review authors whose work does not focus solely on discourse when dealing with environmental issues. They emphasize to a greater degree than the “narrative” researchers the co-constitutive relation between natural and social worlds. Arturo Escobar (1999), in the review article *After Nature*, works out an analytical framework that posits three different kinds of nature: organic, capitalist and technonature. Around these three different types of nature, there are different social groups and different economic regimes, different metaphors, different technologies, practices of resource management (indeed, conceptions of what a resource is) and so on. In fact, his typology arises as a consequence of these different constellations’ different patterns of ways of commodifying, transforming and being transformed by nature.

His typology is based on his extensive fieldwork in Columbia and is an attempt to ameliorate his experiences with the effects of globalization and biotechnology on indigenous people, migrations of peasants and large scale land changes that he has seen. He shares many perspectives with the authors mentioned in sections 2 and 3; the main difference lies in his creating a classification scheme of causal or unifying “regimes.”

He proposes the term “hybrid natures” to describe the tensions that occur when, for example, the world of biotechnology encounters that of indigenous peoples. In these encounters a new nature and a new culture of nature must be negotiated between the different players. His paper’s political purpose is to suggest new ways of being in a nature whose identity is fragmented, partial and caught up in multiple production and meaning-making systems: “Could the new life technologies foster other types of creativity and means for wresting control of life away from purely capitalist goals? Could the present rupture in the meaning of the natural lead to a new art of living in society/nature?” (Escobar 1999, p.15).

Rik Scarce (1997) examines the deep links between economic and scientific terminology and hatchery management practices in the description and manipulation of salmon and their stream habitat. His research links two spheres, which he calls cognitive and physical/behavioural, by pointing out the parallel development between a mass-produced salmon, for example, and a set of metaphors such as “full utilization” of a stream’s “carrying capacity” which would result in “net benefits.” He demonstrates how a political response to a socially created problem – building a hatchery downstream of a hydro electric dam – not only introduced commodification metaphors to salmon fishery scientists, but also changed the salmon themselves. Their life cycle and size, to name two variables, were changed by the practice of rearing the salmon in a hatchery.

His argument comments in a sidewise fashion on Norton’s contention for the need for scientists to become more relevant politically. Many of Scarce’s scientist interview respondents suggested that *because* their research was driven by policy and economic considerations, it was of poorer quality and did not necessarily recognize the fish as intrinsically valuable members of a complex ecosystem. This would seem to contradict Norton’s claim that relevant science would be a better science. I can imagine Norton responding, however: “Exactly! By leaving evaluative statements about the salmon themselves out of your work, and passing it along for others (the politicians and economists) to assign value, you lose the opportunity to participate as an active member in the policy debate.”

Escobar and Scarce work to examine scientific discourse as it relates to discourses in other areas of society. They carry on a similar project to Michel Serres, mentioned in the first section, who seeks to show us how scientific practice is always co-extant with the commonly held

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report mythology of the day, and in fact, can be related to mythological issues articulated by Greek and Roman philosophers millennia ago.

By either exposing aspects of science's integration with other parts of society, as Scarce and Serres do, or calling for a more deliberate integration, these authors challenge us to ask and reflect on "what place does science play, and what place should science play in determining society's course of actions?" These are questions that are at the heart of many of the conflicts and successes of the Clayoquot Sound Scientific Panel – as they attempt to come up with a more inclusive definition of science, and as "ecosystem-based" scientific prescriptions run up against the standard predictions and assessments. These authors show us that an examination of what people say can be a useful in-road to addressing these questions.

## ***VI. The Important Social Life of Representations***

This section could be considered the twin of the previous one. Here, we review authors who also analyze communicative strategies as a route to understanding social and political worlds. The major difference is that they take objects – material, rather than verbal representations – as their topic of study.

Scientific and technical documentation is particularly rich due to the complex nature of its representing practices. By "representations" I mean any artifact that scientists use that stands in for (is taken to represent) the actual object of study. Micrographs, photographs, sketches, graphs (of all kinds), computer assisted engineering designs, chemical structures, space-filling models, sequence alignments, brain scans, MRI body scans, GIS maps, simple tables recording bacterial plate counts, stratigraphic diagrams and so on all count as scientific representations.

These representations are not, as they are often called by scientists, “raw” data, but are historically and culturally saturated entities in their own right. They are also often important players in social organization within labs, organizations and sometimes, empires. Researchers interested in the sociology of science are interested not only in what the representations stand for *per se*, but also in what role they play in the social organization of scientific work. Thus they tend to focus on how representations are deployed by various actors, and to what extent they can be considered actors in various situations. In this review, I will focus on a limited number of case studies that point out a number of ways in which representations shape and reflect various social aspects of the practice of science.

In an article entitled *The Externalized Retina* (1990), Michael Lynch analyzes sets of electron microscopic images and their attendant “simplified” diagrams. His analysis suggests that “seeing” is not all in the eye; particularly in the sciences, our visual descriptions of the natural world are not images of just the object under study. He notes that both in the sample preparation and in the practices of diagram making, “order is not simply constituted, it is *exposed, seized upon, clarified, extended, coded, compared, measured and subjected to mathematical operations*...these modifications, depend on a prior, though relatively indeterminate, *something* which is successfully modified into a more “useful” and at the same time “theoretical” object” (p. 163-164). The analytic point he is suggesting is that scientific diagrams and representations, far from being simplifications or realistic renderings of the world of objects, are *already* coded and imbued with theory *before* any theory is written about them as “raw data”. This implies that in the very practice of representation, the subsequent interpretation is being framed and limited. Thus those who consider science to be value-free are overlooking the values that are incorporated in the very “raw” data upon which theory is built. This has strong implications

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report especially for practices whose visual fields are dominated by scientific imagery such as medicine and microbiology. It also gives us cause to pause when our narratives of the world are challenged by other ways of explaining or rendering it.

Alder (1998), in a historical study, showed how a certain type of mechanical drawing played a crucial part in the 18<sup>th</sup> century, pre-revolution French government's standardization of the manufacturing of its military equipment, and by doing so, wrested power from the artisanal groups who had previously had much more control over manufacturing.

The drive to establish mechanical drawing as a standard practice was coupled with the establishing of standardized objects that defined the error tolerance for military artifacts such as cannon balls (and bores) and the rings that affix bayonets to the ends of muskets. This effort was coordinated by the French government in concert with the creation of a network of inspector engineers and judicial decisions that enforced the government's right to publicly knowable standards of manufacture. No longer were the details of a specific artisanal guild's products "trade secrets." By insisting upon publicly accessible, uniform production standards, the government could coordinate production over large distances. For example, muskets were made hundreds of kilometers away from where skilled German migrant artisan bayonet makers lived. By creating standards and ways of representing them accurately, the government saved money and time. Instead of shipping the bayonets off to the musket producing regions to ensure the proper fit of the bayonets to the musket barrels, and then sending them back to the front, they could count on a certain acceptable degree of fit beforehand, and ship both products, with confidence, to the places where they were urgently needed.

Crucially, through a system of inspectors armed with authority and appropriate representations, they could wrest control of production away from the guilds and bring them

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report under the control of the state to a greater degree. The practice of making realistic representations public knowledge was a crucial part of a larger political shift that saw the eventual disappearance of guilds and the arising of a more capitalist mode of production. This article emphasizes the importance that representations and specific practices of representation play in relations of power and the wielding of authority.

Kathryn Henderson (1995) shows how representations of the “same thing” do different work as they move to different sites within organizations and as a project goes through different development stages. As an example of the types of work that a prototype can do, she lists 4 activities in which it is a central participant during an early development phase: “[it provides] empirical information to decide between two approaches to the design; empirical evidence to substantiate that the functional concepts ... actually worked; elicitation of tacit knowledge in the form of feedback on how the instrument “felt” to people; ... illustrate[ion of] the successful function of the design concept to the medical community and company networks needed for financial and personal support”(p. 283). In her commentary, she makes the point that this last activity, that of networking in order to raise sufficient funds, is just as, if not more important than the conceptual work that the prototype participates in.

