Ministry of Foreign Affairs
Tokyo, Japan

Dear Sir,

With reference to the case concerning Whaling in the Antarctic (Australia v. Japan: New Zealand intervening), I have the honour to acknowledge receipt of your letter No. 141823 dated 23 April 2013, under cover of which you communicated the full texts of the statements to be given by Professor Marc Mangel and Dr Nick Gales during the oral proceedings scheduled from 26 June 2013. By your letter, you also informed us that the Court had decided that the Parties may, if they so wished, submit written statement(s) in response to the statement(s) of the other Party's expert(s).

My Government feels that it would not be in the interests of good administration of justice to offer a point-by-point response at this stage to every argument in the two statements submitted by Australia. We are prepared to elaborate our views in the course of oral proceedings. If, however, the Court should take a different view, Japan is ready to provide further information pursuant to Articles 61 and 62 of the Rules of Court.

In the meanwhile, Japan will continue to prepare for the presentation of its criticisms of Australia's experts' statements during the oral proceedings in this case. The main points of technical criticism in addition to those already set out in Japan's Counter-Memorial are reflected in two notes prepared by Professor Judy Zeh of the University of Washington (herself a former chair of the IWC Scientific Committee) in preparation for Japan's responses to Australia's expert statements. Those notes are attached to this letter.

Accept, Sir, the assurances of highest consideration.

鶴岡公二

Koji TSURUOKA

Agent of Japan

31 May 2013

Mr. Philippe Couvreur
International Court of Justice
Peace Palace
Carnegieplein 2
2517 KJ The Hague
Netherlands

Main points contained in the comments by Professor Judith E. Zeh, a former Chair of the IWC Scientific Committee

(Provided as a part of consultation, in response to requests by the Government of Japan)

Zeh-1: Comments on Appendix 2 of AM, dated 31 December 2012
Zeh-2: Comments on 15 April 2013 Mangel Supplement and Gales Statement, dated 19 May 2013

- Clear conceptual framework for JARPA II, well-defined objectives, testable hypotheses and why the research is needed are clearly described in its plan in considerable detail. (p.9 of Zeh-1; p.2 of Zeh-2)

- Monitoring activity is crucial for determining trends, effects of environmental change, and interactions within an ecosystem (p.2 of Zeh-2)

- It is false that the data obtained by lethal means over a 26 years period have not contributed to the RMP and are not likely to contribute to it in the future. The stock structure data are particularly important for optimal management under the RMP. (p. 2 of Zeh-2)

- Both Professor Mangel and Dr. Gales are incorrect in their understanding of how the RMP works, within the context of the ICRW. Consequently, they have erred in their explanation about the use of lethally obtained data in the implementation of the RMP. (p.4 of Zeh-1; p.5 of Zeh-2)

- Dr. Gales' statements about the use of age data in whale management ignore the most recent discussions in the Scientific Committee, including the fact that the technical problems have largely been resolved. (p.5 of Zeh-2)

- Professor Zeh is "not aware of any general requirement in established scientific practice that lethal methods are appropriate 'only where the objectives of the research cannot be achieved by other means'", and she states that "there are cases in which lethal methods might be preferable". (p. 2 of Zeh-2. See also p.12 of Zeh-1)

- Peer review within the Scientific Committee is rigorous and unbiased because pro-whaling, anti-whaling and unbiased members of the Scientific Committee are

all represented in the reports. (p.3 of Zeh-2)

- The question of whether or not it is worth the effort of conducting the research to get the answer it seeks is irrelevant in the review by the Scientific Committee. That is for the Contracting Government granting the special permit to decide. (p. 8 of Zeh-1)

- The scientific research under Article VIII need not be for "the conservation and management of whales," but it could be simply to study whale physiology. (p.8 of Zeh-1)

From: Judy Zeh (zeh@uw.edu)
To: Akiko Muramoto (akiko.muramoto@mofa.go.jp)
CC: Judith E. Zeh (jezeh@hotmail.com), Ken Sakaguchi
Date: 31 December 2012
Re: Comments on Appendix 2 of AM and Parts I and II of JCM

Hello, Akiko! In the cover letter he sent with copies of the AM and JCM, Ken Sakaguchi indicated that my comments were needed only on Appendix 2 of the AM and Part I and Part II of the JCM, so I will restrict my comments to those parts unless I hear otherwise from you. I do not believe I have an email address for Ken Sakaguchi, so I hope you will pass my comments on to him.

Throughout my comments I will use abbreviations from the list on pp. xiii – xiv of JCM. Further abbreviations I will also use are:

ASM	Age at Sexual Maturity
GOJ	the Government of Japan
ICJ	the International Court of Justice
JCM	the Counter-Memorial of Japan
JCRM	The Journal of Cetacean Research and Management
L	line numbers in this report
Mangel	Appendix 2 of AM by Professor Marc Mangel
Part I	Part I of JCM
Part II	Part II of JCM
p	page
P	paragraph(s)
pp	pages
RIWC19XX	Report of the IWC with 19XX giving the year of publication
SC	IWC Scientific Committee
SC/57/O1	Plan for JARPA II as submitted to the SC
SupplX	JCRM Supplement with X indicating the volume
Supp2X	Used in place of SupplX for the 2nd volume X Supplement

I will begin with comments focused on Mangel although I will sometimes refer to JCM for purposes of comparison. In general, I will not comment on sections in Mangel that are correct and useful, e.g. the subsection headed Fundamentals of the Dynamics of Populations. I also will not comment on Mangel's Executive summary or Introduction. The Executive summary contains numerous misstatements with which I will deal when commenting on the corresponding P in subsequent more detailed sections. The Introduction just tells us why and how the paper was written and what the subsequent sections contain. There are a few other sections and subsections I mention later upon which I will not comment. Should you wish me to summarize my comments by commenting on these summary sections and subsections or to discuss sections I judged irrelevant, I can do so later.

I will not include in my reference list the citations used by Mangel and included in his Literature Cited.

Finally, while there are many comments about the JCM in what follows, my intent is to prepare a separate discussion of Part I and Part II of JCM. I will not be able to complete that until mid-January. I also want you to have a chance to read what I have already written and let me know if revisions are needed to make it useful to you. That would provide some guidance about how I should prepare the report on the JCM. My concern is that what I have written is more detailed than you would prefer. If so, I can make the report on the JCM more concise.

Mangel's Section 3. An overview of whaling in the Antarctic

Mangel's Section 3 is a brief and selective account of whaling in the Antarctic, early attempts to manage it via the adoption of the ICRW and establishment of the IWC and its SC, development of and problems with the NMP, and finally the development of the RMP. For example, Mangel (p342) mentions 'a small group of eminent scientists' who recommended elimination of the BWU as a method of setting catch limits. He neither names them nor notes that they became known as 'the Committee of Three', later expanded to four. JCM provides this information succinctly in footnote 225 on p97. The names are important, because their recommendations led to the approach now used by the IWC of setting catches separately for each individual whale stock, taking account of the estimated MSY for that stock. As noted in JCM (P3.29, 3.51 and 3.52) three of the four scientists making up the expanded Committee of Three presented cogent arguments against a blanket Moratorium on commercial whaling. The SC never agreed that a Moratorium was needed. Nevertheless, the IWC adopted a Moratorium in 1982.

