DOES HISTORY SERVE CONSERVATION?

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It is sometimes said that we should never believe a scientific theory
until it is verified by experience. But…also…we should never
believe an observation until it is confirmed by a theory.
Sir Arthur Eddington, British cosmologist

I have come here from the 11th conference of the North Atlantic Fisheries History Association (NAFHA) in Bergen, Norway, the first I have attended. I was pleased to leave that the answer to my title question was 'yes'. But the historians there were looking at sequences of events, and it seemed that the main lesson was that there had been many failures from which we might learn what not to do in the future. But, in any case my theme today is not so much *events* as *ideas*.

In his majestic science-fiction saga, "Last and First men" (1930), Olaf Stapledon, English novelist and philosopher, wrote:

"Theories, theories, myriads upon myriads of them, streamed over me like windborne leaves, like the contents of some titanic paper-factory flung aloft by the storm, like dust-clouds in the hurricane advance of the mind. Gasping in this vast whirling aridity, I almost forgot that in every mote of it lay some few spores of the organic truth, most often parched and dead, but sometimes living, pregnant, significant."

Stapledon hypothesized the trajectory of human evolution through two billion years to the ultimate burnout of the solar system, though he did not look backwards to before 1930. Later writers, of course, offered us escape routes to planets in other star-systems, lengthening our life span, and even to other universes, *alla* Philip Pullman's trilogy "His Dark Materials".

I think I was sixteen years old when I first read this. I had just decided to become a biologist though I had little interest in natural history. More to the point, the branch of the British Museum in the part of northwest London where I grew up, which housed the museum's newspaper collection, had just been bombed. The Luftwaffe was trying to disable the old aerodrome nearby where new RAF fighter pilots were being trained, a field too small for modern aircraft and where, later, Metropolitan Police drivers in what were called the Flying Squads, being polished at Hendon Police College, at learned how to survive high speed skids.

We in Colindale awoke after the air raid to find our streets and gardens littered with tattered papers. That was my first introduction to the grey literature, although we did not

know it by that name. For some years my home-museum contained a few torn scraps of old newspapers along with a piece of true British anti-aircraft shrapnel from our loft, along with a piece of German incendiary bomb – that failed to explode.

Some of these grey particles, Stapledon's dust-clouds, could coalesce, the condensate being called "the published literature", to be reviewed, revised, retrieved, indexed and filed, and in the traditional scientific manner, metaphorically torn apart, demolished and proven wrong. Nothing - not even theories and ideas -is permanent, yet we strive for continuity within change. I suppose it is in the turbulent teens that worries about survival and mortality, evolution and stability are seeded, some eventually blossoming into notions such as conservation and sustainability. I believe the history of such notions is in the grey literature; the published forms tend to obscure it.

An Iranian-American, Amil Imani, recently wrote, in the on-line magazine **Global Politician:** "*"All things on earth are subject to a limited life span, be they bacteria, trees, mountains, humans or ideas - including religions"*. That last is perhaps a counterpoint to Stapledon's later book, "Star Maker" (1937), which introduced ideas remarkably like the modern *Intelligent Design*, and I suppose it is no coincidence that I found the Imani quotation in the **Newsletter** of the (British) **National Secular Society** – more grey literature! In "Star Maker" Stapledon undertakes – according to Wikipedia – "the immense task of describing the entire history of life in the universe. He tackles philosophical themes such as the essence of life, of birth, decay and death, and the relationship between creation and creator". I intend today to be considerably less ambitious.

Anyway, we see in Imani the idea of *life span* as something looking forward, towards an end, and that is a common interpretation. But a "span" has a beginning as well as an end; it is finite – think bridges. (When I Googled the word I was led, unexpectedly and with delight, to an organization with that name, listed as a Self Publishing and Independent Publishing Trade Association. Most appropriate!) I liked Amil Amani's attribution of mortality to ideas as well as things since I have, since I spoke at the ***IAMSLIC*** conference in Rome two years ago, immersed myself in the history of some of the **ideas** associated with conservation of natural resources, especially marine living resources, as distinct from the sequences of **events** in the real world such as those explored by Jared Diamond in his "Collapse" and by Brian Fagan in his "Floods, Famines and Emperors: El Niño and the fate of Civilizations". I supposed it would be worth asking if such exercises are useful as well as interesting to an eclectic few. And, since that immersion turned out to be largely in the grey literature it seemed appropriate to talk about it here, where tomorrow's theme is ***Capturing the Shadows***. But I'll bear in mind the overall theme of this conference – ***Changes on the Horizon*** – which conjures simultaneously the inter-locked concepts both of space and time. As with life span we usually, most of us, tend to sit watching the setting sun, not the dawn. But here we have to deal with ***cycles*** – I prefer the term ***good vibrations.*** I'll have a few words to say about that later.

