BEFORE THE BOARD OF INQUIRY

IN THE MATTER of the Resource Management Act 1991

AND

IN THE MATTER of applications for resource consent and notices of requirement by Transpower New Zealand Limited for the North Island Grid Upgrade Project

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STATEMENT OF EVIDENCE OF JAMES MARK ELWOOD IN REBUTTAL FOR TRANSPOWER NEW ZEALAND LIMITED (EMF and human health issues (epidemiology))

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Introduction

1. **MY** name is James Mark Elwood. I wish to present rebuttal evidence to the statements of evidence of:

(a) Dr Laura Bennet and Mr Adrian Kinsler;
(b) Dr Robert McQueen;
(c) Dr Allanah Kilfoyle;
(d) Mr Bruce Davidson;
(e) Mr George Vercoe (on behalf of Parahiwi Farm);
(f) Mr John Makin; and
(g) Ms Julie Eliot (in regard to the BioInitiative report).

2. **I** address the evidence of each submitter below.

Dr Laura Bennet and Mr Adrian Kinsler (Dr Bennet evidence) (Submission number 1085 and 1091)

3. **IN** the section of the Bennet evidence headed "On the question of research. What constitutes convincing or satisfactory research?" (pages 2-3), Dr Bennet presents a cost argument on the value to society of preventing childhood leukaemia. The weakness with this argument is that it depends on the assumption that a causal relationship to electric and magnetic fields (**EMF**) exists, so that changes of exposure limits would prevent some cases of childhood leukaemia. This is a very questionable conclusion, for reasons given in my evidence in chief (at paragraphs 288-291).

4. **DR** Bennet states under the heading "The scientific data: specific scientific references to consider" (at page 3) that "[*e*]*very health agency who has assessed the effects … conclude that the human population data (epidemiology) for some health effect such as childhood leukaemia remain valid*". This is very imprecise language. The major review groups agree that the available data show an association, but none of the major review groups conclude that a *causal* relationship exists (as is shown in my evidence in chief, for example at paragraphs 135 to 141).

5. **THE** sentence that "*Current new studies (references Draper and Lowenthal) provide a more robust approach … and have confirmed that the association between childhood leukaemia and EMF remains statistical*" (pages 3-4), highlights an often-made claim which is not valid. This is the concept that as more research has been done, the evidence for health hazards of EMF has become stronger.

6. **THERE** are two main errors with this concept. First, more recent studies are not necessarily of higher quality than previous studies, and the two studies referred to are good examples of this. They are extremely limited studies, looking mainly at information restricted to the addresses of the subjects, without any of the careful measurements of exposure and considerations of confounders used in the previous high-quality large case-control studies, such as those from the United Kingdom[1], the United States[2], and Canada[3], even though those studies were published some time ago.

7. **AS** reviewed in my evidence in chief at paragraphs 249 to 251, the Draper study[4] uses very limited methodology, does not show a clear relationship to magnetic fields, as distinct from merely a geographical association with powerline positions, gives results which are outweighed by the much more powerful United Kingdom study of childhood leukaemia which clearly shows no association either with measured magnetic fields or with distance from powerlines, and is not even interpreted by its authors as supporting a causal relationship between leukaemia and magnetic fields.

8. **ANY** reasonable presentation of this study should note the authors' own cautious interpretation: for example, "*we have no satisfactory explanation for our results in terms of causation by magnetic fields or association with other factors*", and their final statement of conclusions "*We emphasise again the uncertainty about whether this statistical association represents a causal relation*".

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[1] UK Childhood Cancer Study Investigators. Exposure to power-frequency magnetic fields and the risk of childhood cancer. Lancet 1999; 354(9194):1925-193; and UK Childhood Cancer Study Investigators. Childhood cancer and residential proximity to power lines. Br J Cancer 2000; 83(11):1573-1580.
[2] Linet MS, Hatch EE, Kleinerman RA, Robison LL, Kaune WT, Friedman DR et al. Residential exposure to magnetic fields and acute lymphoblastic leukemia in children. N Engl J Med 1997; 337(1):1-7.
[3] McBride ML, Gallagher RP, Thériault G, Armstrong BG, Tamaro S, Spinelli JJ et al. Power-frequency electric and magnetic fields and risk of childhood leukemia in Canada. Am J Epidemiol 1999; 149(9):831-842.
[4] Draper G, Vincent T, Kroll ME, Swanson J. Childhood cancer in relation to distance from high voltage power lines in England and Wales: a case-control study. BMJ 2005; 330(7503):1290.

9. **THE** Lowenthal study[5] is a study of a different group of diseases, mainly in adults, and again is based mainly on address information. It is a much weaker type of study than for example the occupational cohort studies which address adult leukaemia. This study has been reviewed in my evidence in chief at paragraph 332, and I comment further on it later in regard to Dr Kilfoyle's evidence.

