Review of AWI Risk Assessment and Critiques by UBA and Outside Reviewers

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Introduction

With one exception, AWI used the best, most relevant science available in 2009 to write the present version of this RA. The present review will discuss the implications of that one exception, and of several other new papers that were not available in 2009, where appropriate.

The structure of this risk assessment is similar to those used by industry and some North American government agencies. It gives an accurate, lucid, and thorough description of the Southall et al. (2007) noise exposure criteria and then uses these criteria correctly in proposing critical radii for PTS effects.

The RA seems to make environmentally safe decisions throughout. For example, it makes the simplifying assumption that animals remain static during exposure to airgun arrays. Stone and Tasker (2006), the "one exception" mentioned above, showed that most species of cetaceans they studied moved away from the source during shooting. They analyzed behavioral responses recorded by marine mammal observers on 201 seismic surveys in Europe, most of which used large arrays (>3,000 cubic inches). All species observed, except pilot and sperm whales, moved away from the passing array. Pilot whales oriented away; sperm whales remained static. Moving away reduces the received level and the risk of exposure. Therefore, assuming that animals remain static is the worst possible case animals could experience and produces larger, safer critical radii.

The UBA document and most of the outside reviewers overestimate the risk from this project. The AWI array is smaller in number of guns and total volume than typical industry arrays (industry uses up to 45 guns per array with volumes of 4,500 cu in or greater). This difference is relevant because Stone and Tasker (2006) found that small arrays (in terms of volume) had less effect than large arrays. The reason for this difference is not clear from present data, perhaps horizontal propagation of high frequencies, differences in end fire, etc. More importantly, industry shoots in a tight grid with shot lines as close together as 50 m, which means repeated exposures for some animals. AWI proposes shooting in a single shot line with no repeat exposures. Finally, and most importantly, industry surveys may last up to two months instead of the average of 13 days that AWI surveys have historically lasted (present RA). These differences affect many of the risk factors that are discussed in this RA and by the reviewers. By not considering these differences, the reviewers arrive at a much higher level of safeguarding than is used elsewhere for a similar exposure regimen.

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Criteria for "direct, immediate injury"

PTS is the most scientifically defensible threshold for injury. Physical injury involves cell damage; TTS does not, as the RA states. TTS is physiological fatigue of hair cells. No professional in the field of noise effects on mammalian hearing would agree that TTS constitutes injury. It is a normal, evolutionary protective mechanism that animals and humans experience daily. Using TTS as the threshold for injury would be unwarranted extra precaution. Also, it is illogical to define injury by a process that involves no injury.

Some reviewers argued that TTS automatically leads to PTS, and cite the paper by Kujawa and Liberman as justification. That paper just provides the mechanism for a phenomenon that has long been known, namely that daily TTS in the human workplace over years results in PTS. But, because of the exposure regimen of the AWI array (small size, single shot line, see above), Antarctic mammals might experience TTS on one day a year at most. An animal being passed by an array moving at 5 kn might experience a threshold shift if it is within 500 m of the array. But, calculations made by the Southall et al. authors showed that the accumulated energy from all audible shots, excluding the 5 or 6 shots at the closest approach point, will not add up to the SEL associated with TTS. A similar result was reported in Figure 85 of the RA. Stone and Tasker (2006) showed that almost all cetaceans move away from a firing array, and that the average closest distance of approach to arrays is 900 m. Moving to this range basically eliminates the risk of even a single TTS, especially from a small array.

We (Southall et al., 2007) were justified in applying the criteria for mid-frequency cetaceans to low-frequency mysticetes. The Wenz curves for ambient sound level spectra (Wenz, 1962) show much more ambient noise below 1 kHz (the band in which large whales communicate and airguns produce their greatest energy), than at 3 kHz where TTS in mid-frequency cetacean was measured. Because of this higher noise floor at frequencies less than 1 kHz, whales *cannot* have better hearing thresholds in that band than mid-frequency cetaceans do. So, applying the criteria for mid-frequency cetaceans to mysticetes at those frequencies is valid and highly precautionary. Whale hearing thresholds above 1 kHz are not noise limited but have not yet been measured. As the RA states, airgun output is 10-15 dB lower for high frequencies, making it unlikely that the higher frequencies will cause TTS. To assume that they will in the absence of data is a logical fallacy.

It is not true, as one reviewer claimed, that the Finneran threshold measurements were made in a noisy background. The animals were tested in an above-ground tank where background noise was very low compared to harbor waters where the animals are housed (see Finneran et al., 2010a).