The rest of her analysis shows how different representations (including diagrammatic ones) at different stages of the prototype’s development reflect and shape its progress into a “real” medical instrument, made and marketed to the surgical community. The point she makes is that often a “thing” is crafted for, and arises out of struggle and tensions between competing factions, embodied in her study as tensions between the R&D and the manufacturing sections of the company. Her work makes salient the multiple roles – intellectual, embodied and interpersonal – which the prototype can play.

The three case studies reviewed above show how techno-scientific representations are not just reflections of reality, but artifacts deeply suffused with cultural assumptions. They can be potent political tools, can be developed in order to further political agendas, and are circulated and transformed as part of ongoing operations. Finally, they are a site for social interaction and negotiation. But I would be remiss if I did not leave this section without introducing two widely used theoretical concepts that synthesize some of the characteristics of representations listed above.

Bruno Latour (1990) coined the term “immutable mobiles” to denote how scientific representation played a key role in the development of state power and of course, scientific knowledge. The power of scientific representations lay in the fact that information about the world could be written down, transported across the world and assembled in one central place – which he called a “center of calculation.” This, he claims, is an absolutely necessary part of the Modern project of imperialism and coordinating large-scale enterprises. Immutable mobiles, with their standardized formats, could travel around the world, untransformed, be understood in the same way (more or less) by the person who received them as the one who wrote them, and could be recombined, collected, reconfigured and stored. Thus the surveillance and memory necessary to wield power (whether it be over the microbes of Hong Kong or the cotton fields of India) in distant locales could be maintained. Also, immutable mobiles are the key to establishing the communal knowledge practices necessary for a global science regime.

The second theoretical term that emphasizes the social importance of representations is the term “boundary objects” coined by Star (1989). She found, while studying large scientific projects, such as the specimen collection and storage for the Berkeley zoological museum, that “shared understandings” were not necessary for large projects to successfully come to

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report completion. The trappers who brought in specimens did not need to have the same goals or values of the scientists who classified them, or the technicians who preserved them. What was necessary, however, was shared representations, such as maps and standard forms, around which the communities could organize their activities. I do not expect that a trapper, filling in a standard form detailing where and when he trapped an animal will have a shared understanding of the meaning of the form as the scientist who logs it into her species range database, but importantly, they both agree as to what needs to be on the form. To the trapper, the filled-out form was necessary for payment, and an aggravation. To the scientist, it was vital to accepting the specimen; without it, the specimen was meaningless. This difference in meaning attributed to the form did not stop the progress of the project. In fact, Star argues, it is precisely the fact that the object *can* accommodate many different meanings which makes it so useful. Blueprints, maps, prototypes, journal articles and so on can all be considered “boundary objects” – objects which through their centrality to a process can provide a means by which multiple different communities of practice can coordinate their activities on large projects without having to learn about or accommodate the other communities’ interests.

I have reviewed three case studies, showing how scientific representations are deeply entangled in cultural projects. They often arise at the cross-roads of intellectual necessity and important interpersonal tensions. They can be important mediating devices in the wielding of power, and important in coordinating the activity of heterogeneous groups. These findings have led to two important theoretical terms: Latour’s “immutable mobiles,” emphasizing how their ability to easily travel around the world virtually untransformed (unlike the specimens they describe) contributes to the ability of governments and scientists know and control others, and



Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report Star's "boundary objects," a term that underscores the importance representations play in coordinating activities among and between groups working on projects.

This perspective on scientific representations allows us to appreciate why many people, both within and outside "Western" culture, are not at ease with them. By seeing representations as deeply culturally embedded objects, we can let go of the familiar story of cognitive deficiency ("they're not smart enough to understand," "scientifically illiterate," and so on) and directly address the surmountable cultural issues that may arise in their use. Aware of their common use in the past to help one group of people define or dominate others, we can develop an evaluative framework to see if they are being used in a "productive" way. Are they serving as sites for creative social interaction and negotiation, or are they being used to control and exclude? There is good reason to deeply interrogate representations and their circulation when studying a scientific project.

## ***VII. Science in Society: Boundary-Work, Conflict, Alliances, Un/Certainty***

In this section, I present some important foci of work in STS which make use of concepts outlined in the previous sections. STS scholars come from a wide variety of disciplines and treat a tremendous number of topics. I have selected a few to exemplify their approach to analyzing the role of science in society. These have been picked because both they are 'classic' works or bodies of work in the literature and/or they deal explicitly with topics that I imagine to be relevant to environmental decision-making in Clayoquot Sound. I have broken this section into a number of discrete sub-sections, for easier treatment. They are:

- a) Rhetorical claims to rationality

- b) Naturalization
- c) Science, policy and the environment
- d) Science and the law (as proxy for legitimized decision making)

All these studies incorporate an analytical stance known as “the symmetry principle,” which is a taken for granted assumption in most of the STS literature. This principle, developed in the seventies by the members of the “Edinburgh” school of sociologists of scientific knowledge, simply states that the eventual acceptance of one theory over another as correct is not a useful explanatory resource when doing sociology of scientific knowledge. Though this may seem counterintuitive, those who use it defend it by saying that when a fact, procedure or technology is being developed or argued about, there is no concept of “rightness” to fall back on. At the time of controversy, different answers to questions do not have a determinate status – this indeterminate status is precisely the reason for the controversy. And so to understand the unfolding of events through back-casting and attributing a quality to a position that, at the time, that position *did not have*, is poor sociology. Instead, proponents of the symmetry principle endeavor to treat all claims as equally possible (“symmetrically”) at the time. This approach allows analysts to focus on the social and material relations that support a scientific or technical entity’s societal impact. Clearly, in many situations, the objects of study do have a role to play in the settling of controversies or directions taken by society, but as STS scholars show us, the properties attributed to the objects are significantly influenced by the social/material relations that they are caught up in. With that caveat, we proceed onto the rest of this section. The first social process I cover is that of claiming the discursive resource of the “rational.”

***a) Claiming rational territory***

Steve Shapin (1996), in his book on the scientific revolution states “Nothing so marked out the “new science” of the seventeenth century as its proponents’ reiterated claims that it *was* new. Copurscular and mechanical philosophers...vigourously insisted that their innovations represented radical departures from traditionally constituted bodies of natural knowledge”(p.65). After listing a few of the many aspects of “the ancients” intellectual work that these philosophers did incorporate, however, he came to the conclusion that “the scientific revolution was significantly, but only partially a New Thing. Nevertheless, the rhetoric of wholesale rejection and replacement draws our attention to how practitioners tended to position themselves with respect to existing philosophical traditions and intuitions” (p.68). It seems that claims of exclusive access to superior, divinely inspired rationality has been part of the practice of science since its inception hundreds of years ago. This practice of claiming superior knowledge and knowing practices has been used to inspire populations, support research programs and to develop and deploy more and more elaborate technologies. It has also been used to justify acts of assimilation, imperial domination and environmental destruction.

Tom Gieryn has studied scientists’ use of claims of (ir)rationality in public fora. Expounded in a number of publications (Gieryn 1983, 1995, 1999), Gieryn has developed a cartographical metaphor, that of “boundary work” to describe how science is often rhetorically used in disputes. He focuses his research and metaphor on the effort expended to claim privileged social territory.

Let me illustrate this concept with an example: imagine a dispute over an herbal remedy. Herb X is beginning to become popular among the population. Some want to have it scientifically described and licensed as a drug, while some resist. Health issues are raised. The herb is said to be a risk to people’s health if it is not classified as a drug. Others argue there is no

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report risk, and that it has been a part of people's diets for millennia. Should "X" be a food or a drug? Is it legitimate to sell and use poorly quantified leaves and roots carrying variable amounts of some active ingredient?

