Knowing salmon well: indigeneity, biology and policy[1]

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1. Introduction

Sámi people call it 'Deatnu', 'the big river'. For Norwegians it is the 'Tana', and for Finns the 'Teno'. In the subarctic north of Scandinavia, Deatnu (we will use the Sámi name) is a major salmon river that has sustained Sámi people since prehistory and has been attracting tourists since the nineteenth century. It has also been a site of sustained conflict between local fisherpeople (often but not always Sámi) and the states of Norway and Finland. Tourist salmon fishing has prospered (it is a major and lucrative industry). At the same time the states (we'll focus mainly on Norway) have worried about falling salmon stocks and imposed substantial fishing restrictions. These have affected all who fish but have in particular squeezed traditional fishing to the edge of extinction. The story has been told by many (Pedersen 1986, Joks 2015, Skuvlaalbmá 2018). It is a struggle about policy, politics and continuing colonialism. It is a story of a profound clash between two quite different ways of knowing. And it is also a story of how traditional or local ecological ways of knowing (usually called 'LEK' or 'TEK') have been excluded from policymaking that rests on a particular version of fisheries biology.

In this article we start by briefly describing the politics and the policy of this conflict. Then we ask what it is to know salmon, and to know salmon well. We show that the salmon stock modelling used by the relevant agencies in Deatnu knows salmon in a way quite unlike local understandings. Though these fit poorly with each other, fish stock modelling reflects the concerns of the national states and the tourist industry, and it is this that shapes policy. Then we ask: does it have to be that way? Do good ways of knowing and managing salmon in Deatnu necessarily have to take this form? Does policy towards salmon and salmon fishing have to rest on this kind of biology? Our response to each of these questions is 'no'. To explore this, we show that biological ways of knowing salmon in Deatnu are a contingency that took the form they did in particular historical circumstances. And while we are cautious about dismissing fish stock modelling, we conclude by briefly suggesting four possible alternative ways of knowing and managing Deatnu salmon that would make greater space for local expertise and concerns.

Before moving on we want to observe that *terminology is never neutral*. The terms for making sense of the world always carry agendas, and as a part of this they prosper in particular places but not in others. So, for instance, phrases such as 'stock-specific spawning target' or 'exploitation rate' belong to the world of fish stock modelling, and imply the use of statistical reasoning, particular institutional contexts and concerns, and specific forms of learning. By contrast, words such as '*vuoggaguolli'* or '*čáhppes guolli*' belong to the alternative ways of knowing carried by local fisherpeople. As a part of this, they imply context-sensitive relations between locals and salmon that grow out of long experience of particular locations (Joks and Law 2017, 161). But here's the complexity. If this is the case for different ways of knowing salmon, it also applies just as much to the language we use to *make sense* of those differences. In short, *the terms for comparing different ways of knowing are themselves not neutral.*

This is important to our argument in two ways. The first has to do with the language of contrast itself: the distinction, for instance, between such pairs as, 'local ecological knowledge' and 'fish stock modelling'; or between 'Sámi' and 'Norwegian'. Such divides are useful – indeed they are nearly unavoidable – and we make use of them below. At the same time, they come with two major drawbacks. On the one hand, they tend to homogenise the terms that they are contrasting even though, for instance, in practice, 'local ecological knowledge' or 'fish stock modelling' are many different things. On the other hand, they make such distinctions sound neater and clearer than they are in practice. So, for instance, 'Sámi' and 'Norwegian' are entangled together in at least three centuries of asymmetrical colonial conversation, so each includes the other, as it were fractally, all the way down. So, this is our first problem with terminology: such large-scale contrasts are necessary, but they oversimplify. More strongly, they probably oversimplify in ways that favour the concerns of administration by using centralised ways of knowing that promise clarity and avoid that which is dispersed and taken to be 'vague.'[2]

Perhaps even more pressing in the present context, the word 'knowledge' itself carries an analogous series of agendas and blind spots. This is because the term implies that knowing relates to what we might think of as 'knowledge-objects' that can be distinguished from their surroundings. Perhaps located in someone's head or in a local collectivity (as in 'TEK' or 'LEK'), it usually assumes in addition that 'knowledge' created in one place can be transported elsewhere and combined in a privileged epistemological location with other 'knowledge' to achieve an overview. (An example would be the statistical overview of Deatnu salmon populations.) The word 'knowledge', then, also tends to resonate with centralised overviews, the institutional and material arrangements that make these possible, and probably (not necessarily) with management, policy and control.[3] That this is appropriate is taken for granted in most parts of natural science including fish stock modelling (and, to be sure, in the kind of social science text we are writing.) On the other hand, while this may be a good way of characterising centralising ways of knowing, how well it comes to grips with the decentred ways of knowing of local experts is much less obvious.[4] More worryingly still, it risks disqualifying these so they do not count as 'knowledge'. For this reason, in what follows we avoid talking of 'TEK' or 'LEK', are wary of the terms used in fish stock modelling to frame its arguments, and talk of 'knowing' rather than 'knowledge'.