The authors of Southall et al. now know that using noise exposure criteria for mid-frequency cetaceans as criteria for high frequency cetaceans was inappropriate. We used the best data then available, which were for mid-frequency dolphins exposed to a 3 kHz signal. Since then, new data were published showing that at higher frequencies, TTS onset occurs at lower exposures, and TTS growth rate after onset accelerates (Finneran and Schlundt, 2010b). This misuse does not invalidate the other criteria. It does validate our use of the term “initial” in the paper’s title, and our structuring the criteria for fast modification when new data become available. We do need more TTS data to set valid criteria for high frequency cetaceans. But, arguing about criteria for high frequency animals is irrelevant in the present context. There are no high frequency cetaceans in the Antarctic.

The dual criteria approach we used is not a perfect index, but it is highly superior to the use of any single criterion. The criteria expressed in SEL will be the ones most frequently applicable because noise effects on mammalian ears are most closely correlated with energy. We included criteria expressed as pressure for those acoustic events in which an extreme spike in pressure may occur within a longer signal that is best described in SEL. There is a need for some metric other than SEL, but only for extreme exposures where the correlation between SEL and TTS breaks down. SEL does predict TTS well at moderate and low exposures. It is the best metric we presently have; it should not be abandoned because it is not perfect over all exposures. No known metric is. Until a new metric is found, our dual criteria approach represents the best currently available science.

Defining the onset of PTS as 40 dB of threshold shift was based on extensive literature showing that humans and many other terrestrial mammals reliably recover from 40 dB of threshold shift, but not always from shifts of greater magnitude. The conservation of this feature over many taxa suggests that it also applies to marine mammals. This definition has been partly validated by events that are described on page 176 of the AWI RA. Using the best-available scientific information, 40 dB of shift is a good first approximation for the onset of PTS in marine mammals. Actual data on this subject will come from a study that is measuring PTS onset in sick or injured animals that are slated for euthanasia.

The assertion by one reviewer, that NOAA's failure to adopt the Southall et al. criteria implies a rejection of the criteria, is untrue. NOAA has been slow to adopt them due to bureaucratic inaction. In 2005 NOAA let a contract to have the draft criteria converted to regulations. This failed because the criteria were too complex for the contractor to understand. NOAA lost momentum on this effort because it is technically difficult, and because regulatory problems arose that were more pressing than writing noise exposure regulations. NOAA is now actively working on the problem again.

NOAA will most likely adopt the injury criteria as written. NOAA is bound by the Administrative Procedures Act to make no regulatory decision that is "arbitrary and capricious." All regulatory decisions must be based on the best available science, and have a rational link between these decisions and published data. The injury criteria were derived by a rational process that was in no way "arbitrary and capricious."

Criteria for "biologically significant acoustic disturbance"

The type of operation described in this RA is no more likely to cause biologically significant behavioral effects than it is to cause physical injury. The proposed array and its method of use will cause movement away from the array. Stone and Tasker (2006) predict this, and conclude that such movements are temporary. Changes in the fecundity or survival of individuals (i.e., "biologically significant" effects) are not likely to accompany brief, single, temporary moves. Prey (fish and invertebrates) do move in response to the single passage of an anthropogenic noise source, but they move vertically and briefly (example, Doksæter et al., 2009). [The Engås and Løkkeborg studies are irrelevant here because mysticetes do not prey on cod and haddock. They do prey on small, silvery, schooling fish of the type studied by Doksæter et al.] From an exposure standpoint, movements away from the array are beneficial to animals.

The type of operation described in this RA is not likely to cause population level effects. There will be no long-term exposures, and short exposures are insufficient to cause population level effects. Support for this statement comes from recent studies sponsored by the Joint Industry Program (JIP). Four contracts were let to correlate long-term trends in cetacean stocks with industry operations and other factors in the North Sea, Gulf of Mexico, Scotian Shelf, Alaska, Southern California, west coast of Australia and elsewhere. In all areas, multiple surveys using large airgun arrays were shot in grid patterns and repeated over many years. Even under these exposure conditions, many cetacean stocks increased, and those that decreased could not be unequivocally linked to industry operations. Other variables, like commercial whaling and climate change, were always involved. The first of these studies to be published is Thomsen et al. (2011). Three others will follow from LGL, SMRU, and SeaWatch. If long-term effects are not apparent where large arrays are used often, it is not likely that they would develop in the Antarctic under a much reduced exposure regimen. Population-level effects should not be considered a likely risk in this RA.

DCS is an unproven hypothesis about the cause of cetacean strandings. Alternative explanations exist for the emboli that have been found in some stranded animals. Furthermore, blood nitrogen levels in trained dolphin divers were negligibly different from control values. This fails to support the hypothesis that high nitrogen levels will result from surfacing too fast from depth (Houser et al., 2010). At our present state of knowledge, DCS should be considered a very low likelihood risk in this risk assessment.