Often these controversies also involve struggles over proprietary control. If a scientifically described herb is classified as a drug, and then all future formulations must be standardized in terms of that drug. Some companies can afford to do this process, some cannot. What will be the effect on the current herb production and distribution companies if "X" becomes a drug? What will the effect be on its price and hence, availability to consumers?

In cases like these, the financial stakes can be high, and invariably terms such as "quack", "junk science," "bias," "objective reality" and others enter the conversation. Gieryn proposes that these linguistic battles are not necessarily only about "truth" about the herb, but can also be read as markers for social struggles over who gets to claim certain social "territory." Often both sides of an issue have good science, or valuable knowledge available to back their claims. What follows during the course of the resolution of the controversy is a thoroughly social process of valuing one group's claim over another's. He argues that there is a rhetorical style common to this "boundary work:" the style uses "attributions of selected characteristics to the institution of science for purposes of constructing a social boundary that distinguishes "non-scientific" intellectual or professional activities" (Gieryn, 1983, p.791). The social mechanisms for territorial expansion and contraction are myriad, and each new case study tells a slightly different tale.

He suggests four main activities (Gieryn, 1995) that this boundary work supports and is supported by: *monopolization* – a contest for the authority to define what is truly real, and how to go about determining it, *expansion* – the act of expanding the frontiers of scientific cultural

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report authority into arenas previously claimed by others (such as religion), *expulsion* – determining that an individual (or group) is not scientific and should not be accorded the epistemological privileges of that label, and finally, *protection* – work done to preserve scientific autonomy over the control and distribution of the resources it is allocated.

Thus we have a frame for many of the struggles that we encounter in struggles over natural resource management. In these instances, the promise that science holds – to tell us how things *really* are – is especially strong, since we can all see and agree on what salmon, trees and grizzly bears *are*. Since they are so easily identifiable and countable, we imagine science should “get down to the facts” and “tell it like it is.” Tom Gieryn and others caution us otherwise and point our attention to the (often substantial) social stakes involved in being able to *claim the ability* to know. The victors in this knowledge struggle gain the right to legitimately count how many of *entity x* there really are. From this number, they can also determine appropriate harvest levels, protected areas and so forth.

The factor in this process I wish to emphasize is that by struggling for and then claiming access to “rational” procedures, those who win also *discount* alternative methods. Their ways of going about tabulating the population of grizzly bears, for example, becomes *the way*. Then, the procedural contingencies of their methods disappear, and they begin to speak publicly about “the grizzly bear population.” This claim is made despite the fact that in the practice of creating the number, they do not count actual grizzly bears. This process of making contingencies disappear is called “black-boxing” by Callon and Latour (1981), and is another important activity of those who use scientific rhetoric to buttress social position.

Another example is found in Brian Wynne’s (2000) recent work on public consultation around genetically modified organisms. In it, he engaged in a detailed rhetorical analysis of the

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words used to frame citizens' contributions to the debate vs. those used to describe the scientists'. He noticed that in case after case, public input was slotted into the politically impotent "values and ethics" category, while scientists' opinions were treated as scientific and technical, and thus consequential enough to have policy impact. He mentions two problematic aspects of this practice. First, the value and ethics involved in the advice of scientists passes without scrutiny. As Michel Serres (Serres, 1982) and Donna Haraway (1992) point out, science is already deeply imbued with political, cultural and ethical stances. The invisibility of the scientists' stance, the attribution of the labels "value free" and "objective" to their opinions is a powerful political accomplishment. Second, the "values and ethics" that the public presented had no effect on any substantive decision making about the direction or extent of research. Wynne concluded that the categorization of public input as value-laden and of import for only ethical considerations effectively marginalized the role of the public in the decision-making process.

Once the social territory of the ability to know is successfully conquered, the claim of the right to control often quickly follows. With control, comes the potential for profits from the resource. From this point of view, science and scientific arguments and claims to rationality can be seen as part of actors' struggles over social, or in the case of resource management, actual, territory. Much of the confusion between "dueling scientists" with their contradictory conclusions becomes much more clear when viewed from this perspective.

### ***b) To render natural***

In our society, scientists have the privilege of describing the world of nature. Many STS scholars have examined in great detail the very social conflicts and compromises that precede the declaration of some entity or process as "natural" or "known." It is important, in our studies of

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report natural resource management, to understand some of these processes, as they are at play in many of the negotiations around allocation and regulation of resources. It can only help to have a more nuanced understanding of the social practices which are involved in the creation and dissemination of “facts.” Included in this section are the practices of standardization which structure some of our most taken for granted assumptions about difference and identity.

One account of the kind of work done in order to render something natural is that of Susan Leigh Star (1985) in describing the challenges faced by “localizationists,” scientists in the late 19<sup>th</sup> century who were attempting to establish a map of the brain that would link different neurological functions with specific regions of the brain. This loose coalition of neurologists, surgeons, pathologists and physiologists faced many sources of uncertainty in their attempts to promote their theory. The first was taxonomic uncertainty: determining if a range of symptoms was due to a neurological disorder,<sup>14</sup> and once determined, developing a classification scheme for that disorder. One only has to think of the many different manifestations of epilepsy to understand the uncertainties facing these investigators. Hand in hand with that uncertainty was diagnostic uncertainty: knowing whether or not to refer patients to neurologists for study/treatment. Many physicians insisted on prophylactic treatment of all potential patients for syphilis before admitting them to the neurologists. This six week delay could easily result in the loss of the patient if they had, for example, a brain tumour. Then there was political uncertainty: problems to do with divisions of labour, control over hospital admittance, and access to research funding. The hospital administrators wanted to control admissions based on political or financial criteria, whereas physicians wanted to have full control over admitting procedures, which would ensure that they got to see the most interesting cases. Surgeons, in this time before aseptic

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<sup>14</sup> For example, many of the classic symptoms of brain tumours, such as headaches, dizziness, nausea overlap with many other diseases such as syphilis.

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report technique, and physiologists, in the antivivisectionist political climate, were both professions of dubious status; much of their work was not taken seriously, and in the case of the physiologists who wanted access to animal experimentation, was illegal. The final major source of uncertainty was that related to the limits of their technical approach. Doing experiments on live animals was very difficult; many died while being operated on. The electrode excitation experiments suffered from criticisms that the electrical charge applied had traveled to other regions of the brain. Most surgeries resulted in the subject's death due to infection. Localizing certain neural activity to specific regions of the brain was not a straightforward task. It is important to note that in creating this small list of uncertainties, Star roots her analysis in the work of the researchers, both in the social/political realm, in the natural science realm (that of the bloody brains), and in the technical realm. For her uncertainty is a property of the system.

How did the researchers manage this uncertainty? Star suggests six strategies. The first involved attributing certainty to other fields: "Researchers tended to attribute certainty to other fields: physiologists relied on clinical evidence to supplement their anomalous or uncertain results; pathologists turned to physiological evidence when they could not find evidence for discrete areas.... evidence became interlocked; anomalies were passed between lines of evidence...the theory gained credibility as many fields added evidence. Because of the attribution of certainty across disciplinary lines, it was impossible for researchers to trace a simple path of uncertainty" (Star, 1985, p. 408).

Star calls the second type of strategy "substituting processural evaluations for technical failure." In other words, failure was accounted for through discourses of progression such as "the patient died but the operation was a success." There is a focus on "doing one's best" and "recognition of limitation," sometimes by ignoring the *outcome* of a given procedure" (409).



This kind of rhetorical work can be argued to be re-framing the operation (for example) in terms of the progress of a profession, rather than in term of saving a patient's life.

The third type was termed "ideal type substitution." This was manifest in the production of textbooks and medical atlases that created authoritative representations of the brain where certain regions were responsible for certain functions. The ambiguity and contingency was simply excluded. "The ideal types represented in such maps were sold as context-independent (that is, as *the* brain, not as *a* brain)." (410)

The fourth type of strategy was termed "shifting clinical and basic criteria." I see it as related to the second strategy mentioned above. In this case, criteria for success were constructed around the success achieved. For example, if a tumour removal was successful in reducing a symptom, but was not a successful localization, it was still claimed as a successful example of localization because of the *clinical* success.

