

Commentary on the Potential Human Health Impact of the Algonquin Power
Company Proposal for the Windlectric Wind Energy Generating System.

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Introduction

The first public meeting to describe the proposal for the Windlectric 75 MW wind energy generating system on Amherst Island, dated December 2011, put forward a single document to address the potential adverse health impacts, a paper by Knopper and Ollson (2011) “Health effects and wind turbines: A review of the literature.” Other references have been added to the company website but no further document has been prepared in advance of the second public meetings to be held on March 5th and 6th, 2013. Drs. Knopper and Ollson have been retained as consultants by Algonquin Power Co.

The purpose of this commentary is to evaluate the case made by Knopper and Ollson:

Part I discusses the strengths and weaknesses of the arguments made in the 2011 paper.

Part II discusses errors of omission and errors of commission as well the existing state of knowledge as of January 2013, 18 months after Knopper and Ollson’s (2011) publication. A considerable amount of new information continues to evolve (Part II and Appendix C) which appears to have been passed over by Algonquin Power Co.

Part I

Knopper and Ollson: Strengths and weaknesses of authors

Both authors have academic doctorates from recognized mainstream universities in Canada. Knopper’s thesis was entitled: Use of Non-destructive Biomarkers to Measure Effects of Pesticide Exposure in Meadow Voles (*Microtus Pennsylvanicus*) Living in Golf Course Ecosystems of the Ottawa/Gatineau Region, University of Ottawa, 2004. Dr. Knopper’s thesis focused on molecular biological techniques to measure changes in the messenger RNA of voles following exposure to pesticides.

Dr. Ollson’s thesis was entitled “Arsenic Risk Assessments: The Importance of Bioavailability” and was published in 2003 under the auspices of Royal Military College. The core of Dr. Ollson’s work related to soil sampling techniques to determine concentrations of arsenic from natural and other sources as a basis for risk assessment to humans.

In addition to their theses both would have been required to do course work and would be expected to be well trained in research methodologies, critical appraisal and statistics at minimum. The responsible departments were Biology for Dr. Knopper and Environmental Sciences for Dr. Ollson’s graduate work.

In short both have a credible basis of scholarship from which to review scientific literature.

However, neither author has any formal education nor training that qualifies them to be licensed health care practitioners or clinician scientists. Interpreting a person or patient's history, conducting an examination of a human subject, ordering or interpreting diagnostic blood tests or imaging, developing a differential diagnosis, rendering a working or final diagnosis or recommending treatment are all activities that lie beyond their scope of expertise. As will be discussed these constraints appear to be set aside by the authors. In their remarks and conclusions they venture into the sphere of health care practitioners (e.g. diagnosis, pathogenesis and treatment recommendations) areas in which neither of them has expertise. Both authors are qualified in risk assessment of noxious agents pertaining to human health.

Strengths and Weaknesses of the Published Paper

General Comments: Knopper and Ollson (2011) correctly portray government policy directions in favour of wind energy stating that “wind power has become an integrated part of provincial energy strategies across Canada;..” (p. 2/79), and go on to emphasize that in Ontario the “Ontario Power Authority has placed a great deal of emphasis “on procuring ‘renewable and cleaner sources of electricity’”. (p. 2/79) (Ontario Power Authority, 2008, p. 14). The quote and interpretation are accurate.

However this paper is produced by authors who have a legitimate claim to scholarship as noted above. One would expect that a role that they may have considered is critical appraisal of the operating assumption of governments. For example they might have referred the reader to objective sources in the scientific literature that substantiate the priority given to wind energy by governments in Canada and internationally. The claims of benefit, especially the reduction of greenhouse gases, have been put forward by the industry and their consultants and supported by governments but seldom confirmed by third party research. A few months after (December 2011) the Knopper and Ollson (2011) publication the Office of the Auditor General of Ontario (2011) raised this precise point “.... no thorough and professional cost/benefit analysis had been conducted to identify potentially cleaner, more economically productive, and cost-effective alternatives to renewable energy, such as energy imports and increased conservation.” (p. 97). As trained academic scholars the authors have a responsibility to question underlying assumptions. Furthermore in the Action Group Darmstadt Manifesto (1998), over 100 scientists from Germany signed a document questioning the rationale for the wind industry. Knopper and Ollson (2011) did not identify this source, as it was outside the scope of their review (2003 -2011). If there is a paucity of third party evidence for benefit from wind energy the risk/benefit analysis changes. This is especially the case in view of the emerging literature confirming serious adverse health effects. (see Part II)

Knopper and Ollson (2011) state “In terms of noise, high sound pressure levels (loudness) of audible noise and infrasound have been associated with learning, sleep and cognitive

disruptions as well as stress and anxiety” (p. 2/79). The assertion is correct with appropriate supportive references.