One of the objectives of the Moratorium, as noted by Mangel (P3.20, p346), was to provide time to obtain estimates of the status and size of each stock that might be exploited and to determine catch limits that would not exceed MSY if the status of the stock permitted whaling, i.e. if the stock was not so far below its carrying capacity K that it was classified as a protection stock. The NMP being used to manage whaling when the Moratorium was adopted depended on knowing current population size, MSY, and K, with the latter two parameters considered fixed under the population dynamics model assumed by the NMP. As noted by Mangel (P3.13, p344), in reality K and MSY may vary, e.g. changes in the biomass of krill will affect K for a whale stock that feeds on krill. Mangel (P3.17, 3.18) also correctly describes the major problems with the NMP. The most important was the lack of data required for its implementation. Also important was its lack of a robust method for handling uncertainty in estimates of population size, MSY, and K when such estimates were available.

Because of such problems with the NMP, the SC continued to work to improve it during the first decade of the Moratorium by developing the RMP as part of the Comprehensive Assessment called for by P10(e), the Commercial Whaling Moratorium provision of the Schedule. After setting zero catch limits, P10(e) says: 'This provision will be kept under review, based upon the best scientific advice, and by 1990 at the latest the Commission will undertake a comprehensive assessment of the effects of this decision on whale stocks

and consider modification of this provision and the establishment of other catch limits.' As noted by JCM, P3.75, this sentence indicates that the Moratorium was viewed as a temporary measure. By the time the Moratorium took effect, catch limits on all Antarctic baleen whale stocks except minke stocks were already zero without the Moratorium. It is my recollection that the SC took 'Comprehensive Assessment' to involve a thorough assessment of each stock – including all available information on human-induced mortalities, population size and trend, range, and biological parameters – recognizing that these assessments would not reflect effects of the decision to impose a Moratorium.

In 1992, the SC had completed its development of the RMP and was ready to implement it for Southern Hemisphere minke whales, so the SC recommended that the Commission adopt the RMP. However, it was not adopted in 1992 or 1993, when the Chair of the SC resigned, saying '...what is the point of having a Scientific Committee if its unanimous recommendations on a matter of primary importance are treated with such contempt?...' (JCM, P3.81, 3.82). In 1994 the Commission adopted the RMP. Even then it was not implemented because the Commission decided that an inspection and observation scheme was needed before implementation to ensure that quotas would not be exceeded (JCM, P3.83). The Commission has not yet been able to agree such a scheme. These details are omitted by Mangel, who simply says that the Moratorium remains in force.

Although I have not checked every detail, I believe that much of Mangel's subsection on The Revised Management Procedure (RMP) (P3.21 – 3.31) is correct, including his statement that the RMP is 'an effective tool for the future management of whaling' (P3.31). However, based on my reading of the ICRW, I believe there are fundamental errors in Mangel's P3.26 and the claim in P3.31 that the RMP 'is designed so that lethally obtained data are not required'.

The purpose of the ICRW is 'to provide for the proper conservation of whale stocks and thus make possible the orderly development of the whaling industry'. Although Mangel (P3.5) paraphrases or quotes much of the ICRW Preamble, he does not mention this clearly stated purpose. He also omits the paragraph of the Preamble that notes that increases in the size of whale stocks will permit increases in the number of whales that can be captured without endangering the stocks. However, in P3.21 he mentions that goals of the RMP include achieving 'stable catch limits, thus allowing the orderly development and regulation of the whaling industry' and ensuring 'the highest possible continuing yield from each whale stock' as well as ensuring that risk of extinction is negligible.

Article V of the ICRW provides for amending the Schedule which is part of the ICRW but not for amending other parts of the ICRW. It is in the Schedule that catch limits are specified. The Schedule also contains rules governing such matters as times and places where whaling is permitted, whaling methods, size limits by species, and data that must be recorded for harvested whales. Item 2 of Article III of the ICRW makes it relatively difficult to amend the

Schedule by stating: 'Decisions of the Commission shall be taken by a simple majority of those members voting except that a three-fourths majority of those members voting shall be required for action in pursuance of Article V.' Resolutions are sometimes adopted unanimously, but sometimes by only a simple majority in a close vote.

Item 2 of Article V of the ICRW says that 'amendments of the Schedule *(a)* shall be such as are necessary to carry out the objectives and purposes of this Convention and to provide for the conservation, development, and optimum utilization of the whale resources; *(b)* shall be based on scientific findings;...' The key words in *(a)* are 'optimum utilization of the whale resources'; the key words in *(b)* are 'based on scientific findings'.

Mangel (P3.25) is correct in stating that the only data used in *CLA* calculations are total catch and population abundance data, though the specifications of those data in P3.25 are incomplete. For example, total catch must account for human-induced mortalities such as bycatch in addition to whaling (IWC 2005a) and abundance data could come from aerial not just shipboard surveys (IWC 2005b). However, Mangel (P3.26) is incorrect to conclude that the RMP 'thus eliminates the use of data obtained from whaling-dependent or other lethal-source data'. JCM (P3.86 – 3.94) is very clear about why this is incorrect, and I will not repeat everything said in those paragraphs. JCM (Table 3-1, p93) gives an excellent summary. The point is that the *CLA* calculations, once determined, are simple while the RMP is not.

To obtain the *CLA* for a given whale species in a region, the RMP must be implemented for that species in that region (IWC 2005c). The next P briefly describes how this is done, including the sorts of trials (*ISTs*) that are run for each candidate *CLA* and how the *CLAs* are evaluated for acceptability. These trials incorporate uncertainties in the data, e.g. uncertainties in stock structure and MSYR since catch limits are to be set for each individual stock, taking account of its MSYR. It is often impossible for a whaler to know to what stock a targeted whale belongs, and MSYR is never known exactly. Mangel's discussion of the RMP is clear about the role of the trials in accounting for uncertainty, but he does not discuss the implementation process in detail.

The SC must first conduct a *pre-Implementation assessment* to determine whether there are adequate data available to begin the implementation process. Existing and/or future data must include abundance estimates for use in the *CLA* and in conditioning *ISTs*. There must also be catch history data with sufficient spatial and temporal resolution for the whaling operations and stock structure hypotheses likely to be considered in the *Implementation*. Also helpful are data that help to define what stock structure hypotheses must be tested in the *ISTs* and/or to estimate dispersal rates among putative stocks, as well as data useful for conditioning (e.g. fishery selectivity and values for biological parameters such as natural mortality). Assuming that the SC agrees there is enough information to proceed, the next stage in the *Implementation* is to develop and condition agreed *ISTs* and assign plausibility weights to be used in evaluating trial results. Each trial is the

combination of a set of 'hypotheses', e.g. one of the stock structure hypotheses to be tested (including areas occupied within the region and initial depletion of each hypothesized stock), one of the values of MSYR included in the *ISTs* for each stock, etc. Finally, the agreed *ISTs* must be run, usually for more than one RMP variant, and the results of the runs used to determine one or more acceptable *CLAs* if possible. Acceptability is defined by conservation performance during 100 years of management. The definition of an acceptable *CLA* involves specifying management areas within the region, RMP variants governing such matters as how the catch limit is distributed among the management areas and possible operational constraints, and whether with additional research a marginally acceptable variant in terms of trial performance might be made more acceptable because stock structure or MSYR would be known more precisely.

If more than one acceptable *CLA* is found, stability of catch limits and highest continuing yield are considered in choosing among them. The *CLA* judged by the SC to be the best, taking stability of catch limits and highest possible continuing yield as well as acceptability into account, is adopted as the *CLA*.

After *Implementation*, *Implementation Reviews* are held regularly. Relevant new data and/or trials are considered during these *Implementation Reviews*. Potential modifications of the *CLA* may also be considered.

