Crossing the Border:
On the Relationship Between Science and Fishermen's Knowledge in a Resource Management Context

Petter Holm
Norwegian College of Fishery Science
petterh@nfh.uit.no

Abstract The relationship between science and policy within modern fisheries resource management has been built on the idea of a neat separation of nature and policy. It would seem that the current crisis in fisheries management and fishery science also features a challenge to this idea. This paper takes a closer look at one of the discourses in which the issue of science within management has been opened up for debate: That which mobilizes fishermen's ecological knowledge (FEK) as a complement to science within fisheries resource management. In the FEK projects, fishermen and their knowledge are defined and mobilized in a particular way at the same time as fishery science is reconstructed as an interested user of that knowledge. As we shall see, however, the FEK argument is highly ambiguous. On the one hand it can be read as a radical challenge to 'orthodox' fishery science and its monopoly in establishing the facts of nature. In this interpretation, the FEK argument points towards a stronger contextualization or democratization, in short as a promise of mode-2 type science, as theorized by Nowotny *et al.* (2001). On the other hand, FEK research practices suggest that a major thrust in the attempt of to make fishermen's knowledge speak to scientific issues is a process of radical decontextualization, in which valid knowledge bits are mined from fisher lore by a process that cleans out all cultural and political baggage. In this interpretation, FEK does not represent a move towards contextualization of science, but is a return to Mode-1 ideals.

Introduction

The relationship between science and policy within modern fisheries resource management has been built on the idea of a neat separation between nature and society. Whereas fishery science (marine biology and oceanography), has held a privileged position in establishing the facts of nature (the size and condition of fish stocks), the user groups and other interested parties have been allowed some say in the hammering out of a policy based on those facts (the measures affecting the social distribution of catches/revenues). Fisheries resource management may be a meeting place for nature and society, but the two of them are treated as strictly separable and as governed by fundamentally different processes.

The theory of science and its relation to society that we recognize in fisheries resource management is a familiar one. It is the same as we find in Merton's 'ethos of science' (Merton 1996), according to which science must remain pure and strictly autonomous. Science can reach the truth only if it is set securely off from all cultural and political contingencies. This notion of science, which has a long social and philosophical history, is summed up by Nowotny *et al.* as Mode-1 science (Gib-

bons *et al.* 1994; Nowotny, Scott and Gibbons 2001). Science in this mode has been likened with the hard pit in a peach, surrounded by - but strictly isolated from – the soft and runny stuff of society (Latour 1998).

While Bruno Latour (1993) and others insist that this model of science has never been an accurate description of the relationship between science and society, the argument here rests on the less sweeping proposition that Mode-1 science no longer is the only type of science there is. Nowotny, Scott and Gibbons (2001) portray a development in which the boundary between science and society are becoming increasingly porous. Where Mode-1 science is autonomous and clearly set off from society, Mode-2 science is inextricably embedded in society. The science that goes on in industrial laboratories hardly lives up to the Mertonian ideals of autonomy and disinterestedness. Where science ends and politics begins is also uncertain, and often contested, within the regulatory sciences (Jasanoff 1990). As in the cases of civil nuclear power (Beck 1992; Wynne 1996), AIDS research (Epstein 1996), mad cow disease (Ratzan 1998), and global warming (Miller 2001; Shackley and Wynne 1996), science and politics often become mixed-up. It is these tendencies towards contextualization or hybridization which Nowotny, Scott and Gibbons (2001) place at the heart of the transition from Mode-1 to Mode-2 science.

The same tendency towards blurring of the boundaries between science and society can also be observed in the case of fisheries resource management. That is not unexpected. Fishery science, as applied for management purposes, is at the outset fairly remote from Mode-1 science. As a regulatory science, fishery science could never approach the Mertonian ideal of disinterestedness and autonomy in the first place. But this does not mean that such ideals have been irrelevant as a basis for organizing and legitimizing modern fisheries management. Quite on the contrary, I want to argue. Because it is so difficult to distinguish between science and politics within fisheries resource management, Mode-1 ideals became all the more important. The invocation of Mode-1 science as a model for fishery science, in other words, is a crucial part of the boundary work (Gieryn 1999) within fishery science exactly because the distinction between nature and society within fishery resource management at the outset is so fuzzy.

Consider the institutional expectations placed on fishery science by its function within management. In order to make sense of modern stock assessment practices, it is as important to be sensitive to their role in realizing a political project – the instalment of the nation-state as responsible manager of fishery resources – as it is to see them as procedures for uncovering structures in nature (Holm 2000). One example here is the dominance of single-stock models, which achieve predictive capabilities by sacrificing ecological realism (Holm 1996). Another is the tendency within assessment science to assume that fish stocks are large and homogeneous (von Herbing *et al.* 1998), an assumption which is easier to understand as a political artefact than as a fact of nature (Holm, Raanes and Hersoug 1998). If modern fisheries management were going to be able to secure sustainable fisheries, marine ecosystems needed to be robust, predictable, large-scale and simple. When fishery science has lived by models that conform to such assumptions, this is not primarily because they are true to nature, but because they are constitutive of fishery science' role within modern resource management (Holm 1996).

As long as fisheries management is widely perceived to achieve its goals, or at

least to be better than any realistic alternative, the boundary work required to keep up the fiction of a neat science/policy division is not prohibitively costly. This changes when fisheries management is seen to be failing. Such perceptions of failure are now quite widespread, fuelled by events like the series of fish stock collapses across the North Atlantic during the 1990s (Myers, Hutchings and Barrowman 1996), and the persistent reports of the deteriorating health of global fishery resources (Hutchings 2000; Pauly *et al.* 1998). Under these circumstances, fishery science can no longer be black-boxed (Latour 1987). Instead, its fundamental faiths and biases are exposed and become more open to scrutiny and re-evaluation. This happens in part as the earlier consensus – or rather absence of public disagreements – among scientists breaks down. In the quarrels over what went wrong, who are to blame, and how the problems can be fixed, the negotiated, contingent, and contextual character of science becomes visible (Doubleday, Atkinson and Baird 1997; Healey 1997; Hutchings, Haedrich and Walters 1997; Hutchings, Walters and Haedrich 1997). At the same time, the epistemic authority awarded to science in a management context is withdrawn. Scientific advice becomes easier to challenge, and the work required to make them stick increases dramatically. This happens not only because the legitimacy science carries with traditional stakeholder groups is eroded. It is probably equally important that the perception of failure tends to attract new stakeholders, who bring with them different concerns, and new criteria of acceptable risks and due process.

All this means that the traditional understanding of the actual and appropriate role of scientific expertise within fisheries resource management – as conforming to Mode-1 criteria – no longer is taken for granted. While this situation in itself comes with some features that are characteristic of Mode-2 science, we cannot assume that such features are going to become permanent. Rather, reaching agreement on the appropriate role of science is one of the issues in the broad and fragmented negotiation process that constitutes the present crisis in fisheries management. Whether Mode-2 characteristics will feature predominantly when this crisis is resolved or otherwise made to disappear, therefore remains to be seen. In the meantime, we can start looking at the factors that are likely to affect the outcome of these negotiations. To what extent are Mode-2 ideals actually informing social expectations towards science in fisheries management? Which groups are the social carriers of these ideals, and how influential are they likely to be? Are such ideals resisted or welcomed by fishery scientists? To the extent they are embraced by the stakeholder groups in fisheries management, what exactly are these ideals, and how do they vary among groups?

These are broad questions, and my purpose here is only to give a partial answer. I propose to do that by exploring the discourse on fishermen's and fish workers' ecological knowledge (FEK). Taking inspiration from the turn towards indigenous knowledge within the field of development (Chambers 1997; Sillitoe 1998), the participatory research movement (F. Fischer 2000) and the citizen science perspective (Irwin and Wynne 1996; Wynne 1996), FEK research starts from the assumption that fishermen's knowledge about the marine environment can be useful for management purposes. While the fishermen's knowledge differs from science, it can – at least when appropriately collected and cleaned up – be a valuable complement to scientific knowledge for management purposes (Hall-Arber and Pederson 1999; Johannes, Freeman and Hamilton 2000; Mackinson 2001; Maurstad

and Sundet 1998; Neis and Felt 2000a; Neis *et al.* 1999b).

I do not here mean to imply that FEK research somehow automatically foreshadows the future pattern of research within fisheries management. Instead, I see FEK research as one site in which new models of science and its relationship with non-scientists are developed and tested out. It is not the only site where this happens, and we should not assume that the models developed here necessarily are going to be important. With due consideration of these limitations, FEK research is nevertheless interesting, not least because of its inherent ambiguities. While FEK research points towards a less monopolistic and arrogant scientific approach, it also seems to retain a commitment to Mode-1 ideals. This becomes clear in the practical methods developed for collecting and sorting out FEK. While FEK in theory champions the fishermen as competent and reliable knowledge producers on par with scientists, FEK in practice seem to acknowledge fishermen's knowledge only to the extent it can be made to correspond with scientific knowledge. Whereas FEK researchers sometimes seem to recognize in fishermen's knowledge a different voice, speaking of fish from an alternative perspective, as one would expect from a Mode-2 position, FEK research sometimes looks more like a ventriloquist act, in which fishermen are made to speak the truths of science. The question, then, is whether FEK research represents a step towards Mode-2 science, or whether it is better seen as an attempt to return to Mode-1 science under the disguise of Mode-2 rhetoric.

Two Examples and a Perspective

Let us start with an archetypal FEK anecdote, the story about the collapse of the Northern cod stock off Newfoundland:

> A fisheries management debacle brought about in part by the refusal of biologists to take fishers' knowledge seriously was the north Atlantic cod fishery's collapse. One sign of its imminence was the warning of inshore cod fishermen that spawning stocks on their fishing grounds had become alarmingly low … The consequences of ignoring this and related warnings are too well known to need reiteration here (Johannes, Freeman and Hamilton 2000: 258).

Despite the authors' assertion that no further details are required to this story, I think a few are in order: The government scientists charged with the responsibility to do the stock assessment, misjudged the status of Northern cod during the 1980s. They concluded that the stock was healthy and growing when it – as it turned out – was in rapid decline. Because of this mistake, heavy fishing continued and the stock collapsed (Hutchings and Myers 1994; Steele, Andersen and Green 1992). What allows this to be turned into a paradigmatic FEK story is that many inshore fishermen during the 1980s experienced very low catches. Arguing that the cod stock was in peril, they challenged the officially scientific assessment, both in court and by initiating alternative scientific assessments of the status of the cod stock. While the fishermen's view was initially rejected, history proved them right and the government scientists wrong (Finlayson 1994; Harris 1999; Kurlansky 1998; Neis 1992).

The Northern cod case forcefully demonstrates how tragically wrong science can go when it refuses to listen to the fishermen.

At the face of it, the major antagonism in this story seems to be between science and fishermen's knowledge. When we take a closer look, however, we see that this is not entirely true. For instance, there are scientists fighting at both sides of the conflict; the government scientists who thought the stock was healthy, and the scientists initially hired by the fishermen – later they were also consulted by the government – who were more pessimistic (Harris 1999). There furthermore is the problem that research on fishermen's knowledge itself must be considered as a science. How can FEK research reconcile an anti-scientific sentiment with its own identity as a science?

The solution to these problems perhaps becomes easier to see in the context of another cluster of paradigmatic FEK stories, those in which fishermen's knowledge is mobilized in arguing that fishery science's tendency to assume large-scale, homogeneous stock systems is too simplistic. Instead, stock structures may be better described as complex aggregates of localized sub-populations. If such is the case, it may have important implication for the management of the fish resource in question. One of these stories, told by Anita Maurstad and Jan Sundet (1998), concerns the cod off the coast of Northern Norway. While the existence of a 'coastal cod' different from the main north-east Arctic cod stock had been acknowledged for a long time, this complication had not been explored and used in the context of stock assessment. During the fishery crisis in the early 1990s, the potential irrationality of this state of affairs was made into a political and scientific issue (Holm, Raanes and Hersoug 1998). In one of the research projects that followed, Maurstad and Sundet (1998) interviewed inshore fishermen and were thus able to identify 44 local cod stocks in the county of Finnmark alone, several of which had already disappeared. In a similar project across the Atlantic, Edward Ames (1997, 1998; Ames, Watson and Wilson 2000) compiled oral histories from local fishermen, identifying at least 21 different spawning grounds for cod in northern Massachusetts through western Maine, and at least 88 spawning grounds in eastern Maine. Similar studies have also been undertaken in Newfoundland and Labrador, with similar results (Wroblewski 1998, 2000). In all three cases, the existence of localized fish stocks is not founded exclusively on fishermen's knowledge, but has in addition been supported by more traditional scientific methodologies, including genetic analysis (Fevolden and Pogson 1997; von Herbing *et al*. 1998).

A main point of these stories is of course to celebrate fishermen's knowledge and demonstrate the weaknesses of science. Nevertheless, the critique is not directed at all forms of science, only those that ignore FEK. 'Ignore fishers' knowledge and miss the boat', say Johannes *et al.* (2000). As the Northern cod story so forcefully brings out, it is the science that refuses to listen to what the fishermen have to say that will get itself – and the fishermen – into trouble. A more useful science is available, however. All that is required, as demonstrated by the localized fish stocks stories, is that it pays attention to the fishermen's ecological knowledge. In other words, the major antagonism in these stories is not between science and lay knowledge, as a cursory reading might suggest, but between different scientific viewpoints: Between an orthodox science that disparages fishermen's knowledge, and a reformed science that respects and engages it; between a science of the past, clinging to old-fashioned

notions of prediction and control, and a less hegemonic science of late modernity (McGoodwin, Neis and Felt 2000:249-50).