Knopper and Ollson (2011) then go on to state “This minimum setback distance was developed through noise modeling under worst-case conditions to give a conservative estimate of the required distance to attain a sound level of 40 dB(A) (*Ontario Ministry of the Environment: Development of Noise Setbacks for Wind Farms Requirements for Compliance with MOE Noise Limits 2009. [9]*), the noise level that corresponds to the WHO (Europe) night-noise guideline, a health-based limit value “necessary to protect the public, including most of the vulnerable groups such as children, the chronically ill and the elderly, from the adverse health effects of night noise” (*World Health Organization Europe Night Noise Guidelines for Europe. 2009, ISBN 978 92 890 4173 7*) (p. 2/79). This claim is misleading (see Appendix A p3-4). As pointed out in the WHO document identified, it did not address noise from wind turbines which is a uniquely disturbing community based noise (Pedersen and Persson Waye 2004). Furthermore the regulations in Ontario permit noise levels up to 51 dBA (Appendix A p.3) under specified conditions. The omission of this higher upper limit on noise by Knopper and Ollson (2011) has significance in a logarithmic scale such as dBA. For example 50 dBA represents double the sound energy of 40 dBA. The authors also failed to reference the Ontario government commissioned report of Howe, Gastmeier, Chapnik Limited (2010); a late draft version of December 2010 stated that “... a nontrivial percentage of persons being highly annoyed” (p. 2), is an anticipated outcome under Ontario’s regulations. Dr. Ollson acknowledged he was aware of the report in testimony at the Environmental Review Tribunal, Ontario, Chatham Kent hearing of March 2011. (The draft version was not altered in the final report of December 2011.)

Knopper and Ollson (2011) were unable to reference the Møller and Pedersen (2011) publication which concluded: “It is thus beyond any doubt that the low-frequency part of the spectrum plays an important role in the noise at the neighbors” (p. 3742). Knopper and Ollson submitted their paper on May 2, 2011 prior to the publication of the Møller and Pedersen paper. On the other hand the Møller and Pedersen (2011) publication is of such importance that every effort should have been made by Algonquin Power Co. in their current submission especially as low frequency noise was the focus of the Howe, Gastmeier, Chapnik (2010) report and of the testimony of the Chatham-Kent Environmental Review Tribunal, Ontario (2011). Given that Knopper and Ollson (2011) opine that most claims about adverse health effects are “anecdotal in nature” (p. 6/83), Algonquin Power Co. may have considered acknowledging the emergence of important new evidence in their submission since 17 months have passed since the Knopper and Ollson on-line publication. (The new evidence will be discussed in more detail in Part II of this commentary.)

Finally Knopper and Ollson (2011) end the section on “Background” by identifying their intention to use a “weight-of-evidence” (p. 2/79) (WoE) approach. Knopper and Ollson (2011) gave no reference for this approach. However strict criteria for the deployment of the WoE approach do exist as outlined in Balls, Amcoff, Bremmer, Casati, Coecke, Clothier, Combes, Corvi, Curren, Eskes, Fentem, Gribaldo, Halder, Hartung, Hoffmann, Schechtman, Scott, Spielmann, Stokes, Tice, Wagner, & Zuang (2006). For example the following excerpts were found:

Conclusion #6.

WoE validation assessments must be conducted with true independence and transparency, and must be designed and managed according to the highest standards. It is essential that those involved have sufficient expertise and experience, that the test methods or testing strategies are ready for evaluation, and that there is agreement on the nature, quantity and quality of the evidence to be considered and its collection, how the resultant data should be weighed, and how the conclusions of the evaluation should be determined and reported. (Balls et al., 2006, p. 12)

Conclusion #9.