Mangel states (P3.26) that the RMP eliminates the use of lethal-source data and quotes IWC Resolution 1995-9 in that regard. The Commission adopted Resolution 1995-9 by a vote of 23 for, 5 against, with 2 abstentions (RIWC1996, p30). The quote was taken out of context. A more complete version is

> 'NOW THEREFORE the Commission:
> RECOMMENDS
> that scientific research intended to assist the comprehensive assessment of whale stocks and the implementation of the Revised Management Procedure shall be undertaken by non-lethal means;'

In short, it was a recommendation, not a mandate, in spite of the use of the word 'shall'. It went on to recommend:

> 'that scientific research involving the killing of cetaceans should only be permitted in exceptional circumstances where the questions address critically important issues which cannot be answered by the analysis of existing data and/or use of non-lethal research techniques;'

The contribution of JARPA to minke whale management was considered during the reviews of JARPA conducted by the SC in 1997 and 2006. In both reviews, the SC concluded as follows (Suppl10, p348):

> 'The results from the JARPA programme, while not required for management under the RMP, have the potential to improve management of minke whales in the Southern Hemisphere in the following ways: (1)

reductions in the current set of plausible scenarios considered in the *Implementation Simulation Trials;* and (2) identification of new scenarios to which future *Implementation Simulation Trials* will have to be developed (e.g. the temporal component of stock structure). The results of analyses of JARPA data could be used in this way perhaps to increase the allowed catch of minke whales in the Southern Hemisphere, without increasing depletion risk above the level indicated by the existing *Implementation Simulation Trials* of the RMP for these minke whales.'

This conclusion makes clear that the SC would use JARPA results to conduct an *Implementation* or *Implementation Review* for Antarctic minke whales. The SC also agreed with the summary of the main results of JARPA presented in Appendix 2 of Annex O (Suppl10, pp347-8). These results included estimates of such biological parameters as ASM and percentage of mature females pregnant that would likely be used in developing *ISTs*. However, as indicated by the example in the above conclusion, the SC considered that the stock structure information provided by JARPA would certainly need to be used in developing *ISTs*.

The SC agreed 'that there are at least two stocks of Antarctic minke whales present in the JARPA research area. The data do not support the current IWC management Areas for Antarctic minke whales.' (Suppl10, p347) In my view, this is a very important agreement. IWC management based on incorrect ideas about stock boundaries could lead to more depletion of one of the stocks than intended given IWC management objectives. The *CLA* over time would correct any excessive takes from one of the stocks since different stock structures no doubt were and would be considered in the *ISTs* even without the JARPA results. But the contributions of JARPA to the understanding of stock structure are important for optimum (L238-247 above) whaling management as required by the ICRW (L150-155 above). Lethal sampling was required to obtain the stock structure information (JCM, P4.75 and P4.82) as well as the biological parameter estimates mentioned above.

SC/57/O1 (p10) indicates that JARPA II will monitor ASM, pregnancy rates, and other biological parameters using lethal sampling. SC/57/O1 (p12) states that JARPA II will attempt to provide data for improved MSYR estimates and redefinition of appropriate management Areas for Antarctic minke whales. SC/57/O1 (pp17-18) makes clear that these data also require lethal sampling. As discussed below, the SC would use JARPA II results obtained from these lethal samples in developing *ISTs* as well.

Mangel's Section 4. Characteristics of a program for purposes of scientific research

Mangel (P4.7) says '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.' This is an opinion, not a fact. See comments on P4.8 and P4.9 below.

According to Mangel (P4.8), 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; and
d) Is designed to avoid negative ecological consequences.

Although he cites references for these 'generally accepted principles', not all scientists would agree with all of them, especially a) and b). It can be argued that a program for purposes of scientific research might begin with general questions rather than a set of focused hypotheses in order to collect data that could lead to more focused questions. Such a program might not always use the correct empirical tools until exploratory analyses of the data collected and/or assessments by other scientists pointed to the sample sizes that would ultimately be needed and the models and analysis methods that were most appropriate. The JARPA feasibility study (JCM P4.10) was probably such a program.

Mangel enlarges on his P4.7 – 4.8 beginning with P4.9, where he claims that without an over-arching conceptional framework, one is doing 'exploratory analyses' with the hope 'that something interesting will arise from random activity. This rarely works...' If one searches for 'exploratory data analysis' using Google, one finds that there are over a million scholarly articles. There are 9,510 citations for 'Exploratory data analysis – Tukey'. Tukey (1970) is the founder of this approach to data analysis, which allows summarizing the main characteristics of data sets without using statistical models or formulating hypotheses, thus facilitating work on scientific problems. Mangel is simply wrong to call exploratory data analysis random activity that rarely works.

Mangel deals with 'Proper Assessment through the Community of Scientists' in P4.17 – 4.26. Most of this section is relatively non-controversial. However, P4.22 – 4.23 foreshadow a lack of understanding of Section 1 of Article VIII of the ICRW which authorizes scientific research under special permit. For example, 'originality of an idea' would not be relevant for the SC to assess in reviewing a plan for special permit research. Similarly irrelevant in such a review is the question of whether 'getting the answer will be worth the effort'; that is for the Contracting Government granting the permit to decide.

Mangel deals with 'IWC Criteria for Special Permit Whaling' in P4.30 – 4.37. There are a number of problems in this section. The most serious is that he cites IWC (2009), which includes the process for the review of special permit proposals adopted by the SC in 2007 (Suppl10, p61). However, no new or continuing proposals were reviewed by the SC at that meeting (Suppl10, p60); the SC noted that there were no substantial changes from previously reviewed proposals and therefore referred to its comments in previous years. The JARPA II proposal had been reviewed in 2005 using the guidelines for reviewing special permit proposals in effect at that time (Suppl8, pp48-52). These differed substantially from the review process of IWC (2009). Of

course, the JARPA proposal and results were also reviewed before IWC (2009) existed.

I found P4.35 difficult to understand initially but concluded it is entirely an expression of Mangel's opinions. I also concluded that when he argued for weighing the balance between the information produced by killing an individual whale and 'the loss of future information that could be obtained were a non-lethal method used', he must have been thinking about humpback whales. In the case of humpbacks, abundance and stock structure information as well as information on such biological parameters as calving interval can be obtained by biopsy sampling and/or photography if the same whale is encountered multiple times over a period of years. However, for Antarctic minke and fin whales, JCM presents convincing arguments that these techniques are not feasible, see e.g. P4.75 and P4.82 of JCM. Satellite tags are another non-lethal method for obtaining information on stock structure. However, SORP investigators had difficulty tagging humpback whales on the feeding grounds south of Australia and the South Pacific (JCM P5.49 – 5.50). It is reasonable to assume that faster swimming, less approachable Antarctic minke whales would be even more difficult to tag (JCM footnote 697, p252).

In P4.36 there is a reference to Gales et al 2009 which is not listed by Mangel in his Literature Cited section. Finally, in P4.37 – 4.39 Mangel makes arguments regarding scientific research programs 'motivated by' or 'in the context of' the 'conservation and management of whales'. This foreshadows a major problem with his Sections 5 and 6: He either does not realize or does not acknowledge that Section 1 of Article VIII of the ICRW allows Contracting Governments to grant special permits for any scientific research. That research need not be for the 'conservation and management of whales'. For example, it could be simply to study whale physiology.

Mangel's Section 5. Description and assessment of JARPA and JARPA II as programs for the purposes of scientific research in the context of conservation and management of whales

I will not discuss Mangel's comments on JARPA. My reasons are simple. First, the case before ICJ deals with JARPA II, not JARPA. Second, JARPA research was conducted between 1987/88 and 2004/05, and JARPA methods and results have been thoroughly reviewed. JARPA was reviewed by the SC at special meetings in 1997 and 2006. Participants in the 2006 final review of JARPA included invited experts who do not ordinarily attend meetings of the SC. The reports of the special meetings are published (RIWC1998, pp377-411; Suppl10, pp411-45), along with comments on those reports by the SC during the regular meetings at which they were presented (RIWC1998, pp95-105; Suppl10, pp58-9 and pp342-3). Regarding the major findings of JARPA in the context of IWC resolutions, the SC concurred with the summary reported in Appendix 2 of Annex O (Suppl10, pp347-8). Recommendations made during the 2006 JARPA review meeting and their status are given in Appendix 3 of Annex O (Suppl10, pp349-50). Some of those

recommendations, as well as earlier discussions and recommendations regarding JARPA, have no doubt influenced JARPA II methods.