10. **SO** in terms of Dr Bennet's terminology, the studies do not provide "*a more robust approach*", and do not confirm the association between childhood leukaemia and EMF.

11. **FURTHER**, in some other areas where recent studies have been of higher quality than earlier studies, the new information has substantially weakened the evidence for a causal relationship with EMF. The situation with adult breast cancer and occupational and residential magnetic fields is one of the clearest, so that the 2007 World Health Organisation (**WHO**) report[6] states (page 307):

> "*Subsequent to the IARC monograph a number of reports have been published concerning the risk of female breast cancer in adults associated with ELF magnetic field exposure. These studies are larger than the previous ones and less susceptible to bias, and overall are negative. With these studies, the evidence for an association between ELF exposure and the risk of breast cancer is weakened considerably and does not support an association of this kind. In the case of adult brain cancer and leukaemia, the new studies published after the IARC monograph do not change the conclusion that the overall evidence for an association between ELF and the risk of these diseases remains inadequate.*"

12. **DR** Bennet mentions at the second to last paragraph of page 4 of her evidence, one study, not previously mentioned in these discussions, which sounds relevant. This is the study by Anselmo[7]. In Dr Bennet's evidence, this comes immediately

[5] Lowenthal RM, Tuck DM, Bray IC. Residential exposure to electric power transmission lines and risk of lymphoproliferative and myeloproliferative disorders: a case-control study. Intern Med J 2007; 37:614-619.

[6] World Health Organization. Environmental Health Criteria 238: Extremely low frequency fields. 1-519. 2007. Geneva, WHO.

[7] Anselmo CW, Santos AA, Freire CM, Ferreira LM, Cabral Filho JE, Catanho MT et al. Influence of a 60 Hz, 3 microT, electromagnetic field on the reflex maturation of Wistar rats offspring from mothers fed a regional basic diet during pregnancy. Nutr Neurosci 2006; 9(5-6):201-206.

after a discussion of socio-economic issues and house prices, and her text refers to the "*offspring from mothers exposed during pregnancy to EMF in combination with and poor diets* ".

13. **ONLY** by looking at the reference list does one realise that this study was in laboratory rats. It is quite misleading for Dr Bennet to refer to this study without pointing out that this is an experimental study of laboratory animals. It looked at a combination of EMF exposure at 60 Hz with a 3 μT (30 milligauss) magnetic field, and a deficient diet, and assessed delay in the development of seven different reflexes, for example, palm grasp and auditory startle reflexes. Delays were shown in the development of some reflexes in association with a deficient diet and EMF separately, and the combined exposure of a deficient diet and EMF showed delays in all seven reflexes. The issue with this experimental study is of course the relevance of the findings to disease and disability in humans, which is by no means clear.

Dr Robert McQueen (Submission number 1076)

14. **DR MCQUEEN** claims to have reviewed "*many thousands of pages of documents and research papers about the health impacts of electricity transmission lines*" (paragraph 7), but the list of papers included is a very short, highly selected list, ignoring most of the most important studies. There is no information presented in this evidence regarding health effects.

15. **A** further unpublished paper dated 3 July 2005 is authored by Dr McQueen, Dr Smart and Dr Bennet[8]. There is no discussion of the scientific evidence in this report, merely statements of the authors' conclusions. The first sentence of the second paragraph, to the effect that there is recent research "*which establishes the biomedical mechanisms*" for health effects, and the following sentence saying *"[i]t is no longer possible for apologists to say these effects are unproved*", is contradicted by the multidisciplinary comprehensive reviews of major groups, such as WHO, International Agency on Research for Cancer (**IARC**), International

[8] McQueen RJ, Smart R, Bennet L. Proposed New Zealand EMF health standard for new construction of overhead high-voltage AC electricity transmission lines. 3-7-2005. Unpublished Work.

Commission on Non-Ionising Radiation Protection (**ICNIRP**), and others, as noted in my evidence in chief, paragraph 137.

16. **THE** only reference to the scientific literature which is given in this unpublished paper is a reference to an unpublished report by Dr Smart[9]. This has not been peer-reviewed. It is very similar to Dr Smart's earlier submission to this hearing, which I reviewed in my evidence in chief, paragraphs 513-537.

17. **THE** rest of this paper proposes a new exposure standard. The relevant questions with this proposal relate to the likelihood, and extent, of any health benefits from shifting from the current situation to this proposed standard, in comparison with the costs and any hazards related to the change. Given the current uncertainty in the human health evidence, one likely scenario is that no benefits to human health would accrue from making this change.

Dr Allanah Kilfoyle (Submission number 0498)

18. **DR** Kilfoyle's evidence is in the form of a PowerPoint presentation, and so I will refer to the slides by number, based on the order presented (reading from left to right, then to the next row).

19. **THE** presentation is very selective, referring to only a few studies and ignoring many others. It does not represent a balanced or comprehensive summary of the scientific evidence.

