Comments on Some Aspects of the AWI's Strategic Risk Assessment for Airgun Use in the Antarctic Treaty Area (27 March 2009 version)

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In response to a request from the Alfred-Wegener-Institut für Polar- und Meeresforschung, Bremerhaven (AWI), this document provides comments on selected portions of the document "Strategic assessment of the risk posed to marine mammals by the use of airguns in the Antarctic Treaty area" by O. Boebel, M. Breitzke, E. Burkhardt and H. Bornemann (27 March 2009 version; hereafter referred to as the RA). The request for comments was first discussed with me on 9 February 2011 and formalized on 12 April 2011.

As agreed, the present comments primarily concern sections III (hazard identification), IV (Exposure analysis), and V (risk management) of AWI's risk assessment; no attention was given to sections I and II. Also, within sections III – V, these comments concentrate on subsections concerning "*1. Direct, immediate injury*" and "*3. Biologically significant, acoustic disturbance*". Because of time limitations and the fact that it is not my primary are of expertise, I gave little attention to the subsections on "*2. Indirect, immediate damage*". As I did not review some parts of the RA, it is possible that some topics about which I found little or no discussion may be addressed elsewhere in the RA. In addition, I read the revised comments prepared in mid-2009 by three specialist reviewers at the request of the German Federal Environment Agency (UBA), and much of the UBA's 4 April 2011 "Evaluation" of the RA. This document also addresses some statements in those previous reviews and in the UBA's "Evaluation".

General Comments on Approach and Conclusions of the RA

The RA, very appropriately, concentrates on two major questions: the risk of injury associated with exposure to high levels of airgun sound, and the risk of biologically significant behavioral disturbance by those sounds. These are key areas given both their biological relevance and the fact that quantitative (or at least semi-quantitative) risk assessment procedures are available for application to these questions.

The conclusions in the RA about risk of injury and biologically-significant disturbance seem to me to be generally appropriate and (for the most part) well supported by the analyses and deductions presented in the RA. I have various comments about specific aspects of the procedures and assessments, but I do not expect that my suggestions or concerns (if further addressed in the RA) would result in major changes in the conclusions concerning risk of injury or biologically-significant disturbance. This is partly a result of the fact that the procedures used in the RA, especially in evaluating risk of injury, incorporate several "conservative"[1] assumptions, some (but not all) of which are summarized on p. 176 of the RA.

[1] The word "conservative" is used in these comments to mean "in favor of the animals" rather than in the arithmetic sense of "smaller". Thus, a conservative sound exposure criterion (in dB) would be numerically lower than might otherwise have been defined, but a conservative shutdown radius (in m) would be numerically larger. The word "precautionary" is often used as shorthand for this concept, but that word is not further used in these comments because of special connotations that it may have in some contexts and jurisdictions.

With regard to risk of injury, the RA follows the review by Southall et al. (2007)[2] in considering the onset of permanent threshold shift (PTS), not the onset of temporary threshold shift (TTS), as being the onset of injury. I concur with the RA on this important matter, and disagree with the assumption (in the UBA's "Evaluation" and by some earlier reviewers) that any TTS should be treated as injury. Those who consider TTS to be injury do not appear to be properly distinguishing between *onset* TTS (which is not injury) and high-levels of TTS, i.e., *profound* TTS (which may grade into PTS and into injury). This key issue is addressed in more detail later in these comments.

I consider the RA to be correct in concluding that there is little potential for *biologically significant* disturbance from a 2-D seismic survey of the type addressed in the RA. Many questions can be raised concerning various details of the disturbance assessment, mainly because there are few specific and relevant data, and there is much variability and uncertainty even in the few data that do exist. However, the existing data on marine mammal reactions to airgun sound pertain almost entirely to short-term behavioral responses that are not likely to have long-term consequences for individual marine mammals or their populations. These matters are also addressed in some detail later in these comments.

The risks associated with masking of natural sounds by sounds associated with the seismic survey are addressed in the RA in much less detail than are the injury and disturbance issues. The lesser level of detail for masking is somewhat appropriate given the lesser (in my view) likelihood of major biological effects from masking by discontinuous airgun sound as compared with injury or disturbance effects. There has been increasing concern in recent years about the masking effects of reverberation between airgun pulses, and it would have been appropriate to give this topic more attention in the RA. However, I concur that for seismic surveys — especially 2D surveys that do not remain in a given region for an extended period — masking is a topic of lesser concern than injury or disturbance.

Direct and Immediate Injury

"Direct and immediate (auditory) injury" from airgun sounds is, for marine mammals, a *hypothetical concern* rather than a proven fact. Given the available data from marine and terrestrial mammals, there is a scientific basis for expecting that auditory damage could, in some circumstances, occur in marine mammals close to an operating airgun array. However, to my knowledge, there has been no direct empirical demonstration of injury to marine mammals from airgun sounds. (There is, however, empirical evidence of injury to certain fish exposed at close range to airgun sound.)

To estimate the risk of direct and immediate injury, the RA uses the "science-based" approach developed by Southall et al. (2007), based on the more "conservative" of two sound exposure criteria (peak pressure and cumulative sound exposure level, SEL). Although dual criteria are applied, the SEL criterion is (in practice) usually the operative one, as the predicted maximum distance for auditory injury is generally greater when based on the SEL criterion. I should disclose that I am one of the (numerous) coauthors of the Southall et al. (2007) document. Given that, it will come as no surprise that I believe that this approach to estimating risk of auditory injury is (to a first approximation) a reasonable one. Some refinements are possible and desirable given new scientific developments since 2007 (mentioned later), but to a first approximation, the approach can be said to be based on the *current state of knowledge*, and to *reflect best possible practice*.