The types of evidence to be collected, how it is to be obtained and selected, the extent to which it comprises all the available material, how its quality is to be checked, and whether it is relevant and reliable, are crucial issues. It must also be clearly established that the evidence is truly representative of the performance of the procedure or strategy, and that its collection is without bias. (Balls et al., 2006, p. 12)

Unfortunately these and the other conclusions and recommendations appearing in the Balls et al. (2006) paper seem not to have been followed. For example Knopper and Ollson have long been employed and/or retained by the wind industry and other wind industry proponents to provide expert advice which is acknowledged but nonetheless they lay claim to scientific objectivity. Yet the strict criteria outlined in Conclusion #9 have not been respected especially given the omission of references and the (mis) interpretation of others (see Appendix B). Knopper and Ollson (2011) also claim that they “...considered all available evidence.” (p. 3/80) and yet the foregoing point about omissions and interpretations of references remains as a legitimate criticism.

Methods: It is a strength that Knopper and Ollson (2011) used recognized search tools to retrieve the literature but thereafter a weaning process was done:

Although hundreds of articles were found during the search, very few were related to the association between potential health effects and wind turbines. For

example, numerous articles have been published about infrasound, but very few have been published about infrasound and wind turbines. Indeed, only fifteen articles, published between 2003 and 2011, were found relevant. (Knopper and Ollson, 2011, p. 3/80)

The authors cite their references 12-26 as representing the selected articles but the list is not complete using their criteria. For example van den Berg's (2005) paper is omitted. Was there a further weaning by the authors related to redundancy or some other criteria not mentioned? In short clarity surrounding selection criteria are incomplete and/or inadvertent omissions have arisen. Similarly the Howe, Gastmeier, Chapnik Limited (2010) report (as mentioned previously), the American Wind Energy and Canadian Wind Energy report (2009) and the published proceedings of the Fourth International meeting on Wind Turbine Noise (2011) were also omitted. All of these works have a direct bearing on Wind Turbine noise and human health impacts of high annoyance, sleep disturbance and stress and certainly merit inclusion as significant publications which the authors were or should have been aware of.

In the final paragraph under "Methods" Knopper and Ollson (2011) opine: "What is clear is that some people living near wind turbines experience annoyance due to wind turbines, and visual impact tends to be a stronger predictor of noise annoyance than wind turbine noise itself" (p. 3/80). This statement is a conclusion not a description of methods and as such appears to anticipate the outcome of their literature review. Such reasoning fails the test of scholarship the authors claim for their paper. Apart from mis-placement, the statement is inherently inaccurate. According to Pedersen and Persson Waye (2004) wind turbine noise and its genesis of annoyance and sleep disturbance occurs at lower dBA levels than other community noise with the exception of railway shunting yards. dBA is a measure of loudness only and this does not and should not be expected to account for the entirety of human reaction to wind turbine noise owing to a number of qualities of the noise including intermittency, cresting, tonality and infrasound and low frequency noise content. The authors acknowledge some (not all since they exclude ILFN) of these characteristics but nonetheless simply seek correlation between human response and pure measures of loudness. Another recurring error is the claim of the importance of visual impact a priori and the assumption that cross-sectional studies can adequately establish a sequence or direction of causation. Only longitudinal or at minimum before and after studies can provide proof of the direction of causation. Without this study design the question of whether people come to dislike the sight of wind turbines **after** they or their neighbours became ill or did their dislike of the sight of wind turbines exist **before** the advent of turbines in their neighbourhood? According to Krogh, Gillis, Kouwen, and Aramini (2011) many individuals who welcomed the notion wind turbines prior to having to live within 2.0 kilometres of them changed their attitude for a number of reasons. (In subsequent publications Shepherd, McBride, Welch, Dirks, and Hill (2011) and Nissenbaum, Aramini, and

Hanning (2012) also found early enthusiasm for wind turbines changed following exposure to operating wind farms. (see Part II) Family members, including children became ill, neighbourhoods divided and often their plight was ignored by responsible authorities (Krogh, Gillis, Kouwen, & Aramini, 2011). These impacts would all contribute to wind turbines taking on a different and threatening meaning which could account for people not liking the disruption of their view-shed as a consequence of and not in anticipation of wind turbine facilities.