The suggested threshold for behavioral disruption of blue whales, 160 dB rms, seems unnecessarily low given the results of recent research. Foraging blue whales in California showed "few obvious behavioral responses" when exposed to simulated mid-frequency sonar signals and pseudo-random noise at 160 dB rms (see graph on page 21 of a report found at http://www.sea-inc.net/resources/SOCAL10_final_report.pdf). One whale foraging at depth did respond by surfacing and moving away, but such a response would reduce the exposure and would be considered desirable. Source levels of blue whale calls greatly exceed 160 dB rms, and are probably experienced often, even by calves. These preliminary results suggest that the risk of behavioral effects on foraging blue whales of sound at 160 dB rms is low.

Psychological and physiological (stress) effects are highly unlikely in the exposure regimen proposed by AWI. Such responses might be reasonably expected when exposures are repeated over long periods, as in airport or train noise. But, it is not likely that deleterious psychological or physiological responses will result from a one-time passage of a small airgun array. Antarctic marine mammals experience ephemeral noise events from ice calving, underwater earthquakes, landslides, and volcanic eruptions. All of these are more energetic at the source than a small airgun array.

Finally, regarding the duration of behavioral responses, UBA rejects the use of exposure criteria for mid-frequency cetaceans as criteria for high frequency cetaceans, yet advocates using the behavioral responses of harbor porpoises to represent all other cetaceans. The harbor porpoise, a high frequency cetacean, is clearly an outlier in terms of behavioral response; it does not represent other cetaceans. The best information on behavioral response of multiple cetacean species to airguns is in Stone and Tasker (2006) who conclude that movements away from airgun arrays are "temporary." Using harbor porpoise data (Tougaard, 2009) constitutes excess precaution.

Mitigation

UBA suggests using a mitigation zone so large that a second ship would be needed to monitor responses. Setting a zone of that size seems aimed at preventing even the most benign behavioral reactions. This is unnecessary and biologically irrelevant because of the brief, one-time exposures animals will experience.

From my perspective and background, this RA uses the correct critical radii for injury, that is, PTS. The critical radii for behavioral harassment might be determined empirically. Stone and Tasker (2006) found that the average closest distance of approach of cetaceans during shooting of large arrays was 900 m. Whatever the received level was at nearly a kilometer, the animals seemed tolerant of it. If AWI has measured the closest distance of approach in previous surveys, I suggest using it to set the critical radius for behavioral disturbance. If it has not measured that distance before, I suggest doing it in upcoming operations. The strength of this approach is that it bases the critical radii on what animals actually prefer and tolerate.

Summary; the likelihood of risk

The reviews by UBA and the outside reviewers discuss in exhaustive detail every factor that could possibly affect animals in any way, including DCS, stress, the size of the window for estimating RMS, the possible effects of surface and other ducts, the possible effects of side lobes from the array, putative high frequency effects of airguns, the possible effects of prey movements, and many other speculative topics. These are laudable efforts to be comprehensive, but they miss the larger point. Airguns are not a new noise source that has unprecedented effects. Since their introduction in 1968, airguns have been used in literally thousands of surveys worldwide, increasingly with marine mammal observers on board. In only one survey out of the above thousands (the *Maurice Ewing* in the Gulf of California in 2002) were any animals found injured or dead after a seismic survey (two beaked whales). Even this event may have been a temporal correlation, not cause and effect, because the operation and stranding sites were 100 km apart. Other than that event, no individuals have been reported as dead or injured, or to have behaved erratically after a survey. Good evidence exists that animals do temporarily avoid arrays (Stone and Tasker, 2006). At the population level, Thomsen et al. (2011) and three other as-yet unpublished studies have found no obvious airgun-related declines in cetacean populations in areas where industry has shot seismic for decades. That is, past observations have failed to show the most-feared effects of airguns, but have clearly shown others. These observations constitute information that can inform a risk assessment. Statistical methods (Baysian) exist whereby past events can be used to predict the likelihood of future events. For the two parties in this debate to come to closure about the AWI proposal (and to resolve the debate over all acoustic sources, as the tone of this argument suggests) wouldn't it be reasonable to define the likelihood of risk quantitatively by analyzing the results of the 201 surveys reported by Stone and Tasker (2006)?

As a final observation, the arguments lodged against these fairly benign activities, and against Southall et al. (2007), suggest that the real subject at hand is how *other* projects on noise should be regulated. I can think of no other reason why so many detailed concerns have been raised that do not pertain to this type of array, operating among Antarctic species, in the fashion proposed. If the parties focus on the risks posed by these activities, as described, they should be able to reach accord with more clarity.

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