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8 May 2001

Ms. Donna Wieting
Chief, Marine Mammal Conservation Division
Office of Protected Resources
National Marine Fisheries Service
Silver Spring, MD 20910-3226 USA

Dear Ms. Wieting:

Thank you for the opportunity to comment on the FEIS for the proposed US Navy SURTASS LFA sonar system. I strongly oppose granting permission to this system on the grounds that it poses too great a risk over too large an area, to already endangered marine mammals.

I am a bioacoustician, having worked on whale acoustical communication since 1982 (my M.Sc., Ph.D., and post-doctoral work were all in this area). I consider the "No Action Alternative" the only scientifically valid, responsible, prudent, and ethical choice. The Navy has clearly expended considerable effort and expense in the SRP and the preparation of the FEIS, but sadly, to little use, as the large, essential questions remain completely unanswered. The Navy's claims do not bear up under the slightest bit of scientific scrutiny. It is regrettable that the Navy chose to largely dismiss the many thoughtful questions and comments on the DEIS provided by scientists and other citizens.

The answers to comments are often glib and perfunctory. An example is Comment 4-4.13: "What are the effects on a pregnant marine mammal?" Response: "...Because a marine mammal's fetus is composed of the same tissue type as its mother, it is not considered to be at any greater risk than the mother." Yes, but the question pertained to the mother in the first place. Is she at greater risk? Is there any understanding of a pregnant whale's need for better nutrition, her likely different behavior and different sensitivities? The only honest answer would have been "We don't have a clue." Nor is a fetus identical to the mother simply because they have roughly the same "tissue type". This is a throw-away answer with no citation of related research in humans or other species, for instance. Similarly for Comment 4-4.14: "Are effects from SURTASS LFA sonar more serious for the young of a species?" The response side-steps this by saying "[t]he primary factors increasing risk to a marine species would be a more pelagic and deeper distribution of animals in the water column." Then why didn't the Navy wait until more pelagic, deep divers could be studied like beaked and sperm whales? And why is there no

reference to Jones et al. (1994), where gray whale cow-calf pairs showed the greatest reaction (84% reduction in numbers) to noise playbacks in a calving and breeding lagoon?

The FEIS can, therefore, hardly be called an improvement on the DEIS. The same comments (15 pages) I made on the DEIS (p. E-246 to E-253) can as such be re-applied to the FEIS. None of my questions were answered to any degree of satisfaction. The only changes I see in the FEIS are mainly window-dressing (thankfully, the Navy now no longer targets all "Third World" countries as submarine threats but restricts itself to merely China, Iran, and North Korea, etc.). True, the Navy finally addressed the Greek and Bahamanian whale strandings, but in such a dismissive manner that little was achieved by this token treatment of a fatally serious problem.

Mass strandings

A top priority for the Navy must be to seriously investigate the world-wide stranding data from 1838-1999, reported in the International Whaling Commission's Standing Working Group on Environmental Concerns (Anon 2000) where 6/6 multiple species strandings (of which at least one was a beaked whale species) were associated with military activities (the Bahamanian stranding brings the total to 7/7).

It is utterly invalid for the Navy to claim that the Greek and Bahamian strandings have nothing to do with LFA sonar:

1) There is no way of knowing which characteristics of the naval sonars caused these whales to strand. The harmful characteristics may be related to the higher frequencies used by the NATO LFAS or standard mid-frequency sonars or they may not be.

2) The NATO LFAS is broadband, with waveforms centered at both 600 Hz and 3 kHz, and thus, lower frequency energy is also clearly prominent.

3) Even if the higher frequencies are the fatal component, these strandings are still crucially relevant. Buried under comment 4-6.42, we discover that the LFA sonar signal has a second harmonic that is about 150 dB at 1 km at frequencies of up to 1 kHz. Bear in mind that received levels of around 150 dB or less may have caused either stranding (see comments by K.C. Balcomb on the proposed rulemaking).

4) A whale's susceptibility to non-auditory effects is unrelated to its frequency-sensitive hearing. Vestibular effects or resonance effects can be damaging even at frequencies to which the whale is not acoustically sensitive. It is astonishing that the FEIS does not even have a *section* on "non-auditory injury" for marine mammals, as it does for fish and sharks, and sea turtles, for instance. This sort of denial is very telling indeed.