The Five Key Points

Key Point #1: Generally the summary of the Pedersen and Persson Waye (2004, 2007, and 2008) papers under the first point are accurate and well presented. There are however some omissions which lend a different perspective to Pedersen and Persson Waye (2004) research findings. For example Pedersen and Persson Waye (2004) stated: “.. the proportions of respondents annoyed by wind turbine noise are higher than for other community noise sources at the same A-weighted SPL and **that the proportion annoyed increases more rapidly.**” (p. 3467). (emphasis added). The emphasized wording further clarifies how noise from wind turbines is different from other community based noise.

Another statement Pedersen and Persson Waye (2004) made provides some insight into the difficulty of predicting visual influence owing to the existence of conflicting evidence. “A general prediction of the visual influence on noise annoyance, however, can not yet be made with any certainty as both attenuating Kastka and Hangartner, 1986 and amplifying effects e.g., Watts *et al.*, 1999 have been detected.” (p. 3468)

Perhaps the most important comments by Pedersen and Persson Waye (2008) was: “There is thus support both from experimental and field studies **that intrusive sound characteristics not fully described by the equivalent A weighted sound pressure level contribute to annoyance with wind turbine noise.**” (p.4) (emphasis added)

and

In line with the attention restoration theory outlined by Kaplan (1995), it can however be anticipated that people with a need for recovery, for example when coming home from a stressful day at work, would react strongly and with annoyance if their home environment was made unsuitable for restoration by the intrusion of an annoying sound or a visually intrusive object. This should be investigated in further studies. (Pedersen & Persson Waye, 2008, p.4)

The need for further studies is a key point and one that Pedersen and Persson Waye (2004, 2008) recurrently make. The issue of absence of evidence not being proof of an absence of adverse effects will be expanded in subsequent discussion.

These comments suggest that seeking simple linear relations between sleep disruption and sound pressure level A-weighted, i.e. dBA, is not likely to succeed when the explanation is more complex. Among the complexities is that sleep disturbance cannot be measured solely by self-reports of waking since people are not aware of arousals which can impact restoration. Arousals (which fall short of waking) occurring during sleep can only be measured in human subjects by electronic monitoring in a sleep laboratory. The need for these studies has been clearly identified by Hanning, anaesthesiologist and sleep specialist, at the Environmental Review Tribunal, Ontario (2011). Dr. Ollson was present for these hearings and therefore is aware of this information which has not been cited here. As will be noted in Part II Hanning (2012) and Nissenbaum, Aramini, and Hanning (2012) produced peer reviewed publications on the same subject but this was following publication of the Knopper and Ollson (2011) paper.

Key Points #2: Knopper and Ollson (2011) begin with a reasoned literature review of Pedersen (2011), Pedersen and Persson Waye (2004, 2007, 2008) Pedersen, F. van den Berg, Bakker, and Bouma (2009, 2010), Pedersen, Hallberg, and Persson Waye (2007), and Pedersen and Larsman (2008) work over the past 10 years. However, the line of reasoning departs from the factual and into the speculative. The key paragraph in this respect is:

Pedersen notes that this finding is not necessarily evidence of a causal relationship between wind turbine noise and stress but **may be explained** by cognitive stress theory whereby “an individual appraises an environmental stressor, such as noise, as beneficial or not, and behaves accordingly” (emphasis added). In other words, it appears **that it is the change in the environment that is associated with the self-reported health effects, not the presence of wind turbines themselves.** (Knopper & Ollson, 2011, p. 4/81) (emphasis added)

The text quoted from Pedersen (2011) is expressed in speculative fashion “may be” and then Knopper and Ollson (2011) proceed to a higher level of certainty in the last quoted sentence. This claim by Knopper and Ollson (2011) appears at odds with previous statement Pedersen, van den Berg, Bakker, and Bouma (2009) made:

Wind turbines are a new source of community noise to which relatively few people have yet been exposed. The number of exposed people is growing, as in many countries the number of wind turbines is rapidly increasing. The need for guidelines for maximum exposure to wind turbine noise is urgent: While not unnecessarily curbing the development of new wind farms, it is also important to avoid possible adverse health effects. (Pedersen, van den Berg, Bakker, & Bouma, 2009, p. 634)

Knopper and Ollson took the notion a step further and appear to be forming a diagnostic conclusion “**..it is the change in the environment...not the presence of the turbines ..**” (p.