20. **IN** slide 4, Dr Kilfoyle notes two studies published after the IARC review (which was published in 2002). There are many other studies also. More recent reviews such as that by WHO in 2007[10] include these studies, while coming to similar overall conclusions to the IARC 2002 report.

21. **SLIDE** 5 may be easily misinterpreted because of the heading "*Lack of animal data*". This could imply that few animal studies have been done. In fact, many

[9] Smart R. Health effects of high voltage transmission lines: a survey of the medical literature December 2004. 1-32. 2004. Unpublished Work.
[10] World Health Organization. Environmental Health Criteria 238: Extremely low frequency fields. 1-519. 2007. Geneva, WHO.

studies have been done, including studies involving prenatal and lifetime exposures, and these studies in general show neither initiation or promotion effects of cancer. There is a lot of animal data, which in aggregate does not support a cancer-causing effect. However, it is true that there is no accepted animal model for human childhood leukaemia, as noted in my evidence in chief at paragraphs 120 and 126.

22. **SLIDE** 7 says that the National Radiation Protection Board (**NRPB**) regard ionised particles as a "*plausible mechanism*". This means no more than this mechanism can be considered, and the NRPB did consider it in detail. The conclusions of the report to which Dr Kilfoyle refers[11], include the statement that it is not possible to estimate the impact of corona ions precisely, and "*It seems unlikely that corona ions would have more than a small effect on the long-term health risks associated with particulate air pollutants, even in the individuals who are most affected*". In the next paragraph, dealing with further research, it states "*The possible implications for health of the mechanisms discussed in this report do not provide a strong case for further research in this area*" (page 48).

23. **SLIDE** 9 lists a few papers, which form a very small selection of all the available studies. Obviously for any short presentation, there needs to be a selection made, but the basis of the selection is important. The inclusion of the Ahlbom and Greenland studies is appropriate, as these are particularly useful, being large-scale pooled analyses which include data from many other studies. But the choice of the other three studies is questionable, as these are not among the most important studies. It raises the question of whether these studies were chosen because their results fit Dr Kilfoyle's position better than most other studies do.

24. **I** reviewed the Lowenthal paper[12], (referred to in slides 10 to 18), in my evidence in chief at paragraph 332. It is a very limited paper, using a study done over 20 years ago to give information on addresses and occupational histories by job title. No EMF measurements were made, as EMF was not considered in the original study. Dr Kilfoyle points out one of the serious weaknesses of the study (in slide 12); the

[11] National Radiological Protection Board. Particle deposition in the vicinity of power lines and possible effects on health. Documents of the NRPB 15[1], 1-55. 2004. Chilton, NRPB.
[12] Lowenthal RM, Tuck DM, Bray IC. Residential exposure to electric power transmission lines and risk of lymphoproliferative and myeloproliferative disorders: a case-control study. Intern Med J 2007; 37:614-619.

information was obtained by different methods for the case series and the control group. Using identical methods in the two groups to be compared is one of the fundamental aspects of good study design, as using different methods can introduce bias, which can be severe.

25. **THIS** study deals with a rather unusual set of diseases, in both adults and children, different from those assessed in other studies, and analysed in two groups: myeloproliferative diseases (**MPD**) and lymphoproliferative diseases (**LPD**). As shown in slides 17 and 18 at the bottom of page 3, the stronger associations in this study were shown for the lymphoproliferative disorders: in this group of 603 patients, only 22% had leukaemia (lymphatic), while the rest had lymphomas or multiple myeloma. In the other group, of which 55% had leukaemia (myeloid), the associations seen were weaker. Only 47 children with leukaemia were included, 5.5% of the total group studied.

26. **THE** next paper chosen for discussion is that of Draper et al[13] in the United Kingdom (in slides 19 to 23 on page 4). I have discussed this paper in detail in my evidence in chief at paragraphs 249 to 251, and I have commented on the published criticisms of the study and the authors' responses. This is also a very limited study based on address information, using controls with other cancers rather than controls representative of the population, and the results contrast to the much more valuable British study[14] of childhood cancer which had much more information on confounding factors and detailed measurements of exposure to EMFs, rather than simply address information, and gives quite different conclusions.

27. **IT** seems misleading to discuss the Draper study without pointing out the conclusions of the authors themselves, which were basically that the association they found with distance from power lines did not fit the hypothesis of it being due to magnetic fields (as I noted in regard to Dr Bennet's evidence). The investigators found that the variation of leukaemia risk with the distance between the residential address and the nearest powerline did not fit with the estimated magnetic field

[13] Draper G, Vincent T, Kroll ME, Swanson J. Childhood cancer in relation to distance from high voltage power lines in England and Wales: a case-control study. BMJ 2005; 330(7503):1290.
[14] UK Childhood Cancer Study Investigators. Exposure to power-frequency magnetic fields and the risk of childhood cancer. Lancet 1999; 354(9194):1925-1931.

strengths at different distances. Thus, the investigators say that the increased risk at considerable distances from the line "*is surprising in view of the very low levels of magnetic field that could be produced by powerlines at these distances*"; this suggests that it was due to some other factor. They concluded "*we have no satisfactory explanation for our results in terms of causation by magnetic fields or association with other factors*".