2. Salmon fishing on Deatnu

Deatnu has been crucial to Sámi since prehistory. A line of communication, a boundless source of fish including especially salmon, an impressively powerful spectacle and a source of danger, it has offered sustenance and modest wealth for generations of Sámi people, and its salmon have been central to local life and culture. Currently this is under threat. The character of that threat depends on who you ask. Climate change, commercial fishing, tourist fishing, local net fishing, salmon farming, predators and fisheries policy are all on the list. Wealthy tourists started coming to Deatnu more than 130 years ago, but now tourism is a mass phenomenon especially on the Finnish side of the river. And currently, as we briefly noted above, according to state agencies and their fish stock modellers salmon numbers are falling as a result of overfishing by both locals and tourists, though local weir and net fishing is taken to be particularly harmful. This, then, is why fishing is now being

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[2] For this argument developed in other contexts see Magalhães (2020) and M'charek (2020).

[3] This argument was developed in STS (science and technology studies). See Latour (1998).

[4] For further discussion in a Sámi context see Joks, Østmo and Law (2020). But there are also STS literatures which argue that all forms of knowledge are contexted. See, for instance, Hutchins (1995).

regulated to the point that Sámi practices such as drift net fishing have all but disappeared (Pedersen 1986, Joks 2015, Law and Joks 2017, Skuvlaalbmá 2018).

Before the nineteenth century there were no outside regulations and Sámi fishers managed themselves (Pedersen 1991). But when state-building started in northern Scandinavia in the eighteenth century Sápmi was divided as borders were defined, and as time went on movement was curtailed, Sámi lands became subject to national laws, and Sámi people were normalised as national subjects (Oskal 2001, Minde 2003, Lantto 2010). As a part of this, regulations about salmon fishing began to appear in the late nineteenth century. (See Pedersen (1986), Joks (2015) and Skuvlaalbmá (2018, 19)). The specifics of the story are complicated but four interwoven themes run through this century-plus process of regulation.

> **Tightening rules**: As we have just noted, the rules have become progressively tighter, and as time has passed the consequences for local ways of fishing have become increasingly severe (Law and Joks 2019). These regulations reflect a particular version of fish-stock modelling which we discuss below.
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> **National differences**: Regulation is complex because the laws about fishing rights are linked to land ownership which are different in Finland and Norway. However, to a first approximation it is easier for outsiders to gain 'local' fishing rights in Finland than in Norway (Skuvlaalbmá 2018, 18-19).
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> **Sámi resistance**: Those regulations are deeply resented, criticised and resisted by many Deatnu people (Skuvlaalbmá 2018, 8). In addition, as we have noted above, many Sámi local experts are highly sceptical of fisheries science, its focus on statistics, and its claims about why salmon numbers are falling. (Joks 2015)
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> **Sámi legal recognition**: Though this has made little difference in practice, the significance of local ecological knowledge is recognised and the scientists working on Deatnu modelling are legally required to consider this.[5]

The context, then, is that Sámi and other local salmon fishing on Deatnu has been drastically restricted because fish stock modelling suggests that salmon numbers are not sustaining themselves. And though the modellers are supposed to relate to local ways of knowing salmon they do not really do this. But what is fish stock modelling?

3. Fish stock modelling

The most general answer is that it is a range of statistical methods for calculating and balancing the human *benefits* of fish stocks (usually but not always the number of fish caught and therefore by

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[5] Norway is a signatory to ILO Convention 169 (Johnsen, Benjaminsen, and Eira 2015, 230) and formally recognises the rights of its indigenous Sámi population. See Working Group on Salmon Monitoring and Research in the Tana River System (2012) and Tana Monitoring and Research Group (2018, 12)

implication their commercial value) against the *impact* on those stocks of human activity (usually but not necessarily the effects of fishing and overfishing.) The concern is that if commercial fishing is to be profitable it needs fish to catch, a focus that has often been urgent because collapses in economically important fish stocks induced by overfishing have sometimes been dramatic.[6] This tells us that fish stock modelling has been shaped by a combination of biological and social concerns – where often, perhaps usually, 'social' has meant 'economic'. There have been many attempts to model how these relate and turn predictions made by models into policy. So, for instance, one component of many of these has been the figure of 'catch per unit effort' (CPUE) which precisely juxtaposes economics or perhaps some other benefit ('effort'), with the biological availability of fish (Caddy and Mahon 1995, 6). This double logic is also visible in one of the core metrics of fish stock modelling, maximum sustainable yield (MSY), which was devised in the 1930s (Lassen, Kelly, and Sissenwine 2013). To simplify, this calculates the maximum number of fish that can be caught from a population each year while still ensuring that enough survive to reproduce the same population, so allowing the same catch following year.[7] MSY, then, is an overview that is also a policy and management tool. We need to add two further observations. First, fish stock modelling mostly works with single, so-called 'target' species – for instance herring, cod or salmon. This is partly because the complex ecological webs that relate different species are extremely difficult to model (Yodzis 2001), and partly because commercial fisheries tend to focus on a single target species. And second, it models its target populations as age cohorts because most commercially important fish species reproduce only after several years, which means that population projections depend on the size of a series of different cohorts.