8/85) (emphasis added) that are responsible for the reported health effects. As noted at the outset this is very troubling on several levels. First, neither of the authors has training as a health care professional nor as a clinician scientist. Secondly, they have formulated a diagnostic opinion in the absence of any direct evaluation of those being adversely effected. Thirdly, their conclusion contradicts the subsequent (July 2011) finding of the Chatham Kent ERT which stated specifically:

This case has successfully shown that the debate should not be simplified to one about whether wind turbines can cause harm to humans. The evidence presented to the Tribunal demonstrates that they can, if facilities are placed too close to residents. The debate has now evolved to one of degree. (Environmental Review Tribunal, Ontario, 2011, p. 207)

The submission of Algonquin Power Co fails to address this ERT decision

Key Points #3: Much emphasis on the importance of visual impact is repeated but with the same short-comings outlined earlier (direction of causation, difficulty in correlating loudness or dBA with complex human response). However a new point is made about the lower rate of complaints among those receiving economic benefits from wind turbine installation. If economic benefit did not reduce the frequency of complaints it would be surprising. All manner of occupations are accepted if a risk premium is paid from National Football League players to workers employed on high level steel girders, from miners to remote placement health professionals. If a risk premium is paid, the risk is accepted. It is concerning however that hosts of wind turbines are not being screened longitudinally for adverse health effects such as hypertensive episodes or sleep disturbance. These conditions are often insidious and disturbed sleep, for example, is a major risk factor for coronary heart disease (Chandola, Ferrie, Perski, Akbaraly, and Marmot, 2010). In the absence of such screening it may be claimed that complaints are fewer but it cannot be claimed that there are no adverse health effects.

In addition Knopper and Ollson (2011) refer to exposed individuals being “influenced by variables not yet identified, some of which are nonphysical.” (p. 5/82) It is not clear what is meant by “nonphysical”. Throughout the article the authors appear to separate psychological from physical and if so it is dated thinking. In the WHO (2001) report “New Understanding, New Hope” the Director General made the following statement: “Understanding how genetic, biological, social and environmental factors come together to cause mental and brain illness. Understanding how inseparable mental and physical health really are, and how their influence on each other is complex and profound.”(p. IX)

In other words the separation of mind and body or of physiology and psychology represents a misunderstanding that Knopper and Ollson (2011) fall prey to throughout the paper. This is

highly relevant to the reported adverse health effects which include both sleep disorders and hypertensive episodes as well as stress and psychological distress. The effects of stress, psychological distress, sleep disruption and difficulty in initiating sleep are pathogenic in nature and in the long term may lead to irreversible damage through a process of allostatic overload as described by McEwen (1998). Physiological and psychological adverse effects are not separate effects but rather pathologically synergistic and part of the same pathophysiology.

Key Point #4: This section is ably presented.

Key Point #5: In this section Knopper and Ollson (2011) address the question of ILFN by quoting an industry funded study commissioned by NextEra and undertaken by O’Neal, Hellweg, and Lampeter (2011). Apart from the obvious concerns about conflict of interest in the genesis of the study one is left to wonder why the Howe, Gastmeier, Chapnik Limited (2010) report which was commissioned by the Government of Ontario was overlooked by the authors. (See Appendix A p6 reference 34). It is also noteworthy that no human subjects are evaluated in the O’Neal et al. (2011) study but rather reference standards and criteria generated by engineering practices versus anthropocentric or human health assessment.

