Doll II - ELF EMFs and the Risk of Cancer. Docs of the NRPB Vol. 12 No. 1 2001.

Bias and omissions flaw report - incompetence, malfeasance or hidden agenda?

*by **Alasdair Philips***

"Public and politicians must understand and debate risk ... and agree when the precautionary principle must take priority. Advisory committees cannot be stuffed with the great and the good; they must include the lay public and the doubters." [*New Scientist, Editorial, 4th Nov.2000*]

The first major update since 1994 from the NRPB Advisory Group on Non Ionising Radiation (AGNIR) was published on 6th March 2001. This has become informally known as the "Doll II" report.

Although it contains some useful information, this report is seriously flawed by the blinkered approach of what I see as an unenlightened group of scientists who fail to recognise the recent bio-electromagnetic insights that are changing the way that modern science understands life processes in living beings. It is also undermined by missing important studies, misunderstanding others, not providing the pithy analytical comment required, and also by some very sloppy use of scientific and statistical terms. If submitted to a reputable journal it would have been most unlikely to pass the peer review process.

Even worse is that they do admit to a doubling in the incidence of childhood leukaemia in ambient power-frequency (50 Hz) magnetic fields of 0.4 microtesla (μT) or higher, but then go on to dismiss this as a negligible health hazard to the UK population in general. This is politico-scientific prevarication at its very worst. It leaves parents with children living in these higher magnetic fields in a worrying limbo-land and leaves local authorities powerless to prevent the building of new houses close to high-voltage power lines. Either there is a problem that needs addressing with guidance changes, or there isn't a problem. AGNIR admit that there do appear to be real associations between some cancers (including childhood leukaemia) and ELF EMFs, but then seem to say *"problem? what problem?"*.

Indeed, their premise seems to ignore the modern epidemiological concept that it is necessary to identify susceptible sub-groups in the population and advise guidelines that help protect these vulnerable people. Gone is the time that we can expect to identify problems caused by single factors that affect almost everyone exposed (eg cholera spread by drinking contaminated water). We now have many different and novel hazards that people react to in different ways, some being more susceptible to them than others. In whole population studies, the problems of susceptible individuals get masked by the majority of people and so the apparent relative risks (RR) are low and often insignificant, whereas they are much higher and significant for the susceptible groups of people. At the 2001 Bradford-Hill Memorial Lecture, Dr David Strachan described the modern task of epidemiology to help provide ***"safety for the susceptible"***. This requires a significant change of emphasis for many epidemiologists, including AGNIR's.

What we need is a precautionary approach. Dr John Stather, the Assistant Director of the NRPB has repeatedly stated that the NRPB have never chosen to give precautionary advice. I claim that this is just not acceptable for a publicly funded body charged with the duty to advise on health risks of radiation and electromagnetic fields. The AGNIR ask for a high level of scientific proof that EMF causes cancer when for all other walks of life we rely on a Risk Analysis approach. A precautionary approach **IS** needed, and they are the only official voice that can recommend such a course of action. It is a complete cop-out to say that a precautionary approach is political and they, as scientists, can only advise on certainties. **Scientists who sit on advisory committees have a particular responsibility to comment on the implications of scientific findings *especially* in situations where strict causal proof may be lacking.**

They should provide an analysis that covers the spectrum from certainty of hazard (where we need legislation) through to weak association (where a warning might be appropriate). Then the politicians are in a position to do the political bit and choose a level of precautionary approach. There is no way the politicians are going to implement any precautionary science-based policies if their advisers fail to set out the issues and possible ways of dealing with them. We are talking about risk-benefit analysis, but done in a way that identifies susceptible sub-groups of the population.

The introduction (p5) sets out the terms of reference of the AGNIR group: *"to review the work on the biological effects of non-ionising radiation relevant to human health and to advise on research priorities"*. I find it ironic that the NRPB who have repeatedly 'corrected' people for using the word 'radiation' to describe power frequency electric and magnetic fields, choose to use it themselves when setting AGNIR's terms of reference.