Deatnu modellers work in this target species tradition (Working Group on Salmon Monitoring and Research in the Tana River System 2012). They focus on the size and sustainability of salmon population cohorts. In general (we will qualify this statement below) they are only peripherally interested in other aspects of the biosphere. In addition, they distinguish between genetically distinct salmon populations that return to different parts of the river system. In creating their models they start by assuming that the Deatnu and its tributaries have only so much space for salmon to reproduce. This determines the 'carrying capacity' of the river which is the figure for the maximum possible salmon population. Then they reason that if this maximum is to be achieved, a minimum number of mature female fish (the 'spawning target') need to return to the river each year to lay eggs. Then they to (try to) count the number of returning female salmon (the 'spawning escapement') that actually make it back to breed (Working Group on Salmon Monitoring and Research in the Tana River System 2012, 31, 60-61). They do this using a variety of techniques, some automated and some not. Next they compare the spawning target (the number of female fish needed) with the spawning escapement (the number actually arriving). And it is this comparison that leads them to the dismal conclusion that we mentioned above: for most Deatnu populations the figure for those coming back is too low – and often dangerously so. The salmon are under threat. So why is this? The modellers consider various possibilities including parasites, new diseases and water quality. They also discuss and dismiss the importance of predators (to do this they move from their

[6] These include the failure of North Sea herring, Newfoundland cod, and Norwegian inshore cod populations. See Hamilton and Butler (2001), MacGarvin (2001, 18), Ulltang (2002, 443), and Jørgensen *et al*. (2007).
[7] For further discussion of this metric see Holm (1996) and Law and Joks (2019).

focus on a single stock to the interaction between salmon and predator species in the biosphere.) This leads them to their overall conclusion which is that there is simply too much fishing in Deatnu: in the language of fish stock modelling there is 'overexploitation'. The argument, then, is that the number of salmon caught needs to be cut so that it does not exceed the 'maximum sustainable exploitation' level – the latter being a close analogue of MSY, maximum sustainable yield.

We said a moment ago that fish stock modelling has been shaped by a combination of biological and social concerns where 'social' has often meant 'economic'. So how do these concerns work in Deatnu modelling? Biologically, the model is about sustainability: salmon population sizes need to hold up year after year despite the fishing. And as a part of this, it also reflects a concern with biodiversity, a focus absent in earlier generations of single species stock modelling.[8] This is why the modellers say that nearly thirty different salmon populations need protection, and not just one. But if these concerns are explicit, then the social and economic concerns embedded in the model are not. If economics is important, then this is certainly not a reflection of the commercial value of landed fish, for wild-caught (as opposed to farmed) salmon is scarcely a commercial commodity. Perhaps it reflects (albeit entirely implicitly) the considerable economic value of tourist fishing.[9] Then again, probably (and this is our guess), what is of most concern is something that at least partially escapes the logic of economics: the value of salmon fishing as a way of life and a form of recreation. Though this points us to an issue to which we will return, for there are at least two version of this social value. On the one hand, there is the value attached to recreational tourism. And on the other hand, there is the social value of the local fishing traditions, including Sámi net and weir fishing. Currently there is no explicit discussion of this at all, though, (and we are still reading between the lines) at least in policy and arguably in the modelling itself, recreational salmon fishing is being far more highly valued than local tradition.

4. Fish stock modelling and local ways of knowing

Such is the context. But how do stock modelling and local ways of knowing intersect in practice? As we explore this question, we will see that their differences are simultaneously epistemological, institutional, political, material, and metaphysical.

a. Quantification

As we have seen, fish stock modelling uses numbers, while local ways of knowing do not. For modellers this is the nature of their expertise: they count and they work with populations. This they take for granted: it is the best, perhaps the only way, of reliably detecting trends. For local experts, on the other hand, counting is disrespectful and boastful. You may know that there are fewer fish than there were last year, but you do not count your catch (Schanche 2004, 3) and neither do you count the salmon that you see in the river. In one way, then, the disagreement is quite

[8] Concern with biodiversity grew in the 1990s, and there have been lively debates between such approaches and single species fish stock modelling. See, for instance Rice and Legacè, (2007, 718).

[9] Figures for the direct value of salmon tourist fishing for the whole of Norway (not just Deatnu) are around €130m (Myrvold et al. 2019).

straightforward. Since Sámi experts are offended by counting, whilst for modellers it is essential, this means that most of what locals know simply does not fit with fish stock modelling (Working Group on Salmon Monitoring and Research in the Tana River System 2012, 31-31). Note, however, that this epistemological difference is also institutional and material, because statistics only thrive within specialist institutions (including those of science and policy) and travel poorly to other locations (such as those inhabited by Deatnu fisherpeople.) At the same time, they are also material, for they depend on specialist infrastructures including forms of expertise, tools of measurement, spreadsheets, databases, computers, journal issues and reports. In short, quantification implies difference from local ways of knowing in at least three intertwined dimensions: the epistemological, the material and the institutional.

b. Background causality and necessity

The next difference is more abstract: fish stock modelling works with a single causal model of the world, whereas Sámi ways of knowing do not. Modellers appreciate that the world is complex, but assume both that it may be empirically described, and that phenomena in the world are the product of background causes and effects that may in principle be modelled. There are many complications, (Law and Joks 2019), but as with quantification, this is a way of knowing that rests in a mix of the epistemological, the institutional and the material. In addition, however, it is also *metaphysical* precisely because the relevant parts of nature are (taken to be) the expression of a *set of background causal* mechanisms.