The plan for JARPA II, 'Plan for the Second Phase of the Japanese Whale Research Program under Special Permit in the Antarctic (JARPA II) – Monitoring of the Antarctic Ecosystem and Development of New Management Objectives for Whale Resources' was presented to the SC in 2005 in paper SC/57/O1. Throughout this section, I will refer to it as SC/57/O1.

I will not comment on Mangel's P5.1 – 5.3 other than to note that a) P5.1 – 5.3 incorrectly assume, as does all of Section 5, that special permit research must be for the 'conservation and management of whales' and b) I disagree with all of the stated conclusions concerning JARPA II. Instead, I will make my comments on the subsections in which he measures JARPA II against his four characteristics of a scientific research program (P4.8).

Characteristic a) from P4.8 of Mangel

The first subsection under his first characteristic is headed '*Vague & general objectives*'. In P5.8 – 5.10 he criticizes the objectives as too broad. In P5.8 he lists the four categories into which JARPA II objectives are summarized in SC/57/01 as if they were the objectives, although he adds the word 'developing' to the second and omits the word 'Antarctic' from the fourth. In fact, SC/57/O1 lists four specific objectives under the first category, two under the second, and three under the third. Under all four categories, SC/57/O1 describes the actual objectives in considerable detail, as well as relationships among the objectives in different categories, hypotheses to be tested, and why the research is needed. There is a whole 'Research need' section in SC/57/O1 preceding the 'Research objectives' section which includes questions and hypotheses to be examined as well as discussing why the research is needed. Mangel's P5.9 (which seems to imply that only objective 3, not objective 1, requires field work) and P5.10 suggest that he did not read SC/57/O1 carefully.

In his next subsection, *The 'krill surplus' hypothesis*, Mangel (P5.12) incorrectly states that this hypothesis is the only clearly identifiable hypothesis of JARPA II and (P5.13) that it has evolved from a hypothesis to be tested to a 'theorem... whose truthfulness is known'. SC/57/O1 (p11) says 'Several hypotheses, including the krill surplus hypothesis and the process of resource increase due to the age at sexual maturity changing to younger ages will be tested.' Theorems must be proven, so calling the krill surplus hypothesis a 'central theorem' does not presuppose that it does not need to be proven. Mangel (P3.13, p344) says 'For example, the changing biomass of krill as water temperature changes will affect the carrying capacity for whales (Wiedenmann et al 2008)'. It should be equally obvious that changing biomass of krill due to changes in abundance of krill predators other than minke whales in regions where both are found could affect minke whale carrying capacity and hence abundance. Thus the krill surplus hypothesis is a plausible one. Regarding Mangel's P5.14 – 5.15, SC/57/O1 (p16) recognizes that a model 'with krill as the sole prey species and the four baleen whale

449 species, which will compete for the prey...is a simple ecosystem model'. It is
450 not at all unusual for modelers to begin with models that are much simpler
451 than reality and to expand them if they prove inadequate. In the same
452 paragraph on p16, SC/57/O1 notes plans to incorporate other krill predators
453 'to construct a more realistic ecosystem model' in the future.
454
455 Mangel's remaining discussion in P5.15 – 5.22 under his first characteristic,
456 with the exception of a few paragraphs that refer forward to the discussion of
457 his second characteristic of a scientific research program, deal with the
458 relationship between JARPA II and management. SC/57/O1 and JCM make
459 a number of good points about the errors and uninformed opinions offered by
460 Mangel in these paragraphs. However, recent reports of the IWC SC offer
461 even more powerful evidence that Mangel is wrong.
462
463 In the report of the 2010 SC meeting (Suppl12), Section 20 deals with actions
464 arising from intersessional requests from the Commission. I did not attend
465 that meeting, so I did not become aware of these requests until I read Section
466 20. My understanding of Section 20 is that the Commission requested that
467 the SC conduct *Implementations* or *Implementation Reviews* for all whale
468 stocks that would be managed using the RMP if the Moratorium were not in
469 effect from which there are takes under objection or special permit. I may be
470 mistaken, because Section 20 does not say this. Whatever the reason, the
471 SC noted that there are 'reasons to conduct an *Implementation* for Antarctic
472 minke whales starting in 2012'. Because of required preparatory work, the SC
473 recommended that two years be allowed for the *pre-Implementation*
474 *assessment*, which could start in 2014.
475
476 In the report of the 2011 SC meeting (Suppl 13), which I also did not attend,
477 pp21-3 discuss the 'in-depth assessment of the Antarctic minke whale' that
478 the SC 'is in the process of undertaking'. Agreed abundance estimates from
479 the IDCR/SOWER surveys (CPII and CPIII) would be needed, and the SC
480 noted that abundance estimates from JARPA and JARPA II could be used in
481 some of the ongoing SC analyses being employed in the attempt to obtain
482 agreed estimates from the IWC/SOWER surveys. The SC recommended that
483 'Although there are some issues to be resolved with the JARPA and JARPA II
484 estimates...
485 exploratory analyses' using them should be conducted and presented to the
486 2012 SC meeting. Population dynamics models, in particular statistical catch-
487 at-age (SCAA) models would also be used in the assessment. Inputs to these
488 models are 'catch, length, age and sex data from the commercial harvests
489 and both JARPA programmes, as well as abundance estimates from
490 IDCR/SOWER and both JARPA programmes.'
491
492 The report of the 2012 SC meeting, which I attended, has not yet been
493 published but can be downloaded from the Internet. Section 10.1 of that
494 report deals with Antarctic minke whales. The SC was able to agree
495 abundance estimates from CPII and CPIII; these are given in Table 9 of
496 Section 10.1. Kitakado et al (2012) presented a new integrated analysis of
497 Antarctic minke morphometric, microsatellite, and mitochondrial DNA data
498 from JARPA and JARPA II. Their results provided new information about the

spatial, temporal, and sex-specific distribution of the two minke stocks identified by JARPA. The SC noted that the approach of Kitakado et al is simple and potentially powerful and the results relevant to understanding Antarctic minke whale dynamics. The SC believed that the SCAA model of Punt et al (2012) largely resolved problems with catch-at-age population dynamics models that had been identified in previous years. The SC recommended that this model be run using the newly agreed minke abundance estimates and the catch length, age, and sex data from the commercial harvests, JARPA, and JARPA II, including data through the 2011/2012 JARPA II survey, as soon as possible.

In Section 5.1 of the 2012 SC report, it was noted that the SC has been working since 2007 on approaches to obtain more precise estimates of MSYR for use in RMP *ISTs*. This is mentioned in SC/57/O1 among the JARPA II objectives under the heading of 'Improving the management procedure for Antarctic minke whale stocks', with methods to be used specified in a subsequent section of SC/57/O1.

Thus it is clear from SC/57/O1 and the cited SC reports that P5.17– 5.18 and P5.22 of Mangel are not correct. I have no comments on P5.19 – 5.21, which follow the *Ecosystem model* heading. P5.20 refers forward to P5.36 – 5.37, part of the discussion of his second characteristic of a scientific research program; I will discuss it there.