28. **THE** next paper (slides 24 to 26) noted is one of the major pooled analyses[15], but there is no discussion of the authors' reservations about the interpretation of the study in cause and effect terms, which I note in my evidence in chief at paragraphs 203 to 206. For example, the final statement in the conclusions of Ahlbom and colleagues is that "*The explanation for the elevated risk is unknown, but selection bias may have accounted for some of the increase*".

29. **THE** next study (slides 27 to 30) reviewed, is by Feychting[16], and is simply one of a number of individual studies, chosen presumably because this one shows a significantly increased risk. The study is included in both the Ahlbom and Greenland meta-analyses, so does not represent additional information to that already presented.

30. **THE** final study (slides 31 and 32) is the other major pooled analysis[17]; again the association shown is generally accepted, but the critical issue is whether the association represents a causal relationship or is due to selection bias and/or confounding. Again, the authors of this report do not conclude that a causal relationship has been shown; Greenland and colleagues state that "*the inconclusiveness of our results seems inescapable; resolution will have to await considerably more data on high electric and magnetic-field exposures, childhood leukaemia, and possible bias sources*" (page 633).

31. **THE** summary in slide 31 is correct in concluding that a number of well conducted case-control studies show increased risks of childhood leukaemia. Reference to the one study of (mainly) adult disorders is misleading, as there is no mention of

[15] Ahlbom A, Day N, Feychting M, Roman E, Skinner J, Dockerty J et al. A pooled analysis of magnetic fields and childhood leukemia. Br J Cancer 2000; 83:692-698.
[16] Feychting M, Ahlbom A. Magnetic fields and cancer in children residing near Swedish high-voltage power lines. Am J Epidemiol 1993; 138(7):467-481.

the large number of the other relevant studies (covered in my evidence in chief). Magnetic field exposure is classified as a possible carcinogen, rather than high-voltage lines; for example in the British study only 9 percent of high residential magnetic field levels were due to proximity to high-voltage lines[18].

32. **DR** Kilfoyle's conclusion in her last slide is a statement of opinion, which is not supported by a full consideration of the available evidence.

33. **IN** summary, Dr Kilfoyle's presentation may appear to be scientific and knowledgeable, but is in fact misleading because of selected presentations and a lack of distinction between associations and causal relationships.

Mr Bruce Davidson (Submission number 0874)

34. **IN** his comments on health effects, Mr Davidson states in paragraph 4.7 that "*a number of national and international bodies have studied these possible health effects, and in so doing they have apparently come to a variety of conclusions*".

35. **IN** contrast to that impression, and as discussed in my evidence in chief, the major expert interdisciplinary review groups, basing their conclusions on all the scientific evidence available to them, have been quite consistent in their conclusions. These groups include the NRPB in the United Kingdom, the IARC, the ICNIRP, and WHO. All of these groups have concluded that an association exists in epidemiological studies between magnetic field exposures and childhood leukaemia, which is as yet unexplained but is insufficient to lead to a conclusion that a causal relationship exists, and all these groups have concluded that the evidence for any other health effects is even weaker.

Mr George Vercoe on behalf of Parahiwi Farm (Submission number 0781)

36. **MR VERCOE** states in numbered section 2 (a) that "*there is a proven health issue living close to transmission lines*". While I can agree there is an issue, in that there

[17] Greenland S, Sheppard AR, Kaune WT, Poole C, Kelsh MA. A pooled analysis of magnetic fields, wire codes, and childhood leukemia. Epidemiology 2000; 11:624-634.
[18] Day N, Eden T, McKinney P, Roman E, Simpson J. Childhood cancer and power lines: what do the data mean? BMJ 2005; 331(7517):634.

is much research and discussion on this question, I do not agree that there has been any substantial health hazard demonstrated, or "proven", in people living close to transmission lines, when the totality of the scientific evidence is examined.

Mr John Makin (Submission number 0781)

37. **MR MAKIN** presents some comments on health effects under the heading of "Easement requirements" on page 6 and 7 of his evidence. In the first paragraph of this section, he lists several hazardous exposures which he claims were once regarded as safe. While the establishment of a health hazard will always be preceded by a period of ignorance, the analogies made are questionable.

38. **FOR** example, all these exposures listed have been identified as hazardous by international expert multidisciplinary groups basing their findings on the totality of the scientific evidence, the best example being the IARC, which is the pre-eminent organisation conducting these types of assessments. This organisation has reported extensively on electromagnetic fields, and accepts that an association exists in epidemiological studies of childhood leukaemia, and for that reason has classified low frequency magnetic fields exposure as a possible human carcinogen.