While the major antagonism the FEK research engages is that between different sciences rather than between science and lay knowledge, as argued above, this distinction is frequently blurred in FEK literature. One of the things that contribute to this blurring is that the term FEK and its many relatives[1] are used in a way that conflates two different meanings. First, FEK refers to knowledge as held by fishermen. This is, to use Arun Agrawal's (1995) term, *in situ* FEK: Embedded, contextual knowledge as an ongoing social production by lay people. Second, FEK also refers to the end product of a research process on lay knowledge; what you end up with after *in situ* FEK has been collected, refined and gone through all sorts of subtle transformations in the hands of FEK researchers. In order to be able to keep up this distinction – which for the purpose of this paper is crucial – I will in the following call *in situ* fishermen's knowledge for FEK and *ex situ*, scientifically refined fishermen's knowledge for FEK*.[2]

FEK* is different from FEK. If it had not been, there would have been no point in FEK research. Collecting, sorting, coding, standardizing and validating FEK would have been meaningless if such procedures had not turned it into something more potent and powerful. Hence, writing lay knowledge down makes it more stable and easier to transport. Storing it in databases allows it to be accessed more easily, sometimes from a great distance. Standardizing, sorting and validating it make it easier to combine with other data sources. And so on.

I should add here that it is the particular interest in this paper – examining the relationship between science and lay knowledge – that makes it important to differentiate between FEK and FEK*. I readily acknowledge that such a distinction is not always necessary or interesting. This point is important to keep in mind when we later discuss why FEK researchers tend to gloss over this distinction. By the same token, our interest in keeping up the distinction between FEK and FEK* does not commit us to accept Agrawal's (1995) conclusion, that the two necessarily are antithetical; that the procedures of scientific collection and refinement invariably transform lay knowledge into something that is necessarily foreign and hostile to lay people.[3] While this may be the case, we should for the moment suspend that judgment. Or rather, since this is one of the main questions this paper wants to examine, we should be careful not to import the answer in the form of a theoretical preconception. For the moment, then, we do not want to load too much epistemological or political significance into the distinction between FEK and FEK*, only insist that the two are not identical.

The reason this distinction is important here is that it allows us to see FEK simultaneously as a boundary-spanning and boundary-creating activity. On the one hand, FEK research is boundary spanning; its first principle is to undo the knowledge apartheid between scientific and lay knowledge within fishery management. On the other hand, this project is not an unconditional tearing down of the boundary between science and non-science. Quite on the contrary, as we shall see in detail below, it represents a partial and conditional opening of that boundary. In place of a general embargo on lay knowledge, FEK research establishes procedures for discriminating between different types of lay knowledge: That which is acceptable as valid knowledge and thus can be admitted to the land of science and that which is not and

must remain outside. The FEK-FEK* distinction is useful in analyzing this process because it refers to knowledge entities that reside on each side of the (reconstructed) scientific boundary. FEK is the 'raw', unprocessed knowledge we find on the outside. FEK* is the end result of an active selection and refinement process by which some fraction of FEK is transported to the inside.

Translating FEK into FEK*

The fundamental problem that confronts researchers who want to sample and use lay people's knowledge is the huge amount and variety of items that potentially can count as such (Antweiler 1998). Usually, of course, the total universe of relevant knowledge items is greatly reduced by the relevance criteria embedded in any particular research project. So, for instance, knowledge research within the fisheries typically focuses on ecological knowledge, or more specifically, fishermen's (and sometimes fish workers') knowledge relevant to the management of fishery resources. While this may reduce the universe of potentially relevant knowledge items to one of somewhat more manageable proportions, an enormous amount and variety of knowledge still remains. This is because knowledge, according to FEK researchers, cannot be assumed to be homogeneously distributed among those who are involved in any particular fishery. First, a given individual's knowledge will in part be a function of the length of his or her experience from the fishery: Is he or she a 'novice' or an 'expert'? (Neis *et al.* 1999a:221-22). Second, a given individual's knowledge will in part be a function of his or her position within the fishery system, so that a skipper's knowledge will be different from that of a deck hand; a processing worker's knowledge will be different from that of a processing plant manager; and men's knowledge will be different from women's knowledge, and so on (Neis *et al.* 1999a:221; Power 2000). Third, fishermen's knowledge will vary with respect to the technology employed in fishing. This is because the different gear types are equally sensitive to variations in the environment; they have different properties as instruments of 'seeing' what goes on at the bottom of the sea. A trawler crew, then, will get to know different things about the ecosystem they are fishing than a long-lining crew (Eikeland 1998; Johannes 1981: vii; Neis *et al.* 1999a:222). Fourth, the way fishermen perceive of a fish population will also depend on what part of its life cycle they come in contact with. Hence, fishermen who catch salmon when it returns to the river may be more knowledgeable than fishermen exploiting them in a short section of their migration run along the coastline (Felt 1994). Fifth, knowledge may vary to the extent it is politicized. Hence, if there have been strong conflicts over management issues one cannot assume that the knowledge a fisherman presents is drawn from his or her own experience as a fisherman. Instead, what is presented as knowledge may reflect the position of an individual or a group within a polarized political field (Felt 1994).

I could go on here quite a while with factors like age, social status, intellectual capability, devotion to observation, curiosity, profession, region, local topography, technological environment, crew structures, fishing practices, position in social network, and degree of technological change. All of these, and more, have been mentioned by FEK researchers as variables some way or another affecting the dis-

tribution and quality of knowledge within a given population (see Neis *et al.* 1999a: 220-222 for a review). In addition, not all informants will be equally reliable when they talk about their knowledge to researchers. Some may want to portray themselves as more knowledgeable than they really are. Some are dishonest, some are forgetful, and some are strategic (Felt 1994; Johannes 1981; Neis *et al.* 1999a). All this means that FEK, *in situ*, raw fishermen's knowledge, comes in the form of a mixed bag of knowledge items; a huge pile where a few nuggets of genuine insights and well-tested truths are entangled in a wide variety of beliefs, speculations, rumors, misunderstandings, lies, hopes, ideas, exaggerations, superstitions, and anecdotes. The basic problem becomes one of untangling the good stuff from the bad – truths from beliefs, insights from hopes, observations from anecdotes, sound interpretations from politically charged ones. FEK is the huge pile of assorted knowledge items we have at the start of the research process. FEK* is the small, well-ordered and consistent set of valid truths we are left with when the process has been brought to an end. The challenge of FEK research, then, is to develop and agree on a limited set of principles and procedures for getting from FEK to FEK*.

In the following we shall take a closer look at the two main strategies FEK researchers have relied on in order to transform FEK into FEK*. These are, first, to treat fishermen's knowledge claims as sources of hypotheses, which then can be validated or rejected. The second strategy is to accept fishermen as capable observers, so that their observations, when properly collected and sorted out, can be transformed into reliable and valid data fit to enter scientific analyses.

Translation Chain 1: FEK as Hypotheses

By this strategy, fishermen's knowledge claims are treated as a source for hypotheses, which then can be subjected to scientific interrogation. This is a solution to the problem posed by the heterogeneity of FEK, since it only accepts those claims that can be scientifically verified. Treating fishermen's knowledge claims as hypotheses drastically reduces the need to evaluate such knowledge claims by their origin. (The strategy discussed in the next section, according to which FEK is mined for observational data, must do that.) That is, instead of going upstream, judging knowledge claims by the circumstances of their construction, treating knowledge claims as hypotheses means going downstream and look at how they hold up when subjected to specifically designed quality tests. We should note here that a methodology of down-stream hypothesis testing by no means eliminate altogether the need for upstream procedures of process evaluation. An experienced local expert would still, on average, be a much better source of interesting hypotheses than a stupid novice. Nevertheless, when knowledge claims are treated as hypotheses, sorting them by the quality of their source becomes less pressing, since their validity is to be determined later by other means.

Wroblewski (2000) supplies us with a good example of this strategy. He treats as a hypothesis local fishermen's claim that there is a resident cod stock in Gilbert Bay, Labrador, independent from the offshore Atlantic stock. The fishermen's claim was based in part on color difference, in part on behavioral differences between bay and offshore cod. When Wroblewski set out to test the validity of this claim, he first

noted that the indicators the fishermen have relied on to reach their conclusion are not trustworthy. Notably, the color differences between cod found offshore and inshore are not reliable indicators of stock separation, since cod skin has the capacity to change color with the environment. In order to test the hypothesis of stock separation, he had to construct or utilize better indicators. He hence collected and analyzed morphometric data (examining whether cod found in the bay were smaller, and thus less fecund, at a given age than cod found offshore), data on the content of antifreeze glycoprotein level in blood samples (examining whether cod found in the bay in spring had over-wintered in the colder bay water), and genetic data (examining whether cod found in the bay were genetically distinguishable from cod found offshore) (Wroblewski 2000:77-78). As the whole battery of tests converged towards the same result, Wroblewski concluded that the fishermen were correct: There is a resident bay stock of cod in Gilbert Bay, different from the offshore cod stock.

While Wroblewski is careful to praise the local fishermen for their knowledge, there is no escape from the conclusion that this treatment imposes a strict hierarchical relationship between science and fishermen's knowledge. The fishermen get to suggest the hypothesis, but science reserves for itself the privilege to accept or reject it. The fishermen's claim is accepted as (valid) knowledge only after science has done its job. Before science enters the scene, the fishermen *believe* there is a local bay stock. When science leaves, the fishermen *know* there is a local bay stock. Without Wroblewski's validation, the fishermen's interpretation would not have counted as knowledge (outside the local arena) in the same way as it now does.[4]

Such a relationship between science and fishermen's knowledge is not entirely unlike what we know from orthodox science. As an example we can take a look at the Norwegian marine zoologist Johan Hjort's role in the conflict between fishermen and whalers in Northern Norway in the early 1900s. In the center of this conflict was a specific knowledge claim set forth by the fishermen. They believed that the cod fishery was dependent on healthy whale stocks. According to their 'sheepdog theory', the whales would chase the cod's bait, herring and capelin, towards the coast. The cod would follow. Without whales, the capelin and herring would stay out at sea. So would the cod, which then would be out of reach for the fishermen's small coastal vessels. The fishermen believed that the whaling operations threatened the whale stocks, and thus the fishery. Because the intensity of the fishermen's mobilization (this conflict led to the first member of the Labour Party being elected to the Norwegian Parliament) on the one hand, and the prestige of the whaling industry (one of Norway's few contributions to the industrial revolution) on the other, the Government appointed a natural scientist, Johan Hjort, to find out whether the sheepdog theory was true or false. After checking out the facts of the matter, with the methods and resources available to him, Hjort concluded that the sheepdog theory was false. While the whale could be important in order for the fishermen to locate the fish, it was not important in order to bring the fish to shore (Eythórsson 1996, 1998; Johnsen and Tønnessen 1959).

Are there significant differences between Hjort's approach here and that of contemporary FEK researchers' treatment of fishermen's knowledge as testable hypotheses? Despite the obvious parallels, we should not overlook an important difference in emphasis. Hjort, in line with orthodox science's well-known tendency to keep a distance between science and folk knowledge, emphasized what was wrong

with the fishermen's claim. A contemporary FEK researcher would perhaps be more interested in emphasizing what they got right, for instance that the whaling industry indeed was about to destroy the whale stocks (Eythórsson 1998). Another important difference pertains to the way fishermen's claims become available for scientific scrutiny. As in the whaling example, orthodox science usually will take on such issues for scientific investigation that have become the focus of social controversy and political mobilization. In contrast, FEK research may involve a more active search for folk theories that can be turned into researchable hypotheses.

Translation Chain 2: FEK as Data

When FEK researchers talk about fishermen's knowledge, it often sounds as if it is primarily their *interpretations* – statements that effectively summarize large amounts of observations and experiences – that they are after. Sometimes this is indeed the case, as we saw in the previous section. In this section, however, we shall concentrate on a strategy that goes in the opposite direction, as it were, and instead seeks out the observations on which such interpretations are based. While this may sort out a great deal of valid insights, FEK researchers argue that this is an important method for cleaning up fishermen's knowledge. While fishermen's interpretations are frequently wrong, they insist, the observations these interpretations are based on are likely to be valid (Gendron, Camirand and Archambault 2000:70-71; Gunn, Arlooktoo and Kamayoko 1988; Johannes 1993:36-37; McGoodwin, Neis and Felt 2000:252; Neis *et al.* 1999b:1951). The following anecdote, told by J. Fischer (2000: 49-50) illustrates the point:

> [A local informant] stated that a recently introduced African cichlid (*Orechromis spp*.) ate its offspring, and then immediately offered his evidence, i.e. he had occasionally caught a female with spawn or brood in its mouth. In several African cichlids the maternal or paternal mouth cavity is used to hatch the eggs and later serves as a shelter for their progeny in case of danger. Thus, Benjamin's conclusion was incorrect; his observation, however, was accurate.

The distinction between interpretation and observation – so that the former is suspect and should be discarded while the latter is sound and can be collected – becomes a powerful tool for refining a heterogeneous interview material. Obviously, this distinction imposes a hierarchy between science and indigenous knowledge. While fishermen's knowledge activity is restricted to that of observation, science reserves for itself the business of analysis and interpretation. Just like their more orthodox scientific colleagues, then, FEK researchers turn out to be conservative when access to their centers of calculation are concerned – those places or functions where data are aggregated, condensed and transformed into knowledge (Latour 1987). The main difference between orthodox and FEK science does not lie here, as we might be led to believe, but instead in the strategies they adopt for data collection.

In the orthodox case, data are typically collected by delegates who are sent out from the center to some destination, grab the desired knowledge items, and

return (with) these to the center. Such delegates come in many shapes: they might be human (an anthropologist doing extended fieldwork), animal (when a fish or a bird is tagged, set loose and recaptured), or machine (the 2000 Mars Odyssey spacecraft). Regardless of their specific shape, a basic problem in the scientific process concerns how to make such delegates as effective and reliable as possible. That is, how can they be programmed and controlled so that the required observations or samples can be reliably collected, preserved and returned to the center of calculation (Latour 1987, 1988a, b)?

A large number of scientific techniques are employed to handle this problem. These are techniques that allow delegates to focus their attention on the most relevant factors for the research in question (training, sampling procedures, research protocols, interview guides); methods or instruments that enhance their observational capacity (microscopes and telescopes, GPS, video-cameras, soil-samplers, plankton nets); and technologies that allow for stabilization and easy transport of observations, specimen or extracts back to the center (field notebooks, lab protocols, measurement and classification procedures and tape recorders; containers and preservation additives in uncountable varieties). I could go on endlessly here, but the point I want to make is simple and does not hinge on details: While orthodox scientists invest heavily in programming delegates before they are sent out on their excursions, FEK scientists abstain from all that. This is because their key strategy is to rely on naturally occurring observers – fishermen – as their delegates. Instead of constructing and equipping new delegates, or venturing out to study marine ecosystems themselves, which may be costly, time-consuming, and illegitimate, FEK researchers want to use as delegates people who have already been out there and made such observations in the pursuit of non-scientific projects. The downside of this, as we have already seen, is that, exactly because they were not trained, instructed and properly accessorized prior to their excursions, fishermen will not be very effective and reliable observers. Instead of bringing back small and tidy data sets, the construction of which were pre-designed and therefore transparent, as the orthodox delegate will do, the fisherman-observer brings back a mixed bag of knowledge items, the construction of which remains obscure. This also means that, in order to be able to use such data to create knowledge, FEK researchers must develop effective and reliable procedures for selecting good from bad knowledge items. They must compensate for the absence of pre-programmed delegates by investing heavily in post hoc cleanup procedures.