The fifth type relates to the first, which focused on distributing the evidence across multiple domains. Star found that through the standard reporting procedures of the day, evidence in a single journal article was compiled from a small number of research sites, and certainties, from say the physiologist were substituted for uncertain claims by the clinician regarding a specific localization. Thus uncertainty again was excluded from the discourse about brain function localization.

The last strategy Star articulated was that of subsuming epistemological questions with internal debates. She outlined two different debating strategies – the first was to collect evidence to refute the claims by the diffusionists (researchers who maintained that brain function could not be attributed to localized structures). Thus "counterpoints to arguments raised from outside the localizationist research endeavour ...served to bury local uncertainties" (412). The other was to

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report engage in heated internal debates about technique, method and so on. “The more localizationists argued with one another about *how* to do, for example, ablation experiments, the less salient the question of *whether* to do them became”(412). This strategy, she argued, shifted focus away from “high-level” uncertainty to more manageable, low level one uncertainty.

I have presented this article in such detail because it provides an excellent example of the kind of work done by those who study the sociology of science and technology. Note that Star does not concern herself with an evaluation of whether or not these early researchers “got it right” by the standards of today’s PET scanning methodology, but rather focuses on how the researchers worked to establish themselves as a credible scientific field. In her analysis she emphasizes both scientific issues and social ones. It is also an intriguing story on the origins of many of the attributes of the brain that we take to be “natural.” Through her detailed analysis of the many social, technical and scientific practices in which these early researchers engaged, she shows us the complicated and contingent nature of the factual elaboration of “natural” objects. The lessons about the outright erasure of contingency and the consequences of such erasure are ones that are very important to keep in mind in many controversies around natural resource management.

In some cases, naturalization strategies fail, or encounter significant resistance. In his famous piece on the scallops of St. Brieuc Bay, Michel Callon (1986) describes a group of scientists who unsuccessfully make claims about the proclivities of scallop larvae. Their alliances, weakened by the claims that did not bear fruit, fall apart, and their conservation strategy fails. Although the scientists are observed to do the same types of work as Star reported, their audience, fishermen who need scallops to fish, do not believe them, and in the end, walk away from the conservation program. In this paper, Callon emphasizes the importance of scientists’ ability to draw people

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report (and things) into alliances where the scientist can be seen as legitimately speaking for others.

Thus “nature” is created through *interessement* (a process of translating one’s project into terms that gains the compliance of the others), association and translation. He also maintains that things and processes are tightly linked. In this case, the scientists’ efforts at enrollment and translation did not hold. Over time, the nature of the scallop proved elusive and out of control of the “natural resource managers” (as did that of the fishermen).

In another paper examining a failure, Jenny Reardon (2001) examines the efforts that genome scientists have undergone to naturalize certain types of human beings, specifically “isolated indigenous populations.” Her paper is a smart commentary on what can happen when scientists attempt to expand their explanatory territory into already heavily “populated” regions. In this case, in their efforts to design a sampling strategy, which meant that first they must come up with a neutral and robust definition of a “population,” the genome scientists, unwittingly, it appears, stumbled into ongoing deeply contested social fields. For example, they failed to realize the problematic relations that lay in divining a biological basis for race difference. They ran into trouble defining population: was it defined by “geography, language, culture, or biology? Does population also refer to an identifiable sociological group?” (Reardon, 2001, 362). They argued over how to measure relatedness – through nuclear DNA or through mitochondrial DNA? Should they use anthropologically-informed sampling or use a “grid” approach?

They convened a special meeting that included anthropologist, linguists and archaeologists to help them out. The workshop broke into three sections: an American Indian section, an African section and an Indo-Pacific section. Each section returned markedly different criteria for determining population. The situation became more complicated when the Indigenous people themselves became aware that they were to be “sampled.” An international campaign was

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report launched against the genome project. The project became entangled in post-colonialist political issues. The scientists had also not taken into account the collective identities of many indigenous people. The idea of one person being able to give consent for someone else to remove part of his body is foreign to many of these cultures. As the article ends, the project is now mired in negotiations about who defines a “group.” In many legal precedents, group membership is defined by the group members themselves. Under this scenario, scientists have no right to classify these people without their consent or permission.

Reardon’s article is very instructive. She tells the story of the conflicts in which a group of typically reductive scientists become involved in when they attempt to taxonomize a population that can talk back and engage in cultural critique. Her article shows how the cultural assumptions of scientists come to the fore when they are dealing with highly intractable situations. This is also useful for us when we think about the resource management conflicts in Clayoquot Sound. First Nations people readily tell stories of exclusion from the knowledge arena, even in areas where their knowledge is very strong. Work like Reardon’s helps us to clearly see some of the cultural underpinnings of our taken for granted naturalizations.

### ***c) Science, policy and the environment***

Questions of policy about the natural world are very important these days. Whether it is water exports, global warming, biotechnology or toxic waste, the magnitude and complexity of the issues are daunting. In this complex scenario it is no wonder that policy makers turn to scientists for some semblance of surety<sup>15</sup>. The STS literature addresses many aspects of the

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<sup>15</sup> In today’s news is a bill put forward in the US congress that seeks to put science “at the beginning, middle, and end of the agency’s decision-making process” and to institute a special science director to prevent ensure that the “the scientific basis for EPA’s regulatory decisions must be beyond reproach.” (ENN, 2002) at [http://enn.com/news/wire-stories/2002/05/05012002/ap\\_47079.asp](http://enn.com/news/wire-stories/2002/05/05012002/ap_47079.asp). This move is being done to ensure that the

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report science-policy relationship. Some studies explore the value of public participation in setting scientific agenda (Guston, 1999; Rowe and Frewer, 2000). Some show how activists influence and or carry out scientific research (Epstein, 1995, 1997; Helford, 1999; Eisenhart, 1999). Brian Wynne (1996), in his now famous study on the conflicts between government scientists and farmers following the radioactive accident at Chernobyl, demonstrated the deep challenges to personal identity that are implicated when government-mandated science is imposed on a rural community. And there are numerous articles about, how, in the face of policy-makers' often scientific<sup>16</sup> expectations of science, scientists creatively manage uncertainty to keep an appropriate position in the discourse (Campbell, 1985; Wynne, 1996; der Sluis et al., 1998).

This section will be brief. Many of the papers I mentioned above either elaborate points already covered in the sections on boundary work and naturalization, for science in the service of policy is used amply for those two purposes. I will also avoid further mention of uncertainty management, although the authors do contribute more subtle points to the project as introduced by Star (1985); I think my readers have heard sufficiently about types and management strategies to gain a general feel for how the subject is articulated in this discipline.

I will focus instead on the now classic case study by Brian Wynne who investigated the relationships between institutional experts and rural "lay persons" during the course of the English government's response to radioactive contamination of sheep-grazing lands as a consequence of the Chernobyl disaster in the Ukraine. This study elaborates some themes that are disappointingly familiar to many residents of rural communities – experts come in from far

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EPA's decisions have enhanced clout with decision-makers. "The EPA's work is too important to suffer from poor perception," the article quotes a sponsoring congress member. The solution they choose is a scientific regime that is more segregated from the constituencies it answers to, in order produce better science.

<sup>16</sup> Scientism is the belief, or faith, in the ability of the scientific method, with scientists as intermediaries, to unveil Truth, which will then instruct mortal humans how to behave or formulate policy.

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report away, assess the situation, make assertions of great certainty, demonstrate ignorance of important local social and biophysical processes, and mandate, with the force of law behind them, prescriptions that ignore local understandings and threaten local identities. These prescriptions turn out to be disastrous to locals' livelihoods and do not deliver the effect that the experts predicted.