Popular Literature: Knopper and Ollson (2011) are critical of the work of Nissenbaum (2010) because the research had not yet appeared in a peer reviewed journal. The criticisms are no longer valid since the publication of Nissenbaum, Aramini, and Hanning (2012) in a peer review journal. Knopper and Ollson (2011) go on to state that the work is flawed because Nissenbaum (2010) “appear to lack of objectivity as authors are also known advocates who oppose wind turbine developments.” (p. 7/84) No reference is given nor evidence advanced for this pronouncement. Nissenbaum, Aramini, and Hanning have appeared for plaintiffs regarding wind turbine developments just as Ollson has appeared for the wind industry. Both Nissenbaum and Hanning are fully qualified medical specialists while Aramini has a doctoral degree in statistics. The tribunal decision from the Environmental Review Tribunal, Ontario (2011) Chatham Kent portrayed a different and more respectful view: “Not surprisingly, once the definitive statements on the legal test were peeled back, a healthy scientific debate was uncovered”. (p. 195) The comment unfortunately appears to reflect more on Knopper and Ollson than Nissenbaum, Aramini and Hanning. Their claims of objectivity appear compromised especially as they are closely linked to the wind industry and have gained employment in the past from the wind industry as acknowledged in the section “Competing Interests”. In contrast Nissenbaum et al. (2012) pursue their employment independent of any financial influence of special interests, government or industry groups.

Finally Knopper and Ollson (2011) state: “Though sound pressure level in most of the peer reviewed studies was scaled to dB(A) (but refer to O’Neal et al. (2011) for actual measurements of low frequency noise and infrasound), infrasound is a component of the sound measurements

and was inherently accounted for in the studies.”(p. 7/84) This is a claim that is not in accordance with the work of Leventhall, Pelmar, and Benton (2003) nor in agreement with the WHO Guidelines for Community Noise (1999) edited by Berglund, Lindvall, and Schwela position which is explicitly quoted by Leventhall et al (2003) as follows:

- "For noise with a large proportion of low frequency sounds a still lower guideline (than 30 dBA) is recommended"
- "When prominent low frequency components are present, noise measures based on A-weighting are inappropriate"
- "Since A-weighting underestimates the sound pressure level of noise with low frequency components, a better assessment of health effects would be to use C-weighting.
- "It should be noted that a large proportion of low frequency components in a noise may increase considerably the adverse effects on health"
- "The evidence on low frequency noise is sufficiently strong to warrant immediate concern"

The matter of monitoring ILFN is a crucial one. When Denmark was contemplating more stringent guidelines regarding ILFN in 2011 the response from the industry was allegedly negative. According to Aalborg Universitet (2011) in an article citing “New Danish regulations for wind turbine noise” it was found that:

In October 2011, a report from Aalborg University revealed serious errors in the Environmental Impact Assessment for a prestigious Danish test centre for large wind turbines⁶. The law establishing the centre had to be revised, and the handling of noise from wind turbines in the EPA attracted political attention in Parliament. The Minister of the Environment promised to introduce limits for low-frequency wind turbine noise.

Political pressure was also put on the EPA to consult our group at Aalborg University when preparing the rules. This was not accomplished in a meaningful manner, however, and, despite many comments and suggestions made to the EPA, it had no impact at all on the result. (Aalborg Universitet, para 11, 12).

Annoyance: In this section of the Knopper and Ollson (2011) paper the following claim is made: “..the hypothesis that infrasound is a causative agent in health effects does not appear to be supported.” (p. 8/85). This claim is contrary to the paper “Low-frequency noise from large wind turbines” by Henrik Møller and Christian Sejer Pedersen published in June 2011. (The Møller and Pedersen paper appeared the in June 2011 the month following the submission of the Knopper and Ollson (2011) paper.) The submission of Algonquin Power Co. does not consider this new information which is unfortunate considering the conclusion reached by Møller and Pedersen (2011) was quite different from that of Knopper and Ollson (2011) , specifically: “ It is

thus beyond any doubt that the low-frequency part of the spectrum plays an important role in the noise at the neighbors.” [of large wind turbines] (p. 3742)