39. **THE** IARC reports do not conclude that a causal relationship has been established between extremely low frequency (**ELF**) exposures and any human disease, as reviewed in my evidence in chief. This is in contrast to its findings using the same methods of inquiry for many other exposures, including the exposures listed by Mr Makin in his evidence. Indeed many of the prominent scientists who have been involved in assessments of magnetic fields, and have concluded that the evidence falls short of establishing a causal relationship, are scientists who have been prominent in demonstrating and publicising the dangers of some of these other exposures.

40. **FOR** example, the first major British review group on low-frequency fields was chaired by Professor Richard Doll, an eminent cancer epidemiologist who was one of the major figures in establishing asbestos and smoking as major causes of cancers. Professor Nicholas Day, who chaired the IARC review of electric and

magnetic fields, is another prominent researcher in regard to many of these exposures. I myself have made a contribution to establishing ultraviolet radiation as the major cause of human melanoma, using the same basis of logic which leads me to conclude that the evidence for health effects of low intensities of exposures to electric and magnetic fields is insufficient to establish causation.

Ms Julie Elliot, in regard to the BioInitiative report (Submission number 0559)

41. **MS** Elliot draws attention to the BioInitiative report. I therefore will make some comments on that report.

42. **AS** noted in my evidence in chief (paragraphs 468-469), this report[19], released on 31 August 2007, was produced by an organising committee of four, with 10 other participants, with comments and other input from others. It consists of a number of chapters, individually authored.

43. **THE** report considers both extremely low-frequency electric and magnetic fields (**ELF**) and radiofrequency (**RF**) electromagnetic fields. These two wavelength regions are considerably different, and only ELF fields are directly relevant to these proceedings.

44. **ONLY** a few parts of the BioInitiative report deal with human health issues and epidemiology in relationship to ELF fields. I have reviewed the chapter on childhood leukaemia[20] in my evidence in chief at paragraphs 470 to 494, and I have given there my reasons for not accepting the conclusions given in the report.

45. **THERE** is a chapter that deals in part with breast cancer and its association with magnetic field exposures[21]; I have reviewed this section in my evidence in chief at paragraphs 495-501. I have shown that this section of the BioInitiative report

[19] Carpenter D, Sage C, (Eds.). BioInitiative Report: A rationale for a biologically-based public exposure standard for electromagnetic fields (ELF and RF). 1-000. 2007. USA, Sage Associates.
[20] Kundi M. Evidence for childhood cancers (leukemia). In: Carpenter D, Sage C, (Eds.), editors. BioInitiative Report: A rationale for a biologically-based public exposure standard for electromagnetic fields (ELF and RF). USA: Sage Associates; 2007. 1-25.
[21] Davanipour Z, Sobel E. Magnetic field exposure: melatonin production, Alzheimer's disease, breast cancer. In: Carpenter D, Sage C, (Eds.), editors. BioInitiative Report: A rationale for a biologically-based public exposure standard for electromagnetic fields (ELF and RF). USA: Sage Associates; 2007. 1-71.

ignores several important studies, and because of this its conclusions are not valid.

46. **I** have considered the issues raised in these sections of the BioInitiative report in reaching my conclusions in my evidence in chief. I will add to that by presenting comments on some other aspects of the report.

The 'Summary for the public and conclusions' section

47. **THE** major points in the BioInitiative report are given in the first section "Summary for the public and conclusions". I here present my review of relevant parts of that section.

48. **THIS** section is written by Cindy Sage, MA, who is co-editor of the report along with David Carpenter, MD.

49. **IN** section B, the objectives and main conclusion of the report are given. It is stated that the report was written by 14 scientists, with another dozen outside reviewers, and the report has involved an "*independent science and public health policy review process*" (page 4). The qualifications and affiliations of the authors and reviewers are not given, and there is no information about how they were selected. There is also no statement of whether conflicts of interest were identified and taken into account; in contrast for example the WHO has a formal policy to require declarations of conflict of interest, and excludes representatives of national and international associations from its processes of review and conclusions, although such people may be present as observers (WHO report[22], page xiv; also noted in my evidence in chief at paragraph 123).

50. **IT** is stated on page 4 of the BioInitiative report that other scientific review bodies have reached different conclusions "*by adopting standards of evidence so unreasonably high as to exclude any conclusions likely to lead to new public safety limits*". It is relevant that the standards used by, for example, IARC and WHO, were not developed specifically for this issue of EMFs, but are standards of evidence which have been found to be useful and acceptable internationally for a

wide range of potentially hazardous exposures. The standards of evidence used by IARC are compatible with those generally used in clinical medicine to determine whether treatments are effective and safe, and whether public health policies are effective. These standards of evidence have overwhelming worldwide support in regard to health issues in general.

51. **IMMEDIATELY** after that is a statement that "*exposure limits for ELF and RF are developed by bodies of scientists and engineers that belong to professional societies who have traditionally developed recommendations; and then government agencies have adopted those recommendations*." It is further stated that "*The standard-setting processes have little, if any, input from other stakeholders outside professional engineering and closely-related commercial interests*."

