Whales Competing?
An analysis of the claim that some whales eat so much that they threaten fisheries and the survival of other whales

Sidney J. Holt

The Truth of a Proposition has Nothing to do
with its Credibility. And *Vice Versa*

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> *The Notebooks of Lazarus Long*. Robert A. Heinlein, 1973

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ABSTRACT

A critical analysis is presented of documents authored by S. Ohsumi and T. Tamura which claim that cetaceans (whales and dolphins) consume, worldwide, many times more marine living resources than are landed by fisheries. That claim has been inflated in the propaganda distributed by Ohsumi's and Tamura's employers – the Institute for Cetacean Research (ICR) in Tokyo – to an assertion that *whales* eat many times more *fish* than humans catch. That propaganda is directed to the ends of terminating the standing moratorium on all commercial whaling; abolishing protected areas ("Sanctuaries") for whales created by the International Whaling Commission (IWC); overturning other conservation measures taken by the IWC (and by many countries within waters under their national jurisdictions) over the past three decades, including integration of the Precautionary Principle in conservation and management measures; finding a justification for the long-term conduct of commercial whaling on an increasing scale through a loophole in the IWC's founding Convention which allows Member nations to engage in unlimited whaling by the issuance of Special Permits for scientific purposes; recruiting new Member nations to the IWC by creating an unreasonable fear that the recovery of depleted whale populations will endanger their fishing and related industries; and, ultimately, opening the way again to *unsustainable* whaling, under the guise of maintaining global food security.

The analysis demonstrates that the two documents and the associated propaganda material are basic elements in a confidence trick played, for purposes of fisheries policy, on the public, including representatives of governments and inter-governmental organisations (including FAO's Committee on Fisheries – COFI - and the Conferences of Parties to the Convention on International Trade in Endangered Species – CITES); people occupied in fishing industries and with managing them; journalists and other media workers; and even some scientists and technicians, not excluding some of those directly engaged in research directed to better management of the human uses of marine living resources.

The analysis reveals how very limited data have been used selectively, and treated by flawed and scientifically unacceptable methods; how guesses about various quantities related to the metabolism of marine mammals that are embedded in old documents have been presented as if they were established as observed facts; and how recent relevant research results have been ignored where they tended to contradict the assumptions made and not support the policy intent.

The two authors have misinterpreted the fisheries information compiled by the Food and Agriculture Organisation of the UN (FAO) – especially its regional breakdown - such that the comparisons made between those data and cetacean consumption are false and worthless as well as being irrelevant to the matter of fish-fisheries-whale interactions.

Four Appendices present alternative ways of looking at the perceived "problem" of the interactions between whales and humnans, and show how humans are in fact extremely wasteful and destructive in their current ways of using living marine resources and are deriving nutritional benefits from them that are much more limited than they could be.

EXECUTIVE SUMMARY

Through the past decade many critics have given their attention to ethical, legal and political aspects of the extensive conduct of commercial whaling on a substantial scale through unlimited and unilateral issuance of Special Permits for the purpose of scientific research. Attention has also been given to the general opinion of most members of the IWC Scientific Committee that the research conducted in this way contributes little if at all to improved management of any whaling that might in future be authorised internationally. Less attention has been given, however, to what happens not so much at sea as inside the Tokyo Institute for Cetacean Research (ICR) which is at the core of this activity. This analysis may be regarded as a step towards correcting that imbalance.

Two widely distributed and repeatedly circulated documents written by scientists S. Ohsumi and T. Tamura in 1999-2000, and a third by Tamura[2] have been used extensively to promote the idea that cetaceans (whales and dolphins) consume worldwide, and in each of the major ocean regions, far more of living marine resources than are landed by fisheries, and that this constitutes a threat to Global Food Security in general and to the future of sea fishing in particular.

Such promotion is a major function of the ICR, created by the Japanese whaling industry and strongly supported by the Government of Japan, especially its Fisheries Agency. The ICR has full responsibility in planning and operating the large-scale, long-running commercial whaling operations (including the marketing of their products) in the Southern Ocean Whale Sanctuary and the North Pacific, contrary to international conservation and management decisions, by the unilateral issuance of Special Permits for scientific research. The purpose of the three documents is to justify the ICR's whaling activities, to provide material for use in efforts to reverse the management amnd conservation measures enacted by the International Whaling Commission (IWC) over the past three decades, and to encourage accession to IWC membership by governments

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[2] "Estimation of total food consumption by cetaceans in the world's oceans", unpublished document, 1999; "Regional Assessments of prey consumption by cetaceans in the world", *IWC Doc. SC/52/E6*, 2000, included in an undated compilation by the Fisheries Agency of the Government of Japan, entitled "Increasing Competition Between Fisheries and Whales: Japan's Whale Research in the Western North Pacific (JARPN II)" that was distributed during the meeting of FAO's Committee on Fisheries (COFI), Rome, March 2005. Also "Regional assessment of prey consumption and competition by marine cetaceans in the world" by T. Tamura, 2003 as Chapter 9 of a book edited by M. Sinclair and G. Valdimarsson "Responsible Fisheries in Marine Ecosystems" (FAO and CABI Publishing, Wallingford, UK, p 143-70). That book originated as the Proceedings of an international conference on the title subject held in Reykjavik, 1-4 October 2001, sponsored by FAO and the Government of Iceland, with co-sponsorship by that of Norway.
Although this analysis focuses on documents produced and distributed by the ICR, erlated misleading and erroneous papers on the same subject have emerged elsewhere, particularly from the Institute for Marine Research, in Iceland, under the authorships of Drs J. Sigurjónsson and G. A.Vikingsson. Reference is made to those in this critique where relevant.

of developing countries that have been persuaded that the recovery of depleted whale populations threatens sea fisheries industries.

Scientific consideration of the possible impacts of whales on fisheries, and *vice versa*, is the subject of a considerable volume of publications concerning the complex dynamics of marine ecosystems. The three documents reviewed here make no mention of them, and so they are not considered in depth in this critique, but references are given – with indications of their meaning and relevance for those who wish to follow-up this important but difficult subject.

The critique begins with examination of the differences between the first two documents, circulated one year apart. These differences mainly involve changes in the assumed average sizes and numbers of each species of whale. In most cases those changes, made with little if any sound scientific justification, generally are such as to inflate results – the quantities consumed by the whales – while employing the same methods of calculation. One example is the Tamura/Ohsumi decision to assume that the average body sizes of the ordinary blue whale and the pygmy blue whale – a different and by definition distinctly smaller species - are both the same - 103 tonnes, a huge jump from the more plausible 83 and 69 tonnes respectively used in the first, 1999, document. A further example, is that in the 2000 document results are tabulated by three Regions of the World Ocean: the Southern hemisphere plus the Indian Ocean, the North Atlantic and the North Pacific. These results are then compared with international statistical compilations of landings of marine fishes and shellfish by the "capture" industries, also by three regions. The three fishing regions, however, do not coincide with, or have a meaningful relation to, the "whale" regions, nor do any of the whale consumption figures refer to the entirety of the Region to which they are assigned. Furthermore, the classification of the constituent items of the cetaceans' diets and of the catches by humans, for comparative purposes, is invalid; thus "crustaceans" in whales' diets means tiny planktonic organisms such as krill (euphausids) and copepods, while in the fisheries data it means crabs, lobsters, shrimps and prawns, all of which are much larger and in other respects very different from those eaten by whales.

Some other important numbers are those that were *not* changed between 1999 and 2000. Of these the most significant is the retention of an old and high number for the combined sizes of the minke whale populations that feed in the Southern Ocean (said to be about 700,000 whales) that has recently been put aside by the IWC's Scientific Committee, declaring that it no longer had an acceptable estimate, but that the true number, from the more recent survey data, could be very much lower. At the same time Tamura/Ohsumi ignore the fact that associated with the old numbers are very wide statistical confidence limits.

In the Tamura/Ohsumi documents three methods are employed to estimate food consumption by each species of whale or dolphin in each Region. All rely on figures for the *average* body weight of each species and for the population numbers. The latter are mostly so uncertain with respect to the smaller toothed whales and dolphins that this critique sets them aside. Another reason for that decision is that the management and conservation regulations of the IWC apply only to so-called Great Whales - the sperm whale and all the "whalebone" (baleen) whales – plus, marginally, the much smaller killer whales (Orca) and the bottlenose whales of the North Atlantic. Furthermore, Tamura/Ohsumi do not discriminate in their calculations between the numbers of adult whales and population numbers including juveniles, nor take into consideration the vastly different dietary and metabolic needs of animals of different ages and sizes in a species population.

The first method (Method 1) used by Tamura/Ohsumi uses data on the daily consumption of dead fish by 28 captive small cetaceans, ranging in body size - mostly guessed – from 80 to 1300 kg, averaging 385 kg. These data are then extrapolated, using a power function, up over a more than 200-fold range to provide daily per capita consumption figures for the large whales. Such extrapolations are highly questionable – in fact virtually unheard of in serious scientific work – even when the original data are sound and the function used for the extrapolation is well-known and precise. In this case neither of those conditions is met. This critique re-examines the functions used by Tamura/Ohsumi revealing that they have been taken uncritically from other scientific publications and used in a way that was not intended nor could have been supported by the original authors. It emerges that such misuse and misquotation of published authors continues throughout the three documents. Additionally, in this case, the extrapolation is unjustified because it is made to classes of cetaceans with entirely different diets from the captive dolphins and small whales, the constitutents of which have different energy densities (calorific value per kg), and that are mostly comprised of zooplankton (tiny crustaceans) with, in some cases, small fishes, mostly juveniles. Even the extension to the (mainly) squid-eating sperm whale is unjustified since the species preyed upon by the whales are mostly different from the target species of fisheries.

Method 3 is also based on a supposed relation between food consumption and body weight, but in this case the supposition is that daily consumption is 3.5% of body weight, regardless of the actual size of the animal. This comes from an obscure paper published in the USSR 45 years ago and has no credibility in modern biology and physiology. It is now universally acknowledged that food consumption and rates of metabolism are related not directly to weight but to a power of the weight, the power exponent being less than one, and probably about 0.75 or less. Method 3 gives food consumption figures that are inevitably wild over-estimates of the rates of food consumption by the larger whale species, and the results from it provided by Tamura/Ohsumi are worthless.

Method 2 is based on ways of determining, estimating or guessing the rate of metabolism of an animal (depending, as does Method 1, on a power function of its body weight) and then calculating how much, and of what, it must eat to meet the energy required for proper functioning of that metabolic system and growth of the animal. In this critique each stage of that complex – and rather unreliable - calculation is examined. At each stage, it is found that substantial errors and distortions have been made by Tamura/Ohsumi, too many in fact to summarise fully in this resumé. In some cases the authors say they have calculated certain parameters using a certain procedure when in fact they have simply copied numbers from other authors, who have themselves copied them from predeccessors, and so back to what is sometimes a mere guess on the part of the original author. In other cases, the earlier authors' results have been misunderstood and misused. In yet other cases, the voluminous existing scientific literature has ben cited selectively, mostly in a way such as to inflate the authors' figures for food consumption, especially by the larger species of cetaceans. A telling example is an assumption (derived from someone else's guess 30 years ago) that minke whales and presumably other baleen whales cannot digest the chitin "shells" of crustaceans, an assumption that has been disproven by the work of Norwegian nutritionists who have demonstrated that the forestomachs of these whales contain chitin-digesting microbes, analogous with the cellulose-digesting microbes in the stomachs of ruminants. This error alone inflates all food consumtion estimates for at least the most abundant baleen whales, by 25%. This inflation might not seem to be excessive considering all the other unquantified uncertainties in this whole exercise, but it has to be remembered that the final consumption results are obtained by multiplying together, in a "product chain", a series of numbers, most of which are of dubious validity.

Among the authors cited by Tamura/Ohsumi on this stage of the calculations, there are apparent contradictions and some confusion regarding the definitions and quantitative relations among rates of basal, resting and active metabolism. This has let to inappropriate upward "corrections" being made to functions relating energy requirements of whales to their body sizes.

To convert figures for metabolic demand to figures for food consumption it is necessary to know the dietary composition of the whales and the energy density of each item of diet. The application of information about those two requirements has been carried out erroneously by Tamura/Ohsumi, by first assuming an average energy density for the entire diet and then applying that to the specrum of its components. However, a greater procedural error is to make incorrect assumptions about the actual energy densities and food composition. With regard to the first of these Tamura/Ohsumi select certain values for food items, ignoring the great diversity of estimates in the published literature which provides very different values for numerous species of pelagic crustaceans and fishes.

A larger problem is the diversity of data on whales' dietary composition. Again, the scientific literature has been used selectively, and the immense variability of the information about composition has been ignored. Research over half a century, based largely on examination of commercial catches as well as of some "scientific whales", has shown that the apparent diet of a whale species can vary immensely from one location to another location close by, as well as between dates within a year, from one year to another and from one period to another. Whales killed in fishing areas and in the fishery season are often found to be eating some fishes, but outside those areas and times their diet can be completely different, with no fish in it. Furthermore it is now known that different individual whales have persistent food preferences, and what they all eat is certainly not a simple function of what food items are present in the area in certain quantities and proportions. Here we are at the frontier of knowledge of animal behaviour, especially about foraging "strategies", but what little is now known should not be ignored.

Although there are big uncertainties at every stage of the "product chain", possibly the biggest concerns the composition of the whales' diet, and its consistency and what influences its changes. Some of the Great Whales undeniably eat some quantities of fish of some valuable species in some places some of the time. What those quantities are, and how they are and might be affected by a variety of factors, remain essentially open questions., questions that are obscured rather than enlightened by the two documents here reviewed. But the deeper, more interesting and more important questions are what effects predation by whales on a variety of organisms - only a few of which are of direct or indirect economic interest to humans - has on marine ecosystems, and what changes in those effects will happen as depleted whale populations recover from past exploitation or, alternatively, are further reduced by renewed and expanded whaling. Those questions are not even recognised in the Tamura/Ohsumi documents. But behind them are also the even more difficult questions regarding the longer-term effects of ongoing changes in the ocean "climate", both the natural ones and those caused by human activities.

This critique, while focused on Japanese publications, also takes into account similar calculations made, and conclusions drawn, by some scientists working for the Governments of Iceland and Norway, the other two countries still engaged in commercial whaling, and which are in accord with the ICR's views but promoting them less aggressively.

Appendices to this critique are intended to help readers to see the issues raised in broader contexts. One such context is the scientific requirements for rational consideration of proposals to "cull" marine mammals for the alleged purpose of fisheries protection or improvement; these emerged from a UN-sponsored study. Another is the national political environment in which the ICR operates, which is explained by the author in a recent publication.

Since the thrust of the Tamura/Ohsumi documents is a crude comparison of predation by whales with marine predation by humans, another Appendix examines the human side of this unbalanced equation. While practically all of the medium sized crustaceans (shrimps and prawns) caught are consumed as food, nearly half of the fishes landed are converted to fishmeal and oil that enter into the human food chain only via the raising of poultry, other terrestrial livestock and cultured (mainly freshwater) fishes, involving a five- to ten-fold loss of consumable product. In addition there are very high losses between production of edible commodities and ingestion by humans. At the other end of the economic production process we know that there is a huge wastage of marine resources through discarding of unmarketable or less valuable fish catch, spoilage, incidental capture of non-target animals such as turtles and seabirds etc., and in other ways. What some of the whales eat that might have been useful also to humans is far outweighed by the losses of valuable resources in the human operations.

Lastly, it must be said - although the critique does not discuss this in detail – that the main threats to living marine resources now come from mismanagement of their utilisation and from deleterious effects of a variety of human activities on the marine ecosystems. The Law of the Sea, to which most nations adhere, in words if not always in practice, does not authorise the killing of whales to protect commercial fisheries; on the contrary it calls for fisheries for prey species to be so managed as not to impair the biological productivity of dependent species, *i.e.* predators such as the cetaceans.

1 Introduction

"In power and influence counting counts. Quantification brings credibility. But figures and tables can deceive, and numbers construct their own realities" *David Boyle, 2001*

In June, 1999 Dr Seiji Ohsumi, who was at the time Director of the Institute for Cetacean Research (ICR) in Tokyo, and senior member of the delegation of Japan to the International Whaling Commission (IWC), made available to the IWC's Scientific Committee – as an information document – a paper he had co-authored with Dr Tsutomu Tamura entitled "Estimation of total food consumption by cetaceans in the world's oceans". This paper was not taken very seriously by the Scientific Committee, nor was a second, expanded version of it that was provided to the Committee the following year ("Regional Assessments of prey consumption by cetaceans in the world", *IWC Doc. SC/52/E6*.) These two papers have never been reviewed by Ohsumi's and Tamura's peers, nor published in the scientific literature. They have, however, been printed and reprinted several times by the ICR and by agencies of the Government of Japan.[3] This "non-publication" has provided the core propaganda material to justify an expansion and diversification of large scale killing of whales for purportedly scientific purposes, the recruitment of new States Members of the IWC to support Japan's anti-conservation policies there, and to back demands for legalising the resumption of commercial whaling and the abolition of the whale 'sanctuaries' established by the IWC in the Indian and the Southern Oceans in 1979 and 1994. The claim also provides a straw for those to cling to who wish to deny that the crisis in world fisheries is due mainly to bad management and rampant over-fishing.[4]

Before I discuss the two Tamura & Ohsumi (T&O) documents in detail I should mention another paper by Dr Tamura, with similar title [5] to that of T&O 2000, which was published in 2003 as Chapter 9 of a book edited by M. Sinclair and G. Valdimarsson "Responsible Fisheries in Marine Ecosystems" (FAO and CABI Publishing, Wallingford, UK, p 143-70). This book originated as the Proceedings of an international conference on that subject held in Reykjavik, 1-4 October 2001, sponsored by FAO and the Government of Iceland, with co-sponsorship by that of Norway.[6] This was the scene of the first foray by the ICR into the core fisheries field in contrast with what many would regard as its margins, as represented by the IWC. Tamura's paper is essentially a version of the original T&O 2000, and the same methods for deriving the results were cited. The global and Regional totals tabled are the same except for the toothed whales in the

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[3] Notably in an undated compilation by the Fisheries Agency of the Government of Japan, entitled "Increasing Competition Between Fisheries and Whales: Japan's Whale Research in the Western North Pacific (JARPN II)" that was distributed informally at the meeting of FAO's Committee on Fisheries (COFI) held in Rome, March 2005.

[4] This strategy was examined in a document by the author on behalf of IFAW provided to the 2005 meeting of COFI, entitled "Are Whales Enemies of Humans?" (Not published; available from the author).

[5] "Regional assessment of prey consumption and competition by marine cetaceans in the world".

[6] The published contributed papers were not, apparently, peer-reviewed.

North Pacific (NP). The differences arise from a change in the estimates of fish and cephalopod consumption by the common dolphin that leaps from a median value of 4.9 to 5.5 million tonnes. Both the fish consumption estimates and the cephalopod estimates were increased by the same amount – 16%. Since Tamura 2003 and T&O 2000 used the same estimates of population size I could only guess that different assumptions were made for the average body weight of this species. Tamura does not tabulate nor give references to his assumptions for that parameter, but he does tabulate his population biomass values. T&O 2000 do not tabulate biomass figures but those can easily be calculated from the figures they cite for number and average body weight. The two biomass figures for the common dolphin do differ by the expected 16%, and this has been caused by an increase in the assumed body size from 8o kg to 93 kg. What concerns me here is that an increase in one food consumption estimate by over half a million tonnes arises from adjusting the assumed body weight of one small but numerous cetacean. That well illustrates, I think, the fragility of this entire exercise.

In consideration of his new audience, Tamura inserted in his 2003 paper sections with more pointed comment on the potential threat by whales to commercial fisheries, especially in the North Pacific. I make further comment on these as appropriate in later sections of this paper.

The quantities given in the two T&O documents mentioned above differ considerably from each other; the methodologies are the same but revisions were made to the cited estimates of the number of each whale species in various Regions and to the values used (and authorities cited) for the average weights of individuals of each species of whale in each ocean Region. Here I shall refer mainly to the figures in the later paper, but from time to time comparisons between them reveal features that are not obvious from either of them separately.

The authors claim that cetaceans (whales and dolphins) consume as food, by weight, three to six[7] times more of the living resources of the ocean than the annual landed fish and shellfish catches by humans – 84 million tonnes in 1999 - and that this is a matter of great concern regarding the state and future of sea fisheries, globally.

Here, I examine these claims, and their significance – if any – for the determination of fisheries and whaling management policy.[8] **I conclude that the authors have been selective and careless with respect to the published data they have used, that their methodology is deeply flawed, and that their results and conclusions are wholly misleading, they have conned us.**[9] These documents, on which Japanese whaling policy is now publicly justified, are worthless. Worthless, that is, as contributions to rational discussion of relationships between whales and fisheries But certainly not worthless in underpinning the Japanese Government's public relations campaign of disinformation

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[7] This range is not a measure of statistical error but rather the bounds of the use of three different methods of estimating consumption, details of which are examined later in this paper.

[8] Appendices 1 and 4 illustrate the types of information that **are** relevant to evaluating any proposals to "cull" whales.

[9] Played a confidence trick.

on that subject. Furthermore, since the ICR is both responsible for the circulation and political use of the two documents, and for the planning, conduct and evaluation of Japan's "scientific whaling" activities (which have made a very limited contribution to the information about whale diets that T&O have used) the ICR, as an institution claiming to be a scientific establishment, is culpable of the systematic distortion of science for political purposes.

In his delightful book, "The Tyranny of Numbers", David Boyle [10] lists nine "counting paradoxes" of which the eighth is

"Measurements have a monstrous life of their own"

We shall see how true this is of the almost endlessly repeated numbers in the two ICR papers. Boyle makes so many observations pertinent to our present subject - including those quoted at the head of this Introduction - that I cannot resist the temptation to quote another of them:

What can be measured and manipulated statistically is then not only seen as real, it comes to be seen as the only or the whole reality."

And then, quoting J.M.Keynes:

"It is astonishing how many foolish things one can temporarily believe if one thinks too long alone." [11]

As an example of Boyle's assertions about reality and deception, akin to what has been called in another context "being economical with the truth"[12], it is instructive to examine the main lines of T&O's summary tables. **ICR says that "Whales consume a colossal amount of fish"**.[13i] The "colossal amount" is said to be between 249 million and 436 million tonnes/year, as compared with world fisheries landings of 90 million. But the language then slips: the "colossal amount" is identified as "prey"; it is not "fish" (actually most of it is zooplankton). Then, the two "colossal" figures may appear to signal some kind of statistical uncertainty, an error range. But that is not so. They are actually the highest and the lowest of three figures obtained from three distinctly different methods of calculating the food consumption per whale.

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[10] "The Tyranny of Numbers: Why Counting Can't Make Us Happy" Flamingo, HarperCollins, 238 pp, 2001

[11] And, as Descartes reportedly said: "Those who believe absurdities will commit atrocities". He cribbed that from Voltaire, I think; see footnote 115.

[12] A phrase famously coined in 1986 by Sir Robert Armstrong, Secretary to the British Cabinet, when being interrogated in an Australian court by Counsel Malcolm Turnbull about the difference between lying, giving a misleading impression and 'a sort of bent untruth'.The record of this fascinating and illuminating exchange is summarised in Philip Kerr's "The Penguin Book of Lies", Viking, 1990, 543pp.

[13] "Why Whale Research", one of many colourful brochures on this subject published by the ICR, this one in June 2004.

2 Differences between T&O 1999 and 2000.

Table 1, below, contrasts T&O's results as given in their two papers.

METHOD 2 > *REGION*	Consumption (million tonnes) T&O 1999	Consumption (million tonnes) T&O 2000
South Hemisphere SHIO		
Baleen whales	76.9	100.7
Sperm whale	49.5	35.6
Other toothed whales	41.1	12.7
All toothed whales	90.6	48.3
All cetaceans	167.5	149.0
North Pacific NP		
Baleen whales	16.4	19.2
Sperm whale	21.0	15.9
Other toothed whales	28.1	27.0
All toothed whales	49.1	42.9
All cetaceans	65.5	62.0
North Atlantic NA		
Baleen whales	18.6	25.4
Sperm whale	39.1	24.2
Other toothed whales	12.1	10.4
All toothed whales	51.2	34.6
All cetaceans	69.8	60.0
World Total W		
Baleen whales	111.8	145.3
Sperm whale	109.6	75.7
Other toothed whales	81.4	50.1
All toothed whales	191.0	125.8
All cetaceans	302.8	271.0

The above figures all pertain to the median of T&O's three methods of calculating total consumption of food by cetacean populations, i.e. their Method 2 (See later). The adjustments made in the 2000 paper reduce the total by only a little more than 10%, both for the world total and the Regions separately. But this consistency is deceptive. First, all the baleen whale figures have been increased substantially, by up to 30%, while the toothed whale figures are all correspondingly reduced. Second, it is noteworthy that the proportion of sperm whale consumption to that of other toothed whales is very different from one Region to another. But these differences are artefacts arising from the fact that in the NA there are few data for the population sizes of most species of smaller whales and dolphins, whereas in the NP there is more information. Among the baleen whales the NA figures exclude Bryde's whales. The figures for the Southern Hemisphere including the Indian Ocean (SHIO) mainly reflect extremely high consumption figures for the beaked whales, and also for the orca ("killer whale"); the former derive from an estimate of one million tonnes, having hardly any scientific basis, made by ICR scientists based on a small number of observations of beaked whales during the decade of surveys made under the auspices of the IWC in the International Decade of Cetacean research. (IDCR).

Closer examination reveals that the difference between the two global estimates of food consumption by all species of cetaceans is entirely due to a 39% reduction in the estimated consumption by toothed whales except the sperm whale. In fact the totals for baleen plus sperm whales are almost identical: 221 million tonnes. But this identity hides other big differences. The T&O 2000 estimates for baleen whales are 30% higher than those in T&O 1999, with the sperm whale estimates being correspondingly lower. Similar relations hold roughly for the three Regions treated separately.

The two studies are less inconsistent with each other when examined on a hemispheric basis. 69% of total consumption by baleen whales is in the southern hemisphere, 31% in the northern (NH). Likewise 46-47% of consumption by sperm whales is in the SHIO, 53-55% in the NH.

3 Limits of the scope of this analysis

The main value of the T&O papers to those who sponsored them is to provide evidence that the current moratorium on commercial whaling, as well as its indefinite prohibition in the Southern and Indian Oceans should be cancelled, because measures to conserve and protect whales constitutes a danger to world fisheries. The ICR also maintains that restriction on exploitation of minke whales allows that species to increase and impede the recovery of the almost extinct blue whales, since the two species share, in the SHIO, the same diet of small euphausid crustaceans, popularly called krill. These claims are used, meanwhile, to justify the killing of large and increasing numbers of whales in order to examine the contents of their stomachs. Fear of the whales' impact on fisheries has also led many governments with no serious interest in whales or whaling to join the IWC and add their voices and their votes to moves to dismantle existing conservation measures and to impede the adoption of new ones. This strategy and the form of the associated propaganda are described in Appendix I.

The so-called commercial moratorium, adopted by the IWC in 1982, set zero catch limits into the indefinite future for all species of cetacean for which the IWC has to date accepted full responsibility for management and conservation. These are all the baleen whale species (the *Mysticetes*, or "whalebone whales"), the sperm whale, the Northern (Atlantic) bottlenose whale, and the orca. However, the commercial exploitation of the sperm whale was banned, worldwide, in 1980 by an entirely separate IWC decision, while for the rest zero catch limits had been set nearly everywhere for them all, except the minke whale, some years before the 1982 decision (which did not come into force until 1986). So here I concentrate on the baleen whales, whose diets mostly overlap to some degree with that of the minke. But some attention will necessarily be given to the sperm whale, and even in a particular sense to some of the smaller toothed whales because their feeding habits, in captivity, have been used by T&O to estimate the food consumption by free-ranging baleen whales a hundred times larger.