We can tentatively distinguish between three different groups of such procedures. These are, first, procedures focusing on the appropriate selection of informants; second, procedures for selecting what type of knowledge it is appropriate to extract from given informants; and third, procedures for de-selecting knowledge that is particularly vulnerable to interest distortions. The three groups are discussed in more detail below.

Procedures for Selection of Informants

One group of procedures that is very important in FEK research is concerned with the appropriate selection of individual informants, most often fishermen. Random sampling is seldom seen in FEK research (but see Gendron *et al.* 2000 for an exception). Although it is sometimes noted with regret that this prevents generalization

of results (Neis *et al.* 1999b:1951), most FEK researchers agree that random sampling is not appropriate for FEK research (ibid.). Instead, snowball sampling is the preferred method, a procedure by which the first interviewees are asked to recommend particularly knowledgeable individuals as candidates for further interviews. Snowball sampling is sometimes used in combination with a 'saturation' criterion, that is, interviews will continue until the researchers feel that some coherent pattern of views are established and more interviews are unlikely to add new insights (Felt 1994).

We note here that this methodology is not geared towards generating an accurate description of the distribution of knowledge within a population, nor to assess the average or typical information level of individuals or groups. The focus remains on the content of the knowledge, not on its social or cultural context. The point is to seek out and interview the most knowledgeable individuals. When the views of such experts converge, FEK researchers conclude they have identified the real thing (Hall-Arber and Pederson 1999). By the same token, these sampling procedures can also be used to avoid the inexperienced, stupid, dishonest, or politically suspect fishermen from the sample, or at least identify those that somehow got to be included. In this way, a lot of aberrant and misguided viewpoints can be filtered out. Johannes (1981:8-9, 1993:36) thus would ask the informants questions he knew the answers for, or questions he knew the informants would not know the answer for. According to the way they would respond to such questions, the interviewees as well as the data they provide can then be sorted into categories of more or less trustworthy (Neis *et al.* 1999a:223).

Snowball sampling is designed to single out the most knowledgeable individuals within a (vaguely defined) universe of informants. In addition to such procedures, FEK researchers will – explicitly or implicitly – consider whether there are specific groups of informants that are likely to be more knowledgeable than others. Reasoning along such lines, Felt (1994) points out that lack of consensus and even contradiction among fishermen need not mean that their knowledge is irrelevant or useless (see also Hall-Arber and Pederson 1999). The challenge, in such cases, is to focus on the 'social conditions and constraints under which [knowledge] is produced' (ibid.:282). Felt's example is a salmon fishery in Newfoundland, where some groups of fishermen claim – erroneously it would seem from Felt's account – that the salmon stock is healthy, while other groups correctly believe that it is threatened. Felt finds that the differences in beliefs about the status of the salmon stock vary systematically according to the fishermen's geographical location in relation to the stock. The fishermen who believed that the salmon stock was healthy (Profile 1) were located in zones where the salmon was available for harvest for only a short time each year (2-3 weeks). This would result in limited knowledge of the fish's life cycle. Further, because of the large number of salmon migrating past this location, they would perhaps notice any reductions in their number. The fishermen who believed that the stocks are threatened (Profile 2), typically live closer to the salmon rivers:

> The geographical proximity of salmon river and fishers allows for more comprehensive, integrated understanding of salmon. Rather than a fleeting encounter as they pass by, fishers encounter the salmon at various stages of their life cycle (Felt 1994:267).

When researchers have made such 'profiles' based on the conditions under which the knowledge claims of different groups of fishermen have been produced, the well-informed fishermen can be sorted out from misguided ones. The same type of reasoning applies in the argument that fishermen's wives may know more than the fishermen themselves about the timing of changes in gear and landings because they usually are keeping the books (Neis *et al.* 1999a), and that fish workers may know important things about the ecological condition of stocks because they are in a position to register ups and downs as well as quality changes in the supply of fish to processing plants (Power 2000).

Procedures for Selecting Appropriate Knowledge Items

The point of the sampling and sorting procedures dealt with until now is to single out the very best from among the whole population of possible informants (whether on an individual or group basis), thereby compensating to some extent for the absence of pre-programmed scientific delegates. FEK researchers will hence spend considerable effort to convince their readers that these local experts are exceptionally acute observers (cf. Johannes, 1981). FEK researchers are nevertheless aware of the limits of the fishermen's observational powers. Accordingly, a related set of cleanup procedures utilized by FEK researchers is concerned with the ease with which different types of phenomena lend themselves to observation by fishermen. By focusing on those types of observations fishermen are ideally positioned to make, then, reliable data can be obtained even if the observers do not have the same observational powers as a pre-programmed scientific delegate.

Hutchings (1996) and Hutchings and Ferguson (2000), for instance, identify three areas where fishermen's observations are particularly valuable (see also Neis *et al.* 1999b:1950). These are, first, information on seasonal movements and migration of fish; second, information relevant to stock identification; and third, information pertaining to changes in catch rates and fishing effort. It must be mentioned here that Hutchings and Ferguson do not ground the importance of these three areas explicitly on the ease with which fisher-observers can access relevant data, but rather on the ease with which information from such areas can be connected to current issues within fishery science (2000:83-84). A scientific relevance criterion is active as a primary filtering mechanism. I shall have more to say about this below. Here, however, I want to point out that the three areas also conform to the more implicit criterion of easy observational access. In their paper, for instance, Hutchings and Ferguson (2000) zoom in on area three, asking fishermen about catch rates, how fishermen would change the amount of gear according to expected catch rates, and relevant changes in gear design. We note that all this information is directly linked to the activity of the informants as fishermen. While they may have forgotten some of these things over the years, there can hardly be any doubt that the fishermen at one time or another have possessed such knowledge. In other words, changes in gear deployment or catch rates on individual fishing vessels represent a type of phenomenon of which the fishermen will have immediate observational access. This would be different when we come to phenomena like the aggregate changes in catch rates for the fishery, or the status of fish stocks. To take the simplest of these examples, aggregate changes in catch rates, this is a phenomenon that becomes observable only

through some mechanism for recording the practices for individual fishing vessels (or some representative sample) and doing the appropriate aggregation. While we may imagine that a gear wholesaler in a (local) monopoly position together with a fish processor in an overlapping (monopoly) position perhaps could be able to observe this phenomenon, it is much harder to imagine a naturally occurring system for information exchange among the fishermen that would allow them to do this with any accuracy. Indeed, it is the absence of such knowledge that is the reason why Hutchings and Ferguson go to the trouble of collecting fishermen's information as to their individual experiences, thereby creating a position from which the fishermen's behavior in the aggregate can be made visible (see also Gendron, Chamirand and Archambault 2000; Neis and Felt 2000b:13).

Procedures for Sorting Out Politically Tainted Knowledge Items

A related procedure for sorting out information from fishermen on truth content starts from the notion that not all kinds of knowledge will be equally vulnerable to distortion. Here, the focus is not, as above, on how well positioned the fishermen are in relation to a given phenomenon, but on the factors that are likely to influence on whether they want to put some 'spin' on this information, thereby making it more difficult to use for scientific purposes. Johanne Fischer (2000), for instance, distinguishes on such grounds between knowledge about *local fishing performance* (fishing efforts and catches, gear and boats, fishing locations and seasons) and knowledge of *physico-chemical environment and living aquatic resources* (the non-human part of the ecosystem). Fischer assumes that the first category is more vulnerable to distortions than the second because sharing the latter type of information with scientists 'usually does not entail risks to the income of fishermen. Thus these data should be less biased or influenced by the interests of the informants than data on fishing efforts and catches' (2000b:42). I'm not sure that Fischer's argument here is generally valid. At least it seems easy to imagine, in the context of resource management, that information on the ecosystem easily might be perceived by fishermen to affect their income if they believe that this information will be used to legitimize quota cuts. Furthermore, as Maurstad (1999) has pointed out, it is sometimes disturbingly easy to make fishermen volunteer information that can undermine their income if it falls into the wrong hands. It could also be argued that the degree of political mobilization within a fishery, and hence the likelihood that information will be biased, could in part be a reflection of the extent and history of conflicts over management issues. Instead of Johanne Fischer's (2000) general dichotomy between the human and the non-human realm, then, it would seem that the question of interest distortions should be assessed on a fishery-specific basis. This point is brought out in Felt's (1994) discussion why one group of fishermen (profile 1) so adamantly denies the possibility of decline in the salmon stock they harvest while another (profile 2) readily embraces this notion:

> The explanation of such adamancy and denial of any stock decline amongst these fishers [profile 1] lies in the relationship between their partial, instrumental knowledge, on the one hand, and the consequences of certain important external factors on the other. Two general forms of external factors are critical: (1) changes in state fishery regulations and (2) participation in the

> union representing fishers in the province. In combination with an instrumental understanding, they lead fishers to reject claims of stock decline and to perceive salmon management as almost exclusively a political battle. For fishers possessing the more holistic view presented in Profile 2, these factors either have minimal consequences or are absent completely (Felt 1994:269).

Profile 1 fishermen, to cut the story short (and somewhat simplified) have been more directly hit by the regulations (they are not disinterested), and have been more directly involved in the fishermen's union, which has engaged itself in a battle against angler interests (they are political agents). While profile 1 perceptions have been distorted by interests and politics, and are discounted, profile 2 perceptions remain undistorted and can be collected as valid knowledge.[5]

Re-embedding Fek in Science

Fek research is a process of transforming a vast, heterogeneous, contextual politicized body of knowledge, fek, into a small, homogeneous, non-contextual and depoliticized body of knowlegde, fek*. When this transformation happens by treating fek claims as sources of hypothesis, which then are tested by normal scientific procedures, fek research does not represent not much of a challenge to science's epistemological privileges. The question remains whether this also holds for fek research that tries to harvest fishermen and other lay people for valid observations. As we have seen, fek research contains rudiments of a methodology, a set of principles and procedures for turning fishermen's observations into hard, scientific data points. We must also note, however, that the development and precision of such a methodology leaves something to be desired. Two fek researchers using the principles and procedures outlined above independently on the same material will be unlikely to come up with the same results. It is not so much that fek researchers will disagree about these issues, but that there has been no sustained effort to reach agreement on them. Where should the distinction be drawn between observation and interpretation? On which theory about the relationship between science and folk knowledge does the exclusion of fishermen's interpretations rest? What are the appropriate procedures for identifying 'experts'? What role are such experts assumed to play within the local knowledge system? What set of phenomena fall outside fishermen's observational capabilities? How is absence of interest distortion to be determined? Given the tremendous consequences of modern science-based fisheries management for contemporary fishermen, what is the theory of knowledge production and learning that allows fek research to presume that knowledge unaffected by fishery science at all can be found? A search for answers to, and even debate about, such questions in the fek literature comes up virtually empty.

This dearth of discourse on key issues would seem to confirm Sillitoe's (1998:224) characterization of indigenous knowledge research in general, that its underlying philosophy is 'unexceptional', its intellectual stance 'difficult to define', and that it 'lacks theoretical and methodological coherence.' Given such lack of development and coherence, how is it possible for fek researchers to reach agreement on what shall count as fek*? If there are no well-proven, agreed-upon pro-

cedures for mining FEK* out of FEK, how can FEK researchers sustain their faith in the validity and value of fishermen's knowledge? The answer to this question, as already been hinted, has to do with the scientific relevance of FEK claims. Now, it goes almost without saying that fishermen, like any other group of people, have an enormous amount of knowledge the (pragmatic and local) validity of which is constantly confirmed in daily life. In the fisheries, this would for instance be knowledge about how to find and catch fish. Since fishing is their business, and commercial survival depends on some success in it, the notion that fishermen will have some valid knowledge of fishing, and hence of fish, is relatively trivial. The interesting question is not whether fishermen will hold valid knowledge, but the extent to which that knowledge, properly identified, collected and cleaned up, can be made to speak to issues within fishery science. What FEK researchers identify as FEK*, then, is that portion of FEK which has been processed through translation chains 1 or 2 as described above, and which is, explicitly or implicitly, deemed relevant and interesting from a fishery science perspective. Hence, when Hutchings and Ferguson (2000:82-3) single out fish migrations, stock identification and changes in gear deployment and catch rates as particularly salient for FEK research, it is so because data on these issues are directly relevant to contemporary fishery science. So for instance, there is no doubt, as demonstrated by Wroblewsky (2000), that fishermen can distinguish between (what they think is) local and oceanic cod on the basis of color differences. But this knowledge may not be particularly interesting to science, since color variation is an unreliable indicator of stock separation. Instead, other indicators, for instance observations of local spawning, may be more reliable, and hence of much greater scientific relevance (Maurstad 2000).

The argument about relevance does not simply contend that some types of observations will speak more effectively to scientific concerns than others. In addition comes that some types of data are not available by conventional scientific means. Neis *et al.* (1999a:224) hence say that 'a central element of TEK research should be the identification of the types of information most appropriate to be elicited from fishermen *as opposed to from other sources.*' [Italics added.]

Take, as another example, the case of fishermen's knowledge related to stock identification. As already noted, a debate over stock structure has opened up lately, challenging assessment science' tendency to assume that stock systems are large and homogeneous. One reason for this is the development of reliable and cheap techniques for stock identification, i.e. genetic mapping, the results of which seem to suggest that the former assumptions are too simplistic. What consequences this can and should have now constitute a hot topic (von Herbing *et al.* 1998). This, then, is the science and management issue that makes fishermen's knowledge about local stocks interesting. We note, however, that not all their knowledge about local stocks is equally interesting. Since cheap methods for genetic mapping are available, fishermen's knowledge becomes most pertinent when it comes to the identification of local stocks that have been fished out of existence, thus making old or retired fishermen the only possible source of knowledge (Fevolden and Pogson 1997; von Herbing *et al.* 1998).