52. **IT** is important to distinguish between scientific review bodies whose role is to review the available worldwide scientific evidence and reach conclusions about the causal effects of EMFs on human health, and bodies whose role is to set exposure limits and standards. The former activity has worldwide application, whereas standard-setting is usually in the context of a particular country or jurisdiction.

53. **CONTRARY** to the implications of this statement in the BioInitiative report, the international scientific review groups such as WHO deliberately exclude representatives of commercial interests from their deliberations and conclusions; and participants in these reviews are not representatives of groups but act in their independent capacity and are selected for their expertise in the subject. The composition of groups which set exposure limits will vary with context; other witnesses to this hearing have relevant experience in this regard.

The 'Summary of the science' section

54. **I** will now comment on some of the scientific conclusions presented in the report, which start after the heading "summary of the science", on page 8.

[22] World Health Organization. Environmental Health Criteria 238: Extremely low frequency fields. 1-519. 2007. Geneva, WHO.

55. **THE** first key statement is "*there is little doubt that exposure to ELF causes childhood leukaemia*". This statement is incorrect in my opinion, and in conflict with the conclusions of major interdisciplinary review groups and of the authors of the most important individual studies. As discussed in my evidence in chief, there is agreement that there is an association seen in epidemiological studies between exposure to high levels of residential magnetic fields and childhood leukaemia, but the existence of this association is insufficient to justify the conclusion that a cause-and-effect relationship exists. The key issues of alternative explanations of the epidemiological associations, and the lack of supporting experimental evidence, are not mentioned in this section of the BioInitiative report.

56. **THE** second key statement that "*there is some evidence that other childhood cancers may be related to ELF exposure but not enough studies have been done*" is also likely to be misleading. It would be more accurate to say there is some evidence that other cancers may be related to ELF, but the totality of the evidence does not show such a relationship. While it is correct to say that an association cannot be ruled out, the overall evidence does not suggest a causal relationship with other childhood cancers, and that conclusion has not changed between major reports published in 2001 and 2007, as reviewed in my evidence in chief (at paragraphs 292-301).

57. **THREE** recent studies are then highlighted in this section, but they are only indirectly related to the key issue of whether the risk of other cancers is increased in children exposed to high ELF environments. Two of the studies relate to differences in survival of children treated for leukaemia; I have commented on these studies in paragraph 259 of my evidence in chief. The other study, by Lowenthal, is based only on address information of a predominantly adult group with a mixture of diseases, and is a relatively weak study; I have commented on this study in paragraph 332 of my evidence in chief, and earlier in this statement of evidence in rebuttal.

58. **SECTION** 3 deals mainly with radiofrequency exposures, but one study relating brain cancer to employment in electrical occupations is mentioned towards the end of this section. Section 4 also deals with adult cancers, mentioning first the Lowenthal study previously noted in regard to childhood cancers. Other parts of

this section relate to other studies of adult brain tumours, and the various other types of cancer.

59. **THE** conclusion in this section of the BioInitiative report is: "*In total the evidence for adult disease associated with EMF exposure is sufficiently strong for adult cancers that preventive steps are appropriate, even if not all reports have shown exactly the same positive relationship*" (page 11). This conclusion is not shared by any of the major interdisciplinary review groups, which have concluded that the evidence is insufficient, as reviewed in my evidence in chief (at paragraphs 304-307 for residential exposures, and paragraphs 354-358 for occupational exposures). I have reviewed most of the major studies in my evidence.

60. **SECTION** 5 deals with breast cancer; the BioInitiative report concludes that occupational exposures are a risk factor for breast cancer amongst women, and also that ELF exposure can reduce melatonin levels. This section is related to part of chapter 12 in the report. I have reviewed that section in paragraphs 495-501 of my evidence in chief. The BioInitiative assessment omits several important, detailed, and recent studies of breast cancer in regard to residential magnetic fields, which overall show no relationship. Also omitted from the report are several important large-scale occupational studies which also in general show no relationship.

61. **FURTHER**, some of the studies regarded by the authors of the BioInitiative report as demonstrating an association in fact show no overall association. I concluded that this section of the report demonstrates a selective choice of studies which tend to support one particular viewpoint. Compared to the interdisciplinary review groups such as that of the IARC this section of the BioInitiative report is clearly lacking in completeness and objectivity, and its conclusions are therefore unacceptable.

62. **I** have also reviewed the section of the BioInitiative report relating exposures to ELF fields to effects on melatonin below.

