Scientific review of issues raised by the Memorial of Australia including its two Appendices

Lars Walløe

April 9, 2013

About the Author

Lars Walløe is currently Professor Emeritus of Physiology at the University of Oslo. He took his MD and a PhD in physical chemistry in Oslo in 1965. He was Professor of Physiology at the Faculty of Medicine of the University of Oslo from 1988 to 2008, and before that Professor of Applied Statistics and Mathematical Modelling at the Faculty of Science from 1970 to 1988. He has also held part-time positions as Research Director at the Institute of Marine Research in Bergen and Professor at the Department of Arctic Biology at the University of Tromsø.

His current research is in the field of cardiovascular control mechanisms in man and other large mammals (reindeer, seals and whales), and he has developed non-invasive ultrasound instruments for such studies. He has also published papers in the fields of sensory neurophysiology, neuronal nets, robust statistical methods, historical demography, population biology, reproductive epidemiology, and on methodology in the natural sciences.

Professor Walløe is President of Academia Europaea (the European Academy of Science and Humanities). He is Scientific Adviser to the Norwegian Government on Marine Mammals and also scientific director of Norwegian research on marine mammals, a post he has held since 1986. He has also been President of the Norwegian Academy of Science and Letters, Chairman of the Norwegian Population Panel, Director of the Norwegian research programme on acid rain, Chairman of the Norwegian Research Board for Environment and Development, Chairman of the Standing Committee for Life and Environmental Sciences of the European Science Foundation, and Panel Chair for Applied Biology in the European Research Council. Further biographical details are given in Appendix A and at http://folk.uio.no/larswa/e_index.html .

My understanding of my mandate as expert witness under the Rules of the Court

I have been asked by the Government of Japan to prepare an independent report providing a scientific review of certain issues raised by the Memorial of the Government of Australia, dated 9 May 2011, in the case *Whaling in the Antarctic (Australia v. Japan)* before the International Court of Justice. I was in particular asked to consider certain questions relating to Appendix 2 of the Memorial. This contains an independent report by Dr. Marc Mangel of the University of California Santa Cruz, bearing the title *An Assessment of Japanese Whale Research Programs Under Special Permit in the Antarctic (JARPA , JARP A II) as Programs for Purposes of Scientific Research in the Context of Conservation and Management of Whales.* The Government of Japan also asked me to provide this independent Expert Opinion in preparation for possible appearance as an expert witness under Article 57 of the Rules of Court in the above case.

I have chosen to present the issues I was asked to consider under the following five headings:

1. Comments on the restrictive and therefore misleading general understanding of the concepts 'scientific research' and 'scientific method' as presented by Dr. Mangel in Appendix 2 to the Australian Memorial, including comments on the claim that Japanese scientists have not determined the necessary sample size by 'established' statistical methods.
2. Comments on the claim that all information necessary to improve the management of whaling could be obtained by non-lethal methods.
3. Comments on the claim that research whaling carried out under Article VIII should be restricted to research which could improve conservation of whales and management of whaling.
4. Comments on the criticism of other parts of the lethal research carried out under JARPA and JARPA II.
5. Comments on important scientific developments since the two memorials were prepared.

In considering the approach to follow, I have not received any particular instructions or guidelines beyond the questions listed above. I have thus not been given any guidance as to choice of methods or principles in support of scientific arguments, nor have I been given any indications as to the range or depth of scientific evidence to be provided in the report. This statement is addressed to the Court, and I assume that the judges do not have in-depth scientific knowledge of the field. For this reason I have not found it necessary to set out detailed scientific evidence, but have tried to indicate in general terms which deliberations my views are based on.

A discussion of these issues involves complex conceptual and mathematical questions, notably in probability, induction and statistical inference. In particular, concepts such as power analysis and statistical significance are important in marine ecology, population dynamics and sampling, including choice of sample sizes. Although these issues may underlie complex scientific components of the scientific research programmes under consideration in this case, the question is which conceptual frameworks will be of practical assistance to the Court in delivering its judgment.

I have chosen not to include a scientific overview of statistical topics, concepts and methods, nor any references to the ample scientific literature and related evidence or the usual apparatus of statistical and other scientific references, as I consider that they may not necessarily be helpful in this context. Nor have I included a large amount of factual and scientific material. While I would also be prepared to answer oral questions, I concluded that my independent report would be of most practical assistance to the Court if, on the basis of transparency as to my own background and experience and according to my best judgment, it answered the key questions I have been asked as directly possible, based on the scientific arguments raised in the Memorial, and particularly its Appendix 2. To some extent this approach is also dictated by the methodology and assertions of Dr. Mangel in his Appendix. I believe it is possible to discuss some of these issues in ordinary language, and have attempted to do so. I have also chosen to include examples drawn from various fields of work in which I have been engaged, particularly cetacean population dynamics. However, there are also examples from scientific fields that may at first glance not appear to be directly related to whaling, but that in my opinion nevertheless may help to shed light on some of the issues discussed.

Scientific review

Background: I have read the Memorial of Australia, including its two appendices, and the Counter-Memorial of Japan. I have also read both the original and the modified research proposals for JARPA and JARPA II submitted to the Scientific Committee of the International Whaling Commission (IWC), and a number of the scientific papers which have been published from these research programmes. I participated in the three first (open) evaluation meetings of the Japanese research programmes.

I have attended the meetings of the IWC Scientific Committee every year since 1987, from 1989 as head of the Norwegian delegation. From 1990 onwards, I have also been a member of the Norwegian delegation to the annual IWC Commission meetings, and I have taken part in a large number of special IWC meetings arranged either by the Scientific Committee or by the Commission itself. From 1995 to 2009 I chaired the Scientific Committee's Sub-Committee on Bowhead, Right and Gray Whales, which prepared advice on safe catch limits on two of these species for the Russian and the US (Alaskan) aboriginal hunt. During the last three years (2010-2012), I have chaired the Sub-Committee which has been dealing with the problems connected with estimating minke whale abundance in the Southern Ocean. After about ten years of difficult scientific discussions, the Sub-Committee was able to reach agreement on estimates last year (2012), and these were subsequently accepted by the full Scientific Committee.

This is an Expert Opinion devoted to science and scientific methods. In light of the highly politicized context of some topics, I believe it is fair to briefly characterize my own emotional and intellectual views on whaling, although this is not an issue here. I consider that whaling is an ethically permissible human activity similar to the hunting of large terrestrial mammals, provided that two conditions are fulfilled. These are that the hunt must be biologically sustainable, and that animal suffering during the hunt and the killing must be kept to a minimum, comparable to the level we accept in the hunting of large terrestrial mammals. Contrary to popular belief, there is no scientific evidence that whales or dolphins have more

highly developed mental abilities (intelligence or emotions) than large terrestrial mammals like pigs, moose, wolves and elephants.

