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Subject: Review of Strategic Assessment of the Risk Posed to Marine Mammals by the Use of Airguns in the Antarctic Treaty Area prepared by the Alfred Wegener Institute (AWI) for Polar and Marine Research.

1. Overview

This review of the AWI risk assessment primarily considers the methodology for estimating and managing risk of conducting scientific seismic operations in Antarctica. Additionally, some of the previous reviews of this document and the underlying methodologies are considered relative to the current best-available science on specific points. Given the fact that much of this risk assessment, and associated controversy, centers on issues related to the physical and/or behavioral impacts of noise on marine mammals and on recently proposed noise exposure criteria (Southall *et al*., 2007) specifically, this review is primarily focused on these issues. In considering both the proposed risk assessment and potential alternative approaches suggested in the reviews, I address the three levels of possible impact (direct injury, indirect injury, and behavioral disturbance) identified in the AWI risk analysis. I consider the underlying methodology, the proposed criteria as they relate to the Southall *et al*. (2007), acknowledged data limitations and extrapolations, as well as safety radii and other mitigation measures.

It is worth noting at the outset that, as the AWI document does, the Southall *et al*. (2007) criteria were derived using a series of extrapolations and assumptions in the (many) areas where data gaps existed. Within the text we very clearly identified these gaps, some of which are quite relevant in the current considerations, how they should be addressed, and stated the need for caution in their direct application in all cases. So too did we note that their application would not be uniform or simple in practice, owing to different jurisdictions, contexts, and the presence of particularly sensitive or endangered species. Finally, we clearly saw that conclusions and criteria would be expected to change based on evolutions in the science. In my view, the AWI risk assessment and management approach in applying the current best available science (including our criteria and subsequent work) generally takes a comprehensive, accurate, and "conservative" approach, although in several instances I suggest additional considerations.

Finally, I conclude with a summary of what I see as the most important conclusions regarding these issues regarding injury and behavioral response. I also raise several issues regarding assessment methodology and overall mitigation of impact that I think should be considered and/or better addressed, some of which have not been specifically considered in detail there or in other reviewer comments.

2. Summary of documents reviewed

Documents provided in preparation of this review were:

- "*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 of the Alfred Wegener Institute (AWI) for Polar and Marine Research. [Note: both the full risk assessment document and the information paper of the same title submitted to the XXXII Antarctic Treaty were reviewed]
- "*Evaluation by the German Federal Environment Agency of the Strategic Assessment of the Risk Posed to Marine Mammals by the Use of Airguns in the Antarctic Treaty Area*"
- Written comments of three reviewers on the first and subsequent versions of the AWI report [note: most of the relevant comments considered were related to the current draft of the AWI report; reviewer 2 comments on the first version were in German and were not reviewed]

3. Detailed review of risk analysis criteria, thresholds, assumptions, and mitigation

Within the AWI risk analysis, three specific types of impact are considered: "Direct, immediate injury," "indirect, immediate damage," and "biologically significant acoustic disturbance." I consider each of these areas in terms of the exposure criteria used, calculation of safety radii, and mitigation approaches, with particular attention to the direct injury and behavioral sections. Key questions were identified within the AWI review, some of them arising from comments given by other reviewers.

(i) "*Direct, immediate injury*"

The AWI approach to estimating the onset of physical injury is based on the use of PTS-onset estimates from the Southall *et al*. (2007) exposure criteria. As noted, these thresholds are calculated based on the onset TTS values measured in a (small) number of odontocete cetaceans, which are then extrapolated to other marine mammals, including large whales for which no direct measurements of hearing or auditory impacts of noise exist. Since Southall *et al*. (2007), several important papers on auditory fatigue have been published, demonstrating TTS in harbor porpoises (Lucke *et al*., 2009), as well as an expanded understanding of TTS in other odontocete cetaceans (*e.g*., Mooney *et al*., 2009a; 2009b; Finneran and Schlundt, 2010; Finneran *et al*., 2010 a; 2010b). Most of these findings were not published when the AWI report was prepared, but have relevance to the methodology, as noted in some cases in the other reviews. Specific questions/issues related to the overall assumptions in the Southall *et al*. (2007) criteria that form the basis of the AWI approach, as well as any necessary modifications in this methodology, are considered here in the order they are posed within the AWI report.

- Does a (single) TTS constitute an injury?