The excursion on which I spent the longest time tracing myths was to the origins of the modern ideas of sustainability, sustainable use of resources and sustainable development, this last having been described fairly convincingly as a monstrous oxymoron. It has been reviewed critically elsewhere by others and I'll just refer this audience to William E. Rees's and Sharon Bader's contributions to *Gaining Ground* (see below) respectively : "Why conventional economic logic won't protect biodiversity" (Chapter 14, p207-26) and "The changing face of conservation: commodification, privatization and the free market" (Chapter 5, p 83-97). The ideas I want to talk about concern the first two mentioned, and within those the notions of ***density dependence***, ***depensation***, ***maximum*** and ***optimum sustainable yields (MSY, OSY)***, as well as the grey (including secret, confidential and private materials) and published sources concerning them. The relatively new idea of the now much touted – and, by some, feared - ***precautionary principle*** is already worthy of historical analysis, but it is probably too soon to consider whether such a study would serve conservation. A brave beginning was made in 2001 by a team assembled by the European Environment Agency ("Late lessons from early warnings: the precautionary principle 1896-2000". *Environmental Issue Report* No 22, 211pp), and the summary of it – "Twelve late lessons" – by the Editorial Team is a thoughtful progress report. It has a chapter on "Fisheries: taking stock", by Malcolm MacGarvin but the case studies therein are geographically limited and in my opinion do not include some of the most revealing situations and are especially thin on the European experience.

The idea that ***biodiversity***, at genetic, taxonic and ecosystem levels is a good thing is also relatively new and fashionable but perhaps not yet quite ripe for critical historical analysis. It may or may not be closely related to the even more fashionable idea of taking an ***ecosystem approach to management*** of the use of ocean space, natural resources or whatever. The 'may or may not' phrase is there because this much talked about approach is, notwithstanding an enormous grey literature referring to it, has about as many quasi-definitions as there are buns in a baker's dozen. This has been well brought out in a a recent article by Joji Morishita (Japan's Alternate Commissioner to the International Whaling Commission), in *Marine Policy* (2007, in press), entitled "What is the ecosystem approach for fisheries management?" What is clear is that apart from the basic problem of definition there is remarkably little depth to the science about it. I propose to leave it alone until the fog clears.

So, let me begin with "The Notion of Sustainability", the title of my chapter in a book published in 2006 by the University of Limerick, Ireland, and the International Fund for Animal Welfare (IFAW) entitled "Gaining Ground: In Pursuit of Ecological Sustainability". This book was edited by David M. Lavigne who organized the Forum in Limerick of which this 425-page book is the Proceedings. I can recommend reading several chapters – other than my own, of course – especially the first and the last by Lavigne and some of his collaborators and the aforementioned ones by Rees and Bader.

To understand the theory of sustainable use of renewable living resources (the theory is a bit different for non-living resources) we go back to the nineteenth century, when the Rev. Thomas Malthus postulated that human and other animal populations tended to

increase *geometrically* – 2,4,8,16,32 etc. – or, as mathematicians expressed it, *exponentially*. Something had to give to prevent the planet from filling up and Malthus noted that occasional catastrophes knocked off the "surplus" from time to time but, more generally, that the contrast between a tendency for exponential growth and the presumed reality of only linear increase in access to resources (that is proportional to population number) led to periodic collapses and new beginnings, and hence to cycles. These were associated with the class-structure of society – many poor, few rich – so the Reverend was more or less in line with Karl Marx and Frederick Engels, both of whom also had scientific/mathematical credentials. However the mid-nineteenth century, what with the turbulence and revolutions of 1848 and thereabouts, was a time when the European Establishments wanted – and needed – stability, so a series of publications in 1838, 1845 and 1847 by a Belgian mathematician, P.-F. Verhulst, was welcome. Verhulst postulated that the geometric rate of population increase would itself be reduced as a population got larger. To make the algebra simple he further postulated that the relative rate of increase (that is the absolute rate divided by the size of the population at that time) would be a linear function of population size. This leads to an S-shaped curve of population size plotted against time, reaching an equilibrium final size eventually, and with the fastest increase rate occurring when the population has reached about 50% of its final size. The phenomenon of declining rate of increase as population size increases has come to be called ***compensation*** or (negative) ***density dependence***.