A previous reviewer noted that, in the USA, the cognizant regulatory agency (National Marine Fisheries Service/NOAA, hereafter "NMFS") has not adopted the dual injury criteria of Southall et al. (2007) and, for impulsive sounds, uses earlier 180 dB (for cetaceans) and 190 dB (for pinnipeds) re 1 μPa_{rms} criteria. At least for cetaceans, the old 180 dB_{rms} criterion is in effect more stringent than the Southall et al.

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[2] Southall, B.L. et al. 2007. Marine mammal noise exposure criteria: initial scientific recommendations. *Aquat. Mamm.* 33(4):i-iv, 411-522.

recommendations. However, as is widely known, the largely-arbitrary 180 and 190 dB re 1 dB_{rms} "do not exceed" criteria were proposed before there were any specific data on sound exposures necessary to elicit TTS or other auditory effects. (Again, I should disclose that I was on the HESS advisory committee that suggested those "conservative" RMS criteria to NMFS and other regulators many years ago.) Those old criteria are very inconsistent with current knowledge, e.g., they make no allowance for exposure duration, and do not represent best possible practice. Also, it should be noted that NMFS does in fact use the Southall et al. procedures when considering some types of high-intensity sounds other than airgun sounds. Furthermore, NMFS representatives have stated that they are moving toward adoption of the Southall et al. "dual criteria" after completing an internal review (e.g., Scholik-Schlomer 2010)[3].

Some reviewers of AWI's RA and of the Southall et al. approach in general have questioned whether it is appropriate to assume that the onset of auditory injury requires a sound exposure several decibels in excess of the TTS threshold. Some have suggested that any sound exposure sufficient to elicit TTS should be assumed capable of causing auditory injury, i.e., that "TTS = injury". UBA's "Evaluation" document takes that view. Southall et al. assume that PTS onset might occur upon exposure to impulse sounds (e.g., airgun pulses) with cumulative energy level as little as 15 dB above the TTS onset threshold (i.e., 15 dB above the energy level causing slight TTS). This is in fact a very "conservative" assumption, as described in detail in Southall et al. It is known that animals exposed to sound levels and durations sufficient to cause large amounts of TTS may incur slight PTS, i.e., that TTS grades into PTS at sufficiently high exposure levels. However, that requires far higher sound exposures than are necessary to cause the onset of TTS (e.g., Le Prell in press).[4] UBA's "Evaluation" (p. 30, paragr 3 & 6) cites a paper by Kujawa & Liberman (2009)[5] as evidence that TTS should be considered to be auditory injury because total recovery did not occur. However, the mice tested in that study were exposed to sufficient sound to elicit profound TTS (*ca.* 40 dB increase in threshold), not the slight onset TTS discussed by Southall et al. The latter is not associated with permanent damage. The Southall et al. approach (assuming that PTS onset occurs with an SEL exposure only 15 dB above TTS onset) is in fact "conservative".

Although the Southall et al. (2009) "dual criterion" approach is, in my view, an appropriate basis for the AWI risk assessment, it is a "first approximation". Many data gaps, assumptions, and extrapolations (some of them large) were identified by Southall et al. Some of these are further discussed in subsequent papers and in other reviewers' comments on the RA, and most of these issues have not yet been fully resolved. However, it is reasonable to assume that the several "conservative" aspects of the approach (see Table 36 on p. 176 of the RA) offset to a large degree the uncertainties and concerns about data gaps. In stating that, it should be noted that the Southall et al. (2007) approach and/or this specific RA incorporate additional "conservative" assumptions and procedures beyond those explicitly listed in Table 36 of the RA, and it would be useful to add these to that Table. For example,

- The RA does not apply the *M*-weighting functions recommended by Southall et al. (2007), which means that, for pinnipeds and especially odontocetes (e.g., the sperm whale), the RA's predicted radii for injury (and behavioral disturbance) are probably overestimated. (That does not apply to the low-frequency [*lf*, = baleen] whales, as M_{lf}-weighting when applied to airgun sounds has little effect on effective levels.)

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[3] Scholik-Schlomer, A.R. in press. Status of NOAA's guidelines for assessing impacts of anthropogenic sound on marine mammals. In A.N. Popper and A. Hawkins (eds., 2011). Effects of Noise on Aquatic Life. Springer, New York.

[4] Le Prell, C.G. in press. Noise-induced hearing loss: from animal models to human trials. *In* A.N. Popper and A. Hawkins (eds., 2011). *Effects of Noise on Aquatic Life*. Springer, New York.

[5] Kujawa, S.G. and M.C. Liberman. 2009. Adding insult to injury: cochlear nerve degeneration after "temporary" noise-induced hearing loss. *J. Neurosci.* 29(45): 14077-14085.

- The M-weighting functions were themselves established with "conservative" roll-off rates at low frequency. Also, recent data[6] show that, in odontocetes, susceptibility to TTS increases with increasing frequency above 3 kHz. More importantly for present purposes, those data imply (but do not show directly) that susceptibility of odontocetes to high-level sound may be reduced, relative to that assumed by Southall et al., when the predominant energy is below 3 kHz, as is the case with airgun sound.
- For pinnipeds, where TTS (and presumably PTS) thresholds differ greatly among the three species with substantial TTS data, Southall et al. based their proposed criteria on the species with the lowest TTS thresholds, the harbour (=common) seal, *Phoca vitulina*. Some other pinniped species appear significantly less prone to TTS (and presumably to PTS).
- Available data on responses of marine mammals to approaching seismic vessels show that many cetaceans and some pinnipeds begin moving away well before the received sound level is sufficiently high to cause TTS let alone PTS.