The ELF exposure – melatonin reduction – breast cancer hypothesis

63. **THE** BioInitiative report, in Chapter 12, gives much attention to the hypothesis that exposure to electromagnetic fields might reduce the secretion of melatonin, and that the lower levels of melatonin might increase the risk of breast cancer. Melatonin is a hormone produced by the pineal gland, situated within the brain, and is normally produced mainly during the night, triggered by the reduction in visible light perceived by the eye. The BioInitiative report concludes that ELF exposure increases breast cancer risk (Summary, page 11, and also Chapter 12). As I have shown in my evidence in chief, this overall conclusion relating to the association between ELF exposures and breast cancer risk is flawed by selective and incomplete review of the available studies.

64. **MAJOR** review groups have considered this issue. The most detailed review is a 169 page document produced by the Advisory Group on Non-ionising Radiation (**AGNIR**), part of the Health Protection Agency in the United Kingdom, in 2006[23]. This provides a review of the physiology of melatonin, and a detailed review of studies addressing the relationship between exposure to EMFs and melatonin, the relationship between melatonin concentrations and breast cancer, and the overall relationship between exposure to electromagnetic fields and breast cancer. It covers cellular, animal, and human volunteer studies as well as epidemiological investigations.

65. **THE** main findings of this extensive review include the statement: "*investigations using cells, animals and humans have not given consistent or convincing evidence that EMF exposure affects melatonin production or action. However, there are deficiencies in the existing research, which leave open the possibility of an effect*" (page 161).

66. **THE** review group found that the evidence that EMF exposure changes melatonin production in isolated rodent cells or glands was not convincing, and that many but not all studies using rats, mice and hamsters showed that exposure to magnetic fields does not result in a consistent suppression of nocturnal melatonin levels. In regard to human experimental studies, they concluded that the majority of studies did not find any effects of EMFs, but a few laboratory based studies suggest that

acute exposure of volunteers may have some effect on the production or timing of the nightly melatonin rises.

67. **IN** regard to epidemiological studies, they concluded that these did not give convincing evidence that EMFs affect the secretion of melatonin in humans. They noted that many of the published studies did find some significant results in a subset of the data, but there was no consistency as to which subgroup the significant results applied to, and in general significant results in one study have not been adequately re-examined in the same subgroup in subsequent studies.

68. **ON** the question of whether a reduction in melatonin levels increases cancer risk, the review group found that there was evidence that melatonin can inhibit the growth of cancer cells *in vitro* and in animals. However, they found that the information on a possible relationship of melatonin levels to breast cancer risk in humans was limited and inconclusive.

69. **THE** review group concluded that "*There is no consistent evidence, from research using cells, animals and humans, that EMF exposure is a cause of breast cancer, nor has any mechanism for such an association been demonstrated*" (page161).

70. **THE** overall conclusion was "*The evidence to date does not support the hypothesis of exposure to power frequency EMFs affects melatonin levels or risk of breast cancer*" (page 161

71. **THE** melatonin hypothesis was also extensively reviewed in the 2007 WHO report[24]. The WHO report reviews the effects of exposure to ELF magnetic fields, and a few studies of exposure to electric fields, on melatonin and also of several other hormones produced by the pineal and pituitary glands, and reviews studies of the effects of melatonin in experimental situations. Its conclusions were, "*Overall, these data do not indicate that ELF electric and/or magnetic fields affect the neuroendocrine system in a way that would have an adverse effect on human health and the evidence is thus considered inadequate*" (page 186).

[23] Advisory Group on Non-ionising Radiation. Power frequency electromagnetic fields, melatonin and the risk of breast cancer. Documents of the HPA: Series B , 1-169. 1-2-2006. Didcot, Health Protection Agency.

The BioInitiative report on ELF exposures and melatonin levels

72. **THE** BioInitiative report gives a different interpretation of several of the same studies reviewed by these other review groups. Here I will comment on the issue of whether exposure to ELF fields decreases melatonin in humans. The BioInitiative report (page 10 of chapter 12) gives its conclusions as that 11 of 13 published epidemiological studies provide positive evidence that higher magnetic field exposure results in decreased melatonin production.

73. **I** do not agree with the interpretation of the scientific evidence given in the BioInitiative report. As an example, I will comment on one of the studies that is interpreted as a positive study in the BioInitiative report. This is a study of the effect of residential exposure to electric and magnetic fields from high-power lines on melatonin in women in Quebec City, Canada[25]. Melatonin production was measured by urinary excretion of 6-sulfatoxymelatonin (6-OHMS).