They say (p9) that this is a *"comprehensive review"* of work *"carried out since its first report"* (1992). However, they exclude diseases other than cancer (such as Alzheimer's disease, neurological disorders and infections) and frequencies other than those used for electrical power (such as ELF pulsed microwaves from mobile phones). This is a common technique - to split up findings about possible hazards so that the isolated results are inadequate to assess the risk.

The wording is 'sloppy' in many places. For example they say (p12) *"electric fields do not require current to flow"*, and then two pages later say *"for every kilovolt per metre of external electric field ... the short circuit current in an average adult is 14 microamps."* What they meant to say was that an electric field occurs whenever a voltage is present and is proportional in strength to the voltage on the wire and not the current flowing along it. This is poor for an official scientific publication. Clearer explanations of technical terms would be helpful.

They write (p 12) *"naturally occurring EMFs ... normal daily variations from pulses of less than 0.1 Hz are about 0.03 microtesla. This may be compared with the Earth's static field of about 50 μT."*
Trying to relate natural and man-made EMFs is necessary, but it should be done meaningfully.

By their own definitions, ELF covers 30 - 300 Hz, so that 0.1 Hz is virtually static (1 pulse per 10 seconds). They fail to mention that in the 3 - 300 Hz range (where power-frequency fields are) the natural background magnetic flux level is around 50 picotesla (0.00005 μT), so that the (50 nT) ambient power-frequency flux level found in the UK is about 1000 times stronger. That may be important as this 50 Hz noise swamps the naturally occurring Schumann waves that life on Earth has evolved with and is known through NASA and Russian work to be important to the well-being of humans and animals.

Their report implies that the only interaction of alternating magnetic fields with living tissue is due to induced electrical currents. I suggest that oscillating magnetic fields can also influence the orientation of dipolar molecules and vibrate transmembrane proteins. 50 Hz has a period of 20 milliseconds between reversals. Water molecules in air can turn round in microseconds and even in body fluid will have transition times that mean they can fully change orientation every 20 ms. Protein conformation in an aqueous environment, such as found in our bodies, is strongly influenced by the presence of weak non-covalent (e.g. hydrogen, Van-der-Walls, and hydrophobic) bonds that play an important part in living processes. The implications of these interactions should at least be questioned, even if there are no known answers yet.

Instrumentation and Methods
We read (p17) *"Powerlines, substations and electrical appliances ... are not necessarily the dominant contributors to their time-weighted average exposure if the latter is indeed the parameter of interest for such studies. Various other metrics ... ... have been proposed."* What should follow is a reasoned argument about the pros and cons of these different metrics, but this is not forthcoming.

'Exposure Metrics' are the temporal, spatial and other characteristics you use to measure the phenomena under scrutiny (here EMFs) and also how you subsequently analyse the data. In the last few years there have been a number of key papers on this subject that this report doesn't even mention, let alone discuss, yet they cite eleven pre-1992 papers!

One paper that they do cite, is the Ahlbom et al paper [*A pooled analysis of magnetic fields and childhood leukaemia; Br.J.Cancer, 83, 692-698, 2000*] that confirmed the doubling of childhood leukaemia incidence in power-frequency magnetic fields above 0.4 μT. The paper helpfully discussed the various metrics used in the main studies but little of this is commented on in this NRPB report.

Completely omitted is "ELF Magnetic Fields and Childhood ALL: An exploratory Analysis of Alternative Exposure Metrics" [*AJE, Vol.152, 1, pp20-31, 2000*]. This is a very useful exploration of some of the parameters that should be considered.

Another significant source, that amazingly is also not even referred to, comes from an NRPB hosted WHO meeting in 1998 to discuss '*Exposure Metrics and Dosimetry for EMF Epidemiology*', the Proceedings of which contains 15 separate papers about ELF field measurement [*Radiation Protection Dosimetry, Vols 83, Nos 1-2, 1999*].