Nonetheless, TTS data are available for only a very few marine mammal species and a very few individuals of each species, and some unstudied species (or other individuals of studied species) may have lower TTS (and PTS) thresholds than now assumed. For example, Southall et al. (2007) acknowledged that the then-preliminary data of Lucke et al. from a harbour porpoise suggested that its TTS (and presumably PTS) thresholds were lower than those documented in the previous studies used by Southall et al. in setting recommended injury criteria. The Lucke et al. data were subsequently published as Lucke et al. (2009)[7]. They concluded that TTS onset in the harbour porpoise exposed to an airgun pulse occurred with a received energy level of 164 dB re 1 $\mu Pa^2 \cdot s$ (vs. 183 dB in the beluga—Southall et al. 2007). A further study of TTS in a harbour porpoise exposed to non-impulse noise (Kastelein et al. 2011)[8] tends to corroborate that this species can incur TTS with lower sound exposures that assumed by Southall et al. The RA notes that harbour porpoises and other "high-frequency cetaceans" (as defined by Southall et al. 2007) are absent from Antarctic waters. However, it remains a possibility that some other species present in the Antarctic have TTS (and PTS) thresholds lower than those assumed by Southall et al. (2007) and by this RA. Even so, the risk of auditory injury associated with possible overestimation of PTS thresholds for some species seems low given (a) all the "conservative" assumptions made by Southall et al. in setting their proposed criteria, (b) additional conservative assumptions noted above, and (c) the absence of "high-frequency cetacean" species from the Antarctic waters of concern here. It might be appropriate for the RA to address this issue more directly.

The assumption by Southall et al. (2007) that the injury criteria for baleen whales ("low-frequency cetaceans") should be the same as for odontocetes is an especially important assumption. There are no data on TTS thresholds (let alone PTS thresholds) for baleen whales. Southall et al. argue that the assumption of similar TTS (and PTS) thresholds for baleen and toothed whales is "conservative", but this remains an important and unverified assumption. This and the related discussion in a recent paper by Gedamke et al. (2011)[9] deserve at least some brief consideration in any update of the RA.

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[6] Finneran, J.J. and C.E. Schlundt. 2010. Frequency-dependent and longitudinal changes in noise-induced hearing loss in a bottlenose dolphin (*Tursiops truncatus*) (L). *J. Acoust. Soc. Am.* 128(2): 567-570.

[7] Lucke, K. et al. 2009. Temporary shift in masked hearing thresholds in a harbor porpoise (*Phocoena phocoena*) after exposure to seismic airgun stimuli. *J. Acoust. Soc. Am.* 125(6):4060-4070.

[8] Kastelein, R. et al. 2011. Temporary hearing threshold shifts and recovery in a harbor porpoise (*Phocoena phocoena*) and harbor seals (*Phoca vitulina*) exposed to white noise in a 1/1-octave band around 4 kHz. *J. Acoust. Soc. Am.* 129(4, Pt. 2):2432 [Abstract].

[9] Gedamke, J. et al. 2011. Assessing risk of baleen whale hearing loss from seismic surveys: The effect of uncertainty and individual variation. *J. Acoust. Soc. Am.* 129(1):496-506.

The RA gives relatively less attention to auditory effects on pinnipeds (and associated mitigation) than it does to cetaceans. The available TTS data suggest that some pinniped species are more prone to TTS and PTS than are the two best-studied odontocete species (beluga whale and bottlenose dolphin). TTS and PTS thresholds for Antarctic pinnipeds are unknown, but the possibility exists that auditory injury might extend to distances greater than applicable to cetaceans. In at least one location in the RA (item 4 on p. 232), the RA implies that only whales will be considered when determining whether a ramp-up needs to be interrupted upon appearance of an animal within the mitigation radius during ramp-up. (However, on p. 234 and 235, it is stated that detection of any marine mammal within the mitigation radius during ramp-up would trigger a shutdown.)

I note that the mitigation measures proposed in the RA (p. 229) do not include some of the measures used elsewhere. • For example, airgun operations are planned to continue without restriction in periods of low visibility. That is often permitted during seismic surveys, but in some jurisdictions at some times, there are specific visibility requirements — either for continuation of seismic surveys, or at least for startup of the airguns. The RA states that continuation of seismic surveys in periods of low visibility is reasonable partly on the basis that avoidance reactions can be expected at larger radii for most species. Based on our experience with Arctic seals approached by operating seismic vessels, most seals show only slight (if any) avoidance (e.g., Harris et al. 2001).[10] Thus, relying on avoidance of the seismic vessel by pinnipeds is not (in our experience with non-Antarctic species) a very effective mitigation measure. • The RA discusses and discounts (on p. 237) the possible use of passive acoustic methods. The RA states that use of a towed streamer behind *Polarstern* has proven ineffective for mammals other than sperm whales and possibly fin whales very close to the streamer. Our experience has been similar during numerous seismic surveys when a basic towed array was used in temperate and subtropical waters, with the exception that calls by additional odontocete species (along with sperm whales) have often been detected. If it were possible to deploy a more capable towed array system better able to localize calling odontocetes, then such a system could be a useful component of a real-time monitoring and mitigation system.

In contrast, the RA does mention use of the scanning thermal infrared system developed by the AWI, which is (to my knowledge) a unique and potentially a very valuable development for monitoring and mitigation purposes during seismic surveys. It would be useful if the RA provided additional details, or reference to a publication if there is one, documenting the effectiveness of the AWI infrared system.

Biologically Significant Acoustic Disturbance

Disturbance effects are addressed in the RA on the assumption that it is "biologically significant" effects that are of concern, and that lesser effects without long-term consequences for individuals or their populations are not a major issue. I agree with this general concept, which is widely held in the scientific community. Behavioral changes elicited by exposure to anthropogenic sounds are very much a graded phenomenon, ranging from slight and brief changes detectable only through statistical analysis of large samples of quantitative data, to conspicuous disruption of normal behaviour over large spatial and/or temporal scales. It is clear that, at the lower end of this continuum, slight and brief effects with no lasting consequences for individuals let alone populations should not be considered biologically significant. Given present scientific knowledge, the criteria of biological significance listed on p. 190–191 of the RA (from NRC 2005)[11] are reasonable, but to a considerable degree subjective.