Scientific relevance criteria are crucial for turning FEK into FEK*. By such criteria, the landscape of fishermen's knowledge is transformed from a completely unbounded and fairly unstructured field, into one in which a few salient formations

are clearly marked out. The argument here is not simply that scientific relevance criteria in this way allow FEK researchers to focus scarce resources on those parts of FEK that are most likely to pay off within science. In addition, the scientific prestructuring this involves also supplies expectations as to the boundaries within which FEK claims are likely to fall. In order to be accepted as part of FEK*, then, FEK claims must be reasonable and meaningful in relation to the scientific practice in question. Sometimes, of course, the acceptance of FEK claims hinges on explicit scientific validation (Hall-Arber and Pederson 1999; Johannes 1993) or method triangulation (McGoodwin, Neis and Felt 2000:258). But even in the absence of such direct confirmation, the way FEK claims 'fit' into the current scientific puzzles in itself have bearing on their scientific status. In this way, for instance, Gendron *et al.* (2000:67) reported that their finding that Magdalen lobster fishermen had changed their fishing practices 'gave some support to earlier findings and, to some degree, explained the mechanisms that had produced the findings from these indices.' Similarly, I suspect, the wide acceptance of FEK claims in the case of localized fish stocks has something to do with their compatibility with the current scientific viewpoints, as established by a new scientific technology.

In the end, then, the transformation of FEK into FEK* is a more active and constructive process than one of mechanical sorting and selecting. We can identify a number of filtering processes in FEK research, and there can be no doubt that they play an important role in the construction of FEK*. They nevertheless remain fairly coarse and underdeveloped, and cannot by themselves account for why some of the fishermen's claims are accepted as valid and useful for science, while others are not. Instead, transforming FEK into FEK* is as much about their re-embedding in scientific practice as it is about their dis-embedding from the cultural, social and political contexts in which they originate. For this to happen, it does not suffice that such claims are stripped from interpretive efforts, are collected from the upper echelon of fishermen experts, that only claims concerning phenomena these experts have direct observational access to come into consideration, and that no obvious politicizing strains are active. In addition to this – and perhaps even more important – they must fall in place and make sense within a scientific practice.

Purification

FEK researchers tend to underline the huge difference between their own approach to fishermen's knowledge and the approach typically found in orthodox fishery science. Whereas orthodox science dismisses fishermen's knowledge as anecdotal, FEK research presents itself in sharp contrast to that, as an approach that recognizes fishermen's insights as useful and valid. As we have seen here, however, FEK researchers do not dispute that FEK is different from scientific knowledge, and that a lot of work must be invested to turn it into FEK*. In fact, we can note at this point that the FEK researchers' assumptions as to the status of FEK in epistemological terms are not all that different from those of orthodox scientists. Here is a quote from an assessment scientist reported to 'disparage fishers' ecological knowledge':

> I imagine there is probably integration of all kinds of variables going on simultaneously in any particular fisheries situation on any given day, and also over the years, as people modify the traditional lore. You can't really do a controlled experiment under these situations to say, 'we falsified the null hypothesis so now we can move on to the next step of the method.' That reductionist approach would seem to me to be different from what you would consider to be traditional lore that integrates a lot of different observations and people's intuitions and gut feelings and is kind of tested but you don't know what kind of testing it's undergone from generation to generation. Have the conditions remained constant over time, or have they been changing? If they have, then how do you know what you are seeing is really the result of the causal mechanism that is attributed to it? (quoted from Neis 1992:166)

While a FEK researcher certainly would not phrase it in such a negative way, it is difficult to find anything in the above characterization of FEK that they will take exception to, apart from the conclusion. Whereas the orthodox scientists and the FEK researchers are in broad agreement that FEK comes as a mixed bag of knowledge items, they do not agree when it comes to the prospects of cleaning it up and integrating it with science. The assessment scientists in question concluded that fishermen's knowledge and science will be 'hard to integrate', since the former has so much information in it that is 'unspoken or already subsumed'. This is the orthodox position, in which fishermen's knowledge claims are arrogantly rejected as 'mumbo jumbo', or – put more politely – as based on 'anecdotal evidence' and hence not amenable to quantification (J. Fischer 2000; Johannes 1981; Neis 1992; Ruddle 1994). While orthodox science, because FEK is so heterogeneous and polluted, generally rejects the possibility of a scientific usage for it, the FEK researchers – while conceding that FEK is heavily polluted – insist that cleaning it up and transforming it into FEK* is both possible and rewarding.

Now, it is well known from the history of science that much of what counts as scientific knowledge builds on and integrates a great deal of lay or traditional knowledge (Atran 1990; Ellen, Parkes and Bicker 2000). While the degree to which such is the case varies considerably among scientific disciplines, it is usually more pertinent to see a strong demarcation between science and non-science as part of a boundary strategy to uphold science's epistemological privileges than to accept such claims on face value (Gieryn 1999). It makes a lot of sense to say that FEK research applies the reverse boundary strategy, seeking to collapse the distance between science and fishermen's knowledge. Even though, as we have seen, it takes a lot of work in order to extract FEK* from FEK, and hence that – in the eyes of FEK researchers – there are important differences between science and fishermen's knowledge, FEK researchers use considerable effort to downplay such differences.

We have already seen examples. In the case of Northern cod, it was hence intimated that Newfoundland fishermen at the time knew what had eluded the scientists – that a stock collapse was imminent – without worrying too much about the work that has gone into making the fishermen's concerns into common knowledge. In the localized cod cases, similarly, it was concluded that the fishermen had knowledge of the existence of local stocks, without much emphasis on the work undertaken by scientists – both orthodox and FEK researchers – to turn such speculations into accepted facts.

More generally, FEK literature makes an effort to argue – against the orthodox notion that affords to science alone a privileged access to truth – that fishermen's knowledge and scientific knowledge are not at all that different. Consider, for instance, this quote from Berkes (1993:3), for whom knowledge, scientific as well as traditional, and art, is that which sets the human realm apart from the non-human realm:

> There are both similarities and differences between traditional science and western science. Boronowski considers the practice of science (including magic) as the fundamental characteristic of human societies: ' … to me the most interesting thing about man is that he is an animal who practices art and science and, in every known society, practices both together.' (Boronowsky 1978:9). Thus, one can probably say that both western science and TEK (and art) are the result of the same general intellectual process of creating order out of disorder.

In this way, science ('western science') and TEK ('traditional science') are conceptualized as two different exemplars of the same class of phenomena. The close relationship between the two knowledge systems is further underlined when FEK researchers talk about lay 'knowledge systems' in terms that originate in the discourse on scientific knowledge production:

> Mailhot (1993:11) defines [Traditional Ecological Knowledge] as 'the sum of the data and ideas acquired by a human group on its environment as a result of the group's use and occupation of a region over many generations' (1993: 11). As such, TEK is cumulative over generations, empirical in that it must continuously face the test of experience, and dynamic in that it changes in response to socio-economic, technological, physical, or other changes (Neis and Felt 2000b:12).

Besides the notion that the knowledge of fishermen, just like that of scientists, is held as 'data' and 'ideas', we are informed that this knowledge is 'cumulative', 'empirical', and that it is continuously 'tested' if not by experiment, at least by experience. This is not to say that FEK researchers never want to talk about the *differences* between science and lay knowledge. When this occurs, however, such differences are usually portrayed as minor, so as to underline the fundamental identity:

> As indicated by Fischer …fishers' observations are acquired during fishing and are mediated by knowledge transmitted from previous generations, where they fish, when they fish, how they fish (gear, division of labour), the species and sizes they target, and who they fish with. Their knowledge tends to have a fine spatial scale but involves intensive sampling over extended periods. They rely on visual inspection, and what they see and recall is dictated by their fishing practices, by fish-finding and fishing technologies, and what they and those around them consider to be important. Many of their observations are transmitted orally rather than in written form and are thus subject to problems with recall. The data contained in fishers' knowledge

> have a high degree of complexity and are not standardized in terms of temporal scale, territorial coverage, technology, effort and expertise. The peer review process for this knowledge is poorly understood and different from that associated with science. Their knowledge is shaped in part by their work and community culture, and such cultures vary (McGoodwin, Neis and Felt 2000:252).

In the paragraph following immediately after the one quoted above, the authors go on to characterize scientific knowledge. The interesting thing from our point of view is that this characterization runs through the same list of 'knowledge functions' as was covered with respect to the fishermen's knowledge: traits of their observational behavior and sampling strategies, how data are recorded and transmitted, the nature of peer review, and the way knowledge production is culturally embedded. While there certainly is mention of differences here – such as the research protocols of scientists, their preference for written records, the technologies and training that allow them to move beyond visual inspection – these come out as minor variations. While this is not always explicitly argued, such a reading is implied since these variations are not evaluated and compared with regard to their role in knowledge generation. There is furthermore no mention of the possibility that the noted differences may add up so as to make the scientific knowledge paradigmatically different from fishermen's knowledge. On the contrary, FEK researcher will insist that '[t]he distinction between the two kinds of knowledge "is a difference of degree (quantitative) rather than type (qualitative)", as Giarelli (1996) puts it' (Berkes 1999:10).

How can we understand this? Why is it so important for FEK researchers to define the knowledge of fishermen or other resource users as if it were a close relative to scientific knowledge? Why are FEK researchers so keen to have us believe that traditional people 'possess scientific curiosity' (Berkes 1999:9; J. Fischer 2000:49), that they are capable of performing controlled experiments (Berkes 1993:4; Johannes 1981:7), that they are capable of quantification (Berkes 1993:4), and that they use sophisticated methods of inference and reasoning (Ruddle 1994:183-84)? The paradox here is that FEK researchers seem to be of two minds. In the context of sorting out and cleaning up FEK, turning it into FEK*, FEK researchers acknowledge, and in some ways actively construct, a great distance between science and fishermen's knowledge. When FEK researchers theorize over this exercise – in their explicit model of fishermen as knowledge producers – they tend to abolish that distance. FEK research thus seems to switch between two quite different models of fishermen's knowledge. For practical research purposes, FEK researchers stick quite close to an orthodox attitude towards folk knowledge, as a mixed bag of assorted knowledge items that must be drastically cleaned up and validated before it can be put to use. For legitimation purposes, however, FEK researchers take recourse to a model of fishermen as little scientists, with great but preposterously ignored knowledge capabilities.

From Mode-1 Towards Mode-2 Science

This paper has explored the discourse on fishermen's knowledge in search for indications of a possible transition from Mode-1 towards Mode-2 science (Nowotny,

Scott and Gibbons 2001) within the context of fisheries management. As the previous analysis suggests, Mode-2 elements are indeed present in FEK discourse. Within FEK research, the orthodox out-of-hand rejection of fishermen's knowledge has been replaced with an insistence that fishermen do have valid and useful knowledge. Although FEK researchers sometimes talk as if the gap between FEK and science is quite small, they still believe it is and should be there. What FEK research has done is not to make this gap disappear, but instead to have built a bridge across which communication between the two sides becomes possible. While FEK research in this respect represents a move towards Mode-2 science, it is nevertheless a very restricted and modest adjustment. FEK research's acknowledgement of fishermen's knowledge does not grant fishermen the right to have valid knowledge by themselves, only that they have knowledge fragments which – if collected and refined in proper ways – can be turned into valid knowledge. In order for fishermen's knowledge to be valid, it must first be carried across the bridge and find a home in the land of science, a process which turns out to be cumbersome and highly selective. So, while everything has changed, in that fishermen have been granted what they were previously denied, it nevertheless remains the same, in that the yawning gap between fishermen and scientists is still in place. While FEK research sometimes is presented as an epistemological parallel to the tearing down of the Berlin Wall, it has more in common with the construction and operation of a Checkpoint Charlie.

This may at first look like a disappointing conclusion. Where FEK at the outset seemed to represent a huge step from Mode-1 to Mode-2 science, it turned out to be a much more modest adjustment in boundary strategy. It is nevertheless important to acknowledge that this is perceived as disappointing primarily when it is evaluated on the basis of the purified gloss on FEK research, the perspective from which fishermen's knowledge is presented as a variety of science, and FEK research as a complete brake with orthodoxy. It is in contrast to the image of fishermen as naturally born scientists that we become disappointed when it takes so much work – by accredited scientists – in order to turn fishermen's knowledge fragments into truth.

The problem here is not in itself the attempt to abolish the distance between fishermen's knowledge and science. It is not, in my view, useful to grant to science epistemological privileges, accepting that it has found that sacred passage to true knowledge. The position of FEK research on this point – that science and folk knowledge are but varieties of the same type of phenomenon – is appropriate and fertile. It is nevertheless a problem with the strategy FEK research employs in order to make science and folk knowledge the same. As we have seen, FEK research accepts fishermen's knowledge only to the extent it fits into a scientific model. It is a model of knowledge developed for science that informs the understanding of folk knowledge here. This approach actually confirms the epistemological privileges of science, since it only accepts as valid non-scientific knowledge claims that conforms to the scientific model. The reason this happens, I think, is FEK research's unacknowledged commitment to a specific model of science, namely a standard, representational one in which knowledge is understood in terms of ideas, theories and mental representations. This model starts by postulating a great gap between the world – the objects of knowledge – and the word, representations of that world, so that valid knowledge claims – truth – obtains when there is correspondence between the two. In this perspective, science becomes the paradigmatic exemplar of valid knowledge

production, engaged in systematic confrontation and testing of theories against the world. Working from this model, FEK research' only avenue to restore the respect for fishermen as knowledge producers is to turn them into little scientists. In the process, it cannot avoid putting science's truths in the mouths of fishermen.

Better ways of thinking about science are available, however. If, for instance, science is understood in terms of skills, practices, interventions or heterogeneous networks, and not primarily as theories and mental representations, it leads us to think differently about the relationship between science and folk knowledge (Ingold 2000; Latour 1987; Pálsson 2000; Pickering 1992). In this picture, all epistemological privileges are abolished. But this does not mean that science is not different from other forms of knowledge. On the contrary, science is a type of local knowledge that employs a number of mundane but powerful technologies – writing, counting, and specialized metrologies – to make itself harder and tougher. While these technologies does not give access to truth, they do work, in that knowledge claims that are produced by such means usually have much greater likelihood of being accepted as valid and put to work than knowledge claims that are not (Latour 1987).