5) It is not enough to blithely state that "analyses of potential correlations between known marine mammal stranding events and SURTASS LFA sonar operations by Dr. Peter Tyack have revealed no evidence of any relationship between the two" (Comment 4-4.21). Strandings are rare, not necessarily because they don't often happen, but because we don't often discover them. Moreover, whales may die at sea if there is no beach nearby for them to strand on, and deaths at sea are almost impossible to detect. In other words, that the sonar used in the Bahamas stranding was "..of the type commonly found...in most of the world's navies" (p. 3.2-47) is hollow "reassurance" indeed. We could have easily decimated populations of beaked whales the world

over and never known about it.

6) Finally, the most important point the Navy is missing is that **both of these strandings caught everyone, even the experts, by surprise**. Most of us didn't dare think that immediate death could be a plausible risk of these sonars. Does this lead the Navy to any sense of humility? On the contrary, the FEIS states (Comment 4-10.15): "Because there is no scientific or engineering basis for immediate mortality upon exposure to SURTASS LFA...and all prior research results support this conclusion, the discussion of potential biological removal (PBR) is not in the FOEIS/EIS." This is like a cigarette company arguing that its particular mixture of tobacco is not deadly, though another brand might be. How many more times can we afford to be caught off-guard? Who would have predicted, years ago, that car exhaust would cause the climate to change? That a pesticide would cause fragile egg shells? Will we ever learn this most vital lesson? And will we learn it in time?

Thus, to claim, as the FEIS does (p. 3.2-47), that "...even if the investigation ultimately concludes that the mid-frequency sonars in use during the [Bahamas] transit caused or contributed to the strandings, such a conclusion would not appear to present any significant new information relevant to the proposed deployment of SURTASS LFA sonar" is nothing short of outrageous. The Navy needs to wait for the final NMFS report on the necropsies of the beaked whale heads, investigate any and all possible non-auditory effects thoroughly, and present the detailed results openly and honestly. Preliminary results should have certainly been included in this FEIS. To say (response to Comment 4-4.15) that because the LFAS signal only remains at one frequency for 10 s and that therefore the potential resonance effect would not occur, is, as is so often the case in this document, an unproven assumption. Ten seconds could be enough to induce resonance, for all we know.

180 dB criterion

The Navy is selective in the research it chooses to cite and how to interpret that research. This is very poor science. There is no convincing scientific evidence to support this criterion as the level at which physical harm begins to occur. There is at least as much evidence to support lower, more conservative criteria (Richardson et al. 1995, Myrberg 1990). It is alarming that the Navy felt confident enough to use what amount to little more than wild guesses (pieced together from selected research) upon which to place the safety of our marine life. The criterion of safe exposure level is absolutely critical to determining the scale of impact. If the Navy happens, by chance, to be correct in that its transmissions are harmless under received levels of 180 dB (for all individuals, species, habitats, behaviors, conditions, etc.), it only needs to be concerned about mitigating the 1 km radius of impact. If, however, the Navy is just 10-20 decibels off (i.e. if a 160-170 dB criterion is more appropriate) in selecting the "safe" level, the house of cards so carefully constructed around the 180 dB criterion collapses entirely. If 120 dB is used as the safety level (which is borne out by at least as much research as is the 180 level, especially when behavioral effects are given due consideration--Richardson et al. 1995), then suddenly the radius of impact is not 1 km, but 500 km (J. Potter, BIOACOUSTICS-L Internet list, 26 Sept. 1996), and the area of impact increases radically from 3 sq. km to 800,000 sq. km, **an over 250,000 fold increase in area impacted**. It is interesting why the FEIS never offers us these figures (the distance of the 120 dB contour)--even ATOC's EIS did this much. It should be apparent why it

is so critical to base the safety criterion on more than "assumptions upon assumptions". Naturally, even those that believe the Navy is capable of "leaping tall buildings", would question whether they could effectively mitigate over 800,000 sq. km of ocean (which is larger than Texas).