Local ecological ways of knowing work more or less differently. Sámi experts also see causes and effects. They say (we touched on this above) that predators protected by environmental legislation are a serious threat to salmon (Joks and Law 2017, 162). But alongside causes, in the Sámi world there are lively forces including the weather and the river itself whose actions cannot be predicted because they are endowed with wills and often, too, with moral sensibilities (Østmo and Law 2018, Joks, Østmo, and Law 2020). What happens is unpredictable or may happen for no directly explicable reason at all. '[T]hat fish was not meant for us' [*Diet guolli ii lean munnuide oidnojuvvon*] says Petter Somby, Deatnu fisherperson, watching a salmon being caught by someone else (Joks and Law 2017, 151). As with fish stock modelling, this is a way of knowing in which a distinct epistemology is sustained by institutional location, skills, and material circumstances. But it is also its own form of metaphysics because the world is 'social', broadly conceived, and certainly not a set of (mechanistic) causes and effects (Law and Joks 2019, Joks, Østmo, and Law 2020). The argument, then, is that fish stock modelling and local ways of knowing also are far apart metaphysically. They exist in – and they sustain – different worlds.

c. Nature and culture

As a part of this, *fish stock modelling separates 'nature' from 'culture', whereas Sámi ways of knowing do not*.[10] As we have just seen, for Deatnu fisherpeople there are patches of causality in the world, but the willed and discretionary 'social' extends into what stock modellers think of as nature.

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[10] See, for instance Berkes (2012) and Nadasdy (2003).

Again as we have also seen, fish stock modelling combines the 'natural' with (a largely implicit version of) the 'social'. Crucially, however, it also *distinguishes these and treats them in different ways*. This is the basis of MSY (maximum sustainable yield), but it is also embedded in the language used in fish stock modelling:

> 'There is very little biological basis for arguing that naturally occurring predators are a threat to salmon, and predation must rather be viewed as an integral and natural part of the ecosystems that salmon live in …'
>
> 'An overview of threat factors in the Tana show that overexploitation of salmon in the different parts of the salmon migratory system is the major threat factor for Tana salmon.' (Working Group on Salmon Monitoring and Research in the Tana River System 2012, 5)

These quotations (from one of the reports of the Deatnu modellers) suggest that there are two modes of catching salmon. There is the 'natural', which is what predators do. And then there is that which is not 'natural', so to speak the 'social', which is classified as 'exploitation' or 'overexploitation' (Skuvlaalbmá 2018,36). And that is the divide. Natural reality is what it is, indifferent to meaning and negotiation. It belongs to the world of science. Scientists try to model it and therefore hope to speak for nature. In the present context, institutionally linked as they are to policy, they successfully speak for nature in the context of management. But, and despite those institutional links, in this logic the social is analytically and metaphysically unlike the natural. It is the realm in which (human) actors have wills, concerns, and politics. Arguments about salmon numbers may pass from modelling into politics. The two sets of institutions are indeed tied together. But facts are distinct from values, and nature is separate from the world of policy.

d. Universality

Go back to the quotation in the section above and look again at the language: 'predation must … be viewed as a[…] … natural part of … ecosystems.' Note that there is no space for negotiation here. Instead we are being presented with a general causal claim. There are no exceptions. But how does this work? How does it sustain itself?

First the metaphysics. As we have seen, there is a natural world with its causes and mechanisms. But, an important additional point, this is a *single* natural world. Individual rivers differ, but the *mechanisms* that determine fish populations do not. These are causes and effects that are putatively universal, outside time and place. And then alongside this there is an epistemological claim. This is that (in this context) there is one good way of trying to know those natural mechanisms – and this is population modelling. To be sure, biologists may change their minds. The history of fish stock modelling is one of endless debate, tinkering, innovation and improvement. But (this is both about epistemology and institutional location) you have to qualify as a modeller to be able to debate the merits of this or that model. To join the conversation. This consequence? This is that if the Deatnu fishermen see things differently, then this is because they do not have the skills to join that conversation. They have simply got it wrong, epistemologically. And they are wrong in particular because they think intuitively, on the basis of experience and what they can see, rather than sampling and systematic data collection; because they know things qualitatively rather than quantitatively; and because they are 'subjective' rather than 'objective'.[11]

This is how the Deatnu fish stock modellers understand the difference. And it works well on the assumption that there is indeed a single, 'one-world', world (Law 2015) with universal mechanisms that may be (more or less approximately) modelled. It is also the metaphysics that underpins most science, for the fish stock modellers are only doing what almost every scientist does. And, we might add, it is a metaphysics that is also embedded in most forms of Western common sense. But there is an alternative way of thinking about this. This is to say, following recent work in anthropology and STS, that science misunderstands itself. And the argument runs so. Yes, the different branches of science seek to describe general mechanisms and universal laws. But those putatively general mechanisms are only universal (only appear to be universal?) when (because) they are located *within particular institutions, material arrangements and networks*. Places like laboratories, field stations, and state agencies and journals. They are, that is, only universal in places that have been formatted to make them welcome. But here is the corollary. The means that *they do not rule in other places*. And this is *not* because the latter have failed to understand the character of (a universal) truth. Instead, it is because in those other places there are other and different epistemological, institutional and material circumstances, and because those other places therefore generate quite other realities.[12] Such as the places of the Deatnu fisherpeople.