4 Relevance of ICR studies to whales' impact on fisheries.

A vast number of publications – scientific and popular – have been issued during the past twenty years or so about the impact of marine mammals on fisheries, and of fisheries on marine mammals, and it is far beyond the scope of this analysis to review them here, or even to list them.[14] Many of them discuss the common proposals that predators such as cetaceans, as well as seals and even seabirds, should be "culled" to benefit fisheries production and operations. Virtually no such proposals, however, stand up to scientific examination. There are several reasons for this. One is that the "target" species selected for possible culling are rarely the only species consuming the prey species of commercial interest, usually there are many competing predators; in the sea these are mostly other fishes, squids, seabirds, seals and other less conspicuous types of animals. Cannibalism is also common – for example an important food of some of the larger fishes is the young of the same species.

Food left "surplus" by killing some of one predatory species are as likely as not to be eaten by other marine predators rather than accrue to the fishermen's future catches. In addition, most predators consume a variety of prey species, some of which are themselves predatory on species of commercial interest to humans. The relative amounts of the different prey species that one predator may consume also changes with the changing abundances of the prey and also the changing abundance of the predator itself, whose deliberate selectivity of diet can alter in complex and unpredictable ways. In this situation identifying the species preyed upon, and even measuring the quantities eaten, contribute little to an enhanced understanding of the processes going on in the marine "food-web", let alone an ability to predict changes. (See Appendix II). The United Nations Environment Programme (UNEP) has produced a "Protocol" specifying the kinds of information that will be needed for any rational consideration of proposals for "culling" marine mammals, but without making any promise that even with such information successful predictions could be made.[15]

Both the structures of marine food webs and ecosystems, and the behaviours at least of the mammalian predators, perhaps even of all the vertebrates, are complex and little understood. Consequently it has proven quite impossible so far for scientists to predict with confidence the likely effect of deliberately reducing one or more of the predatory species, nor even the likely *direction* of change. An important consequence of such difficulty is that those responsible for the management of fisheries, and also representatives of the media, the political decision-makers, and the broad public commonly find it easier to believe simply that if whales eat fish, then reducing the whales *must* lead to increased fish catches. It is this simplistic, widespread but wholly erroneous presumption that

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[14] Greenpeace International distributed at the IWC meeting in 2005 in the Republic of Korea a compilation of several of the most useful and accurate of these, some in languages other than English, such as French, Spanish, Japanese and Arabic. The book is entitled "Whales and Fish", and is available gratis from GP International, Amsterdam.

[15] "Protocol for the Scientific Evaluation of Proposals to Cull Marine Mammals." A Report of the Scientific Advisory Committee of the Marine Mammals Action Plan, UNEP with the support of World Wildlife Fund, Greenpeace International and the International Fund for Animal Welfare.

has been exploited by the ICR publications. That is one good reason for examining them critically.[16]

Before leaving this subject, however, I should mention the recent work (still in progress) by Kristin Kaschner and Daniel Pauly.[17] These authors have looked closely at the geographical distributions of fish and fisheries for the species eaten particularly by the whale species, including the minke, that do consume significant quantities of fishes and other organisms of commercial interest to humans. Their analysis "clearly shows that there is no evidence that food competition is a global problem, even when the uncertainties associated with the available information are considered.[18] Consequently there is no basis to blame marine mammals for the crisis world fisheries are facing today. There is even less support for the suggestion that we could solve any of these urgent problems, caused by a long history of mismanagement of fisheries, by reducing marine mammal populations." These researchers acknowledge that "there may be potential for conflct in the small geographic regions in which marine mammal food consumption overlaps with fisheries and that these areas warrant further investigation." They are particulalry scathing – rightly – about the promotion of the argument that culling marine mammals would contribute in any way to human food security: "Combined with references to hunger in poor countries, these arguments articulate a potentially dangerous and misleading view of the interactions between humans and marine mammals on the one hand and hunger and natural resources availability on the other." My own view of this matter is, I admit, stronger; I think the juxtaposition and attempt to link the two is an obscenity. It would be fair to say, however, that a different and contrary view is held by Professor Milton M. R. Freeman, of the Canadian Circumpolar Intitute, Alberta, although – despite the all-encompassing title of his article on this question – he confines his case (perhaps wisely) to subsistence whaling by

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[16] Readers who wish to get into the scientific literature on this subject could first read Peter Yodzin's "Must top predators be culled for the sake of fisheries?" *Trends Ecol. Evol.* **16**(2): 78-84, 2001, followed by Ian Boyd's comment on Yodzis' paper and Yodzis' response, in *Trends Ecol. Evol.* **16**(6): 281-2, 2001. Yodzin's 1994 paper "Predator-prey theory and management of multi-species fisheries" (*Ecol. Applic.* 4**(**1): 51-8) elegantly demonstrated that even common models of only two or a few interacting species give rise to seriously iassed advice because they assume inappropriate mathematical functional relationships between predator and prey populations. D. M. Lavigne's "Marine mammals and fisheries: the role of science inn the culling debate" (p 31-47 *in* Gales, N., M. Hindell and R. Kirkwood (*eds*.). "Marine Mammals: Fisheries Tourism and Management Issues". CSIRO Publishing, Collingwood, Victoria, Australia), is up-to-date, thoughtful and fair, and has an extensive list of references.

[17] The most accessible non-technical reference to this is "Competition between Marine Mammals and Fisheries: Food for Thought", May 2005, 28pp. Distributed at the 56th Meeting of the IWC, Sorrento, Italy.

[18] In an important paper on the Antarctic krill fishery and minke whale distribution T. Ichi has shown that not only are two different species of *Euphausia* involved, with different distributions, but the fishing areas overlap only partially with the minkes, with the whales feeding close to the ice and krill mainly concentrating further north at continental and insular shelf breaks. "Distribution of Antarctic krill concentrations exploited by Japanese krill trawlers and minke whales" *Proc. NIPR Symp. Polar Biol.* **3**: 35-56, 1990.

indigenous peoples and *small-type coastal whaling* for minke and Baird's beaked whales in Japan.[19]

5 The ICR Methodology

Quantities of food ("marine resources") consumed annually were calculated in the following way. For each of the three ocean Regions values were assembled from some of the published scientific literature, as well as some unpublished studies, for the current number of whales of each species. For each of these a value of the average body weight of the species was chosen. From the average body weight alone (i.e. regardless of species) a simple formula was used to estimate either the weight of the daily food consumption by an individual (Methods 1 and 3) or the metabolic requirements of the animal (Method 2). For methods 1 and 3 these consumption figures were multiplied by the number of animals of each species.[20] In the later paper (T&O 2000) stomach-contents data are then used to divide the total quantities into different classes of diet. For method 2 – which as we shall see is the most reasonable of the three, at least in principle – it is of course necessary to convert the metabolic needs (expressed as energy, in kilocalories) to weight of food consumed. But, where the diet is comprised of mixed species items, which may nave different energy values per kg, the composition of the diet must be determined *before* the conversion can be made. In addition, it is necessary to estimate the efficiency and the degree to which the intake of food is actually utilised.

It should be noted that the results of examining the stomach contents of dead whales play no part in estimating how much they have eaten, except for the energy adjustments mentioned above for Method 2. Furthermore the data regarding dietary composition obtained in this way, either from commercially caught animals in earlier days of whaling, or from animals killed under special scientific permits, are of little more than qualitative interest, although T&O are not the first scientists to try to use them as quantitative indications of diet.

T&O were not the first to apply Methods 1 and 2 for this purpose. Their calculations are practically identical with those of Sigurjónsson and Vikingsson, 1998 (S&V)[21], but extended from the central North Atlantic to the entire North Atlantic and thence the world, with some changes in assumed parameter values.

For most species groups and Regions the T&O's estimates of food consumption are lowest for Method 1, highest for Method 3, and intermediate for Method 2. The exception is the toothed whales except the sperm whale, in the NP, where

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[19] "Energy, f ood securityand A.D.2040: the case for sustainable utilization of whale stocks". *Resource Management and Optimization* **8** (3-4): 235-44, 1991.

[20] Strictly speaking the *average* body weights should not be used, but rather the *distribution* of weight in the population to which the formulae mentioned below would be applied, but for the present purposes the use of the average might not be much biased.

[21] Sigurjónsson, J. and Vikingsson, G. A. "Seasonal abundance of and estimated prey consumption by cetaceans in Icelandic and adjacent waters" *J. Northw. Atl. Fish. Sci.* **22**: 271-87.

Method 1 gave much higher values than Method 2 and resulted in the total for all species being 16% less for Method 2 than for Method 1. This general qualitative consistency in differences is due to the use in all of them of the same values for average body weight and numbers of whales. The differences themselves arise from the differences in the conversion formulae from body weight to either consumption or energy needs, so these conversions deserve close examination.

Quantitatively the differences between the results of applying the three methods are large, especially between Methods 2 and 3. Considering the totals for all species in SHIO the Method 3 estimates are 64% higher than the Method 2 values. Corresponding values for the NP and NA are 37% and 78% respectively. For the SHIO Method 1 gives a value for consumption 23% higher than method 1; for the NP and NA the corresponding differences are *minus* 16%, and *plus* 8%.

Differences among the results from the three methods are greater when one looks at values for baleen whales, and even more with respect to the larger species. For example, in the NP Method 3 gives a value for consumption more than double that from Method 2, for baleen whales overall; the difference for the blue whale is almost threefold. In SHIO the baleen whale difference between methods 3 and 2 is 58%, while that for the blue whale is 130%. Corresponding ratios for NA are 104% and 170%. Thus the three methods give very different results when examined by species, species groups and Regions, and so comparative evaluation of their validity is essential.

In their later paper T&O broadly group food items of cetaceans into three classes: fishes, cephalopods (mainly squids) and crustaceans, and they similarly group recorded catches (in 1996), from recent FAO of UN publications, plus a fourth class – "Other". These sets of classes are, however, not really comparable. There is, for example, very little overlap between the fish species recorded as being eaten by whales and dolphins and those in FAO's classification of commercial catches. FAO's "cephalopods" class includes octopus, and many squid that are mostly near-surface living pelagic species, while the squids eaten by toothed whales, especially by the sperm and deep-diving beaked whales, are very different, pelagic but deep-living, species. As to crustaceans, the only species eaten by the baleen whales - with the possible exception of the mainly bottom-feeding grey whale of the eastern North Pacific – are tiny zooplankton: euphausids, copepods and amphipods, while the catches by humans are, obviously, nearly all large crabs and lobsters and medium sized shrimps and prawns, mostly living on or near the seabed.

6 Comparisons of fish catches with consumption by cetaceans.

As we shall see, the baleen whales digest practically everything that they eat, or at least the minke whales do so. Humans, on the other hand, destroy enormous quantities of "marine resources", directly and indirectly, and only a small proportion of what is killed is metabolised. The "losses" are of many kinds. First, for every fish of a certain species caught and brought to market, several are thrown back into the sea because they are too young and small, or in numbers exceeding such catch quotas as may be set for a particular fishery. Then, many fish that could in principle be consumed are thrown overboard because there is no local market for them; the demand is in many places very selective. Third, the destruction of other organisms than those of direct use to humans is enormous; that ("rubbish") which is hauled up in nets if discarded but much else simply does not reach the surface. The wanton destruction of dolphins, seabirds, seals and turtles is well known and increasingly being documented. In addition there is increasing evidence of what has long been a controversial matter: the destruction of marine habitat, especially on the seabed, by trawls and other mobile fishing gears.[22]

The wastage does not end there. Some fish brought to shore does not get sold and eaten before its condition deteriorates. Half of the annual fish catch is converted to fishmeal and oil, which is used as a feed supplement for raising chickens and trout and other "cultivated fishes. The estimates of the ration of fish protein input to output of fish vary over a wide range but are for accounting purposes commonly assumed to be 10:1, though some efficient process may achieve a ratio of 5:1. So, the amount of the recorded landed catch that reaches the market as edible fish is roughly 60% of the landed catch in wet weight. After that, the fish is processed at home, and of course the head, skeleton and internal organs, the carapaces of crustaceans, and a considerable part of the bodies of the cephalopods are neither ingested nor digested by the human consumers.

There are several partial estimates in the literature of various elements of this wastage, and it is difficult to put together a global figure. But such information as we have suggests that for every tonne of protein of marine origin that enters the human digestive system perhaps ten tonnes are destroyed by fishing operations, are discarded or are not utilised. This should be borne in mind when thinking about the amounts consumed by cetaceans. Some more detail is given in Appendix III.

T&O 2000 also present summaries of FAO catch (landings) statistics by three 'regions', to which they give the same labels as to the whales' food consumption data. One would expect those to correspond, at least roughly, with the three major groupings of the baleen whales. They do not, however, divide the world ocean into three parts in the same way. For each fishing region T&O provide a list of FAO's Statistical Areas. Their *North Atlantic* fishing region is actually defined as including not only the Mediterranean and Black Seas – which is perhaps not unreasonable – but also the two Areas called the *Central Atlantic* by FAO, down to 5°N latitude in the west but to 6°**S** latitude in the east. While the migratory whales in the *Western Central Atlantic* (FAO Area 31) belong to the North Atlantic populations, at least some – perhaps most – of those in the *Eastern Central Atlantic* (FAO Area 34) belong to the Southern Hemisphere populations.

The ICR definition of *North Pacific* fishing is even more bizarre. It includes FAO Areas 21 and 27, which divide the region at 42°W and extend down to 35-36°N. But it also includes Areas 71 and 77, which extend far into the Southern Hemisphere, to 25°S, including the entire tropical *Western* and *Central Pacific* down to Brisbane in Australia, and all Central America and the waters of the North-Western part of the continent of South America on the east. Thus the

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[22] FAO has recently published a review of this problem: "Impacts of trawling and scallop dredging on benthic habitats and communities" by S. Løkkeborg *FAO Fish. Tech. Pap*. **472**. 49pp.

ICR's *North Pacific* fishing region includes the distribution of large whales in both the real North Pacific and the Pacific sector of the Southern Hemisphere.

It is also worth bearing in mind that the ICR's SHIO comprises the Southern Ocean (roughly south of 40-45°S) – including the Antarctic waters (roughly south of 55-60°S) where the baleen whales mostly feed - and the temperate, tropical and sub-tropical waters further north, through which most of them migrate, where they breed and on the whole do not eat, or not much. FAO distinguishes the "Antarctic" zones of each of the Atlantic, Indian and Pacific Oceans (in some Tables totalling the three} from the seven Areas northward to the temperate zones and the tropics. Thus the ICR *Southern Hemisphere and Indian Ocean* fishing extends well northward of the Equator in some sectors, while in other sectors the boundary is far to the south of the Equator. These discrepancies make the regional comparisons between whale food consumption figures and fish catches more questionable than they might otherwise be.

In fact the precise quasi-equatorial distributions of the baleen whales are not very well known. Different seasonal appearances provide one source of data, and there are some confirming returns of tags. It might be supposed that the latitudes of the major equatorial currents might indicate likely boundaries, too. In that connection it may be worth noting that in the Pacific the warm eastward flowing Equatorial Counter Current runs at about 5°N, with the westward flowing North and South Equatorial Currents at roughly 13°N and 0° rspectively. The surface oceanographic "equator" in the Atlantic is rather different and less clear; the westward flowing Equatorial Currents are close together just a few degrees south of the geographical Equator.

7 The numbers of Whales

Although my focus here is on the baleen whales it is worth commenting on T&Os' presentation on the sperm whale. There are no current "official" (IWC) estimates of the number of sperm whales that have survived the intensive whaling world-wide, of the 1950-70s, or evidence of whether they are increasing under protection or still declining. For many years the IWC Secretariat provided enquirers with a round number - one million - but that had no real scientific basis and has since been dropped. The IWC Scientific Committee has made no assessments of sperm whale populations during the past 25 years. T&O use a figure of 501,000, of which 20% are said to live in the NP, 38% in the NA and 42% in the SHIO. Dr Hal Whitehead, of Dalhousie University, Canada, has recently done his best – and he is a recognised world expert on the matter – to assemble a global figure for the number of sperm whales.[23] Whitehead's figure is 32% of an original (pre-commercial whaling number of 1.1 million, i.e. 350,000. Even this number has, however, wide statistical confidence limits: roughly ±40%. Whitehead's work is not cited by T&O, who chose to use a highly questionable, unpublished value for the SHIO. This is based on the IDCR surveys of minke whales in the Antarctic, during which other species were of course sighted. However, the behaviour of the sperm whale, which undertakes prolonged deep dives for feeding purposes is entirely different from that of the baleen whales, and the sorts of calculations from sightings cruises made for the latter are not

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[23] Whitehead, H. (2003) "Sperm Whales" Univ. Chicago Press, 431 pp.

applicable to the former. The two sources cited by T&O for the sperm whale in the NA are fairly recent publications. They are not, however, original analyses but, rather, uncritical compilations of much earlier material, much of it now discredited for methodological reasons. The number for the NP that is used comes from an unpublished paper by H. Kato and T. Miyashita provided to the IWC Scientific Committee in 1998 and might be more reliable than the citations for the other Regions.

T&O s' 2000 global sperm whale number is 16% lower than their 1999 number, only because they revised down their earlier estimate for the SHIO of 290,000 to 209,000, yet – puzzlingly – they cite the same reference for it. On the other hand, although they give the same figures in both papers for the NP and the NA, they cite *different* references! However, since - as we shall see - the calculation of food consumption quantities for the sperm whale is a meaningless exercise, I shall not pursue these puzzles further.

With respect to the baleen whales it is revealing again to begin with a comparison of the figures in T&O 1999 with those in T&O 2000. Take, for example, the blue whale in the SHIO. In T&O 1999 this is given as 610 animals (citing Laws, 1977 and Butterworth *et al*, 1994); in T&O 2000 as 1255 (citing IWC, 1996 and Perry *et al*, 1999). Low numbers, but one is twice the other. This change does not come from the availability of new data, but rather from different reviews of old data. Did not T&O know, in 1999, about the IWC 1996 report? Was Butterworth *et al* 1994 discarded by the scientific community between 1999 and 2000?

Similarly, in the NP the blue whale jumps in number from 1600 to 3300, again with different citations. But in the NA different citations lead to a *fall* from 1378 to 330. No explanations are offered regarding the selection of different citations, or comment on the validity of one set rather than the other.

The blue whale contributes, on an individual basis, a high consumption figure because of its size and of the way in which consumption is assumed to be related to size, as we shall see in Section 7 below. Contribution by this species to overall estimates of food consumption by the baleen whales is relatively low because of its residual numbers. But what are we to make of T&O's results for the fin whale, which is both large and numerous enough to account for a great part of the total consumption by baleen whales? In T&O 1999 the fins number 18,000; in T&O 2000 they jump to 85,000 in the SHIO, with different citations. The Bryde's whale makes a minor jump from 78,000 to 89,000, but from the same source - Ohsumi, 1981. In T&O 1999 there is an additional Bryde's reference: IWC 1997, that has disappeared from the T&O 2000 paper, without any explanation such as that the IWC might have rejected its own estimate made only two years before. For the NP the Bryde's whale estimate has increased by 13,000 over the original 22,000 by the inclusion of the Eastern Tropical Pacific, omitted from the first paper. And in the NA there are no data for this species. These and similar features for other species make it very clear that not only are the 1999 and the 2000 papers not comparable, neither are Regional comparisons within the 2000 paper valid.

Most attention has been given in the claims made in recent years about baleen whales' impact on fisheries, to the minke whale. This species is still fairly numerous in all Regions. According to T&O 2000 the minke whale biomass in the SHIO is only slightly more than that of the fin whale (5.0 compared with 4.7 million tonnes) but its food consumption is about twice as much. In the NP, however, the minke whale is said to consume much less than either the fin or the Bryde's whale and in the NA very much less than the fin whale. The role of the minke depends critically on the validity of estimates of relatively large numbers of a small species.

For the SHIO T&O consistently use a figure of 761,000 minkes, derived many years ago (IWC 1991) from the *first* series of IDCR surveys. In fact, no such number is cited by the IWC Scientific Committee, which gave its best estimates of the number in each of six sectors of the Antarctic, surveyed in successive years. The Committee did not add these six figures together to get a global total, for good reasons: having been counted in different years (and, indeed by slightly differing methods) one cannot be sure that one is counting different "populations" of a highly mobile species. Furthermore, with that consideration, the statistical error ranges quoted by ICR scientists are meaningless. However, the fact is that when the data from the *second* series of survey cruises under IWC auspices came to be analysed it was found that the results were not compatible with the earlier results; they were very much lower (less than one third), and now **the Committee has decided that there is no acceptable estimate of minke whales in the Antarctic**.[24]

T&O use a figure of 32,600 minke whales in NP (Okhotsk Sea, West Pacific and Sea of Japan), again citing references from 22 and 14 years ago. Since then there has been much debate and controversy in the IWC Scientific Committee regarding those figures, and one should expect at least that those uncertainties and controversies would find mention in T&O's papers. Once again we find the T&O 1999 and T&O 2000 papers giving the same numbers for the various parts of the NP Region, but with different authorities being cited, with no explanation.

T&O's account of the abundance of sei whales presents similar puzzles. For the SHIO the 1999 number is 17,000 while the 2000 number is only 10,860, coming from three different cited sources, only one of which is new. The NA presents a similar pattern: 9,250 brought down to 4000. But in the NP the number given – 9,110 - is exactly the same in both papers, though the cited references are different!

The figures for the humpback whale are virtually a clone of the sei. Down from 15,000 to 10,000 in SHIO; up from 2000 to 7000 in NP; again unchanged in the NA, at 10,600 but this time with the same cited source.

The above sequence of comparisons illustrates something of the uncertainty of the cited figures. One cannot say, unequivocally, that those in the 2000 paper are "better" than those in the 1999 paper. None of the changes were due to the acquisition of new data. Many of the numbers come from the application of analytical methods that are no longer considered to be reliable by most scientists – particularly the use of descending series of commercial catch-per-unit-effort data. Some come from survey data obtained from scouting vessels attached to

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[24] The Committee is committed to reviewing this matter at its annual meeting in mid-2006.

Japanese whaling fleets, a biased method that has never been accepted by the IWC Scientific Committee. The method of counting favoured at present is systematic sightings surveys, organised and evaluated under the auspices of the Scientific Committee. By far the most extensive of these are the minke surveys in the Antarctic conducted by the IWC for twenty years, yet we have seen that replications even of those produce incompatible results, with as yet unexplained three-fold differences. Possibly the most reliable numbers are those for the grey whale, which has been counted for several decades during its migrations along the west coasts of North America, from observation points on shore, and some of those for the humpback whale, whose coastal habit is more accessible to observers than the oceanic habitat of the Balaenopterids, and which is of special interest for reasons other than lethal exploitation. Some of the very small residual populations of the several species of "right" whales have also been given serious attention because of the interest in attempting to monitor their recovery under protection, or because – in the case of the Bowhead – they are the object of whaling by indigenous peoples (In IWC jargon "Aboriginal-Subsistence whaling").

T&O assert that many of the numbers they have used represent under-estimates of the true numbers, because surveys and other studies have not covered the entire Region. While that is true, surveys covering larger areas, but made at different times suffer from the facts that these animals are migratory, highly mobile, and that the same animals can therefore in effect be counted in different places. The estimates coming from descending series of catches-per-unit-effort data, while being obtained from localised whaling operations, might nevertheless reflect what is happening to the population as a whole, and provide (poor) estimates of numbers of the whole exploited population. For these and other reasons, and given the great uncertainty of nearly all the figures, it cannot be taken for granted that all of T&O's figures are systematically too low or too high.

According to T&O 2000 the fin and minke whales together account for more than three-quarters of the world biomass of the Balaenopterid whales (80% in SHIO; 47% in NP; 86% in NA).[25] It is worth noting, too, that overall the grey and right whales account for just over 4% of the total baleen biomass, but those species account for 22% of the biomass in NP. With these broad results in mind, and remembering that they depend on the validities of the cited population numbers – which are multiplied by the figures for average body weight – it seems that we should give particular attention to the two "dominant" species – fin and minke.

8 The average body weights of whales

I begin, as before, by looking at some of the changes made between T&O 1999 and T&O 2000. The blue whale of the SHIO, which we saw doubled in number from one year to the next, also increased its size – from 83.0 to 102.7 tonnes. Most remarkably, the pygmy blue whale, which jumped in number only from 4300 to 5000, increased *its* size from 68.9 to 102.7 tonnes – i.e. to the same as the

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[25] These percentages are somewhat distorted by the absence of an estimate of Bryde's whale in NA and the very high number of Bryde's in NP with a correspondingly very low number of minke there.

"ordinary" blue whale! In both the other Regions the blue whale weighed only 69.2 tonnes in the 1999 paper but jumped to 102.7 in 2000. In such ways the blue whales' contribution to total biomass, and hence to food consumption was inflated. But it is of course still small relative to the contribution from fin and minke whales.

There is one crucial difference between the 1999 and the 2000 papers: in 2000 each species has the same average weight in all Regions; in 1999 the weights differ from Region to Region or, more precisely, as between the Northern and Southern Hemispheres. In 2000, compared with 1999, the sei and humpback whales are smaller, the Bryde's and minke, and well as the blues and fins, are bigger. The average weight of Balaenopterid species (regardless of the differences in relative abundance) in SHIO is 47 tonnes in 2000 as compared with 39 tonnes in 1999. In the Northern Hemisphere it is 38 tonnes in 2000, 30 tonnes in 1999.

T&O give no details in their 1999 paper of the origins of the average body weights that they used: "we…estimated average body weight of each species in each area (Region) from published sources." But they do not reference those sources. It is possible to deduce in some cases what those sources might have been, from the details of the application of their Method 2, by consultation with the papers by other authors from which that method was derived. Certainly there is much published evidence suggesting that the average weights of various species, and their growth rates, differ from Region to Region, and so the 1999 choices appear to be more realistic. The average weight depends on the pattern of growth in weight and the age-composition of the population. Unfortunately most data come from catch samples, the age distribution of which is not the same as the distribution in the population. It may be that T&O realised this and tried to get around the problem by relying entirely on an indirect estimation method devised by Trites and Pauly, 1998.[26]

Although T&O 2000 claim that "We… estimated their body weights by use of the formula by Trites and Pauly" they in fact *copied* the values tabulated by those authors, who estimated by their new method the average weights of each sex of each Balaenopterid species, as well as others. T&O simply adopted the averages of the two sexes. Actually, Trites and Pauly calculated by their method only a mean weight for the Bryde's whale, among the Balaenopterids, and for the grey, northern right, southern right and bowhead among the other baleen whales. These were the species for which no growth curves were available, and their method was developed precisely to obtain average weight estimates for such species. By contrast, the values for all other Balaenopterids were in fact taken from growth curves in various papers by C. Lockyer. Their method is complicated but appears to rest on the assumption that the age composition of the whale populations are similar to that of the human population in the USA at the turn of the 20th Century, together with a figure for the longest individual ever recorded of each species, and another for the maximum life-span of the species. For the Balaenopterids the life-spans were taken from a 1979 paper by Ohsumi; they were assumed to be 100 years for the blue whale, 98 years for the fin whale, 69 years for the sei whale, 75 years for the humpback, and 47 for the minke. But these spans were practically arbitrarily chosen; none of those species has had the

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[26] Trites, E. W. and Pauly, D., 1998 "Estimating mean body masses of marine mammals from maximum body lengths" *Can. J. Zool.* **76**: 886-96.

ages of very old individuals reliably determined by recognised methods, such as earplug rings.

Trites and Pauly were well aware of the shortcomings of their method in the absence of reliable data. They wrote "We were not able to substantiate the maximum lengths reported in the species compilations by Klinowska (1991) and Jefferson *et al* (1993). Original data sources should be consulted wherever possible, given the wide range of unsubstantiated maximum lengths reported for many marine mammals on the World Wide Web and in the published literature. Humpback whales, for example, have been reported to have a wide range of maximum lengths. Unfortunately a number of the cited lengths are incorrect, such as the 75 ft humpback that was actually a fin whale!" Klinowska's and Jefferson *et al*'s compilations were certainly not made in such a way and with such care as would be needed for the application of the Trites and Pauly formula. It is also worth noting that Trites and Pauly's calculations for the blue whale used information about females, and they simply *assumed* that the maximum length of a male would be 5% less. The origins of T&O' figure for the pygmy blue whale remains mysterious, since Trites and Pauly did not deal with that species.