Successive generations of scientists have sought, with limited success, to identify the assumed phenomenon as the result of a recognizable biological *process* or complex of processes, and some have advised the use of this ***logistic*** S-curve for the management of major sea fisheries, most notably the tuna fisheries of the tropical eastern Pacific. Even economists have clutched it like a straw for insertion in econometric models, thinking – mistakenly - that it is a scientifically established theory in the biological sciences. The S-curve or ogive, especially a symmetrical one, has had a mystical appeal to some: Michael Graham – one of the "greats" of fisheries science - was particularly attracted to it and elaborated that in his classic book, "The Fish Gate" (1943, Faber and Faber, London). But direct evidence of density dependence is weak, and evidence that it is linear with population size is non-existent (despite the misleading entry in Wikipedia on the subject). Verhulst's simplistic postulate has been conceptually useful but can be disastrous in unthinking application. A later variation of the idea has been that the dependence is not linear but more like a parabola or even a polynomial function of a higher order. Such modifications nevertheless retain two important features of the simple logistic. One is that the supposed equilibrium upper limit to population size (sometimes called "carrying capacity", or in fishing and whaling circles "virgin stock") is only reached after infinite time, and that applies to any restoration process after a resource has been exploited but subsequently protected, as well as to the attainment of sustained exploitation at any chosen level of population and yield. As playwright Tom Stoppard once wrote: "*Eternity's a terrible thought. I mean, where's it all going to end?"* For more about eternity I recommend John D. Barrow's "The Infinite Book" (Vintage, 2005, 327pp), and for a view from the other end of the scale "The Book of Nothing", by the same author (Vintage, 2001, 380p). And while we are on the subject of good books I only recently

discovered the writings of Nassim Nicholas Taleb, especially "Fooled by Randomness: The Hidden Role of Chance in Life and in the Markets" (Penguin, 2004, 316p) and "The Black Swan: The Impact of the Highly Improbable" (Penguin, 2007, 366p). Taleb, who is a market trader, does not like the Normal/Gaussian form of a bell-shaped curve, considering it to have little empirical justification except in trivial pursuits like measuring the heights of people and calculating means and the spread of differences. I'm inclined to agree with him and was rather pleased to find myself not entirely alone on a limb of heresy or, worse, of apostacy. Taleb has two gripes. One, that the Normal curve is symmetrical while real life is asymmetrical. The other, that the tails of the Normal curve seriously under-estimate the likelihood of extreme events. Fair enough. And the paucity of empirical substantiation of a neat theory is exactly what worries me about the famous logistic curve of population growth, at the heart of the shaky discipline of population dynamics, *my* discipline.

An important recent shift in ideas about managing the use of renewable resources is the realization that *eternity* has to be abolished, and replaced by the notion of sustainability as referring to a finite, and not necessarily very long, period of time. In practical terms this was first implemented by scientists working in the International Whaling Commission (IWC) who devised the Commission's Revised Management Procedure (RMP) in the 1990s, but it's application is spreading to fisheries management advice more generally.

The other feature of the logistic and its relatives is that the highest rate of population increase occurs when the population is tiny. The opposite phenomenon, in which the rate of increase of small populations *increases* as population *increases* is called ***depensation***. It's existence results in, among other things, the possibility that a population which has been over-exploited continues to move towards extinction even if exploitation ceases. If that extreme and irreversible event does not occur, because the strength of depensation is less than "critical", it will result in a slower recovery than expected, speeding up only when the population has grown considerably bigger. This is characteristic of the dynamics of many "initial" animal populations: a slow start, then take-off. So in that little detail of the bend in a curve lies the devil of nasty behaviour. Fisheries managers had better watch their backs.

