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3aNS2. A critical analysis of: Wind Turbine Health Impact Study: Report of Independent Expert Panel

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The "Wind Turbine Health Impact Study: Report of Independent Expert Panel" study says: "The Massachusetts Department of Environmental Protection (MassDEP) in collaboration with the Massachusetts Department of Public Health (MDPH) convened a panel of independent experts to identify any documented or potential health impacts of risks that may be associated with exposure to wind turbines, and, specifically, to facilitate discussion of wind turbines and public health based on scientific findings." It continues, saying: "The scope of the Panel's effort was focused on health impacts of wind turbines per se." The Massachusetts study treats health affects broadly in accordance with WHO and includes direct health effects, annoyance, and sleep disruption. In many ways the Massachusetts study is a critique of the literature relating to wind farm acoustic emissions and health effects. This paper is a critique of the critics. In particular, this critique examines some of the physical acoustic findings and some of the social survey findings. The Massachusetts study reveals itself to have problems similar to those that it criticizes in other reports.

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I. Introduction

Proponents of wind farms usually state that there is no scientific evidence that low frequency sound affects people. What we term herein as “the Massachusetts (MA) study” is one such example of this class of reports. In many ways this class of reports contains critiques of the literature relating to wind farm acoustic emissions and health effects. The MA report treats health effects broadly in accordance with WHO definitions and includes direct health effects, annoyance, and sleep disruption.

The state of Massachusetts developed such a report whereby the Massachusetts Department of Environmental Protection (MassDEP) “convened a panel of independent experts to identify any documented or potential health impacts of risks that may be associated with exposure to wind turbines, and, specifically, to facilitate discussion of wind turbines and public health based on scientific findings.” Because of the emphasis on health effects, this project was undertaken in collaboration with the Massachusetts Department of Public Health (MDPH).

In this paper, we critique the MA study; in particular, this critique examines some of the physical acoustic findings and some of the social survey findings.

We deal with the following two topics in this report:

> a) The quality of the analysis of the physical wind turbine sound description and discussion. In particular, we review the use of references.
>
> b) The quality of the analysis of the literature that discusses human response to the wind turbine noise.

II. Discussion

a) Physical sound

The MA study in several places uses as its foundation the "fact" that wind turbines do not produce significant infrasonic sound; rather, they produce higher frequency sound modulated at the blade passage frequency. The MA study relies on two papers, one by Oerlemans (2009), and one by van den Berg (2004), to make its case that there is no significant infrasonic sound.

The van den Berg (2004) paper does not appear to support much of the material attributed to it by the MA study. The MA study says:

> “Amplitude modulation in wind turbines noise has been discussed **at length** by Oerlemans (2009) and van den Berg (2004).”

The van den Berg (2004) paper does not contain the word modulation or anything like it. The van den Berg main text does include one sentence on this topic and this same material is included in two shorter sentences in the conclusions. The aforementioned sentence from the main text of van den Berg says:

> “On these nights, certainly at distances between 500 and 1000m from the wind park, one can hear a low pitched thumping sound with a repetition rate of about once a second (coinciding with the frequency of blades passing a turbine mast), not unlike distant pile driving, superimposed on a constant broadband ‘noisy’ sound.”

This is the "at length" discussion. The MA study attributes the following to van den Berg:

> “Amplitude modulation is what causes the whooshing sound, referred to as swish-swish by van den Berg (that sometimes becomes a thumping sound).”

Van den Berg (2004) never says the swish-swish sound becomes a thumping sound. Rather, he suggests the unequal blade loading of the rotor disk as the source of acoustic emissions.

The complete paragraph from the MA study says:

> "It is important to make the clear distinction between amplitude-modulated noise from wind turbines and the ILFN from turbines. Amplitude modulation in wind turbine noise has been discussed at length by Oerlemans (2009) and van den Berg (2004). Amplitude modulation is what causes the whooshing sound referred to as swish-swish by van den Berg (that sometimes becomes a thumping sound). The whooshing noise created by modern wind turbines occurs because of variations in the trailing edge noise produced by a rotor blade as it sweeps through its path and the directionality of the noise because of the perceived pitch of the blade at different locations along its 360^0 rotation. The sound is produced in the audible range, and it is modulated so that it is quiet and then loud and then quiet again at a rate related to the blade passing frequency (rate blades pass the tower) which is often around 1 Hz."