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[10] Harris, R.E. et al. 2001. Seal responses to airgun sounds during summer seismic surveys in the Alaskan Beaufort Sea. *Mar. Mamm. Sci.* 17(4):795-812.

[11] NRC. 2005. Marine mammal populations and ocean noise/Determining when noise causes biologically significant effects. U.S. Nat. Res. Counc., Ocean Studies Board, Committee on Characterizing Biologically Significant Marine Mammal Behavior (D.W. Wartzok, J. Altmann, W. Au, K. Ralls, A. Starfield, and P.L. Tyack). Nat. Acad. Press, Washington, DC. 126 p.

I also agree with the main conclusions summarized in section IV, Risk analysis: Exposure Analysis — Discussion (p. 224-225). Notwithstanding the limited and highly variable data on disturbance responses to seismic surveys, available data suggest that, for a wide-ranging 2-D seismic survey of this type, few (if any) individual marine mammals are likely to be subject to biologically significant disturbance. I concur with the RA's conclusion that mammals engaged in feeding and nurturing are of most concern, and that (during a largely linear 2-D seismic survey) few if any of these individuals will be exposed to strong air-gun sound for sufficiently long or sufficiently often for biologically significant disturbance to occur.

The RA quite appropriately notes (top of p. 225) that these conclusions pertain to wide-ranging seismic surveys ("2-D surveys") where the ship does not remain in or return repeatedly to a specific biologically-important area. Potential effects could be greater in the case of 3-D surveys involving prolonged airgun operations within a small area. If that type of seismic survey is planned in future, the effects on marine mammals should be re-evaluated.

I also concur that biologically-important levels of TTS will not extend far enough from the airguns, and will not extend to sufficiently high frequencies, to cause more than a negligible increase in risk of killer whale predation. The relatively high theoretical TTS radii in the lower right part of the RA's Table 46 concern "slight TTS". Radii for biologically-important TTS are not calculated in the RA but would be less than those in Table 46, and would be further reduced at the frequencies of killer whale sounds.

Although I concur with these general conclusions about biologically-significant disturbance, various specific points raised in the RA concerning disturbance deserve (in my opinion) some comment and perhaps further consideration. Some of these specific points are briefly noted below. I do not expect that revisions of the RA in response to any or all of these specific comments would result in major changes in the conclusions, but there would be some refinements. Some of these refinements would result in somewhat greater predicted disturbance effects and other refinements would result in reductions in predicted disturbance effects:

1. On p. 189, 2nd-last paragraph it is stated that "To our knowledge, the only [behavioral] response documented in the field is avoidance". In fact, other types of changes in behaviour upon exposure to airgun sound have often been documented, e.g., quantitative changes in durations of surfacings and dives, and in the number of respirations per surfacing. It is doubtful that these types of changes are biologically significant. Changes in the predominant activity of the animals, e.g., from resting, feeding or socializing to travel, are also common upon exposure to airgun sounds, but those changes could be considered part of an avoidance response.
2. On p. 190 (middle), the RA states that a received level of 160 dB_{rms} (representing the approximate centroid of reported response thresholds) will be used in the RA as a response criterion. Although 160 dB re 1 μPa_{rms} is probably close to the centroid, for some species and situations behavioral responses (although mainly of a temporary nature) occur at lower received levels. For cetaceans, this is illustrated by Table 39 (p. 192) in the RA.[12] For other species and situations (especially pinnipeds) responses generally are not evident unless received levels of airgun pulses are higher than 160 dB re 1 μPa_{rms}. Thus, no single received level criterion (160 dB_{rms} or otherwise) is universally appropriate in predicting when disturbance will occur. However, it should

[12] Another issue with some of the values tabulated in Table 39 of the RA is that measurement procedures used to determine acoustic exposure levels during studies in the 1980s differed from the "RMS over duration of pulse" method widely used since the 1990s. For example, the levels quoted by Richardson et al. (1986, *J. Acoust. Soc. Am.*) are 3 dB less than peak pressure levels. "RMS over pulse duration" levels would have been ~10 dB lower.

also be noted that most of the behavioral responses summarized in Table 39 would not be considered biologically significant based on the criteria listed on p. 190–191. Also, I note that, later in the RA (chapter IV), a 170 (rather than 160) dB_{rms} criterion is apparently used for pinnipeds, and a 170 dB re 1 μPa_{rms} criterion is more consistent with available pinniped data.

3. The three review articles cited near the bottom of p. 191 probably summarize most of the relevant formally-published papers on disturbance to marine mammals, as stated in the RA. However, there are a large number of additional relevant "unpublished" technical reports (some of them more intensively refereed than most journal papers). My general impression, however, is that consideration of those additional studies would not appreciably change the RA's conclusions.

4. A few of the entries in Table 39 do not seem entirely appropriate. Entry S6.4 (Ljungblad et al. 1988) dealt with migrating as well as feeding bowheads. Entries S6.5 (Todd et al. 1996), S7.4 (Akamatsu et al. 1993) and G10 (Ridgway et al. 1997) did not deal with exposure to airgun (or similar) pulses insofar as I can recall. However, refinement or exclusion of these entries would not materially change the conclusions.

5. On p. 193, item (a) states that "160/170 dB is the currently used [disturbance] threshold for cetaceans/pinnipeds (i.e. by NMFS…". In fact, in estimating numbers of marine mammals "taken by harassment" during seismic surveys, NMFS has—in recent years—considered 160 dB re 1 μPa_{rms} to be the threshold for disturbance to pinnipeds as well as all cetaceans. In my view, the NMFS position is not well supported by the available scientific data, and a 170 dB_{rms} criterion would be more appropriate not only for pinnipeds but also for delphinids (dolphins and related small toothed whales). Thus, in my view, the RA's use of 170 dB_{rms} for pinnipeds is appropriate, and its use of 160 dB_{rms} as the disturbance criterion for delphinids is "conservative" (i.e., likely to overestimate actual numbers disturbed to a biologically significant degree).