Counterintuitive though this may be to dominant Western metaphysics, such is our argument: this is that to a first approximation *Deatnu fisherpeople live in a different world with different realities*. Importantly, and as a part of this, their ways of knowing, their epistemologies, are not only unlike those of the modellers, but they are also more *modest*. So, as we have seen, they have patches of causal reasoning, but this is a world in which knowing is contexted, it is social, it only reaches so far, and no-one makes claims to universality. Thus, Deatnu fisherpeople talk to one another and have overlapping repertoires and narratives for thinking about and relating to salmon and salmon fishing. But the extent to which what they know can be detached from the pragmatics of fishing in a particular place to be moved elsewhere is limited and uncertain. The extent to which it can be added together to create an overview is similarly uncertain. Instead? Instead they know their part of the river, how salmon are likely to behave there, how the weather is likely to affect the fish in that location, and how the particular unfolding of the seasons might influence the ways in which the fish will behave. But none of this is created to be moved elsewhere. It is local. It does not make claims to universality. It is *knowing* rather than *knowledge*.

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[11] These terms or their near analogues are all to be found in Working Group on Salmon Monitoring and Research in the Tana River System (2012, 31ff)

[12] On laboratories, see Hacking (1992). On extending laboratory realities to agriculture see Latour (1988). On the multiplicity of medical realities see Mol (2002). On differences between indigenous and political and scientific realities see de la Cadena (2015) and Verran (2002). For an overview see Law (2015).

5. Histories

So how did fish stock modelling come to dominate its local alternatives? We have rehearsed one answer to this question. This is that there is a link between the universalising claims of science and the concern, often state-related, to create overviews in order to administer and control. But this relation is also the end of a long historical process, and in this section we touch on three crucial episodes in that history. We make this historical detour for two reasons. First, it underscores the contingency of Deatnu fish-stock modelling: it teaches us that in other circumstances knowing and managing salmon might well have been different. And second, and perhaps even more important, it suggests possible alternatives to the current divide between fish-stock modelling on the one hand, and local ways of knowing on the other. We return to this topic in the final section of the paper.

a. The origins of fish stock modelling

The notion of a *stock* came to fisheries at the end of the nineteenth and the beginning of the twentieth century. The presenting question was why herring catches in the Baltic and the North Sea were so variable. This was important for the reason discussed above: industrial fishing is expensive and owners worried about declining herring numbers (Gordon 1954, Ulltang 2002). There were two major ways of thinking about this. One was to say that for practical purposes the number of fish in the sea is inexhaustible, so stocks would return to normal as new fish arrived from elsewhere. Others including many who fished disagreed, and argued that different places are inhabited by different species of herring.[13] The idea was not new, but in a Darwinian era with much interest in species and racial difference, the idea that there might be morphologically distinct races of herring was popular (Sinclair and Solemdal 1988). And the implication of this way of thinking was bad news for fisheries, for if different races of herring lived in different places, migration was presumably less likely. But how to explore this?

Johan Hjort, the first director of the Norwegian Fiskeridirektorat (Schwach 2014), began to study North Sea herring before World War I using quantitative tools he had learned about when he was reviewing the life assurance arrangements for Norwegian fishermen.[14] These included the sampling methods used in the insurance industry to assess life-expectancies, methods that had been under development for at least half a century (Sinclair and Solemdal 1988, 207, Porter 1995, Schwach 2014, 1997). Hjort's reasoning was that these might be applied to fish numbers and their variations. Why not, for instance, see if it was possible to distinguish between age cohorts of North Sea herring, sample the size of those cohorts, make actuarial calculations of life expectancies, and projections of likely future numbers? Such was the idea, and though it took some time, in the end, as we have seen, this approach mirroring the logic of life insurance became popular in fisheries science. Henceforth instead of working descriptively, fisheries science would study *populations*, and instead of working qualitatively, it would use *quantitative* methods. And as we have also seen, its question, simultaneously economic and biological (Hubbard 2014, 365), became: *how many fish can be*

[13] Thomas Huxley probably ignored fishermen's concerns about declining numbers of fish to conclude that stocks were inexhaustible partly for reasons of class prejudice (Hubbard 2014, 365).
[14] He was initially sceptical about the idea of different species (Sinclair and Solemdal 1988, 207).

sustainably extracted from a fish population? Sometimes this logic worked well and sometimes it did not, and it went through many iterations and variations.[15] But for our purposes, the basic shape of fish stock modelling was set by Hjort and his fellow fisheries scientists before 1914.

b. The origins of populations

This, then, was a Norwegian (and subsequently an international) historical contingency. But where did the commitment to counting and populations that made this possible come from? For, and contrary to what many assume to be the case, *good science does not necessarily imply quantification*. Qualitative field sciences (botany, geology, palaeontology) have a long history that continues to this day, and even in the so-called hard sciences it took two centuries for quantitative methods and formalisms to become embedded in most branches of physics (Kuhn 1961). But if fish do not have to be counted and treated as populations, why did this idea appeal to Hjort? How did the actuarial techniques crucial to insurance companies come into being (Porter 1995, 105)? And why was the idea of numbering and enumerating so attractive in the first place?