It can, but as for many of the subsequent points regarding potential injury from exposure the answer relates to the magnitude of TTS; this is an issue that seems to be lost in some of the other reviews. As we discuss in detail in the Southall *et al*. (2007) criteria paper, there is not, for humans or any other animals, a clear point at which a TTS becomes PTS for either single or repeated events. Our estimate of 40 dB TTS as an approximation of where PTS onset may occur was based on a relatively conservative interpretation of the human literature indicating at this point and higher TTS values the likelihood of PTS increases. However, in some cases people have been shown to experience greater TTS with no measurable PTS. Higher level TTS events have the potential to result in PTS and this effect is likely increased as a function of repeated exposure (though in a manner not understood in humans or other animals).

The problem here regarding TTS as a proxy for injury is that many people, apparently including the German Federal Environmental agency and at least two of the reviewers of the AWI report, feel that any level of TTS should be considered an injury. Clinicians and researchers testing auditory fatigue in people generally don't deal with TTS values below about 20 dB because they are so relatively common and unreliable in terms of testing issues such as duration or level dependence, or recovery. People that live in cities, much like colonially-breeding marine mammals including some of the southern hemisphere pinnipeds considered in this assessment, probably experience small to moderate TTS' every day as a result of environmental noise including their own vocalizations. In a certain regard, small TTS may be thought of in fact as an adaptive mechanism to cope with a changing noise environment, much like the chemical adaptations in the eye that occur in the process of light adaptation. Obviously I am not suggesting here that TTS as a function of noise exposure from human noise is a good thing, but rather than any suggestion that onset-TTS levels be used as a proxy for estimating injury is completely unsubstantiated in the relatively well-established human literature and the limited but growing marine mammal hearing literature.

Thus, the question, in my mind, becomes at what level of TTS is the potential for PTS sufficient to use a threshold for injury. I have seen nothing in the terrestrial or human literature to suggest that our suggestion to use 40 dB TTS is not a reasonably conservative approximation for the onset of injury. The UBA assessment references Kujawa and Liberman (2009) as evidence for their conclusion that TTS-onset levels should be used as the metric for injury. However, they fail to recognize that the exposures and resulting TTS in this study exceed the values at which by the Southall *et al*. (2007) exposure criteria, by which those (>40 dB) TTS values would be considered injurious. Again, there is a failure to appreciate the relatively common, and likely completely innocuous nature of very low level TTS values, and an unsubstantiated and uninformed desire to consider these values as representing direct and physical injury in spite of the body of scientific evidence against such a conclusion. As discussed below, given the nature of extrapolation from odontocete to mysticetes in terms of TTS onset or taking into account the endangered status of many of the species in question here, there may be a valid case for a somewhat more conservative approximation of the TTS levels used in estimating TTS and PTS in mysticetes. However, to suggest that the onset levels (*e.g*., 6 dB TTS) be used as the criterion is completely unsupportable.

One final point here regarding injury from a single TTS exposure is to agree with the MMC comments regarding potential for reduced ability to evade predators in the context of a (higher level – in my view) TTS. This is an important consideration which has thusfar not been sufficiently considered in a regulatory sense. I also agree with the AWI conclusion of this as relating more so to an “indirect” injury from a single TTS event.

- *Can multiple TTS cause injury?*

This is discussed briefly above and in detail in the Southall *et al*. (2007) criteria publication. The precise mechanisms and (complex) underlying factors are not well-understood even in humans. However, there is a general consensus that multiple repeated exposures may result in a loss of sensitivity that may not occur in a single event; differences in age-related hearing losses in people from heavily industrialized areas versus extremely rural places supports this conclusion.

The question in regard to marine mammals exposed to noise from intermittent seismic surveys is what is the likelihood of an animal receiving several (or probably many) exposures resulting in higher level TTS (~20 dB or higher) that could in aggregate result in a PTS. Given the limited information about some basic behavioral patterns and responses to noise in many of these species, this is a difficult question to answer. While it is possible, it seems unlikely to me that in the context of a survey lasting days to a few weeks that such repeated exposures would occur.

One final note here has to do with the use of the dual criteria metrics (peak pressure and SEL) presented in the Southall *et al*. (2007) criteria. Without the use of an energy metric (SEL), as has been done by NMFS in a regulatory sense using the (completely inappropriate RMS sound pressure) and is suggested be retained by reviewer 1, it is impossible to even consider this question.

