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From certainty to doubt in fishery science

'Less fishing is wasteful, for the surplus of fish dies from natural causes without benefit to mankind.'

W. M. Chapman, 1948

Fishery science is usually perceived by its practitioners as being a critical and quantitative activity, deeply dependent on mathematical analysis; indeed, the introduction to a well-known text on fisheries science suggests that only those who are comfortable writing computer programs or playing with numbers should become involved in fisheries management.[1] This blunt statement demonstrates what went wrong with the science: it forgot that it is heavily dependent on two other disciplines – biology and ecology – in which numerical predictions are quite often unsatisfactory. Consequently, there is a fundamental contradiction between the potential capability of fishery science and its stated task of making routine and quantitative predictions concerning the effects of specified levels of fishing on a stock of fish.

Biology is notorious for its lack of predictive theory, and for its high content of inductive and a-priori generalisations that are based on simple observation of nature. As Murray noted, 'the fact that biology lacks . . . universal laws and predictive theory . . . poses a serious problem for both biologists and philosophers'.[2] Nevertheless, it is possible to deduce simple biological laws and to verify them by the prediction of ecological observables: I shall discuss one such example in Chapter 2. Such laws may be used to falsify theories that have been arrived at by inductive methods.

Ecologists (and I use the term in its original sense) have long recognised that their discipline, being a subset of biology, similarly lacked rigour and predictive ability. Critical reviews and debates about many of its central assumptions characterised the development of ecology during the second half of the twentieth century:[3] trophic levels, succession and stability, energy flow, co-evolution and density-dependent population regulation were all subjects of warm (not to say heated) discussion, although very little consensus was achieved.

Density-dependent population regulation is the central concept in the theory of fish population dynamics, stemming from the logistic function of Verhulst that describes the absolute growth rate of a population that is constrained by a limited resource: in this, the absolute growth rate is maximal at the inflexion between increasing and decreasing growth rates that occurs when population size is exactly half the potential maximum.[4]

The population growth of the entities represented by the logistic function is thus constrained by changing rates of reproduction and mortality, the causes of which are not specified, so that the rate of population increase is a simple function of biomass. In the real world, the entities suffer both parasites and predators, must accommodate to a food supply that changes with time, and their social behaviour may respond to progressive crowding. Thus, the trajectory of the unadorned logistic function resembles nothing in the natural world, and the consequent tension between theory and reality has been at the centre of the great debate concerning environmental population regulation (Andrewartha and Birch) and density-dependent growth (Nicholson) since the 1930s, which is still generating as much heat as light. One of my correspondents suggests that this debate is the nearest thing to scholasticism that he has seen in biology.

Entomologist Berryman remarked in 2002 that population regulation had been recently described as a 'bankrupt paradigm', 'a monumental obstacle to progress' and 'a mind-set, a dogma, a faith'.[5] Although there are semantic problems concerning regulation and density-dependence, he concludes that regulation is not universal, but is merely one of several population behaviours possible in complex ecological systems; a recent study of long-term records of size-at-age and biomass data for 16 populations of marine and freshwater fish appears to confirm his remarks. Significant density dependence was detected in nine populations, while in four others where the relationship was not statistically significant there were point estimates of growth that were consistent with an among-population effect. In three populations, no relationship could be observed, even though other studies had demonstrated density-dependent growth in these species.[6] Despite the difficulty of observing it in every case where it would be expected to occur, regulation remains critical to management theory, and density dependence continues to be quoted as the central tenet of fisheries science,[7] because 'density dependence gives populations the resilience required to sustain elevated mortality from fisheries' – as a recent student text has it.[8]

In fact, the great Andrewartha–Birch–Nicholson debate seems not to have concerned fisheries scientists as much as it did general ecologists and it did not displace the idea of maximal yields, based on the assumption of logistic population growth, that came to take a central place in management theory after the rediscovery of the logistic curve in the 1920s and its exploration by Hjort and Graham. The logistic curve was also the origin of M. B. Schaefer's logistic model of surplus production of tuna populations under non-equilibrium conditions, which was formulated in the mid 1940s while he was working in the US federal fisheries laboratory in Hawaii.[9]