Conclusions: Knopper and Ollson (2011) state categorically in the conclusions of their paper: “To date, no peer reviewed scientific journal articles demonstrate a causal link between people living in proximity to modern wind turbines, the noise (audible, low frequency noise, or infrasound) they emit and resulting physiological health effects.” (p. 8/85) Assuming for the moment that this claim is true (see previous commentary where even proponents acknowledge annoyance occurs and that it is an adverse health effect) the conclusion is flawed. A more defensible conclusion would be that in the absence of definitive evidence for or against significant adverse health effects being experienced by people in the environs of wind turbines, more research is needed. While Knopper and Ollson (2011) do accurately reflect the position of the industry, governments and proponents who favour wind energy, there is insufficient evidence to make the claim quoted above. Knopper and Ollson might also have reviewed the evidence of Dr. Warwick Anderson (Commonwealth of Australia, Official Committee Hansard, Senate. (2011) : “...we believe that a precautionary approach should be taken to this, because, as you would understand, the absence of evidence does not mean that there might not be evidence in the future...” (Commonwealth of Australia, Official Committee Hansard, Senate, 2011, “Social and economic impact of rural wind farms,” p. 86) . This claim of absence of evidence by Knopper and Ollson (2011) appears to be put forward as proof that there are no significant adverse health effects. To the contrary the dearth of evidence should stimulate more research not denial. In addition more consideration should be given to application of the precautionary principle as suggested in the Australian Senate hearings above.

Finally in the conclusions Knopper and Ollson (2011) claim: “..and the fact that it appears that a limited number of people have self-reported health effects that may be attributed to the indirect effects of visual and attitudinal cue.” (p. 9/86) In view of the evidence to date and subsequent to the Knopper and Ollson (2011) paper, this claim cannot be defended. It seems Knopper and Ollson (2011) are victim blaming and expressing willingness to trade off damaged people for a presumption of a greater good. They express the willingness in the absence of any critical appraisal or quantification of how many people are being adversely affected, how serious the impact is over time and whether or not claims made by critics about the possible ineffectiveness of wind energy are valid as claimed by Action Group Darmstadt Manifesto (1998) and Office of the Auditor General of Ontario’s Report of December 2011 among others.

Mr. Peter Hadden (a United Kingdom medical specialist presenting to a parliamentary committee in 2008) identified his doubts about the benefits of wind energy and the possible violation of human rights. Subsequently in Falmouth, MA, USA, C. Devlin (posted 2013, January 16) pointed out in November 2012 some governments and the wind industry are conducting

“Experimentation on people without their consent”. These concerns would be effectively addressed if the requisite research were to be done that would enable the creation of evidence - based guidelines for wind turbine setbacks. To date that research has not been done.

Conclusions of Commentary

1. While Knopper and Ollson (2011) are well qualified academically, they have committed errors of omission and commission identified throughout the text of this commentary as well as inaccuracies in the use of references (see Appendix B).
2. Bias has been suggested by the Knopper and Ollson (2011) description of experts testifying on behalf of complainants of adverse health effects from the wind industry installations as “known advocates who oppose wind energy development” (p. 7/84) in spite of the fact (revealed in testimony at the Environmental Review Tribunal, Ontario (2011) Chatham Kent which Dr. Ollson attended) that physicians such as Hanning and Nissenbaum have a professional responsibility to be health advocates.
3. Knopper and Ollson (2011) have inappropriately attributed adverse health effects reported by complainants as a matter of “indirect effects of visual and attitudinal cue” (p. 9/86) in the absence of direct clinical evaluation or testing of complainants by health care professionals and clinical scientists.
4. The stance of the wind energy proponents is accurately represented by Knopper and Ollson (2011) that in the author’s opinion: “To date, no peer reviewed scientific journal articles demonstrate a causal link between people living in proximity to modern wind turbines, the noise (audible, low frequency noise, or infrasound) they emit and resulting physiological health effects.” (p. 8/85). However the absence of evidence is no reason to conclude no harm exists since there is a shortfall in the relevant research.
5. The possible violation of human rights noted by Hadden (2008) in a presentation to the UK parliamentary committee (and post publication by Devlin, 2012) has not been addressed by Knopper and Ollson (2011).
6. As scholars with academic credentials Knopper and Ollson have an ethical responsibility to update and address the short-comings of their paper and evolving evidence (See Part B).
7. The Knopper and Ollson (2011) paper falls short as an objective literature review and does not advance the understanding of global reports of adverse health effects in the environs of wind turbines.

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