- *Can exposures at sub-TTS levels accumulate to TTS?*

The AWI report clearly and accurately deals with this question (p. 174-175) and the issue of effective quiet as it relates to it with input from Dr. Finneran

- *Have the numerical values of TTS and PTS onset levels been estimated scientifically correctly and/or conservatively by Southall et al. (2007)? Is the scientific PTS onset level an appropriate injury threshold under the precautionary principle?*

These are related questions and some of the issues relating to the use of 40 dB TTS as an approximate sign-post above which PTS may be likely are discussed above, along with why I believe that this is a reasonable and conservative approximation for direct physical injury and why the implication of TTS onset as a proxy for injury is unsubstantiated. That said there are several key issues here that are directly relevant to the issue of assessing the impact of high-energy seismic signals on Antarctic marine mammals that are important to consider.

The first of these regards the extrapolation of mid-frequency ceteacean (odontocete) TTS measurements to high-frequency cetacean (odontocete) species. We were aware of some of Lucke's harbor porpoise results when the criteria were being published, but they were coming out just as we were finishing our work. We noted that this work was underway in the criteria and that the extrapolation to high frequency species was likely among the more tenuous , based on the expected greater sensitivity of high frequency species given their lower overall hearing thresholds than other marine mammals, among other reasons. The Lucke *et al*. (2009) data indicated a lower TTS onset value both in terms of SEL and peak pressure. Clearly, these data should form the basis for estimating TTS onset and potential for injury for harbor porpoise and other species sharing similar functional hearing capabilities, such as the spectacled porpoises if they were to be considered here (they were excluded based on being only an infrequent inhabitant of the Southern Ocean). But, as Lucke *et al*. (2009) acknowledge, these (important) data are less relevant in considering noise impacts on hearing in other marine mammal functional hearing groups. In my view there is no scientific basis for using these limited results for an individual of a species that is known to be particularly sensitive to noise exposure in place of the (larger) body of data on noise impacts on hearing in mid-frequency cetaceans.

A more difficult question, for AWI as it was for us in the criteria paper, is how to establish the correct exposure criteria for the low-frequency cetaceans (mysticetes) where we lack any direct measurements of hearing or noise impacts. A key consideration here is the frequency bands in which that these animals operate and the typical background noise levels that occur in those bands on the evolutionary time horizon (before industrial noise). The high frequency animals operate in a frequency regime (many tens to ~100 kHz) where there is very little background noise. The mid-frequency species function in a zone (~tens of kHz) where ambient noise is somewhat higher. The mysticetes are, by all reasonable accounts given their sounds, size, and auditory anatomy, low frequency specialists and function in a zone (tens of Hz to a few kHz) where there is commonly a lot of ambient noise. This is important in the context of hearing and potential hearing damage because the relative sensitivity of hearing across many taxa is strongly driven by nominal background noise levels and the susceptibility to noise-related hearing loss is related to the relative level of noise exposure (sensation level is the level of noise above the hearing threshold). Clark and Ellison (2004) discuss this issue in detail in a seminal paper considering the likely hearing capabilities of mysticetes. Based on this assessment, absolute hearing capabilities in regions of best hearing (low frequencies) for whales is very likely not as good as in other cetaceans, and thus susceptibility to noise exposure would consequently be expected to be relatively lower for the same absolute levels of noise exposure. This is the case Southall *et al*. (2007) made in justifying the extrapolation of mid-frequency cetacean results to low-frequency species. While this remains a compelling point, I remain aware that we lack direct information and that these species are likely most sensitive in the low frequency band where the primary energy of airguns occurs. I am also aware of the recent results by Finneran and Schlundt (2010) demonstrating a greater sensitivity to noise exposure for mid-frequency cetaceans at higher frequencies (within their region of best sensitivity) than had been tested when the Southall *et al*. (2007) criteria were published. While I don't think these data affect the conclusions regarding noise impacts for seismic sounds and mid-frequency species, they do have some bearing, in my mind, on the extrapolation of mid-frequency cetacean data to low-frequency specialists exposed to low-frequency sound. I don't see a clear scientific way to resolve whether the likely lower susceptibility to noise exposure level as a function of higher absolute thresholds would offset the potentially greater sensitivity to sounds in their region of best hearing sensitivity. Nor can I suggest a clear and defensible mathematical way to take a more conservative approach, other than to reiterate that I find the UBA suggestion of using the Lucke *et al*. (2009) harbor porpoise data for all species in this assessment to be untenable. I do feel, however, that given the level of uncertainty, amplified by the Finneran and Schlundt (2010) findings, and the endangered status of most of the mysticetes in question, that a somewhat more conservative interpretation of the Southall *et al*. (2007) injury criteria for mysticetes would be warranted.