Scientific Research Program

Unless one appreciates how little one can actually see and study of whales at sea, one cannot truly recognize the utter folly of attempting to use three short-term field studies to extrapolate to the long-term health of whale populations worldwide.

Let's look at the possible effects (those that we can imagine) which are detectable for cetaceans at sea and which are not. I am not implying that these effects will happen--only that should they be happening, we would never know. This list is clearly incomplete.

Detectable Effects (All these are only over the short-term, usually minutes to hours, and only on a few individuals usually)	Non-Detectable Effects--unless catastrophically dramatic
Respiration rate Swim speed Vocalizations Dive times Dive depth Residence time Distribution Movement relative to sound source	Birth rate Miscarriage rate Pregnancy rate Birth defects Mating rate Rate of finding mates Lactation rate Changes in mating dynamics Death rate Injury, disease, morbidity Vulnerability to hazards (shipping, fishing nets) Vulnerability to predation Growth rate Feeding rate Change in echolocation ability (ability to process echoes) Change in group bonds and coordination (both within and between groups) Change in mother-calf bonds Change in navigational ability Annoyance, pain, panic, confusion, anxiety, etc. Change in memory, learning ability, cognitive functions, etc. Change in aggressiveness Change in maternal behavior Stress Resistance to immunity Change in appetite Changes in metabolic rate Non-auditory resonance effects Effects on vibro-tactile system Effects on contractile forces of muscles Irregular heartbeat Lung-gas interface Rectified diffusion Vestibular/CNS effects Cavitation Hyperthermia Tissue shearing due to radiation pressure Deafness and hearing impairment (TTS, PTS) Change in susceptibility to the "bends" Change in stranding rates Change in population (long-term) **Any long-term effects whatsoever**

Above, I have used the "Possible Effects of Exposure to Low Frequency Acoustics" in "Exposure Guidelines for Navy Divers" (Comment 4-9.15) under "Non-Detectable Effects". As humans are much more easily studied, in terms of reaction to noise, than whales, it would make some sense to base exposure criteria on what humans underwater have actually been exposed to and found annoying or worse (145 dB). Humans are afforded a margin of safety of < 2% (p. 4.3-5) chance of injury, yet, as L. Rendell's comments on the DEIS note (p. E-126), the Navy has to be 95% sure it will cause harm to marine mammals before shutting the system down. "A 95% probability of causing harm," Rendell writes, "would be a good statistic for a weapon *designed* to cause injury" to marine mammals.

The FEIS (p. 4.2-29) notes that "short-term behavioral responses do not necessarily constitute significant changes in biologically important behaviors." They ignore the converse: that significant changes in biologically important behaviors do not necessarily manifest themselves in short-term, visible behavioral responses, i.e. these significant changes can go undetected.

The FEIS argues (p. 4.2-26, 4.2-28) that the dramatic avoidance responses of inshore migrating gray whales cannot be applied to sources that are offshore, since migrating whales did not avoid these. This conclusion is not scientifically valid unless one can prove that the inshore and offshore populations of migrating gray whales are not significantly different in terms of sensitivity to noise, composition (there are indications that mothers and calves migrate inshore), etc. In other words, it remains to be proven whether it is something about *the inshore environment* that causes whales to show a greater reaction to noise, or something about *the composition of whales that migrate inshore*.

J. Calambokidis, himself involved in the SRP, cautions that "[s]ome of the research projects conducted, especially the phase on blue and fin whales that I was involved with, were not able to fully achieve their objectives...It is important that the limited sample size from this experiment not be construed as indicating a lack of impact." (p. E-348).

Models

Once again, I must emphasize that the accuracy and reliability of the input data are missing from these sophisticated models, rendering them all but meaningless: the "emperor has no clothes". On p. 4.2-58, the Navy attempts to use the AIM estimations to help quantify how the risk of exposure would affect an animal's life history. It claims that it is "using a very conservative assumption" when it supposes "that half of the animals [lose] one quarter of their breeding season". What the Navy fails to recognize is that the effects of sound on the reproductive success of an animal can potentially last well beyond the duration of the 20 days of transmissions. As Kastak et al. (1999) noted, their pinniped subjects avoided locations they associated with noise experiments even though these were areas where they also received food. They postulate that marine mammals could avoid critical breeding or feeding grounds if they associate these with exposure to loud sounds, resulting in a dramatically decreased reproductive output.