The answers take us outside biology. Indeed, historians offer a range of overlapping responses to these questions, but most are elaborations of a single central thought that we have already discussed, the idea that *numbers are a potentially important tool for centralising power*. Aspects or versions of this argument include the following. One, that centralised control works best if local discretion and strictly local ways of knowing can be minimised.[16] Two, that this requires *standardisation*, which is a double process of excluding almost everything while formatting the world by creating and imposing uniform ways of knowing and measuring (Busch 2011). Three, that what is known needs to be in a form that may be *easily transported* from the periphery, where knowledge (not knowing) is collected, to a centre where it is collected Latour (1987, 1998). Four, that once it gets to that centre it needs to be *tractable* so it can be organised, collated, manipulated and understood as a whole (Latour 1990). Five, that this in turn depends on well-ordered *technical and administrative arrangements*. Six, that there is also need for correspondingly reliable techniques for *imposing the results* of all of this on those at the periphery who are to be controlled or dominated. That, in other words, the world has to be formatted twice over, first as an apparatus that generates a central overview, and then as a set of elements that can be ordered.[17] And seven, that since doing this is exceedingly *expensive*, it also demands paymasters with very deep pockets, which in practice in European history has usually meant either nation states or large economic enterprises.

All of this describes how science and policy work in practice in Deatnu, but the argument is much more general, and its basic logic is well caught by Michel Foucault (1979) who shows that while European states exercised power through symbolic acts of terror in the seventeenth century, by the

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[15] MSY is probably linked to US post World War 2 political manoeuvring (Finley 2011, Hubbard 2014, 373-375). And Petter Holm and Kåre Nolde Nielsen (2004, 2007) suggest that it was also popular partly because it could easily be integrated with administratively convenient catch allocations between different countries.

[16] The paradigm case is railway timetabling (Zerubavel 1982).

[17] For the Pasteurisation of France, see Latour (1988). For a larger argument about states, visibility, and their dysfunctional character, see Scott (1998).

middle of the nineteenth century they were working quite differently, with disciplinary tools for surveillance, administration, and correction. But does this lead to numbering? The answer is: no, not necessarily, though usually it led to paperwork which is reasonably durable, easily transported and assembled, and after the invention of the printing press, could also be fairly easily reproduced (Ong 1988, Eisenstein 1980). This paperwork came in a wide a variety of forms[18] all of which were important for the history of European states and commercial enterprises.[19] Nevertheless, numbers were particularly important for at least two reasons. One, with the right skills, material arrangements and institutions in place, they become particularly tractable. Bookkeepers, treasuries and accountants were powerfully combining and manipulating numbers long before the nineteenth century growth of statistical techniques (Yamey 1949, Bryer 1993). If you wanted an overview, then numbers were an excellent tool if you had the right techniques to hand. And two, as Theodore Porter (1995) has argued, they were particularly handy in contexts where there was little trust between competing political interests, because they offered the appearance of objectivity and transparency.[20]

Overall, then, it is no coincidence that as states grew in the nineteenth century, they judged it wise to count in order to know what it was they were (seeking to) govern. So, for instance, censuses were created, and for the first time in history nations systematically enumerated and classified their subjects as an aggregate of individuals in this newly created collective reality, the *population*. And once populations were crested and counted, other and quite new collective statistical realities, stabilities and trends could be created. Differences between cohorts, divisions by class, region, gender or ethnicity, crime rates and rates of mortality and morbidity, all of these came into being.[21] Whole new ways of understanding the world and whole new sets of realities were being born. And, this is the population context necessary to the creation of quantitative fisheries. So if Hjort was creative, he was also acting as a man of his time. Albeit in the face of resistance, he was doing what many others were also doing: working on the assumption that progress depended on ignoring local knowledge – and locally formatted knowledge – and replacing this with reliably centralised and probably statistical ways of knowing.

The conclusion? This is that a politics of statistical centralisation became appropriate in many contexts both natural and social, including those that confronted the creators of fisheries science. The argument needs to be carefully made, for statistics can be turned against the governors by the governed. However, to a first approximation, methods such as fish stock modelling *cannot* take the decentralised local knowledge of those who fish seriously without at the same time undoing their implicit commitment to a particular centralising mode of administration and politics.[22] But at the

[18] These include narratives and stories; visualisations; maps; legal documents; land titles; money (we edge towards numbers); invoices and receipts; balance sheets; and statistics of all kinds.
[19] Particularly relevant for present day Sápmi are cartography and cadastral surveys (Mitchell 2002).
[20] Often it is the weakest forms of science that most insist on quantification (Porter 1995, 200).
[21] Suicide rates showed regularities and differences between different populations (Durkheim 1951).
[22] This is an argument that also needs to be handled carefully for several reasons: one, counting may produce private numbers; two, it does not necessarily imply *state* centralisation; three, as we have noted in the text, once they are established, numbers may also be turned around and used as forms of resistance in (possibly

same time, the historical contingency of these population studies reminds us that there are other ways of knowing and – or so we will suggest – managing fish.

c. The origins of facts and values

As we saw earlier, to use numbers in this way is to make a series of assumptions that are not shared by Deatnu fisherpeople: first, that there are general mechanisms at work in the world; second, that these and the facts that lead to them may indeed be described; three that those descriptions may (and need to) be transported from where they were made; and four, that they can (and need to be) separated from politics, or more generally, from values. In short, the bedrock on which contemporary natural science (including population statistics) rests, simultaneously separates the *local* from the (purportedly) *general*, and *facts* from *values*. Without assumptions such as these notions of population and fish stock modelling make no sense. So where did these assumptions come from?