A few other methodological issues were raised in the reviews regarding TTS onset and calculation. One regards the use of masked hearing thresholds in some of the studies underlying the Southall *et al*. (2007) criteria. While this is the case for some of the earlier studies conducted in shallow bays, many of the more recent papers have measured TTS in relatively quiet test enclosures with generally comparable results in terms of absolute exposure levels consistent with TTS onset. Another is the validity of the equal energy hypothesis in assessing sounds of variable duration and level. As discussed in the Southall *et al*. (2007) paper in detail, and more recently demonstrated by Mooney *et al* (2009b), there are conditions where the equal energy assumption may not be a perfect (or even very good) way of predicting exposure effects. However, the use of SEL, along with a dual criterion that accounts for high-energy transients, remains a vastly better approach to accounting for noise impacts than previous methods using RMS sound pressure and is probably a reasonable first-order predictor in most cases.

- *Estimating critical radii:* I have several brief points regarding the estimation of effects radii:

  - It is worth noting that the AWI approach adds a degree of caution here by not using M-weighted levels, by assuming the loudest level across depth to calculate exposure radii, and by assuming that animals remain within this layer during exposure.

  - A point raised in the UBA analysis I do agree with is concern with limiting the modeling to low frequencies (256 Hz) and what this may mean for species that are more sensitive at higher frequencies (like mid-frequency cetaceans). While it is clear that the energy at higher frequencies is greatly reduced, it remains significant and for some nearby species may in fact be the salient aspect of exposure, probably more so for behavioral effects. I would like to see the modeling take into account higher frequencies, though I realize this may best be represented in a band-limited manner (1/3rd-octaves would make sense) rather than in terms of spectrum levels.

- Mitigation zones for injury:

In general, I agree with the approaches proposed for mitigation zones around operations. Also, I note the importance of not creating such strict zones that are either unobservable, and thus ineffective or detracting from more meaningful monitoring, or so restrictive that they result in very frequent shut-down of operations, thereby increasing the duration of the overall activity. There is a risk of actually increasing the overall impact by developing supposedly more protective measures. One comment relates to the suggestion that there be a somewhat more conservative approach to the estimation of injury for mysticetes in this particular application. That would logically relate to a somewhat greater safety zone for these animals as well. Reviewer 1 notes this point as well and suggests a larger zone. Given the uncertainty with mysticetes and the modeled levels for pinnipeds, in terms of operational consistency, a shut-down zone of 1000m for all marine mammals might reasonably take each of these issues into account.

(ii) "Indirect, immediate damage"

I am not the most qualified person to comment on some of the issues considered here in terms of DCS or thermal stress. In terms of indirect behavioral responses leading to potential harm, there appears to be an emerging picture from some of the recent work on beaked whales that this is a plausible explanation for some of the previous marine mammal stranding events (Moretti *et al*., 2011; Tyack *et al.*, 2011). These kinds of responses appear to be more likely in beaked whales than other marine mammals. While the previous stranding events that are relatively well known involve military mid-frequency tactical sonar rather than seismic airguns, Tyack *et al*. (2011) noted a lack of dependence on strong avoidance behavior in tagged beaked whales to the type of sound. Thus, as is recognized in the AWI assessment, there is a greater potential for this kind of potential "damage" in these particular species relative to other marine mammals. Fortunately, as noted, most of the surveys tend to occur in depth regimes that are either shallower or deeper than is typically inhabited by beaked whales. Nevertheless, potential behavioral responses in these species may occur over relatively great ranges (beyond observation capabilities) and this remains an important behavioral consideration for seismic activities in Antarctic waters. Specific attention should be made to continue to avoid high density beaked whale habitat, particularly in areas with canyon-like bathymetry that may complicate the sound propagation environment.

(iii) "Biologically significant acoustic disturbance".

Obviously one of the major complications with Southall *et al*. (2007) was our lack of presenting explicit sound exposure levels for behavioral response. This was not for a lack of effort, but rather for a lack of convergence in the available literature on broadly-applicable sound exposure values that result in significant behavioral responses across the functional hearing categories we established. Contextual factors relating to different sound types, different exposure conditions, and differing activity states complicate efforts to derive a simple level-only approach. What we did conclude was that the likely best approach was to consider the exposure conditions of studies that are the most similar to those in the situation in question in assessing the potential for disturbance.