So, no, saying half the animals could lose one quarter of their breeding season is not conservative. If an animal stays away from critical areas it associates with loud noise, it has lost ALL its breeding season for that year. Moreover, it may never return in subsequent years. That

the effects of noise can extend well beyond the actual period of transmission has been suggested for gray whales on the breeding and calving grounds (Jones et al. 1994). The authors observed dramatically fewer cow-calf pairs and their premature leaving of the breeding/calving lagoon after one month of noise broadcasts. *Even the following year*, cow-calf counts remained low, possibly, the authors postulate, as a result of the previous year's decreased breeding success. The Navy's calculation therefore strongly underestimates the potential impacts of its noise on an animal's lifetime reproductive potential.

Bottlenose whales

Very disappointing is the Navy's unwillingness to protect one of the few populations of beaked whales worldwide which are well-studied (off Nova Scotia). Given that the Greek and Bahamas strandings point to possibly greater sensitivities to noise in the beaked whales, given that these whales can be very elusive and difficult to detect acoustically, given that they are especially vulnerable because they are curious and approach ships, and given that theirs is a year-round resident population concentrated over a very small area (Whitehead et al. 1997), it seems incomprehensible that their unique habitat not be considered an exclusion zone for LFA sonar. Particularly galling is the claim that they are being protected by listing them under Area Number 1 of OBIAs (p. 2-12), when in fact, protection only extends to the 200 m isobath, where they almost never occur. This is like saying that the Navy is committed to protecting all marine mammals that inhabit the skies!

It is also astounding that the Navy does not feel the need to keep the 180-dB SPL out of all National Marine Sanctuaries (Comment 5-1.7). It is doubtful, and certainly unproven, that operation of LFA sonar will "...not destroy, cause the los[e] [*sic*] of, or injury [*sic*] any sanctuary resources...". I could not find the letter mentioned in Appendix A.

"Independent" scientists

I note that none of the "independent" scientists who were principal investigators for the SRP chose to submit comments on the DEIS. Why?

Mitigation

Mitigation is grossly inadequate, partially because the area over which potential serious effects could occur is likely greater than a radius of only 1 km (see above under "180 criterion"). Furthermore, the HF/M3 sonar could use frequencies above 200 kHz to impact odontocetes less. It makes little sense to have the mitigation be a potential threat as well.

Selected specific comments

p. 4.2-59: If we assume that there is no noise other than LFA sonar, it still would not be adequate for a whale to experience no masking 80% of the time, if during the other 20% of the time a predator is masked, resulting in the whale's death.

p. 4.4-4: "...four SURTASS LFA sonar systems...would introduce far fewer signals and far less total energy into the ocean than seismic survey airguns in the Gulf of Mexico alone."

Clearly, some sort of consistency in regulations is desirable, but is the Navy proposing that LFA sonar would be a substitute for the airgun noise? If not, then wouldn't the noise be cumulative? I'm sure that every single point source of air or water pollution can make the same argument--that it is all a "drop in the bucket" compared to the rest, but we couldn't possibly protect our environment if this sort of logic prevailed. It is simply not good enough and juvenile besides (my kids try this argument on me all the time!) to point the finger at worse offenders in an attempt to "get off the hook".

Comment 2-2.1: "How effective is SURTASS LFA sonar going to be because of restricted areas?" The FEIS states that "[t]he restricted areas will not affect SURTASS LFA sonar routine training and testing, as well as the use of the system during military operations." However, p. 2-23, contradicts this by noting that "...Alternative 2 [unrestricted operation] would provide Fleet operators with ...maximum submarine detection capability..."

Comment 2-4.7: I am glad the Navy admits that "...the conservative assumptions about the risk continuum cannot be verified by the Long Term Monitoring Program."

Comment 2-3.5: I was amused to read that one reason why LFA deployment cannot be deferred until long-term effects have been determined is that the Long Term Monitoring Program could then not be implemented! Better to study the whales than actually protect them from threat, I guess.