Thus was the formal proposal for management for MSY hatched from Verhulst's logistic, and one might assume that its rapid passage into the heart of fishery science must have occurred because it had received positive peer reviews from the fishery science community. But, in reality, the rapid acceptance of the principle was almost entirely politically motivated, and occurred prior to the eventual publication of Schaefer's model. In writing its epitaph, Larkin evoked the level of enthusiasm and certainty in American fisheries science during what he calls that golden age for the model of maximum sustained yields, when it was the duty of fisheries science to ensure that the seas everywhere were harvested to this maximum.[10]

This singular state of affairs, in the years immediately following the end of the Pacific war, resulted from the fact that the US State Department was then working to enforce a policy of open seas and open skies, intended to minimise the ability of coastal states to restrict the freedom of American naval and fishing vessels to operate everywhere outside the narrow territorial seas. In the latter stages of the war, US fishermen had worked freely over much of the SW Pacific to help feed the armed forces, and the industry wanted that freedom to continue and, indeed, to expand to other oceans. Japanese fishing was forbidden in much of the NE Pacific by a clause in the peace treaty with that country, but Ecuador, Peru and Chile had other ideas, and the upcoming Santiago Declaration of a 200-mile national jurisdiction was already in the cross hairs of a worried State Department. This movement was at least in part a response to the 1945 Truman Doctrine and the unilateral declaration of sovereignty, including the establishment of fishing conservation areas, over the US contiguous continental shelf. There was some dissidence among US fishermen concerning this initiative for, although it was heartily endorsed by the West Coast salmon fleet, it was not at all appreciated by the tuna fleet, which wanted free access everywhere and feared exclusion from foreign 200-mile zones.

Schaefer was among the fishery scientists who advised the State Department in those years and, in 1948, his colleague and friend Wilbert Chapman (to whom I owe a personal debt of gratitude[11]) was appointed under-secretary of state for fisheries. Chapman considered that it was not fit, at this time of stress in the world and concern for food supplies, that the stocks of sea fish should be wasted through lack of fishing: this anthropocentric notion rapidly came to lie at the core of fishery science.[12] Global landings of marine fish and invertebrates were then only about 15 million tons annually and there were quite unreasonable expectations – greatly in excess of catches actually achieved at the end of the century – of the total potential of the oceans to supply proteins for human consumption.

Chapman had a reputation for moving fast, and he quickly crafted a US High Seas Policy which, as Mary Finley has noted,[13] formally specified MSY as the goal of US fishery management policies on the high seas, although the relevant work of Schaefer had until then been published only in an un-refereed house bulletin of the State Department. Within a few weeks of the publication of the US High Seas Policy, in January 1949, a bilateral treaty had been signed with Mexico and multilateral treaties to establish two fishery commissions, for Pacific tuna and for the North Atlantic, respectively, had also been signed. Each of these three treaties formally specified that management should be for MSY, and this fact profoundly influenced the future course of fishery science – even though these treaties had been signed some years prior to the publication of Schaefer's paper of 1954, so the US High Sea Policy was based on the acceptance of work untested by peer review: Finlay suggests that MSY was, at that time, no more than policy disguised as science.

The European nations, and perhaps especially the UK where Graham was a dominant figure of the times, were not at all enamoured of managing for MSY because their theory of fishing, and its application to management, was going in the direction of analytical models based on cohort analysis, and involving assumptions of age-structured estimates of recruitment, growth, fishing mortality and natural death; subsequently, MSY played little part in European negotiations, and management went in rather different directions on each side of the ocean. 'The European side' wrote Sidney Holt[14] recently 'sought to find ways ... to ensure continuity and stability in the face of growing fishery pressures ... the North American side sought optimisation of fishing, especially through setting the target of ... MSY'. Despite this divergence, by the time the UN Conference on the Law of the Sea had established a 200-mile Exclusive Economic Zone, each of the 39 regional fisheries organisations (RFMOs) then in existence had already accepted MSY as the basis for management.[15]