One of the areas where there is a relatively large literature is behavioral responses of marine mammals, particularly large whales, to seismic airguns. From that literature, as reviewed in Southall *et al*. (2007) a relatively clear picture emerges in which migrating bowhead and grey whales show fairly strong avoidance response at received levels around 120-140 dB (re: 1µPa (rms – which is more valid in this case)), whereas the same kinds of behaviors are not seen in other species (including feeding bowheads) until received levels are around 160 dB. The AWI approach to assessing significant behavioral response uses the 160 dB rms criterion, based on the conclusion that for the species present in the areas around seismic operations in Antarctica, the later are the most relevant data for consideration with which I would agree. Just because marine mammals have been observed in some cases (*e.g.*, predator avoidance responses or particularly sensitive species) to respond to sounds just above background noise does not necessarily mean that these should be the exposure criteria for behavior for all situations. In this case, I favor the AWI approach rather than something that is arbitrarily conservative but likely not applicable to the conditions as we understand them.

The one caveat I would give here relates to migrating species. We lack sufficient information on some of the other mysticete species, like blue whales, in this behavioral state but recent information from behavioral response studies with controlled sound playbacks to sonar-like sounds and bands of noise suggest both that they do respond to human noise in certain conditions and that behavioral context is a relevant mediating factor (Southall *et al*., 2011). Whether migrating blue, sei, humback, or other whales are more sensitive to sound exposure while migrating as opposed to other conditions is not known, although the work of McCauley *et al*. (2000) would argue against it for humpbacks. But it should be recognized that in some mysticetes, migration appears to be a behavioral state where responsiveness is heightened. As mentioned in the conclusions section below, one strategy to reduce potential impacts in this regard might be to try and avoid seasons and locations where interactions with migrating whales would be more likely.

4. Overall conclusions and recommendations for consideration

In addition to dealing with the above detailed areas of the risk assessment methodology regarding (particularly) injury and behavioral criteria, which I found to be the primary areas on which to focus my review given my role in the noise exposure criteria paper, I have a number of related/overall thoughts, as well as some final recommendations for consideration.

General conclusions

The sound sources in question produce intense, low-frequency sounds that propagate long distances and do have the potential to affect large numbers of marine animals. The geophysical research described here is occurring in biologically-important areas inhabited by many species, some endangered and still recovering from depletion due to human harvesting. As emphasized in the Southall *et al*. (2007) criteria paper there are significant data limitations in basically all relevant areas and caution is required in extrapolation to other species. There is also an understandable need to consider more protective assessment/regulatory approaches in the case of particularly sensitive or endangered species. However, assessments of potential impact, mitigation measures, and regulatory requirements must take into account the best available science (with all appropriate considerations of its limitation) and not necessarily just default to the most protective criterion possible in all cases. Where the science suggests a more protective approach is needed (*e.g.*, particularly sensitive species, endangered species where little information is available), stricter criteria are valid. But where data are available that indicate less restrictive methods of assessing or reducing impact is required, these data should be used.

In general, I find the AWI risk assessment to be a thorough and scientifically-valid application of the best available science in these regards. I think they do a very good job – one of the best I have seen in this kind of assessment regarding seismic surveys – in comprehensively considering the underlying biological and acoustic issues and in segregating the kinds of potential effects and dealing with each using different threshold and mitigation approaches. Alternatively, the review of the German Federal Environmental Agency of the risk assessment too often defaults to the most possible protective level, even in the face of strong evidence to the contrary; their conclusion that TTS-onset levels from one individual of the most sensitive species measured be used as a proxy for injury for every species is not scientifically supportable. However, I do tend to agree with the essence of some of the comments of the other reviewers that a somewhat more conservative approach is appropriate regarding the estimation of injury for mysticetes and/or the use of slightly larger safety radii for these species in the context of seismic surveys in Antarctica. As a final general comment, I would agree with the AWI conclusion that, based on the very limited amount of time of seismic operations annually and the limited potential for direct injury from operations, there is likely a very low risk of potential population-level effects from these surveys. The likely effects of these operations are most likely behavioral in nature, and among these temporary avoidance behavior will probably be the most common.