Writing in the context of a book confronting the issue of “public understanding of science,” Wynne’s careful analysis explored issues of epistemology and power – whose knowledge counts when important decisions are made? Local citizens are often characterized as “scientifically illiterate” and unable to engage in technical decision-making. Wynne argues to the contrary. His paper suggests that much of the local “ignorance” can be attributed to a clash between two cultures, and unacknowledged interpretive framework that local residents bring to bureaucratic scientists. Through this framework, the scientists are often judged by the local citizens as not legitimate knowledge practitioners. The scientists then encounter local resistance, which they often interpret in terms of local ignorance.

Below is the table summarizing the lay criteria for judgments of science, created through the analysis of in-depth interviews with affected farmers. These criteria can all be used to explain why lay people “fail to understand” scientific assurances and problem descriptions involved in environmental controversies.

The seven criteria (with my comments following “e.g.”) are (Wynne, 1996, p. 38):

- Does the scientific knowledge *work*? e.g. do predictions hold true?
- Do scientific *claims* pay attention to other available knowledge?

- Does scientific *practice* pay attention to other available knowledges? e.g. do institutional scientists design experiments that local people know will not work due to ignored local contingencies?
- Is the form of knowledge as well as the content recognizable? e.g. do the degrees of claimed certainty and standardization of landscape features fit with local understandings?
- Are the scientists open to criticism? e.g. can local actors have an effect on subsequent scientific research by challenging the present research?
- What are the social/institutional affiliations of experts? e.g. what is the previous trustworthiness of the institutions for which they work?
- What issue “overspill” exists in lay experience? e.g. whereas research scientists may see well-delimited problems, residents see issues in term of long-term relationship with a certain institution, DFO for example.

From this checklist, we see that when faced with scientific knowledge claims, especially about issues central to their livelihoods, local people bring a rich interpretative framework to bear. They assess these claims in terms of the quality of their relationships with the scientists, with the history of the institutions for which the scientists work, and the efficacy of the scientific prescriptions “on the ground.” Thus they use cultural and epistemological frames of reference.

The residents in Wynne’s study were also very aware of the areas in which scientific research was not being done, for example, in the non-testing of their drinking water for radioactive contamination. The link between this lack of knowledge and the political consequences of generating the knowledge was very plain to them. From their point of view, it was too politically

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risky for the government to determine the presence of radioactivity in their water. It would open up a series of issues that the government did not want to engage in.

Wynne's "take home message" from this paper was that the science that was performed in this setting was inextricably wrapped up with political contingencies, and that widespread public resistance was not a result of ignorant farmers not understanding the science. Instead he argued, this was a case of the cultural assumptions and practices of a type of science, science within a bureaucracy, conflicting with those of the farmers, which led to resistance and "failure to understand."

In particular, Wynne noted five assumptions of the bureaucratic scientists that challenged not only the knowledge, but the identity of the local farmers:

- That the natural (and achievable) purpose of knowledge was control and prediction; (farmers had more contingent and flexible stance toward knowledge)
- That standardization of environmental measurements and concepts over given areas and social units was natural even though it imposed standardization on the social units too;
- That uncertainties in scientific knowledge could be contained within the private discourse of the scientists and would be misunderstood if disclosed in public;
- That local knowledge was effectively worthless;
- That scientific methods of research could fully simulate realistic-farming conditions as practices, transmitted and valued in hill-farming culture.

Wynne's paper reminds us to carefully consider the full suite of assumptions that scientific and technical workers take into the field when they attempt to measure and regulate natural



Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report resources. It cautions us to value local information, and be aware that resistance to “scientific” concepts may not just be due to lack of understanding, but one of many relevant cultural differences.

This type of study is particularly important in situations like Clayoquot Sound, where cultural battles that still bear the mark of colonialism are ongoing. As previous papers in this section have shown (Reardon, 2001), science as practiced by large institutions bears many of the cultural trappings of domination and control that indigenous people in particular are very sensitive to. Writers such as Brian Wynne argue that it is not necessary to abandon scientific action, but rather, to allow local knowledge to inform the scientific activity, and to acknowledge and address the institutional histories embedded in the context of the science. The result, a sort of “hybrid” scientific practice, will be discussed in the section after next.

#### ***d) Science and the law***

Science and the legal system have a strained relationship. As an American judge said: “there are important differences between truth in the courtroom and the quest for truth in the laboratory. Scientific conclusions are subject to perpetual revision. Law, on the other hand, must resolve disputes finally and quickly.” (Solomon and Hackett, 1996). The courts have a schizophrenic relationship with science. On the one hand, in order to deliberate with any certainty about a topic that fits into science’s explanatory realm, they must buy into the doctrine of science’s ability to answer questions definitively. Yet their disputational proceedings provide fruitful sites for all the contingencies and internal divisions within science to be prominently displayed. In one way, this struggle by the courts can be viewed as the modernist project confronting one of its most deeply held myths, that of objective truth, through another treasured myth, that of impartial

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report justice. And unlike community or environmental science, an inclusive, adaptive form of knowledge assessment is ruled out by the deliberative and punitive function of the justice system. The courts must work within the bounds of the modernist settlement, constructing certainty out of evidence of which they can never be certain. STS studies of the justice system focus on how the courts struggle with this tension.

I will focus on three papers that take up this struggle: one, by Michael Lynch (1998), shows how the highly paid OJ Simpson defense “Dream Team” engaged in research very familiar to STS scholars, with similar results, and one key difference. A second paper, by Solomon and Hackett (1996), describes the different ways in which the US court system has sought to rectify the tension between science’s claims of certainty and the reality of science’s internal divisions, methodological limitations and contingent findings. Finally, Edmonds (2001) proposes that we do away with the struggle across science/law boundaries, with its unproductive rhetoric of “junk science” and move to conceptualize a hybrid law:science, whose practices and conclusions are shaped by its own unique cultural milieu and demands.

Michael Lynch writes a paper that expresses bald, tongue-in-cheek envy<sup>17</sup> at the research resources of the lawyers who were hired to defend OJ Simpson (the “Dream Team”). His article describes the methods that the “Dream Team” used to discredit the DNA fingerprinting technology that was being used to implicate Simpson in the murder of his wife. Lynch describes how the defense lawyers used research methods and lines of argument very similar in kind to those used in the STS literature. They visited labs and observed science in action, made

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<sup>17</sup> “the prosecution and defense teams conducted their own investigations and (not surprisingly, given the resources available to both sides) their efforts were impressive...as ethnographies of such a controversy.” (p. 833) and “far from requiring assistance from expert social scientists, the Simpson team’s “ethnograph” already possessed a high degree of access and epistemic authority, a vast documentary archive and a fund of financial and human capital.”

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report themselves familiar with the disputes and contingencies regarding the technology under consideration, and integrated the larger social issues into the normally straightforward account of fact making. This “constructivist” approach was used to de-stabilize the prosecution’s “realist” argument that the technology was not problematic and provided direct evidence that Simpson had been the murderer.

The defense was able to document the many contingencies and human factors involved in the handling and processing of Simpson’s blood samples. In that way, their work was similar to STS scholars’. In one major way, however, it wasn’t. Unlike STS scholars, who assume that this day-to-day messiness is an always present and inescapable part of routine scientific activity, the “Dream Team” lawyers argued that it was symptomatic of poor practice and generated unreliable findings. They maintained the image of a Pure Science in order to discredit the one they were attacking.

Then, as Lynch tells us, they introduced a cultural trump card: the racist reputation of the Los Angeles Police Department (LAPD). Arguing to a mainly African-American jury, they introduced a damaging cultural context to the prosecutions’ evidence. As Wynne (1996) emphasized, lay people take institutional alliances into consideration when they interpret scientific information.<sup>18</sup> Against this contingent, culturally embedded picture, the prosecution’s portrayal of an unproblematic, objective and direct access to reality through the technology of DNA finger printing could not stand up. OJ Simpson was declared not guilty.