One answer takes us to London in the 1660s and the 1670s and a moment of particular importance for the birth of modern science. To understand what happened, we first have to note that in early modern Europe what we now think of as 'nature' and 'culture' were not distinct. Indeed, the terms are an anachronism. And neither was there a large division between 'facts' and 'values'. This is why the Inquisition prosecuted Galileo for arguing that the earth circled the sun. Thus, since the church taught that the earth lay at the centre of the universe, in making this claim about the universe Galileo was also disrupting the moral order. And since the church and temporal power were intertwined, he was also trespassing into politics. This world in which truth and values necessarily went together was the context in which Robert Boyle, natural philosopher and member of the newly created Royal Society of London, found himself after 1660 when he started large scale experiments with an air pump. He risked potential charges of political and theological subversion. So how was this handled?[23]

The answer is that a series of negotiations led to an historically important agreement that he might explore the character of air and air pressure, but only if he stuck to down-to-earth, matter-of-fact, and moment-by-moment descriptions of the experiments. He could, that is, modestly describe putative *facts* about air. But what he was *not* to do was to move beyond those facts to speculate about their larger significance. The latter belonged to the realm of values – to theology and to politics. The significance of this agreement? It drew a line between the experiments and facts of this newly created natural philosophy on the one hand, and what we would now think of as 'the social' on the other. (The term is an anachronism, but it is difficult to avoid using it.) Henceforth, these were to belong to different domains. This was controversial – some dissented – but the distinction held, and it was to underpin developing European understandings of the role of inquiry in natural philosophy, its value-neutral descriptions, and the relation between scientific inquiry and politics. Henceforth natural philosophy (later 'science') would in large measure work by discovering facts and

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centralised) political spaces; and four, numbers may be reconstituted in ways that enact progressive forms of politics and resistance. For this argument in the context of indigeneity see Walter and Andersen (2013).

[23] In what follows we draw on Shapin and Schaffer (1985), and Shapin (1984).

hypothesising relations of cause and effect in the natural world.[24] And then, if appropriate, these might be passed to the realm of 'the social' where questions about value, rights and wrongs, goods and bads, and political or policy responses, might properly be judged.

Fast forward. This widespread set of epistemological, metaphysical and institutional divisions was at work a century and a half later at the beginning of the nineteenth century. Though they are historically remote from the world of fish stock modelling, they were the necessary foundation for the creation of the idea of statistical population that we have briefly described above. And they were the necessary foundation of the twentieth century fisheries science that drew upon those statistics and the notion of the population. And this means that they also frame present-day disputes between fish stock monitoring and Sámi ways of knowing, for as we have seen, in the latter there are few distinctions between either facts and values, or nature and culture.

To this we want to add one further but crucial point. For Boyle, since it was always possible that experiments might be wrong, these needed to be witnessed and checked by others. In practice, this either meant that the right kind of person (independent and therefore disinterested men of means) should attend and witness the experiments in person.[25] Alternatively, in an era when travel was difficult and slow, they might be witnessed by appropriate people who could read and judge Boyle's exhaustive account of those experiments. And such indeed, was the rationale for Boyle's experimental descriptions. The significance of this? This is a question with two responses relevant to the Deatnu dispute. One, it created an epistemological, institutional and material mechanism, the scientific report, which allowed descriptions of the world to be *abstracted and transported* to other locations. It distinguished, that is, between the 'local' and the 'general' by creating a class of supposedly disinterested persons able to judge the reliability of knowledge about the natural world because they qualified as proper witnesses. And two, it generated a particular version of the metaphysical assumption that *there are general mechanisms at work in the physical world*. Indeed, that when they are properly witnessed, the facts are invariant. That there is universality, and that we live in a single world.

6 Conclusion: Other ways of knowing salmon well

Sámi ways of knowing salmon in Deatnu are local, context-related, qualitative, only partially articulated, and do not travel well. They work on the assumption that much of what happens in the world is not readily explicable, they do not readily distinguish between nature and culture, they presuppose a morally sensible world, and they do not claim privilege. And they are being squeezed for a mix of, institutional, political, material, metaphysical and epistemological reasons. None of this applies to fish stock modelling. The latter, we have suggested, treats itself as a provisionally accurate description of a natural world shaped by population-relevant causes and effects. We have also argued that it rests on: quantification; the idea that events in nature reflect the operation of general

[24] This requires a further genealogy beyond the scope of this paper, but points to the importance of the monotheism of the Judaeo-Christian world-view with its rejection of paganism, and the idea that a single God designed a rationally functioning universe that might be traced and known. See Worster (1994, 28ff).
[25] On the (class-based) legal context that led to this judgement see Shapin and Schaffer (1985).

causal mechanisms; a distinction between nature with its mechanisms on the one hand, and the human world with its values on the other; and the assumption that scientific and reliable ways of knowing are abstract, transportable, and appropriately collected in privileged epistemological locations. Finally, we have linked this to a concern with knowing the world in ways that secure the possibility of central power and control

We have also drawn on three episodes in the history of science to show how this arrangement grew out of particular biological, economic, social and political contexts. We did this to emphasise the contingency of that history, to erode the epistemological certainty exhibited by fish stock modelling in Deatnu, but most importantly, as a resource for imagining alternative epistemological, institutional, political, and material ways of knowing salmon well in Deatnu that might accommodate local understandings and practices. At the same time we also insist on the need for caution. This is because fish stock modelling also engages with social and natural realities in particular and more or less powerful and sometimes effective ways. So, and notwithstanding the general direction of our argument, we are not suggesting that it should be lightly abandoned. We are not even suggesting that it is wrong, for within its epistemological and institutional context it generates substantially workable knowledge of its particular realities. But the history that we have sketched above nevertheless suggests the possibility of a series of alternatives.