6. Pages 193–194 of the RA outline a rationale for selecting a single "centroid" value (160 dB re 1 μPa_{rms} for cetaceans) as the disturbance criterion. For some species and situations, a high proportion of the individual animals react behaviorally to airgun pulses with received levels lower than this, e.g., for migrating bowhead whales in the Arctic, as acknowledged in the RA [p. 193] and also to a lesser degree for beluga whales in the Arctic (Miller et al. 2005).[13] In other situations, most individuals do not react unless received levels are notably higher than 160 $dB_{rms.}$ For the first group of species, numbers disturbed will be underestimated if a 160 dB_{rms} criterion is assumed. Although neither bowhead nor beluga whales occur in the Antarctic, data on responses to airguns are lacking or inadequate for some species that do occur in Antarctic waters, including southern right whales (which are closely related to bowhead whales). Thus, it should be acknowledged that using a uniform 160 dB_{rms} criterion will overstate numbers of some species that are likely to be disturbed, but it may also underestimate numbers of other species that could be disturbed.

7. The concept of risk functions (p. 194 of the RA) is reasonable, and more realistic than the common approach of assuming one particular "threshold" of disturbance (step function). However, with one possible exception (Malme and Miles 1985)[14], currently available scientific data on behavioral responses of marine mammals to airgun sound do not provide a good basis for

[13] Miller, G.W. et al. 2005. Monitoring seismic effects on marine mammals—southeastern Beaufort Sea, 2001-2002. p. 511-542 *In*: S.L. Armsworthy et al. (eds.), Offshore oil and gas environmental effects monitoring / Approaches and technologies. Battelle Press, Columbus, OH.

[14] Malme, C.I. and P.R. Miles. 1985. Behavioral responses of marine mammals (gray whales) to seismic discharges. p. 253-280 *In*: G.D. Greene, F.R. Engelhardt and R.J. Paterson (eds.), Proc. workshop on effects of explosives use in the marine environment, Jan. 1985, Halifax, N.S. Tech. Rep. 5. Can. Oil & Gas Lands Admin., Environ. Prot. Br., Ottawa, Ont. 398 p.

defining the shape or parameters of the risk function. The one possible exception, concerning migrating gray whales in California, is not directly relevant to this RA. An as-yet-unpublished study of the disturbance "risk function" was a well-controlled study of captive dolphins exposed to sonar-like pings; in that study, the risk function showed evidence of asymmetry, i.e., a longer tail to the left than to the right.[15] The near-absence of data needed to parameterize the risk function for Antarctic species exposed to airgun pulses is a data gap, but is not a specific problem for this RA as the risk function concept is not actually used in the analyses presented in this RA.

8. Page 195 of the RA discusses an appropriate averaging time for computation of the RMS pressure metrics used in relating received levels of airgun pulses to disturbance. A standard duration of 200 ms is chosen. This procedure differs from that used in most of the studies that led to the selection of the 160 dB_{rms} disturbance criterion. Most of them calculated the SPL_{rms} over the interval within which 90% of the pulse energy was received. That interval varies considerably with distance from source, water depth, and probably other factors. The appropriateness of using the "90% energy duration" in these calculations has been strongly criticized, and opinions differ regarding what approach would be best. Some of the reviews of the RA prepared for the UBA raise concerns about this issue. In any case, most of the available data on marine mammal responses to airguns involved use of an acoustic metric based on the "90% energy duration", not a fixed 200 ms duration. The 90% energy duration is often less than 200 ms (especially when close to the airguns), but is often more than 200 ms (especially at long distances). Sound propagation modeling is not my specialty, but I understand that full-waveform models exist that can directly predict the received RMS pressure level on a 90% energy basis. If so, then it might be better to apply such a model to predict received pressure levels than to assume a fixed 200 ms duration that is inconsistent with procedures during the empirical work that led to the 160 dB_{rms} criterion. Such an approach would, I expect, predict received levels ranging from a few dB lower to a few dB higher than those obtained with the 200 ms assumption. There would be corresponding differences in predicted distances from the source beyond which any specified RMS level would not occur. However, there is much variability in acoustical measurements and observed behavioral responses, and it is not known which acoustical metric is best correlated with the degree of behavioral response to airgun pulses. Given that, any problems associated with use of the fixed 200 ms duration are probably slight relative to other sources of variability and uncertainty in the data.

9. I concur that, in calculating maximum potential distances out to which disturbance might occur, there should be allowance for the maximum depths of dives by the species under consideration. Page 219 (bottom) of the RA notes that species-specific maximum depths were considered. However, I did not find a specific tabulation of the maximum depths assumed for certain species. For most species, Tables 28–30 in the RA quote a variety of depths that might have been considered as maxima.