I describe this article in order to show that STS analysis “works” in arenas outside of academia, and also to demonstrate a classic example of how even routine, straightforward

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<sup>18</sup> As do scientists. All journal articles contain references to where the scientists are employed, and now the prestigious journal Nature now has another designation to indicate affiliations with corporate or non-profit funders.

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science can be reduced to shoddy, questionable and unreliable practice through the right types of research and questioning strategies. This is most problematic aspect of science's relations with the courts (and indeed policy): its unacknowledged locality. That is, within its own community of practice knowledge claims are less problematic because workers are familiar with the embodied lab practices and approximations and know how to incorporate them into their knowledge claims so they effectively disappear. Thus a community has a generally agreed upon narrative practice. When the scientific practices (such as DNA fingerprinting) that generate its confident claims are transported to a different arena (such as from biomedical research to forensic science), these social practices become apparent ("made strange") as *social practices per se*. The "erasures" of embodied experience and other forms of boundary work then do not seem so commonsensical. Tension then ensues, as people from other communities of practice attempt to grapple with the contradiction between scientific discourse, rooted in the familiar modernist equations of control, certainty and prediction, and its practice, which rarely has external guarantees of its freedom from bias and particularity.

Thus, in the Lynch paper, we meet one strategy of confronting science's limits to certainty – using them to discredit knowledge claims grounded in a particular scientific practice at a particular site. In the next paper, we examine another strategy for dealing with the inherent uncertainties of science in the courts – through strictly defining boundaries of admissible "scientific" evidence.

The case of *Daubert v. Merrell Dow Pharmaceuticals, Inc.* ("*Daubert*") became a watershed case for the use of science in the courts. The plaintiffs, Jason Daubert and Eric Schuller had been born with "reduced" limbs. Their case against Merrell Dow sought to establish that an anti nausea drug, Bendectin, taken by their mothers during the mothers' pregnancies, had caused

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report their birth defects. This case, like many others before it, was a type of case called a “mass tort,” which are “large-scale lawsuits involving a number of plaintiffs suing wealthy defendants... for alleged injury or damage sustained through some product or activity” (Edmond and Mercer, 2001, p. 265). *Daubert* became famous because it prompted the Supreme Court of the United States (“the Court”) to revisit the rules for admissibility of scientific evidence in such cases.

The previous rule, known as *Frye*, had been established in 1923, and although commented upon in a 1975 congressional act, had been the mainstay of the American judiciary in these types of cases (Solomon and Hackett, 1996). Under *Frye*, the plaintiff was required to show that the scientific techniques or testimony s/he was using to plead her case must be “sufficiently established to have gained general acceptance in the particular field in which it belongs” (Solomon and Hackett, p. 135). This rule’s applicability was being challenged by the complex cases being presented before the courts in cases such as *Daubert*.

For example, the lawyers for *Daubert* presented four independent lines of evidence for their clients: *in vitro* studies (showing the appropriate effect on cells grown in culture), molecular structural arguments (similar molecular structure to known teratogens<sup>19</sup> suggests similar properties), animal studies, and re-analysis of previously published epidemiological data (meta-analysis). Each of these fields has their own controversies over what results mean; each field has their explanatory strengths and weaknesses. Which science would be the proper one to believe? The “general acceptability” standard would be of no use in context of the multiple connections that the plaintiffs were presenting.

In building their case, I imagine that the lawyers and expert witnesses engaged in similar cross-discipline uncertainty management as the brain researchers in Star’s study (1985), creating

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<sup>19</sup> A *teratogen* is a substance that causes birth defects – literally “monster causing.”

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an interlocked field of evidence where a single path of uncertainty would be impossible to trace. The judge's response to the array of scientific data was to deny the experts this essential rhetorical privilege. Instead, he claimed that only peer-reviewed epidemiological data would be judged admissible. Thus even the plaintiff's re-analysis of published data, performed by credible experts, was inadmissible, as it had not been published in a peer-reviewed publication. From the plaintiff's challenge to this ruling, the *Daubert* guidelines emerged. These rules are important to us because they are the opinion of one of North America's most influential decision-making bodies on what counts as admissible evidence. Their comments will influence what can be considered "good science" in hundreds of courtrooms.

What, is "good science" under the *Daubert* guidelines? First, good science is falsifiable, or testable. It engages with hypotheses that can be tested and proven false. Second, the court acknowledges that peer review and publication is a relevant concern, but should not be a standard for admissibility. Third, the Court suggested that the judge keep the known or potential error rate of a scientific or technical procedure in mind when interpreting claims. Fourth, the general acceptance of a scientific technique or theory may be taken into consideration when weighing evidence's admissibility. The Court framed their recommendations as "flexible" and emphasized that "Its overarching subject is the scientific validity of the principles that underlie a proposed submission. The focus, of course, must be on the principles and methodology and not on the conclusions that they generate" (Solomon and Hackett, 1996, p. 142).

When we compare this last statement to the rhetorical work done by the turn of the century localizationists who adapted their criteria for success around the type of success achieved, we can begin to foresee some of the difficulties that will continue to plague the use of science in the courts. By ignoring the ways in which scientific facts are built and the messy social structures

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report which support them, the Court has contributed to the persistence of an untenable expectation of science which will perpetuate the awkward fit between those who seek truth and those who seek justice.

Finally, Edmond (2001) suggests that we abandon the idea of awkwardly fitting science into a legal framework and instead wholeheartedly embrace a hybrid practice, that of “legal science.” This type of hybrid practice embraces the notion of co-production – where we see a legal discourse grounded in the discussion and evaluation of scientific data and practice, and similarly a scientific discourse that is constrained and mobilized in order to be effective in a legal context. In Edmonds’ words, these resulting law-science knowledges are “a unique combination of scientific knowledge, forms of legal procedures and practices, non scientific knowledges, and social mores, the shaping and strictures of legal procedures that combine to produce outcomes represented to be (and generally perceived as) socially adequate” (Edmond, 2001, p.198).

From this perspective, the law-science tension dissolves. Placing expert testimony within the context of the disputative courtroom, constrained by judges who, for example, remove scientists’ normal means of fact-building, ensures that science as presented in this forum will always be different from that in the lab, and certainly from that in the textbook.

As an example of this hybrid knowledge in action, he analyzes the court transcripts of a nineteenth century English sodomy trial. In this case the conviction of the accused relied on medical testimony about the nature of anal scars observed on him. The expert witnesses called upon had virtually no experience in interpreting signs such as these scars. The extent of medical ignorance about the physical symptoms of sodomy, which intuitively might be taken as an obstruction to justice, were instead heralded as a sign of English virtue. This was indicated by the summing up of the Lord Chief Justice at the end of the trial: “Happily that [ignorance of

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report experience in interpreting anal scars] is the case with all the medical gentlemen, therefore that vice has not yet tainted the habits of the men of this country – for that thank Heaven” (Edmond, 2001, p. 214). Indeed disavowals of studying “repugnant” cases, Edmonds argued, helped maintain the expert witness’s claim to legitimacy. Admittedly an extreme case, this article nonetheless dramatically shows that what counts in a courtroom is the cultural – both in terms of legal quirks such as deferring to precedent<sup>20</sup>, and in terms of appealing to the imagined values of the jurors – context in which science is presented.

This section, though long, has been only a cursory overview of the wealth of studies of how science participates in society. I began with a theme that resounds through much of the STS literature – the boundary work that distinguishes and often (though not always, in the case of the courts) privileges scientific knowledge over other forms of knowing. The second section gave a brief overview of some of the ways that scientists work to naturalize their results, that is, turn their contingent, incomplete work into direct comments on the nature of things. I also highlighted cases where this work was resisted and failed. Next, the use of science in environmental policy was examined from the perspective of a social account of local resistance to imposed policy decisions. Science was discussed as entwined with many other complicating factors including identity and institutional histories. Finally, I reviewed a number of papers that examined how science was enacted in legal settings, focusing on a paper that detailed how the United States Supreme Court attempted to ameliorate the conflicting claims of scientific certainty with its practiced heterogeneity, contingency and incompleteness. As an overview of the scholarly work of science in general society, I hope to have convinced the reader that science is always deeply intertwined with institutional mandates, and cannot travel unaffected through social space,

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<sup>20</sup> Another example of that was the original *Daubert* trial judge’s refusal to admit evidence that was not epidemiological and peer-reviewed. His decision was based on precedent; one for which no rationale was given.



Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report generating objective, value free knowledge in its wake. I, with many of the authors cited, do not view this as a bleak claim. Instead, it is a call for a revitalization of the scientific project, a breaching of boundaries, and a commitment for working with different kinds of knowledge on a common project. In the final section, entitled “hybrids,” I explore some conceptions of what this might look like, or how we might talk about it.

## **VIII. Hybrids**

Hybridity has worked itself into a number of the previous sections of this review. It has been suggested as a way out of the modernist divide in several distinct areas. In this section, I will elaborate on the different calls to hybridity appearing in the literature, spending the most time on the ones that seem most important to this project: hybrid objects, hybrid discourse, hybrid disciplines and “boundary organizations.” Each are different *types* of hybrid, bringing together differently different things or communities.

The first hybrid mentioned in the STS literature are the hybrid objects as proposed by Latour and Callon (1981); Latour is often cited as their primary champion. I suspect, however, that these hybrid objects are modeled on Serres’ notion of “quasi-object,” which was introduced prior to their exposition by Latour (Serres, 1982b). A quasi-object, to Serres, is an object that has or imparts significant social meaning – “this quasi object is not an object [as in subject:object relation], but it is one nonetheless, since it is not a subject, since it is in the world; it is also a quasi-subject, since it marks or designates a subject, who, without it, would not be a subject” (Serres, 1982b, p. 225). Examples of quasi-objects are the button in “button button, who’s got the button?,” and a soccer ball in the context of an ongoing soccer game. Serres explains – “he who is not discovered with the furet [button] in his hand is anonymous, part of a monotonous chain

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report where he remains undistinguished.... If he is discovered, he is “it.” Who is the subject, who is an “I,” or who am I? The moving furet [the button passing between the players] weaves the “we,” the collective; if it stops, it marks the “I.”” (p. 225).

Through his analogic reasoning, he argues for objects’ central role in the creation of subjects’ identities. We are all familiar with this conception through ideas like “status symbols.” Serres links objects, particularly ones that are traded and circulated through groups of people, to the identities of both the individual and the collective, thus breaking the important modernist subject:object divide. The neat distinction present in so much modernist thought falls apart when we consider objects that structure and are implicated in human relations. As you may recall from earlier sections, this intellectual work then paves the way for the blending of social and natural worlds that STS scholars such as Latour and Callon developed more empirically. It also sets up argumentative conditions for describing social phenomena in terms of assemblages between humans and non-humans.

Latour and Callon develop the socially active role of “non-humans” (as they call them) in a number of ways. Latour discusses the development of scientific ability, sometimes ascribed to the “raw intelligence” of the investigator, to the array of instruments, databases, laboratory notebooks and so on that support the scientist’s ability. In a more whimsical mood, he writes essays about the how society delegates important moral tasks to non-humans like speed bumps and automatic seat belts (Latour, 1992). In another interesting essay, Callon and Latour analyze power in terms of the non-humans that an agent can count on. The difference between power relations among apes and humans, they argue, is that the apes must constantly re-negotiate power relations, whereas humans contrive scores of non-humans – from gated communities to pepper spray to smart bombs – which stabilize existing relations of power (Callon and Latour 1981;

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report (Latour 1996). Thus from their perspective, there is little difference between our modern society and an indigenous one that hold certain objects as sacred or imbued with special powers<sup>21</sup>.

This is our first example of hybrids discussed in the literature: hybrid objects, whose materiality also mediate and structure human relations. An example noted earlier was that of culturally modified trees, but we can think of many other natural resources that are much more than “just fish” or “just seaweed.” *As* fish and *as* seaweed they mediate numerous social relations as diverse as traditional ecological knowledge transfer from elders to young, the reconfirmation inter-family relations, and building structures to preserve the fish or seaweed. This insight into the socially vital aspect of *things* allows us to clearly distinguish the positions of those who seek to negate the role of things in any other networks of meaning but their own, such as a forester who talks about a forest in terms of standing board feet. We now can see this type of discourse as boundary work made possible only by that forester’s particular social location.

A second type of hybrid is more germane to our future discussion – the discursive hybrid, or hybrid discourse. Bakhtin wrote about different languages as *intersecting* in social situations that brought together different social groups (Bakhtin, 1981b). The result, he argued, were hybrid languages, ways of conceiving of the world that, as combinations of previously isolated ways, were new entities, irreducible to their progenitors. Bakhtin considered this intermingling essential for a society’s creative evolution.

All this [mixing] set[s] into motion a process of active, mutual cause and effect and interillumination. Words and language began to have a different feel to them;

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<sup>21</sup> In fact, a recent development in our political arena involved the expulsion of a member of parliament for grasping and wielding the mace – the symbol of the Crown’s presence in parliament. The story is made more interesting by the fact that the member did the act in dialogue with this symbolic function of the mace in order to make a point about the plight of democracy in our national system of governance. This is a classic example of an object – not just an object, but a focus of rules, meaning and probity – mediating a very clear expression of meaning in a way that a purely verbal statement of resistance could not.

objectively they ceased to be what they had once been. Under these conditions of external and internal interillumination, each given language--even if its linguistic composition (phonetics, vocabulary, morphology, etc) were to remain absolutely unchanged--is, as it were, reborn, becoming qualitatively a different thing for the consciousness that creates in it. (Bakhtin, 1981b, p.12)

Hybrid language as a site of emancipatory learning is treated in the work of Gutierrez, Baquedano-Lopez, and Tejeda (1999) who study language acquisition by young bilingual chicano and chicana children. Proposing a “third space” of hybrid language between the “authoritative” and “slang” discourse, the authors argue that children learn more complex language repertoires when they are not censured for using slang, but are engaged at the informal level by teachers who then also incorporate formal statements (Gutierrez Baquedano-Lopez, and Tejeda, 1999). This argument is supported with transcript analysis coupled with narrative about the student’s participation in the classroom. In a different publication, hybrid language is also used – in this case, literally a mixture of Spanish and English – to demonstrate the heightened complexity and nuance available to children who are allowed to use both languages in order to communicate with their instructors (Gutierrez, Baquedano-Lopez, Alvarez, and Chiu, 1999). This practice is contrasted favourably to the usual one of forcing the children to communicate in the formal English of the school.

This qualitative research supports the point that the children, through interacting with bilingual instructors, develop more sophisticated communication repertoire than they would in English alone. The student whose e mails are analyzed shows a deliberate switching between English and Spanish, and formal and informal Spanish, as meaningful and meaning-making activities in their own right. The student can share jokes and cultural references with the instructor that would be impossible in “English only” discourse. Their interchange thus becomes

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report full of external references, word play and switches between languages that have meaning in and of themselves (Gutierrez, Baquedano-Lopez, Alvarez, and Chiu, 1999). Through engaging students in this kind of hybrid language use, Gutierrez argues that they extend their linguistic and expressive abilities in ways impossible in a mono-lingual classroom. Thus the linguistic hybridity allows for new and richer communication that is literally impossible in a non-hybrid situation.

The process adopted and reports produced by the Clayoquot Sound Scientific Panel are examples of cultural hybrids. Both the reports and the process adopted elements of First Nations' philosophy and traditional knowledge and incorporated them together with the panel scientists' ways of interacting and describing the world. The result was a report that could easily be described as a "hybrid document." The Western science was presented in a framework that honoured the needs and values of the Nuu-chah-nulth peoples. Their values influenced what the science was used for, and how decisions about natural resource management would be made.