Comment 4-4.10: The FEIS writes: "[h]earing impacts are...analyzed at greater length because they are believed to occur at lower sound levels, and shorter durations, than non-hearing impacts." The best available data from the Greek and Bahamas strandings so far appear to contradict this "belief".

Comment 4-5.14: The Navy writes: "[m]ethods to investigate physiological reactions (e.g. TTS, PTS, stress) to underwater LF sound are not *yet* [my emphasis] available for free-ranging large whales." "Yet" implies these methods are just around the corner, when in fact, most physiological reactions of large whales at sea will remain unknowable, for all intents and purposes.

Comment 4-5.16: The FEIS states: "...the SRP selected the most plausible and likely impacts to address, in particular, significant change in a biologically important behavior. They observed none...Other less plausible and unlikely effects were not addressed." Is migration a "biologically important behavior? Are mating calls? Both of these DID show change, according to the SRP. It is very open to interpretation whether these changes were "significant" or not. As far as "plausible and unlikely effects" are concerned, was it plausible and likely that beaked whales over distances of several tens of kilometers die as a result of NATO LFAS or the Navy's standard operating sonar? If it was considered so plausible and likely, why didn't the Navy/NATO take steps to prevent it or at least study the phenomenon ahead of time?

Comment 4-5.21: I am glad that the Navy admits that "...injury cannot be studied in the wild."

Comment 4-5.38: As I understand the Hastings et al. (1996) study from conversations with Dr. A. Popper, a co-author of the paper, there was indeed delayed sensory damage that was not an artifact of the sacrificing schedule.

Comment 4-9.18: What about surface ducts?

Comment 4-10.2: "It is assumed that marine animals have evolved to adapt to current oceanic ambient noise levels." Evolution in whales takes a long time--this assumption is, in all probability, false.

Comment 8-1.3: "The page IX comment concerns the possible effects to cetaceans of increased anthropogenic noise in the oceans as compared between pre-shipping conditions and present shipping conditions...". No, as I read it, the page IX comment specifically refers to LFA: "...the most serious potential impacts of *LFA* [my emphasis] are likely its potential contribution to a long-term decrease in foraging efficiency or communication of marine animals..."

In conclusion, I ask NMFS to act responsibly to safeguard our (meaning: the world's) marine natural heritage.

Sincerely,

Linda Weilgart, Ph.D., Research Associate

References

Anon (2000) Report of the Standing Committee on Environmental Concerns. Report of the Scientific Committee of the International Whaling Commission. SC52.

Hastings M C, Popper A N, Finneran J J, Lanford P J (1996) Effects of low-frequency underwater sound on hair cells of the inner ear and lateral line of the teleost fish *Astronotus ocellatus*. J Acoust Soc Am 99 (3): 1759-1766.

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Whitehead H, Faucher A, Gowans S, McCarrey S (1997) Status of the northern bottlenose whale, *Hyperoodon ampullatus*, in the Gully, Nova Scotia. Can Field Nat 111:287-292

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An example is Comment 4- 4.13: "What are the effects on a pregnant marine mammal?" Response: "...Because a marine mammal's fetus is composed of the same tissue type as its mother, it is not considered to be at any greater risk than the mother." Yes, but the question pertained to the mother in the first place. Is she at greater risk? Is there any understanding of a pregnant whale's need for better nutrition, her likely different behavior and different sensitivities? The only honest answer would have been "We don't have a clue." Nor is a fetus identical to the mother simply because it has roughly the same "tissue type". This is a throw-away answer with no citation of related research in humans or other species, for instance.