This was a controversial meeting - one that is documented in published Science & Technology Select Committee Minutes [*1999, 489-II, Appendix 18*]. Of the 49 participants, 14 are listed as directly representing the mobile phone and electricity industries. Independent researchers were excluded from this meeting which limited the scope of the debate to already established viewpoints. Although this was an official World Health Organisation meeting to discuss the future of EMF epidemiological measurements, it was funded by The National Grid Company plc UK, the Mobile Manufacturers Forum (Alcatel, Ericsson, Mitsubishi, Motorola, Nokia), and the GSM MoU Association.

The official response was that numbers were limited to key scientists in order to allow an adequate debate. It is surprising, therefore, to see that Jo-Anne Basile, a non-scientist senior cellular telephone industry Washington lobbyist, was allocated a place. Was there a hidden agenda?

The Geometric Mean (GM) UK residential 50 Hz field level is introduced on page 17 as 36-39 nT. Over the page we find Table 2.3 which is a comprehensive and useful table from Alan Preece of flux densities from household appliances, giving Means and Standard Deviations at various distances. Doll & Co fail to mention that these field levels are Arithmetic Means (AM). Nor is there any discussion of the differences between GMs and AMs despite the fact that they use both in an ad-hoc way throughout the report.

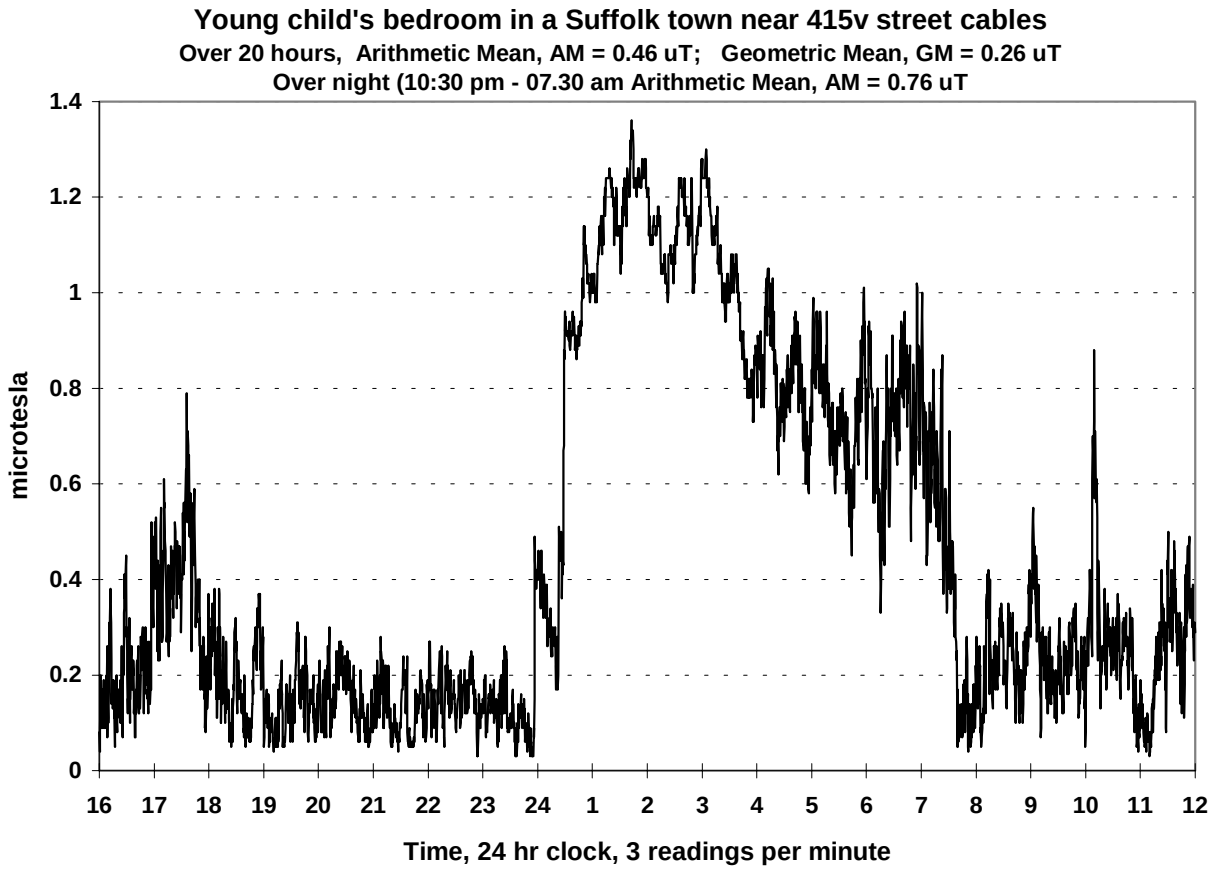
This matters, especially for magnetic fields from overhead power lines. AMs are what we all think of as averages. i.e. we add all the numbers together and divided the total by the number of points added. It is possible that a few very high outlying data points might unduly increase the overall average. This can be easily controlled for by excluding very high outliers from the main calculation and noting them separately, although the effect is usually small and this is rarely necessary.