74. **IN** contrast to the BioInitiative assessment, the published summary of this study gives a negative conclusion*:* "*A sample of 221 women living near a 735-kV line was compared with 195 women the same age living away from any power lines. Participants provided morning urine samples on 2 consecutive days and wore a magnetic dosimeter for 36 consecutive hours to measure personal magnetic exposure. The indoor electric field was assessed by spot measurements. After adjustment for other factors associated with low melatonin secretion, such as medication use or light exposure, nighttime concentration of 6-OHMS was similar in the two groups. When either 24-hour or sleep-time exposure to magnetic field or electric field measurements was used, no exposure-effect relation was evident.*"

75. **A** possible explanation of the differences in interpretation is given by some further analyses within the study. After the main result, it is stated "*However, the trend of decreasing 6-OHMS concentration with age was more pronounced for women living near the lines, as was a lower 6-OHMS concentration in women with high body mass index. Chronic residential exposure to magnetic fields from high-power lines may accentuate the decrease in melatonin secretion observed in some*

[24] World Health Organization. Environmental Health Criteria 238: Extremely low frequency fields. 1-519. 2007. Geneva, WHO.

vulnerable subgroups of the population." So the authors emphasise one subsidiary finding from the study, that in women with higher exposures, the normal decrease of melatonin levels with age was more pronounced. That is a legitimate thing to do, as it raises a new hypothesis; but as the finding arises from a subset of the data, this new hypothesis needs to be confirmed in another study before it can be accepted.

76. **THESE** findings came from the last part of the analysis, assessing interactions between other factors and living near or far from a powerline. Many different interactions could have been assessed in this study. Three interactions, with age, body mass index, and the use of several medications, were examined. One previous study had found a statistically significant interaction with medications: this was not found in this study.

77. **IN** my opinion, a fair summary of this study is to assess it on the basis of whether the main effect tested was confirmed, and I would classify it as not supporting the main hypothesis that melatonin levels are affected by magnetic field exposures. This is implied in the review from the AGNIR group, and is different from the BioInitiative report which regards it as a 'positive' study.

78. **SIMILARLY**, some other studies are reviewed by the AGNIR group and interpreted generally negatively, in contrast to the BioInitiative report. In these studies, although the main effect does not show a relationship between magnetic fields and melatonin, there are some subsidiary results which do show some relationship. In most cases these subgroup results were not assessing a preset hypothesis, but emerge from the data analysis. In the BioInitiative report, a study seems to be considered as supporting the hypothesis if any result within the study can be interpreted in that way, rather than concentrating on the main result.

79. **IN** addition, one major study published in 2002[26] and interpreted as negative is reported by both the AGNIR and WHO groups but omitted from the BioInitiative review. The WHO group also includes three other studies that were not included in

[25] Levallois P, Dumont M, Touitou Y, Gingras S, Masse B, Gauvin D et al. Effects of electric and magnetic fields from high-power lines on female urinary excretion of 6-sulfatoxymelatonin. Am J Epidemiol 2001; 154(7):601-609.

[26] Youngstedt SD, Kripke DF, Elliott JA, Assmus JD. No association of 6-sulfatoxymelatonin with in-bed 60-Hz magnetic field exposure or illumination level among older adults. Environ Res 2002; 89(3):201-209.

the work of the other groups, all of which are interpreted as negative, although they were all weak studies.

80. **ON** the other hand, the BioInitiative report appropriately includes a recent human experimental study[27] published too recently for inclusion in the AGNIR and WHO reviews. This study gives results supporting the concept that magnetic field exposures reduce melatonin.

81. **THERE** are a large number of other studies of the topic which are also relevant. The WHO review lists 14 experimental studies on human subjects relating magnetic field exposures to melatonin responses, and state that 12 showed no effect, with two studies showing some possible effect (Table 46, pages 166-7). The AGNIR report summarised 19 such studies of which only three they regarded as showing some type of effect (Table 4.6, pages 106-7). These studies are not discussed in any detail in the BioInitiative review, where it is simply stated (chapter 12, page 10) that studies under experimental conditions have not found an effect on melatonin production.

82. **FOR** these reasons, I do not accept the claim made in the BioInitiative report that magnetic field exposures reduce melatonin production. It remains an unclear issue.

Conclusion on the BioInitiative report

83. **OVERALL**, based on my review of the parts of the BioInitiative report dealing with human health effects such as cancer and epidemiological studies, I find these sections do not meet the standards of objective and comprehensive review of available scientific studies which I regard as essential in coming to reliable conclusions. Excluding many important studies, and in some cases presenting a misinterpretation of the results of individual studies, is likely to mean that the report's conclusions, at least in terms of epidemiology, are considerably biased.

[27] Davis S, Mirick DK, Chen C, Stanczyk FZ. Effects of 60-Hz magnetic field exposure on nocturnal 6-sulfatoxymelatonin, estrogens, luteinizing hormone, and follicle-stimulating hormone in healthy reproductive-age women: results of a crossover trial. Ann Epidemiol 2006; 16(8):622-631.

84. **I** do not accept that the BioInitiative report demonstrates that ELF exposures are causally related to increases in childhood or adult cancers. In my opinion, the reports of several other interdisciplinary review groups are much more comprehensive, objective, and reliable.

85. **MUCH** of the BioInitiative report deals with appropriate exposure limits for EMFs, and other witnesses will comment on that. From my review, this report is of much lower scientific quality than other available relevant reports, making any further implications drawn from it very questionable.

James Mark Elwood

29 April 2008