Fisheries research and management have a history of it being assumed that there is no depensation unless data demonstrate that it exists, and classical statistical analysis has difficulty in producing such evidence. That approach is evidently a misuse of Occam's razor. William of Occam was a 14th century Franciscan friar who enunciated "the principle that the explanation of any phenomenon should involve as few assumptions as possible, eliminating those that make no difference in the observable predictions of the explanatory hypothesis or theory" (Thanks to Wikipedia, which tells us that this is a heuristic maxim that advises economy, parsimony or simplicity in scientific theories.). The precautionary principle, on the other hand, would advise us that the safer option is to assume that depensation **does** exist and to formulate our statistical tests accordingly, taking care correctly to balance as well as seek to minimize both type I errors (false

positives) and type II errors (false negatives). In any case, the idea that non-existence of depensation is the simpler assumption is entirely illusory; no more parameters are needed in a population model to include than needed to exclude it.

Sorry, yet another diversion. While making sure that I got Bill Occam's date and pronouncement right I found, to my delight, a web-site dedicated to Occam's Toothbrush. What's that? "Making easy things hopelessly complex". With the by-line: " If you're going to multiply propositions needlessly, you're going to need good teeth". Hurrah for the World Wide Web!

I think this notion of depensation is a clear case of where the study of the history of an idea and of its consequences serves conservation. So now let's move to my second example: the ideas of maximum and optimum sustainable yields as objectives in managing the desired sustainable use of fisheries and wildlife resources. The 'family' of logistic population models, and others, predict that the highest continuing (i.e."sustainable") yield can be obtained by holding the population at some intermediate level between the "carrying capacity" and the low level at which depensation might come into play. There are many economic and other practical reasons why targeting on the maximum might not be a good idea, including computer simulations showing that management procedures intended to strike that target do not work well. The idea is biologically dangerous, too, because it puts all the emphasis on the size of the population and ignores its structure – by age, sex and so on. Naïve efforts have been made in recent years to resolve the practical problems by defining other "reference points" than the maximum on a curve of sustainable yield against population size, but all of these depend – as does MSY itself - on knowing the shape and scale of the curve and where the population now is on it, and also having the ability to aim at a chosen reference point efficiently.

Despite all these serious problems the MSY target is still present in many fisheries management plans, is at the heart of the UN Convention on the Law of the Sea, and was endorsed by the UN environment conferences in Rio de Janeiro and Johannesburg. And the European Union is now considering replacing its so far disastrous Common Fisheries Policy (CFP) by one incorporating the MSY target. So how has this situation come about, that a management objective which scientists know to be an unreachable mirage (and which would be undesirable even if it was real) has become virtually cast in stone in international treaties? The answer is in politics – surprised?

The period 1955-1958 was a very active one with respect to development of the law of the sea, partly as a consequence of the Truman administration claiming, a decade before, administrative and jurisdiction rights over the resources of the seas adjacent to the US coast, up to 200 nautical miles, an act which was swiftly followed by the more general territorial 200-mile claims of a number of Latin American states. The US delegations came to the various UN meetings determined that MSY should be the general objective for international fisheries management. Most of the Europeans – then the other major powers in fishing apart from Japan and the USSR – were more inclined to looser,

negotiable objectives and were also more ready than the US to accept restrictions – in the cause of conservation - on fishing effort rather than merely on catches. The US argued that a universal, numerically determined, physical catch limit objective was essential and that no international agreement was feasible on economically or socially defined reference points. It was probably correct in this, but it was not the real reason for the US insistence on the MSY target. To understand why we have to look back a decade to the post-war negotiation of a peace treaty between Japan and the USA.

I was a staff member of the Food and Agriculture Organisation of the UN (FAO) from 1953 but had also been involved – from the side of the UK Government - in fisheries treaty negotiations with the US, Canada and other European states, in 1949. So I had some idea of what might be going on. But I have also been fortunate in the last few years in meeting Dr Mary Carmel Finley who has just completed her doctoral thesis on the history of Japan-US fishery relations, of which she has kindly given me a copy. It is entitled "The Tragedy of Enclosure: Fish, Fisheries Science and Foreign Policy (University of California, San Diego, 2007, 549pp.) Dr Finley reached me by coming across my name in State Department files recently de-classified under the 50-year rule, and getting in touch first with FAO and then with the International Whaling Commission. So what with as yet unpublished theses and no-longer-secret cables from Rome to Washington DC we are looking at a grey literature *par excellence*.