1. Comments on the restrictive and therefore misleading general understanding of the concepts 'scientific research' and 'scientific method' as presented by Dr. Mangel in Appendix 2 to the Australian Memorial, including comments on the Australian claim that Japanese scientists have not determined the necessary sample size by 'established' statistical methods

Dr. Mangel's Appendix includes a ten-page chapter called "Characteristics of a program for purposes of scientific research" (Memorial, pp 349-359), which presents the author's general understanding of "scientific method". This interpretation is in my view too restrictive and does not provide an adequate description of research as it is carried out in different biological fields. Dr. Mangel claims that *"in accord with generally accepted principles of scientific practice a program for purposes of scientific research:*
a) Has an over-arching conceptual framework that leads to a set of focused questions (hypotheses);
b) Employs the correct set of empirical tools to answer the questions including setting sample sizes with sound statistical reasoning, and linking mathematical models and data appropriately;
c) Has proper assessment through the community of scientists." (p 350).

This is perhaps an adequate – though somewhat idealistic and oversimplified – description of research in a fairly advanced biological field in which there are generally accepted hypotheses about the main functional connections in the system under investigation. However, not many biological disciplines are this advanced at present, and especially not ecological disciplines.

Existing knowledge about the Southern Ocean ecosystem is very limited. This makes it difficult to tell which observations are likely to be important for an understanding of the dynamics of the system. In this situation, all physical, chemical and biological oceanographic data that can be collected may be considered potentially valuable, not only data relevant to specific hypotheses.

Of course there are always general hypotheses behind any collection of primary data, which suggests to the researcher the type of data that might be of interest at the time or in the future. However, these underlying hypotheses are often vague and not easy to formulate in scientific language. Thus, Dr. Mangel is, in this context, in error to claim that: *"--science does not consist of simply accumulating data. Indeed, we now often face the problem of data 'poisoning' by having too much data and too little understanding"*, and further *"Simply put, the essence of science is to extract knowledge from data and, if one does not know in advance how the data will be analyzed to extract such knowledge, one is not ready to collect the data."* (p 350). The research carried out under the JARPA and JARPA II programmes includes both data collection to test specific hypotheses and collection of data to provide background primary data in physical and biological oceanography, which may be valuable in the future. It is easy to provide examples from the history of biology of research projects

during which data collected without any specific hypothesis in mind turned out to be of great value for an understanding of the dynamics of the system under investigation.

Let me take one well-known example from the history of biology: Gregor Mendel (1822-1884) published his article "Versuche über Pflanzen-Hybriden" (Experiments in Plant Hybridisation) in 1866. His experiments showed that the inheritance of certain traits in pea plants follows particular patterns, today known as Mendel's laws of inheritance. These subsequently became the foundation of the biological discipline of genetics. But Mendel did not start his work with clearly formulated hypotheses. And he did not confine himself to studying inheritance in peas, but also tried to find patterns of inheritance by cross-fertilising varieties of mice and bees, but without success. Between 1856 and 1863 he cultivated and studied some 29 000 pea plants. He started with 34 varieties of one particular pea species which he obtained from different plant breeders, and cross-fertilised the different varieties. Some properties of the hybrids were intermediate between the properties of the parent plants, but others showed a different pattern. After years of observations he decided to use for his further experiments 22 varieties of peas with characteristics that remained constant through the generations. In his final experiments, he studied seven of these traits in detail. It is very clear from his description that most of the time was spent on detailed observations, trying to identify patterns in what he observed, which is not at all surprising, since the prevailing idea at the time was that hybrids should show properties intermediate between the parents. Only at a late stage did Mendel formulate his two laws as hypotheses.

Another example comes from environmental research in which I was involved myself. In the early 1970s, the public and scientists in Norway noticed that first salmon and later trout seemed to be disappearing from lakes and streams in Southern Norway. The general hypothesis was put forward that the fish were being killed by acidification of the water caused by long-range transport of sulphur compounds from British and German factories and power plants. A large-scale Norwegian research programme was established by the Ministry of the Environment (SNSF 1974-1980). I was appointed its scientific director from 1976. Using methods from medical epidemiology, we soon established that the geographical distribution of fish mortality was as expected on the basis of geology and deposition of acid from the air. However, the British authorities did not accept the results. Their claim was that fish kept in captivity could survive much higher levels of acid than those they were exposed to in Norwegian lakes. So another major research programme was established (the Surface Waters Acidification Programme (SWAP), 1986-1990), this time involving collaboration between the Royal Society, the Norwegian Academy of Science and Letters and the Royal Swedish Academy of Science. I continued as head of the Norwegian part of this programme. The programme soon confirmed the main findings from the Norwegian programme, but the question asked by the British authorities remained unanswered. The scientists then started to collect all kinds of data from the acid rain, from the soil and from the runoff. They were searching for a possible unknown factor which could explain the death of the fish. They looked for various organic molecules and different metal ions which were known to be toxic, without results. The random analyses finally showed that the toxicity was correlated with the concentration of aluminium ions in the water (released by the acid from the soil). The result was unexpected, since previously aluminium was considered to be non-toxic. However, further investigations showed that it was a special molecular form of aluminium which was the toxic component. This example clearly shows that a random search within a

large set of possible chemical and physical variables, i.e. a broad collection of data, may yield important results without a specific hypothesis as the starting point. Prime Minister Margaret Thatcher accepted the results on behalf of the British government at a Royal Society meeting in London in March 1990, where the Norwegian and Swedish Prime Ministers were also present.

Today powerful computer programs exist that can be used for such 'exploratory data analysis', or 'data mining', as it is sometimes called. Their development was initiated in 1977 by the distinguished American statistician John W Tukey (1915-2000). He made fundamental contributions to statistical practice and articulated the important distinction between *exploratory data analysis* and confirmatory *data analysis*, believing that much statistical methodology placed too great an emphasis on the latter. 'Far better an approximate answer to the right question, which is often vague, than an exact answer to the wrong question, which can always be made precise' (Tukey 1962). Dr. Mangel's comments in his methodological chapter on *'the problem of data 'poisoning' by having too much data'* (p 350) and on *'exploratory analyses' which 'rarely work'* (p 351), clearly show an outdated approach to this development. The projects carried out under JARPA and JARPA II are in my view a good mix of projects carried out to test specific hypotheses and projects in which data were collected without predetermined hypotheses with the intention of investigating whether any interesting information could be extracted. In reality there are no sharp boundaries between the two types, but a continuum from precisely specified hypotheses, through problems which are precisely formulated, but not in the language typical of a hypothesis (e.g. has there been any change in the age of sexual maturity of minke whales during the JARPA years?), to sets of variables that were recorded because the scientists suspected or hoped that some interesting information would materialise. For example, as I understand it, the morphometric and stomach contents data were collected without any prior hypotheses or assumptions, and have nevertheless yielded very interesting scientific results.