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Dive times
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Distribution
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Effects:--unless catastrophically dramatic

Birth rate
Miscarriage rate
Pregnancy rate
Birth defects
Mating rate
Rate of finding mates
Lactation rate
Changes in mating dynamics
Death rate
Injury, disease, morbidity
Vulnerability to hazards (shipping, fishing nets)
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Rectified diffusion
Vestibular/CNS effects
Cavitation
Hyperthermia
Tissue shearing due to radiation pressure
Deafness and hearing impairment (TTS, PTS)
Change in susceptibility to the "bends"
Change in stranding rates
Change in population (long-term)
Any long-term effects whatsoever

Above, I have used the "Possible Effects of Exposure to Low Frequency Acoustics" in "Exposure Guidelines for Navy Divers" (Comment 4-9.15) under "Non-Detectable Effects". As humans are much more easily studied, in terms of reaction to noise, than whales, it would make some sense to base exposure criteria on what humans underwater have actually been exposed to and found annoying or worse (145 dB). Humans are afforded a margin of safety of < 2% (p. 4.3- 5) chance of injury, yet, as L. Rendell's comments on the DEIS note (p. E-126), the Navy has to be 95% sure it will cause harm to marine mammals before shutting the system down. "A 95% probability of causing harm," Rendell writes, "would be a good statistic for a weapon *designed* to cause injury" to marine mammals.

The FEIS (p. 4.2-29) notes that "short-term behavioral responses do not necessarily constitute significant changes in biologically important behaviors." They ignore the converse: that significant changes in biologically important behaviors do not necessarily manifest themselves in short-term, visible behavioral responses, i.e. these significant changes can go undetected. The FEIS argues (p. 4.2-26, 4.2-28) that the dramatic avoidance responses of inshore migrating gray whales cannot be applied to sources that are offshore, since migrating whales did not avoid these.

This conclusion is not scientifically valid unless one can prove that the inshore and offshore populations of migrating gray whales are not significantly different in terms of sensitivity to noise, composition (there are indications that mothers and calves migrate inshore), etc. In other words, it remains to be proven whether it is something about the inshore environment that causes whales to show a greater reaction to noise, or something about the composition of whales that migrate inshore.

J. Calambokidis, himself involved in the SRP, cautions that "[s]ome of the research projects conducted, especially the phase on blue and fin whales that I was involved with, were not able to fully achieve their objectives...It is important that the limited sample size from this experiment not be construed as indicating a lack of impact." (p. E-348).

Models

Once again, I must emphasize that the accuracy and reliability of the input data are missing from these sophisticated models, rendering them all but meaningless: the "emperor has no clothes". On p. 4.2-58, the Navy attempts to use the AIM estimations to help quantify how the risk of exposure would affect an animal's life history. It claims that it is "using a very conservative assumption" when it supposes "that half of the animals [lose] one quarter of their breeding season".

What the Navy fails to recognize is that the effects of sound on the reproductive success of an animal can potentially last well beyond the duration of the 20 days of transmissions. As Kastak et al. (1999) noted, their pinniped subjects avoided locations they associated with noise experiments even though these were areas where they also received food. They postulate that marine mammals could avoid critical breeding or feeding grounds if they associate these with exposure to loud sounds, resulting in a dramatically decreased reproductive output.

So, no, saying half the animals could lose one quarter of their breeding season is not conservative. If an animal stays away from critical areas it associates with loud noise, it has lost ALL its breeding season for that year. Moreover, it may never return in subsequent years. That the effects of noise can extend well beyond the actual period of transmission has been suggested for gray whales on the breeding and calving grounds (Jones et al. 1994). The authors observed dramatically fewer cow-calf pairs and their premature leaving of the breeding/calving lagoon after one month of noise broadcasts. Even the following year, cow-calf counts remained low, possibly, the authors postulate, as a result of the previous year's decreased breeding success. The Navy's calculation therefore strongly underestimates the potential impacts of its noise on an animal's lifetime reproductive potential.

Bottlenose whales

Very disappointing is the Navy's unwillingness to protect one of the few populations of beaked whales worldwide which are well-studied (off Nova Scotia). Given that the Greek and Bahamas strandings point to possibly greater sensitivities to noise in the beaked whales, given that these whales can be very elusive and difficult to detect acoustically, given that they are especially vulnerable because they are curious and approach ships, and given that theirs is a year-round resident population concentrated over a very small area (Whitehead et al. 1997), it seems incomprehensible that their unique habitat not be considered an exclusion zone for LFA sonar. Particularly galling is the claim that they are being protected by listing them under Area Number 1 of OBIAs (p. 2-12), when in fact, protection only extends to the 200 m isobath, where they almost never occur. This is like saying that the Navy is committed to protecting all marine mammals that inhabit the skies!