Characteristic b) from P4.8 of Mangel

The heading for his second characteristic is extremely long, because he has added a new requirement to P4.8 b): 'use of lethal methods only where the objectives of the research cannot be achieved by any other means (i.e. by the analysis of existing data and/or the use of non-lethal research techniques)'. At first glance this sounds good. Why kill an animal if it is not necessary to do so? However, humans kill animals viewed as pests (e.g. moles) or needed for food (e.g. deer) regularly, even though it is not necessary. SC/57/O1 suggests another intriguing answer, which depends on management objectives that consider more than a single species. Before commercial whaling came to the Antarctic, there were many more blue and fin whales than there are now and probably fewer minke whales. All three of these species feed on krill. They are not necessarily competitors for the krill resource; they may feed in different times and places. But if they are competitors, and if it is a management goal to increase blue and fin whale populations towards their pre-whaling numbers, could harvesting, or even overharvesting, of minke whales ease the competition with blue and fin whales, allowing these larger baleen whales to move more rapidly towards their pre-whaling numbers while minke whale populations decrease correspondingly? There are many questions to be answered here, as SC/57/O1 recognizes. JARPA and JARPA II are attempting to answer some of these questions, but SC/57/O1 is asking the IWC to consider its management goals, particularly with regard to blue and fin whales. Depending on those goals, and a better understanding of which krill-eating

549 species are competitors, there could be a reason to use lethal in preference to
550 non-lethal research techniques on minke whales. In cases in which research
551 objectives could be accomplished using non-lethal techniques, the lethal
552 takes might contribute to management objectives.
553
554 Under the subheading *Appropriate empirical tools*, P5.23 gives a selective list.
555 Succeeding paragraphs describe the listed items. *Sightings surveys* are
556 described in P5.24 – 5.26. If I understand what I have read in SC/57/O1 and
557 in SC reports correctly, the last sentence of P5.26 is deliberately misleading.
558 In all of JARPA II and in the later years of JARPA, sightings surveys were
559 conducted independently of lethal sampling using different ships on different
560 track lines.
561
562 *Lethal take* is discussed in P5.27 – 5.30. This subsection makes clear that
563 Mangel is biased against research requiring lethal take.
564
565 The second sentence of P5.27 is completely incorrect; see SC/57/O1 and the
566 reports of the SC meetings in 2010-2012 described above. The third
567 sentence of P5.27 is not factual but rather is an expression of Mangel's
568 opinion.
569
570 P5.28 – 5.30 claim there are problems with the age data from JARPA. P8.1.3
571 of the report of the final review of JARPA (Suppl10, p434) discusses age and
572 natural mortality estimates from JARPA. It does not say the effort 'failed' but
573 rather points to problems with the 'commercial age data', not the JARPA data.
574 A longer section on *Reliability of age determination* (Suppl10, pp422-3)
575 contains more detail in this regard and ends with the recommendation 'that
576 the comparability of commercial and JARPA age data be investigated by re-
577 reading a subset of the commercial samples in an appropriately designed
578 blind test.' This was done and reported at the 2010 SC meeting by Lockyer
579 (2010), cited in Mangel P5.29 – 5.30. In the report of that meeting (Suppl12,
580 Section 10.1.2), the SC agreed 'that no further experiments or analyses on
581 age reading errors are needed to resolve ageing related problems raised in
582 e.g. the JARPA review'. Suppl12 (Section 10.1.3) records the SC agreement
583 that 'other issues' associated with the catch-at-age based assessments
584 should continue to be investigated, contrary to the claim by Mangel (P5.30)
585 that the catch-at-age model approach of JARPA and JARPA II to estimate
586 natural mortality 'had demonstrably failed'.
587
588 Mangel's subsection on *Other tools* begins with P5.31, which lists non-lethal
589 methods for assessing stock structure, pollutant concentration in tissues,
590 gender, and reproductive status. These methods all require biopsy sampling
591 or satellite tagging. JCM (P4.75) presents arguments regarding the
592 impracticality of biopsy sampling of Antarctic minke whales. However, when I
593 read the entire passage from which P4.75 was taken (Supp211, pp425-6), I
594 learned that SOWER cruises have undertaken experimental sampling of
595 Antarctic minke whales, and that to mitigate 'the risk of unwarranted
596 penetration and damage to the target animal; the collar (preventing
597 penetration beyond the depth of the biopsy tip) needs to be of an appropriate
598 size'. The argument of P4.75 would be more convincing if these details were

included instead of omitted. Plans for and/or results of experimental work in response to these comments and/or reasons such work was not undertaken should have been reported. JCM (P5.49 – 5.50) presents convincing arguments regarding why satellite tagging is not practical for Antarctic minke whales. However, I could not check all the footnotes for these comments because I did not have the references in footnotes 696-698.

There is one problem that bothers me about the argument that biopsy sampling and satellite tagging are not practical for minke whales. Obviously, the hunters who harvest the whales manage to hit them. The Inupiat hunters who hunt bowhead whales have been very successful at tagging them in recent studies (Quakenbush et al 2012) and even a relatively small number of tags have provided much information. Although I recognize that minkes are much smaller and faster than bowheads, they are successfully hunted. GOJ should be prepared to respond to this sort of question.

Regarding P5.33, O'Hara et al (2005) showed that epidermal samples such as those obtained via biopsy sampling had no predictive value for organ concentrations of such toxic elements as lead and cadmium in bowhead whales, likely because of bioaccumulation in the organs. This supports JCM P4.78 – 4.79.

Regarding Mangel's subsection on *Linking methods to objectives* (P5.36 – 5.37), the first sentence in the quote from Nicol et al (2007) in P5.36 says that monitoring of krill and its major predators is required for testing the krill surplus hypothesis. That is exactly what is proposed in SC/57/O1 under the heading *Monitoring of the Antarctic ecosystem*. JARPA II sighting surveys each year will record whales, seals, and possibly other krill predators. Acoustic surveys will be used to estimate krill abundance, and trawl surveys may be used later in the program to monitor krill. The Report of the Joint CCAMLR-IWC Workshop to review Input Data for Antarctic Marine Ecosystem Models (Supp211, pp541-86) held in 2008 makes clear that CCAMLR and the IWC SC have been collaborating on ecosystem models and the monitoring data they require for many years. This collaboration (Supp211, p542) 'will link IWC knowledge of whales with that of other krill consumers.' Existing data on krill as well as krill predators including seals, penguins, and flying birds were summarized. Much work remains to fill data gaps, particularly for seals and birds, but that work is ongoing. Since SC/57/O1 proposes to monitor krill abundance and oceanographic and meteorological aspects of the cetacean habitat in connection with the ecosystem model, as well as recording seals in addition to whales during sighting surveys, JARPA II will clearly be contributing to and drawing data from the CCAMLR-IWC collaboration via the involvement of many JARPA II researchers in both CCAMLR and the IWC SC. See the list of participants in Annex B of the Workshop report (Supp211, pp577-9).

Regarding Mangel's subsection on *Setting sample sizes* (P5.38 – 5.45), P5.38 – 5.43 deal with JARPA and hence are irrelevant to evaluating JARPA II.

Regarding P5.44 – 5.45, I agree with Mangel that 'the determination of a sample size must be grounded in statistical reasoning', not the abundance of the stocks being studied. The sentence in P5.44 italicized by Mangel, however, has nothing to do with the sample size calculations for JARPA II, as indicated by the beginning of the quote from Hatanaka et al (2006). Section V of SC/57/O1 and several SC/57/O1 appendices describe the statistical sample size calculations in considerable detail. One needs to remember that Hatanaka et al (2006) was prepared under a tight time deadline at an SC meeting in response to Childerhouse et al (2006). I believe that only one of the authors of Hatanaka et al (2006) is a native speaker of English, and I do not know whether Hatanaka et al (2006) was drafted in English or drafted in Japanese and translated. In either case, it is not surprising that the less than scientific italicized sentence slipped through. JCM P5.57 – 5.71 also does a good job of describing the JARPA II sample size calculations. Please note that although I have glanced at the sample size calculations in the references cited in this paragraph, I have not had time to check any of them carefully.