I believe that GMs are unhelpful as they minimise patterns in data, although interestingly the recent key Ahlbom et al paper (see above) found a slightly higher relative risks when using GM rather than AM. All the 'n' numbers are multiplied together and the n^{th} root taken of the total. [i.e. multiply all numbers together, Log(the result) / divide by n, and then anti-Log]. The important downside of this method is that one zero entry will cause a zero final answer(!), so a few very low points in 'noisy data' will give an even less realistic value (this time tending to zero) than the AM would have done for a few high outlying values.

From the table 2.8 we see a general range of power industry workers occupational exposures. The AMs average 27% higher than the GMs.

More importantly, the 1994 National Grid [*Marchment et al*] paper briefly mentioned in Doll II (p20) raises a very concerning issue. In the paper we find that for houses further than 100 metres from 132 kV (or higher) lines the GM is 36 nT and the AM is 51 nT (42% higher). For houses closer than 100 m to these high voltage lines the GM is 153 an the AM is 973 nT (i.e. the AM is 536% higher - i.e. over five times higher). Surprisingly none of this is even *mentioned* in the NRPB report!

Here is a graph of the magnetic fields in a child's bedroom in a town in Norfolk, measured in 1994. Over the 20 hours of taking readings every 20 seconds we can clearly see the night-time peak due to higher loads flowing in the cables outside his house. The GM of this data is 0.26 μT, the AM is 0.46 μT, and the 9.5 hour night-time exposure AM is 0.76 μT. A number of studies have suggested that it is the night-time exposure that matters and so this "Doll II" report should have discussed the choice of exposure metrics.



A new German study [*Schuz et al, IJC, 91, pp728-735, 2001*] reports statistically significant increased incidence odds ratios (OR) for overnight (10pm-6am) exposure but not for 24 hour exposure. The find a clear dose-response relationship for both their data, and for polled data from all published German research. For children exposed overnight to fields between 0.2 and 0.4 μT the OR is 3.21 and for those in fields 0.4 μT or over the OR is 5.53.

The UK Childhood Cancer Study [*Lancet, V 354, No.9194, pp1925-31*] used separate data for overnight exposure as part of their overall analysis. Hopefully they will now publish a separate analysis against the four magnetic field elements *(Bed winter, Bed summer, School, other)* used in their overall study.

Time Weighed Averages

This highlights another questionable metric - the 'Time Weighted Average' (TWA) which always plays down the importance of high periods of exposure, especially at night. TWAs can be based on GM or AM means from the data and assume that the totality of exposure is the most important. In the tragic case of leukaemia victim Simon Studholme, his head was in magnetic flux levels of over 3 μT during every night. But his final calculated TWA was only about 0.12 μT, well below any suggested threshold for association with leukaemia incidence, and this stopped his legal case proceeding.