It is also astounding that the Navy does not feel the need to keep the 180-dB SPL out of all National Marine Sanctuaries (Comment 5-1.7). It is doubtful, and certainly unproven, that operation of LFA sonar will "...not destroy, cause the los[e] [sic] of, or injury [sic] any sanctuary resources...". I could not find the letter mentioned in Appendix A.

Mitigation

Mitigation is grossly inadequate, partially because the area over which potential serious effects could occur is likely greater than a radius of only 1 km (see above under "180 criterion"). Furthermore, the HF/M3 sonar could use frequencies above 200 kHz to impact odontocetes less. It makes little sense to have the mitigation be a potential threat as well.

Selected specific comments

p. 4.2-59: If we assume that there is no noise other than LFA sonar, it still would not be adequate for a whale to experience no masking 80% of the time, if during the other 20% of the time a predator is masked, resulting in the whale's death.

p. 4.4-4: "...four SURTASS LFA sonar systems...would introduce far fewer signals and far less total energy into the ocean than seismic survey airguns in the Gulf of Mexico alone." Clearly, some sort of consistency in regulations is desirable, but is the Navy proposing that LFA sonar would be a substitute for the airgun noise? If not, then wouldn't the noise be cumulative? I'm sure that every single point source of air or water pollution can make the same argument-- that it is all a "drop in the bucket" compared to the rest, but we couldn't possibly protect our environment if this sort of logic prevailed. It is simply not good enough and juvenile besides (my kids try this argument on me all the time!) to point the finger at worse offenders in an attempt to "get off the hook".

Comment 2-2.1: "How effective is SURTASS LFA sonar going to be because of restricted areas?" The FEIS states that "[t]he restricted areas will not affect SURTASS LFA sonar routine training and testing, as well as the use of the system during military operations." However, p. 2- 23, contradicts this by noting that "...Alternative 2 [unrestricted operation] would provide Fleet operators with ...maximum submarine detection capability..."

Comment 2-4.7: I am glad the Navy admits that "...the conservative assumptions about the risk continuum cannot be verified by the Long Term Monitoring Program."

Comment 2-3.5: I was amused to read that one reason why LFA deployment cannot be deferred until long-term effects have been determined is that the Long Term Monitoring Program could then not be implemented! Better to study the whales than actually protect them from threat, I guess.

Comment 4-4.10: The FEIS writes: "[h]earing impacts are...analyzed at greater length because they are believed to occur at lower sound levels, and shorter durations, than non-hearing impacts." The best available data from the Greek and Bahamas strandings so far appear to contradict this "belief".

Comment 4-5.14: The Navy writes: "[m]ethods to investigate physiological reactions (e.g. TTS, PTS, stress) to underwater LF sound are not yet [my emphasis] available for free-ranging large whales." "Yet" implies these methods are just around the corner, when in fact, most physiological reactions of large whales at sea will remain unknowable, for all intents and purposes.

Comment 4-5.16: The FEIS states: "...the SRP selected the most plausible and likely impacts to address, in particular, significant change in a biologically important behavior. They observed none...Other less plausible and unlikely effects were not addressed." Is migration a "biologically important behavior? Are mating calls? Both of these DID show change, according to the SRP. It is very open to interpretation whether these changes were "significant" or not. As far as "plausible and unlikely effects" are concerned, was it plausible and likely that beaked whales over distances of several tens of kilometers die as a result of NATO LFAS or the Navy's standard operating sonar? If it was considered so plausible and likely, why didn't the Navy/NATO take steps to prevent it or at least study the phenomenon ahead of time?

Comment 4-5.21: I am glad that the Navy admits that "...injury cannot be studied in the wild."

Comment 4-5.38: As I understand the Hastings et al. (1996) study from conversations with Dr. A. Popper, a co-author of the paper, there was indeed delayed sensory damage that was not an artifact of the sacrificing schedule.

Comment 4-9.18: What about surface ducts?

Comment 4-10.2: "It is assumed that marine animals have evolved to adapt to current oceanic ambient noise levels." Evolution in whales takes a long time--this assumption is, in all probability, false.