The third type of hybrid that is relevant to this discussion is that of "hybrid disciplines." An example that has been covered in detail was the "law:science" hybrid profession suggested by Edmond as a way of advancing thinking about the use of science in the courts (Edmond, 2001). His hybrid discipline was characterized by the constraints of the location in which it was being performed. Thus science in the courts cannot be expected to be the same as science in academic labs because the two social worlds are different. Similarly, science in policy, science in industry, science in medicine and so on will have a different character, depending on the mixtures of the social worlds, or communities of practice or networks of relations involved.

This conception of "hybrid disciplines" allows us examine the activity of the Scientific Panel as an instance of a new hybrid formation rather than an aberration or dilution of "pure science."

We can ask questions about the nature of the Panel, their deliberations and reports coming from the analytic position of examining the mixtures taking place in a legitimate and novel circumstance. We free ourselves from expecting any one way of knowing to be dominant or present. We also free ourselves from the deficit model, where the Scientific Panel is measured against either pure scientific work or pure traditional knowledge and found to be lacking. Instead we can work to elaborate the emergent properties of this new hybrid discipline, irreducible to neither of its progenitors.

The final major occurrence of the concept of hybridity that has relevance to this paper is that of “boundary organizations.” This is a term coined by Dave Guston (2001), which he used to describe organizations whose explicit role is to bridge the social worlds of science and politics. He argues that boundary organizations are active sites for co-production, which, in his words, is “the simultaneous production of knowledge and social order” (Guston, 2001, 401). They do this by facilitating collaboration between scientists and non scientists, and by being sites where boundary objects and other organizing entities are produced and used. As examples, he cites the American institutions of the Office of Technology Assessment (OTA) and the Health Effects Institute (HEI). Both institutions, he writes, were supported by clientele on either side of the political fence, or in the case of the HEI, by both industry and government. By being organizations with scientific mandates in highly charged political territory, they came up with solutions to ameliorate the needs of the political and scientific communities to whom they were responsible. Using the terminology we are developing in this article, they could be said to be engaged in hybrid work, explicitly creating technical and scientific representations, alliances, and boundary objects that fulfilled the needs of the political communities of practice they served.

In a special edition of *Science Technology and Human Values* dedicated to fleshing out the notion of boundary organizations, Clark Miller (2001) takes the concept a step further and introduces the term “hybrid management.” He uses the U.N. Framework Convention on Climate Change’s Subsidiary Body for Scientific and Technological Advice (SBSTA) as a case study. The mandate of the SBSTA is both scientific and multicultural. It is the body responsible for “manag[ing] and interpret[ing] the production of scientific knowledge and its incorporation into policy” (Miller, 2001, 479-480). Because the SBSTA operates by consensus, scientific knowledge and its attendant social order must satisfy the demands of all member countries. Thus it is also an organization explicitly engaged in “co-production.” Hybrid management can be seen as a productive process leading to successful boundary management, that is, negotiating the different political and scientific worlds without taking recourse to the aggressive “boundary work” detailed by Gieryn (1999). Miller (2001) argues that there are four activities characterize hybrid management.

The first is *hybridization*. He uses the term to mean the bringing together of many different values, norms and needs of regulators, policy makers and diplomats of the participating countries along with the issues surrounding technical questions. The second activity is *deconstruction*. By deconstruction he means the opening up of previously “black boxed” or “naturalized” knowledge claims in order to expose the “tacit and often value-laden assumptions embedded in their construction” (Miller, 2001). This is a key aspect of the SBSTA’s work because, as we have noted in earlier sections, science, by claiming “value-neutral” territory, often disguises its relations to power as merely knowledge claims. Many former colonies are very sensitive to these issues. The third aspect of hybrid management is maintaining appropriate boundaries. This term means actively managing the jurisdictions and responsibilities of the different political and

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scientific parties involved in climate change research, defining what category – political or scientific – different questions belong to and so on. This work of maintaining appropriate boundaries is important in the delegation and organization of the work involved and important in presenting the SBSTA as a credible body to those to whom it must answer. The fourth activity involved in hybrid management is cross domain orchestration. This activity involves ensuring that the two social worlds – political and scientific – are responsive to, and perceived as legitimate by the other party. Thus, the scientific advisors must be certain that their advice is being taken up by appropriately structured political process, and the politicians must be sure that the scientists are asking and answering questions that are relevant to their needs. Hybrid management, therefore, involves the explicit negotiations of aspects of science in society that so often are ignored and replaced with the rhetoric of rationality.

Clayoquot Sound has seen a proliferation of organizations explicitly working at the interface between Western science-informed resource management and First Nations' practices. Institutions such as the Central Region Board, Regional Aquatic Management Society and Iisaak all can be considered hybrids involved, more or less explicitly, in co-productive work. In the following half of this document, some of the social changes accompanying the epistemological ones will be noted.

Science and Technology Studies has found the notion of hybrid a very fruitful one. We have reviewed four contexts in which it has been used: social:material hybrids, discursive hybrids, disciplinary hybrids and hybrid organizations. This focus on hybridity comes as a direct theoretical consequence of the refusal to reduce something to something else, but instead trace the alliances, assemblages and trajectories that the different irreducible mixtures engage in. Analytically, they allow us the ability to articulate the often strange and surprising



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social/material arrangements in which people and organizations involved in technical work find themselves. This concept along with the notion of coproduction will be central in the analysis of the reports of the Clayoquot Sound Scientific Panel.

## Appendix 1

A sample of M. Serres' writing. This paragraph is part of a book-length essay which treats the subject of human relations – communication, interruption, domination, invention and many other topics, all made to swirl around the tripartite notion of host-guest-parasite (as either human/social parasite, s/he who gains material without paying, biological parasite, or, a third definition, French, which means noise, or static). Clearly there are relations to biblical, microbiological, thermodynamic, economic and geographic issues woven into this text. Note the turbulence and bifurcation in the text itself.

From *the Parasite*, p.156. [in this passage, referring to the Biblical story of Joseph, where Joseph successfully interpreted the Pharaoh's dreams and saved Egypt from starvation during a time of famine]

This is perhaps the first treatise of political economy. Fat cows: years of abundance; thin cows: harvests of scarcity. When there is an excess harvest, the usual practice is to get rid of this surplus by lifting the bar. And then they die of hunger during the years that the cows are thin and the stalks of wheat are burnt by the wind. What else could be done? We must return to these simple peasant practices from which all of culture came. Here are abundant fruit, vegetables, milk, wine, wheat. The fruit spoils, the milk sours, the wine turns to vinegar, the vegetables rot, the stores of wheat are filled with rats and weevils. Everything ferments; everything rots. Everything changes. Rotting and plague are not only symbols of violence but also real, singular

Hybrids: A Literature Review and Analysis of the Clayoquot Sound Scientific Panel Report referents that only need themselves to give rise to clearly defined processes. The surplus is gotten rid of because it is perishable. In fact the rotten is expelled, merchandise is disposed of (*ecouler*), because it might start to run (*couler*). Exchange is born in that change of state. Exchange is to this change what excess or surplus is to sufficiency, or exaction to action, and so forth. Exchange does not want it to change. It wants to stabilize the flight (*fuite*). Contrary to everything thought about exchange, it does not mobilize things; it immobilizes them, it disposes of them, everything flows, of course, everything dies, everything rots, if it dies, it bears much fruit.<sup>22</sup> What runs (*coule*) is disposed of (*ecoule*); what changes is exchanged. The very simple idea of the equilibrium of exchanges is ontological. By the very movement of the exchange, what changes, no longer changes. It might have become rotten, and now it is money. The fact that money is refuse or feces is not at all a symbol or a fantasm. It is exactly the substitute of the expelled rot, the equivalent of disposal by corruption. The stroke of genius, of course, was to go look for the stable in the unstable, or rest in movement, to go look for what is opposed to change in the exchange itself.

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<sup>22</sup> (John 12:25 – a common French saying)

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