10. A key assumption of the analysis in the RA was that exposure to 160 or 170 dB re 1 μPa_{rms} for less than 2 h would not result in biologically-significant disturbance to migrating or feeding marine mammals (p. 221 of the RA). This assumption was related to guidance provided by Southall et al. (2007, p. 448), who indicated (as noted on p. 191 of the RA) that "a reaction lasting less than 24 h and not recurring on subsequent days is not regarded as particularly severe unless it could directly affect survival or reproduction". The RA makes the "conservative" assumption that the duration of disturbance associated with exposure to 160 or 170 dB for <2 h would be less than the 24 h specified by Southall et al. In contrast, the U.S. NMFS approach is to assume that exposure to even a single airgun pulse with a received level exceeding 160 dB re 1 μPa_{rms} should

[15] Houser, D.S. et al. 2011. Behavioral reactions of dolphins and sea lions to sonarlike sound exposure. *J. Acoust. Soc. Am.* 129(4, Pt. 2):2432 [Abstract].

be counted as a potential "take by harassment". However, the RA is attempting to estimate numbers of animals that would incur biologically significant disturbance as defined on p. 190–191. In contrast, the NMFS approach is simply to calculate numbers of marine mammals that might be exposed to at least one pulse with received level ≥160 dB re 1 μPa_{rms}. For most if not all species and situations, exposure to a single 160 dB_{rms} pulse, or even a fairly prolonged sequence of pulses at that or somewhat higher levels, is unlikely to elicit biologically significant disturbance responses as defined on p. 190–191. The approach in the RA seems to me to be reasonable for obtaining a "conservative" estimate of numbers that might incur biologically significant disturbance. However, that is largely a matter of professional judgement, given the near total lack of specific data as to the biological consequences of various levels of sound exposure, and on the effects of exposure duration on those consequences.

11. The RA (p. 222) acknowledges that one cannot disprove the possibility that noise-induced disturbance might increase the likelihood that a young seal pup or cetacean calf could be permanently separated from its mother. I concur that this possibility cannot be totally excluded, but I consider it to be quite unlikely. Considering ***cetaceans***, we have observed the reactions of bowhead whale mother / calf pairs in the Arctic to airgun sound and other noisy industrial activities. Upon approach by an anthropogenic sound source, the mother bowhead normally approaches and rejoins the calf when the noise source is still distant and the received anthropogenic sound is still weak and thus unlikely to mask calls used by the re-joining mother and calf (Koski et al. 1988)[16]. There are few specific data on received sound levels that elicit re-joining or significant disturbance responses by mother or calf cetaceans as opposed to "others". However, my general sense is that (at least in bowhead whales) rejoining and subsequent avoidance responses by mother/calf pairs tend to begin at a lower received level than that typically eliciting avoidance in "other" bowheads. That is, mother/calf pairs tend to be somewhat more responsive than "others". Available data are insufficient to show how much lower the response threshold is for mother/calf pairs than for "others". In general, a lower response threshold is probably beneficial in that it will reduce the likelihood of a close encounter with the source of disturbance, and the potentially more significant effects that might then ensue.

12. In the case of ***seals with young pups on the ice***, there might be circumstances in which disturbed mothers would enter the water leaving the pups on the ice. I am not familiar with the seasonal cycles of Antarctic seals, but expect that most if not all seal pups would be weaned and largely independent by the date when ice conditions would be suitable for marine seismic operations. In any case, seismic vessels normally avoid floating ice to avoid damage to the seismic equipment. Also, adult seals routinely leave their pups alone on the ice under natural conditions, so I would expect that pups on the ice could tolerate temporary separation triggered by passage of a seismic vessel. This assumes that the vessel would not approach seals on the ice sufficiently early in the season, or sufficiently closely, to prompt pups to dive into the water before they are fully capable of doing so. If the (limited) reactions of Arctic seals to approaching seismic vessels (e.g., Harris et al. *op. cit.*) are relevant to the Antarctic, adult seals that leave the ice in response to an approaching seismic vessel would not be expected to move very far away. I would not expect the vessel-induced separation to be lengthy or qualitatively different from mother–pup separations that occur naturally when a foraging mother seal leaves her pup alone on the ice. Besides these biological considerations, the seismic operator will no doubt (for operational reasons) seek to avoid scheduling seismic operations in seasons and regions with much floating ice, and during a seismic survey will seek to avoid floating ice, thus reducing potential effects on any unweaned seal pups that might still be present on the ice.

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[16] Koski, W.R., G.W. Miller and R.A. Davis. 1988. The potential effects of tanker traffic on the bowhead whale in the Beaufort Sea. Environ. Stud. 58. Rep. from LGL Ltd., King City, Ont., for Dep. Indian Affairs & North. Devel., Hull, Que. 150 p. NTIS MIC-90-04552.

13. The RA addresses (p. 222–224) the possibilities that masking or TTS might increase the vulnerability of marine mammals to predators, particularly the killer whale. I concur with the RA's conclusion that any such increased vulnerability would be slight. Both masking and TTS occur primarily at and near the frequencies of the introduced sound—here ship sound and airgun pulses. For both of these sources, most of the emitted energy is at frequencies well below those of killer whale calls. Although the weaker components of airgun and ship sound at and near the frequencies of killer whale calls could have a slight masking effect, detectability of those calls would not be strongly affected by masking or TTS induced by ship or airgun sound.

As noted near the start of this section, many questions can be raised concerning various details of the disturbance assessment. These questions arise because there are few specific and relevant data, and there is much variability and uncertainty even in the few data that do exist. It is important to recognize that the existing data on marine mammal reactions to airgun sound pertain almost entirely to short-term behavioral responses that are not likely to have long-term consequences for individual marine mammals or their populations. Given the reasonable assumptions listed on p. 190–191 of the RA concerning the definition of biologically significant disturbance, I consider the RA to be correct in concluding that there is little potential for biologically significant disturbance from a 2-D seismic survey of the type addressed here.

Additional Matters Arising from UBA's "Evaluation"

This section briefly comments on some specific statements in parts of UBA's "Evaluation" document.