Comment 8-1.3: "The page IX comment concerns the possible effects to cetaceans of increased anthropogenic noise in the oceans as compared between pre-shipping conditions and present shipping conditions...". No, as I read it, the page IX comment specifically refers to LFA: "...the most serious potential impacts of LFA [my emphasis] are likely its potential contribution to a long-term decrease in foraging efficiency or communication of marine animals..."

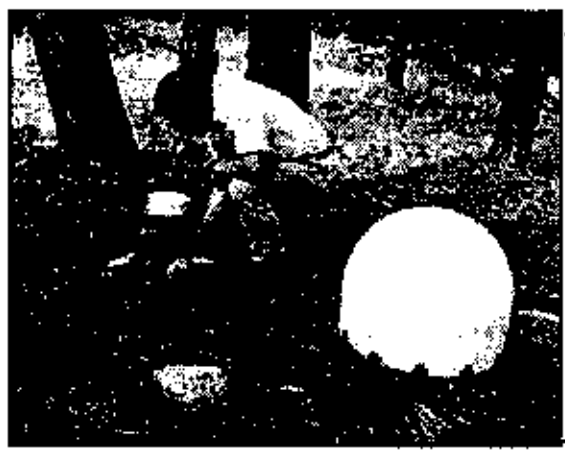
In conclusion, I ask NMFS to act responsibly to safeguard our (meaning: the world's) marine natural heritage.

Sincerely,

Linda Weilgart, Ph.D.

Listening for secret nukes, hearing giant meteors

May 23, 2001 Posted: 12:34 PM EDT (1634 GMT)



Los Alamos researcher Rod Whitaker checks an infrasound station

By Richard Stenger
CNN

(CNN) -- Intelligence scientists listening for covert nuclear blasts had their ears rattled by other explosive sounds -- the detonation of meteors as they streaked over the Pacific Ocean.

The Earth eavesdropping, conducted by researchers at the Los Alamos National Laboratory in New Mexico, was intended to detect atomic weapons tests by rogue nations or organizations in remote locations.

Instead, the Los Alamos listening stations picked up the sound of two large meteors as they plunged into the atmosphere off the coast of Mexico, the Los Alamos lab said this week.

The space rocks raced across the sky in April and August. But the lab waited to announce its findings until other U.S. space scientists last week confirmed the two objects.

The meteors were unusually big, between 6 and 10 feet in diameter. The first one created an explosive pressure wave with as much energy as 2,000 to 3,000 tons of TNT, according to Los Alamos researchers. The second, larger one could have produced a shock wave equivalent to 8,000 tons of TNT.

"Had anyone seen the April 23 event, they would have seen quite a show. That meteor was one of the five brightest ever recorded," Los Alamos scientist Doug ReVelle said.

Meteoroids, meteors and meteorites

Confused by the space rock terms?
A **meteoroid** is a pebble or stone in space.
A **meteor** is the bright flash of light that a meteoroid produces as it streaks across the sky, and also refers to the stone itself while in the atmosphere.
A **meteorite** is a meteoroid that survives its fiery atmospheric entry and strikes the Earth's surface.

Each year, listening stations at the lab record an average of 10 meteors 6 feet in diameter or greater. Those that appear as huge fireballs in the sky, like the April and August specimens, are known as bolides.

Bolides make dazzling displays dozens of miles above the planet.

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Fortunately, most explode into thousands of pieces or burn up entirely before they reach the surface. If these two survived, they probably smacked into the ocean, well away from populated regions, the scientists said.

Scientists think that two sizable meteors exploded into huge fireballs like the Yukon meteorite, which left this smoke trail in January, 2000

The destructive capability of bolides that strike land is considerable. An extremely large one blasted the huge Meteor Crater in Arizona.

The unaided human ear cannot detect the low frequency pressure waves when at a great distance. But specialized microphones at four Los Alamos monitoring stations in the United States can both detect the infrasonic waves and help plot their locations.

The infrasonic information takes minutes or hours to reach the stations, which therefore cannot provide advance warning about approaching large meteors.

However, the Los Alamos scientists welcome the opportunity to monitor falling space rocks, which allows them to fine tune the instruments to use to detect nuclear blasts.

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