T&O's 1999 figures for the NA can be traced to a paper by J. Sigurjónsson and G. A. Vikingsson (see footnote 9) which I shall look at in more detail in Section 10. S&V based their mean weight figures for fin and sei whales on weight-length relations in Icelandic commercial catches. For the other baleen species they used Lockyer's weight-length equations (which came from commercial catch data from other Regions)[27] applied to the length distributions in Icelandic catches. T&O then simply used the same values for NP, without any adjustments for differences in size and age composition or in growth rates. T&O's values for the SHIO come, in the cases of the blue, fin, sei, humpback and minke whales, from a compilation by R. M. Laws,[28] that for the pygmy blue whale from T. Ichihara[29] (and it is surely more nearly correct than the figure given in the 2000 paper). Lastly, the value for the Bryde's whale in SHIO came from a paper by S. Ohsumi using data from the Japanese whaling there under scientific permits in the late 1970s.[30] This value is perhaps more likely to be correct than most others because it comes from "scientific whaling" which is supposedly unselective by size.

This last point raises the question of the relation between the abundance figures and those for average body weight. The population estimates will of course vary considerably depending on whether they come only from commercial catches (which – in theory at least – are legally bounded by minimum size limits, except for the minke whale) or from surveys that were not bound by a size limitation. But, whatever the choice of definition of "population", any estimates of average

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[27] Lockyer, C. (1976) "Body weights of some species of large whales". *J. Cons. Int. Explor Mer* **36**(3): 259-73.

[28] Laws, R. M. (1977) "Seals and Whales of the Southern Ocean". *Phil. Trans. R. Soc. Lond.* E **279**:81-96.

[29] Ichihara, T. (1966) "The pygmy blue whale, *Balaenoptera musculus brevicauda*, a new sub-species from the Antarctic. *in* Norris, K. S. (*ed.*)Whales, Dolphins and Porpoises" Univ. Calif. Press, pp 79-113.

[30] Ohsumi, S. (1981) "Further estimation of population sizes of Bryde's whales in the South Pacific and Indian Ocean using sighting data" *Rep. int. Whal. Commn* **31**: 407-15.

size should be matched to the same "population". Estimates from the T&P formula do not do that.

If a choice had to be made I would select the values in the 1999 paper as being the more reasonable. However, with respect to the minke whale there is a presumably reliable source of data that T&O – surprisingly - did not use: the sizes of animals in the Japanese catches under scientific permits.

Although we are not focusing here on the sperm whale it is of some interest to compare the body weights used by T&O with those provided by Whitehead, which illustrates the pitfalls of the kinds of calculations performed by T&O and by Trites and Pauly. In T&O 1999, the body weight of this species is taken to be 27.0 tonnes in SHIO and 34.3 in NP and the NA. In T&O 2000, the estimates from Trites and Pauly are used: 26.9 tonnes for males in SHIO, and 18.5 tonnes as the average of males and females in NA and NP. These latter involved the assumptions of the growth curves published by Lockyer, maximum lengths of 9.8 and 12.0 m respectively for females and males from Klinowska, and a maximum age of 69 years from Ohsumi - but actually taken from Rice, 1989. Whitehead comments on this: "The oldest age estimates from Rice probably under-estimate the longevity (of sperm whales), perhaps substantially." The reasons for Whitehead's assessment of the situation are clear and indeed, obvious: "How long do sperm whales live? I am afraid that, once again, our ability to answer a question about a population parameter is limited by aspects of the whaling industry and the science based on it, as well as by the constraints of modern studies. Many of the sperm whales that lived in the second half of the twentieth century were killed by humans, but scientists began estimating ages, using tooth layering, only towards the end of the catch, by which time most of the older animals had probably been killed. Also, the closure of the pulp cavity within the tooth as animals age makes it hard to discern some layers, so that older animals cannot be accurately aged." The same can be said about the figures for maximum ages of baleen whales and age determinations from their earplugs.

As to the sizes of sperm whales, all the figures for the ***average*** lengths of males cited by Whitehead, from a compilation by Letteval *et al* [31] (of which he was a co-author), from a number of study locations, with data not coming from whaling operations, are considerably larger than in Klinowska's compilation of ***maximum*** lengths, and range from 13 to 15 m in four areas. The difference between a length of 12 m and one of 14 m, in terms of weight, is +60%. Furthermore, Whitehead points out that male sperm whales are three times the weight of females, in contrast with the less than double given by Trites and Pauly. In this case, seeing that as far as we know the adult sex ratio in sperm whale populations is ***not*** 50:50 - especially in populations that have been intensely exploited - it makes little sense for T&O to have assumed approximately 50:50 in taking the simple average of two (highly questionable) sex-sizes as the figure from which to estimate food consumption in the Northern Hemisphere.

9 Food consumption, T&O Method 3

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[31] Letteval, E. C. *et al* (2002) "Social structure and residency in aggregations of male sperm whales" *Can. J. Zool.* **80**:1189-96.

We need spend little time in examining this Method. S. K. Klumov published, in 1963[32] a paper in which he reported that he had found that daily food consumption by a baleen whale is 3.5% of its body weight. But there is no experimental or theoretical support for a relationship of simple proportionality between body weight and food consumption or metabolic needs, either in cetaceans or, indeed among warm bloodied animals such as mammals and birds, or even among fishes. Quite the contrary; there is overwhelming theoretical and observational evidence that such relationships are power functions with an exponent substantially less than unity, typically about 0.75. Naturally, use of the Klumov equation for Method 3 gives greatly inflated estimates of food consumption or metabolic needs of very large animals, and T&O's results from use of this method must be rejected at the outset.[33]

10 Food consumption T&O Method 1

Regarding their Method 1 T&O assert that "Innes et al (1986) proposed (that):

I=0.42w^0.67, I being expressed in kg/day and w in kg. **(r^2 = 0.81) "**

This formula is identical to that used by S&V in their 1998 study of whales in Icelandic waters. The use of this equation was also proposed by Armstrong and Siegfried (1991).[34] The short paper by Innes *et al* [35] was written only to demonstrate that D. E. Sergeant's assumption that cetacean food consumption is proportion to the weight of the animal, or of a particular organ – the heart in his case - is disproved by his own data.[36] Sergeant had figures for feeding rates and body weights of 28 animals. These pertained to 10 species (8 Genera) of "small" cetaceans, all odontocetes ranging in size from about 80 kg to 1300kg, all captive animals that has been fed entirely on fish, presumably dead ones. His data are grouped by species, with the smallest (harbour porpoise – 8 animals) and the largest (killer whale – *Orca*, 4 animals) being the most numerous. Some of the data used came from a literature search, others from unpublished

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[32] Klumov, S. K. "Feeding and helminth fauna of whalebone whales (Mystacoeti)" *Trudy Inst.Okeanol*. **71**:94-194.

[33] R. Leaper and D. Lavigne reviewed this matter in a paper to the IWC Workshop held in June 2002: "Scaling prey consumption to body mass in cetaceans" *IWC Doc.* SC/J02/FW2, 2002. That year I. L. Boyd published a review suggesting a value for the power parameter - 0.524 - even lower than the lowest of the three values used by Tomura and Ohsumi, and other authors. (Boyd, I. L.,2002. "Energetic consequences for fitness". *In Marine Mammal Biology: an evolutionary approach. Ed* A. R. Hoelzel. Blackwell Science, p432). Leaper and Lavigne pointed out that for a forty tonne whale a estimates by Method 3 and by Boyd's equation can differ by a factor of 10, and even if adjustments are made to standardise the results to a 2 tonne whale by a factor of 3 or 4.

[34] A. J. Armstrong and W. R. Seigfried "Consumption of Antarctic krill by minke whales". *Antarctic Science* **3** (1):13-8. S&V acknowledged this.

[35] Innes, S., Lavigne, D. M., Earle, W. M. and Kovacs, K. M (1987)." Feeding rates of seals and whales". *J. Animal Ecology* **56**:115-30.

[36] Sergeant, D. E. (1959)" Feeding rates of Cetacea" *Fiskeridiektoratets Skrifter*, Serie *havunderskoleser* **15**:246-58. *and* S. Innes, D. M. Lavigne, W. M. Earle and K. M. Novacs (1986) "Estimating Feeding Rates of marine mammals from Heart Mass to Body Mass Ratios" *Mar. Mammal Science* **2**((3):227-9.

correspondence with aquarium operators and research staffs. Sergeant observes that "*Least* information (my emphasis – *sjh*) was obtained for body weight, since curators are loath to weigh living animals, especially those of larger size, and estimates of weight are usually too inaccurate to be useful." However, an independent search of literature and unpublished records of lengths and weights of whales allowed him to construct curves from which weight could be estimated from body length; this information was useful for the killer whale for example". (In other studies concerning cetacean feeding rates, the body weights have occasionally been estimated from such measures as the size of residual bones or meat or blubber yields in commercial operations, but more often by calculating from a curve of weight against length.)

Innes *et al* used the common procedure of estimating the exponent and the multiplier from Sergeant's data by a minimum least squares linear regression of log ***I*** on log ***w*** They did not report exactly how they had done that but I have repeated their estimation and find they fitted the log-log equation to the grouped data, *not weighted* by the size of the "sample" for each species. They noted that their value "of the exponent, 0.67, was not significantly different from the expected value of 0.75 for ingestion rates of marine mammals and mammals generally."[37]

Sergeant presented his data only grouped by species, not for individual animals. I have made the same analysis as Innes *et al* but weighting each point by the number of specimens for each species. This gives

I = 0.313.w^**0.713** (**r**^{2} = 0.93)

There may be two reasons for treating, in a regression, the weight as the independent variable (x) and the food intake as the dependent variable (y), one being that one wishes only to predict ***I*** given a known value for ***w***, the other being that the weight really is a known quantity while the intake is estimated. We have seen that, at least in Sergeant's data – but, as we shall see, most other information available about cetaceans, especially large ones - the weight is only very roughly estimated, and often merely guessed. In any case, however, if we want to try to determine the form of the actual relationship between the two variables another approach is called for.

Possible approaches have been discussed by J. M. V. Rayner, 1985 and W. E. Ricker, 1973.[38] One is to calculate both the regression of y (***I***) on x (***w***), and the regression of x on y. The estimated slope in the second case can then be transposed to a straight line for which values of w lie along the x-axis and of I along the y-axis. From Rayner's theoretical analysis the bisector of the two regressions (arithmetic mean of the slopes) or, alternatively, the geometric mean

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[37] Farloe, J. D. (1976) "A consideration of the trophic dynamics of a late cretaceous large-dinosaur community (Oldman formation)" *Ecology* **57**:841-57.

[38] Respectively, "Linear relations in biomechanics: the statistics of scaling functions" *J. Zool. London (A)* **206**:415-39. and "Linear Regressions in Fishery Research" *J. Fish. Res. Bd Canada* **30(**3):409-34. P. E. Schmid, M. Tokeshi and J. M. Schmid-Araya (2000) "Relation Between Population Density and Body Size in Stream Communities" *Science* **289**:1557-60, discuss this, and draw their general conclusions from the "bisector" slope. See also P. A. Marquet "Invariants, Scaling Laws, and Ecological Complexity" *Science* 2000) **289**:1487-8.

of the slopes, best estimate the actual relationship between the two variables. From Sergeant's data, and using the same weighting factors, I obtained

$\boldsymbol{I} = 0.276w^{\wedge}\mathbf{0.}74$ [I refer here to the constant = 0.276 as the ***scaling constant***]

that is, with an ***exponent*** very close to the value 0.75 expected from theory.
The question then is whether it is reasonable to extrapolate such a regression, based on rough observations for captive fish-eating animals ranging from 40 to 1300 kg body weight, with an average weight of 385 kg, to a mainly cephalopod-eating species – the sperm whale – more than fifty times that average, and even to mainly krill- or copepod-eating baleen whales up to 250 times bigger. Here we are faced with the problem not only of extrapolating two orders of magnitude beyond the data, but also the matter of the different types of diet. For example, the energy-content/kg ("energy density") of some fishes is more than double that of some invertebrates, such as krill and copepods. Lavigne (1996) concluded that the energy densities of prey species of marine mammals can vary by a factor of three.[39] Using data from Lockyer (1987) and Steimle and Terranova (1985), S&V (1998) – see later - used a factor of 1.4 for the ratio of the energy densities of fish and cephalopods to that of crustaceans.[40]

So, extrapolation of regressions far outside the range of the original data, using an estimated exponent value, carries with it the additional uncertainties concerning qualitatively very different diets, which would be reflected in a changing value for the scaling constant ***m***. It may also be questioned how useful are observations of feeding rates of captive animals in predicting the feeding rates, even of the same species, in the wild. It has been noted that captive animals obviously expend less energy in obtaining their food than foraging wild ones. On the other hand it has also been noted that cetaceans and other animals in captivity are commonly over-fed, and to complicate matters are sometimes artificially stimulated to exercise for the sake of their health. What is clear is that the overall estimates given by T&O, based on the Innes *et al* analysis of Sergeant's data, and for the above reasons alone, could be astray by a factor of several-fold.

To judge the effect of my reanalysis of Sergeant's data I have calculated the ratios of projected food consumptions per day from my revised equations (the geometric mean of the weighted regressions of I on m and m on I) to those from Innes *et al* and used by T&O, for various sizes of cetaceans. The smallest dolphins would be judged to be eating a little *less* by my equation, and the

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[39] "Ecological interactions between marine mammals, commercial fisheries, and their prey: unravelling the tangled web" pp59-71 *in* W. A. Montevecchi (Ed.) "Studies of high-latitude seabirds. 4. Trophic relationships and energetics of endotherms in cold ocean systems" *Occ. Pap.* **91**, Canadian Wildlife Service. Lavigne's paper is a key analysis of the problems of determining intake rates, among other matters pertinent to the T&O enterprise.

[40] C. Lockyer (1987) Evaluation of the value of fat reserves in relation to the ecology of South Atlantic fin and sei whales. *In* Approaches to Marine Mammal Energetics. A. C. Huntley, D. P. Costa, G. A. J. Worthy and M. A. Castellini (*eds.*) Society for Marine Mammalology *Special Publication* **1**:183-203. F. W. Jr Steimle and R. J. Terranova (1985) Energy equivalents of marine organisms from the continental shelf of the temperate Northwest Atlantic. *J. Northw. Atl. Fish. Sci.* **6**:117-24.

bottlenose whale somewhat *more* (+7%). However, the difference gets bigger for larger whales, so that for an 80 tonne blue whale it would be 20% *more*. This is without making any adjustments, in either case, for a different average energy density of the dietary items as between the smaller toothed whales and the larger baleen whales. The differences are obviously quite substantial.

I conclude this section with some observations on the matter of adjustments for differences in energy density of food intake, bearing in mind the comment made in several places in the scientific literature on this matter that energy from stored lipids and "excess" muscle are probably the decisive measures for baleen whales. I shall return to this question in detail when I look at the application by T&O of their Method 2. But here the problem is of extrapolating food intake rates of smaller, captive, exclusively dead-fish-eating cetaceans to very much larger baleen whales which everywhere subsist mainly on zooplankton having very different nutritional characteristics. T&O made no such adjustments although, following S&V, they necessarily did so in applying their Method 2.

Although Brodie, Kleiber and others have repeatedly found that metabolic rate can be expressed, at least to a good first approximation by a power function spanning a broad range of species, it does not follow that a similar function can be applied to mass food intakes over such a range. In the case of the extrapolation of Sergeant's data, with no adjustments for food type ("quality") as between the lower and upper ends of the range of body weight, there is a contradiction: if the simple unmodified power function can be extrapolated validly up to the large baleen whales, then the data and the predicted values could not be fitted by a similar function in terms of energy requirements/metabolic rates.[41]

In their application of Method 2 T&O relied on the studies of energy content of various prey species by F. W. Steimble and R. J. Terranova, 1985[42] (as did S&V) and also on that by A. Clark, 1980 [43]. Clark was concerned only with Antarctic krill, and I shall come to that later, but Steimle and Terranova report a very wide range of analyses of energy density, including both their own and the published results of others. They summarise data for sixty species of teleost ("bony") fishes, one euphausid common in the North Atlantic, five species of amphipods (some of which are eaten by baleen whales) and cephalopods, some of which occasionally find their ways into the guts of baleen whales – presumably by accident!

One observation by the above authors is that the energy content per kg wet weight is much more variable that the content per kg dry weight, which is in turn more variable between similar species than is the energy content of the

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[41] The properties of power functions are of great general importance in biology as well as in most other scientific disciplines. An excellent non-mathematical but comprehensive account of this has been given by Mark Buchanan (2000) "Ubiquity: the science of history, or why the world is simpler than we think" 230 pp. Weidenfeld and Nicolson, London. See also "Critical Mass: How one thing leads to another" by Philip Ball, Arrow Books, 2005, 644pp.

[42] "Energy equivalents of marine organisms from the continental shelf of the temperate Northwest Atlantic" *J. Northw.Atl. Fish. Sci.* **6**: 12-24.

[43] "The biochemical composition of krill, *Euphausia superba*, DANA, from South Georgia". *J. Exp. Mar.Biol. Ecol.***43**: 221-36.

same species expressed per kg ash-free dry weight. This is mainly because the water content of the species is variable. Here is an illustration of how dramatic such difference can be. They give (from other literature) a value for the energy density of "zooplankton" as 1.64 (they express these densities as kilojoules per gram (KJ/g); of "pelagic malacostraca" - which are probably mainly amphipods, which occur in diets of baleen whales - as 4.1KJ/g; and of pelagic fishes as in the range 6.1-8.1 KJ/g. That is, such fishes have an apparent nutritional value, at least in energy terms, up to four times that of zooplankters. But the corresponding figures for ash-free dried weight were in the ranges, respectively 23.1-27.6, 20.0-31.5, and 23.6-25.1. Evidently the conversions of energy to food bulk have to be made carefully, but in this case - since we are looking at predictions of bulk food intake - it would seem to be appropriate to increase the figures extrapolated from Sergeant's data to baleen whales up to fourfold.

I now summarise my conclusions about Method 1:

1. Sergeant's data, on which it is exclusively based, are entirely for feeding dead fish to small captive cetaceans, and very few even of those. Some of those data were from scientific publications, others from hearsay. The actual weights of the animals used were unknown, and guestimated, in some cases, by very dubious methods. Even the calculations from body length are questionable. Nevertheless, within this small group of similar predatory animals fed on essentially the same diet, the intra-specific relationship of intake with body-weight, usually associated with metabolic studies, holds good, with a value for the exponent of the power function close to the expected value of 0.75. It is however, unclear whether those data can validly be applied to predict the food intake of foraging wild animals **even of the same species**.

2. Most scientists are wary of predictions based on extrapolations of regressions beyond the ranges of the actual data. They are especially wary of extrapolating power functions, something the hazards of which some of those currently looking at food consumption by marine animals seem to be unaware. Extrapolations by several orders of magnitude, to animals that are entirely different ecologically would usually not only be judged invalid but considered to be reckless, even absurd. If, in addition, no account is taken of the very different nutritive values of the dietary constituents then the absurdity is compounded.[44]

11 T&O Method 2. Energy needs

When introducing their Method 2, T&O write that S&V, 1998 [45] "proposed a method for estimation of daily prey consumption from the standard metabolism

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[44] It would be fair to note here that S&V, whose methods Tamura and Ohsumi copied, also used questionable data uncritically. Their concluding words were "The results show that the total biomass consumed (by cetaceans around Iceland) is substantial, or more than three times the total landings of the Icelandic fishing fleet."

[45] J. Sigurjónsson and G. A. Vikingsson "Seasonal Abundance of and Estimated Food Consumption by Cetaceans in Icelandic and Adjacent Waters" *J. Northwest Atl. Fish. Sci*. **22**: 271-87.

of each cetacean species." Again, as with the reference to Innes *et al* concerning Method 1, this is misleading. S&V used two methods. The first was exactly T&Os's Method 1 which we have evaluated above. Their second was derived from a paper published by C. Lockyer in 1981 (not referenced by T&O), one of two papers she published that year. There, and in her second review paper[46], Lockyer deduced values for "near basal metabolic rates" of whales from consideration of their lung capacities and therefore *presumed* oxygen intake and usage.[47]

To find the origins of the equation used by S&V, and also by T&O, we need to look back to Lavigne's 1996 paper. He proposed, based on the extensive studies by M. Kleiber,[48] that on an *annual* basis, the average daily metabolic rates (ADMR, expressed as kcal/day[49]) of individual marine mammals, could be approximated by

$$\mathrm{ADMR} = \boldsymbol{\beta}\,(70.\boldsymbol{w}\text{\textasciicircum}0.75)$$

where the function within the parentheses is "an estimate of the basal metabolic rate (BMR in kcal/day), and $\boldsymbol{\beta}$ typically takes a value between 1.0 and 3.0, depending on such things as age, sex, reproductive condition, activity and species involved." Lavigne's formula applies equally to aquatic and terrestrial mammals. The ADMR should approximate the average field metabolic rate (FMR), and is a simple multiple of BMR.[50]

S&V chose to adopt an exponent value of 0.783 instead of 0.75. (They expressed energy in kiloJoules rather than kilocalories, so Lavigne's "70" became their "293.1"). However, they did not, as one should expect, otherwise adjust the multiplier accordingly. Hence their intake estimates would be higher, especially for larger than average species, than those that would have been obtained from the Lavigne expression. S&V also *assumed* "an assimilation rate of 80%". This apparently means that the actual intake would have to be 25% higher by weight than would have been required to meet metabolic needs if the assimilation rate was 100%. Further, they *assume*d "an activity coefficient" of 1.5. Although they

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[46] C. Lockyer "Estimation of the energy costs of growth, maintenance and reproduction in the female minke whale (*Balaenoptera acutorostrata*) from the southern hemisphere, south of 40° S." *Rep. Int Whal. Com*mn **31**: 337-43.

[47] Other biologists have proposed and used different methods of estimating food consumption, from metabolic rates and inter-specific comparisons, that might in principle be applicable to cetaceans but involve much the same problems of extrapolation from limited observations. W. F. Humphreys, 1981 "Towards a simple index based on live-weight and biomass to predict assimilation in animal populations". *J. Anim. Ecol.* **50**: 543-61. ('Assimilation' is used here to mean intake, consumption. *sjh*)

[48] M. Kleiber (1975) "The Fire of Life.– an introduction to animal energetics". 2nd edition, Wiley, New York, and R. E. Krieger Publ. Co.Huntingdon, NY. See also D. M. Lavigne *et al* (1986) "Metabolic rate – body size relations in marine mammals". *J. Theor. Biol.* **122**: 123-4; and Lavigne *et al* (1986) "Metabolic rates of seals and whales". *Can. J. Zool.* **64**: 279-84.

[49] Kcal/day = 0.04843 Watts = 0.04843 Joules/second = 4.184 kJ/day.

[50] In his paper Lavigne answers some criticisms made of the assumption of proportionality.

do not define this, it appears to be the β of Lavigne's formula, lying appropriately between 1 and 3.

Kenny *et al* (1997), in their study of prey consumption by cetaceans feeding over the continental shelf of the Northeastern United States, also assumed an assimilation rate of 80%, though they refer to this as "assimilation efficiency". But, in contrast with S&V, they assumed a value for β of 2.5, with consequent much higher estimates of consumption of biomass.[51]

This question of the assimilation rate provides an intriguing demonstration of Boyle's *bons mots* that "measurements have a monstrous life of their own". T&O attribute their assumption of an 80% assimilation rate to S&V, giving the impression to readers that this figure comes from data. But for S&V it is an assumption that *they* attribute to Kenny *et al*, 1997. But Kenny *et al*, in their turn, *assumed* the 80% value, attributing it to Lockyer's three papers - one dated 1978 and two 1981. So, from where did Lockyer get this figure? It is at last clear, from her 1978 paper. She cites a number of publications concerning the body composition of krill and other small crustaceans. One found that "krill chitin constituted 26.4% of the wet weight" although Lockyer thought this was unreliable as the dissection of the animal had not been carried out correctly. Another reported, she writes, "that in *Meganychtiphanes norvegica* the indigestible portion of ash and chitin constitutes about 20% of the dry weight." But then, she observes, "By comparison with other euphausids the actual chitin content of krill (she is referring to *Euphausia superba)* is probably far less than 10% of dry weight, especially in larger krill of 30 mm in length upward. The krill is a far larger euphausid than the other species mentioned here; the overall indigestible or non-utilisable fraction in terms of energy assimilation is about 20% of the dry weight or approximately 4.4% (of the) wet weight. Whole krill would therefore consist on average of about 78% water, 5% ash and chitin, 4% fat and 13% protein." Later, Lockyer writes "…an extremely high assimilation efficiency might be expected in animals feeding on krill…" Nevertheless Lockyer made the infamous 80% assumption – calling it just that, an assumption – in one of her 1981 papers concerning the minke whale.

So, even if the larger baleen whales that Lockyer was mainly studying and reporting on in the late 1970s could not digest chitin, their assimilation efficiency – at least when feeding on krill – could well have reached 95%. Since we know that minke whales, at least, *can* digest chitin, by virtue of the bacteria in the forestomach, there is no reason, from all these data to assume that the assimilation efficiency is significantly less than 100%, and the arbitrary inflation of consumption by 25%, merely to take account of "inefficiency", is spurious.

Other authors have recently used variants on the above method. Thus Trites *et al* (1997) [52] rounded up the exponent value of 0.78 (from Lavigne *et al*, 1987 and from Lockyer, 1981), to 0.80, and adopted a scaling constant of 0.1 to give

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[51] R. D. Kenny, G. P. Scott, T. J. Thompson and H. E. Winn (1997) "Estimates of prey consumption and trophic aspects of cetaceans in the USA Northeast continental shelf ecosystem". *J. Northw. Atl. Fish. Sci.* **22**: 155-72.

[52] A. W. Trites, V. Christensen and D. Pauly (1997) Competition between fisheries and marine mammals for prey and primary production in the Pacific Ocean. *J. Northw. Atl. Fish Sci.* **22**: 173-88.

estimates of intake in kg wet biomass. But S&V, using an exponent value of 0.783 and a factor for conversion of energy to (wet) biomass of about 930 kcal/kg for crustaceans and 1300 for fishes, came up with a scaling constant of 206.25 for energy. This has within it their adjustments for assimilation efficiency - +25% - and for an excess of active (field) metabolic rate over Lockyer's "resting metabolism" (which S&V refer to as "near basal rate") - +50%. (Their estimates would be close to those of Trites *et a*l if the latter implicitly assumed assimilation efficiency to be 100% and the FMR to be only a little higher than BMR). T&O used this form of calculation but with slightly different assumptions.

T&O's calculations are, as far as energy intake is concerned, identical with those of S&V, and contain the same assumptions. To convert to wet biomass intake they use different factors. S&V assume that crustaceans in the diet have an energy density of 930 kcal/kg, and that fishes and cephalopods provide 1300 kcal/kg. T&O assume energy densities of the diet of baleen whales (presumably mainly crustaceans) in the SHIO to be 1110 kcals/kg, and for baleen whales elsewhere and toothed whales everywhere 1300 kcal/kg. This last figure comes, in both cases, from Steimle and Terranova (1985). The figure 930 is from Lockyer (1987), the 1110 figure from Clark (1980).[53] The figures 930 and 1110 can be taken as two fairly close estimates of the energy densities of several species of euphausids, copepods and, presumably, some amphipods. However, T&O's adoption of 1300 kcal/kg for baleen whales throughout the Northern Hemisphere, from the Steimle and Terranova paper, implies that all those whales eat only fishes, which is, of course, nonsense!

Before looking more deeply into the process of converting energy expenditure to intake biomasses we need to inquire into the origins of the equation relating near basal energy to body weight, and of the adjustment ratios. Lockyer's Fig 44, in the published version of her paper to the FAO Bergen Conference in 1976, contains essentially two different sets of figures. One set, at the lower end of her body weight range, is culled from an assortment of published papers, not all of which actually used comparable methods to express rather different definitions of "metabolic rate". She did not tabulate the data but a glance at her graph shows that the correlation between the logarithm of the metabolic rate and that of the body weight is low. The figures in the upper part of her graph she derived from her calculation of respiratory rates. This calculation involves an estimate of lung capacity, an account of the respiratory cycle (number of breaths, breathing duration and dive duration), an **assumption** that only 10% of air inhaled is utilised, and an **assumed** Respiratory Quotient (RQ) of 0.82. The data she presents pertain to one blue whale (calculated body weight 122 tonnes), two fin whales (calculated weights respectively 50 and 72 tonnes), three sperm whales (for one of which she notes "lung capacity very dubious" but nevertheless plots it on her graph and apparently included it in her fitted regression) and what is designated as a "hypothetical bottlenose, which is also included in the plot and apparently used in the regression.