The MA Study ignores the major findings by van den Berg (2004) that appear to be contrary to the findings of the MA Study. The main thrust of van den Berg’s paper is that the sound levels go way up above the predicted levels, up to 18 dB according to van den Berg, and concomitant with this increase is low frequency “thumping." Van den Berg does not say that the swish-swish becomes a thumping sound.

van den Berg (2004) continues with a discussion that deals with the wind velocity gradient at night and how this contributes to uneven blade loading that generates the low-frequency audible and infrasonic energies produced by the wind turbine. He shows how the thumping and concomitant human effects will be exacerbated as turbines get bigger and hence, the uneven blade loading grows.

As part of his conclusions van den Berg states:

> “Thus, the logarithmic wind profile, depending only on surface roughness and not on atmospheric stability, is not for wind profiles at night. Especially for tall wind turbines, estimates of the wind regime at hub height based on the wind speed distribution at 10 m, will lead to an underestimate of the immission sound level at night: at low wind speeds ($v_{10} \leq 4$ m/s) the actual sound level will be higher than expected for a significant proportion of time. This is not only the case for a stable atmosphere, but also, to a lesser degree, for a neutral atmosphere. The change in wind profile at night also results in lower ambient background levels than expected: at night the wind speed near the ground may be lower than expected from the speed at 10 m and a logarithmic wind profile, resulting in low levels of wind induced sound from vegetation. The contrast between wind turbine and ambient sound levels is therefore more pronounced at night.”

In terms of emphasis, van den Berg’s conclusions occupy one full journal page. Of this, one sentence talks about swishing noise and what certainly is in excess of 90% of the conclusions talk about increased annoyance, increased "thumping", etc. The point is that the swishing is a minor part of the van den Berg paper which discusses and draws conclusions about serious low frequency noise problems coming from the wind turbines. The MA study seems to draw conclusions opposite of those intended by the author. They attribute terms like "amplitude-modulated sound" to van den Berg, but modulated or modulation cannot be found in the van den Berg paper.

If all of the above does not explain van den Berg's material clearly enough, van den Berg says and presents the following:

> "At night the sound from the wind park contains repetitive pulses, unlike the sound in daytime. According to the long-term auditory observation of residents this pulse-like character or 'thumping', is more pronounced and more annoying at high turbine rotational speed. Fig. 8 shows a recording of the sound pressure level every 50 ms over a 180 s period, taken from a DAT-recording on a summer night (June 3, 0:40 h) on a terrace of a dwelling at 750m west of the westernmost row of wind turbines (this sound includes the reflection on the facade at 2 m). There is a slow variation of the 'base line' (minimum levels) probably caused by variations in wind speed and atmospheric sound transmission. There is furthermore a variation in dynamic range: a small difference between subsequent maximum and minimum levels of less than 2 dB is alternated by larger differences. In the lower part of Fig. 8 part of the sequence is amplified and shows at first a somewhat irregular pattern of dynamic range 1–1.5 dB leading to a more regular pattern of a pulse every second with a pulse height of 3–4 or 5–6 dB. This pattern is compatible with a complex of three pulse trains with pulse height of about 1 dB and slightly different repetition frequencies of about 1 Hz. When the pulses are out of phase (around 150 s in Fig. 8), there are only 1 dB variations. When 2 of them are in phase (around 160 s) pulse height is doubled (+3 dB), and tripled (+5 dB, 170 s) when all three are in phase. The rotational speed of the turbines at the time was 20 r.p.m., so the repetition rate of blades passing a mast was 1 Hz."