1. Page 7 last paragraph of the UBA "Evaluation" notes that "…it is essential to choose a standard of protection that gives equal consideration to protecting the environment and preserving the freedom of scientific research in the Antarctic. The overriding aim should be to achieve the greatest possible gain in scientific knowledge with the least possible adverse impact on the Antarctic environment." As I interpret it, this statement describes a generally "balanced" approach that seems appropriate to me. However, in subsequent parts of the "Evaluation", I saw no further reference to achieving the greatest possible gain in scientific knowledge.
2. On p. 11 (item "1"), the Evaluation notes that risk is underestimated because frequencies above 256 Hz are not considered in AWI's modelling. In most (not all) situations, the received seismic energy above 256 Hz is only a fraction of that below 256 Hz, and in those cases pressure levels and particularly SEL values would not be much different if higher frequencies were included.
3. On p. 13 (paragr 2), the Evaluation indicates that—when received levels do not diminish progressively with increasing distance—the rationale for safety radii is dubious. Although that is correct, safety radii are normally based on relatively high broadband exposure levels (e.g., 180 dB re 1 μPa_{rms}). It is my impression that the fall-off of received broadband level with increasing distance is usually fairly consistent out to distances beyond the 180 dB re 1 μPa_{rms} distance. As noted earlier, I am not a specialist in sound propagation phenomena, but my impression is that the more complex non-monotonic relationships between received level and distance tend to occur predominantly at longer distances and/or when dealing with a single frequency (where interference effects become important).
4. UBA concerns (p. 14 paragr 2 in "Evaluation") about AWI's use of a 200 ms time window were addressed earlier in these comments (see p. 8, item 8, above).
5. Page 22 paragr 3 of the "Evaluation" (and also p. 27, near bottom) concludes that Antarctic seals must be considered in any effective mitigation strategy. I concur with this. [I limit my comments to marine mammals and do not comment on questions about penguins.]

6. Page 24 paragr 1 states that studies by Williams et al. (2006, *Biol. Conserv.*) and Miller et al. (2009, *Deep-Sea Res. I*) showed that sound levels below 160 dB SPL_{RMS} can lead to significant changes in foraging.

   - The Miller et al. citation is generally appropriate, although it is debatable whether the observed changes in foraging would be biologically significant if elicited on just one occasion (as expected during a 2-D seismic survey).
   - Williams et al. were dealing with boat noise, not pulsed sounds. It has been shown that received RMS levels generally must be higher in order to elicit a given response if the sounds are pulsed (e.g., from airguns) than if they are ongoing (e.g., boat noise).[17]
   - The same paragraph cites Clark and Gagnon (2006, *IWC SC/58/E9*) with reference to sounds "at this level". However, I believe that Clark and Gagnon's levels were based on a measurement method very different from the usual "RMS over 90% pulse duration" method so direct comparison is inappropriate.

7. Page 24 paragr 2 notes that seismic activity could ensonify krill swarms several hundred kilometres away, "thus having a considerable impact on the regional availability of food resources". Although weak airgun pulses could at times be detectable by instruments several hundred kilometres away, I am not aware of any evidence that such weak airgun pulses (or, for that matter, stronger pulses) have negative effects on krill. The "Evaluation" does make clear that this comment about "considerable impact" is speculative.

8. p. 26, last paragr, expresses concern about overlap in frequency between whale vocalizations and the spectra of airgun pulses, and the potential for masking of whale calls. I concur that the masking issue should be discussed in more detail in the RA. However, this paragraph in the "Evaluation" should acknowledge that the potential for masking by airgun pulses is much reduced (in most situations) by the intermittent nature of the pulses. Reverberation can reduce audibility of calls received between pulses, and occasionally this reduction in audibility is severe. However, the majority of the time, the reverberation is relatively weak. Many studies have shown that whale calls received "between the pulses" are detectable to researchers listening by hydrophone and presumably also detectable to other whales.

9. p. 27, 4th paragr (also p. 28, bottom): UBA notes that, for deep and long divers, visual monitoring will often miss animals that are present and will not achieve sufficient risk reduction to prevent airgun pulses from causing harm. I concur that visual observers have limited ability to detect deep divers, and that this is a concern. However, the "Evaluation" should acknowledge that the risk of exposure to high-level sounds and potential harm will be reduced through avoidance reactions. Many of the deep-diving cetaceans (like the majority of other cetaceans) are expected to exhibit avoidance as the seismic vessel approaches. As a result of this responsive movement, the number of individuals that will occur within the safety radius is expected to be considerably reduced relative to the number expected purely on the basis of the density of animals in the region prior to the approach of the seismic vessel.

10. p. 28, paragr 2: The "Evaluation" suggests that displacement of marine mammals or their prey from feeding areas, or additional stress, may be significant in affecting marine mammal fitness, and that this is not adequately addressed by AWI. AWI's RA did point out the limited duration over which any given marine mammal would be exposed to high-level sound. The RA noted that, during a 2-D seismic survey of the type AWI conducts, exposure to high-level sound would not be expected to cause biologically significant effects according to the criteria proposed by the U.S.

[17] Richardson, W.J., C.R. Greene Jr., C.I. Malme and D.H. Thomson. 1995. Marine Mammals and Noise. Academic Press, San Diego, CA. 576 p.

NRC (2005—*op. cit.*). UBA's concerns about displacement would be more relevant to a 3-D survey in which the seismic vessel might operate in a given area for a much longer period than planned for the 2-D seismic projects addressed in the RA.

11. On p. 30, paragr 3 and 5, the "Evaluation" cites Kujawa & Liberman (2009—*op. cit.*) as evidence that TTS should be regarded as an injury. As discussed earlier (p. 3 of these comments), Kujawa & Liberman exposed mice to airborne sounds that were sufficiently prolonged and strong to elicit 40 dB of TTS. That is a profound level of TTS, not at all consistent with the TTS onset thresholds discussed by Southall et al. (2007) and others. Onset TTS is not permanent and is not injury.