P5.46 – 5.47 of Mangel describe the areas where commercial whaling and the initial JARPA research took place. In P5.48 he then criticizes JARPA II for collecting data in the same areas as commercial and JARPA whaling because 'The potential development of new knowledge in this situation is very low.' In fact, long-term monitoring by JARPA II might well uncover new knowledge since, as pointed out in Section 1 of SC/57/O1, some changes in the Antarctic ecosystem are already evident. In addition, continuing research in essentially the same area covered during the later years of JARPA using essentially the same methods will provide longer time series of monitoring data. JCM P5.38 – 5.40 provide further details and rationale for the choice of JARPA II research area. Most importantly, the JARPA II research area is where Japan would conduct commercial whaling if the Moratorium were lifted and the RMP implemented for the Antarctic minke whale stocks in this area. Thus it is better knowledge concerning these stocks and the ecosystem and environment in this area that will permit wise management and sustainable use of the resource by Japan.

I have no comments on P5.49 – 5.51 beyond those I have made earlier.

Characteristic c) from P4.8 of Mangel

P5.52 – 5.62 deal with peer review and responses to it. Although JARPA II is mentioned in most of these paragraphs, the dates of references and discussions make clear that they involved JARPA, not JARPA II, and hence are not relevant here. Nevertheless, some of the comments made do apply more generally and require a response.

Regarding P5.52 and P5.56, it is true that reviews by the IWC SC are not anonymous, but to imply that they are not 'rigorous' and are not conducted 'by experts in the field' betrays a huge misunderstanding of the scientists who are members of the SC and of how the SC works. Since many SC members are opposed to lethal research in principle, their reviews of a plan for scientific permit whaling or of a paper reporting results from scientific permit whaling

will certainly uncover any methodological or other flaws. SC members chosen by their governments or invited because of their particular expertise are members because they are 'experts in the field'. Many SC members present their work first in a meeting document submitted to the SC and then take SC comments into account as they revise the document for submission to a journal. I am certainly one of those members.

Many times reviewer comments do lead to change. For example, Mangel's comment (P5.26) that 'some of the sighting surveys in JARPA and JARPA II are compromised because their methods involve both counting whales and preparation for lethal take' was expressed by SC members early in the JARPA program, and methods were changed so that in the later years of JARPA and throughout JARPA II the sighting surveys and surveys involving sampling would be completely independent. In JARPA II there are two dedicated sighting vessels that do no sampling and even cover areas between 60° and 62° S that are not covered by the sampling/sighting vessels.

GOJ should be prepared to submit to ICJ a summary table that lists the most significant criticisms of JARPA II made by the SC (and/or Mangel and the rest of the AM) together with the response of JARPA II researchers. Cases like the one in P5.26 just cited in which Mangel implies a problem in JARPA II although it was resolved in the later years of JARPA and did not occur in JARPA II should be included in this table. Of course, the response to some criticisms must be that methods cannot be changed because they are essential for achieving JARPA II objectives. Such a table would make clear the falsity of the claim that JARPA II researchers do not respond to peer review.

Regarding P5.53, even Mangel (P4.34) admits that lethal take is required for age estimation, and the importance of catch-at-age models (e.g. Punt et al 2012) for assessment of Antarctic minke whales as part of the process of implementing the RMP for those whales has been discussed above (L484-490). A number of the objectives of JARPA II absolutely require lethal sampling.

Regarding P5.55, Mangel should have noticed that Item 2 of Article VIII of the ICRW requires that 'Any whales taken under these special permits shall so far as practicable be processed and the proceeds shall be dealt with in accordance with directions issued by the Government by which the permit was granted.'

Regarding P5.56 – 5.59, the reason papers are published by JARPA researchers in fields that are outside JARPA and JARPA II objectives is that no part of any whale taken should be wasted but rather used to advance science in JARPA researchers' fields, as Mangel himself suggests in P5.59. In P5.58 Mangel distinguishes between IWC publications and non-IWC publications in a way that suggests that he may think the former are inferior. It is not surprising that papers dealing with cetacean management would appear in JCRM. Peer reviews for JCRM, and RIWC before JCRM existed, are as rigorous as any I have received from any journal. In fact, right now I

am a coauthor on a paper now in press at a different journal that was rejected by JCRM.

Regarding P5.60 – 5.61, it is true that many good journals accept papers based on lethal research, but in the competitive world of scientific publication, having some journals refuse papers based on lethal research does reduce the publication possibilities for researchers involved in lethal research. P5.62 is a summary paragraph, so I have already commented on its assertions.

Characteristic d) from P4.8 of Mangel

P5.63 – 5.67 deal with avoiding adverse effects on the stocks being studied.

P5.63 describes estimates of the number of minke whales in the Southern Ocean as 'highly uncertain, but ...of the order of magnitude of 300,000-500,000'. This was a reasonable description at the time the AM was written. However, as reported in Section 10.1.2 of the report of the 2012 SC meeting, the SC finally obtained agreed abundance estimates from CPIII of the IWC-IDCR/SOWER cruises. These estimates by IWC management area and in total are shown in Table 9 of that report. The total estimate for CPIII was 515,000 (CV 0.18). Area V had the highest estimated abundance (184,000 with CV 0.36). The last cruise of CPIII was in 2003/04. Japanese scientists should use the estimates by area (or data on a finer scale if they have it) to estimate numbers during CPIII for I-Stock and P-Stock, the minke stocks inhabiting the JARPA II research area. These numbers can then be compared with abundance estimates from the final years of JARPA and those obtained so far from JARPA II. They can also be compared with JARPA II takes from those stocks during CPIII and subsequently. Clearly it would be advantageous to obtain versions of the JARPA and JARPA II abundance estimates considered completely acceptable by the SC, but that may not be possible before the SC completes its Antarctic minke whale *Implementation*.

I do not understand Mangel's point in P5.64, so I cannot comment on it. In P5.65 Mangel worries that, though unlikely, it is possible that there could be impacts on small local populations. He claims that JARPA II would not be able to monitor such impacts. It seems to me that JARPA II would be able to monitor such impacts. The area where there is a possibility that the I-stock and P-stock mix will be sampled every year, so each year it will be possible to estimate the proportion of whales sampled from each stock using the techniques of Kitakado et al (2012). The proportions can be turned into numbers using data from the
sighting surveys and added to the numbers obtained from the sighting surveys in the areas in which only one stock occurs. Planned genetics analyses should also detect small local substocks if any exist, and their abundances would be estimated similarly.

Regarding P5.66 of Mangel (and P5.103 of AM which refers to it), from what little I.know about the Allee effect, I believe P5.86 of JCM is correct in its response. It is clear that 'the stocks of minke whales that are the subject of JARPA II are sufficiently large' not to be subject to this effect.

I have no additional comments on Mangel's summary P5.67.

Mangel's Section 6. Conclusion

This is a summary section, as indicated by its title. I believe every paragraph in this section is incorrect, for reasons I have detailed in previous sections of this report. I will not comment further here unless GOJ would like me to write a summary.