Figure 1 is taken from van den Berg (2004) Figure 8. It shows about 180 seconds of wind turbine noise recorded at night, and pulses at a 1 Hz rate are clearly evident; this is infrasound at 1 Hz. And if van den Berg's main Figure 8 was not clear enough by itself, it includes a 30-second blowup that shows the 1 Hz infrasound pulses with absolute clarity. This is not modulation at 1 Hz; van den Berg never says that this 1 Hz infrasound is "modulation." Thus, it is difficult to understand how the MA study comes up with their statements. In addition to van den Berg, the Massachusetts study cites Oerlemans (2009) for "amplitude modulation," but mistakenly places the reference under the author's first name placing it in the wrong position in the list of references so that it is difficult to find. More importantly, again leaving out critical information, they cite this study as definitive when it was a study that used a scale model in a wind tunnel, for at least a majority of the study, where there is no wind gradient, to collect much of the data, and Oerlemans used a 500 Hz high pass filter for some of the field measurements. Nevertheless, Oerlemans is correct for daytime and "daytime-like" conditions; the modulation sound is the dominant A-weighted sound. But as reported by van den Berg (2004), the critical time is at night when there is a large wind gradient. Then, the low-frequency thumping sound frequently occurs and, as a result, van den Berg reports up to an 18 dB increase in the sound level, which he says will increase the annoyance.

Also, measurements at Shirley, WI (Walker *et al.*, 2013) clearly show low-frequency audible and infrasonic acoustic energy at the blade passage frequency.

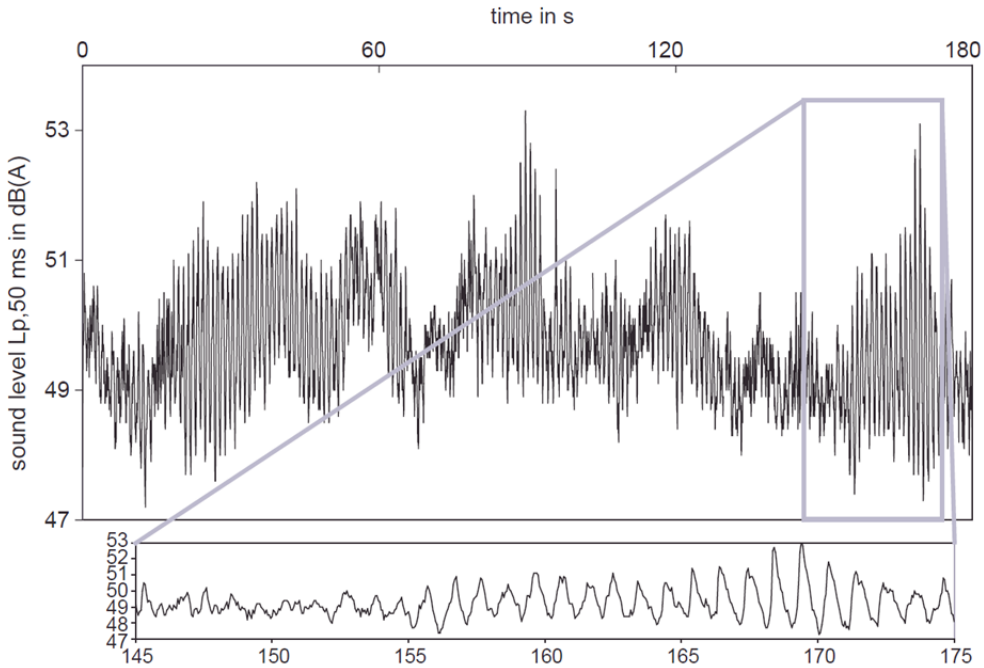

Figure 1. Three minutes of recorded wind turbine sound. The sample rate is 50 ms and note that the recording is A-weighted. A-weighting attenuates by 148 dB at 1 Hz, so the sound pressure level, flat-weighted, should be very high--over 100 dB (after van den Berg 2004, Figure 8).

b) Human response

The MA study appears to be a careful analysis of much of the literature related to annoyance, and it appears to find something wrong with every study that has been done. However, what it finds wrong is questionable. For example on page 17, the study makes the statement:

> "The limited description of the selection process in this study is a limitation as well, as is the cross sectional nature of the study. Cross-sectional studies lack the ability to determine the temporality of cause and effect; in the case of these kinds of studies, we cannot know whether the annoyance level was present before the wind turbines were operational from a cross sectional study design. Furthermore, despite efforts to blind the respondent to the emphasis on wind turbines, it is not clear to what degree this was successful."

And on page 20 it says:

> "The Dutch study has the limitation of being cross sectional as were the Swedish studies..."