The American occupying power was sympathetic to the Japanese wish to re-engage in global high seas fishing and to that end, among other things, encouraged the resumption of Antarctic whaling. But at that time by far the most valuable fisheries on the North American seaboard were for Pacific salmons, traditionally taken by US and Canadian fishermen in rivers and coastal waters. Japan wished to catch those salmon on the high seas; North Americans contested this – they said they had originally "developed" the fishery and were spending money and foregoing other benefits to keep the rivers good for spawning salmon. So their problem was to halt the Japanese. To that end the very influential fishery advisers in the State Department – all of them biologists, incidentally - invented the "principle" of ***abstention*** The idea was that if a coastal state was fully utilizing high seas resources adjacent to its shores and managing that utilization on a sound scientific basis, then other states should refrain from entering the fishery. Pursuit of that principle involved defining "fully utilizing, etc." and – Hey presto! – the MSY target-notion provided the definition. Japan's acceptance of the abstention principle in the International North Pacific Fisheries Convention (INPFC) was one requirement for completion of the peace treaty; another was that Japan – which had always refused to participate in various conservation management measures adopted by the dominant Europeans - would join the IWC and abide by its rules.

One consequence of the adoption of the abstention principle was a gross distortion of scientific research. Under the INPFC a line, roughly north-south, had to be drawn through the North Pacific Ocean to mark the separation of the salmon stocks that spawned in Asia from those spawning in North America and therefore subject to Japanese abstention. For years great efforts were made to define the line, though it became pretty clear quite early

on that there was no such fixed line. I have since then come across legal and administrative rules that have similarly distorted scientific efforts; the most recent, that I know best, is the corruption of research on whale stocks by Japanese efforts to justify whaling on commercial scale and with commercial intent by the issue of Special Permits for scientific purposes, and especially now to provide "evidence" that the fact that some whales eat some fish of commercial interest is a threat to fisheries and to the Food Security of humans.

When I was involved in the negotiations in Washington DC for the creation of an International Convention for the Northwest Atlantic Fisheries (ICNAF, now transmuted to NAFO), being faced with pressure for insertion of the MSY target in the proposed new treaty, we in the UK delegation asked – among ourselves – why? One suspicion was that the US, and possibly also Canada, wanted, one day, to be able to exclude the Scots and other Europeans from the then-great fisheries off their Atlantic coasts. To counter this we began to talk about "historic rights", something the Japanese could not plausibly claim in the northeast Pacific. So the European northwest Atlantic fisheries were "saved" – until they were practically destroyed by over-fishing (despite ICNAF) and eventually by the declaration of 200-mile Exclusive Economic Zones (EEZs). But "historic rights" subsequently found their way into the negotiations that resulted in the UNCLOS.

Soon after the INPFC "saved" the salmon – and also the valuable halibut – fisheries for the North Americans, the predominant US economic interest switched to Pacific tunas – which were also, of course, of great interest to the Japanese. Now US political priorities were reversed – US operators wanted to fish freely off the coasts of Latin American and other states, not only for the tunas themselves but also for small bait fishes, which mostly lived close to shore. This development naturally divided, in terms of policy, the US west coast fishing industry, but the tuna interests prevailed and the US government spent many years opposing – ultimately unsuccessfully - the "enclosures" being increasingly claimed by Latin Americans and others. Further big changes came when the tuna fishermen switched from baited lines to purse-seines and no longer needed to venture close inshore off Central America and northwestern South America.

Returning to the matter of the revision of EU fishery policy, when I was invited to suggest specific changes I faced a dilemma. Clearly the European Commission wanted to ensure depleted stocks were permitted to increase to more biologically productive levels, as a major step towards renewal of fisheries in the European Community's EEZ, and I believed good management for long-term sustainability – and, more urgently, providing for recovery of severely depleted fish stocks – would call for abandoning the unreachable and inappropriate MSY target. However, a confrontation over that, in the face of the UNCLOS and the Rio and Johannesburg words, would at this stage probably not be successful, and I advised instead an objective based on the work of the IWC's Scientific Committee. This could, I thought, be regarded essentially as involving ***redefining*** MSY, not as the highest possible sustainable annual catch but as a maximum **cumulative catch (*i.e*** the sum of all catches**)** over a defined management period, consonant with meeting other objectives, principally a minimal probability that a stock will be unintentionally