Apart from the "hypothetical bottlenose" there is no correlation among these derived data points. The graph also shows values for two grey whales, of unstated provenance and validity. **The slope of the regression illustrated is**

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[53] A. Clark (1980) "The biochemical composition of krill, *Euphausia superba* DANA from South Georgia". *J. Exp Mar. Biol*. **43**: 221-36.

generated entirely by the difference between the lower cluster of smaller animals, from the literature, and the higher cluster of Lockyer's estimates from extremely limited information about breathing.

Now let us look at the "adjustments" to the constant, 110, in Lockyer's equation. Lockyer wrote "The energy content of the food ingested is generally only partially utilized. Only a portion of the energy consumed is assimilated, the remainder being in an indigestible form which is rejected from the body." (She continues to discuss possible differences between juveniles and adults in this matter, but we need not concern ourselves with that here because neither S&V nor T&O used different assimilation rates to allow for particular age-distributions in the populations to which they would apply such information.)

Later in her paper Lockyer writes that "blue and fin whales feed at an average rate of 35g/kg body weight/day for 120 days in the Antarctic summer, and at an average reduced 10% of summer rates of the order of 3.5g/kg body weight/day for the remaining 245 days (but whales may not feed at all during migration)". She then converts those quantities to energy from krill, using a conversion factor of "1 kg wet krill = 1000 kcal". And then: "The unobtainable energy in food is trapped in indigestible tissues. In krill and crustacea generally most of the unobtainable energy is trapped in the chitinous integuments…at least 5% of wet weight or **20% of dry weight of krill is in the unutilizable form so that about 80% efficiency of assimilation is unlikely to be greatly exceeded.**" That is the source of, and only "evidence" for, the adjustment used by S&V, and copied uncritically by T&O. Notably, there are no observations reported of faeces. Elsewhere Lockyer attempted, unsuccessfully, to derive information about assimilation efficiency from her figures for feeding rate (based on mouth volume, frequency of meals etc.) and fattening rates of feeding whales.

The idea that baleen whales cannot assimilate chitin derives from an assumption that their stomachs do not secrete appropriate enzymes. But Norwegian scientists have shown that minke whales are capable of digesting practically everything they eat, especially since "they depend to a great extent on microbial fermentation in a multi-stomach system.[54] (Thus) findings from seals have little application for baleen whales. This probably is particularly true when it comes to digestion of crustaceans, since we have shown that minke whales are rich in chitinase (EC 3.2.1.14)-producing microbes in their forestomach."[55]
Nordøy, 1995 [56] , noted that "A large proportion of the dry matter of many polar krill species consists of wax esters…which are known to be poorly digested by many mammals, including humans." He therefore undertook a study "to determine the efficiency of wax ester digestion in minke whales and thus

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[54] The anatomy of this system was described in detail by H. Hosokawa and T. Kamiya, 1971, in *Sci. Rep. Whales Res, Inst.* **23**. They concluded that "The cetacean stomach is an exceptional case in the animal kingdom" and although chambered is structurally and functionally quite different from that of the terrestrial ruminants.

[55] Nordøy, E. S., Sørmo, W. and Blix, A. S. (1993) "In vitro digestibility of different prey species of minke whales,(*Balaenoptera acutorostrata*) " *Brit. J. Nutrition* **70**: 485-9. Mathiesen, S. D., Aagnes, T. and Sørmo, W. "Microbial digestion in minke whales (*B. acutorostrata"). IWC Doc SC42/NHMi9*. 1991

[56] Nordøy, E. S. "Do minke whales (*Balaenoptera acutorostrata*) digest wax esters?" *Brit. J. Nutrition* **74**: 717-22.

examine whether these marine animals have developed an adaptation to utilize an energy source poorly utilized by terrestrial mammals." His analyses showed that the very high DMD (dry-matter digestibility) of 94%, and the occurrence of fatty alcohols - one of the products of wax-ester hydrolysis, in faeces - confirm that minke whales are very efficient digesters of wax esters and absorb most of the energy-rich products of this process.

Olsen *et al*, 1994,[57] studied the digestion of herring (*Clupea harengus*) by minke whales. They found that in addition to the usual enzymic digestion in the second chamber of the multi-chambered stomach (common to all baleen whales) digestion began and was largely completed in the forestomach by indigenous symbiotic – mostly lypolytic – bacteria. "Scanning electron microscopic examinations revealed large numbers of bacteria, surrounded by a glycocalyx, attached to partly digested food particles in the forestomach." In comparison with ruminants (cattle etc) in which microbial digestion is necessary for the breakdown of cellulose, the baleen whales face the problem of breaking down proteins, lipids and polysaccharides which can all be downgraded by fermentation involving resident bacteria.

Olsen *et al* cite other authors' similar findings for other baleen whales. They also searched for the anaerobic bacteria that are known to break down chitin in minke whales and presumably other species of baleen whales. They found rather few in the herring-eating animals they studied. But they offered the interesting suggestion that such bacteria would multiply and become effective digesters if and when the whales switch prey and start feeding on krill. These authors concluded that the whales' absorption efficiency is as high as 92%. In ruminants digestion by symbiotic microbes is accomplished by loss of some energy as ammonia and methane. Presumably there are similar by-products with residual energy from microbial digestion in the baleen whale forestomach, which could be released via the anus or perhaps in exhaled air (Might this account for the common reports of "bad breath" of the whale's blow?)

These studies, and others I have not cited, all indicate that it is unreasonable to suppose that the assimilation efficiency of baleen whales is as low as 80%; it is likely close to 100%.

I turn now to the matter of relating basal and/or resting metabolism to active metabolism, however they might be defined. S&V attribute their adjustment figure for what Lockyer called "swimming about", and they call "activity coefficient" to a paper by Overholtz *et al*, 1991[58] and another by R. K. Hinga, 1979.[59] Overholtz *et al* guessed an activity coefficient of 1.5 to multiply up values of **basal** Metabolic Rates, whereas S&V applied this to an equation for **resting** rates, defined by Lockyer as "while swimming from place to place or feeding"

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[57] Olsen, M. A., Aagnes, T.H.and Matheisen, S. D. "Digestion of Herring by Indigenous Bacteria in the Minke Whale Forestomach" *Applied and Environmental Biology* **Dec, 1994**: 4445-55.

[58] Overholtz, W. J., Misaki, S. A. and Foster, K. L. "Impact of predatory fish, marine mammals, and seabirds on the pelagic fish ecosystem of the northeastern USA". In "Multispecies models relevant to management of living resources", *eds* N. Daan and M. P. Sissenwine, *ICES Mar. Sci. Symp*. **193**: 198-208.

[59] "The food requirements of whales in the Southern Hemisphere." *Deep Sea Research* **26A**: 569-77.

and as "total energy expenditure", which are already 65% above basal rate! So from where came Overholtz *et al*'s guess? They cite Henga, and also R. D. Kenny et al, 1983.[60] I have not seen Kenny *et al's* 1983 paper but the relevant information is given in what is in effect the published version of that, Kenny *et al*, 1997 [61] which also cites the Hinga paper. These authors note that "Active metabolism in cetaceans has been estimated at approximately 2 to 5 times **basal** MR", and "A low value of 2.5 times BMR was chosen at near the bottom of the range in order to scale for active metabolism." Discussing, specifically, "Animals which do not feed, or feed at significantly lower rates during migration and/or on their wintering grounds" (i.e. most of the baleen whales) and noting that for most species there is very little information about their distribution and behaviour during the winter, a relatively low value" (of the ratio of active to basal metabolism rates) "was used here of 1.2 times. This factor was applied only to baleen whales, and only during spring, summer and autumn." Kenny *et al* applied it to estimates of basal metabolism from Kleiber's equation.

So, while Kenny and his collaborators scaled up basal rate of migratory baleen whales to active rate by 20%, S&V, followed by T&O, scaled up by 50% above "resting" rate, which was already considered by Lockyer to be 65% above basal rate; a total of nearly 250%.[62]

Before leaving this subject it is worthwhile looking briefly at some other estimations of cetacean food consumption and the uses to which these have been put. Stefánsson *et al*, 1997,[63] in developing a model for estimating consumption of various dietary items by minke whales and other cetaceans, use the first method of S&V (coming from Sergeant's data on small captive odontocetes), but not the second method, based on assumptions about energy needs. They did not explain why they made that choice but presumably did so because S&V did in applying their results to obtain their estimates of total consumption; they noted that the first method gives somewhat lower totals than the second one: 15.1 million tonnes as against 15.7 million. However, the estimated consumptions by baleen whales differ in the reverse way, being 30% higher for the second than for the first method.

Tjelmeland and Lindstrom, 2005[64] were concerned only with the minke whale and thus not in need of inter-specific adjustments to take account of body weight differences. But they did wish to take into account possible consumption differences between mature and immature animals (estimated average body

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[60] Kenny, R. D., Hyman, M. A. M. and Winn, H. E. "Calculation of standing stocks and energetic requirements of the cetaceans of the Northeast United States outer continental shelf." Final Report to NMFS in fulfilment of contract NA-83-FA-C-06009 October 1983, University of Rhode Island.

[61] Kenny, R. D., Scott, G. P., Thompson, T. J. and Winn, H. E. "Estimates of Prey Consumption and Trophic Impacts of Cetaceans in the USA Northeast Continental Shelf Ecosystem" *J. Northw. Atl. Fish. Sci.* **22**: 155-71.

[62] The constant in the metabolic equation used by S&V, and by T&O,

[63] G. Stefánsson, J. Sigurjónsson and G.A. Vikingsson (1997) "On Dynamic Interactions between Some Fish Resources and Cetaceans off Iceland Based on a Simulation method." *J. Northw. Anl. Fish. Sci.* **22**: 357-70.

[64] S. Tjelmeland and U. Lindstrom (2005) "An ecosystem element added to the assessment of Norwegian spring-spawning herring: implementing predation by minke whales. "*ICES J. Mar. Sci.* **62**: 285-94.

weights 4800 and 3800 kg respectively), and they used a more complicated estimation procedure. This originated with Lindstrom *et al*, 2002, and Folkow *et al*, 2000[65] and requires information about loss of input energy in faeces and urine, additional energy required by immature animals, ("growth factor"), energy deposition in blubber, muscle, visceral fat and foetus growth, as well as the usual energy densities of prey, and the relative abundances in the whale stomachs of different types of prey. Although this is an interesting attempt to construct a more plausible metabolic model the reality is – as the authors admitted – that there were few data to provide parameter estimates to put in the model and, inevitably, the output was coming largely from guessed values.

Folkow *et al* did not have data for the body weights of minke whales and instead estimated them from body lengths (≥ 8 m equivalent to an average of 4800 kg). They also assumed that the daily energy expenditure by these mature animals was 80 kJ/kg (i.e. 470,000 kJ/day), and 445,000 kJ/day for the smaller but growing immature animals. In their case too, it is evident from their long explanations, that they were building a large super-structure on weak foundations – and they realised that.

A common feature of all these estimation procedures is an assumption that application of a power function (*e.g.* describing the relation between body weight of an individual animal and it's food consumption to a group or groups of animals with specified mean weight(s) but with wide range(s) of individual sizes, is a valid procedure. It is, evidently, valid if the exponent is unity or close to it, and may give a fair approximation for fairly high values of the exponent – e.g. 0.8 – but is increasingly biased as the exponent is lower. This and other biases and uncertainties identified here might not in themselves - even in combination - unacceptably bias the big picture, but they certainly do contribute to the uncertainties of the final global figures.

To obtain their figures for annual consumption T&O multiply their estimates of daily rates by 365. This may at first sight seem inappropriate, especially for the highly migratory baleen whales that feed only for a part of each year. However, if the ADMPs are what they should be – true averages - then the multiplication is appropriate; the whales would be expected to consume much more than the ADMP during the intensive feeding season when reserve fat is being laid down, and much less in the longer period when they do not feed or perhaps do so only occasionally and not continuously. S&V, who wished to look at the seasonal feeding behaviour, did in fact do that but they adjusted the conversion multipliers in such a way that the resulting annual totals were exactly what they would have been if the average had simply been multiplied by 365.

I conclude that, in Applying Method 2 it is not correct to increase the constant, 110, in the already very shaky Lockyer equation either by 50% to account for "activity" or by 25% to account for unassimilated energy Thus the use of the

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[65] L. P. Folkow, T. Haug, K. T. Nilssen and E. S. Nordoy (2000). "Estimated food consumption of minke whales (*Balaenoptera acutorostrata*) in the northeast Atlantic waters in 1992-1995." *NAMMCO Sci. Publ.* **2:** 65-80. and U. Lundstrom, T. Haug, and I. Røttingen (2002) "Predation on herring *Clupea harengus* by minke whales *Balaenoptera acutorostrata* in the Barents Sea." *ICES J. Mar. Sci.* **59**: 58-70.

value 206.25 by S&V, and by T&O inflates all estimates of food consumption by Method 2 by 88%, i.e. nearly doubling.

Finally, in this section, it is of interest to consider the results of minke food consumption studies reported by T. Tamura, T. Ichii and Y. Fujise.[66] They carried out metabolism research as part of the JARPA programme of lethal research under Special Permits in the Antarctic. They tried three methods: first, looking at the diurnal change in the weight of food in the forestomachs of the whales; second, by calculations like T&O's Method 2, using the Kleiper equation and applying the mean weights of whales in the Permit catches which, they write, were "collected at random". They made similar assumptions to those discussed earlier, including the assumption that assimilation efficiency was only about 80% (They actually used the value 84%, declaring it too came from Lockyer, 1981.) A new feature was that the whales were actually weighed, and also separated by sex, and distinguished as immature or mature.

Tamura *et al's* results are summarised in Table 2 below.

TABLE 2. Daily food consumption as % of body weight
(From Tamura *et al*, 1997)

Sex & state	Number in sample	Average weight (tonnes)	Daily consumption (% body weight)	Daily consumption (kg per animal)
Immat. males	243	3.00	4.0 – 4.4	120 - 132
Immat. females	371	3.90	3.9 – 4.1	152 - 160
All immatures	614	3.54		139 - 149
Mature males	1145	6.90	3.3 – 3.5	228 - 242
Mature females	603	8.10	3.8 – 4.0	308 - 324
All matures	1748	7.31		256 - 270
All males	1352	6.38		215 - 229
All females	1011	6.26		239 - 284
ALL	2363	6.33		225 - 253

Note Data are combined Area IV, 1989/90 to 1995/96 and Area V, 1990/91 to 1994/95. Figures are not all given as such in Tamura *et al* but may be deduced from their Tables 1 and 3 and the text.
Note also that the average weight of the females is less than that of the males; because the ratios of mature to immature in the two sexes are very different.

One can see the importance of these discrepancies by considering the data referred to earlier indicating that in the catches during the first years of commercial hunting of minke whales most of the females caught were pregnant, *i.e.* certainly mature. So one would expect the caught females to have an average weight of about 8 tonnes.If most of the caught males were also mature (which seems *a priori likely*, because males are smaller than females of the same age and the whalers were seeking the larger animals without being able to distinguish

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[66] Tamura, T. *et al*, 1997 "Consumption of Krill by Minke Whales in Areas IV and V of the Antarctic" *IWC Doc. SC/M97/*17 I9 pp. This, so far unpublished, paper, was presented to the JARPA review meeting, Tokyo, May 1997

sexes before killing the animals) then we would expect an overall average of 7.0 - 7.5 tonnes, close in fact to the value of 7.0 tonnes used by T&O. These discrepancies in assumed average body size, of the order of 10-15% may seem small and perhaps negligible, but their consequences need to be taken into consideration for two reasons. One is that according to T&O the minke accounts for more than half of the total consumption by baleen whales in the SHIO. The other is that the process of estimating food consumption involves the calculation of a rather long series of products, and if the errors tend to be more in one direction than the other the effect on the result can be enormous.

It is of some interest that these data reveal a very unbalanced sex-ratio (*i.e.* far from a 'default' 50:50) in what is supposed to be a random or representative sample of the entire population, at least in the selected Antarctic Areas, and very different from the ratios commonly found in commercial catches. Studies of other species (non-cetacean; see footnote 76) have suggested that under improved feeding conditions the proportion of males (at least at birth or in the juvenile stages) may increase, thus apparently providing a density-dependent regulatory mechanism. However, in this case the discrepancy is only in the numbers of mature animals, and it is not obvious how such information could be interpreted. But at least we should be warned that simple averaging of relative numbers and body sizes of the two sexes can lead one astray in the kinds of calculations performed by those who try to estimate food consumption from such data and rather complex, even obscure, metabolic hypotheses.

Now, look at the last column of Table 3, below (next page). **(***In this Table I have provided some summaries of data by species and species groups, from T&O 2000, that do not appear as such in their Table 5. In Table 5-4 of T&O 2000 the totals for baleen whales and toothed whales are shown separately. Method 1, giving the lowest overall total, shows* ***baleen*** *whales eating nearly 30%* ***less*** *than given by Method 2. But Method 1 gives a result for* ***toothed*** *whales 7%* ***more*** *than Method 2)*

T&O 2000 arrive at a daily consumption figure of 181 kg by their Method 2, with a lower figure, 152 kg, by Method 1 (Their Table 4.1). The former would, according to Tamura *et al*, be the level of consumption expected in a population of about 67% immature animals and 33% mature. The latter figure would correspond to a population only of immature animals. In both studies the Kleiber equation, as modified by Armstrong and Siegfried, is the basis of the calculation. And yet Tamura *et al* comment that "Armstrong and Siegfried's food consumption rates appear to be *over*-estimated."

TABLE 3 T&O 2000 Estimated consumption (million tonnes)

REGION	*Method 1*		*Method 2*	
South Hemisphere and I Ocean (SHIO)				
Baleen whales	**76.6**		**100.7**	
Blue		*0.44*		*0.71*
Fin		*19.73*		*30.00*
Minke		*42.12*		*50.30*
Sperm whale	29.8		35.6	
Other toothed whales	14.9		12.7	
All toothed whales	44.7		48.3	
All cetaceans	**121.3**		**149.0**	
North Pacific (NP)				
Baleen whales	**16.2**		**19.2**	
Blue		*1.15*		*1.60*
Fin		*3.73*		*4.85*
Minke		*1.80*		*1.84*
Sperm whale	13.8		15.9	
Other toothed whales	42.5		27.0	
All toothed whales	56.3		42.9	
All cetaceans	**72.5**		**62.0**	
North Atlantic (NA)				
Baleen whales	**21.5**		**25.4**	
Blue		*0.12*		*0.16*
Fin		*10.95*		*14.22*
Minke		*8.25*		*8.41*
Sperm whale	21.1		24.2	
Other toothed whales	12.9		10.4	
All toothed whales	34.0		34.6	
All cetaceans	**55.5**		**60.0**	
World Total (W)				
Baleen whales	**114.3**		**145.3**	
Blue		*1.71*		*2.47*
Fin		*34.41*		*49.07*
MInke		*52.17*		*60.55*
Sperm whale	64.7		75.7	
Other toothed whales	70.3		50.1	
All toothed whales	135.0		125.8	
All cetaceans	**249.3**		**271.0**	

12 Comparing the results of Methods 1 and 2

As an example of Boyle's assertion that figures and tables can deceive and numbers construct their own reality, it is instructive to examine the main lines of T&O's 2000 summary tables (Table 3, above).

The totals for smallest (Method 1) and middle (Method 2) figures look in fair agreement (just 9% difference), and since they arise from different methodology they might appear to substantiate each other. But the rough agreement conceals a profound difference. If we exclude the sperm whale from the toothed whale figures the discrepancy is even greater: Method 1 gives a figure over 40% higher than does Method 2.

At this stage it is useful to take a look again at the general question of the value of the power exponent in such calculations. There is a voluminous scientific literature on this matter, not, of course, relating only to cetaceans, or even to marine mammals generally, but rather to a great range of types of animals, especially the warm-blooded vertebrates (birds and mammals) and also to fishes. None of those studies supports the idea that either food consumption or metabolic needs are directly proportional to the body weight, and most of them explicitly reject such a hypothesis. This is why I earlier put aside T&O's Method 3, which gives vastly inflated consumption figures for the larger whales in which we are most interested.

What is going on here? As we have seen, both methods depend on extrapolating equations in which consumption is related not directly to the average body weight of individuals of each species, but to a *power* of a rough estimate of – or even practically a guess at - the body weight. The power *exponents* are not the same: for Method 1 the exponent is 0.67 (which implies that food consumption is roughly proportional to the surface area of the animal); for Method 2 it is 0.783. The constants in the two equations are adjusted in such a way that they give the same answer for a cetacean weighing about 6 tonnes (rather less than the weight of an average mature southern hemisphere minke), but as one extrapolates to higher weights or lower ones the calculations diverge.

Apart from anything else this means that the two methods must each be carefully scrutinised, which is what I have tried to do in the preceding sections. In particular, any extrapolations are sensitive to the exact value of the power exponent adopted, but also the constants in the two equations should be properly calibrated.

So, the rough agreement between the overall results from the two methods is due to the addition of two sets of figures that diverge from each other in opposite, cancelling directions.

It so happens that the body weight for which the results of Methods 1 and 2 roughly coincide is about that of the smallest baleen whale – the minke - and of the largest of the toothed whales except the sperm whale, that is the bottlenose whales of which there is a different species, of roughly the same size, in each ocean Region. In particular it should be noted that Method 2 gives, for the large baleen whales, up to nearly 50% higher consumption than Method 1. (The discrepancy would have been less if the two power exponents had been chosen closer to each other, provided appropriate adjustments were made to the constants, of course.)

First, notice that the overall average body weight used for minke whales, 6.33 tonnes, is close to the weight assumed by T&O 2000 in all Regions – 6.57 tonnes – which came from Trites and Pauly, 1998, and is the simple average of their estimate for each sex. (Trites and Pauly seem to have aimed to provide figures for the entire population, not just the exploited part.) But the similarity is coincidental, the value 6.33 being a weighted average of animals in a particular sex ratio (not even close to 50:50) and a particular partitioning of smaller immature and larger mature animals, which is very different between the two sexes.

The studies of the power function are of two kinds – theoretical, and empirical. The results of theoretical approaches can be summarised as, first, that the expected value of the exponent is usually 0.75. Second, that while this applies to animals that forage in two-dimensional space, the exponent might be as high as 0.83 for animals foraging in three-dimensional space. The baleen whales are essentially foraging in two-dimensions, i.e. in the thin 'skin' surface of a deep ocean; the sperm whale and other deep-divers such as the bottlenose whales, can be considered to be foraging in three dimensions.[67]

The empirical evidence is somewhat confused, though none of it contradicts the theoretical conclusions. Several kinds of data have been explored. Some are actual measurements of food consumption. Others – most – are measurements of metabolic needs and rates that are then converted to estimates of the weight of food consumed to meet those needs. The most comprehensive studies have looked at data for animals – particularly mammals - over a size range from mice to elephants. The best known of these, and the most quoted, were by S. Brodie (1968) [68] and M. Kleiber (1961).[69] Both expressed a metabolic rate as a power function of body weight, as follows:

$M = 70.W^{0.75}$ [Kleiber] *and*

$M = 70.5.W^{0.7325}$ [Brodie]

where W is body weight in kg, and M is a measure of *basal metabolic rate* (BMR) in kCals/day in Brodie, and *resting metabolic rate* in Kleiber which Lockyer calls "*normal resting rate*" or, sometimes "*near basal metabolic rate*" (NRR).

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[67] The best general references to the theoretical debate are, I think, "Newton Rules Biology: A physical approach to biological problems" by C. J. Pennycuick, Oxford University Press, 1992 (Precise but easy reading), and "A General Theory of Evolution: By Means of Selection by Density Dependent Competitive Interactions", by Lars Witting, Pregrine Publisher, Denmark, 1997 (Difficult mathematics) For a recent detailed analysis of the exponents within metabolic pathways see Darveau C-H (2002) "Allometric cascades as a unifying principle of body mass effects on metabolism" *Nature* **417**: 166-70.

[68] "Bioenergetics and Growth" Hafner, New York. In fact Brodie, in providing this equation, was quoting E.F.Adolph, 1946 "Quantitative relations in the physiological constitution of mammals" *Science* **100**: 579-85

[69] "The Fire of Life" Wiley, New York and London.

Subsequently, Lockyer derived another equation:

$M = 115.5.W^{0\text{-}75}$ [Lockyer [70] which I refer to later as Lockyer (a)]

Here, M is NRR incremented by an allowance for energy expended in swimming, which she refers to as "total energy expenditure while swimming from place to place or feeding" (TEE). This has been done by adjusting upwards the value of the constant in the Kleiber equation, by a fixed multiplier to represent the additional energy This actually implies an assumption that the additional energy is itself proportional to the same power function of body weight as the RMR, which may not be correct. The adjustment used by Lockyer was 70+65%=115.5
Next, in her papers Lockyer presents a fourth equation:

$M = 110xW^{0.783}$ [Referred to later as Lockyer (b)]

This is based on her own analysis of selected data, for marine mammals, shown as a graph in her 1981 paper. The actual equation is not provided, but had been included in an early draft of Lockyer. (1)

Lockyer's equation (b) is the one tucked inside T&O's Method 2. We shall see later the implications of this, but for now I note that (i) Lockyer used only data from the literature for marine mammals – almost all cetaceans but also a few species of seals; (ii) this third value for the exponent does not differ significantly from the others or from the theoretical value 0.75, but all three do differ significantly from 1.0.

Lockyer notes that the energies calculated from her equation are higher than those from Brodie or Kleiber, but that hers "should be regarded as **probable upper limits** for the purposes of an annual calculation of energy costs", which she did by multiplying her preferred daily figure by 365. Lockyer (1981) also remarks that "For all practical purposes the [Brodie and Kleiber] formulae are similar", but recognised that Brodie was looking at what she calls "basal (starvation level) rates" while Kleiber considered "normal resting rates" (NRR). By the same criterion (exponent in the narrow range 0.750-0.783) both are similar to her own, except of course for the adjustment to the constant to account for energy demand over and above BMR or NRR. Unfortunately this "similarity" is limited in an important way despite the exponents appearing to be close in value This can be seen most easily by calculating a body size for which each pair, or all three, give the same answer, and then extrapolating them to larger or smaller body sizes.

First, we find that the Kleiber equation always gives higher Resting Rates than Brodie's BMR for animals weighing more than 1.5 kg, so of course it will always

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[70] There are three related papers by C. Lockyer, cited by various other workers: (1) "Estimation of the Energy Costs of Growth, Maintenance and Reproduction in the Female Minke Whale, (Balaenoptera acutorostrata), from the Southern Hemisphere" *Rep. Int. Whal. Commn* **31**:337-43; (2) "Growth and energy budgets of of the large baleen whales from the Southern Hemisphere" 1976, *Paper ACMRR/MM/SC/41* to the FAO Consultation on Marine Mammals, Bergen, Norway [Cited in (1)]; (3) Same title as, and revised published form of, (2*) FAO Fish Ser.* **3** (Mammals in the Seas, Vol. 3: 378-487, 1981.

give higher values for cetaceans, but how much higher depends on the size of the animal. Lockyer thought the difference would generally be about 18%. Using the average weights adopted by T&O 2000 I find the difference is only about 8% for a dolphin (100-200 kg), 16% for a minke whale (6.5 tonnes), and 21% for a large baleen whale, so her statement is roughly true for baleen whales.

Lockyer (a) based the derivation of her estimates of "quiet" metabolic rates (which were to be incremented to take activity into account) on the Kleiber equation. She wrote that the "The energetic cost of swimming continuously would increase the resting metabolic rate by from 7.5 to 65%." Since these figures would be for a whale mainly engaged in feeding, she wrote, she would take account of high costs of migratory swimming by increasing the Kleiber constant by 65%, giving the third equation. However, the "upgrades" from resting to active given by Lockyer (b) are very different. The active rate for a dolphin comes out at about 90% higher than the resting rate, for an animal the size of a bottlenose whale it would be double, and for a large baleen whale the active rate would appear to be 2.3 times the resting rate. It was these high rates – the highest among a number of calculations - that were used by T&O.