Perhaps it was in part the relative simplicity of Schaefer's model that ensured its rapid acceptance and propagation, at least in North America: it has a much lower information demand than the more sophisticated dynamic pool models being developed in the same years elsewhere in succession to Baranov's yield-per-recruit formulations. Later, of course, the simplistic MSY concept came to be modified by the inclusion of economic considerations, for a maximum economic yield, and by the use of a more cautious approach – as in New Zealand, where a policy of management for maximum constant yield, set at 2/3 of the computed MSY, was adopted during the 1980s.[16] This level of fishing mortality (corresponding to $F_{0.95}$ in a Yield/Recruitment function) was later recommended by Ray Beverton as a universal standard which, he suggested, might have prevented the 1968 crash of the Icelandic summer-spawning herring had it been in use at that time. Despite these modifications of the original simple idea, MSY survived remarkably well after such a strange beginning. Indeed, it is only in recent years and after the recognition that a global fishery management crisis was at hand that MSY has been fundamentally questioned in North American circles: Larkin's 1977 'Epitaph for the MSY' was not at the time taken sufficiently seriously. As shall be discussed in Chapter 11, the manner in which the new paradigm of ecosystem-based fishery management (EBFM) has been taken into the policy of fisheries agencies prior to extensive evaluation and peer review is startlingly reminiscent of the origins of MSY.

It is difficult, at this distance, to understand the confidence felt by fishery scientists in the mid-twentieth century that their numerical solutions to the problem of stock management were valid, and would function well, as soon as some uncertainties, such as the relationship between stock size and recruitment, were resolved by observation. This relationship was, of course, another inductive generalisation that many authors have struggled to define for individual stocks – because, intuitively, it was felt that such a relationship must exist and that it must be rather simple. Yet it has proved very intractable to this day, for reasons to be discussed later.

There was a general sense at this time that the various techniques then being developed – and used to quantify allowable catches – were satisfactory, even if diverse, leaving only the subsidiary tasks associated with managing each individual stock still to be investigated: obtaining data on size-specific escapement through the meshes of cod-ends, on stock-specific age and size at first maturity and on the current status of each fished stock. It was thought that these and other simple matters could safely be left to the biologists, even though this was a period when, as Ray Beverton noted in his posthumous essay,[17] 'biology became subservient to maths, in both staffing and philosophy' in the European fishery laboratories.

During this period, fishery scientists apparently did not listen to J. Z. Young's contemporary Reith lectures broadcast by the BBC: his studies of brain function, as related to speech and thought, led him to suggest how we interpret (and can discuss) the realities around us. He pointed out that the Descartian method of interpreting the human body in mechanistic terms – using the analogy of clockwork – could be extended to our scientific view of the world. Machines, he said, 'are the products of our brains and hands. We therefore understand them thoroughly and can speak conveniently about other things by comparing them with machines. The conception of living bodies as machines, having, as we say, structures and functions, is at the basis of the whole modern development of biology and medicine.' As Professor Young suggested, our brains may use such methods to create a model of the reality around us; unfortunately, it may not be easy to separate the virtual world, so created, from reality.

If we substitute mathematical models for machines, and populations of fish for J. Z. Young's 'other things' we have an explanation for why the development of mathematical models in fishery science led to the comfortable feeling that – because we 'understand them thoroughly' – we also understand the biological system that they represent. Many will disagree with this assessment, for models of ecosystem function and of the dynamics of single populations are now very widely available and widely used in ecological and fishery science; I shall return quite frequently in the following chapters to comment on the limitations of the understanding that we can obtain with such tools.

Although Beverton wrote of those days as being carefree, I prefer to remember the ambient certainty that fishery science was on the right track; there was an air of self-confidence in the Lowestoft laboratory that was palpable, for this was one of the places where age- and sex-structured analytical models of fish populations were being developed from such origins as Russell's formulation; one principal result of this work was published almost simultaneously with Schaefer's dynamic pool model and subsequently became the basis of management in Europe.[18] Here, the principal impetus that led to the development of such techniques was not strategic, as in the United States, but rather the 1946 European Overfishing Convention: in retrospect, it seems remarkable that such activities were undertaken only a year or so after the end of the Second World War, so that, as Beverton put it, 'by 1951, the pioneering age of stock assessment was in full swing, with international trials, research cruises and exercises'.