One very important finding, based on both genetic and morphometric data from JARPA, is that minke whales in the area between 35°E and 145°W consist of two stocks, which may be called the Indian Ocean stock and the Pacific stock. They mix during the summer feeding season in an area around 160°E, but separate again when they migrate north to their breeding areas in the austral autumn. The results show no indication of further subdivision of the two stocks, which is a significant finding because of its implications for possible future implementation of RMP.

These results are very important for the implementation of the Revised Management Procedure (RMP), and in general for the management of minke whaling in this area. Two other sets of results, based on systematic measurements of blubber thickness and on stomach contents, respectively, show that blubber thickness and stomach contents have decreased substantially over the JARPA period (and the beginning of the JARPA II period), when controlled for a number of other variables which influence these measurements. These two time series show that substantial changes must have occurred in the Antarctic ecosystem during the last twenty years.

Dr. Mangel claims (p 362) that *'the only clearly identifiable hypothesis in JARPA or JARPA II is the krill surplus hypothesis, according to which the over-harvesting of the great whales lead to a krill surplus, which in turn lead to an abundance of minke whales.'* He quotes Murase et al (2006), who wrote that the krill surplus hypothesis *'has been central theorem of the Antarctic ecosystem study'*, and claims that *'In describing the possible 'krill surplus' as a 'central theorem', Murase et al suggest that it has already been proven.'* Murase's choice of words may have been unfortunate, but it is certainly not the case that Murase or other Japanese scientists regard the krill surplus hypothesis as proven. In many papers they discuss the relative merits of this hypothesis and other possible explanations, such as changes in the ecosystem caused by global warming. The krill surplus hypothesis is an interesting hypothesis, but it is definitely not *'the only clearly identifiable hypothesis in JARPA or JARPA II'*.

Setting sample size

A representative example of Dr. Mangel asking for 'an exact answer to the wrong question' is his treatment of 'Setting Sample Sizes' (p 352). He writes: *'Setting the size of a sample of data to be taken in order to estimate an unknown parameter so as to test a hypothesis depends on:*
a) how accurately the parameter needs to be known (how close the average value of the estimate is to the unknown parameter);
b) how precisely it needs to be known (how much variation surrounds the estimate of the average value); and
c) what kinds of statistical assessments will be done with the data.
Formal statistical methodology provides procedures by which the sample size required to obtain a specified confidence that we have in a particular conclusion can be determined.'

It is correct that formal statistical methodology provides procedures for determining the required sample size. However, this requires very strict conditions to be fulfilled, for example the statistical distributions of the variables under investigation must be known both under the 'null hypothesis' (≈ no change) and under the 'alternative hypothesis' (≈a change of at least a certain specified magnitude). Meeting such conditions is usually unrealistic in environmental and fisheries research.

Even in scientific fields where it is reasonable to assume that the necessary background information is available, statistical power analysis, as this discipline of statistics is called, turns out to be beset by difficulties. This can be illustrated with an example from medicine. Let us assume that we have an established pharmacological treatment for a common condition, e.g. high blood pressure, and that a new drug is developed for the same condition. At a certain stage in the development the new drug will be tested in a formal statistical test ('double blind', randomised) against the old. How many patients of a certain kind would we have to recruit? This is the question about sample size. In this case it is in theory possible to answer all three questions a), b) and c) above. Since different patients give different responses (e.g. fall in blood pressure) to the same treatment, we need to know the distribution of the responses in the patient population, and we also need to know (or assume) the distribution of the responses in the same population under the new treatment. Then we have to decide how small an improvement we are interested in detecting, and the

probability of being able to detect that the new drug is better than the old (called the power of the test). If these conditions are fulfilled, we can calculate the necessary sample size. However, even in this type of clean situation in medical research, the calculations very often turn out to be misleading, and subsequent analyses show that the sample was far too small (because the patient population turned out to respond differently from what was observed previously). For this reason, many clinical trials are now carried out either by increasing the sample size considerably above that calculated by formal methods (a 'precautionary' approach), or by using sequential statistical methods, where no sample size is determined beforehand. I have together with a graduate student developed a sequential version of the common Wilcoxon (Mann-Whitney) two-sample test, which is now used in many clinical trials because of the difficulties involved in calculating sample sizes.

It can be difficult enough to determine the necessary sample size for one precise hypothesis in environmental sciences, because the distributions of the variables are often not known with sufficient accuracy. The problems increase considerably when the investigation involves many hypotheses and also important scientific questions which the researchers wish to investigate without yet having established precise hypotheses (*exploratory* investigations). In this situation, the standard statistical theory would have to be used for each of the precise hypotheses, with some addition to sample size as precaution to account for any incorrect assumptions about the statistical distributions. The final decision about sample size would then have to be the largest of the different sample sizes determined for each hypothesis.

Many of the sample sizes determined in this way by the Japanese scientists yield numbers in the range 600–1000 minke whales per year or more for JARPA II. I have repeated the calculations for a few of the variables by making a set of necessary assumptions, and believe that the numbers are of the right order of magnitude for many of the scientific questions when the aim is to obtain clear results within a period of six years. For other questions the samples are clearly too small, but in such cases a longer observation period may give interesting results, as illustrated by the blubber thickness and stomach contents data series mentioned above. Longer data series have also proved to be needed for population modelling.

There are additional considerations that must be taken into account when sample sizes are determined. When research proposals are discussed in research councils or other funding bodies, an important question is always whether there is sufficient funding available to carry out the research. It would be impossible to carry out a major research programme in the Southern Ocean using a large research vessel with crew and bunker oil, often at sea for many weeks, without also ensuring some income from the operation. The importance of this is generally accepted in fisheries research, where research vessels are often issued with quotas for the relevant fish species to fund at least part of the research. For example, this has been the case for Norwegian research on Greenland halibut, and to a lesser extent for Norwegian research on mackerel, cod, herring, saithe and haddock. Some of the fish which is caught is of course used in the research, but the rest is sold on the market to cover part of the costs of the research programme. This is far from uncommon practice in other fishing countries as well. I have specific knowledge about the Russian fisheries research in the Barents Sea. Japan has chosen to cover part of the costs of its whale research programmes by selling whale products on the commercial market. To obtain sufficient income in this way,

the yearly catch has to be of a certain magnitude. Again, a catch of the order of 800–900 whales per year does not seem to be unreasonable, on the clear condition that there is absolutely no chance that this will result in overexploiting the minke whale stocks. This condition has been established beyond dispute for the two minke whale stocks in the relevant area of the Southern Ocean.