13 Converting energy needs to food consumption

For the completion of Method 2 it is necessary to convert the theoretical energy costs into weights of food intake, to "live" or "wet" weight, in terms of the measurement standards used in compiling fisheries landings statistics. To do that one has to know the **energy densities** of the particular dietary constituents, in kCal/kg, and, of course, the relative frequencies of different items in diets, as well as to decide what exactly will be the species body weights to adopt. I return to these matters later when looking in more detail at the Method 2 application. Here, however, it is appropriate to comment on the fact that not only in the analyses presented by T&O, but also in most of the debate about the food consumption of marine predators, there has been, and continues to be, a virtually exclusive concern with gross intake rates, as biomass, and/or energy as measured in a calorimeter. There are, however, other measures by which the **quality** of its diet is perceived by an the importance of selection of food also to meet needs for specific elements and compounds animal than as mere bulk or energy content. Many animal behaviour studies have shown that are required for health, growth, reproduction and survival – variously elements such as iron, phosphorus, manganese etc., certain fats and other compounds – and which are sometimes in limited supply. There is no reason to suppose that cetaceans are in this respect different from other vertebrates, in particular from other mammals, so even good estimates simply of biomass or energy intakes are insufficient to determine the nutritional status and needs of the animal.

It remains to look more closely at the conversion of energy to estimates of food consumption expressed as wet biomass. We have already noted that S&V used, for "calculation of ingested biomass, in the absence of data on seasonal variation in energy content of prey species", the values 930 kcal/kg for crustaceans and 1300 kcal/kg for fishes and cephalopods, while T&O used the values 1110.3 for all food of "baleen whales in the Southern Hemisphere" (presumably nearly all crustaceans) and 1300 kcal/kg for baleen whales in the Northern Hemisphere and also for all toothed whales throughout the world. S&V said they took their numbers, respectively, from Lockyer, 1987 and F. W. Steimle and R. J. Terranova, 1985[71] while T&O took their second figure also from Steimle and Terranova, but their first figure from A. R. Clark, 1980. No explanations were given for those choices, and in particular for the two different uses of the 1300 figure.

There is an important difference between the treatments by the two pairs of authors. S&V looked at the prey composition and sought to apply the appropriate factor to each kind of dietary item. T&O could have done likewise, but instead chose to use a single figure for all baleen whales in the Northern Hemisphere as well as all toothed whales, regardless of what they were supposedly eating, and then they – erroneously – divided the total consumption among the various categories of prey: fish, crustaceans and cephalopods.

So let us look again at Steimle and Terranova's data and try to figure out where the 1300 figure comes from and what it means. These authors express the energy densities in kilojoules per gram (KJ/g); I have converted these to kcal/kg for ready comparison with figures in the papers we are reviewing:
1 kJ/g = 239 kcal/kg. First, the KJ/g equivalent of number 1300 (5.44) appears nowhere in their paper, in text or in the voluminous tables. And, curiously, these tables include data for planktonic crustaceans that have not been used by either pair of authors.

Steimle and Terranova's Table 1 groups their results, and "comparable" others culled from the literature, by broad prey classes. Fishes are grouped as demersal (1040, 32 species) and pelagic (1506, 27 species); the simple average of these is 1273. This is close to 1300 but does not unambiguously reveal the latter's origin. The "comparable" figures range from 1056 to 1226 for demersal fishes and from 1460 to 1926 for pelagic fishes. Although some of the smaller toothed cetaceans might eat some demersal fishes, baleen whales surely do not (I suppose a possible exception is the grey whale), so from the present point of view a more appropriate figure than 1300 might be somewhere in the range 1554 to 1673. On the other hand cephalopods eaten by the larger toothed whales were found by Steimle and Terranova to have an energy density of 1315, while a comparable figure from another publication is only 1052. From looking at these averaged figures the use by S&V of 1300 as a conversion figure for fish and cephalopods combined may be reasonable, the use of the same figure for all food consumption by baleen whales in the northern hemisphere certainly is not, since by far the greater part of food consumption by baleen whales in both the NP and the NA, by T&O's own calculations, is of crustaceans – zooplankton such as krill, copepods and amphipods. So an appropriate figure would be closer to the range 930 to 1100 than to 1300 if the former range is correct for krill, etc.

Steimre and Terranova's Appendix table, giving all their ungrouped data, provides a rather different perspective. Firstly, it does provide results for analysis of 15 specimens of a euphausid, *Meganyctiphanes norvegica* - 813 kcal/kg, *i.e.* distinctly lower than those we have encountered before. That table includes, however, figures for five species of amphipods, and those range from 406 to 2653. This draws attention to another feature of such data – the great variability in energy density between closely related prey types (and also the same species

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[71] "The biochemical composition of krill, *Euphausia superba* DANA from South Georgia". *J. Exp. Mar. Biol.Ecol.* **43**: 221-36.

sampled under different conditions of their life cycles), especially when expressed in terms of wet weight. Steimle and Terranova point out that dry weight values vary less, and especially ash-free dry weights as compared with dry weights including ash. So, some of the variation is due to differing water contents of the prey animals.

With this in mind it is worth looking at the details for some of the fishes given in their Appendix table, especially those fish species found by S&V and others in the stomachs of baleen whales in the North Atlantic. Most of the Icelandic data are, according to S&V, unpublished, but they refer to capelin, sandeels and juvenile blue whiting (*Micromesistius poutassou*). Elsewhere in this Region Norwegian workers report herring, sometimes in substantial quantities. Unfortunately Steimle and Terranova did not include capelin in their study, but they do give their data for herring, for a sandeel, and for some other small pelagic fishes such as anchovies (two species) and a sardine. Their figures (converted and rounded) for these are, respectively 2500, 1600, 1400 and 1400. Evidently any estimates of fish consumption by baleen whales depend very much on whether they were eating herrings or smaller clupeids or sandeels, with an almost 1:2 range among just these few species. So any figure to be applied to Northern Hemisphere baleen whales must necessarily be derived from knowledge of the relative quantities of pelagic fishes and crustaceans in the diet and, if the quantities of fish are substantial, the kinds of fish being consumed. T&O's calculations conform with neither of those requirements. In this connection, Steimle and Terranova's concluding remarks are of interest.

"Most energy budgets are still crude, and the variables are often inaccurate by a factor of 2 or more" and "Attempts were made to substitute the present energy data in several published models to see if the conclusions might be altered, but the information in those papers was not sufficiently specific."

They point out that the variability within taxa, for energy per dry weight, is a function of the condition of the prey organism (age, reproductive stage, health etc) but the greatest variability as between species and species groups, even of closely related species, is in water content.

14 The composition of diets

We have seen that power functions relating food intake to energy requirements can only properly be applied if the species composition of the diet is known – quantitatively, that is, as well as qualitatively – and taken into account by application of a wide range of appropriate values for energy density of the prey. Not only did T&O ignore this requirement but they applied inappropriate gross figures differing only between the Northern and Southern Hemispheres. Despite the fact that they decided that the Bryde's whale was apparently an abundant consumer of substantial quantities of fish in the Southern Hemisphere, accounting for practically all their estimated fish consumption in that Region, they used an energy density figure which others have generally used for pelagic crustaceans. Of course, one consequence of this is to **inflate** their estimates for total food intake by whales globally, especially baleen whales, and particularly in the Southern Hemisphere. This, in turn, **inflates** the estimates of the amounts of **fish** consumed in the Region, mostly by Bryde's whales.[72]

To get a better perspective on this matter it is necessary to look beyond the few published references to the diets of whales that T&O cited, and especially to Norwegian research in the Northeast Atlantic.

In examining contents of the forestomachs of baleen whales, especially minke whales, many researchers have tried to identify fish species but few to measure the fish. Among the few were Skaug *et al*, 1997.[73] During a "scientific whaling" programme, in August 1992 in northern Norwegian waters – Lofoten and Finnmark – and the Kola peninsula (Russia), they counted the total number of each fish species in the forestomachs of minke whales by adding the number of as yet undigested fishes, the number of intact fish skulls and half the number of free otoliths (the hard objects in fish ears from which age of the animal can often be determined.) Each otolith was measured, and from such measurements the length and hence the weight of each fish estimated by methods well-known to fishery scientists. Similar care was taken with the crustaceans in the stomachs though their sizes could not so easily be measured; instead trawl samples were obtained and the average size of the species in the minke diet thereby estimated. Hence – again very unusually – it was possible for them to present dietary composition in terms of the fresh weight of each constituent species. Almost all other presentations of diet data – including all those used by T&O – come from counting the *frequency of occurrence* in a number of stomachs. That can give very biased results, especially because - as we have seen - although there are no gastric enzymes functioning in the forestomach, digestion has normally begun there, particularly of certain chemical constituents, and therefore – selectively – of certain species components, by the activity of bacteria.

Skaug *et al* were able to distinguish and assess six categories of prey: plankton (almost all krill and some other crustaceans); herring; capelin (*Mallotus villosus)*; cod and haddock *Melanogrammus aeglefinus*); pelagic fishes (capelin and saithe (*Pollachius virens*)) and bottom (mostly demersal) fish species. A seventh category they called "O-group". These are fish in their first year (called in other papers in English by Norwegians "brits" (no slight on the British intended, I'm sure!). In Skaug *et al's* samples these were mostly young of herring, with some gadoid fishes – cod, haddock, whiting. In other studies other O-group species appear.

When we read that minke or other baleen whales eat fish such as cod (*Gadus morhua*) we tend to think of the animals we see in fish shops, which are themselves major predators on other fishes – including their own juniors. But much of the cod and the herrings eaten by whales, as well as the herrings eaten by cod, are young, of sizes that are not retained by fishermen but which do die at the hands of humans as "trash" or "discards" – overboard. Furthermore, that is not to say that minke whales do not sometimes eat large gadoids; in one

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[72] Comparisons with the NA are not possible; T&O do not include that species in their tables.

[73] H. J. Skaug, Gjøsaeter, H., Haug, Tore, Nilssen, K. T. and Linstrøm, U. "Do Minke Whales (*Balaenoptera acutorostrata*) Exhibit Particular Prey Preferences?" *J. Northw. Atl Fish. Sci.* **22**: 91-104.

Norwegian study of a commercially caught minke whale a 4 kg haddock was found in a stomach, but by and large the whales and the fishermen seek different sizes of the same fish species (One has to wonder how a baleen whale that has evolved to specialise in filtering off relatively tiny organisms, manages, without the help of teeth, occasionally to ingest a large fish. That is one of Nature's enduring mysteries).

Nordøy and Blix, 1992 [74]– they of the "brits" - wrote that stomachs of minke whales caught off northern Norway on other occasions, and off Spitzbergen, rarely contained krill, but did contain pollock[75] larvae (*Pollarchius*) and great silver smelt (*Argentina*) as well as the usual herring, cod, haddock and capelin. They do not mention "O-group", but one stomach with only crustaceans in it contained a huge volume of one species of krill (*Meganyctiphanes*), while another contained a mixture of a *different* krill species (*Thysanoessa*) and herring in about equal amounts. They did make one new observation, which they considered to be "potentially important". This was that minke whales also eat fish *fry* – technically within the O-group but in fact virtually macro-plankton. The case in question was the finding of large quantities of 5-7 cm herrings in the stomach of a minke whale caught off northern Norway in 1989.

I have mentioned these results of Norwegian research not only to establish that at least one of the baleen whale species of greatest current interest has a very diversified diet, but also to indicate that the variety is such that it is not possible, from present scientific information, to provide sensible estimates of the average annual diet for an entire Region, even by broad categories such as "fish" and "crustaceans". A further source of variability, commented upon in some papers but little studied, is the difference in apparent diet of minke whales caught near the coast and those taken further out to sea, over deeper water with a very different pelagic fish and plankton fauna.

We briefly looked above at the fact that some of the "fish" consumed by minke whales, at least in the North Atlantic, consists of O-group animals of various species. Haug et al, 1996,[76] made two important observations. One was that in a certain season, in the southern Barents Sea, minke whales were feeding on, among other things, cod and herring, *and the cod were feeding on herring*. That poses rather clearly, I think, the difficulty of deciding whether baleen whales are friends or enemies of human interests. More seriously, they record that "From the end of July the fish biomass … is usually augmented by an influx of O-group fish species (mainly cod and herring) which are transported into the area by currents from the southwest." Such concentrations and movements, on a substantial oceanic scale, are exactly what one would expect to be tracked by clever predators such as the baleen whales. The other observation – not new, but emphatic here – was that diet varies enormously from sample to sample, from area to area, season to season, distance from shore and year to year – and, I would add, among individuals who, from what we know about many other

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[74] Nordøy, E. S. and Blix, A. S. "Diet of Minke Whales in the Northeastern Atlantic". *Rep. Int. Whal. Commn* **42**: 393-98.

[75] Not to be confused with the unrelated pollock (*Thaeragra chalcogramma*), in the N. Pacific.

[76] Haug, T., Lindstrøm, U., Nilssen, K. T., Røttingen, I. And Skaug, H. J. "Diet and Food Availability for Northeast Atlantic Minke Whales, *Balaenoptera acutorostrate*." *Rep. Int. Whal. Commn* **46**: 371-82.

animals, have their own preferences which they exercise when conditions permit.

With respect to the O-group fish there was one puzzle: "Despite observations of large numbers of O-group cod in the upper water layers, none were found in the stomachs from minke whales caught in the northernmost areas (Spitzbergen and Bear Island)" yet there was an apparent preference for O-group fish – mainly herrings – further south, off Finnmark.

Unfortunately, Skaug *et al* fall into the common error in the construction of multi-species models incorporating mainly predator-prey relations, of making simplistic, "Cartesian", assumptions about the way animals behave, and not only mammals (and other apparently more "intelligent" groups such as birds) but also fishes and at least some invertebrates such as crustaceans, insects and molluscs. Their model incorporates two basic assumptions about "the process in which the whale chooses its prey". First, I think reasonably, that "it seeks out areas in which there is a high density of preferred prey"; second – which I think contradicts much of what we have learned about animal behaviour – that "faced with a choice among available prey items while feeding, the whale preys on the most abundant item in the neighbourhood, irrespective of which other species may be present." They are not alone in making such simplistic assumptions; they still pervade multi-species models by which some hope better to manage fisheries, denying the ways predators actually behave, asserting that they are automatons in which the process of selection is merely a function of some crude measure of prey "abundance".

These authors were aware of the difficulties of proving or disproving such an assumption simply by comparing compositions of stomach contents with estimates of relative abundances of the prey species "in the environment". Such studies are rarely made anyway, but when they are, and when - as frequently happens - good correlations are not found, it is tempting to "improve" them by devising different measures of the "environment", rather than to make more effort to study the behaviour of the live predator, difficult though that may be.

On this matter a recent paper by Mayntz *et al*, 2005 is interesting.[77] "It is commonly believed that the body composition of prey animals is nutritionally balanced for carnivores, which consequently should be limited by the availability rather than the nutritional balance of the prey. Herbivores and omnivores, by contrast, are known to face a heterogeneous nutritional environment and therefore possess a well-developed ability to balance their intake of multiple nutrients. **There is, however, accumulating evidence to suggest that some carnivores too might benefit from having nutrient-specific regulatory abilities**. One species of spider has been observed to mix its intake from several prey species, leading to the hypothesis that it does so to compose diets with optimal amino acid makeup. These authors recall that experiments show that "some domesticated carnivorous fish can compose diets from pure micronutrient sources in proportions that differ from random selection."

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[77] Mayntz, D., Raubenheimer, D., Salomon, M., Toft, S. and Simpson, S. J. "Nutrient-Specific Foraging in Invertebrate Predators." *Science* **307**: 111-2.

Experiments by Mayntz *et al*, with, for example, predatory beetles and spiders, showed that such invertebrate predators "can regulate their intake of protein and lipids, and that they can use this ability to redress an existing imbalance of these nutrients. The fact that only short periods of time were sufficient to generate nutrient-specific compensatory responses suggests that the observed regulatory mechanisms are involved in the fine-tuned compensation for the day-to-day variations in prey nutrient composition. The need for such a capacity is highlighted by the demonstration that prey composition varies markedly, even within a single species. Furthermore, nutritional selection may occur at several stages of prey handling, by selecting among foods of different nutritional composition, by adjusting consumption of a single prey type depending on its nutrient composition, or by extracting nutrients selectively from within individual prey items. These results are highly relevant for optimal foraging models where it (has been) generally assumed that, for carnivores, prey quantity rather than quality matters."

It seems to me reasonable to suppose that what beetles and spiders can do cetaceans can also do. In fact one problem many of us still have in thinking about such things (apart from the disbelief that mere animals can behave in such a sophisticated manner) is that there is still a widespread folk belief that baleen whales feed by swimming about in the sea with their mouths open!

There is little observational, and no experimental, evidence regarding selective foraging by baleen whales. However, one publication has significant data, and it relates specifically to the minke whale. A. R. Holzel, E. M. Dorsey and J. Stern studied the foraging behaviour of 23 individually recognisable minke whales, mostly in the near-shore waters surrounding the San Juan Islands in Washington State, USA.[78] This was a remarkable demonstration of the power of closely observing live animals, as opposed to looking in the guts of dead "scientific whales" (as they were quaintly called by earlier generations of whaling biologists), to elucidate important facts about their feeding behaviour and its consequences. These researchers found that "Most individuals specialised in one of two feeding strategies distinguishable by behaviour visible at the surface: lunge feeding and 'bird-association' feeding. These strategies were consistent over a period of up to five years. Each strategy was largely associated with a particular feeding area. It is suggested that individual specialisations represent learned strategies that may have equivalent payoffs and that they are adapted to variations in the distribution of the same prey species".

The lunge feeding behaviour was directed to the consumption of small pelagic fishes that were nevertheless large and active enough to be seen jumping out of the water in apparent efforts to escape. Bird-associations, on the other hand, were directed to consuming fish fry that were also the target of gulls and auks. Prey were identified and aged from scales found immediately afterwards at the feeding sites. In both the lunge and the bird-associated groups of feeders the animals taken were most frequently juvenile Pacific herring (*Clupea pallasii*), but the bird-association diet consisted almost as frequently of juvenile sand lance (*Ammodytes* spp - sandeels). Statistical analysis of their – admittedly limited – observations suggested that the choice of strategy (resulting, it appears, in a

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[78] Hoelzel, A. R. et al (1989. "The foraging specialisations of individual minke whales" *Anim. Behav.* **38**: 786-94.

different diet) was made by individual choice rather than, as might have been expected alternatively, as a consequence of the fact that a particular animal repeatedly and consistently returned to the same feeding site.

[It is worth noting at this point that the population/ecological effect of whales consuming, say, a tonne of herring fry is quite different from the removal by fishing of a tonne of adult herring, or even of juveniles. Although one might suppose that the huge number of herrings in a tonne of fry would eventually comprise much more than a tonne of grown animals, in fact the natural mortality rates of such fish species during the "pre-recruit" period from egg through larvae to juveniles is so high that less than a tonne of the young might survive to the size and age of "recruitment" to the fisheries, although – given time – the survivors will probably eventually grow to far exceed the original weight].

Similar food preferences, associated with different styles of feeding behaviours have been demonstrated by Paul Spong for orcas off the coast of British Columbia, also by observing identified individuals over many years. Some specialise in hunting salmon, others in catching seal pups. Evidently the problems here identified will not be resolved by looking in the stomachs of more dead whales.

Å. Jonsgård, 1982 [79] reported on examinations of 237 stomachs of minke whales caught commercially by Norwegians in the Northeast Atlantic in 1943, 1945, 1946, and 1950. Most of the recent studies do little more than replicate his general findings. *Only* fish were found in 44% of the total 237 animals, and *all* the whales examined off East Greenland and off Vesterålen had fed *exclusively* on fish. However, "*pelagic crustaceans only* were found in 32% of the stomachs of all whales examined." Three species of krill, and a copepod species comprised the bulk of the diets of the whales killed in the Barents Sea (proper) and at Spitzbergen. But in the Bear Island area most stomachs contained a mixture of one of the krill species and capelin. There do appear to have been some changes in recent years, which several authors suggest is attributable to real changes in the relative abundances of different prey species in the ocean, at least partly as a result of intensive fishing. Jonsgård refers to numerous small cod, but also to occasional full-grown haddock. He refers to stomachs full of krill, but also others full of copepods (not mentioned by any of the recent workers) and to mixtures of the two types of crustacean. And, since the Norwegian industry was at that time far ranging, these commercial samples give a broad picture of the diversity of diets in various localities in the NA.

In his 2000 review of what minke whales eat when near the west coast of Greenland, P. B. Neve cites a previous review by F.Larsen and F. O. Kapel, 1981, stating that ***pteropods*** (tiny planktonic snails) were frequently reported, along with euphausids, other crustaceans and capelin.[80] These animals deserve more than a footnote here. Murray and Hjort, 1912, in their classic work,[81] describe the

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[79] Jonsgård, Å. "The Food of Minke Whales (*Balaenoptera acutorostrata*) in Northern North Atlantic Waters." *Rep. int. Whal. Commn* **32**: 259-62.

[80] "The diet of the minke whales in Greenland A short review" *NAMMCO Scf. Publ.* **2**: 92-6.
and "Collection of biological material of minke whales off West Greenland, 1979" *Rep. Int. Whal. Commn* **31**: 279-85.

[81] "The Depths of the Ocean". MacMillan, London 821 pp.

pelagic pteropods (there are also many benthic species, the tiny shells of which constitute some of the seabed oozes) as "comparatively few (in species) but as regards abundance of individuals few groups of pelagic animals can compare with these 'winged snails'". One species, *Limacina helicina,* "occurs in immense quantities in the seas around Greenland and Spitzbergen, while *L. balea*, called 'flueaart' by Norwegian fishermen, and *L. retroversa* are abundant in the Norwegian Sea. The presence of these two is feared by the fishermen because they very often 'spoil' (from our point of view *sjh*) the herring which feed on them." Another species, *Clione limacina*, was known as "whale's food" and was very abundant in the NA, especially south of Iceland, in Norwegian Fjords and the Norwegian Sea and in the Arctic Ocean. It seems to me likely that the pteropods play a bigger role in minke diets than they have been credited with. Although not minute (*Clione* is 3-4 cm long), they are, according to Murray and Hjort, "perfectly transparent" and easily overlooked w hen stomach contents of whales are being inspected. Similar pteropods are extremely abundant in the near-surface, colder and warmer waters of the Southern and Indian Oceans.

The uncertainty about what really does constitute the diet of minke whales, specifically in the NA Region, spills over from the "simple" calculation of how much they eat of what, to the inclusion of such information in multi-species models intended – among other things – to generate advice about culling whales. For example T. Schweder *et al* produced such a model for the Greater Barents Sea [82]. In this they assumed that the minke diet comprised *only* cod, capelin and herring.[83] But N. H. Markusson *et al* had earlier estimated the consumption of food by the Northeast Atlantic minke population, using a simulation model.[84] Neither their study, nor that by Jonsgård, were cited by Schweder *et al*. They did, however, cite several other references from which it was clear that "Stomach analyses show much variation in the diet". Furthermore, "the energy content of the diet varies from 900 kcal/kg for amphipods to 3000 kcal/kg for herring." Markussen and her co-authors noted that "most of the stomachs analysed had a mixture of crustaceans and fish" and that because diet varies greatly from place to place, season to season and year to year " the daily food consumption ranges from 63 to 462 kg in adult females, and from 47 to 340 in adult males", *i.e*. 7 to 8-fold.

Even though Jonsgård's observations were scattered through a long period, it is difficult to see how Schweder *et al* could justify their very different assumptions about minke diet in the Greater Barents Sea. To add to the array of inconsistent assumptions made about minke diet in the eastern NA by a number of workers who have juggled with multi-species models for provision of fisheries

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[82] T. Schweder, G. S. Hagen and E. Hattlebakk, 2000. "Direct and indirect effects of minke whale abundance on cod and herring fisheries: A scenario experiment for the Greater Barents Sea" *in* "Minke whales, harp and hooded: Major predators in the North Atlantic Ecosystem." e*d*. G. A. Vikingsson and F. O. Kapel, 2000. *NAMMCO Scientific Publications* **2**:120-32. They define this area as the Barents Sea Proper plus the Norwegian Sea

[83] Strictly speaking their model is structured such that its dynamics are not affected by whatever else the whales may be eating, nor by what quantities.

[84] N. H. Markussen, M. Ryg and C. Lydersen, 1992. "Food consumption of the NE Atlantic minke whale (*Balaenoptera acutorostrata*) population estimated with a simulation model" *ICES J. mar. Sci*. **49**: 317-23.

management advice, I note that S. Tjelmeland and U. Lindstrom, 2005,[85] "assumed that minke whales in the southern Barents Sea feed on juvenile herring, capelin, krill, cod and other food, whereas minke whales outside the Barents Sea feed on adult herring and other food."

A striking example of how the perceptions of minke diet are related to the precise circumstances of the kill that provides convenient stomach samples is provided by D. Sergeant, 1963.[86] Capelin was the dominant constituent of the contents of his samples from the coastal whaling operation in eastern Newfoundland, being found in 85% of the stomachs examined. But this species spawns in mid-summer on and near the beaches of this area and is, or was, the target of a local fishery. "The abundance of minke whales in these bays" Sergeant observes, "is therefore directly dependent on dense schools of capelin." Minke whales caught before or after the capelin fishing season either had empty stomachs or contained other dietary items, including euphausids, copepods, squid, cod, herring and salmon. Because there was no offshore industry there is no evidence of what minke whales were eating when they were not patrolling the beaches for spawning capelin.

I have given special attention to studies of minke whales in the North Atlantic not only because that species is now the one of most interest to whalers and to fishermen who worry about the whales' intrusions on "their" resources, but also because it has been in recent years the subject of much research and controversy. The difficulty in interpreting the data sources for other species, which I explore later, does not arise only from their paucity and variability and the weakness of estimates of *quantities* in stomachs from observation simply of frequency of *occurrence.* There is also the problem of selective location of sparse observations. For example, T&O write that "Perkins and Beamish reported that the humpback whale feeds mainly on capelin in Newfoundland".[87] That finding is hardly surprising because Perkins' and Beamish's data came from animals entangled in fishing nets! But, more seriously, E. Mitchell, among others, long ago published important material on the localisation of feeding habits.[88] He looked in the stomachs of baleen whales brought to the Canadian land-stations at Blandford, Nova Scotia, and Dildo, Newfoundland, in the seasons 1965 to 1972, and 1970-1972 respectively. The Blandford sample included fin and sei whales; the Dildo samples were only fin whales. Mitchell did not report the total numbers of whales examined, but he noted that at Blandford 22% of the stomachs had been cut at sea, and 18% were, for some reason, not examined. 13% of the stomachs were empty. Of the rest krill always predominated in the contents. Mitchell gives monthly summaries for May to November. In May krill accounted for about half the occurrences. In every other month it accounted for more than 85%. The rest

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[85] "An ecosystem element added to the assessment of Norwegian spring-spawning herring: implementing predation by minke whales" *ICES J. mar. Sci* **62**: 285-94

[86] "Minke Whales, *Balaenoptera acutorostrata* Lacépède, of the Western North Atlantic". *J. Fish. Res. Bd Canada* **20**: 1489-504.

[87] J. S. Perkins and P. C. Beamish (1979). "Net entanglements of baleen whales in the Inshore fishery of Newfoundland." *J. fish. Res. Board Canada* **36:** 521-8.

[88] E. Mitchell (1974) "Trophic Relationships and Competition for Food in Northwest Atlantic Whales" *Proc. Canadian Soc. Biologists Annual Meeting* June 2-5, pp 123-33.

was made up of an assortment of sandeels, mackerel, herring, squid, assorted unidentified fishes, and copepods.

Mitchell's findings in Newfoundland, not very far away – in both cases in the area off the mouth of the St Lawrence River – were entirely different. Only 64% of the animals landed; could be investigated; 26% had cut stomachs and 10% could not be examined. For the rest krill were entirely absent in May, they were marginally present in June and July only. The May diet was 100% capelin; capelin comprised more than 85% of occurrences in June, July and August, and about 50% in October and November. The other species present was, again, sandeels, but no herring, mackerel, squids or copepods. The rest of the Dildo diet was comprised of lantern fishes (Myctophidae) and cod.