The certainty that correct solutions to the fishery management problem were to hand persisted, at least in the teaching of fishery science, for several decades; John Gulland's text of 1974 exuded great confidence in the stock management methods that had been brought into general use during the previous 15–20 years.[19] Gulland simply explained how a fishery scientist, to predict stock size in coming seasons, should use a series of mathematical expressions – more or less complex – that represented and predicted the evolution of the target fish population under fishing pressure.

However, by the turn of the century, less than 30 years later, the certainty that simple mathematical models could satisfy the requirements of stock management was starting to evaporate. Nevertheless, it is still evoked as a control rule in setting allowable catches, as in the long-term plan for northern hake management formulated by the EU Commission in 2009 which notes that 'This MSY approach will in the long-term ensure an increased number of fish in the seas, greater yields for fishermen and stable catch limits.'[20]

Although there are many analyses of the progression of fisheries science from confidence to uncertainty, the graphic account by Ray Beverton is especially revealing of what he called the 'gruesome story' of fishery science during the last decades of the twentieth century, dominated by the entirely unexpected stock crashes in the NE Atlantic after 25 years of what was thought to be rational management. He describes the difficult discussions in ICES and elsewhere concerning matters such as virtual population analysis, stock and recruitment relations, the introduction of reference points and the gradual evolution of understanding that year-to-year differences in ocean conditions had important effects on stock development. Already, the use of population models in stock management had progressively declined as their derivatives, biological or target reference points, were progressively incorporated into operational management.

I note in passing that Beverton's retrospective take on reference points was that they caused 'only confusion and gave managers an excuse to do nothing'. Whether managers did nothing because of confusion, or because they had no fishery control plan in effect to specify the actions to be taken under each set of circumstances and previously agreed with industry, is not clear. Nevertheless, resulting from international discussions in 1982 and 1995, target or limit reference points came to be incorporated in the UN Convention on the Law of the Sea and in the UN Agreement on Straddling and Highly Migratory Stocks; signatories of this agreement agreed that 'States shall take measures that when reference points are approached, they shall not be exceeded. In the event that such reference points are exceeded, States shall, without delay, take the additional management and conservation action determined under paragraph 3(B) to restore stock(s).' Further, included in this instrument was a precautionary approach to fishery management that must be 'acknowledged at every step of management from planning through implementation, enforcement and monitoring'.

The specific wording of the UN Agreement reflected the progression of fishery science towards management by reference points: it was very specific, stating that 'fishery management strategies shall ensure that the risk of exceeding reference points is very low' or, in plainer words, that the precautionary principle was to be adopted. As Richards and Maguire commented in 1998, a core component of the precautionary approach is that the absence of adequate scientific information shall not be used as a reason for postponing or failing to take conservation and management measures. Although the same authors described them as new directions for fisheries management science, these admonitions from the UN really brought nothing novel to the scene, because they simply reflected the already-developed philosophy and practice of the more influential fishery scientists who had been involved in the drafting of the instruments. The use of biological reference points had long been central to the work of ICES and other management organisations responsible for advice on the levels of total allowable catch (TAC) to be set annually for stocks under their jurisdiction: what the UN instruments achieved was perhaps to influence the more rapid adoption of modern techniques by nations and agencies that had not yet thought to do so.

Biological reference points are now widely used in providing advice to the management sector and are often formulated as the level of fishing mortality (F) that can safely be imposed on a stock, either as limit or targets; ideally, the chosen level should be sensitive to both management priorities and to observations of stock abundance. A value of F that has been very commonly used in ICES recommendations is the mortality (F_{max}) that would produce the maximum sustainable yield, MSY, as computed by the yield per recruit curve of Beverton and Holt.