From such a starting point, the perspective on FEK research shifts. The point is no longer to identify those fragments of fishermen's knowledge that can be accepted as valid from a scientific standpoint. Instead, FEK research becomes an exemplar of what Callon, Meadel and Rabeharisoa (2002) call a hybrid forum, and Nowotny, Scott and Gibbons (2001) call an agora: An arena where different types of knowledges meet and mix. Now there is no need to force fishermen's knowledge into a scientific mould since the difference between fishermen's knowledge and scientific knowledge is the point. There is no need to reject fishermen's knowledge, as orthodox science tend to, or to reject scientific knowledge, as FEK research sometimes pretend to, since both bring important things to the table. From this starting point, there is hardly any doubt that a number of phenomena pertinent to modern fisheries management requires a scientific approach. For instance, entities like fish stocks and marine ecosystem can only become available for systematic monitoring and control by way of specialized, complex and large-scale measurement systems of a scientific type. If current fishery science tends to get these things wrong, the solution is not to return to the naked eye of lay observers, but to build a better, more adequate and inclusive science. This said, there is no doubt that fishermen – since they spend so much time engaged with marine ecosystems – do have knowledge that is relevant and useful for science and management. In the present situation, when so much scientific effort is committed to simplistic single-stock models, systematically ignoring ecological interactions, fishermen observations will be particularly important when it comes to ecological changes. This is not so much because fishery science cannot deal with such issues – it surely could – but that current measurement systems are built with a single-minded focus on the stock. This is why it is the fishermen, not the scientists, who tend to be the first to notice things like sudden growth in jellyfish populations, the absence of sea-birds in areas where there used to be plenty, or changes in species composition in the catch. While fishermen cannot be expected to do better than science on issues that science targets, they will often have knowledge on issues that science – because it must be specialized – does not target.

We should not be too critical of FEK research. In practice, FEK research is a hybrid forum; it does bring together and mix different types of actors and knowl-

edges. The main problem is in the way this practice is understood and legitimized. Since FEK research is so committed to a standard, representational notion of knowledge, in which science gets to keep its epistemological privileges, it only accepts as valid those fragments of fishermen's knowledge that conforms to the standards of science. It is this (mis)understanding of science and its relationship to folk knowledge that makes current FEK research a variety of Mode-1 science, instead of a move towards Mode-2 science, as it easily could have been.

Notes

[1] Some of the most important are Indigenous Knowledge (IK), Traditional Ecological Knowledge (TEK), and Local Ecological Knowledge (LEK).

[2] I prefer this to Agrawals *ex situ/in situ* distinction because the latter carries with it the presupposition that FEK is embedded and contextual while FEK* is not. While I agree that the process of creating FEK* entails dis-embedding FEK from some local context, there is an added connotation in the *ex situ/in situ* distinction that FEK* remains dis-embedded and non-contextual. But this returns us to the Mode-1 notion of science. It is better, at least in this context, to see FEK* as FEK re-embedded in a different, but equally local context.

[3] According to Agrawal, the strategy of *ex situ* conservation of knowledge, the dominant collect-and-archive mode of indigenous knowledge research, will tend to 'disadvantage those who do not have access to international travel, Western languages, or technical expertise in computer based information storage – in short, indigenous peoples' (Agrawal 1995:431). As an alternative to *ex situ* conservation, Agrawal therefore recommends a strategy of *in situ* conservation. Instead of cataloging and storing knowledge, neo-indigenistas should strive to protect the cultural practices within which local knowledge is constantly utilized and reproduced. To this, Michael Warren has objected, rightly I think, that: 'The argument that recorded knowledge systems represent a "sterile and undynamic" database reflects a double standard, with knowledge generated through the global knowledge systems being recorded and placed in libraries and archives while indigenous knowledge systems are not to be removed from their cultural context' (Warren 1998:244). It is interesting, and not untypical of the present IK discourse, that Agrawal's paper, which set out to 'dismantle the divide between indigenous and scientific knowledge' ends up insisting on that divide.

[4] Other examples of the FEK-as-hypotheses approach is reported by Huntington (1999) for effect on a herring population by the Exxon Valdez oil spill, by Poizat and Baran (1997) for seasonal variations in abundance of fish in Fatala Estuary, Guinea; by Johannes et al (2000) for the disappearance of bonefish spawning runs in Kiribati; by Sarda and Mayony (1998) for catch fluctuations in a Catalan shrimp fishery; and by Prince (2001) for the pelagic and oceanic behavior of fish species in the Australian South East Trawl fishery.

[5] In addition to the considerations discussed above, which concerns how, in the context of a (partial) politicized fishery, one may avoid or sort out polluted FEK claims, FEK researchers have also brought up how different methodologies have different propensity in bringing out politicized claims. Hence, Neis et al (1999a:223), observed that: 'Fishers may be more honest if they are interviewed alone, and observations of group discussions can provide an impression of the he impact that public conflict may be having on "stated views".'

References

Agrawal, A.

1995 Dismantling the Divide between Indigenous and Scientific Knowledge. *Development and Change* 26(3):413-439.

Ames, E.P.

1997 *Cod and Haddock Spawning Grounds in the Gulf of Maine: From Grand Manan Channel to Ipswich Bay*. Rockland, Maine: Island Institute.

1998 Cod and Haddock Spawning Grounds in the Gulf of Maine from Grand Manan Channel to Ipswich Bay. In I. H. v. Herbing, I. Kornfield, M. Tupper, and J. A. Wilson (Eds.), *The Implications of Localized Fish Stocks*. Ithaca: Natural Resource, Agriculture, and Engineering Service Cooperative Extension. Pp. 55-65.

Ames, E.P., S. Watson, and J.A. Wilson

2000 Rethinking Overfishing: Insights from Oral Histories of Retired Groundfishermen. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 153-164.

Antweiler, C.

1998 Local Knowledge and Local Knowing - an Anthropological Analysis of Contested "Cultural Products" in the Context of Development. *Anthropos* 93(4-6):469-494.

Atran, S.

1990 *Cognitive Foundations of Natural History: Towards an Anthropology of Science*. Cambridge: Cambridge University Press.

Beck, U.

1992 *Risk Society: Towards a New Modernity*. London: Sage.

Berkes, F.

1993 Traditional Ecological Knowledge in Perspective. In J. T. Inglis (Ed.), *Traditional Ecological Knowledge: Concepts and Cases*. Ottawa: Canadian Museum of Nature. Pp. 1-6.

1999 *Sacred Ecologies: Traditional Ecological Knowledge and Resource Management*. Ann Arbour, MI.: Taylor & Francis.

Callon, M., C. Meadel, and V. Rabeharisoa

2002 The Economy of Qualities. *Economy and Society* 31(2):194-217.

Chambers, R.

1997 *Whose Reality Counts? Putting the First Last*. London: Intermediate Technology Publications.

Doubleday, W.G., D.B. Atkinson, and J. Baird

1997 Scientific Inquiry and Fish Stock Assessment in the Canadian Department of Fisheries and Oceans - Comment. *Canadian Journal of Fisheries and Aquatic Sciences* 54(6):1422-1426.

Eikeland, S.

1998 Flexibility in the Fishing Commons. In S. Jentoft (Ed.), *Commons in a Cold Climate*. Paris/New York: UNESCO/Parthenon. Pp. 97-114.

Ellen, R., P. Parkes, and A. Bicker

2000 Indigenous Environmental Knowledge and Its Transformations. Amsterdam: Harwood Academic Publishers.

Epstein, S.

1996 *Impure Science: Aids, Activism, and the Politics of Knowledge*. Berkeley: University of California Press.

Eythórsson, E.

1996 Men Han Tager Som Regel Feil: Kunnskap og modernitet i den 'gamle' hvalfangstdebatten i Nord-Norge. *Heimen* 33:289-300.

1998 Voices of the Weak: Relational Aspects of Local Ecological Knowledge in The Fisheries. In S. Jentoft (Ed.), *Commons in a Cold Climate: Coastal Fisheries and Reindeer Pastoralism in North-Norway*. New York: Parthenon UNESCO. Pp. 157-173.

Felt, L.F.

1994 Two Tales of a Fish: The Social Construction of Indigenous Knowledge among Atlantic Canadian Salmon Fishers. In C. L. Dyer and J. R. McGoodwin (Eds.), *Folk Management in the World's Fisheries: Lessons for Modern Fisheries Management*. Niwot: University Press of Colo-

rado. Pp. 251-286.

Fevolden, S.E., and G.H. Pogson

1997 Genetic Divergence at the Synaptophysin (Syp I) Locus among Norwegian Coastal and North-East Arctic Populations of Atlantic Cod. *Journal of Fish Biology* 51(5):895-908.

Finlayson, A.C.

1994 *Fishing for Truth: A Sociological Analysis of Northern Cod Stock Assessments from 1977 to 1990.* St.John's, Newfoundland: ISER Books.

Fischer, F.

2000 *Citizens, Experts, and the Environment: The Politics of Local Knowledge*. Durham, N.C.: Duke University Press.

Fischer, J.

2000 Participatory Research in Ecological Fieldwork: A Nicaraguan Study. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 41-54.

Gendron, L., R. Camirand, and J. Archambault

2000 Knowledge-Sharing between Fishers and Scientists: Towards a Better Understanding of the Status of Lobster Stocks in the Magdalen Islands. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 56-71.

Gibbons, M., C. Limoges, H. Nowotny, S. Schwartzman, P. Scott, and M. Trow

1994 *The New Production of Knowledge : The Dynamics of Science and Research in Contemporary Societies*. London: Sage.

Gieryn, T.F.

1999 *Cultural Boundaries of Science: Credibility on the Line*. Chicago: University of Chicago Press.

Gunn, A., G. Arlooktoo, and D. Kamayoko

1988 The Contribution of the Ecological Knowledge of Inuit to Wildlife Management in the Northwest Territories. In M. Friedman and L. Carbyn (Eds.), *Traditional Knowledge and Renewable Resource Management in Northern Regions*. Edmonton: Boreal Institute for Northern Studies. Pp. 22-30.

Hall-Arber, M., and J. Pederson

1999 Habitat Observed from the Decks of Fishing Vessels. *Fisheries* 24(6):6-13.

Harris, M.

1999 *Lament for an Ocean: The Collapse of the Atlantic Cod Fishery*. Toronto: McClelland & Stewart.

Healey, M.C.

1997 The Interplay of Policy, Politics, and Science - Comment. *Canadian Journal of Fisheries and Aquatic Sciences* 54(6):1427-1429.

Holm, P.

1996 Fisheries Management and the Domestication of Nature. *Sociologia Ruralis* 36(2):177-188.

2000 Ressursforvaltning som heterogent nettverk. In H. Gammelsaeter (Ed.), *Innovasjonspolitikk, kunnskapsflyt og regional utvikling*. Trondheim: Tapir. Pp. 83-102.

Holm, P., S.A. Raanes, and B. Hersoug

1998 Creating Alternative Natures: Coastal Cod as Fact and Artefact. In D. Symes (Ed.), *Northern Waters: Management Issues and Practices*. Oxford: Fishing News Books. Pp. 79-89.

Huntington, H.P.

1999 Traditional Knowledge of the Ecology of Beluga Whales (*Delphinapterus Leucas*) in the Eastern Chukchi and Northern Bering Seas, Alaska. *Arctic* 52(1):49-61.

Hutchings, J.A.
1996 Spatial and Temporal Variation in the Density of Northern Cod and a Review of Hypotheses for the Stock's Collapse. *Canadian Journal of Fisheries and Aquatic Sciences* 53(5):943-962.
2000 Collapse and Recovery of Marine Fishes. *Nature & Resources* 406(6798):882-885.
Hutchings, J.A., and M. Ferguson
2000 Links between Fishers' Knowledge, Fishery Science, and Resource Management: Newfoundland's Inshore Fishery for Northern Atlantic Cod. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 82-110.
Hutchings, J.A., R.L. Haedrich, and C. Walters
1997 Scientific Inquiry and Fish Stock Assessment in the Canadian Department of Fisheries and Oceans and the Interplay of Policy, Politics, and Science - Reply. *Canadian Journal of Fisheries and Aquatic Sciences* 54(6):1430-1431.
Hutchings, J.A., and R.A. Myers
1994 What Can Be Learned from the Collapse of a Renewable Resource - Atlantic Cod, Gadus Morhua, of Newfoundland and Labrador. *Canadian Journal of Fisheries and Aquatic Sciences* 51(9):2126-2146.
Hutchings, J.A., C. Walters, and R.L. Haedrich
1997 Is Scientific Inquiry Incompatible with Government Information Control? *Canadian Journal of Fisheries and Aquatic Sciences* 54(5):1198-1210.
Ingold, T.
2000 *The Perception of the Environment: Essays in Livelihood, Dwelling and Skill*. London: Routledge.
Irwin, A., and B. Wynne (Eds.)
1996 Misunderstanding Science? The Public Reconstruction of Science and Technology. Cambridge: Cambridge University Press.
Jasanoff, S.
1990 *The Fifth Branch: Science Advisers as Policymakers*. Cambridge, Mass.: Harvard University Press.
Johannes, R.E.
1981 *Words of the Lagoon: Fishing and Marine Lore in the Palau District of Micronesia*. Berkeley, Calif.: University of California Press.
1993 Integrating Traditional Ecological Knowledge and Management with Environmental Impact Assessment. In J. T. Inglis (Ed.), *Traditional Ecological Knowledge: Concepts and Cases*. Ottawa: Canadian Museum of Nature. Pp. 33-39.
Johannes, R.E., M.M.R. Freeman, and R.J. Hamilton
2000 Ignore Fishers' Knowledge and Miss the Boat. *Fish and Fisheries* 1(3):257-271.
Johnsen, A.O., and J.N. Tønnessen
1959 *Den Moderne Hvalfangsts Historie: Opprinnelse Og Utvikling*. Oslo: Aschehoug.
Kurlansky, M.
1998 *Cod: A Biography of the Fish That Changed the World*. New York: Walker and Co.
Latour, B.
1987 *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge, Mass.: Harvard University Press.
1988a *The Pasteurization of France*. Cambridge, Mass.: Harvard University Press.
1988b A Relativistic Account of Einstein Relativity. *Social Studies of Science* 18(1):3-44.
1993 *We Have Never Been Modern*. New York: Harvester Wheatsheaf.
1998 From the World of Science to the World of Research? *Science* 280(5361):208-209.