In his discussion of the implications of these findings Mitchell pointed out that Jonsgård had found similar differences in fin whale diet along the coast of Norway and as far south as Ireland. He speculated about possible separate populations and acquired feeding habits, or simply the consequences of extreme but regular patchiness in the distributions of various prey species. What is quite clear is that if data had been available from only one or the other of those two close locations their application to a wider area and an entire whale population (or group of populations) would have been a major error.

Mitchell's data also included information from landings of sei whales at Blandford. He observed that the sei, unlike some of the other baleen whales, has specialised fine baleen that retains copepods – smaller than krill – and appears, together with a feeding behaviour that is quite different from that of the fin whale, for example, to make it difficult for it to take much fish. Indeed, there were no fish at all in the sei whale stomachs from Blandford, but the relative occurrences of krill and copepods were quite different from month to month, and bore no relation to the diet of the fin whales being caught at the same time and place. In May and November the sei diet consisted *only* of krill, while from June to October it was mostly copepods – that is, at the time that the fin whales there were eating mostly krill .

Mitchell's 30-year-old paper was worth revisiting for his discussion of many aspects of whale research and conservation. But for us, in the present context, I think it warns that selective, limited and localised data, such as have been used throughout by T&O, can be extremely misleading. It is worthy of note that at the time that the Japanese authorities decided to concentrate in their scientific whaling on examining the diets of caught whales, they also decided to take the samples – at least in the North Pacific – in fishing areas. Naturally, in view of the known variability, this would provide inflated estimates of fish consumption by whales in that Region, and even of particular species of fish.

15 Inter-specific competition

"..an ecosystem's complexity almost always exceeds our power to grasp it. That is a truth we would do well to recognise before we repeat (our) mistakes". [89]

Although the main thrust of the ICR propaganda and its supporting "scientific" materials is to claim that whales eat huge quantities of fish and hence threaten commercial fisheries, such a claim is hard to sustain with respect to the baleen whales that feed in the Southern Ocean,[90] and live virtually entirely on euphausids and copepods. Consequently the main argument used for culling the still relatively abundant southern minke whale which, like the blue whale, subsists on *Euphausia superba,* is now that minke whales are impeding the recovery of the "protected" blue whales.

This general idea was promulgated by Ohsumi as early as the mid-1970s,[91] and more urgently when the "moratorium" was decided in 1982. He argued that the blue whale, which was expected to have been recovering after decades of "protection", was not apparently doing so. Many repetitions of that argument led to a more definitive statement being made by propagandists that the blue whale was *not* recovering. This notion received a blow when it was revealed, soon after the political collapse of the USSR, that Russian and Ukrainian pelagic expeditions had been killing illegally all the blue and right whales that they happened to come across while hunting primarily for other species.

There is no clear evidence, one way or the other, for changes in the number of southern blue whales. Earlier expectations of recovery came from the application to depleted whale populations of simplistic "logistic-type" population models – that many scientists (me included) consider out-dated and misleading; they contain the "strong" assumption that populations increase at their fastest rate when they have been reduced almost to extinction. Nevertheless there is undoubtedly an overlap between the foraging places of the two species, so the

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[89] Arno Karlen (2000) "The Biography of a Germ" Gollancz, London. See, again, Appendix II.

[90] The Bryde's whales of the SHIO do eat some fish but they do not feed in the S. Ocean.

[91] The context then was the implementation of the IWC's New Management Procedure, in 1975, that quickly led to cessation of *fin* whale catching in the SHIO. Ohsumi's "hypothesis" was comprehensively demolished by A. Kawamura (Mie University). In a 1993 paper to a meeting of the Working Group on krill of the Commission for the Conservation of Antarctic Marine Living Resources (CCAMLR) he wrote that "A scenario that is increasingly being accepted without real evidence is that the "krill surplus [caused by the near extermination of the larger baleen whales]" has led to rapid population growth of minke whales, crabeater and some phocid seals and penguins and thereby has prevented recovery of the blue and perhaps the fin whales. After criticising the whale research methodology "largely based on examination of dead animals, Kawamura continued " Considering that the absolutely highest estimates of the consumption of krill by minke whales in the Southern Ocean was only a small percentage of the estimates of krill abundance, He observed that "it appears unlikely that the apparent low recovery rate of blue whales is simply a reflection of food partitioning among baleen whales, especially the minke whale."("A Review of Baleen Whale Feeding in the Southern Ocean" *Rep. Int Whal. Commn* **44**: 261-71, 1994.

idea that they are competing with each other remains plausible.[92] So, naturally, the JARPA scientific whaling programme now pays particular attention to this supposed competition between the minke and the blue whales.[93]

Yasuaki Masaki published in 1979 an analysis of reproductive features of the minke whale, based on Japanese commercial catches in the Antarctic, 1971/72-1976/77.[94] He concluded that there was no evidence of changes in the pregnancy rate nor in the very small proportion of twins and triplets among the foetuses, but there had been a decrease in the average age at sexual maturity of the females.[95] Furthermore, by extrapolating backwards to the time before the commencement of minke whaling in 1970, using what are called "transition layers" in the waxy earplugs from which age is determined, he concluded that the maturity change had begun some time earlier. He noted, reasonably, that such change could hardly be related to a reduction in the numbers of minke whales by whaling, so suggested that it might be related to an overall decline in the abundance of baleen whales generally, leading to a "surplus" of krill.

Drawing conclusions from changes, such as in age-at-maturity or pregnancy rates in the population, from samples of the commercial catches, has always been problematic, since such catches are selective, and the selectivity alters with markets and demand, with the precise area exploited in a given season, and even with the timing during the season. (The pregnancy rates in Masaki's samples ranged from 86 to 95%, showing quite obviously that the whalers were avoiding the capture of small and hence immature whales, despite the absence of a minimum legal size for this species). R. Gambell, for example sought to draw such conclusions about change in age at maturity in fin and sei whales.[96] He noted that the *apparent* age at maturity of both fin and sei whales had declined (although he also detected no significant changes in pregnancy rate), and that this had begun to happen before the intense exploitation of the latter species had begun (in the mid-1960s, after the much larger blue and fin whales had been depleted). This led to speculation by Gambell and others that the apparent changes were connected with an overall decline in total baleen whale biomass in the Region. But it seems to have escaped their notice that while fin whales in the Southern Ocean subsist almost entirely on krill, the sei whales – feeding mostly

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[92] T. Kasuya submitted a paper on the "Possibility of competition between southern baleen whales for food resources" to the IWC Planning Meeting of Comprehensive Assessment of Southern Baleen Whales, in March 1992 on the apparent overlaps in diets.

[93] See numerous brochures issued by ICR, that contain statements such as "An increase of 680,000 minke whales equals taking away the food for 80,000 blue whales" One might ask from where comes this 680,000 number? The answer is that it is the presumed difference between the number of minke whales now – which the ICR asserts is 760,000 – and the number that ICR says were living "in the past", which ICR defines as before the beginning of pelagic whaling in the Antarctic in the 1920s – 80,000.

[94] Masaki, Y. (1979) "Yearly Change of the Biological Parameters for the Antarctic Minke Whale." *Rep. Int. Whal. Commn* **29**: 375-95.9

[95] Another way in which some vertebrates, at least, exercise density dependence of net reproduction rate is by changes in the sex ratio, with males increasing relatively under good feeding conditions. "Regulating Food Intake" *Biol. Lett.* 10/1098/rsbl.2005.0430 (2006) – see also *Science* **311**: 579, 2006.

[96] Gambell, R. (1975) "Variation in reproduction parameters associated with whale stock sizes" *Rep. Int Whal. Commn* **25**: 182-9.

at lower latitudes – feed mainly on copepods, so serious direct competition between then and the fins (and blues and minkes) is not entirely plausible.

Masaki's conclusion was, roughly, that the age at maturity of male minke whales declined from about 12 years in 1945 to about 6 years in 1970; for females the decline was from about 13 years to about 5 years in the same period (From his Figure 8). In that period the total biomass of baleen whales was supposed to have halved. However, the biomass also halved, apparently, from the onset of Antarctic whaling to the beginning of the Second World War, when Antarctic whaling practically paused. Yet the transition layer data from the older whales in the 1970's Japanese catches, did not show any trend in age at maturity of either sex in the pre-War period, and therefore does not support the hypothesis of strong inter-specific interactions between these species populations of baleen whales.

The increase rate of a whale population, growing from a former period of equilibrium (steady-state, or stability) depends upon the difference between the relative rates of reproduction in the population and of mortality changing from zero to some positive number. If, as is usually assumed, the mortality rate itself does not change then one looks for explanation to changes in the reproductive rate. The only such change that has – possibly – been validated from data (mostly from commercial catches) is an increase in age at maturity. Estimation of just what rate of population increase such a change could cause is not straightforward. Although the changes that Masaki noticed seem to be large (a halving over 25 years) the reproductive rate itself depends on the age composition of the population. Masaki's data show that the majority of the animals in his samples were over 15 years old, ranging up to 50 years or more. Thus it seems unlikely that this change in the proportion of mature animals in the population could generate a very high rate of increase in it. Furthermore, since there was no apparent change through the 1920-30s when baleen biomass was reduced most vigorously by whaling, there is no evidence to support the idea that the minke whale population was rapidly increasing through that period.

ICR propaganda seeks to strengthen its case for culling minke whales by reproducing fake graphs showing an increase in numbers of minke whales, beginning around 1925. These graphs show a number of about 80,000 from 1890 to about 1925, then a rapid and continuous rise to a peak of 7-800,000 in 1970 when Japan and the USSR began pelagic whaling for this species on a large scale. There is no evidence, direct or indirect, for such a population growth, and the calculations on which the spurious ICR graphs are supposedly based have never been published. No citations of origin are ever presented in the propaganda material. One can only guess that estimates of the krill "not eaten" by diminishing numbers of blue and fin whales were presumed to have been taken up by the minke whales and some bizarre backward calculation made on the basis of this speculation. My term "bizarre" is justified because the graphs show the minke increasing in number from about 220,000 in 1945 to about 750,000 in 1970. This nearly three-and-a-half-fold increase implies a continuous population growth rate averaging between 10 and 15% per year (indeed, in the earlier part of that 25 year period the whales are supposedly increasing at more than 15% annually), a rate that has never been documented for any baleen whale species. And this despite the fact that in this process the minke whales are competing also with other baleen whales such as the fin and humpback, the krill-eating seals and seabirds, and certainly the abundant krill-eating fishes there.

It may be of some interest, as a sort of game, to look at what the krill-eating baleen whales might have been consuming in the Antarctic before their near-destruction by pelagic whaling. By T&O's own calculations (taking for example T&O's Method 2 estimates) a total of about **81 million tonnes** of krill and other crustaceans are now being consumed annually by 1255 blue whales, 85,200 fins and 761,000 minke (I leave aside the pygmy blues and the humpback for this purpose). One estimate of the number of blue whales before pelagic whaling started, quoted by the ICR, is 200,000. We have seen that the ICR figure for minkes then was 80,000. Estimates of the original number of fin whales that have been presented over the years to the IWC, and are "semi-official", are around 500,000.[97] According to T&O's consumption rates these would then have been consuming **330 million tonnes** of crustaceans, i.e. four times as much as now. One must wonder at what has happened to that huge annual "surplus" of about 250 million tonnes of krill! Have as a result all the other populations of krill-consumers grown explosively? Is it really conceivable that with such super-abundance of food available compared with pre-whaling times the minkes and blues and fins are competing so destructively for limited resources?

Of course, all this is fantasy. The numbers are not reliable and nor are the consumption rates per whale of each species. And in particular the numbers for pre-exploitation minke whales have simply been taken out of the ICR hat, like a conjuror's rabbit (see below).

In his 2003 paper referred to in my Introduction, and which was addressed primarily to a conference audience concerned about the future of the sea fisheries,[98] T. Tamura referred rather inconsequentially to another fear: that among the prey species of baleen whales are fishes that are also the prey of bigger, commercially valuable fishes, such as tunas. Tamura makes no analysis of this nor even provide a justification for such a fear, and discussion of it is outside the scope of this paper. It is however typical of his throw-away observations on this subject that Tamura writes, with respect to the Bryde's whale: "*There is at least one report of a Bryde's whale (observe the singular!* sjh*) that had been feeding on penaeid shrimp in the South China Sea. …Prey species of sei and Bryde's whales also varied both geographically and temporarily in the North Pacific. However, their prey species overlap with some commercial fisheries catch, so there may be an interaction to some extent in the North Pacific. In JARPN II, most of the Bryde's whales sightings occurred close to these fishing grounds (Could that have been because the scientific whaling programmes were designed to be concentrated on fishing grounds?*

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[97] 350,000 blue whales and 735,000 fins were killed in the Southern Ocean since 1904: "How many whales were killed in the Southern Hemisphere in the 20th century?" P. J. Clapham ad C. S. Baker, *IWC Doc SC/53/O14* (2005)

[98] Many, perhaps most, of the populations of small species of pelagic fishes that some baleen whales prey upon, have now been depleted by intensive and poorly regulated (or unregulated) commercial fishing (particularly for fishmeal prduction). A major concern is therefore whether sufficient food is being left for the wellbeing or even the survival of predators other than humans. The UN Convention on the Law of the Sea *requires* that fishing be regulated in such a way as to avoid such deleterious effects on "dependent species". There is as yet little sign of the signatories to UNCLOS doing that.

sjh*), they feed mainly on Japanese anchovy but do not feed on skipjack tuna (That should surprise no one* sjh*). The skipjack tuna is reported to feed on the Japanese anchovy.* ***There appears to be indirect competition between Bryde's whales and skipjack tuna in the western North Pacific.*** *"* (My emphasis)

The above quotation serves to illustrate one not uncommon but entirely erroneous presumption. Qualitative overlap of diet does **not** in itself signify *competition*, which means one thing having a significant effect on another, usually a deleterious effect, whether it be direct or indirect. For competition to become the subject of serious concern there has to be at least some evidence of substantial overlap in space and time, of significant consumption quantitatively, of what else is in the diet of the whales, of what else is in the diet of the tuna, whether the tuna and the whales are actually eating anchovies of the same size/age range, and so on. Yet this and similar remarks, repeated incessantly, were enough for a national representative - from the Western Tropical Pacific region - to the FAO/COFI meeting in 2005 to tell the assembled delegates that his Government has joined the IWC because it feared that under the existing moratorium on commercial whaling whales would increase so much that the valuable tuna fisheries of interest to his country could collapse. [99]

Such dialogue serves a duel purpose in addition to persuading governments with no interest in whaling, and little if any interest in the conservation of whales, to join the IWC and add their votes to demands for immediate relaxation of the few standing regulations aimed at conservation of whales *for the benefit of future generations*, as the Preamble to the ICRW 1946 specifies. The other purpose is to open the path towards eventual legitimisation of *unsustainable* whaling – and fishing, probably – and to provide excuses not only for increasing scientific catches of minke whales but for the killing of other, larger species – such as the Bryde's, sei and fin whales under Special Permits. One has to wonder how much this accelerating expansion of "scientific whaling" has to do with the facts that one Bryde's whale provides about four times as much meat to the market as does one minke, that a sei whale provides five times as much and a fin whale eight or nine times as much.[100] Thus, the earlier JARPA programmes produced each year nearly 2000 tonnes of meat from 440 minke whales. The earlier JARPN programmes added to this a further 300 tonnes annually from 100 minke whales. But the recent JARPN 9 yielded 1300 tonnes from the 100 minkes plus 50 Bryde's, and 35 sei whales. By adding in some of the bigger species the meat production from the North Pacific was increased more than four-fold, enough, perhaps, to shake the ICR and the residual whaling industry more independent from the large subsidy to "science" provided until now by the Government of Japan.

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[99] This absurd claim, mouthing a Japanese briefing, was recently dealt a blow when the Scientific Committee of the inter-governmental tuna commission responsible for management in that region reported that recent catches of yellowfin and big-eye tunas had been so high that over-fishing is now a serious concern.

[100] Data from Japanese market statistics and ICR press statements.

16 The Pestilence[101] of Junk Science[102] : Recapitulation, Conclusion

"Every lie must beget seven more lies if it is
to resemble the truth and adopt truth's aura."
Martin Luther, 1483-1546

Arno Karlin, whom I quoted earlier from his fascinating book about the relations between humans and the bacterium *Borrelia burgdorferi,* was writing about the irrational persistence of the flawed theory that diseases such as malaria (= bad air), syphilis and cholera were caused and spread by malodorous atmospherics and dirt, even after the discovery of the micro-organisms that really cause them. Such false beliefs continued in part because some groups within the human societies that harboured these diseases had vested interests in the persistence of the associated myths.[103]

We are facing, I suggest, a similar situation. It is a familiar gut reaction for a fisher or hunter to feel anger towards another predator that gets his intended prey before he does, and *in extremis* to kill the intruder if he has the means to do so. For centuries, even millennia, that has been a recurring encounter, becoming at times a battle between species. But what we see now, coming especially from certain sections of Japanese and also Norwegian and Icelandic societies are demands for total war – against whales, in fact against all the carnivorous marine mammals. The real aims are to be able to continue for a while to profit from the commodities that whales can supply to markets[104]. To support those aims a number of unsubstantiated claims are made. Some species, as a consequence of their deplorable fecundity – the minke is the current *bête noire* – might be impeding the recovery of other, threatened species that were nearly exterminated by previous efforts to gain profits and accumulate capital. Now those same monstrous marine carnivores are gobbling up the fish in the sea that rightfully belong to us. Soon, a sort of plausibility descends into farce when

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[101] ***Pestilent***: Destructive to life; injurious to morals etc., troublesome; annoying. OED

[102] Activities in the Southern Ocean and North Pacific under Special Permits for scientific research have been variously labelled in the media as "Pseudoscience" and "Fake science". My preferred term recalls the materials that are repeatedly thrust into one's home or onto one's computer screen, that are not wanted and cannot be used. Participants in the IWC, in FAO's COFI, in Conferences of the Parties to CITES, and some other international gatherings have long been pestered with the gruesome brochures put around by the ICR, by its parent in Government, the Japanese Fisheries Agency, and by its other parent, the Japan Whaling Association (JWA).

[103] Another myth closer to our topic here, of historical origin and still prevalent in some cultures, is that whales are fish. Since fish are generally supposed (wrongly) to be stupid it is not too difficult to believe that baleen whales really do feed by a sort of blind filtration process.

[104] Anyone doubting this might consider this statement on "Norway's policy on marine mammals" in the Report by the Ministry of Fisheries to the Storting (Parliament), as approved by the Council of State on 19 March 2004: "The purpose of the measures proposed… is to take political action to improve the profitability of whaling, sealing and the fisheries. Considerably better profitability…is an essential basis for rational and sustainable harvesting of marine." This action was followed by a doubling of the Norwegian take of minke whales under objections to IWC decisions.

representatives of the Government of Japan declare at international conferences that World Food Security will be threatened if we do not do something about the multiplication of whales or, alternatively, "harvest" them – unsustainably, of course - to build a sort of food bank.[105]

Such nonsense is, as usual, accompanied by frenzied or comical rhetoric (depending on one's viewpoint). With reference to the supposed high reproductive rate and voracity of the minke whales (in comparison with that of other whale species) the Alternate Commissioner of Japan to the IWC has publicly described minkes as "the rabbits of the sea". A couple of years later he revised his zoological nomenclature and described them as "cockroaches of the sea"! Such abusive, inflammatory language is, of course, the common accompaniment of irrational and pernicious political opinions, but – thankfully – now rare in international diplomatic discourse. Hearing it one cannot help being reminded of medieval demonising of Leviathans, cats, serpents and old women.[106]

So, let me now review what has emerged from my deconstruction of two documents that were intended to offer intellectual support for the Alternate Commissioner's Theory of Life.

First, it is obvious that T&O have been very selective of their sources. There is an abundant literature on the diet of whales, qualitatively speaking, dating from the early days of industrial whaling, when naturalists were commonly on board the ships. There are some quantitative data, too, though almost all data - older and newer - refer only to the frequencies of occurrence of various prey species in the guts of whales. Most stomachs examined contained only or mostly one type of food, and the numerical data merely specify the relative frequencies of those. Occasionally, when stomachs were found with more than one species in the partly digested contents, the numbers given are a frequency list, without regard to weights of the different species, their often partially and differentially

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[105] The matter of ensuring global or national food security is complicated. Eliot Slater, for example, commented thirty five years ago that if the southern blue whale stocks were permitted to recover to about half their original numbers they could sustainably yield about 100,000 tonnes of oil and 200,000 tonnes of meat annually ("General Introduction. Ethics and the Population Increase" in *Population and Pollution, ed* P. R. Cox and J. Peel, Academic Press, 1972). Recovery and eventual exploitation of the fin and sei whales in SHIO would probably triple that. On another tack, M. M. R. Freeman has argued that the Japanese *coastal small-type* minke whaling is relatively efficient in terms of fossil fuel energy use per calorie of energy in meat and blubber produced. ("Energy, Food Security and AD 2040: the Case for Sustainable Utilization of Whale Stocks." *Resource Management and Optimization* **8**: 235-44, 1991). Although as far as I know no comparable figures have been published for *pelagic* whaling in the Antarctic and other High Seas areas I doubt whether they are energy efficient in the same sense.

[106] The increasing use of the language of war, and attitudes going with that, to justify a sort of species-cleansing, has became the subject of correspondence in *Science*, see "The language of fighting invasive species", P. Clergeau and M. A. Nunez, D. Turner and M. Patterson, **311** (17 February 2006): 951. There the matter of concern is its use by "managers" and "conservationists" regarding the proposed extermination of "alien" species, including those originally introduced deliberately by humans.

digested state or their nutritive values. What is even more remarkable is that T&O apparently found little use for the 15 years of samples from the "scientific whaling" conducted by the Japanese pelagic fleet under ICR supervision.

Second, the ICR authors have been entirely uncritical of the sources from which they extracted their numbers and formulae. This is particularly true of the methods they used to calculate either food or energy intakes from body weights. Sergeant was trying to get an idea about consumption from pretty rough and very limited data, from species of toothed cetaceans varying over a sixteen-fold range of size, and he expressed his results crudely on a per-kg-body-weight basis, with the weights being calculated from rough or even guessed lengths. Innes *et al* pointed out that Sergeants data, for what they were worth, should be graduated as a power (less than 1) of the assumed weight, not to the weight itself. They were not especially interested in obtaining the statistically best estimate of the power exponent or of the most appropriate estimates of it for defined purposes. They were certainly **not** trying to find a valid way of extrapolating Sergeant's figures to animals two orders of magnitude bigger than, and with completely different diets from, those for which he had data.

Other authors, trying to make sense of energy-needs information for animals over a huge size range, have occasionally introduced confusion as to the meanings and implications of the many elements of energy budgets and definitions of various metabolic processes. This led to assumed additions to basal metabolic rate to account for such activities as Lockyer called "swimming about", and to the additions of various increments to account for energy consumption beyond basal needs being sometimes made twice. Had the authors of T&O 1999 and 2000 looked carefully at the origins of some of their assumptions instead of simply quoting them second-, third- or fourth-hand, they might have been more circumspect about the validity of their final figures. Similarly T&O were uncritical of the information they picked up from a few sources about energy densities of various prey species, and the few published figures for the partition of diet among several prey species.

Lets us now look again at the results. Although the global totals from T&O 1999 and 2000 do not differ excessively, the relative compatibility disappears when examined by species, species groups and Regions. Similarly, the results of applying T&O's Methods 1 and 2 (Method 3 being rejected) differ substantially as global totals, and greatly by species and species groups.

The population estimates cited by T&O for most of the smaller toothed cetaceans are more often than not little more than guestimates[107] or even unadorned guesses, as inspection of the publications T&O used to compile them quickly reveals. A major purpose of the ICR papers is to provide material to challenge the IWC's conservation decisions, particularly with respect to moratoria and sanctuaries, as well as to justify expanded scientific whaling and the recruitment of more fishing countries to Japan's coalition. As the IWC does not yet accept management responsibility for any of the smaller cetaceans except the orca and

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[107] "Estimate based on guesswork combined with reasoning" OED

the North Atlantic bottlenose whale, that large but heterogeneous group of species is not further considered here.[108]

T&O claim to be providing information that would facilitate comparison of cetacean food consumption with fish catches, Region by Region. But the definitions used for the three major Regions are quite different for the two sets of data. For T&O the North Pacific reaches, on the west side as far south as Brisbane, and the North Atlantic reaches on its eastern side to far south of the equator. Even within each of these vast areas the locations from which the data about the feeding of the whales are in many cases quite different and remote from the locations of the reported fish catches; this is especially true of T&O's mutilated southern hemisphere. In these circumstances such comparisons are not worth further consideration.

We have seen that the numbers of the large whales (baleen and sperm) of each species, overall and in each Region of the ocean, used by T&O are disputable,[109] and that some, such as for the most intensively studied of all - the Southern Hemisphere minke whale - may be wrong by a factor of three or more, and are probably greatly inflated.

The values used for average body weights are also disputable and more often than not inflated; the values from Trites and Pauly, used in the 2000 paper, are inappropriate and generally too high, and include absurdities, such as putting the average weight of the pygmy blue whales the same as that of the ordinary blue. We have also seen that further errors arise from incorrect treatment of the differences between the sizes of mature and immature animals, and between the sexes, and ignoring the variable ratios of these pairs of information even when it is available.

Enough has been said about how critical is the value of the power exponent[110] in trying to predict energy needs or food consumption over a 200 to 300-fold range of body size, and the care needed, in altering the exponent, to adjust the constant in the equations accordingly. Then, in applying their Method 2 (really Method b of S&V) to calculate food intake by mass from energy needs, they made the procedural error of making a coarse assumption about the prey composition in

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[108] The IWC does recognise, in the *Interpretation* section of the ICRW 1946, the pilot, beaked and the other three bottlenose whale species as "whales", but there is no consensus that binding management actions may be taken with respect to them, so they are not taken. It is an interesting piece of history that until the intensive exploitation of minke whales began in the Antarctic in about 1970 that species was classified as a small cetacean. The government of Norway at the time resisted discussion of management, even provision of catch statistics, on the grounds that the IWC does not regulate the catching of "small cetaceans".

[109] Until a few years ago the IWC Secretariat provided enquirers with tables of numbers of animals in each putative whale stock, where these were available, together with error estimates. Most of the latter gave lower and upper confidence limits around half and double the central ("best") value, including the values for minke in the six Antarctic Areas, separately, the sum of which provides the number used by T&O. This was, however, before analysis of the second IDCR series of surveys threw doubt on the central values.

[110] Note that even the decimal rounding of the value of an exponent can lead to considerable biases in extended extrapolations.

order to produce overall figures for the energy density of the prey, and then applied those results to the observed prey compositions by Region and whale species. The correct method would have been to look first at the composition of the prey, then apply appropriate energy densities to each component and only then convert the total energy-needs figures proportionately to a spectrum of consumption of prey items, by wet weight.

Within these simple calculations were other errors, the most significant probably being the assumption that baleen whales eating crustaceans are not metabolically very efficient because they were once thought not to be able to digest the chitin which comprises the carapaces of the krill, amphipods and copepods on which those whales mainly subsist. That error alone – which could have been avoided if they had studied the research publications of their Norwegian colleagues – led T&O to inflate their consumption estimates by 25%. But, again, the fault is perhaps mainly in not looking critically at the origins of the figure they used – 80% - for efficiency of utilisation.

That figure itself points to a major fault with this work. Throughout T&O 2000 figures are cited from previous authors and the impression is given to the reader that these are to actual observed data or verified relationships. In several places T&O claim that they have derived estimates using cited methods of others, but in fact have done no such thing: they have merely adopted unchecked numbers from those other publications. But, as one traces these back into the past, one becomes aware that many – maybe most – of these citations refer back through a cascade of citations to initial numbers or statements, sometimes made half a century or more ago, that are in fact either insecurely based on observation or, worse, were themselves mere *guesses*, or not even that, but simply *assumptions* made to permit certain calculations to proceed.