Having said this, it must be admitted that the Japanese scientists have not always given completely transparent and clear explanations of how sample sizes were calculated or determined, and on reading the research proposals for JARPA and JARPA II submitted to the IWC Scientific Committee, I often had the impression that sample sizes were also influenced by funding considerations. However, even larger sample sizes than 850 would have been advantageous for some of the scientific questions. Furthermore, results that do not reach the magical 5 % significance level may also, in retrospect, be found to add to scientific knowledge. Depending on the circumstances and subsequent analysis, such results may indicate anomalies or variations that are worth investigating further. Setting a 5 % level is to some degree discretionary and based on convention, and results with significance probabilities in the range 5-10 %, and the corresponding confidence intervals, still carry appreciable weight.

The Memorial states that *'a proper interpretation of the words "for purposes of" as used in the phrase "for purposes of scientific research" /Article VIII, 1/ requires that the activity be assessed to be genuinely motivated by an intent to conduct scientific research, and not for any other purpose or purposes.'* I cannot speak for the legal interpretation. However, this topic is important here as it also relates to common conditions and standards for the planning and conduct of scientific research in general.

I agree with the first part of this statement above, but not with the second part. As long as an activity is genuinely motivated by an intent to conduct scientific research, other additional motivations, e.g. obtaining some of the funding by selling products, may even be regarded as an advantage and not as a counterargument. This situation is not specific to Japanese whaling research or to lethal whaling research otherwise. Similar additional motivations are generally found in fisheries research, as mentioned above, and also in many types of medical research (e.g. studies of the effect of vaccination against papillomavirus in preventing cervical cancer, which also aim to vaccinate a substantial proportion of the young female population) and in archaeology (in connection with construction or road building). Mixed motivations are common in many different types of costly research programmes.

2. Comments on the Australian claim that all information necessary to improve the management of minke whaling could be obtained by non-lethal methods

It is important under this heading to keep two issues separate. The first (I) is how to obtain and improve the information needed to run the 'Revised Management Procedure for baleen whales' (RMP) in its current version. The second (II) is how to obtain information that could be used to improve management procedures (e.g. the RMP) in the future. My interpretation of the Japanese research proposals for JARPA and JARPA II is that they are designed to obtain information needed to resolve both issues.

1) Information necessary to run the RMP:

The practice that has developed in the IWC Scientific Committee is that the information necessary to set maximum catch quotas by running the current version of RMP is a) the catch history for minke whales in the different regions of the Southern Ocean, b) at least one abundance estimate, again for each of the different regions of the ocean, and c) knowledge about the stock structure of minke whales in this ocean.

The information required under a) and b) is obtained by non-lethal means, but in the Japanese research programmes, the genetic information necessary to determine stock structure is obtained from samples of killed whales. Morphometric data from killed whales provide additional information on stock structure.

The Memorial and Dr. Mangel's assessment in Appendix 2 (p 362) claim that DNA analyses of biopsy samples obtained non-lethally from minke whales can provide sufficient genetic information to determine stock structure. In theory this is correct, but in practice it would be impossible. A research programme with the primary goal of obtaining a large number of biopsy samples from minke whales in the Southern Ocean would be prohibitively expensive in the absence of funds generated through the sale of meat from whales caught as part of the programme.

Moreover, for reasons of logistics, the number of biopsies that could be obtained with a similar effort to that in the current research whaling programme would be far lower than the number of genetic samples obtained from killed whales during the research whaling.

In more detail, the difference is explained by the constraints on biopsy sampling. Samples are obtained by shooting a syringe (a hollow steel tube) into the skin and subcutaneous fat (blubber) of a minke whale. The syringe with its contents (the biopsy) then falls off. In the Japanese research programme, it is retrieved by means of the string attached to it. In the Norwegian whale research programme, a darting airgun (the 'Larsen gun') is used to shoot the needle into the skin of the whale, whereas the Japanese use a crossbow. Both devices are rather imprecise, and the projectile has a low starting velocity. This means that the vessel has to be quite close to the whale. In the Norwegian programme, the distance from the whale is usually no more than 10-15 m, and in the Japanese programme, 18 m is regarded as the maximum distance. The maximum distances for firing a harpoon during a whale hunt are considerably longer. Norwegian minke whalers are advised not to fire if the distance to the whale is more than 50 m, and commonly operate from a distance of about 30 m. In Japan, 60 m is regarded as the upper limit in the minke whale hunt, and in practice 20 m is the lower limit. In conclusion, the vessel needs to be much closer to a whale to obtain a biopsy than to fire a harpoon. Weather conditions in the Southern Ocean do not always permit the use of Zodiacs or other small boats to get close to whales. Since it is difficult to get sufficiently close to whales with the large whaling vessel, the number of biopsies that can be obtained by one ship will be much smaller than the number of whales that can be killed during the same period.

A reasonable conclusion is that it is only feasible to obtain a sufficiently large number of genetic samples from minke whales by lethal sampling.

2) Information that could be used to improve management procedures:

The current RMP sets relatively low catch limits because of uncertainties about the productivity levels of whale stocks. The population models that are being developed may produce information on the lower bound of the productivity level. If, for a specific stock, research results make it possible to raise this lower bound, this would allow for larger catches without any increase in perceived risk to the resource. This has a clear bearing on the question as to what may constitute the most important additional research activity that could be used to improve management procedures in the future. In my view this is undoubtedly modelling population dynamics using a variety of methods. Over time, this could be extended to take species interaction into account.

Most of the models depend on information about the age of the animals. Dr. Mangel admits in his appendix that *'there are still no effective non-lethal means of ageing whales, so if age information is absolutely required, then lethal take is also required'* (p 357), but he continues by stating that *'there are problems with reading the ear plugs at all and often a large number of the killed animals do not provide readable ear-plugs.'* (p 366). Dr. Mangel is referring to Lockyer (2010) here, but this quote does not give a fair and representative picture of her findings. Lockyer showed in her 'blind' age determination of minke whale earplugs from JARPA that the Japanese age readings were generally reliable. Moreover, age information is clearly required for many of the investigations involving modelling of population dynamics. These may provide a way to explore long-term changes in minke whale abundance and the carrying capacity of the ocean for minke whales.

A key difference between age data derived from killed whales and the information obtained from sighting surveys is that the former provides information on the sizes of individual year-classes, and thus on recruitment patterns, whereas the latter can only produce estimates of total abundance. Thus, lethal research methods give a much stronger information base for attempting to ascertain the impacts of environmental change.