However, because MSY must vary according to the level of natural mortality (M), computation of F_{max} is not always satisfactory and a simpler reference point ($F_{0.1}$) is often used. $F_{0.1}$ corresponds to the level of fishing mortality (or effort) at the point on the yield/effort curve where the slope is 1/10 of that at the origin. Other formulations have been used, such as a set of arbitrary reference points (F_{high}, F_{med} and F_{low}), which are based on a scatter diagram of spawning stock biomass against recruitment.

Progressively, it came to be understood that a direct method of initiating management action was required so that if targets were missed this would automatically define what management action should be taken. From this understanding came the suggestion by Mace in 1994 that simple threshold values for stock biomass and for fishing mortality might be established in every fishery, so that when critical values for these were transgressed, action to reduce F would automatically be required. This would, it was thought, be more effective than attempting to maintain an ideal relationship between, for instance, the value of F imposed by the fleet and stock biomass as observed by assessment surveys.

This was, as Caddy remarked, how limit reference points (LRP) were initially envisaged, after which they were rapidly incorporated into the discussions of those who were at that time planning what came to be called a precautionary approach to fishery management. For this to be functional, it was understood that it would be essential for agreement with each respective branch of industry to be reached, prior to fleet operations at sea, that a certain cause of action – usually a restriction on fishing activities – would automatically ensue should the value of any LRP be transgressed. Caddy likened this control plan to the action of a thermostat that controls the rate of burning in a domestic heating furnace.

Although I shall also be returning later to recount the multiple causes of the infamous collapse of the cod stocks of the NW Atlantic,[21] it will be useful here to mention just one aspect of this event. These fish had been decimated by heavy and largely unregulated fishing on the outer shelf and offshore banks by foreign trawlers prior to the unilateral declaration by Canada of a 200-mile extended fishery jurisdiction in 1978. Subsequently, using an approach that was based on an $F_{0.1}$

target reference point, associated with data from commercial catches and their own stock assessment surveys, Department of Fisheries and Oceans (DFO) scientists were confident that cod stocks were rebuilding in a sustained manner: contrary suggestions by inshore fishermen who noted that their stocks were in decline appear to have been ignored. The Canadian DFO biologists in the St. John's laboratory were said to have been stupefied when it became clear that the predicted strong growth of the northern cod stocks had not, in fact, occurred.[22]

Their confidence in simple constructs appears to have been nurtured by assumptions that the universe is mechanistic and governed by a few simple laws – that the marine ecosystem is robust and insensitive to small perturbations, and that its state varies around a natural dynamic equilibrium – although others had already pointed out that the application of $F_{0.1}$ to a situation in which recruitment had been reduced to a very low level was both inappropriate and dangerous. The use of reference points in setting allowable catches requires, in this case as in others, an assumption that the steady state of fish stocks is dominated by a very small number of knowable variables whose effects may be readily modelled. All of which is very far from reality, but criticism – both from formal enquiries and informal discussion – failed to deter the management trajectory in Ottawa. Confidence in their assessment data, and in their mechanism for setting TACs, was unshakeable, until at last it was clear to everybody involved that the stocks had passed the point of no return.

With the collapse of cod stocks on the Grand Banks, it came to be acknowledged, perhaps especially in North America, that the future in fishery science could in no way resemble the past: there were now too many examples of stocks managed with widely accepted techniques, but that nevertheless collapsed, to support the belief that the same techniques would serve on into the new century: people began to talk of the need for Kuhnian paradigm shifts in the science. The search for new ways of managing stocks developed – as I shall discuss in a later chapter – into something resembling a feeding frenzy.

But it is surprising, in retrospect, that it should have taken so long for somebody to point out that fishery management models – whether of surplus production (e.g. Schaefer), or yield per recruit (e.g. Beverton/Holt) or of spawner/recruitment (e.g. Ricker) – all put the cart before the horse, as Bob Francis so wisely commented.[23] By this, he meant that they were used to infer something about nature – rather than the reverse process: this is, of course, a rather common error in scientific methodology. He further suggested that population dynamics