Mackinson, S.
2001 Integrating Local and Scientific Knowledge: An Example in Fisheries Science. *Environmental Management* 27(4):533-545.

Maurstad, A.
1999 'Dem e hjertelig velkommen alle kystfeskeran': Om kunnskapsformidling mellom fiskeri og vitenskap. *Tradisjon* 29(2):21-34.
2000 Trapped in Biology: An Interdisciplinary Attempt to Integrate Fish Harvesters' Knowledge into Norwegian Fisheries Management. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 135-152.

Maurstad, A., and J.H. Sundet
1998 The Invisible Cod: Fishermen's and Scientists's Knowledge. In S. Jentoft (Ed.), *Commons in a Cold Climate: Coastal Fisheries and Reindeer Pastoralism in North Norway*. New York: Parthenon UNESCO. Pp. 167-185.

McGoodwin, J.R., B. Neis, and L.F. Felt
2000 Integrating Fishery People and Their Knowledge into Fisheries Science and Resource Management: Issues, Prospects, and Problems. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 249-264.

Merton, R.K.
1996 The Ethos of Science. In P. Sztompka (Ed.), *Merton on Social Structure and Science*. Chicago: The University of Chicago Press. Pp. 267-276.

Miller, C.
2001 Hybrid Management: Boundary Organizations, Science Policy, and Environmental Governance in the Climate Regime. *Science Technology & Human Values* 26(4):478-500.

Myers, R.A., J.A. Hutchings, and N.J. Barrowman
1996 Hypotheses for the Decline of Cod in the North Atlantic. *Marine Ecology-progress Series* 138(1-3):293-308.

Neis, B.
1992 Fishers' Ecological Knowledge and Stock Assessment in Newfoundland. *Newfoundland Studies* 8(2):155-178.

Neis, B., and L.F. Felt
2000a Finding Our Sea Legs. St. John's, Newfoundland: ISER Books.
2000b Introduction. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 11-24.

Neis, B., L.F. Felt, R.L. Haedrich, and D.C. Schneider
1999a An Interdisciplinary Method for Collecting and Integrating Fishers' Ecological Knowledge into Resource Management. In D. Newell and R. E. Ommer (Eds.), *Fishing Places, Fishing People: Traditions and Issues in Canadian Small-Scale Fisheries*. Toronto: Toronto University Press. Pp. 217-238.

Neis, B., D.C. Schneider, L.F. Felt, R.L. Haedrich, J. Fischer, and J.A. Hutchings
1999b Fisheries Assessment: What Can Be Learned from Interviewing Resource Users? *Canadian Journal of Fisheries and Aquatic Sciences* 56(10):1949-1963.

Nowotny, H., P. Scott, and M. Gibbons
2001 *Re-Thinking Science: Knowledge and the Public in an Age of Uncertainty*. Cambridge.: Polity press.

Pálsson, G.
2000 'Finding One's Sea Legs': Learning, the Process of Enskilment, and Integrating Fishers and Their Knowledge into Fisheries Science and Management. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 26-40.
Pauly, D., V. Christensen, J. Dalsgaard, R. Froese, and F. Torres
1998 Fishing Down Marine Food Webs. *Science* 279(5352):860-863.
Pickering, A.
1992 *Science as Practice and Culture*. Chicago: University of Chicago Press.
Poizat, G., and E. Baran
1997 Fishermen's Knowledge as Background Information in Tropical Fish Ecology: A Quantitative Comparison with Fish Sampling Results. *Environmental Biology of Fishes* 50(4):435-449.
Power, N.G.
2000 Women Processing Workers as Knowledgeable Resource Users: Connecting Gender, Local Knowledge, and Development in the Newfoundland Fishery. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 189-204.
Prince, J.D.
2001 Ecosystem of the South East Fishery (Australia), and Fisher Lore. *Marine and Freshwater Research* 52(4):431-450.
Ratzan, S.C.
1998 *The Mad Cow Crisis: Health and the Public Good*. London: UCL Press.
Ruddle, K.
1994 Local Knowledge in the Folk Management of Fisheries and Coastal Marine Environments. In C. L. Dyer and J. R. McGoodwin (Eds.), *Folk Management in the World's Fisheries: Lessons for Modern Fisheries Management*. Niwot: University press of Colorado. Pp. 161-206.
Sarda, F., and F. Maynou
1998 Assessing Perceptions: Do Catalan Fishermen Catch More Shrimp on Fridays? *Fisheries Research* 36(2-3):149-157.
Shackley, S., and B. Wynne
1996 Representing Uncertainty in Global Climate Change Science and Policy: Boundary-Ordering Devices and Authority. *Science Technology & Human Values* 21(3):275-302.
Sillitoe, P.
1998 The Development of Indigenous Knowledge - A New Applied Anthropology. *Current Anthropology* 39(2):223-252.
Steele, D.H., R. Andersen, and J.M. Green
1992 The Managed Commercial Annihilation of Northern Cod. *Newfoundland Studies* 8(1):34-68.
von Herbing, I.H., I. Kornfield, M. Tupper, and J. Wilson (Eds.)
1998 *The Implication of Localized Fish Stocks*. Itacha: NRAES.
Warren, D.M.
1998 Comments to Sillitoe: Indigenous Knowledge and Applied Anthropology. *Current Anthropology* 39(2):244-245.
Wroblewski, J.S.
1998 Substocks of Northern Cod and Localized Fisheries in Trinity Bay, Eastern Newfoundland and in Gilbert Bay, Southern Labrador. In I. H. von Herbing, I. Kornfield, M. Tupper, and J. Wilson (Eds.), *The Implications of Localized Fish Stocks*. Itacha: NRAES. Pp. 104-116.
2000 The Colour of Cod: Fishers and Scientists Identify a Local Cod Stock in Gilbert Bay, Southern

Labrador. In B. Neis and L. F. Felt (Eds.), *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St.John's, Newfoundland: ISER Books. Pp. 72-81.

Wynne, B.
1996 Misunderstood Misunderstandings: Social Identities and Public Uptake of Science. In A. Irwin and B. Wynne (Eds.), *Misunderstanding Science? : The Public Reconstruction of Science and Technology*. Cambridge: Cambridge University Press. Pp. 19-46.

Signposts at the Border: A Comment on Holm

Barbara Neis
Memorial University of Newfoundland
bneis@mun.ca

Holm suggests that FEK research should be evaluated in terms of the extent to which it is achieving its potential to move fisheries science and management from reliance on Mode-1 type science towards a more contextualized, democratized Mode-2 type science. His review of some of the FEK research leads Holm to conclude that much of it actually reflects a return to Mode-1 ideals, an outcome he attributes to an ambiguity in FEK research between critiquing science and its own identity as a science. At one level, the failure of traditional Mode-1 fisheries science is often used to legitimize attention to FEK, while at another level FEK researchers tend to argue that FEK is like science and can be combined with findings from traditional scientific research. Instead of tearing down boundaries between lay and scientific knowledge, FEK researchers have tried to span them by turning fishermen into 'little scientists' and turning their knowledge from 'a vast, heterogeneous, contextual, politicized body of knowledge, FEK, into a small, homogeneous, non-contextual and de-politicized body of knowledge, FEK*'. Holm also argues that FEK research is not very good Mode-1 science because it tends to lack methodological rigor, relying in its stead on appeals to the relevance of FEK as a source of hypotheses and new data for science. The underlying problem with FEK research, he concludes, lies with the model of science accepted by FEK researchers who tend to see a gap between the world and the objects of science instead of seeing science as 'skills, practices, interventions and networks', i.e. as embedded in the world.

It is good to see a social scientist with a background in the sociology of knowledge taking an interest in FEK research. This is a potentially fertile area for research of this kind from which FEK research can substantially benefit. Holm argues correctly that contemporary interest in FEK partly reflects the crisis of legitimacy experienced by traditional fisheries science and management precipitated by the collapse of many 'managed' fish stocks. He also correctly sees it as an attempt by new stakeholders to enter into the fisheries science and management fray. I agree with Holm that FEK research involves not simply reporting but rather translations and transformations of the knowledge of fish harvesters into what perhaps should be seen as a third kind of knowledge linked to FEK/natural science and social science. I also agree that the translation process is a fertile area for investigation. Finally, Holm has correctly identified an important ambiguity within FEK research. Despite these strengths, there are several problems with Holm's analysis.

While Holm advocates that FEK research should promote a Mode-2 view of science he never clearly explains what this would look like in the case of fisheries science; nor does he situate critical reviews such as his own in relation to Mode-1 and Mode-2 science.

What are the networks, skills and practices that have influenced his research (including this piece of work)?

Holm is researching FEK researchers and their knowledge production. How-

ever, his analysis largely ignores the embeddedness of FEK researchers including the relationships between their networks and interventions and their approach to knowledge production. As a result, he fails to differentiate between FEK researchers in terms of their objectives, assumptions, approaches and the influence of the networks within which they are working on their research. People who do FEK research have come to it from different places and do it in quite different ways. Holm's summary gives us no sense of this diversity within the literature or the relationship between their networks, experiences and their research. Many of those he accuses in the paper of subordinating FEK to science are in fact natural scientists (Fischer 2000; Wroblewski 2000; Hutchings 2000) who did not come to an interest in FEK from disillusionment with Mode-1 science but from limited critiques of standard fisheries science – particularly bureaucratically embedded fisheries science. The work by social scientists such as Pálsson (2000), Roepstorf (2000) and Maurstad (2000) receives little attention. Their work is much more reflexive, pointing variously to how their own embeddedness and that of fish harvesters shapes their knowledge production activities.

Holm's work appears to suffer from the same struggle between critique of science and identity as science that he has identified within FEK research. A Mode-2 science approach to understanding knowledge production within FEK would try, as writers like Maurstad (2000) have done, to understand the processes that have tended to push FEK research in the direction of Mode-1 science. In my own case, most of the FEK research I have carried out has taken place in the context of interdisciplinary research teams involving myself as the single social scientist working with several different natural scientists and often students trained primarily in natural science. Fairly extensive knowledge about ecology, fisheries science and fishing is a prerequisite for FEK research and in the course of trying to learn this knowledge and learn from fishermen about their ecological knowledge it is perhaps not surprising that, like Maurstad (2000), I became somewhat 'trapped in biology'. This dynamic and the prominence of natural science researchers within FEK have contributed to the poorly developed thinking within FEK research related to such concepts as 'experts', 'knowledge' and 'knowledge production'.

While Holm argues correctly for more attention to social relationships and culture as factors in FEK and science, it is noteworthy that it is precisely our use of contextualizing variables like age, vessel, gear, crew structure, location and education that Holm sees as evidence of our attention to turn FEK into Mode-1 science. Contrary to Holm's explanation for the emphasis on these variables in FEK research, I see exploring the ways these different variables mediate FEK and science not as ways to distinguish between polluted and unpolluted knowledge or between stupid and bright fishermen so much as prerequisites for treating science and FEK symmetrically and for helping us see how both are social and ecological constructs. By this I mean that they are products not simply of social relationships, networks, culture, power (the focus of Holm's discussion) but also products of ecology – i.e. the spatial, temporal and ecological dimensions of both knowledge systems tend to be heterogeneous and different and this affects what gets observed and how this is interpreted (Neis and Morris 2002; Neis and Kean in press).

Just as fisheries scientists are aware that there are different paradigms within fisheries science, fishermen are, I think, quite aware that FEK is heterogeneous; that

there is often disagreement between them. Trying to understand with them the way their vessels and gear as well as when and where they have fished might mediate their observations and interpretations is not something they find uninteresting, unimportant or, in my experience, offensive. I acknowledge, however, that we need to study more systematically how they interpret and respond to each other's knowledge claims and that the work we have done has seldom involved sufficient consultation and review of the results with fish harvesters.

For me, a major issue with fisheries science is the possibility of a shifting baseline, i.e. the risk that as each new generation of scientists begins their work the fish size, abundance and species diversity that they encounter becomes for them the baseline against which subsequent abundances are assessed and, as a result, this baseline can shift across generations (Pauly 1995). I think similar risks exist within FEK and this is one of the reasons why we need to interview different generations of fish harvesters and try to reconstruct ecosystem change. It is also one reason for sharing the results of our research with fishermen. How we do this and whether or not the result will be more democratic science depends only to a limited degree, however, on how I do my work. Other, larger institutional factors over which I have limited control are likely to play a more substantial role.

When I started working in this area, I tended to see FEK and fisheries science as monolithic. Not surprisingly, I quickly found that this was not the case.

Similarly, FEK research is not monolithic. It is already, even though it is little more than a couple of decades old, quite nuanced and some lively debates exist. There is no way to avoid selecting, simplifying, interpreting and translating our experience of the world around us. It is true that it is not easy to separate observation and interpretation but there are different types or levels of interpretation. FEK publications are, like most of our material world, frozen social relations. Analyzing those publications means taking into account the social relations, including the peer review process and relations between academic researchers, government researchers and fishermen in different spatial and temporal contexts that have shaped the production of FEK. It also means, in my view, tracking the heterogeneity of FEK research, reasons behind that heterogeneity, changes over time in science, FEK and the relationship between them (Hutchings, Neis and Ripley 2002), as well as changes in the work of individual researchers and reasons for those changes. Ideal types like Mode-1 and Mode-2 science might work as signposts along the road to change but they don't tell us much about how to build the institutions and networks needed to get from one to the other or about the best way to ensure there are still some fish left in the ocean when we get there.

References

Hutchings, Jeffrey A., B. Neis and P. Ripley

2002 The 'Nature' of Cod, *Gadus morhua*. In: Rosemary E. Ommer (Ed.), *The Resilient Outport: Ecology, Economy, and Society in Rural Newfoundlan*d. St. John's: ISER Books. Pp. 140-185.