reduced below a defined threshold in any year during the period. A management algorithm to achieve this can be found by computer simulation and if necessary in such a way that it is effective and robust in being - for practical purposes - independent of any particular population model. This proposal and various responses to it are all in the grey literature of the European Commission, the Green Party in the European Parliament, the European Policy Office of WWF and in Parliamentary documents. Whether this little egg will hatch – and, if so, produce a monster - depends on the complex relations between the Commission (generally favouring the long-term view, and hence conservation); the Council of Ministers (which tends to favour short-term commercial views that please most of the present generation of fishermen); Parliament (rather unpredictable) and the scientific establishment (which tends to be schizophrenic, with some not wanting to admit the weakness of past methods and others hoping to build their careers on new approaches).

Chosing the duration of the defined management period is not a trivial matter. The IWC scientists chose 100 years. This was, at the time, the longest time for which available computers could practically carry out trial simulations. It happens to be also of the same order of magnitude as the life spans of whales and humans, but is surely longer than a management instrument and system are likely to endure. For most fisheries a period of 20 to, say, 50 years might be more appropriate. But Norwegian government scientists associated with the whaling industry have recently suggested a longer period – two, or even three, centuries. Present computer power can cope with simulating that, but it is of course quite unrealistic to expect management instruments/arrangements/procedures to persist for so long; we are not living in Byzantium and running the Eastern Roman Empire for a millennium! But there is method in Norwegian apparent madness. With a longer period, current and near future whale catches could be much higher – which is naturally what the whalers want – but after a couple of centuries the then depleted stocks could be subject to a much lower exploitation rate, and so be expected to recover to high levels by the end of the third century – and who is there to care now what happens to whales then?

Let me conclude by asserting that understanding of the birth and evolution of key ideas, notions, concepts, unproven theories, untested hypotheses and glorious principles can help us invent better ones and thus serve conservation if that is our purpose. But, possibly more important: that history can help us distinguish between, and remember, what is a hypothesis, what is an observed set of facts, what is truly a principle, what is a mere assumption, and so on. In particular to be aware of the process by which what is convenient assumption (like easing the application of the calculus, in Verhulst's case) becomes believed by nearly everyone, after a generation or so, to be reliable and incontrovertible fact on which can be based the management of marine ecosystems.

I have learned that one should not under-estimate the passage of time since an idea is first promulgated and when it may be lost, then reinvented. In fisheries research a classic example of this was the re-discovery after World War II of a mathematical model by a Russian scientist, F. I. Baranov, published in Russian in 1918, which described a

relationship between sustainable yield and fishing effort. I have only very recently come across an even older reference, It is by a Professor G. Chrystal who, on 30 November 1901 wrote, in a letter to MR Noel Paton an eequation for the exponential decline of a fish stock, without recruitment, under conditions of steadily increasing efficiency of fishing operations. (Remarkably, if efficiency increases linearly with time the trajectory of the stock is the right-hand side of a normal curve). Chrystal's letter was published in 1902 as a Appendix III to the "Report of the Committee (of the British House of Commons) Appointed to Inquire and Report as to the Best Means by which the State or Local Authorities can Assist Scientific Research as Applied to Problems Affecting the Fisheries of Great Britain and Ireland, together with Minutes of Evidence.", pp56–57; 136 (HMSO, London). I liked Prof. Chrystal's closing paragraph: "There are many questions to be solved, but they do not seem to be beyond the reach of statistics combined with patient and intelligent observation controlled by well-directed special experiments. Hasty conclusions and premature application of formulae are to be earnestly deprecated. All that mathematics can do at this stage is merely to assist the imagination." I don't know whether such Reports count as grey literature but they are pretty obscure as to their location; this one was found for me in the excellently catalogued library of the CEFAS Fisheries Laboratory in Lowestoft, Suffolk, England, and found for me by Andrew Payne, Editor of the ICES Journal of Marine Science.

So, as to the theme of this conference as a whole, my experience has been that such historical studies can only be pursued through access to the grey literature of all kinds. I have been making such studies on the whaling issue – particularly concerning Japan's policy – which would have been practically impossible if the IWC had not, until recently, kept verbatim records of its plenary sessions and – after some early resistance – been forced to put them in the public domain. Much of the crucial scientific work on which conservation actions are based remains in the grey documents of committees, not all being published eventually, and some never. Theses are immensely important because they usually contain detail that gets omitted from later publications. In looking at whaling history the log-books of the whalers are among the best sources of hard information; they are steadily being scrutinised, but finding funds for such laborious pursuits is difficult.