These important modelling studies were initiated by Butterworth and Punt, continued by Punt and Polacheck, and are currently being refined by Punt. Valid age determinations are very important for these virtual population analyses (VPA), the related catch-at-age modelling investigations, the determination of age of sexual maturity and its change with time, and many other analyses. Other observations that are only available from killed female whales, and which could be of importance for the improvement of management procedures, are the reproductive history of the individual whale and possible changes in the pregnancy rate.

For the data obtained from the hunted whales to give useful results, it is important that these whales can be regarded as a random sample from the whale population in terms of age distribution, area distribution and time during the season the whales spend feeding in the Southern Ocean. It is impossible to obtain a perfectly random sample in this kind of fieldwork. However, the Japanese scientists have designed the sampling procedure carefully, and in my judgement the samples are likely to be sufficiently random for most purposes. Data are generally collected in a similar way in fisheries research, and also in medical epidemiological research, to choose an example from another scientific field.

3. Comments on the claim that research whaling carried out under Article VIII should be restricted to research which could improve conservation of whales and management of whaling

On p.8, the Memorial appears to limit Article VIII of the Convention to *'scientific research in the context of conservation and management of whales'*. Again, I cannot speak for the legal interpretation, but I note that this assertion is addressed in some detail in the Japanese Counter-Memorial. Speaking from my own scientific experience, including in the Scientific Committee of the IWC, I once again find this to be too restrictive an interpretation of the purposes of scientific research. This is also illustrated by the deliberations within the Scientific Committee itself, which has accepted other reasons for conducting lethal research.

The 1986 IWC Resolution on /lethal/ Scientific Research Programmes includes the following criterion: *'(1) The research addresses a question or questions that should be answered in order to conduct the comprehensive assessment or to meet other critically important research needs.'* (38th RIWC 1987, p 27). From the context and from the discussion in the Commission it was clear that *'other critically important research needs'* did not have to be related to comprehensive assessments of whale stocks or be relevant for management of whaling. When Norway presented its plans for scientific whaling in 1987 and 1991 (a feasibility or pilot study of 50 whales from 1988 to 1990, then a full study of approximately 300 whales from 1992 to 1994), the justification given was not at all that the results from the research would be of value for management of minke whaling in the North Atlantic, but that Norwegian fishermen were concerned about interactions between whales and commercially important fish stocks (RIWC 43, p 29, 1993). In order to consider this problem, scientists needed to investigate the diet of minke whales and how much they were taking of various species in different regions of the ocean and at different times of year. The information from the research programme was later used in a multispecies simulation model, MULTSPEC. In 1987 and 1991, the discussions in both the Scientific Committee and the Commission focused on whether studying the feeding ecology of minke whales could be considered a 'critically important research need'. The criterion cited above from the 1986 Resolution was later listed in Annex Y of the Scientific Committee Report of the 52nd meeting, 'Guidelines for the Review of Scientific Permit Proposals' (JCRM, 3 Suppl 2000), and has been used in all discussions on scientific permit whaling after 1986. Nevertheless, the Memorial (including Dr. Mangel's assessment in Appendix 2) repeatedly claims that any valuable lethal research programme must be motivated by its importance to the conservation and management of whale stocks. This is a fundamental misunderstanding. As mentioned above, this argument has never even been raised by the Scientific Committee.

4. Comments on the criticism of other parts of the lethal research carried out under JARPA and JARPA II.

In addition to objectives dealing with improving the conservation of whales and management of whaling, both JARPA and JARPA II have also had more general ecological objectives, e.g. 'Elucidation of the role of the whales in the Antarctic marine ecosystem' and 'Elucidation of the effect of environmental change on cetaceans'. Several of the comments on these aspects of the Japanese research programmes in the Memorial appear to be devoid of scientific basis and even tendentious and misleading. For example, on page 211, the

Memorial states the following about the Japanese objective of *"monitoring the Antarctic ecosystem"* and the accompanying justification for lethal fieldwork in JARPA II: *"In fact, the Antarctic ecosystem in its ordinary meaning covers a much larger area than the narrow region in which Japan conducts its whaling."* The reality is that this *"narrow region"* covers the area from 60°S to the ice edge and from 35°E to 145°W, which is exactly 180° or half of the circumference of the Antarctic.

Many of Dr. Mangel's comments in his Appendix on the corresponding research projects appear to me to be equally erroneous. According to him, the following non-lethal methods are available and should be used instead of lethal takes: chemical and biochemical analyses of biopsies of skin and blubber for pollutant and metabolites, collection and analyses of faeces for feeding studies, and satellite tagging for study of whale migration. It is true that satellite tagging of many large whale species has been successful, but this is not the case for minke whales. The success rate is very low, and the few successful tags do not last long, probably because of the strong drag on equipment attached to a fast swimming whale. This is also recognised by Australian scientists. Dr Nick Gales writes *"Tagging of this species [minke whale] will likely only be achieved with a significant investment of effort."* (IWC-SC/62/O12, p 6). Thus the information Dr. Mangel gives on tagging methodology (p 367) is definitely incorrect as regards minke whales. For obvious practical reasons, it is impossible to collect faeces of minke whales in the Southern Ocean. Moreover, even if such samples could be collected, they would only give information about food items, not about the quantity of food eaten. Skin and blubber samples may give information about some pollutants (especially fat-soluble pollutants), but not all, and not about pathological changes, for example in the liver, kidneys or reproductive organs.

Sections 5.49 to 5.81 (pp 215 to 230) of the Memorial repeat many of the misunderstandings and much of the erroneous information presented in Dr. Mangel's Appendix, which are referred to above. These include the misunderstanding of the availability of satellite tagging technology for minke whales, which is described as the *'gold standard of methodology'*, and the claim that *'all important research needs for conservation and management of whales can be appropriately (and - - - more effectively) addressed using modern non-lethal techniques; Work that does require lethal methods – such as obtaining data on the age of whales through examining their ear plugs – is either unreliable or unimportant.'* I have discussed above why these claims are clearly erroneous. The suggestion that the amount of prey consumed can be estimated through allometric techniques (using whale lengths determined non-lethally at sea to assess food intake) is, at best, a hopeful guess at what might be possible in the future. It is not an accepted methodology today.

The Memorial claims that three different methods for estimating circumpolar Antarctic minke whale abundance *'have yielded vastly contradictory results, providing current abundance estimates for Antarctic minke whales ranging from 338,000 to 1,486,000. The reasons for these significant differences are, as yet, undetermined'* (p 59). These figures are not correct. In 2011, the year when the Memorial was written, the range of best estimates for the 'current' estimate (based on the third circumpolar survey, CP III) was 382,000 to 712,000, as can be seen from Appendix I (by de la Mare, Kelly and Peel) and in the Scientific Committee report (JCRM, 12 Suppl p 25-26). However, the latter also states that work was in progress to determine the reasons for the differences between the statistical models. This

work was successfully completed in 2012, as explained in part 5 below. There is now full agreement in the Scientific Committee on the abundance estimates from CP II and CP III, and the decline from CP II to CP III is not statistically different from zero at the 5 % level. Thus the statement *'However, all these methods demonstrate a significant decline in circumpolar minke whale abundance south of 60°S between 1985 and 2004'* (p 59) is no longer justified.