The essence of T&O's procedures for arriving at their desired conclusions is the multiplication together of a string of numbers, a "product chain". Inevitably there is a high statistical uncertainty attached to each number in the string, and as the multiplications proceed the uncertainties propagate and accumulate. The difficulty of evaluating the end result is bad enough when each of the numbers has a statistical error estimate associated with it. But in the present exercise the only numbers in the strings that have a few calculated confidence limits attached to them are some of the population estimates, and those few are very wide. [111]

In one published study an attempt was made to examine the relative importance of various errors in such multiplicative strings.[112] Its subject was not the cetaceans but the harp seal, to which virtually the same method of estimating energy needs and food consumption derived from these has been applied, as in

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[111] If each element in a chain has been estimated, and the errors of the estimates are normally distributed, then the standard error of their product is bigger (wider) than the biggest SE in the chain.

[112] P. A. Shelton, W. G. Warren and G. B. Stenson, 1997. "Quantifying Some of the Major Sources of Uncertainty Associated with Estimates of Harp Seal Prey Consumption. Part II: Uncertainty in Consumption Estimates Associated with Population Size, Residency, Energy Requirement and Diet" *J. Northw Atl. Fish. Sci*. **22**:303-15.

Method 2 of T&O. The results are revealing and might be applicable to the baleen whales.

The authors describe their primary objective as "…to determine which components contribute most to the uncertainty, as a guide for research planning. However, a thorough quantification of uncertainty would also be useful in evaluating alternative management options for harp seals which have the objective of reducing possible impacts on prey," in this case cod, Arctic cod (*Gadus ogac*) and capelin. They concluded that, notwithstanding the *considerable uncertainty in estimates of seal population size,* that *"is the **smallest** contributor to uncertainty in consumption*. It is also one of the few inputs for which a formal estimate of the uncertainty is available." Elsewhere in their paper they observe that the ***greatest*** contributor *is uncertainty about the **quantitative composition** of the diet.* They conclude, mildly but pointedly, with the remark that "Over-reliance on a point estimate should be avoided where possible."

Then there is the matter of bias. Some of the numbers in T&O's strings are evidently biased. A clear example is the figure for assimilation efficiency they have assumed. There is a likely bias, too, in the figures adopted for average body weight. We have seen how the complication of extrapolating regressions far beyond the data from which those regressions have been determined can lead to huge biases, depending not only on the value of the power exponent assumed but also on the calibration of the extrapolation by the constant of multiplication. The ultimate effect of multiplying together a string of biased numbers depends, of course, on whether the biases are mainly in the same direction or vary in direction and so tend to cancel each other out. In the former case it is not obvious to the untrained eye just how deviant is the end result; people not mathematically inclined tend to think of the errors being additive rather than multiplicative.[113]

When I began this critical examination of the T&O and related materials, in which there were some rather obvious mistakes, I thought it might be possible to come up with better estimates of whales' consumption of food using the same and additional data from the published literature, a literature that T&O had cited selectively. Several scientific colleagues to whom I expressed this hope told me not to bother, because even – unlikely – correct results would still have little bearing on the problem of calculating the effect on fisheries of allowing depleted whale populations to grow or alternatively culling them. Of course, they were right, but I still thought a better answer might be interesting, and very different from the tables of results given by T&O. But I concluded, in the end, that the available methods are so hazy (for this purpose at least) and the "data" so sparse and variable and "flaky" that indeed no such task would be worth the effort.

Nevertheless something can be said about comparisons of the kind made by T&O between the "use" of living marine resources by whales and the "use" of them, and impacts on them, by humans. Since I have, for practical reasons, excluded from this study the food consumption by toothed cetaceans we can ignore the consumption of cephalopods (and shelled molluscs) and focus on

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[113] As an example, suppose we multiply four numbers each of which is biased upward by, say, 50%. Then the result is not simply rather more than triple what it should be, but over fivefold.

T&O's categories of "fish" and "crustaceans", and even overlook the fact that most of the crustaceans and many of the fishes consumed by whales are quite different from those valued by humans. According to FAO the World Capture Production from the sea was, in the period 1999 to 2004, about *84 million tonnes/year*. Of these about *70 million tonnes were marine fishes and 3 million tonnes small crustaceans* (including 100,000 tonnes of Antarctic krill). We could add in some part of the one million tonnes of diadromous fishes such salmon that feed in the oceans (especially as one baleen whale has been observed to have consumed a salmon).

So, the relevant fisheries landings are about 74 million tonnes/yr. *Less than 20 million tonnes equivalent biomass from this quantity is eventually, one way of another, assimilated by humans.* (See Appendix 3 for details*) To obtain that up to five to ten times as much biomass is directly destroyed in fishing, mostly by discarding bycatch.* This naturally takes no account of indirect destruction by, for example, clearance of the mangrove nurseries of the shrimps and inshore fishes, destructions of coral reefs and other modifications of marine habitat for reasons other than improving biological production. I have also taken no account of the direct effects of bottom trawling on benthic habitat; these are a matter of ongoing scientific research and it might be premature to assume that all of them are detrimental.

Against this "harvesting" of about half a billion tonnes of marine resources in order to obtain 20 million tonnes of nutrients we may set T&O's Method 2 estimate that baleen whales consume about 15 million tonnes of fishes and 130 million tonnes of small crustaceans annually. Now, although I am not presenting revised estimates of those figures it is clear that in several respects their "best" estimates are much biased upwards, for example by the erroneous adjustments for assimilation efficiencies and the use of inappropriately high figures for average body weight, and wrongly chosen energy densities for various types of prey. My best guess is that a more realistic figure might be about one half of T&O's, *i.e.* about 70 million tonnes, so the same as the corresponding human landings and 3 to 4 times the sustenance that humans assimilate from those landings. And 90% of that consumption by baleen whales consists of euphausids and some other even smaller crustaceans in which humans have at present very limited interest as "resources". The consumption of fishes by baleen whales would be at most one half of the equivalent nutrients assimilated by humans, less than 15% of that landed from fishing vessels, and between 1 and 2% of what is "harvested", and mostly destroyed, by fishing.

All these calculations still tell us next to nothing quantitatively or ecologically about the interactions of whales and fisheries, about the impact of whales on fisheries or the impact of fisheries on whales. For that knowledge, and evaluation of its meaning for us, it is, as some might say, "back to square one!"

In these circumstances it seems to me irresponsible for some scientists, including Drs Tamura and Ohsumi, but not forgetting Drs Sigurjónson and Vikingsson, to draw public attention to the fact that there are some overlaps in the dietary constituents of humans and cetaceans, and to assert that "this looks like a problem", without at the same time pointing out that it might not be a "problem" at all.

With respect to finding out what whales eat, how much, and to what end and purpose, it is not sufficient simply to try to measure quantities. The baleen whales, like other predatory mammals and birds, at least, do not move around vacuuming up plankton and small fishes at random. They forage systematically, have their species and even individual dietary preferences, which change over time and space in ways and for reasons of which we humans have only the tiniest of hints. If we seriously wanted to try to predict what might happen if more whales were to die we would need to know immensely more about those processes than we do now, and at the same time have a greatly refined ability and knowledge to put such information into working models of real food-webs, even greatly simplified ones. Until that time comes we would be advised to learn how, and decide, to share this biosphere with an infinity of other creatures.[114]

So, the results produced by T&O, and so triumphantly used as propaganda and recruiting materials by the ICR and the JWA and the Japan Fisheries Agency, are based on the selective and often inappropriate use of few and ambiguous data, employing a flawed methodology uncritically, and incorporating several assumptions that have no basis in observations.

I have recently been greatly impressed with the wisdom of the astute and witty biologist Arno Karlen, aware that he has spent a life pondering on the smallest organisms that evolution has produced, I on the very largest of them, and I see similarities in the relations between them and us from one end to the other of that vast spectrum: "Obviously, germs (I read this as 'whales') do not deserve disgust or reflexive fear. Some must be treated warily and others thanked; all should make us attentive. The variety and ingenuity of their survival mechanisms is awesome. It is natural that when speaking of microbes that make us sick (read 'whales that annoy us') we fall into the rhetoric of warfare, as if bacteria (read 'cetaceans'), like the cholera miasma, stalk us in rabid rage (read 'with voracious hunger'). But that is just a metaphor for what it feels like to be on the other end of a natural balance. Pathogens ('hungry whales') lack malice; they are just trying to survive and sometimes they must do so at other creatures' expense. The same could be said of humans."

There is, however, one crucial difference between our relations with microbes and with whales. Once they become aware of their existence individual humans may become antagonistic towards microbes if they themselves fall sick or see their neighbours and families ill. **But modern humans tend to empathise with whales**, especially now that they rarely endanger small boats. We have seen in the last decades how people who have never seen a whale, let alone swum with them as some of we lucky ones have done, empathise with the ***notion*** of a whale. Those tens of millions who have watched whales, at sea or from comfortable shore, are usually enchanted by them and would certainly never want to deny them their nourishment.[115]

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[114] "Share the Planet" was the rousing slogan of the Center for Action on Endangered Species, an INGO that 'observed' IWC meetings for many years.

[115] In past centuries the large whales were commonly regarded as dangerous monsters, but in our time they and the smaller cousins, the dolphins, have been come to be appreciated for their grace and beauty. Wildlife photography and

Such antagonism as does exist has been created, stoked and fanned not by adversely affected individuals but by corporate bodies – including agencies of some governments - dedicated to the generation or defence of profit in the short-term. They have enlisted a sort of science in their service and have systematically distorted it.

The two papers from the ICR analysed here are key elements of that process. A remark by Voltaire is appropriate at this point: Those who can make you believe absurdities can make you commit attrocities."[116] The attrocities in this case are, naturally, mass killings ("cullings") of sentient non-humans – whales, seals and, I'm afraid, some terrestrial species – in the cause of profit but in the name of conservation and good management of "our" biosphere.

So let me grant the eminent and wise biologist Richard Lewontin the last word here. Of his fine book, ***The Triple Helix***[117] it has been said "It is a primer on the complexity of biological processes, a reminder to all of us that living things are never as simple as they may seem." Having likened scientists to "a medieval army that besieges cities for a period, subduing those whose defences are weak, but leaving behind, still unconquered, islands of resistance." Lewontin wrote:
"Science as we practice it solves those problems for which its methods and concepts are adequate, and successful scientists soon learn to pose only those questions that seem likely to be solved. Pointing to their undoubted successes in dealing with the relatively easy problems they then assure us that eventually the same methods will triumph over the harder ones."

Successful prediction of what would happen in the ocean, and to valuable fish populations, if whales and dolphins were to be removed, is one of the harder ones.

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film have enhanced that perception and encouraged whale-watching as a pastime and as a profitable industry. Roger Payne's discovery in the early 1970s that humpback whales truly sing contributed to the new 'notion' of a whale, although John Lilly'es fforts to communicate verbally with dolphins was looked at askance by most scientists at that time. That sperm whales and perhaps other species announce their individual names when they begin a sound train has been known for several years. However, as this was being written came news of experiments with captive bottlenose dolphins demonstrating that dolphin Alpha can address dolphin Beta about dolphin Gamma; if that is confirmed it brings us into another dimension of understanding communication among non-human animals.

[116] I am indebted to John Pilger for drawing my attention to this aphorism, though he was applying it to the rationales offered for recent wars. It is very hard to resist other quotes more or less to the point, so I won't try hard:
" For every credibility gap there is a gullibility fill." *Richard Clopton*
" Every man prefers belief to the exercise of judgment. *Seneca.*
"Why abandon a belief because it ceases to be true?" *Robert Frost.*
" The essence of belief is the establishment of a habit." *Charles Pearce.*
"Modern man has not ceased to be credulous.. *William James.*
" Nothing is so firmly believed as that which is least known. *de Montaigne.*
And, cutting both ways, "Reading made Don Quixote a gentleman, but believing what he read made him mad. *George bernard Shaw.*

[117] Harvard Univ. Press, 2000

17 Acknowledgements

Many scientific colleagues have contributed over the years to my knowledge of, and thoughts about, the subject of this document. I particularly wish to acknowledge the help and wisdom of Profs. David Lavigne, William de la Mare, Dr Justin Cooke, and the stimulation of conversations especially with Prof Daniel Pauly, Dr Kristin Kraschner (The Sea Around Us Project, UBC, Vancouver), Dr Michael Earle and the late Peter Yodzis. Mr Fred O'Regan and others connected with the International Fund for Animal Welfare (IFAW) – particularly Dr Vassili Papastavrou and Mr Joth Singh - have encouraged me to continue with this work. IFAW has provided much of the financial support for that. Dr Kees Lankester and Mss Melanie Salmon (Global Ocean), Margaret Bowman (Pew Charitable Trusts), Mary Carmel Finley and Leslie Busby (Third Millennium Foundation) added their encouragement.

Ms Jean Collins, the custodian and mistress of the excellent fisheries library in the Department of Fisheries of the Food and Agriculture Organisation of the United Nations (FAO), in Rome, and her small staff, patiently, promptly and cheerfully dealt with my many demands for help in finding reference material and providing copies of those that seemed to be worth reading carefully, as did Ms Sheryl Fink (International Marine Mammals Association (IMMA) in Guelph, Canada.

My son, Timothy, weighed in with some classy proofing and, not being a scientist himself, was in a good position to correct my syntax, and tweaking the graphics.

I wish also to recognise the encouragement and inspiration I gained from long associations with Brian Davies, Ms Patricia Forkan, the late Sir Peter Scott and the late David McTaggart, *Prime Movers* of, respectively, IFAW, the Humane Society of the United States (HSUS), the World Wildlife Fund (WWF) and Greenpeace International.

Postscript

Nos numerus sumus et fruges consumere nati
"We are just statistics, born to consume resources"
Horace, 1st century BC *Epistles* I i 27

I am indebted to Ian Boyd, of the British Antarctic Survey, Cambridge (see footnote 13), for reminding us that two thousand years ago Horace had much to say pertaining to the matters I have discussed in this paper. After having explained that one of his prayers had been for "…a parcel of land not so very large, which should have a garden and a spring of ever-flowing water near the house, and a bit of woodland as well as these", Horace - wrote Boyd - "considered the conflicting paradigms of ***use*** and ***delight*** for his garden."[118] Boyd noted that before Horace people "had battled with the difficult balance of utilitarianism versus aestheticism" and observed that "perhaps the justification for or against culling predators as a form of fisheries management has greater dimensionality than the complex food web models described by Yodzis. For a variety of reasons, marine predators appeal (to most people but obviously not to all *sjh*) more for aesthetic reasons than for their use. Whatever the ecological and economic arguments might be about how best to manage predator-fisheries interactions, ecologists and economists cannot ignore the shifting balance of public opinion**. *However elegant the ecosystem models turn out to be, they are nothing without public support for their underlying assumption that* use *outweighs* delight** ". Whale watchers take note!

It seems to me that the increasingly bitter "whaling controversy" is not only - or perhaps even mainly - about a conflict between use and delight. It is equally about a conflict of interest between use and/or delight now and that *choice* in the far future. If the Southern Ocean ecosystem were to "recover" from the hammering of a century of commercial whaling (as the French proponents and the other supporters of the Southern Ocean Whale Sanctuary argued in 1992-94), by being allowed to "rest" for another century, and if future human generations so decided, the baleen whales there could yield, sustainably, far more value than is garnered now from the stocks of small pelagic fishes in the northern hemisphere, which the ICR says are now threatened by whales.[119]

Economists of conventional mind who apply high discount rates to their evaluations of resource values might dispute this. But the Royal Society of

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[118] At the end of the 18thC AD Voltaire seemed to haves imilar ideas about his estate on the French-Swiss border.

[119] There is one snag in the expectation or hope that protection of the Antarctic/Southern Ocean ecosystem will lead eventually to the baleen whales becoming as abundant, overall, as they were up to the twentieth century. This is that there is evidence that the abundance of krill may be changing as a result of climatic changes, especially those causing a retreat of the Antarctic ice edge, and hence its shortening. This was detected first by William K. de la Mare from his historical study of the positions of pelagic whaling factory ships, but subsequently confirmed by satellite observations ("Abrupt mid-twentieth-century decline in Antarctic sea-ice extent from whaling records". *Nature* **389**: 57-60, \997. "Sea ice meltdown" *New Scientist* , 6 September 1997, p 4.)It seems that the phytoplankton on which the krill subsist are in the winter locked into, under, the ice edge, released during the thaw and so give rise to rapid expansion of the krill populations close to the edge.

London has apparently now taken another view of that process. In a provocative *op-ed* in the *New Scientist* about the costs of avoiding or ameliorating human-caused climate change, entitled “No way to run a planet: Blinkered bean-counting will sell our climate short”[120] the Editors noted that the Royal Society was now arguing that conventional economic analysis is not up to the job of assessing the effects of long-term (climate) change. Traditional methods, they say, give an inadequate representation of how people value the future, and make the impacts of…change beyond a few decades seem almost irrelevant. Those methods also fail to include factors that cannot be easily accounted, such as social, economic and environmental impacts.”

Japanese – and Norwegian – whaling interests are looking *only* to the maximisation of short-term profits. Notwithstanding their platitudes about “sustainable use” their real intentions are, I think, revealed by the steady escalation – and, in the case of Japan, geographic and species-by-species expansions - of their whale hunts under Special Scientific Permits and under objections to IWC decisions, since the declaration of an indefinite pause in commercial whaling beginning in 1986. Those hunts are contrary to the spirit and intent of the UN Convention on the Law of the Sea and other relevant UN legal instruments which mandate that any killing of Highly Migratory Species must be managed, and in a precautionary manner, by the appropriate inter-governmental organization which, in the case of the whales – all of which are designated in legal terms as Highly Migratory - is the IWC. In this scramble for limited profits precaution and orderly use of marine resources have been abandoned, as has the notion of international cooperation in that enterprise.

And to that end, lying by the wayside, is the wounded body of honest science.

Through the past decade critics have expounded repeatedly on the ethical, legal and political aspects of the conduct of commercial whaling on a substantial scale through unlimited and unilateral issuance of Special Permits for the alleged purpose of scientific research. Attention has also been given to the general opinion of most members of the IWC Scientific Committee that the research conducted in this way can contribute little if anything to improving management of any commercial whaling that might in future be authorised by the IWC. Less attention has been given, however, to what happens not so much at sea as inside the Institute for Cetacean Research (ICR) and associated institutions and bodies that are at the core of this activity. The analysis presented here may be regarded as one step towards correcting that imbalance.

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[120] 11. February 2006, p5.

Appendix *I* *Marine Pollution Bulletin,* March 2006.

Viewpoint: Propaganda and Pretext

This *Viewpoint* is not about contamination of the ocean and its inhabitants by chemicals, sound or other forms of intrusive energy, but rather about verbal pollution, distortions and incompetence. My subject is whaling, fishing, the conservation of whales and the sustainable use of marine living resources.

For two decades the governments of a few countries whose vessels and nationals were engaged in commercial whaling controlled the International Whaling Commission (IWC) which had been created in 1946 to regulate whaling and conserve whale resources for the benefit of both present and future (human) generations. That the IWC – labelled as a "whalers' club – failed in all those tasks is widely known. In the third decade of its existence (1970-79) those same states, augmented by a few new whalers, counterbalanced by a few new non-whalers, continued to hold control to the extent that the whalers controlled enough votes to block most proposed conservation measures. The outside world – represented as the UN General Assembly - had meanwhile called upon the IWC to consider declaring a precautionary ten-year moratorium on all commercial whaling. Whaling governments, such as Norway, that had supported the UNGA Resolution, of course took the opposite view within the IWC, and continued to block action until 1979. In that year, however, the whalers lost their stranglehold and a proposal by the newly independent Republic of Seychelles to ban commercial whaling from the Indian Ocean was adopted.

Two years later all catching of sperm whales was banned, and the following year a pause in all other commercial whaling was declared, on another proposal by Seychelles, not for ten years but indefinitely. This decision – commonly, but erroneously, called "the moratorium" - applied to all whale species for which the IWC accepts responsibility, i.e. all baleen whales and, among the toothed whales, the sperm, the northern bottlenose and the orca. These supposedly binding decisions were made by the necessary three-fourths majority of voting IWC Members. The critical point had arrived when, early in 1982, some of the countries conducting whaling from land-stations to supply the Japanese meat market let it be known, very discreetly, that they would phase out their operations if given sufficient time – three years - to meet their advance contracts to supply meat and to make other social readjustments. The crux was that one of these – Spain - voted in favour of the pause and by so doing cast the decisive vote. Japan, Norway, Iceland and the USSR voted against the 1982 decision and subsequently all of them but Iceland filed "objections" to it, as the ICRW gives them the right to do. The Icelandic Parliament over-rode, by a single vote, the Government's intention to object. And although the USSR had objected as a matter of principle it did in fact also cease it's whaling in the Southern Hemisphere, which had until then been conducted solely to serve the Japanese market and earn hard currency. Other Members engaged at the time in commercial whaling also voted against the pause but did not object to it and they all subsequently ceased operations, some of them - notably Brazil and Chile - becoming among the strongest advocates of continuing the pause in whaling.

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The delayed general "moratorium" came into effect in 1986. The "recruitment", by Seychelles, and other countries, of Indian Ocean coastal states such as India, Kenya, Oman and Egypt, to IWC membership, in order to implement their national interests in marine conservation, had encouraged other non-whaling states to do likewise. But Norway, having exerted its right to "object" to the moratorium, as well as to a declaration by the IWC that the minke whales in the northeast Atlantic were depleted and therefore "Protected", continued its operations. Japan, which had also "objected", withdrew its objection as part of a deal with the USA that would allow Japanese fishing vessels to operate for a while in the US Exclusive Economic Zone (EEZ) in the North Pacific under licence, but it continued its whaling through a loophole in the ICRW which allows Member states to award their nationals unlimited numbers of Special Permits for the professed purpose of scientific research. Such permits have no constraints with regard to the species of whales to be killed, their location, the duration of the killing programme, the method of killing or the type of individual whale (e.g. its size, whether it is a calf or nursing mother etc.). (1)

In the year that Japan began large-scale "scientific whaling" its authorities decided to launch a secret exercise to "turn around" several of the countries that had supported the moratorium decision in order to ensure that a blocking vote (one fourth plus one of voting Members) would be able to prevent any further conservation moves. (2) They thought they had succeeded by 1993 but in 1994 failed to block the declaration by the IWC of the entire Southern Ocean as a whale sanctuary, following a proposal by France. This hitch was only temporary, however, and in any case the Southern Ocean was, in both political and scientific terms, a very special case, linked to the efforts of many Antarctic treaty powers to delay (for at least fifty years) exploration for, and exploitation of, minerals on the Continent. Thereafter, once assured of a blocking one-fourth, the Japanese authorities decided to move towards gaining a simple majority with which to block even non-binding "unwelcome" IWC decisions and, as soon became apparent, to dismantle the fragile structure of conservation measures that had been erected since 1972. This was called, in Japan, "vote consolidation" and by others "vote buying" (see References 1).

Readers of *Marine Pollution Bulletin* might be surprised at the attention now being given to this process. Various means of persuasion have, after all, been used by richer or more powerful states to ensure the support of the governments of weaker or poorer states throughout the array of intergovernmental negotiations and decision-making, not least in the United Nations system. However, what makes the IWC different is that the Japanese efforts have continued for two decades, with respect to what is, after all, a rather trivial and economically unimportant issue in world affairs. This is not the place to speculate on the deep reasons for this apparently irrational behaviour by the national authorities of one of the world's richest and most influential countries. But from one perspective it should be realised that the protection of endangered whales has been a global "flagship" environmental campaign since 1972, and, from another perspective, Japan's actions have involved the systematic corruption of a particular field of application of marine science: the management of sea fisheries. That such corruption was possible raises serious questions about the way that scientific advice is formulated and used – or not used – in international marine affairs, and in particular the inherent weaknesses and flaws in the structure of the IWC and of its Scientific Committee.

Japan did of course have another option: to withdraw from the IWC and continue its commercial whaling virtually unimpeded. But this could have had adverse political repercussions, not least to flout the imperative under the Law of the Sea for management and conservation to be assured by intergovernmental instruments such as regional and specialised fisheries management commissions. Fear of those repercussions did not, however, prevent the authorities from repeatedly threatening withdrawal from time to time. (There were precedents for this, notably withdrawals by Norway and The Netherlands, in the years before 1972, when they did not like IWC decisions.) Instead, however, and to create an illusion of seriousness and responsibility, Japan constructed a rationale for its scientific whaling programme. This was, initially, that it was needed in order to provide better estimates of the parameters of mathematical expressions used to provide scientific advice regarding the application of the New Management Procedure (NMP) that the IWC had adopted in 1974 (formally in 1975, coming into effect in 1976) following the UNGA Resolution of 1972. In particular the claim was that the scientific whaling programme would, by allowing the taking of unbiased samples of whales, provide estimates of the natural mortality rate of minke whales in the southern hemisphere. This claim was subsequently shown by the IWC's Scientific Committee to be unrealisable. By the 1990s a revised claim was being made: that data would be provided to make more efficient the application of a Revised Management Procedure (RMP) for the regulation of the catching of baleen whales that had been developed by the Committee and adopted provisionally, though not yet implemented, by the Commission itself. This claim, too, was quickly shown to have no substance and was not upheld by the Scientific Committee.

Some time in the early 1990s a decision was taken in Tokyo to work for a simple majority in the IWC. There were now few if any governments left to "turn", so the strategy would have to be to persuade more countries to join the IWC and strengthen the Japanese-led coalition. The persuasion used in all cases was the false assertion that recovery of whales and the failure of the IWC to re-open commercial whaling constituted a threat to those countries' fisheries. The "vote consolidation programme" went into second gear and, by the time of the 2005 IWC meeting in the Republic of Korea, the simple majority had been achieved. Or, at least the Japanese authorities *thought* it had been. Again it turned out that there were some hitches such as the required payments not being paid on time, credentials of delegations not being in order, and accessions to the IWC of some other countries adhering to the other, non-whaling coalition. Unless there are more adhesions to the latter coalition in 2005-2006, Japan will in 2006 control a secure simple majority – and government officials have publicly declared that intention, and indicated in some detail what they will do with it.

So what will Japan do with its simple majority, since it cannot (without controlling three-fourths of the votes, an extremely unlikely development) end the current pause or abolish existing sanctuaries, as it would like to do. Previous speculations about this were proven correct when the aims were revealed at the meeting in Korea. They include: ensuring that future voting is by secret ballot (in the expectation that this will help take public pressure off the politicians and delegations of countries that back Japan in the IWC); abolishing the recently established Conservation Committee; removing from the IWC's Agenda annoying subjects such a whale-watching, humane killing and consideration of proposals for more sanctuaries, specifically in the South Atlantic and the South Pacific; and instructing the Scientific Committee to cease its ongoing consideration of the conservation status of the smaller cetacean species. We could also expect a cascade of Resolutions deeply critical of all other conservation measures, such as sanctuaries and of catch limits that might be set, outside sanctuaries, under the extremely conservative and precautionary version of the RMP adopted but not implemented 14 years ago. Another speculation, to which I do not personally subscribe, was that Japan would act to exclude all international non-governmental organisations (INGOs) from attending IWC meetings as observers. (3)

While all this political manoeuvring has been going on, and closely associated with it, Japan has been steadily expanding its "scientific" whaling activities. They are now conducted also in the North Pacific (this helps the cost-benefit calculations for year-round pelagic whaling by a single fleet of one factory ship and a group of catcher boats). The annual catches of minke whales have been doubled, and special permits awarded also for the catching of Bryde's, sei and sperm whales and the endangered humpback and fin whales. These expansions will contribute to the profitability of these operations if and when the large government subsidy to them is discontinued or reduced, especially as one humpback, Bryde's, sei or fin whale yields meat equivalent to several of the much smaller minke whales.

As in earlier years the Japanese Government feels the need to give a plausible but spurious "scientific" justification of its new strategy. This takes the form of excavating an old claim, dating from the 1970s, and renewing and updating it, that the minke whales (which official Japanese sources insist have been increasing rapidly, despite the complete lack of scientific evidence for this) are impeding the recovery of other depleted species such as the blue whale, and even of the humpback and fin whales, (despite the fact that at least the humpbacks, which were almost exterminated, *are* increasing).