And state archives are, as Dr Finley found, treasure houses of revelations. But with those one must take care and be more than usually skeptical. She came across cables from Rome to Washington, during the law of the sea meetings in the 1950s, saying that I had recruited a Dutch biologist – a good friend and colleague, now deceased – to go on a prolonged mission to Panama, ostensibly to help the local shrimping industry but actually to spy upon, and possibly interfere with, the burgeoning US tuna-fishing operations in the region. Bizarrely untrue! I suspect this happened because I had been spotted, during the UN technical meeting in Rome, being unusually friendly with the Soviet scientists who were experiencing their first excursion outside the USSR; I was helping edit their documents, written in not-very-good English!

Now as promised, and finally, I return to the Rev. Thomas Malthus and exponential growth. His argument began, as did countless subsequent arguments, with an assumption

that population growth is essentially geometric, and his successors made further assumptions about what factors would modify the rate of growth, and how. However, a Danish mathematical geneticist living in Greenland, Dr Lars Witting, has demonstrated that the simple exponential assumption is incompatible with evolutionary processes as now understood. His idea is conceptually simple but mathematically difficult; I won't bother you with the latter ("*A general Theory of Evolution by Means of Selection b Density Dependent Competitive Interactions*".Arhus, Denmark, 1997). But as to concept, suppose each individual in an initial population has a tendency to reproduce periodically or even only once, and also a probability of dying one way or another, that the tendencies vary from one individual to another, and that the individual's tendencies have some genetic element, that is they are at least partially genetically determined. Witting has shown that the population must evolve in such a manner that the population increase rate will not be exponential in form, but what he calls *hyper-exponential*. This has immense implications for population dynamics. In particular it introduces long-term cycles in population size with different properties from those with which we have been familiar, arising from interactions between populations, such as predators and prey. One application of Witting's theory explains why, for instance, eastern Pacific grey whales, which were nearly exterminated by American commercial whaling in the nineteenth century, have not only recovered under decades of protection, but are now far more abundant than they ever were, despite the elimination of a large part of their breeding habitat along the coasts of California and Baja California, and possibly also by some destruction of their food supply by bottom trawling. This application also tells us that these whales are reaching the peak of a cycle, are about to decline naturally whether or not whaling is resumed, and hence - notwithstanding their abundance -, now have zero or negative sustainable yield. This is in stark contrast with assessments made with the orthodox population dynamic models which say that catches of several hundred animals annually would be sustainable, i.e. would not cause the population to decline.

However, amother paper has very recently been published, by three other geneticists, on the history of the grey whale; they have really put a cat among the pigeons They claim that grey whales were enormously more abundant before nineteenth century commercial whaling than they are now. Whom to believe? It's not grey literature; it's in the august *Proceedings of the US National Academy of Science*. How is such a discrepancy to be resolved? Not, I think, in the Scientific Committee of the IWC. (The reference is: S. E. Alter, E. Rynes and S. R. Palumbi "DNA evidence for historic population size and past ecosystem impacts of gray whales" *PNAS* **104**(38): 15162-7, 2007).

To summarise, I have given a few examples where critical examination of the history of certain ideas – exponential growth, density dependent limitation of growth, sustainability, and so on – reveals that what begins as a hypothesis or an assumption can turn, over time, in peoples' minds, into a "theory" or an empirically validated fact, or even – almost – into an ideology. My thesis is that these are not isolated cases and that such study *can* serve conservation.

I close by acknowledging the value of conservations over the years with many friends and colleagues, some alive and some deceased, among them Sir Peter Scott, Dr David Lavigne, Prof. Peter Jewell, Dr Philip Thomas, Ms Melanie Salmon, Dr Mario Ruivo, Ms Leslie Busby, Dr Mary Carmel Finley, Dr Geoffrey Kesteven, Dr Vassili Papastavrou, Mr. David McTaggart, Dr Justin Cooke, Dr William de la Mare, Dr Lyall Watson, Lord May of Oxford, Dr Philip Clapham, Mr. Maxwell Bruce, Dr Kees Lankester, Mme Elisabeth Mann Borgese and Dr Salvino Busuttil. None of them are in the least to blame for my errors and misunderstandings.