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From: Judy Zeh (zeh@uw.edu)
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CC: Judith E. Zeh (jezeh@hotmail.com)
Date: 19 May 2013
Re: Comments on 15 April 2013 Mangel Supplement and Gales Statement

Hello, Akiko! Given the very near deadline for submitting your counter-statement to Marc Mangel's Supplement (hereafter referred to as MM) and Nick Gales' Statement (hereafter referred to as NG), I provide brief comments in this memo. I will also refer to LW, the most recent version I have seen of "Scientific review of issues raised by the Memorial of Australia including its two Appendices" by Lars Wall□e. Since I had more time to prepare my earlier memos dated 31 December 2012 (hereafter referred to as Memo1) and 15 January 2013 (hereafter referred to as Memo2), I recommend that you give them more weight than you give to this brief memo.

Throughout my comments I will use abbreviations from the list on pp. xiii – xiv of JCM. Further abbreviations I will also use are:

ASM	Age at Sexual Maturity
AWMP	Aboriginal Whaling Management Procedure
GOJ	the Government of Japan
ICJ	the International Court of Justice
JCM	the Counter-Memorial of Japan
JCRM	The Journal of Cetacean Research and Management
L	line numbers
Mangel	Appendix 2 of AM by Professor Marc Mangel
Part I	Part I of JCM
Part II	Part II of JCM
p	page
P	paragraph(s)
pp	pages
RIWC19XX	Report of the IWC with 19XX giving the year of publication
SC	IWC Scientific Committee
SCAA	Statistical Catch at Age
SC/57/O1	Plan for JARPA II as submitted to the SC
SupplX	JCRM Supplement with X indicating the volume
Supp2X	Used in place of SupplX for the 2nd volume X Supplement

Comments on MM

I will begin with comments on pp of MM on which I noted problems.

p3: As in Mangel, MM fails to recognize that scientific permit research is not required to be directed toward conservation and management of whale stocks. This failure is reflected on subsequent pp of MM, but I will not mention it again. In general, I will mention other flaws in MM only the first time I notice them. I am

not aware of any general requirement in established scientific practice that lethal methods are appropriate "only where the objectives of the research cannot be achieved by any other means". In fact, there are cases in which lethal methods might be preferable.

p4: It is false that "the data obtained by lethal means over a 26 year period have not contributed to the RMP and are not likely to contribute to it in the future". The stock structure data are particularly important for optimal management under the RMP, as noted by LW. It is also false that "the data obtained by lethal means could be obtained by other methods." Even Mangel (P4.34) admits that lethal take is required for age estimation, and the age data obtained by JARPA and JARPA II are critical for the SCAA models used in assessments that would be part of RMP implementation for Antarctic minke whales.

p5: The 'several hypotheses' that MM claims are not described are in fact described in SC/57/O1 (pp15-16). MM focuses on hypothesis testing, ignoring the scientific contributions of modeling and analyses of monitoring data. Regarding lethal take, LW provides a clear explanation of why it is necessary to obtain adequate numbers of genetic samples to elucidate stock structure.

p6: It is not "extraneous information" that Antarctic minke whales can sustain a take. It is not "needless" (although it is probably not important) to describe the failure of age estimation methods that do not require lethal take as part of making the point that age estimation requires lethal take. Regarding "using biopsy to measure pollutants", see L592-596 of Memo1.

p7: Regarding P3.1 – 3.2, I believe that SC/57/O1 presents a clear conceptual framework for JARPA II, well-defined objectives, and testable hypotheses. Regarding whether "a program for 'purposes of scientific research' requires a testable and operationally defined hypothesis" (P3.3), LW and I have both provided examples of very important scientific research which began with many years of collecting and examining data to identify patterns and structure, with hypotheses developed later if at all. Note that James Watson, Frances Crick, and Maurice Wilkins (a colleague of Rosalind Franklin) won the Nobel Prize in physiology or medicine in 1962 for their 1953 determination of the double helix structure of DNA. Rosalind Franklin could not be included because she had died of cancer in 1958; the Nobel Prize can be awarded only to the living. Thus MM is clearly wrong in P3.3. Monitoring (P3.6) is critical for determining trends, effects of environmental change, and interactions within an ecosystem. Ecosystem modeling is clearly a scientific endeavor that requires monitoring data. Thus monitoring contributes to science even if it is not solely for 'purposes of scientific research'.

p8: P3.7 admits that science can 'produce' as well as test hypotheses. P3.8 makes clear that a 'question' (e.g. 'What is the structure of DNA?' or 'What is the stock structure of minke whales in the Antarctic?) is an alternative to a

'hypothesis' for beginning a scientific program. Yet P3.9 nevertheless demands 'hypotheses'. MM is far too focused on hypothesis testing.

pp9-11: These pp deal with setting sample sizes, and LW has an excellent section on that. I have little to add. I thought SC/57/O1 and its appendices, as well as JCM were clear. It is not a fact but rather an opinion of MM that they were not. MM complains about the 3.5% margin of error. P3.14 is incorrect in saying that choosing a margin of error requires a hypothesis. However, if it is possible, it would be useful to add specifics about the motivation for the 3.5% choice or other such choices. E.g. does the SCAA modeling require that level of precision? LW is clear about the problem of choosing a sample size when there are a number of parameters of interest. A sample size that is too small for some parameters can be mitigated by a longer observation period or by use of, e.g., a 10% instead of a 5% significance level for those parameters. In my view, P5.70-5.71 of JCM also provide good reasons for the sample size chosen. However, P5.70-5.71 could be improved. For example, Figure 5-4 of JCM is helpful, but it would be clearer if it showed the 594 value from P5.67 instead of the 1,288 value because P5.70 focuses on the 594 value. I agree with MM P3.16 and P3.21 that "comprehensively integrating many different data and analyses" in JCM P5.71 is a weak explanation of how compromised accuracy for some research items will be mitigated. More specific descriptions such as "extending the observation period" are clearer and more relevant. Nevertheless, the words "arbitrary and ad hoc" in MM P3.17 are not appropriate. In MM P3.21 the word "comprised" should be "compromised". The first sentence of MM P3.22 is an opinion with which I partially agree, as just noted, but the second sentence of P3.22 is simply false.

pp12-13: GOJ needs to emphasize cooperation with CCAMLR, which uses JARPA, JARPA II, and other SC data on baleen whales and collects data on other mammals and birds which is shared with the SC. The second sentence of the Figure 1 caption (p12) is wrong, as admitted in P3.27 (p13). Regarding Tamura and Konishi (2009), it would be interesting to determine whether they used only the non-lethal method to compute estimated prey consumption in response to peer review. If so, this would provide an example showing that JARPA II researchers are responsive to peer review, something MM claims is not the case.

pp14-16: This section on peer review of JARPA II is full of outrageous statements. Peer review within the SC is rigorous and, in balance, unbiased because pro-whaling, anti-whaling, and unbiased SC members are all represented in SC reports. JCM does not consider peer review outside of the SC as "not worth the effort and delay". See JCM P4.113, which reports that 107 papers on JARPA results were published in peer-reviewed journals between 1988 and 2009. This number needs to be updated and emphasized. MM P3.34 cites the figure of 107 but complains that (i) most are in IWC journals, which are implied not to have rigorous peer review, and (ii) most are not relevant to conservation and management of whales. See L711-720 of Memo1 regarding (i).

Regarding (ii), it does not matter whether published papers are relevant to conservation and management. The fact that they were accepted by scientific journals after peer review means that they responded to peer reviews appropriately and qualify as science. MM P3.37 and P3.39 are particularly outrageous. First, they complain that 12 papers in Norwegian or Japanese published since 2009 are "inaccessible". I have not investigated whether English translations of some of these can now be found on the internet, but even if they cannot, anyone who really wants to know what they say could arrange to have them translated. Second, they claim that 8 of these 12 papers are only 2-3pp long and "appear to be nothing more than abstracts of work rather than full analyses." Yet MM (P3.33 and P3.40) cites Clapham et al. (2003), which is in English and only 3pp long. Note also the following paper, in English and only 2pp long, which led to a Nobel Prize for its authors, as noted above:

James D. Watson and Frances Crick (1953). "A structure for deoxyribose nucleic acid." Nature 171 (4356): 737-738.