Ecosystem services. As we saw earlier, Deatnu fish stock modelling substantially reflects the value of salmon not as a commercial harvest but as an activity. But if it values tourist recreational fishing very highly, *this is not made explicit* because it hides in arguments about spawning escapement and biodiversity. This leads us to our first thought, which is to borrow techniques from *ecosystem services*. This recent approach tries to assess the broader benefits of the services offered by ecosystems. These include 'cultural services', for instance in the form of 'recreation, aesthetic, scientific, cultural identity, sense of place' (Costanza et al. 2017,5), the aesthetic, the spiritual (Millennium Ecosystem Assessment 2005, v) and indigenous ways of life (Millennium Ecosystem Assessment 2005, 57).[26] There are many unresolved questions about how to value such cultural services. Nevertheless, to focus on ecosystem services would force hidden assumptions about the value of different forms of fishing out into the open. In particular, it would require an *explicit and not necessarily quantitative debate* about the value to be placed on recreational tourist fishing as against local fishing. As a part of this, it would also reveal that any conclusion about this is not simply biological but also political.

Catch per unit effort. Our excursion into history suggests that knowing salmon well does not necessarily depend on population statistics. What this might mean in practice would need to be explored, but earlier we touched on one possibility, the 'catch per unit effort' (CPUE) technique that is part of the history of fisheries management. How might this be related to

[26] 'There are clear examples of declining ecosystem services disrupting social relations or resulting in conflicts. Indigenous societies whose cultural identities are tied closely to particular habitats or wildlife suffer if habitats are destroyed or wildlife populations decline. Such impacts have been observed in coastal fishing communities, Arctic populations, traditional forest societies, and pastoral nomadic societies.' (Millennium Ecosystem Assessment 2005, 54)

local ways of knowing? The answer is that we do not know. No doubt the notion of 'effort' would need careful consideration in a context in which fisheries are not industrial. No doubt, too, there would be need to distinguish the different kinds of 'effort' appropriate to different kinds of fishing. This would certainly require creativity. But while it would probably involve some version or other of counting, it is not impossible that it might lead to policy somewhat closer to Sámi understandings of the proper way of knowing fish.[27]

Narrative contrast. More radically, as we have seen, overviews do not have to take the form of counting and statistics. If the judgement were made that it was appropriate to collect and collate knowledge in other forms, then it is possible to imagine creating an overtly hybrid knowledge-value space, for instance, around the juxtaposition of a range of contrasting narratives.[28] This would require the creation of novel institutional, material, and epistemological methods for judging ways of knowing and valuing in Deatnu. As a part of this, it would impose a burden of responsibility on all concerned – scientists, policymakers and local experts alike – to craft such a space. But to think in this way is to suggest that there are ways of managing salmon in centred, state-responsive, ways that do not depend on counting, and there is no reason to suppose that these could not be imagined if the political will were available.

Local knowing and local judgement. More radically still, the history that we have sketched also suggests that what happens with on Deatnu does not need to rest on centrally organised ways of knowing detached from the contexts in which they were generated. In principle, the organisation of fishing might instead be devolved to local ways of knowing, local judgements, and local policies or understandings about what is appropriate. This is a shift that would demand two contrary moves. One the one hand, it would need to draw on state power, because this is the only plausible way in which the current asymmetries between ways of knowing and the state and its agencies might be undone. On the other hand, it would imply loosening state control to create the space required for distributed modes of knowing and decision making. At the same time, it would, of course, again imply a weighty burden of responsibility for local experts. They would be confronted with the onerous task of working out the character of distributed ways of knowing and valuing required for respectful salmon fishing on Deatnu in the modern world and modern circumstances.

These possibilities do not exhaust the possible options, and there are other contexts in which local ways of knowing relate much more fruitfully to biological arguments about fish populations.[29] In addition, none are easy options either politically or conceptually. Thus, to a greater or lesser degree,

[27] Appropriately worked out, this would be consistent with Walter and Andersen's argument that reframed numbers may '"speak[..] back to the state" in a manner that both incorporates Indigenous knowledges and is ontologically translatable to state actors.' (Walter and Andersen 2013, 73).

[28] The importance of narrative and narrative collation has been explored in indigenous studies. See, for instance, Tuhiwai Smith (2012). And it is, to be sure, crucial in a range of other contemporary institutions including politics and the law.

[29] See, for instance, Brattland (2013a, b), and Strand (2019),

they throw down challenges to fish stock modelling as this is practised in Deatnu and the policy that derives from that modelling. They challenge the various branches and agencies of the state to think carefully about how their values and their realities are intertwined. But they also pose a major challenge to local ways of knowing: to how local experts might rework their commitments to the lively world of Deatnu in ways that fit the exigencies of the contemporary world. And, to be clear, the possibilities that we have rehearsed are indeed just possibilities, and there are many more. We hope, however, that the historical contingencies that we have described reveal that Deatnu fisheries science carry their own set of values, that these might be different, and that it would be appropriate to find ways of exploring alternatives.

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