Maurstad, Anita

2000 Trapped in Biology: An Interdisciplinary Attempt to Integrate Fish Harvesters' Knowledge into Norwegian Fisheries Management. In: B. Neis and L. Felt (Eds.), *Finding Our Sea Legs:*

Linking Fishery People and Their Knowledge with Science and Management. St. John's: ISER Books. Pp. 135-152.

Neis, Barbara and M. Morris

2002 Fishers Ecological Knowledge and Fisheries Science: The Capelin Fishery, 1975-1996. In: Rosemary E. Ommer (Ed.), *The Resilient Outport: Ecology, Economy, and Society in Rural Newfoundlan*d. St. John's: ISER Books. Pp.205-240.

Neis, Barbara and Rob Kean

In Press Why Fish Stocks Collapse: An Interdisciplinary Approach to Understanding the Dynamics of 'Fishing-Up'. In: Reginald Byron (Ed.), *Retrenchment and Regeneration in Rural Newfoundlan*d. Toronto: University of Toronto Press.

Pálsson, Gísli

2000 'Finding One's Sea Legs': Learning, the Process of Enskilment, and Integrating Fishers and Their Knowledge into Fisheries Science and Management. In: B. Neis and L. Felt (Eds.) *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St. John's: ISER Books. Pp. 26-40.

Pauly, Daniel

1995 Ancedotes and the Shifting Baseline Syndrome of Fisheries. *Trends in Ecology and Evolution* 10(10):430.

Roepstorff, Andreas

2000 The Double Interface of Environmental Knowledge: Fishing for Greenland Halibut. In: B. Neis and L. Felt (Eds.) *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St. John's: ISER Books. Pp. 165-188.

Bridging the Equality of Embeddedness. Commentary on Holm

Evelyn W. Pinkerton
Simon Fraser University, School of Resource and Environmental Management
epinkert@sfu.ca

Fisheries researchers have come to count on Petter Holm to challenge established paradigms in an insightful, thorough, and balanced fashion, and to explore new and useful ways of framing discussion. True to form, Holm questions the way some social scientists have characterized fishermen's ecological knowledge (FEK). Here he opens a middle ground on which players will have a better chance of engaging an authentic dialogue: natural and social scientists, policy-makers, managers, and fishermen. We must applaud this much-needed initiative, and hope that all of these players can keep an eye on the importance of having this discussion, above and beyond the smaller quibbles that each will necessarily have with parts of Holm's argument. All comments below should be viewed within this larger awareness of the value of his contribution.

Many will wish, for example, that Holm had focused the center of the discussion on the key point he raises only in the concluding section: that 'it is not ...useful to grant to science epistemological privileges', that [natural] science cannot lay claim to the entire truth because it too must be understood 'in terms of skills, practices, interventions, or heterogeneous networks'. Holm stops short of explicitly laying out these concepts. Unfortunately, many will not understand this shorthand to mean that natural science is as fully embedded within its socio-political context as is social science or the fishermen's knowledge that social scientists document. Natural scientists who most need to hear this are the very ones least likely to have read Holm's sources.

One wishes, therefore, that Holm had illustrated these points from his own natural science background, or through material in the body of the paper. For example, the Newfoundland cod collapse stories told earlier could have included the fact that there were contending natural science theories and associated methods regarding how to measure the health of the cod stocks. The Catch Per Unit Effort (CPUE) method of collecting data from the corporate-owned fleet was the favored tool of a group of natural scientists (or their bosses). Yet this data source supplied information which was so faulty a measure of cod abundance that up to the year before the collapse, the owners of the corporate fleet claimed they were fishing sustainably and managers ignored contending claims both from discard data and from inshore fishermen that the stocks were collapsing (Pinkerton and Weinstein 1995; Finlayson 1994; Rose 1992; Harris 1999). Thus the struggle here was not simply the one between the unexamined FEK of inshore fishermen (who claimed the cod were collapsing) and those natural scientists collecting the CPUE data from the offshore fleet. In Newfoundland, as elsewhere, other important actors included: (1) the offshore fishermen and corporate fleet owners who were benefiting from the current approach natural science approach to data collection and analysis; (2) the natural scientists ignored by managers who believed that other data sources were key and

should figure more prominently in the calculus of stock abundance (e.g. data on discards of smaller cod by the offshore fleet, discards which skewed the CPUE data; data on the increasingly younger age classes taken in inshore traps); (3) the offshore fishermen who reported the undisclosed discards in the inshore fishing villages where they resided; (4) the political and modeling culture of the Canadian Department of Fisheries and Oceans (including the laboratory culture in vogue, control of the flow of information, knowledge, and funding); and (5) the Canadian federal government's view of the relative importance of fisheries and offshore trade (leading to their reluctance to take action against foreign vessels over-fishing cod within Canada's 200 mile zone of jurisdiction). One would like to see Holm, or others, flesh out these stories so that the extent to which *both* natural and social science are situated, contentious, and politicized, with winners and losers among not only fishermen and their ethnographers, but also among natural scientists and managers associated with any particular approach to data collection and analysis could be clearly seen. Although he says this in the abstract, Holm stops short of making a convincing case by showing *how* these relationships work.

Curiously, Holm also fails to identify one of the most important oppositions underpinning his discussion: not that between simply science and policy or science and nature, but that between natural science and social science. Holm neglects throughout to identify social science as a science (since the majority of those he cites who document and theorize FEK are social scientists), even while showing that we need an agora where the two sources of knowledge can mix. He appears sometimes to confuse epistemologically the social scientists who theorize FEK with the fishermen who are the sources of FEK. It is not clear whether Holm ignores this distinction deliberately to create a bridge between the 'two solitudes', or whether he does not recognize that his language throughout privileges natural science in the very way he claims to dispute. An explicit acknowledgment of social science would lead to a discussion of the qualitative methods through which social scientists deal with validity and reliability, and how qualitative and quantitative methods can complement each other. This would surely help bridge the solitudes.

Some acknowledgment of a key arena in which FEK is activated would have helped Holm underline his point about embeddedness. Yes, FEK is politically situated, but in ways that go far beyond simple ignorance or fact distortion for allocation advantage. The possession of FEK automatically gives power to individuals or groups who may contest the way both scientists and government use the data. Thus Alaska fishermen have been known to destroy fish tags in front of managers (showing they took the trouble to collect the data but won't share it because they disagree with how the data is being interpreted and used), or Canadian aboriginal fishermen may deliberately withhold knowledge of their fishing practices and knowledge, believing it will be used against them in treaty negotiations. These fishermen see FEK in the context of their assertion of claims to share power in fisheries management. They recognize their importance as sources of data to managers and scientists and take the position that the quid pro quo in data sharing is co-management rights. Their refusal to decontextualize the data in this situation is thus a deliberate exertion of power, because they want the data to inform their own fisheries, not merely be used to develop general models that may not be relevant to them. More fundamentally, they see data collection as inextricably connected to data analysis, harvest plan-

ning, allocation planning, habitat protection, enforcement, and/or even the policy-making in which they aspire to participate. Thus they could be seen as demanding a re-contextualization of FEK as intrinsic to their own partnership in management. I do, however, applaud Holm's point that social scientists should not be reducing FEK-holders to 'little scientists' who have to clean up their act in order to be acceptable to natural scientists. The co-management literature documents inverse processes, in which managers and natural scientists who adapt to the way fishing communities wish to share FEK learn a great deal in the process. Natural scientists and managers often have an initial reaction to co-management arrangements as frightening because they involve power sharing on new terrain without assurances recognizable to natural scientists that the data yielded is valid. The important discussion Holm has begun will thus have to eventually include a discussion of what co-management partnerships ultimately could deliver in terms of increased resources and effectiveness in management decision-making.

References

Finlayson, A.C.

1994 *Fishing for Truth: A Sociological Analysis of Northern Cod Stock Assessments from 1977 to 1990.* St.John's, Newfoundland: ISER Books.

Harris, M.

1999 *Lament for an Ocean: The Collapse of the Atlantic Cod Fishery*. Toronto: McClelland & Stewart.

Pinkerton, E. and M. Weinstein

1995 *Fisheries That Work: Sustainability Through Community-Based Management*. Vancouver, B.C.: The David Suzuki Foundation.

Rose, G.A.

1992 Indices of Total Stock Biomass in the Northern and Gulf of St. Laurence Atlantic Cod (Gadus morhua) Stocks Derived from Time Series Analyses of Fixed Gear (trap) Catches. *Canadian Journal of Fisheries and Aquatic Sciences* 49:202-209.

It's Pretty Fishy . . .

Madeleine Hall-Arber
MIT Sea Grant College Program
arber@mit.edu

Holm's article delves into an important issue that parallels additional challenges currently facing many fisheries social scientists. How can fishermen's ecological knowledge (FEK) be incorporated into fisheries management policy without reducing it to simply another version of orthodox fisheries science, stripped of its value as something apart and enlightening by its own merit? He argues that the techniques researchers use to legitimize FEK sufficiently for application in policy removes its 'contextualization'. In the process, FEK reverts to a version of standardized science typical of Mode-1 science, far from the ideal of Mode-2 science posited by Nowotny *et al.* (2001).

Because fishery science serves as the basis for regulation, as Holm notes, it cannot be truly disinterested (also see Weeks 1995), despite great efforts to convince the regulated that the ideals of Mode-1 science have been adhered to and, therefore, the results constitute truth, providing an appropriate and dependable basis for policy. In fact, Mode-1 science, some may argue, is an ideal that has never actually been achieved. Science has always been affected by its context, particularly politics. Nonetheless, the image portrayed of science used for fisheries management emphasizes the formal, the ideal and depicts truth in numeric form. Whole numbers perfectly express Mode-1 science since they imply absolutes and exactitude. Regardless of social or cultural impacts of the regulations, who can argue with the complicated biological models that generate precise numbers?

As reports of failures to manage fish stocks for sustainability mount, though, the culpability of both policy and the science upon which it is based is being debated. Interestingly, in the Northeast United States, the questions raised about the science still focus on numbers. 'How many fish are there, really?' 'Is the target biomass too high?' 'What is the historical high catch that could be sustainable?' Or, 'How many of [specified] fish can I catch?' The answers to these questions offered by the regulated can differ quite radically from the answers offered by national government-employed scientists, but both groups validate the use of single stock assessments by articulating their answers within the same frame.

Nevertheless, perhaps because of a confluence of mistakes publicly acknowledged by the National Marine Fisheries Service (NMFS), a change in leadership of the two dominant management Councils in the region (New England Fishery Management Council and Atlantic States Marine Fisheries Commission), as well as the development of an active core of fisheries social scientists, a change in both the source and substance of data used in management decisions is developing. The most visible example of this change has been the increase in funding for collaborative research.

Holm takes FEK researchers to task for making fishermen 'speak the truths of science', essentially by verifying fishermen's observations before reporting them. The verification may be done by using the observations as a hypothesis that is then

tested by scientists, or by requiring independent observation by other fishermen (or by collecting and sorting, as Holm puts it). That the observations of fishermen have long been questioned by society is suggested by the informal definition of 'fishy', that is, 'inspiring doubt or suspicion' and 'fish story', that is, 'an implausible or boastful story' (American Heritage College Dictionary 1993). These expressions probably grew out of the apocryphal story of anglers who claimed to have allowed the big one to get away. The speed with which fish can deteriorate, shady business practices of some dealers, and the 'mysterious' operations of a working waterfront surely contributed to the extension of distrust to participants in the commercial fishing industry. Regardless of its origins, the stereotype of fishermen's observations as one of self-serving exaggeration, is a stereotype that FEK research takes pains to reverse.

Such distrust was not evident in the relationship between fishermen and government scientists when G.B. Goode was the U.S. Commissioner of Fisheries. In his 1887 book, *The Fisheries and Fishing Industries of the United States*, Goode often named fishing vessel captains whose observations and samples contributed to his understanding of the fish stocks and species behavior. Did the rejection of fishermen's knowledge claims as anecdotal (especially since they were not amenable to quantification) that Holm notes, parallel the rise in modern society of commodification?

FEK as an 'arena where different types of knowledges meet and mix' is a wonderful metaphor and the idea that fishermen's knowledge is 'relevant and useful for science and management' is indisputable. What remains unresolved by the article is how managers can use the different knowledge forms for decision-making and policy formation without the translation to numeric form ('making fishermen little scientists'). I would suggest that FEK researchers must still transform the fishermen's knowledge for it to be acknowledged and applied. Questions about representativeness, validity, quality and standards otherwise cast doubt, making it easier for managers to ignore FEK. The challenge that Holm raises is how to retain that part of their knowledge that extends outside the box of specialized science, while maintaining a sense of validity.

The same types of criticisms that are made of fishermen's ecological knowledge are also raised when the social or cultural context of the fishing industry is described. Social science is by its nature Mode-2 science. The context is the science, after all. Furthermore, the social and cultural context, and potential impacts of regulations, is required by U.S. law to be taken into consideration by management decision-makers.

But it is extremely difficult to quantify the results of social science research. More attuned to numbers than many other social scientists, some economists have attempted to develop techniques to quantify abstract concepts such as norms or values using contingent valuation techniques. However, the transformation of norms and values to monetary expressions for ranking or comparison may be more art than science. Values and norms for most people are complex, often contradictory or uncertain, and even malleable. Other social scientists use random sampling, statistics and percentages (that is, numbers) to present data that will be taken seriously. Otherwise, the information is dismissed as 'anecdotal' and ultimately unfit or at least unusable for application in decision-making or policy development.

Despite the credibility issues associated with FEK and the difficulties associ-

ated with resolving these while retaining its context, the process of building a 'better, more adequate and inclusive science' has begun. Recently, funding for collaborative research has proliferated in the Northeast region of the U.S. Either fishing industry participants or scientists may initiate projects, but each must partner with the other for the project to be funded. Many of the funded projects are technical fixes to species selection (for example, development of specialized gear), no particular challenge to the underlying legitimacy of either form of knowledge. Nevertheless, the projects are intriguing in that they do rely on science for the 'powerful technologies' that Holm identifies, 'writing, counting, and specialized metrologies' while relying on the fishermen's ecological knowledge for ideas about possible solutions to specific problems.