12. On p. 30, paragr 4, the "Evaluation" points out that Lucke et al. (2009—*op cit.*) found that the TTS threshold for a harbour porpoise exposed to an airgun pulse was lower than previously found for larger odontocetes. This was an important finding, now to some degree corroborated by an independent study on another harbour porpoise (Kastelein et al. 2011—*op. cit.*). The implications, for areas where porpoises and other high-frequency odontocetes occur, are discussed on p. 4 of these comments. For high-frequency odontocetes, an appropriate injury criterion might be the expected PTS onset value for the harbour porpoise, which would be 15 dB above the Lucke et al. TTS onset level (i.e., 164 +15 = 179 dB re 1 $\mu Pa^2 \cdot s$). Contrary to UBA's conclusion at the bottom of p. 37 of the "Evaluation", the 164 dB re 1 $\mu Pa^2 \cdot s$ TTS value from Lucke et al. is <u>not</u> a logical injury criterion. It is an onset TTS threshold, not a PTS threshold; onset TTS is not injury, as previously discussed. Also, it pertains to a high-frequency odontocete species, and that category of odontocetes apparently is absent from AWI's operating area.

13. On p. 31, the UBA "Evaluation" discusses whether multiple TTS incidents cause injury. I will not comment in detail on this, given that it is based in part on the assumption that "a single TTS constitutes injury", which is unfounded. However, it should be noted that

    - the Southall et al. criteria applied by API in their RA are "conservative" in that they assume no recovery between pulses.
    - Recent TTS research in marine mammals is starting to show recovery effects even with relatively short intervals between successive sound exposures.
    - With a 2-D survey of the type evaluated in the RA, a given marine mammal is unlikely to be exposed to more than one sequence of strong airgun pulses.

14. On p. 32 of the Evaluation, UBA correctly notes that the Lucke et al. (2009) finding of a lower TTS-onset threshold in harbour porpoises was important, indicating that the injury criteria proposed by Southall et al. (2007) for high-frequency odontocetes are not "conservative". In the absence of such species in the proposed operating area, this finding is of somewhat limited relevance to this assessment. However, given the very limited number of cetacean species for which data on TTS onset thresholds are available, it is appropriate to be cautious. One cannot be sure that the TTS and PTS onset values assumed by Southall et al. (and AWI's RA) for mid- (and low) frequency cetaceans apply to all species in those groups. Nonetheless, the risk of auditory injury associated with possible overestimation of PTS thresholds for some species seems low given the variety of conservative assumptions made in the assessment (as discussed on p. 3–4 of these comments).

15. The discussion at the top of p. 36 of the Evaluation is inappropriate. Williams et al. (2006, *Biol. Conserv.*) were dealing mainly exposure to different types of vessels than those for which levels (at unspecified distances) are quoted. Also, one cannot assume that cetacean response threshold for pulsed airgun sounds will be similar to that for continuous vessel sounds. (Available data indicate that response thresholds are typically lower for continuous sounds.)

16. Page 36 paragr 2 and p. 38 paragr 3 of the "Evaluation" express concern about "biologically significant disturbances that only take effect in the long term". In my view, it is implausible that a 2-D seismic survey involving a few widely-spaced survey lines would have sufficiently large or prolonged disturbance effects for the disturbance to be biologically significant.[18] Few marine mammals will be closely approached by the airguns on more than one occasion in a given season.

17. Page 36 paragr 6 and p. 38 paragr 4 of the "Evaluation" argue that the disturbance criterion should be something far below the 160 (or 160–170) dB re 1 μPa_{rms} value used by AWI's RA. However,

    - most studies that have shown behavioral responses to received levels below 150–160 dB re 1 μPa_{rms} have involved sounds that are more continuous than are sequences of airgun pulses;
    - the responses occurring at lower received levels generally do not come close to meeting the criteria of biological significance outlined by NRC (2005—*op. cit.*) and used in AWI's RA;
    - the 120 dB sound level said (on p. 36 paragr. 6) to be associated with fin whale avoidance, based on Castellote et al. (2010)[19], is not at all comparable to other reported values. The other values are based mainly on the "RMS over pulse duration" method. The 120 dB value apparently was obtained by averaging received sound over 15 min periods. The received levels during the pulses themselves could have been much higher than 120 dB, but those levels were not reported.

18. Table 4 on p. 40 of the "Evaluation" identifies 5 possible SEL criteria that might be used in defining safe exposure limits. The SEL values listed in Table 4 are 198, 186, 180, 171 and 160 dB re 1 $\mu Pa^2 \cdot s$. The 198 and 186 dB SEL values correspond to those recommended by Southall et al. (2007) for cetaceans and seals, respectively, based on "conservatively"-estimated PTS onset values. As discussed above, based on now-available data, there would be justification for recommending a lower value for porpoises and other high-frequency odontocetes, on the order of 179 dB re 1 $\mu Pa^2 \cdot s$ (similar to the 180 dB SEL value listed in Table 4). However, porpoises and other high-frequency odontocetes apparently do not occur in AWI's planned Antarctic operating area. Use of a 179 or 180 dB SEL criterion in the Antarctic would, therefore, be a very "conservative" approach. It would protect against the possibility that some species of marine mammals that do occur in the planned operating area have auditory systems as susceptible to injury as is the harbour porpoise. Use of any lower SEL criterion (e.g., 171 or 160 dB SEL) in defining safety radii does not seem justified based on present knowledge, assuming that safe exposure limits are intended to avoid risk of auditory injury.

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[18] As noted on p. 1 paragr 2 of these comments, I have not specifically addressed the topic of "Indirect, Immediate Damage", which is discussed in the RA. In general, I am not aware of any conclusive evidence that marine seismic surveys have caused the types of disturbance-related cetacean strandings and deaths that have been associated with Navy use of mid-frequency sonars. A beaked whale stranding in Gulf of California (Mexico) concurrent with a marine seismic operation may or may not have been associated with the seismic work; the link is inconclusive.

[19] Castellote, M. et al. 2010. Potential negative effects in the reproduction and survival on fin whales (*Balaenoptera physalus*) by shipping and airgun noise. Intern. Whal. Comm. Working Pap. SC/62/E3. 12 p.