A different justification is needed, however, for the activities in the North Pacific. This is found in the fact that some of the baleen whales in that Region – especially the minke – eat significant quantities of fishes, some of them of species that are exploited commercially by Japan and some other fishing nations. Claims that the whales are a threat to fisheries coincide with the revelations by the Food and Agriculture Organisation of the UN (FAO) and by other bodies and analysts that there is in fact a global fisheries crisis, although the general opinion is that this is due mainly to over-fishing with perhaps contributions from both natural and man-made environmental changes. Apart from other intentions, this claim allows the Japanese authorities to be more persuasive of yet more countries, none of which have ever previously shown the slightest interest in whales or whaling, to join the IWC. But they go further. In recent years we have seen the remarkable sight of those same members of the Japanese supporting group in the IWC playing exactly the same role in bodies such as FAO's Committee on Fisheries (COFI) – in many case with delegations composed of the same people (Holt, 2005). Accompanying these activities is an unending spate of propagandistic materials declaring whales to be a prime cause of the fisheries crisis.

I think the underlying strategy of the Japanese authorities in this matter goes beyond the now revealed issues mentioned above. The "whales-are-eating-our fish" argument is a pretext for "culling", to prevent the recovery of depleted populations and reduce others. It is a policy of deliberately *unsustainable* whaling. In the present state of the world's whale stocks only levels of commercial whaling that are biologically unsustainable could possibly provide some short-term profits without substantial subsidy. In the case of pelagic whaling by Japan the subsidy of course is in the partial government funding of "research"; the rest of the "research" costs are explicitly covered by the proceeds from sale of the whale meat obtained, a device that was certainly not envisaged by the drafters of Article VIII of the ICRW. If we may assume, reasonably, that the subsidised whaling by Japan has been just balancing its books, then it is evident that the greatly increased numbers of minke whales to be killed in the 2005/06 southern and the 2006 northern hemisphere seasons, together with the significant numbers of larger species of baleen whales, will make these operations profitable with a reduced subsidy or perhaps even with none. Any other biologically sustainable and economically profitable pelagic whaling would necessarily have to await the recovery under protection of, at least, the fin, sei and blue whales in the Antarctic and the North Pacific: that will take several decades, at least.

At this time in world affairs it needs a strong argument to swim against the idea of sustainable development and the sustainable use of wild living resources such as fishes. "Culling" predators claimed to be harmful to human interests unfortunately appeals to many people with little appreciation of the complexity and essential unpredictability of ecosystems, particularly marine ones. Robert A. Heinlein's comment, attributed to the Notebooks of Lazerus Long, 1973, is appropriate here:

"The Truth of a Proposition has nothing to do with its Credibility. And vice versa".

To support this "culling" strategy, and justify expanded "scientific whaling" involving looking inside the stomachs of many more whales, the Government of Japan has, through its Institute for Cetacean Research (ICR) in Tokyo, distributed a number of pseudo-scientific documents purporting to show that whales consume immensely more "living marine resources" than are caught by humans, and that this makes them a threat to human welfare. These papers have been very widely used as propaganda but never peer-reviewed or published except in numerous glossy pamphlets issued by the ICR, and quoted in many press releases and briefings all over the world. The claim is itself false but is also essentially irrelevant to the question of whether or not there really is a serious competition for food resources between whales and humans.

Critical analysis reveals that the ICR documents contain stupendous mistakes of method and errors of calculation, highly selective use of limited data, grossly misleading "conclusions" and other evidence of scientific incompetence. An institute that produces such materials, and uses them to prop up a national strategy to further deplete, for more short-term profit, already stressed marine resources, should not be treated by the international scientific community as a legitimate *research* body (see also Gales 2005).

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Notes

1. The Icelandic authorities made the same decision and conducted "scientific whaling" for several years. That was eventually stopped by pressure from the people of Iceland, and the huge cost to country whose economy depends on fishing, of boycotts of fish imports from Iceland by traders in Europe and North America, encouraged by vigorous consumer campaigns by environmental NGOs, principally Greenpeace International.
2. An attempt to bribe the Government of Seychelles (to dismiss its delegation and to vote with Japan contrary to its original conservation policy) failed because its President resisted and went so far as to publish the exchange of messages between his Government and the diplomatic representatives of Japan. The proposed pay-off was more than twenty million dollars A few years later Seychelles decided simply to leave the IWC rather than be submitted to further continual diplomatic and financial pressures
3. It is true that Japan has from time to time sought, and failed, to make specific exclusions – these have included IUCN, IFAW, Greenpeace and others – but exclusion of *all* would cause a financial crisis in the IWC because several hundred iNGOs have to pay exorbitant annual fees for the mere privilege of sitting in the conference room and not distributing documents or talking to delegates while doing so!

Appendix 2

Marine food webs relevant to culling proposals

On the next page is reprinted, by kind permission of Prof. David Lavigne, a diagram produced by D. Huyek for the International Marine Mammal Association, (See D. M. Lavigne, 2003. *Marine mammals and fisheries: The role of science in the culling debate.* pp 31-47 *in*: Gales, N., M. Hindell and R. Kirkwood (eds.). *Marine Mammals: Fisheries Tourism and Management Issues*. CSIRO Publishing, Collingwood, Victoria, Australia.) It is one of several compiled by IMMA, another being for the Benguela ecosystem that was used to illustrate Peter Todzis' article in TREE, referenced in footnote 13. This one is **a partial representation of the Scotian Shelf system in the Northwest Atlantic off eastern Canada.** It is incomplete because the feeding habits of all components have not yet been fully described. Furthermore, not all species shown spend entire years of their life cycle in this area. ***Species enclosed in rectangles are those exploited by humans.*** Web-like diagrams illustrate one idea in ecology, that "everything is connected to everything else" and therefore warn strongly against pursuit of simplistic arguments such as those in the ICR documents. Perhaps equally important is that the numerous connections within the web are not all the same strength. It might one day be possible to draw the lines defining the connections in different thicknesses and colours, thus making distinctions of power, direction and other features.

An unintended consequence of such diagrams is that they can reinforce a notion that systems such as so-called Large Marine Ecosystems (LMS's) are "each a single unanalysable structure of interactions which cannot be broken down into parts in any way without destroying what is essential to it." These are the words of Richard Lewontin,[121] who continues "But extreme holism fails as a program for biology for two reasons. First, even if it were true that everything is strongly connected to everything, that should not be confused with the methodological claim that no success at all in understanding the world or in manipulating it is possible if we cut it up in any way. Such a strong methodological claim we know to be wrong as a matter of historical experience. Second, the holistic claim is wrong as a description of the world. Everything is not effectively connected to everything. The world is divided into nearly independent subsystems within which there are effective interactions but between which there are no palpable relations. The problem of science is to find the boundaries of these subsystems. Moreover, the boundaries of the subsystems within which there are significant interactions change with circumstances. The definition of effective subsystem boundaries is a major practical task for the biologist in all circumstances."

The IMMA diagram is commended to readers of this document with these thoughts in mind.

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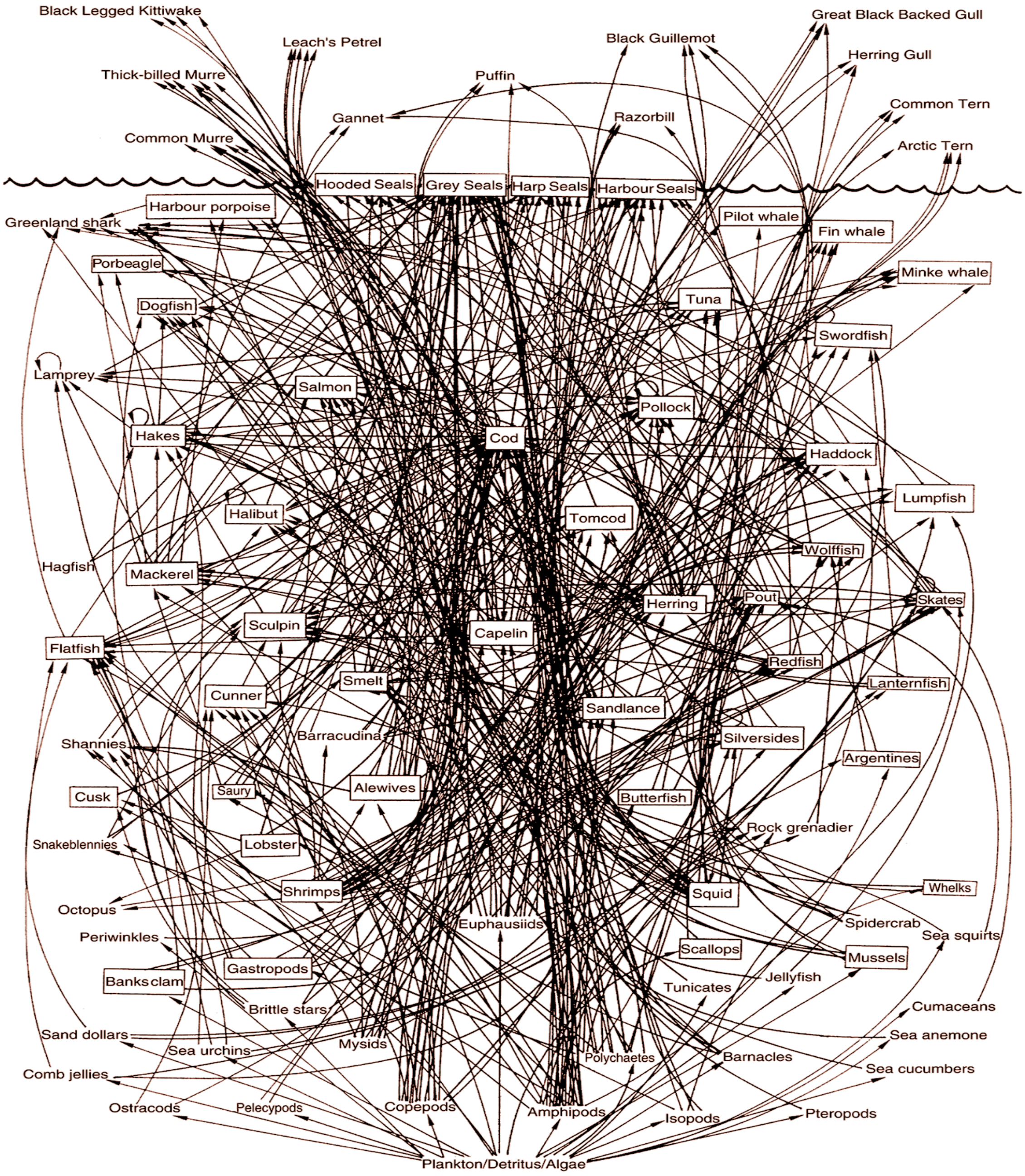
[121] "The Triple Helix" Harvard Univ. Press, 2000 136 pp. See Chapter IV *Directions in the Study of Biology*

Northwest Atlantic (Scotia Shelf) Food Web (partial)



Appendix 3

How much of marine life do humans consume, metabolise or destroy?

Crude comparisons of estimates – even good ones - of food consumption by whale populations, with records of fish landings, even when they are broken down by incompatible geographical areas and by broad coarsely defined species assemblies such as "fishes", "crustaceans" and "cephalopods", do not make sense. Those groups of marine animals constitute the sole diet of the baleen and other whales, while produce from the sea, significant in human diets and important in some locations and circumstances, nevertheless has a limited role in human nutrition.[122] And, as far as we know, the removal of some quantities of those organisms by the foraging of whales has little or no other effect on the marine environment. (It does, of course, have some effect, in some places, on fishing activities by, for example, whales becoming entangled in fishing gear, probably through trying to remove fish from those gears; that problem is not our concern here). But the ways humans acquire food from the sea, and the processing and utilization of that food, have consequences far beyond the direct effects of removing certain quantities of selected species.

In briefly reviewing those consequences it seems sensible to start with the recorded landings themselves. These, as given in the statistical publications of FAO and national governments, are nowadays usually derived quantities. Some primary processing is done at sea (filleting and so on) and the recorded quantities of products are re-converted by FAO to "wet weight." What (physically) happens after that is not well documented, and certainly not globally; what we mostly see is the prices the catches/products fetch in the wholesale markets and beyond.

I propose to look at the problem of the catch-all term ***wastage*** in and by sea fisheries first (**A**) with respect to what happens to fish after being landed, and, second (**B**), during the fishing process itself.

A. After landing

One useful publication is Robin Roy's "Wastage in the UK Food System".[123] This is probably rather out-of-date (published in 1976). The findings reported may or may not be typical of affluent countries, and surely are not of the world as a whole. But the processes described are, I think, more or less universal in market economies, although the figures will differ from country to country.

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[122] S. J. Holt "Marine Fisheries and World Food Supplies, p 77-96 *in* "The Man/Food Equation", *eds* F. Steele and A Bourne, 1975. Academic Press, 289 pp.

[123] Sub-titled "An analysis of the flow of food in the United Kingdom and of the losses incurred within the system". Earth Resources Research, London. This followed up a discussion paper published the previous year, "Losing Ground" which had voiced doubts about the long-term food security of the UK. It was based on review of scattered literature and some new surveys. It also includes an account of an examination of the same problems in the USA during World War II.

First we should note that about 40% of all "notional" landings from the sea, of fishes and the smaller crustaceans, globally, are reduced to fishmeal, with some associated production of fish oils. That becomes 42% if the crustaceans, which are generally too valuable to be reduced, are excluded.[124] The meal is almost all used as supplementary feed for livestock, mainly poultry and freshwater fishes raised in ponds and fenced streams. There are various published and unpublished estimates of the conversion efficiency in these activities, ranging from about 10 to 20%; let us say for illustrative purposes 15%. Thus the effective marine supply for human consumption, as fish or as poultry and cultured fishes fed on fish, is not the 73 million tonnes of fishes and smaller crustaceans landed annually but just over 50 million tonnes.

Roy first looks at *Wastage in production.* About 1% of fish landed by British trawlers was rejected at the market either because they were varieties that are currently unacceptable to customers or because they did not reach a minimum price at auction. However, he notes, not all the rejected fish were entirely wasted; some were used for animal feed.

Next, *Wastage in processing and distribution.* In this category the first item is *Loss in storage and transport.* Although figures were given for several farm crops none were available for fish. However, within this category is the sub-category of *Business losses*, due to *evaporation* and *contamination*. "Fresh" fish, along with meat, poultry and vegetables, is subject to evaporation but, again, no estimates of its scale were available. However, Roy points out that wholesalers and retailers are particularly concerned about water loss because they buy and sell by weight and because the food texture (and hence the price) may be affected. Nutritionists, on the other hand, would not count evaporation as a loss since water does not contribute to the nutritional value of a food.

The second item in the chain of processing and distribution is *Wastage in processing*. In the UK 70% of food consumed had been processed; 80,000 tonnes/year of waste was produced by the food industry. "But although processing leads to wastage, it can also mean that more of the plant or animal is utilised than would otherwise occur." Roy describes some extremes of "deliberate" wastage in the vegetable industry in the USA, but then points out that "At the other economical extreme is the almost complete utilisation of fish in a highly sophisticated freezing industry in which trimmings from fish fillets and less acceptable varieties of fish are made into fish fingers. Even the residues from the fish from the fish finger making process are sent to Scandinavia where they are used to make fish soup." Only about one third of the British food industry's wastes are utilised, but what part of that fraction not utilised consists of fish is not estimated. "It is economic factors, especially the cost of transporting

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[124] FAO's "The State of World Fisheries and Aquaculture 2004 states that " in 2002 76% of estimated world fish production was used for direct human consumption. The remaining 24% was destined for non-food production, in particular the manufacture of fishmeal and oil." However, the total from which these percentages are derived includes all inland/freshwater fisheries and all produce from aquaculture. I have made appropriate adjustments. It may be noted that the FAO figures, from our point of view, also include a form of double accounting, since the aquaculture totals come in part from the use of feeds derived from the capture fisheries

and drying wet wastes that are the main obstacles to utilisation. Wastes that are utilised are usually termed *by-products*."

Although in fish processing there may be almost complete utilisation of the edible parts of the fish, **"the edible portion represents only some 40-50% of the landed weight.** The 50-60% waste, mainly heads, backbones and skin, goes for the manufacture of animal feed. Sometimes up to half of a trawler's catch of fish has to be sold for fertiliser manufacture simply because the fish in the bottom of the hold are crushed by the weight of fish above them." I would add here that such loss by crushing is not uncommon in seine fisheries for small pelagic species such as the Peruvian anchoveta that are prosecuted solely for fishmeal production, although the loss by crushing begins even before the haul is brought on board the fishing vessel.

Roy's next heading is *Wastage in Marketing*. He remarks that information about this is in general hard to obtain since detailed records are not often kept. One figure he has is that "Wastage at Billingsgate, the London wholesale fish market, is believed to be about 0.3% of the fish offered for sale, mainly due to condemnation by inspectors." As for wastage at the retail stage, that is very variable, depending on the kind of retailer, whether a large supermarket or a small independent shop. In supermarkets the wastage of frozen foods (which would include most of the fish) is said to be only about 0.2% It is said that some managers reduce the wastage by reducing the prices of perishables reaching the end of their safe storage life and by selling off left-overs and damaged produce to staff at cut prices. Roy notes that wastage is probably less in supermarkets than in small independent shops, but regrets that "no major surveys of retail wastage appear to have been made" at the time of writing.

Roy's last category is *Wastage in consumption*, and that seems to be the most important from our present point of view. "More food is lost or wasted after purchase by the housewife[125]or caterer than at any other stage in the food system. Various surveys have shown that 5-10% of the *edible* food (measured by calories or weight) purchased by housewives is simply thrown away as kitchen or plate waste. The young and the old tend to throw away more, and some caterers may find themselves discarding up to one third of the edible food they buy. Perhaps it is fortunate that only 5-10% of the food in Britain is eaten in canteens and restaurants." Wastage also occurs in the preparation of food, for example when fat is melted out of fish or when food is left stuck to cooking utensils.

In the sub-category *Wastage in food preparation* we get down to more verifiable data. With respect to fish the three species most commonly marketed in the UK are considered: plaice (a *Pleuronectid* flatfish), herring and cod. In a Table of *Material discarded during preparation of food (Percent bought weight)* the percentages of waste of these three items are, respectively, 70, 45 and 30%. These figures are from several sources reporting *inedible* waste.

In the next of Roy's sub-categories, *Losses in cooking*, it is noted that "Boiling or shallow frying fish may give rise to protein losses between 14% and 50%,

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[125] I do wish he had written "houseperson"!

through crumbling. Discarded fats and juices from cooking meats and fish are one of the main sources of wasted calories in households."

Kitchen and plate waste: "Estimates of the amount of edible food thrown away by householders and caterers vary widely", Roy writes. "A pilot study of civil servants indicated that in this category about 5% of the edible calories were discarded. This compares with an estimate of 7-10% from American middle-class households in 1960, and 2-3% from British households in the 1930s. Higher income families tend to waste more food than those with lower incomes. Very little data exists on kitchen and plate waste in catering, but earlier surveys indicated that such wastage there is higher than in households. "Figures of 5-8% of purchased edible weight for plate wastage alone were recorded for British and US army catering and 17% for US student catering during World War II. Surveyed schools in Wales showed *plate* waste to be somewhat less than 10% of the available food, but much higher losses arose in the *kitchens* from the poor organisation of catering. A synthesis table shows that in two US surveys the kitchen and plate waste in households, for meat, poultry and fish, as a proportion of purchased food was about 8% by weight but 18% by calories.

It is not easy to pull all these sequential stages of loss together for the purpose of this analysis, and of course the data are fragmentary as well as biased towards the culture of affluent countries, although they do take into account the behaviour of the relatively poor within those countries. My guess is that we may not be very far wrong is we assess that of the global landings of fish about one third is inedible and of the edible fraction a half reaches the human gut and nourishes its owner. **This would give the human population an adjusted annual intake of the equivalent of roughly 20 million tonnes of food originating from marine fish catches.**

B. Before landing

We may now retrace our steps to the point of landed catch. To obtain the 70-80 million tonnes in order to provide only a quarter of it to human nutrition, a vastly greater quantity of what T&O and ICR call "living marine resources" is. It is worth mentioning here that some of the fish caught is not landed, it is consumed at sea by the fishers themselves and certainly never enters official statistics.[126] I have, however, found no estimates of the quantities.

People are by now more or less familiar with the fact that different modes of fishing result directly in the deaths of large numbers of seabirds such as albatrosses,[127] and of turtles, large sharks, several of the smaller species of cetaceans and even a few large ones, and that this endangers the very existence

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[126] From my limited experience in North Sea trawling that which is eaten at sea, as well as that taken home for the fishermen's family, as part of the "perks" of the occupation, includes high quality species

[127] See *e.g.* "Sudden Death on the High Seas. Longline fishing: A global Catastrophe for Seabirds." American Bird Conservancy Undated; 2002 or later. 16pp. See also "Tangled in he food web". By N. Holy. *Earth Island J.* **Spring 2004**: 25-30, and "Interactions between fisheries and seabirds" by W. A. Montevecchi, p527-57, in *The Biology of Marine Birds* eds E. A. Schreiber and J. Burger, 2001, CRC Press LLC, Boca Raton, Florida USA.

of some species.[128] Many of those "accidental" catches can be avoided, technically, and efforts are underway at least to reduce some of them. Although some of these impacts on marine ecosystems might be quite severe, they probably do not represent very large chunks of biomass. I would guess – and I'm afraid it is only a guess, because accounts of such kills are nearly always given only in terms of numbers, not biomass – that adding them all to the landed catches would at most give us a round figure somewhat less than 100 million tonnes.

All types of fishing gears catch fishes and other organisms that are not specifically targeted, but both bottom and pelagic trawls and dredges generally take more than others. FAO does not publish statistics either of catches or fishing effort according to type of gear used, but my not very well-informed guess is that about half the recorded global landings come from bottom- and mid-water-trawling. The unwanted catches that are discarded at sea (mostly but not all dead) [129] are called by some *by-catch* or *discards* or *incidental catch* (as distinct from *accidental catch* mentioned above) and by others *trash*. (This last term is also applied – at least by British fishers - to inorganic "rubbish" such as rocks , as well as dead coral and old tin cans, plastic bottles and non-biodegradable fishing lines and nets that increasingly come up in trawls.) It is not necessary here to go into the subtle, but in some circumstances important, distinctions between these forms of waste; I shall use the term ***by-catch*** for all of them for the present purpose.

The discards, by the way, include valuable fish that are unintentionally caught in mixed-species fishing over and above catch limits in force for certain species in many fisheries while continuing to fish for those species of which the limit has not yet been reached.[130]

Much of the organic by-catch consists of species that are not edible by humans or, at least, not much liked except perhaps *in extremis*. Such by-catches include corals, worms, jellyfishes, small molluscs and the like, the quantities and composition of which are rarely recorded except by naturalists on research vessels. But most of the by-catches usually consist of species for which there is either no market at all or which are of much lower unit monetary value than the target species, but not necessarily lower caloric or general nutritional value. There are various circumstances that cause catches to be regarded as by-catches. There may be no local market for them, for instance. They may be more perishable than the targets. But most commonly retaining them would take up limited and therefore precious fish-hold space. Designation of a *catch* as a *by-catch* is an economic decision.

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[128] It has been estimated that shrimp trawlers alone kill about 150,000 sea turtles annually.

[129] FAO has recently published a review of the escapement of fish from trawls during the hauling- in process as part of the campaign to reduce unwanted catch: "Mortality of fish escaping trawl gears" by Petri Suuronen, 2005 *FAO Fish. Tech. Pap.* **478**, 72pp.

[130] Unintentional fish mortality is also a feature of "ghost nets", usually bottom-moored gill- and trammel-nets that have been lost but continue to catch fish. Lost, abandoned, non-biodegradable, lines, seines and trawls also kill many animals, especially cetaceans that become entangled.

In the present context the most important feature of by-catches is that they are often bigger – in weight and/or in nutritional value – than the recorded landings, in some fisheries very much bigger. One recently published review concluded that the global bycatch of commercial fisheries is at least 20 million tonnes, i.e. a quarter of the recorded landings. Other studies have found the discarded catch to range from 26 to 64% of the total coastal catches in the NW Atlantic.[131]

Another useful summary of data for some fisheries was published in 2003 by the Environmental Justice Foundation, entitled *"Squandering the Seas: How shrimp trawling is threatening ecological integrity and food security around the world"*.[132] Notwithstanding its sub-title that review covers more than shrimping. A summary table lists ten fisheries – all trawl fisheries, of course – with the highest records of by-catch relative to landings. One of the ten is the trawl fishery of the Northwest Atlantic, where this ratio is six times. The other nine are all shrimp fisheries in various locations. The ratios for these range from *eight* up to *fifteen*, although ratios as high as twenty-one have occasionally been found in some shrimp fisheries. Overall, shrimp fisheries in temperate waters have a by-catch of *five* times the landings of shrimp; in tropical waters the average ratio is *ten*. The discarded catches include up to 400 species, and where they have any market value at all it is commonly about one-thirtieth/kg that of the shrimps.

Shrimps constitute only about 3% of global landings from the sea, but shrimping accounts for one-third of the global discards. The annual (wild) shrimp catch by the twenty major producing countries totals about 2.7 million tonnes, (2000 figure) of which 38% is taken by China [133]. The tabulated data do not include discard estimates for the Chinese catch, but since about 56% of the global catches entered the import-export trade (mostly to the USA, European Union and Japan) giving very high profits in hard currencies, it seems unlikely that the discards from the Chinese operation would be much different from those others for which information is available.

More shrimps are caught in tropical and sub-tropical waters than in temperate waters.. Let us suppose, then, that the average ratio of discarded by-catch to landed shrimp catch is about *eight*. The discards from shrimping alone therefore total about 20 million tonnes. If the EJF estimate is correct, that discards from shrimp fisheries are about one third the total by all fisheries, then the "best" discard estimate reaches 60 million tonnes.

Such biomass totals do not, of course, fully reflect the consequences of this form of waste for the fish stocks. Very many of the "trash" fish caught are the young of species that become valuable in their own right when they have grown bigger,

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[131] "Oceans at Risk: Wasted catch and the destruction of ocean life" by T. Dobrzynski,
C. Gray and M. Hirshfield, 2002. Washington DC, 28pp.

[132] The EJF is a London-based NGO (info@ejfoundation.org)

[133] The other very big producers are India (13%) and Indonesia (10%)

and even in some cases are the adults of such species that are simply not so pricey as the shrimps. [134]

The EJF report gives a good example of this matter with respect to the red snapper (*Lutjanus* sp) of the Gulf of Mexico., the young (0-1 age) of which share the shrimping grounds there. In 1995 the shrimp trawlers caught and discarded 34 million young snappers, while the fishing industry caught only 3 million bigger ones.[135]

Conclusion.

The annual "consumption" of living marine resources by humans, in terms of biomass of catch and kill is probably of the order of 150-200 million tonnes annually, that is up to ten times the amount of biomass of marine origin that enter the human nutrition system.

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[134] One instructive analysis on relative values of commodities from multi-species fisheries was recently written by J. R. Nielsen and C. Mathiesen "Stakeholder preferences for Danish fisheries management of sand eel and Norway pout" *Fisheries Research* **7**: 92-101, 2006. These small fishes are exploited in the North Sea and elsewhere solely for reduction to fishmeal. Fishers interested in fishing for human consumption are opposed to the reduction-fishery because it removes the food of birds and of valuable fishes. The industry also is the cause of large bycatches of haddock and whiting. Sand eels are relatively important in the diets of minke whales in some places and in some seasons. They occasionally also eat pout (*Trisopterus esmarkii*), as well as the young of haddock and whiting (*Merlangius merlangus*).

[135] The importance of this waste for future food security and the state of marine ecosystems is now internationally recognised. The UN General Assembly in 1994 affirmed that "the issue of bycatch and discards in fishing operations warrants serious attention by the international community." FAO and UNEP are taking various steps to encourage the use of Turtle Excluder Devices (TEDs) and Bycatch Reduction Devices (BRDs) in fishing gears, particularly in the context of FAO's Code of Conduct for Responsible Fisheries (CCRF). There are, however, obstacles still to be overcome. The present generation of such devices is said, for example, to reduce the efficiency of shrimp trawlers and to significantly increase fishing cost – and hence reduce profits.

Appendix 4

Contents of
Protocol for the Scientific Evaluation of Proposals to Cull marine Mammals

Report of the Scientific Advisory Committee of the
Marine Mammals Action Plan (of UNEP and FAO)
Funded by UNEP, WWF, IFAW and Greenpeace October 1999