The chapter on 'Development on whale conservation and management measures under the IWC' in the Counter-Memorial of Japan (pp 90 to 138) gives an interesting and detailed history of post-1972 developments in whale management regimes. However, in my view there are two aspects of these developments that are not discussed in sufficient depth:

1) The discussion in the IWC Scientific Committee on multispecies management of marine species including whales started much earlier than specified in this chapter (and presented in Figure 3-1). It began as early as 1987 in connection with the plans for JARPA and for a Norwegian feasibility study of the feeding ecology of minke whales in the North Atlantic, and was continued in subsequent years.

2) It is correct that the Commission adopted the RMP in 1994 by consensus. However, an important part of the RMP, the Catch Limit Algorithm (CLA), had already been adopted by the Commission in 1991, at a meeting held in Reykjavik. In its presentation to the Commission, the Scientific Committee gave a range of possible values for a particular parameter (the 'tuning level' (TL)), but the Commission decided in a resolution that a specific value was to be used in further development of the RMP (the highest value of TL, which gives the lowest quotas). Many states, including Japan but not Norway, accepted this decision, even though it had no scientific basis, because they expected that the moratorium would be lifted the following year. As we know, this did not happen. The question of the TL was an important part of the discussions during the 'Future of the IWC' process (2007-2010), and is also of some importance for possible future implementation of the RMP for Antarctic minke whales. The Scientific Committee began to work towards its implementation in 1992, but stopped in 1994 when the Southern Ocean Sanctuary was established. During this two-year period, the Committee suggested that the 'Small Areas' used for implementation of the RMP should be 10° sectors; however, in some sectors, a high TL could cause problems for commercial whaling. The 'Small Areas' used in any future implementation should in my opinion be based on the stock structure results obtained from the JARPA and JARPA II genetic and morphometric investigations. These stock structure results could not have been obtained without lethal sampling.

5. Comments on important scientific developments since the two memorials were prepared.

During the last ten years, there has been heated discussion in the Scientific Committee concerning the abundance of minke whales in the Southern Ocean. Three circumpolar IDCR/SOWER surveys were carried out between 1978 and 2004. Each circumpolar series took at least six years, since at most only one 60° longitudinal sector could be surveyed each year. The Scientific Committee accepted the results of both the first (CPI) and the second (CPII) survey, but was disturbed when the results from the first sectors surveyed during CPIII

became available, and there were strong indications that abundance in these sectors was much lower than estimated during CPII.

Since then, two pairs of scientists have tried to improve the estimation methods, following different approaches. The Japanese scientists Okamura and Kitakado developed one method, called the OK method, while Bravington and Hedley (Australia/UK) developed another, called the SPLINTR method. When these two methods were applied to the primary sighting data from CPII and CPIII, they gave very different results. The OK method gave much higher abundance estimates than SPLINTR. My impression from the Commission meetings in this period is that the differences were interpreted in the context of the political conflict between Australia and Japan. The Scientific Committee's Sub-Committee for In-Depth Assessment set up an Abundance Estimation Working Group to try to resolve the differences. It consisted of the four scientists mentioned above and three independent scientists with statistical expertise. However, when I became chair of both the Sub-Committee and the working group in 2010, I gained an impression of the work of the two pairs of scientists quite different from that which was prevalent in the Commission. All four were genuinely concerned to resolve the problems and understand the reasons for the differences in the results.

After several years of hard work, involving cleaning of the primary data, applying the two methods on simulated data, and making the changes in both methods that proved to be necessary, the estimates for both CP series were closer together. In June 2011, the working group, the Sub-Committee and the Scientific Committee concluded that it should be possible to reach an agreement by the following year, and that *'from preliminary calculations, the Committee agrees that the final estimates for each Area will most likely lie between the numbers given by the two methods - - - and be probably closer to the OK estimate.'* This turned out to be the case. In 2012, the working group and the Scientific Committee presented agreed estimates for all sectors from both CPII and CPIII to the Commission. The total circumpolar estimate for CPII was 720,054 minke whales, with a 95% confidence interval of 512,000 to 1,012,000. For CPIII the estimate was 514,783 minke whales, with a 95% confidence interval of 361,000 to 733,000. It was concluded that *'- - - a null hypothesis of no change in overall abundance between the two periods would not be rejected'* /at the 5% significance level/.

It is clear from the Memorial (and especially Appendix 1 by de la Mare, Kelly and Peel) that at the time of writing in 2011 the Australian scientists did not believe that the differences could be resolved, and that they were convinced that the lower abundance estimates, involving a large decline from CPII to CPIII, were likely to be most accurate. Developments since then mean that the arguments in the Memorial that use this as a postulate can no longer be considered valid.

Appendix A

LARS WALLØE

ADDRESS INFORMATION

P.O. Box 1103 Blindern, NO-0317 Oslo
Street address: Sognsvannsveien 9, Oslo, Norway

EDUCATION AND EMPLOYMENT

1961 Cand. mag. (chemistry, physics, mathematics) B.Sc., University of Oslo

1965 (Jun.) Cand. real. (physical chemistry) Ph.D., University of Oslo

1965 (Dec.) Cand. med. M.D., University of Oslo

1968 Dr. med. (simulation study of neural nets) Ph.D., University of Oslo

1965 - 1968 Research Fellow in Physiology (Faculty of Medicine, University of Oslo)

1968 - 1970 Assistant Professor of Physiology (Faculty of Medicine, University of Oslo)

1970 - 1978 Associate Professor of Informatics (Faculty of Science, University of Oslo)

1978 - 1988 Professor of Informatics and Head of the Department of Informatics

1988 - 2008 Professor of Physiology, Faculty of Medicine, University of Oslo

1995 - 2005 Research Director (part time), Institute of Marine Research in Bergen

2002 - 2007 Professor II (part time), Department of Arctic Biology, University of Tromsø

2008 - present Professor emeritus of Physiology, Faculty of Medicine, University of Oslo

PROFESSIONAL HISTORY

1974 - 1988 Chief Scientific Advisor to the Ministry of Environment

1976 - 1980 Director of the Norwegian research program on acid rain (SNSF)

1977 - 1984 Member of the Norwegian Medical Research Council (NAVF-RMF)