The MM citations seem to suggest that MM believes short papers in English are worth citing, but not short papers in another language. The Watson and Crick paper shows that the scientific value of a paper cannot be determined by its length.

pp17-21: Memo1, Memo2, LW, and previous pages of this memo have already dealt wih the claims on these pages. The problem is that MM shows no understanding of how the RMP works to accomplish (ii) of MM P4.1.

pp23-26: Only a few additional comments here. P5.5 reports that Mate et al. (2007) tagged a humpback calf "which is about the same size as a minke whale". Size of the target is not the only issue for tagging success. The humpback calf would likely have stayed at the surface longer and moved much more slowly than a similarly sized minke whale. LW provides good responses to all the claims in these pages regarding tagging. MM P5.13 claims that photography is "an important, non-lethal technique that is summarily dismissed by Japan". The P of JCM cited contain good and clearly stated reasons for dismissing this technique.

pp27-31: The reassessments and conclusions in these pages are full of errors, just as the assessments and conclusions in Mangel were. Many of the details claimed to be lacking are indeed lacking from the brief summaries of objectives quoted in these pages but are provided in SC/57/O1. I wondered as I read both Mangel and MM whether he ever looked at SC/57/O1.

Comments on NG

I will not comment on the Introduction, Section 1 of NG, even though it contains questionable statements, because most P of Section 1 refer forward to later sections. The remaining ones deal with the author's qualifications, experiences,

or opinions. I also note that many P throughout NG are non-controversial statements of fact. I will only comment on P in Sections 2-6 that I find problematic.

Section 2 of NG: The SC

P2.2 is not in itself problematic, but it refers to Annexure 2, in which I did find problems. Since Annexure 2 is referenced repeatedly throughout NG, I will discuss it now. The first two pp of Annexure 2 are factual and without problems.

The first problem I found was in P13 of Annexure 2, which says that "the RMP relies entirely on data that can be acquired non-lethally." The rest of P13 is factual, including the final sentence, which says "informative, though not indispensable, inputs are information about stock structure", where "inputs" refer to inputs to the RMP. Nothing I have quoted is technically incorrect. Only within the context of the requirement of the ICRW for "optimum utilization of whale resources" does it become problematic. NG is correct throughout when it says that the RMP can operate without using lethally acquired data. The problem is, as NG acknowledges in P14, that catch limits may be higher if additional data such as stock structure data are used. Use of SCAA results might also provide higher catch limits, and these require age data obtained by lethal sampling. It is notable that NG mentions catch-at-age data only twice. The first is on pp25-26, where the SC Working Group on MSYR in 2009 is cited regarding "problems in the interpretation of the catch-at-age data" and the conclusion is drawn "that MSYR could not be estimated sufficiently reliably for direct use in management." The second is in P35 of Annexure 2, which says, "It has recently been suggested that catch at age data derived from JARPA and JARPA II may be relevant" but that "the major problems that have confounded interpretation of these data for the past two decades will limit their utility".

These statements ignore 2012 SC discussions. See Section 10.1.4 of the Report of the SC, IWC/64/Rep1rev1. The SC concluded that the SCAA approach was the most appropriate for catch-at-age modeling for Antarctic minke whales and stated that "technical problems and inconsistencies identified in previous years have largely been resolved". See also L469-473 and L484-496 of my Memo1. Note there is a small typo in L488: There should be a comma after the word "catch".

P15 of Annexure 2 touches on the process of implementing the RMP for a particular whale stock. During that process, biological characteristics of the stock may play a role. This is not clearly acknowledged by NG. However, NG does say in P15 that "the RMP sets up simulations which account for (and test) the plausible range and variations in biological characteristics and the environmental features that drive them". Here, the important word is "plausible". Good information about a biological parameter such as MSYR for the stock may reduce the range of values of that parameter that can be considered plausible. Although

the RMP does not require biological information, such information can be used in the implementation process to narrow the range of values considered in the *IST*s. Biological information can indicate that certain values are not plausible.

This concludes my comments on Annexure 2. I return now to the remainder of Section 2 of NG. As with most of my criticisms of NG, the next one addresses a subtle implication of a particular sentence. The first sentence of P2.6 could suggest to a reader that if the moratorium on commercial whaling were ended, the SC would no longer be able to keep policy considerations separated from its scientific work. In fact, commercial whaling under objection by Norway has led to no such problems as far as I know.

Section 3 of NG: The SC and Special Permit Whaling

P3.1 states that, among all aspects of its work, the SC has had notable difficulty only in managing "its roles of review and advice in relation to JARPA and JARPA II". P3.2 alleges "a continuation of the problematic manner in which the SC operated prior to the moratorium" which is clearest in regard to (i) emphasis in these programs on collection of lethally acquired data for the assessment of biological parameters and (ii) compromised ability of the SC to provide evidence based advise to the IWC regarding these programs.

P3.4-3.7 deal with (i). I will not repeat them but just make a few observations. As already noted, biological parameter data is needed only for implementation, not for running the *CLA* of the RMP. Unlike commercial whaling, JARPA and JARPA II attempt to sample randomly. The RMP needs no revision to use the data they collect. Their elucidation of stock structure would certainly be used in *IST*s and results from SCAA models almost certainly would. Because of the large differences in the amount of available biological data among stocks subject to subsistence whaling, the AWMP group decided early on to make case-specific Strike Limit Algorithms (*SLA*) instead of a single one for all stocks. Thus, for example, *IST*s for the Bowhead *SLA* were judged in terms of plausibility based on biological characteristics determined primarily from harvested bowheads. The resulting *SLA* could then be used without a separate implementation step. Methods were kept constant between JARPA and JARPA II so that data from the two programs would be comparable and could be combined in analyses.

The remaining P of Section 3 purport to deal with (ii). I note that some P (e.g. 3.13) are criticisms of JARPA, not JARPA II, and thus are not relevant to this case. I also note that the SC moved to reviews by qualified scientists outside the SC because many SC members believed that review comments by SC members might not be unbiased. This is because of the polarization within the SC between members who believe whales should not be killed (except perhaps for aboriginal subsistence) and other members who believe that sustainable subsistence or commercial harvests of whales for food are appropriate. Many P

in NG appear to be based on reading MM rather than reading SC/57/O1. I do not have time to comment on each in detail.

P such as P3.28 claim that Japan believes it need not respond to scientific criticism from SC members. This is simply not true. For example, in the early years of JARPA, SC members pointed out that sighting surveys should be separated from sampling of whales. That advice was followed, and surveys and sampling remain separated in JARPA II.

Section 4 of NG: Japan's Counter-Memorial

As with the previous section, time does not permit me to discuss every P in detail. As in earlier sections, details that support Japan's work are omitted. E.g. under P4.3 of NG, the P from the mid-term and final reviews of JARPA summarizing the evaluation is truncated before its mention of possible higher catch with no increased risk to stocks based on JARPA data. P4.8 of NG claims non-lethal biopsy sampling is a better way than lethal take to obtain genetic samples; LW explains why that is not the case for Antarctic minke whales. Re P4.13, lethal sampling is required to obtain ages for use in the SCAA modeling that the SC has encouraged.

Section 5 of NG

The first bullet point under P5.9 of NG applies only to the early years of JARPA and not at all to JARPA II, as discussed above. Other bullet points contain similar mistakes and omissions.

Section 6 of NG

I will not comment on this section since it deals with a different research project, SORP, rather than with JARPA II.