In addition, while only a few of the funded projects are specifically focused on the socio-cultural context of the fishing industry and fishing communities, the results of the process are similar. As Nowotny *et al.* (2001:246) suggest, the 'shared definition of problems, the setting of research priorities and even to some extent the emergence of new criteria of what it means to do good science may be affected . . .' and ' . . . the process of contextualization moves science beyond merely reliable knowledge to the production of socially more robust knowledge'. Also, with this process, expertise will become more 'socially widely distributed'.

What is disheartening is that the requisites of Mode-1 science so dominate our society that even the participants in the socio-cultural projects, who will be engaged in the analysis and interpretation of the data, insist that the research results be reported in as numerical form as possible to assure that they are weighed in the management process. The appropriation of the 'discourse of science' by fishers as a resource (along with political power and moral authority) in management has been noted before (Weeks 1995), whether this discourse can be broadened to incorporate fishermen's ecological knowledge in its fuller, richer context remains to be seen.

References

American Heritage College Dictionary

1993 *The American Heritage College Dictionary*. Boston: Houghton Mifflin Company.

Goode, G.B.

1887 *The Fisheries and Fishing Industries of the United States*. Washington: Government Printing Office.

Nowotny, Helga, Peter Scott and Michael Gibbons

2001 *Re-Thinking Science*. Cambridge: Polity Press.

Weeks, Priscilla

1995 Fisher Scientists: The Reconstruction of Scientific Discourse. *Human Organization* 54 (4):429-436.

Petter Holm Seems to Cross No Border

Jojada Verrips
University of Amsterdam, Department of Sociology and Anthropology
verrips@pscw.uva.nl

Petter Holm has written a fascinating and thought-provoking essay on FEK researchers, who – in contrast with orthodox scientific researchers – systematically seek to include the lay observations and interpretations of fishermen in their scientific work on the world of the fisheries and who emphatically plead for taking them serious in the policy sphere. Though he shows great appreciation for their efforts to bridge the gap between scientific and lay knowledge with regard to 'nature', he is not convinced that the strategy FEK researchers use to give the fishermen a voice in the administrative and political arena is the most appropriate. If I understand him correctly, then his main argument is, that FEK research not only implies a rather arbitrary, scientifically coarse and undeveloped filtering process of the mixed bag of knowledge fishermen have with regard to the natural environment they exploit, but also the transformation of fishermen into a particular kind of scientists. Contrary to what one might think, namely that this sort of research means a radical break with the situation in which only well-trained scientists produce relevant knowledge for the development of a sound fisheries resource management, it does not represent such a break at all, for FEK researchers go at great length to translate the observations and interpretations of fishermen or FEK *in situ* into FEK *ex situ* or FEK*, that is, just another type of scientific knowledge. Though they suggest combining the scientific and lay knowledge on the assumption that they do not differ fundamentally, they first filter and clean the fishermen's knowledge on dubious methodological grounds and then present it in a scientific format. FEK research therefore is no example of a mode-2 type science, but only a slightly modified version of mode-1 type science. 'While FEK research sometimes is presented as an epistemological parallel to the tearing down of the Berlin Wall, it has more in common with the construction and operation of a Checkpoint Charlie.' In short, this research is only a 'modest adjustment in boundary strategy', because FEK researchers still are the prisoners of an orthodox view on and of science. According to Holm it would be better to change this perspective 'in which knowledge is viewed in terms of ideas, theories and mental representations' for one in which science 'is understood in terms of skills, practices, interventions or heterogeneous networks'. This would imply the abolishment of all epistemological privileges and therefore place the relation between science and folk knowledge in a new light. Instead of a hierarchy of knowledge systems – one of which has to be translated into the other before it might, for instance, become relevant for management purposes –, one just accepts their juxtaposition. Or in Holm's own words: 'Now there is no need to force fishermen's knowledge into a scientific mould since the difference between fishermen's knowledge and scientific knowledge is the point. There is no need to object fishermen's knowledge, as orthodox science tends to, or to reject scientific knowledge, as FEK research sometimes pretend to, since both bring important things to the table.'

Though I find Holm's analysis of the position of FEK research fascinating,

I have a few comments and questions. My main problem relates to his suggestion to juxtapose scientific and lay systems of knowledge in one arena, so that they can meet and mix in a fruitful manner. In my view he does not pay sufficient attention to at least three important points; the difficulty to get rid of the hegemonic position of orthodox science, the conflicts which will therefore continue to occur between laymen and classical scientists with regard to the status of their interpretations and observations; and the development of a really balanced approach of the discourses with regard to the ecological environment of both (fishery) scientists and laymen. In general I think that Holm is too optimistic concerning the effects in the management sphere of the strategy he seems to propose. Though he talks about the abolishment of all epistemological privileges as a consequence of the adoption of another view on science, he overlooks the fact that such a step probably will not imply a radical change, at least not immediately, in the status and the prestige of what will be called science or scientific knowledge and practice. I think that it is rather naïve to neglect this side of the matter and to assume that a simple replacement of perspectives will lead to a fair treatment of the interpretations and observations of fishermen outside FEK research as a 'hybrid forum' or 'agora', that is, in circles of fisheries resource management or the policy sphere. How difficult it is to get rid of the notion that science produces another, better type of knowledge than laymen come up with is acknowledged by Holm in the conclusion of his essay where he states: 'From this starting point, there is hardly any doubt that a number of phenomena pertinent to modern fisheries management requires a scientific approach.' Scientists in his view thus basically remain the producers of (correct) interpretations and fishermen the providers of keen observations that, as he gallantly admits, in some cases might lead to a reformulation of targets of the former. It seems then that the abolishment of epistemological privileges, in spite of Holm's suggestion to the contrary, boils down to a rather well known, a-symmetrical division of labor between the two categories involved. Whereas FEK researchers do their utmost to translate fishermen's knowledge into a kind of scientific knowledge, so that it is not immediately put aside as non-scientific rubbish but instead taken seriously in the political arenas, Holm in the last instance refuses to do so. This entails the risk that we still have to wait for a long time before the situation is reached which he seems to prefer, as his rather radical view on science is far from widespread within academe, let alone in the wider society. I am afraid that an adoption of his viewpoint as long as the prestige of so-called scientific knowledge has not yet eroded – and why should it erode, as long as it provides us with seemingly correct or, as Holm suggests, harder and tougher interpretations of the world we exploit –, will not lead to the fertile meeting and mix of the two knowledge systems he is talking about. On the contrary, it will bring us nothing else but a continuation of fights over who in the last instance is producing and presenting valid and therefore acceptable knowledge: fishermen or scientists, or – as FEK researchers advocate – both. I think that Holm's essay would have gained in clarity and strength had he realized that it contains a contradiction between his claim that the adoption of another perspective on science on the one hand implies the abolishment of all epistemological privileges of science and on the other his privileging of the scientific knowledge system above other such systems at the same time on the other.

This brings me to my last point. I think that Holm's (Latourian) ideas on how the relation between scientific and non-scientific forms of knowledge best can be

perceived (as an in essence a-symmetrical one, at least where it concerns their eye-opening and revealing nature) form a serious obstacle for the comparative study of these forms, that is, of such questions as to when, where and why they show family resemblances. Whereas Holm seems to emphasize the difference (and inequality) of these forms, I would like to point to their occasionally striking sameness. In this respect I side with the FEK researchers, though with this important difference, that I see both sorts of knowledge systems as particular metaphorical products (or constructs) with regard to the reality we are and by which we are surrounded. That one nowadays not only can observe a continuity, but often also a striking overlap between lay and scientific forms of knowledge seems to be directly related to the high degree of education and schooling in many societies, not to mention the informative role of the modern media. Against this background Holm's emphasis on such differences as skills, practices, interventions, technologies between different knowledge forms can be put in perspective and be seen as similar boundary maintaining mechanism as theories and mental representations of orthodox science, which led FEK researchers to their translation work. Whereas in both cases the hegemony of a scientific universe of discourse seems to stand central, I suggest to be careful, because such an attitude might lead to serious problems of communication and therefore of policy making. A less outspoken viewpoint with regard to the relevance and usefulness of different forms of knowledge might lead towards better relationships between both scientists and, in this particular case, fishermen. Especially when scientists, orthodox or not, can not claim any longer to reveal ultimate truths, they have to be extra careful and diplomatic in presenting their insights as more valid and relevant and judging the interpretations and observations of experienced laymen as less relevant and valid.

The difference between Holm and me is that he seems to think that a simple juxtaposition of the different forms of knowledge of fishery scientists and fishermen without abolishing the hierarchical relationship between the two (as he apparently does not want) will more or less automatically lead to a better understanding between the two categories and therefore to a more effective management policy, whereas I evidently do not think so. Therefore I am in sympathy with those FEK researchers who seek to close the gap between scientists and fishermen by translating their knowledge. The adoption of Holm's perspective at this moment in time would mean no improvement to me, on the contrary, just a continuation of a situation which has been existing already for such a long time and which only leads to a lack of cooperation and conflicts between parties which need to work together in order to be very careful with all what lives in our seas and oceans and what is so crucial for our further existence on this globe.

Reply

Petter Holm

I would like to thank the commentators for insightful and inspiring perspectives on my paper. They represent partly overlapping, partly differing interpretations, of my paper, and form as a collective a useful complement to it. I actually agree with most of the points that are raised. This reply is therefore in a way superfluous. Since the editors have gracefully allowed me another 1000 words to bring home my points, I will point out a few tensions and unresolved issues.

The first goes to the confusion of my own disciplinary background. For Neis, I am a social scientist. For Pinkerton, I am a natural scientist. Actually, I am neither – or both – since I come out of the interdisciplinary fisheries program at the Norwegian College of Fishery Science. I am, just like FEK research, a hybrid. I should assure the reader here that I wouldn't bring this up if it had no bearing on the way these commentators read my analysis of FEK research. For Neis, my approach to FEK research overemphasizes the natural science-approaches, ignoring the interdisciplinary approaches to FEK, and Mode-2-like approaches of, say, Maurstad and Pálsson. Pinkerton, on the contrary, would have wanted me to focus *more* on the natural science aspects of FEK, for instance in the context of the Newfoundland cod collapse story. Although Neis and Pinkerton want to place my author-position at symmetrically opposite corners of the field, they are in agreement that I have not identified that field correctly. For Neis, a main concern is that FEK research is not monolithic; there are a number of different perspectives or paradigms, FEK research is embedded in political and management structures, and must be understood from such contexts. In Pinkerton's view I have 'failed to identify one of the most important oppositions underpinning his discussion: not that between simply science and policy or science and nature, but that between natural science and social science'. Like Neis, Pinkerton hence finds my analysis short on contextualization. Despite their differences, both Neis and Pinkerton think I simplify too much. That ideal types like Mode-1 and Mode-2 science, 'might work as signposts along the road, but they don't tell us much about how to build institutions and networks …' Do I really have to say that I agree here and that my attempt to describe FEK research is – and cannot avoid being – a simplified image? That FEK research must be idealized, distilled and dis-embedded from the practices in which it is produced in order to be re-inserted into a new practice, the sociological meta-discussion that MAST now provides a forum for? Of course there are great variations in FEK research. Of course there are important contexts to account for, some of which I may have missed or chosen to ignore. Of course FEK research is not monolithic. In order to transport such a complex object as FEK research reasonably intact to the reader through a few pages in MAST, a vast mass of details, some of them important, most of them not, must be sorted out. Such an object cannot avoid being a translation, and as such a drastic simplification. Here is my point: Just what FEK researchers do to the fishermen's knowledge, my paper does to FEK researcher's knowledge. While this sometimes can be problematic and illegitimate when fishermen's knowledge is concerned, since fishermen tend to be non-readers of academic books and journals, this is seldom the

case for FEK researchers' knowledge, since FEK researchers usually can read (although with varying degree of precision) and sometimes even write in their own defense. The result, then, is that I don't get to talk on behalf of FEK researchers, at least not without opposition. Instead of a ventriloquist act, we get a multi-vocal event.

For Hall-Arber, the main issue raised by FEK research, and that which she (correctly) reads my paper as a commentary on, concerns the importance and legitimacy of numbers within fisheries management. To Hall-Arber, the notion of FEK as an arena where different types of knowledges meet and mix, is a 'wonderful metaphor and the idea that fishermen's knowledge is "relevant and useful for science and management" is indisputable'. She is still not entirely happy, since my paper leaves unresolved 'how managers can use the different knowledge forms for decision-making and policy formation without translation to numeric form'. This is also an issue taken up by Verrips, who believes that the main problem in the paper is that it does not pay sufficient attention to the difficulty of getting rid of the hegemonic position of science and such things. Verrips goes on to charge me of contradiction, 'between [Holm's] claim that the adoption of another perspective on science on the one hand implies the abolishment of all epistemological privileges of science and on the other his privileging of the scientific knowledge system above other such systems at the same time on the other'. Verrips therefore wants to side with the FEK researchers, as if I were against them. I find this charge wrong but fascinating, not least because it echoes one that featured prominently in the debate over Arun Agrawal's (1995) paper 'Dismantling the Divide Between Indigenous and Scientific Knowledge', which I cite and discuss (cf. note 3). As it turns out, then, Verrips is not the only one who can participate in that well-proven academic game of issuing charges of contradictions against an opponent. While I could have retorted in kind here, I fear that we may never resolve this issue to everyone's satisfaction. The reflexive game – which this is a variation over – simply contains too much interpretive flexibility, hence being able to accommodate virtually any sustained effort of turning an opponent's position into one of internal contradiction. Resisting that temptation, I find the direction suggested by Hall-Arber's comments much more promising. While FEK research represents one, or as Neis would have it, a collection of different 'hybrid fora', another type of such fora has arisen in the context of participatory research, particularly in the US Northeast. Here, under the umbrella of the Northeast Consortium (http://www.northeastconsortium.org/index.html), fishermen and scientists meet, not only to exchange given knowledges, but also to design and participate in new research projects. This allows, as Hall-Arber notes, the establishment of shared definition of problems and the negotiation of research priorities, as well as joint participation in research. While the commentators may have a point when they think my paper is stronger in its critique of shortcomings of established FEK research than it is in laying out a viable alternative, I find such proliferation of Mode-2 type arenas as in FEK research and the Northeast Consortium highly promising.

References

Agrawal, A.

1995 Dismantling the Divide between Indigenous and Scientific Knowledge. *Development and Change* 26(3):413-439.