The MA study authors make much of the fact that the surveys are cross-sectional rather than longitudinal, attacking several of what are just a few studies with this argument. The only way for these surveys to have been longitudinal and to demonstrate that the annoyance was due to noise and not an attitude to wind turbines is for the studies to have begun long before the turbines were announced as a possibility.

In 2011, Fidell et al. published an analysis of all aircraft noise surveys worldwide for which there was some information in English. Fidell et al. report on 43 surveys, only one of which had longitudinal elements. Every one of the other 42 surveys was cross-sectional. Schomer et al. (2012) performed similar studies for road traffic (37 surveys worldwide) and railroads (11 surveys worldwide). Every one of these 48 surveys was cross-sectional. Also, there have been a number of studies--at least four or five--by the military of the noise from large weapons, and each of these was cross-sectional (Bullen and Hede, 1984; Cook et al., 1994; Schomer, 1981, 1985). Cross-sectional is the state-of-the-art in acoustics. There are hundreds of refereed papers on cross-sectional surveys in acoustics. The experts on the MA study knew or should have known the state-of-the-art in acoustics.

These experts also fault the study above saying:

> "Furthermore, despite efforts to blind the respondent to the emphasis on wind turbines, it is not clear to what degree this was successful."

This is an argument that ignores the state-of-the-art. How well is an aircraft noise survey hidden in a survey near an airport; how well is artillery noise, demolition noise, bombing noise, and tank fire noise hidden in a survey near an army fort? Nobody knows, but this is the state-of-the-art.

In another comment on page 26, they state:

> "This study is somewhat limited by its size-much smaller than the Swedish or Dutch studies described above-but nonetheless suggests relevant potential health impacts of living near wind turbines. There are, however, critical details left out of the report that make it difficult to fully assess the strength of this evidence. In particular, critical details of the group living 3-7 km from wind turbines is left out. It is stated that the area is of similar socioeconomic makeup, and while this may be the case, no data to back this up are presented--either on an area level or on an individual participant level."

And on page 20, they state:

> "The New Zealand study recruited participants from what the authors refer to as two demographically matched neighborhoods (an exposed group living near wind turbines and a control group living far from turbines), although supporting data for this are not presented."

The MA study faulted other studies for lack of this matching; here the "problem" changes to the journal article's arguments being invalidated because they did not include all of their data in the article. The most prestigious journal in acoustics, the Journal of the Acoustical Society of

America would charge somebody $60 per page if they would even permit an article with excessive data, which is not likely since one of their questions for reviewers asks if the manuscript is sufficiently concise, and if it is efficient use of the journal space. It seems more than probable that the problematic New Zealand study is not in fact problematic by academic standards.

Also the MA study says the following about the New Zealand study:

> "Although there were no statistically significant demographic differences between the two groups, 43.6% of those in the exposed group had a university education while only 34.2% in the control group did."

Here, the MA study is clearly saying, there is no statistical difference between the two groups, and then they go on to make the observation that two numbers look different to them, even though it's not statistically significant. After having made this observation they go on to say in their summary paragraph on this study:

> "This raises concern that self-selection into the study could differ by important factors in some way between the two groups. The difference seen in education level between the groups exacerbates this concern."

In their summary paragraph for this study they include phrases such as, "this raises concern," and, "this study could differ," and they draw support for their concern by difference between the percentages of respondents who are college graduates while admitting that this difference is not statistically significant.

III. Conclusions

The authors of the Massachusetts study show problems with their analysis at many points, such as by selectively quoting evidence in a reference when the majority of the reference is contrary to a major thesis of the MA study. The perfect survey is impossible to create from a practical standpoint, but that seems to be the only evidence satisfactory to the authors for establishing a relationship between wind turbine noise and annoyance. The authors state that the surveys should have been longitudinal and imply that each survey should have been implemented and in operation before a community even knew that a wind farm was being considered. Their statements also suggest that they believe journal articles need to include every piece of data and every analysis that a panel of professionals can question or request. And even when a statistical analysis is complete and shows two groups do not differ significantly, the MA study panel finds problems just as if they had been statistically different rather than accepting the evidence that they are not different.

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