1981 - 1984 Chairman of the Norwegian Population Panel

1983 - 1988 Chairman of the Management Group of ØKOFORSK - an applied ecology research programme

1984 - 1990 Norwegian member of The Management Group of the UK-Scandinavian

Surface Waters Acidification (research) Programme (SWAP) (The Royal Society - The Royal Swedish Academy of Science - The Norwegian Academy of Science and Letters)

1986 – 1987 Organiser and member of the International Group of Scientists Appointed by the Norwegian Government to Review the Basis for Norway's Harvesting of Minke Whales

1986 – present Chief scientific adviser to the Norwegian government on management of marine mammals

1988 – 1994 Chairman of the Steering Committee of the Norwegian Marine Mammals Research Programme

1988 – 1992 Chairman of the Norwegian National Committee for Environmental Research (re-organised to the following Research Board in 1993)

1993 – 2001 Chairman of the Norwegian Research Board for Environment and Development (One of six research boards in The Research Council of Norway)

1989 – present Head of the Norwegian delegation to the Scientific Committee of the International Whaling Commission (IWC)

1993 – 2004 Member of the Board of The Norwegian Academy of Science and Letters

1993 – 1996 Chairman of the Science Class of the Academy

1997 – 2004 President of the Academy

1995 – 1998 Norwegian Member of the European Science Foundation's (ESF) Standing Committee for Life and Environmental Sciences (LESC)

1999 – 2003 Chairman of ESF - LESC

2000 – 2005 Member of the WHO Global Advisory Committee on Health Research

2001 – 2004 Member of the European Research Advisory Board for the European Commission (EURAB)

2004 – 2006 Chairman of the Scientific Committee of the North Atlantic Marine Mammal Commission (NAMMCO)

2004 – 2008 Chairman of the Section for Organismic and Evolutionary Biology of Academia Europaea

2008 – 2011 Panel Chair of Panel LS 9 (Applied Biology) of the European Research Council

2008 – 2014 President of Academia Europaea – The European Academy of Science and Letters

PRESENT PROFESSIONAL QUALIFICATION

President of Academia Europaea

Scientific Adviser to the Norwegian Government on Marine Mammals

MEMBER OF

Academia Europaea

The Norwegian Academy of Science and Letters

The Royal Norwegian Society of Science and Letters

The Physiological Society (UK)

The Royal Statistical Society (UK)

The American Physiological Society (US)

The American Statistical Association (US)

HONOURS AND AWARDS

1992 Fridtjof Nansen Award for his physiological research

2002 Commandeur dans l'Ordre des Palmes Académiques (France)

2005 Kommandør av Den Kongelige Norske St. Olavs Orden (Commander of the Royal Norwegian Order of St. Olav)

2009 Order of the Rising Sun, Gold Rays with Neck Ribbon (Japan)

PUBLICATIONS

A complete list of the approximately 200 scientific publications by Lars Walløe and his research groups can be found on his home page http://folk.uio.no/larswa/e_index.html Most of the publications are in the scientific disciplines of neurophysiology and cardiovascular physiology. The most relevant publications for the court case are listed below:

Statistics - planning of experiments

G.U. Fenstad, L. Walløe and S.Ø. Wille: Three tests for regression compared by stochastic simulation under normal and heavy tailed distribution of errors. *Scand j statist* 1977, 4: 31-34.

G.U. Fenstad: A comparison between the U and V tests in the Behrens-Fisher problem. *Biometrika* 1983, 70: 300-302.

G.U. Fenstad, M. Kjærnes and L. Walløe: Robust estimation of standard deviation. *J statist comput simul* 1980, 10: 113-132.

E. Skovlund and L. Walløe: A simulation study of a sequential t-test developed by Armitage. *Scand j statist* 1987, 14: 347-352.

E. Skovlund and L. Walløe: A non-parametric sequential two sample test developed by stochastic simulation. *J statist comput simul* 1988, 29: 87-104.

E. Skovlund: Correction of a two sample sequential t-test developed by Hajnal. *Comput stat data analys* 1988, 145-155.

E. Skovlund: A simulation study of three sequential two-sample tests with triangular continuation regions. *J stat plan inference* 1988, 19: 383-394 and 1989, 22: 403-404.

E. Skovlund and L. Walløe: Estimation of treatment difference following a sequential clinical trial. *J am stat ass* 1989, 84: 823-828.

G.U. Fenstad and E. Skovlund: A two sample sequential rank test by Sen investigated by stochastic simulation. *J statist comput simul* 1990, 36: 129-137.

E. Skovlund: Truncation of a sequential two-sample Wilcoxon test. *Biometrical journal* 1991, 33: 271-279.

E. Skovlund and L. Walløe: Sequential or fixed sample trial design? A case study by stochastic simulation. *J clinical epidemiol* 1991, 44: 265-272.

Environmental sciences

I.P. Muniz and L. Walløe: *The influence of water quality and catchment characteristics on the survival of fish populations.* pp 327-340 in B.J. Mason (ed.): The surface waters acidification programme. Cambridge University Press 1990.

S. Andresen, L. Walløe and K. Rosendal: The precautionary principle: Knowledge counts but power decides? Chapter 3 (pp 39-54) in R. Cooney and B. Dickson (eds.): *Biodiversity and the precautionary principle*. Earthscan, London, 2005.

Whales and whaling

R.M. Anderson, R.J.H. Beverton and L. Walløe: A note on the north atlantic minke whales and IWC policy. *Rep int whal commn* 1989, 39: 227-228.

L. Walløe: The geographical distribution of the Norwegian minke whale catch in the period 1950 to 1983. *Rep int whal commn* 1991, 41: 151-152.

L. Walløe: Whale numbers in dispute. *Nature*, 362:389, 1993.

L. Walløe and A.S. Blix: Swimming speeds of minke whales feeding off the coast of northern Norway and Spitsbergen. *Rep int whal commn* 1995, 45: 194.

E.O. Øen: Description and analysis of the use of cold harpoons in the Norwegian minke whale hunt in the 1981, 1982 and 1983 hunting seasons. *Acta vet scand*, 1995, 36: 103-110.

E.O. Øen: A Norwegian penthrite grenade for minke whales: Hunting trials with prototypes and results from the hunt in 1984, 1985 and 1986. *Acta vet scand* 1995, 36: 111-121.

E.O. Øen: High velocity projectiles for killing whales. Hunting trials using 20 mm high velocity projectiles for minke whales in 1982. *Acta vet scand* 1995, 36: 153-156.

E.O. Øen: A new penthrite grenade compared to the traditional black powder grenade: Effectiveness in the Alaskan eskimoes' hunt for bowhead whales. *Arctic* 1995, 48: 177-185.