Specific, related conclusions

Below are several somewhat related conclusions that don't fit well into the above structure, but that I wanted to include in this review on several points:

- Reviewer 1 comments that the US National Marine Fishery Service (NMFS) has not yet accepted the Southall *et al*. (2007) exposure criteria and uses this as a justification for retaining the outdated RMS threshold approach for assessing the potential for injury. This is no longer the case. NMFS has applied exposure criteria that based in large part on the Southall *et al*. (2007) thresholds for injury from sound exposure for assessing potential impacts of Navy active sonar operations (NOAA 2009a; b) for a host of species, including large whales and pinnipeds. In fact, these NOAA regulations actually include higher exposure values for certain species for which higher TTS onset values were directly measured than the more conservative values used in Southall *et al*. (2007). NMFS, and other regulatory agencies have been relatively slow in making the large and fundamental changes away from the simpler and established, yet no longer supportable, threshold criteria. However, this is increasingly occurring incrementally as relatively larger programmatic assessments, such as the Navy EIS' or the in-progress EIS for the U.S. National Science Foundation seismic research which also uses the Southall *et al*. (2007) threshold criteria, as I understand it.

- Several of the reviewers and the UBA comments center on the averaging time window for consideration of seismic pulses. I tend to see both sides of the argument here in that by using a longer averaging window there is some inconsistency with the previous research results which have been calculated using a 40 ms window, but doing so negates the characterization of reverberation that may have significant energy. I would tend to think that for the purposes of behavioral criteria estimation, the 40 ms window should be used, but that a longer integration time should be used for characterization of pulses for assessment of potential hearing effects where the integrated energy is the important calculation. I am not sure how practical this is in reality, but I see reasons for handling the integration time differently depending on the question being asked.

Recommendations for consideration

1) Much of the focus in the AWI risk assessment, and the controversy in comments, centers on the acute and high-level effects of airguns on marine mammals. While there, of course, must be consideration of the potential for direct harm and there is certain to be disagreement given the level of scientific uncertainty, in my view there remains a disproportionate attention to the relatively small footprints over which high-level effects may occur while some of the larger impact zones are ignored. Behavioral responses, most

likely temporary avoidance behavior, are likely to occur over fairly large areas; these are considered specifically and discussed above. Additionally, a striking omission from the AWI risk assessment is any explicit consideration of the potential masking impacts arising from these operations, or other impacts on communication behavior. These low-frequency sounds may affect the low-frequency ambient noise over many hundreds of miles, in frequency bands where (particularly) whales are functionally operating to communicate in the context of mating and other life-critical functions. We can calculate the zones over which these kinds of masking effects can occur (see Clark *et al*., 2009). While such assessments are typically applied to more continuous noise sources, the argument that seismic noise is intermittent and thus has no masking impact because animals can listen between the pulses is a weak one, particularly given that the pulses become increasingly more continuous with propagation from sound sources. While I clearly recognize the difficulties in assessing and the even-greater obstacles to trying to mitigate these effects, I believe it is fair to say that many more animals may be affected by the presence of the background noise generated by these transmissions than will be potentially injured or acutely disturbed. This effect should be accounted for or at least explicitly considered in the risk assessment in some way.

2) On a related note not necessarily relevant to the AWI risk assessment but more of a general comment regarding the use of loud and potentially invasive remote sensing technologies in biologically-important areas with endangered species, there should be a sustained effort to develop quieter methods to obtain the same kinds of information. The use of advanced technologies to reduce horizontal propagation of sound energy and alternative technologies should be explored, particularly for use in biologically-important areas such as around Antarctica.

3) While establishment of safety zones and other mitigation measures are necessary and will remain a focus area, perhaps the most meaningful measures may be those that seek to reduce the overlap between potentially disruptive activities and potentially more sensitive species, sex/age classes (such as mother-calf pair), or behavioral states such as migration. In some cases in this regard, this may argue for pushing seismic operations later into the austral summer. Additionally, surveys should be planned and conducted, as much as

possible, to generally avoid large concentrations of marine mammals, such as those recently described by Nowacek *et al*. (2011) in Antarctic waters. I realize the complexity and uncertainty in some of these conditions, and the difficulty in practically accounting for this while planning a complex field operation, but simply conclude here with the comment that time-area avoidance of areas where more sensitive species or behavioral states/conditions occur may be a more meaningful way of approaching mitigation in a broad sense than ever-more refined means of calculating impact or establishing “safety” radii.

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