S.K. Knudsen, E.O. Øen and L. Walløe: Minke whale hunt and animal welfare. *Animal welfare* 2007, 16(3):405-406.

K. Konishi, T. Tamura, R. Zenitani, T. Bando, H. Kato and L. Walløe: Decline in energy storage in the Antarctic minke whale *(Balenoptera bonaerensis)* in the Southern Ocean. *Polar Biol* 31:1509–1520, 2008.

K.A. Glover, T. Haug, N.I. Øien, L. Walløe, M.C. Lindblom, B.B. Seliussen, and H.J. Skaug: The Norwegian minke whale DNA register: a data base monitoring commercial harvest and trade of whale products. *Fish and Fisheries* 2011, 13(3):313-332.

Forthcoming papers

K. Konishi, T. Hakamada, H. Kiwada, T. Kitakado and L. Walløe: Decrease in stomach contents in the Antarctic minke whale *(Balaenoptera bonaerensis)* in the Southern Ocean. – *in review*

T. Kitakado, T. Schweder, N. Kanda, L. Pastene and L. Walløe: Dynamic population segregation by genetics and morphometrics in Antarctic minke whales. *–in manuscript.*

Conference summary

F.T. Last, G.E. Likens, B. Ulrich and L. Walløe: *Acid precipitation - progress and problems.* pp 10-12 in D. Drabløs and A. Tollan (eds.): Ecological impact of acid precipitation. SNSF-project, Oslo - Ås, 1980.

Text-books

A. Høyland and L. Walløe: *Elementær statistikk (Elementary Statistics).* 3rd edition, 250 pages, Tapir forlag, Trondheim, 1981.

D. Føllesdal og L. Walløe: *Argumentasjonsteori, språk og vitenskapsfilosofi.* 7th edition, 300 pages, Universitetsforlaget, Oslo, 2000, 1. edition 1977; translated as: *Rationale*

Argumentation - Ein Grundkurs in Argumentations- und Wissenschaftstheorie. 371pp, Walter de Gruyter, Berlin, 1986, and as: *Politikens introduktion til moderne filosofi og videnskabsteori.* 296pp, Politikens forlag, København, 1992, and as: *Argumentasjonsteori, språk och vetenskapsfilosofi.* 428pp, Thales, Stockholm, 1993.

T. Haug and L. Walløe (eds.): *Sjøpattedyr - om hval og sel i norske farvann. (Marine mammals – on whales and seals in Norwegian waters).* 240 pages, Universitetsforlaget, Oslo, 1998.

Symposium proceedings

A.S. Blix, L. Walløe and Y. Ulltang (eds.): *Whales, seals, fish and man.* Developments in marine biology 4. 720pp, Elsevier, Amsterdam, 1995.

G.B. Søfting, G. Benneh, K. Hindar, L. Walløe and A. Wijkman: *The Brundtland commission's report - 10 years.* 237pp, Scandinavian University Press, Oslo, 1998.

Others

U. Nordgarden, L.P. Folkow, L. Walløe and A.S. Blix: On the direction and velocity of blood flow in the extradural intravertebral vein of harp seals *(Phoca groenlandica)* during simulated diving. *Acta physiol scand* 2000, 168:271-276.

A.S. Blix, L. Walløe, E.B. Messelt and L.P. Folkow: Selective brain cooling and its vascular basis in diving seals. *J exp biol* 2010, 213:2610-2616.

A.S. Blix, L. Walløe and L.P. Folkow: Regulation of brain temperature in winter-acclimatized reindeer under heat stress. *J Exp Biol* 2011, 214:3850-3856.

L. Walløe, O. Bøckman, H. Dovland, B. Herrlander, A. Johnels, P. Kauppi, H. Rodhe, H.M. Seip and C.O. Tamm: *Acid rain - an environmental problem across national boundaries.* Working document for the Third meeting of the World Commission on Environment and Development, United Nations, Geneve, 1985, 1-17.

The state of the North-east Atlantic minke whale stock. Report of the group of scientists appointed by the Norwegian government to review the basis for Norway's harvesting of minke whales (chairman: Lars Walløe), Økoforsk, Ås, 1987, 1-100.

L. Walløe: *Framework for a marine mammal research programme.* Norwegian Fisheries Research Council, Trondheim, 1988, 1-45.

E.O. Øen og L. Walløe: *Norsk vågehvalfangst 1993 - Fangstutøvelse og inspeksjon.* Fiskeridepartementet, Oslo, 1993, pp 1-5, også på engelsk: *Norwegian minke whaling 1993 - Whaling activities and inspection routines.* Fiskeridepartementet, Oslo, 1993, pp 1-5.

E.O. Øen og L. Walløe: *Norsk vågehvalfangst 1994 - Fangstutøvelse og inspeksjon.* Fiskeridepartementet, Oslo, 1994, pp 1-5, også på engelsk: *Norwegian minke*

whaling 1994 - Whaling activities and inspection routines. Fiskeridepartementet, Oslo, 1994, pp 1-5.

E.O. Øen and L. Walløe: Hunting methods for minke whales in Norway. Results of scientific and traditional whaling in 1994. *IWC/47/WK 9.*

L. Walløe: An analysis of recent Japanese whale killing data with special emphasis on the use of the electric lance as a secondary killing method. *IWC/48/WK 2.* pp 1-16

E.O. Øen and L. Walløe: Hunting methods for minke whales in Norway. Results of the traditional whaling in 1995. *IWC/48/WK 3.*

L. Walløe: Translation to English with annotations of the description of whales given in Hans Strøm: Physisk og oeconomisk beskrivelse over fogderiet Søndmør beliggende i Bergens stift i Norge. Sorøe, 1762. Early descriptions of whales, *J cetacean res manage* 1999, 1(1): vii-viii.

L. Walløe: Translation to English with annotations of the description of whales given in Konungs skuggsjá (The King's mirror). Norway, about 1250. Early descriptions of whales, *J cetacean res manage* 1999, 1(2): iii-iv & 2(1): iii-iv.

Lars Walløe: *Har norsk hvalfangst en fremtid? (Is there a future for Norwegian whaling?)* pp 132-144 i P2-akademiet, Bind XXXI, Transit, Oslo, 2004.

Lars Walløe: The early development of Norwegian Antarctic whaling, pp 10-30 in H. Hatanaka: *Learning from the Antarctic whaling*, The Institute of Cetacean Research, Tokyo, 2005.

Lars Walløe: The meaning of excellence and the need for excellence in research, pp 63-68 in Henryk Ratajczak (ed): *Science and Society,* Wydawnictwo Uniwersytetu Wrocławskiego, Wrocław, 2010.

Lars Walløe: The ERC should be protected from the threat of applied science. *Research Europe*, 5 August 2010, p 7.