Aerosols and Corona Ions

On page 23 they get on to the Bristol (Fews & Henshaw) electric field work, and inexcusably get themselves into a real muddle. Firstly they bundle both Fews et al 1999 papers together (and they were about different things - corona ion effects and oscillating aerosol effects). They also cite a Swanson and Kaune 1999 paper that is solely about magnetic fields and not relevant to the effects of electric fields featured in the Fews and Henshaw papers. The last sentence reads: "*However, it has not been demonstrated that any such enhanced deposition will increase human exposure in a way that will result in adverse health effects to the general public.*"

This is incorrect. It has been demonstrated that such enhanced deposition will increase human exposure in a way that could result in adverse health effects to the general public. Preliminary data from Dr Alan Preece's ecological study was presented to the Bioelectromagnetics Society (BEMS) meeting in summer 2000 which showed a 14% increase in skin cancer and 29% increase in lung cancer within a few 100 metres of high-voltage powerlines. He has since publicly stated a 50% increase in mouth cancer downwind of such lines.

Preece's data is provisional, but the NRPB and AGNIR were aware of it last summer and it provides extremely good confirmation of the cancer effects predicted by the physical science of Henshaw and Fews. **There really is no excuse for AGNIR to so grossly misrepresent this important work in such a deceitful way**. They cite other abstracts from various BEMS and other workshop meetings in this report.

On page 20 the NRPB cite Swanson's work showing a 4.5 fold increase in general population exposure to power frequency magnetic fields between 1940 and 1990 and no similar increase in cancer. What never gets shown in the increase in exposure to ELECTRIC fields which rose in the 20s to 50s and then flattened off. There is a interesting new paper by Dr Sam Milham, linking the electrification of America with the rise in the low age (1 to 5) Acute Lymphoblastic Leukaemia incidence [*Milham & Ossiander, Historical evidence that residential electrification caused the emergence of the childhood leukemia peak, Medical Hypotheses (2001) 56 (3), 290-295*]. This could provide further support for the Bristol hypothesis as most electrification in the USA used overhead high-voltage cables.

They comment on the UKCCS E-field measurements, the results of which have yet to be published. These results were first promised over a year ago, then by last September, now we are told "within a few months".
The need to make measurements in an unperturbed field is stated. This is very questionable - people significantly change the E-field pattern, usually increasing the incident field by factors of up to 10 - in a similar way lightning targets conductors. Surely what we need to for effective epidemiology is the e-field people actually experience?

Other issues
We get an admission (p17) that the main external source of ambient domestic magnetic fields is from underground cables, especially ones with unbalanced (net or stray) currents flowing on them. Something that has been in the electricity industry's and Powerwatch's literature for many years. There is no mention of simple reversing fields compared with the rotating fields that you get from three-phase high-voltage overhead power lines and underground street distribution cables to houses. This is an important issue and has particular relevance to pineal melatonin synthesis (see later), and it should be discussed.

Chapter 3 is "Recent Cellular Studies" and cites 121 studies, over a third of which are dated 1994 or earlier. They conclude that "*there is no clear evidence that exposure to weak ELF fields can effect biological processes*" ... and ... "*Most of the positive effects involved fields greater than 100 μT*". It seems odd that they do not then go on to question the current NRPB 1600 μT maximum guidance level. There seems plenty of evidence here to warrant a re-assessment of the guidance level and a precautionary approach to public exposure.

Chapter 4 is "Animal and Volunteer Studies". Here they admit that there are no natural animal models for some cancers, including childhood ALL. I have many problems with the discussion in this chapter, but will restrict my comments to a few of them.

They report a number of studies showing significant increases in mammary tumours in rats, and comment that a number of studies showed a highly linear dose-response relationship over the flux range 0.3 - 100 μT but not up to 30,000 μT (p74). They then suggest that the opposite effect (a decrease) seen at 30,000 μT suggests that the experiments were not well enough controlled for other variables. There is no mention of the most obvious reason - a non-linear (and most living systems are inherently non-linear) 'amplitude or frequency response window' effect that occurs at chronic exposures but disappears at acute high-level exposures. Such a response could explain the apparently contradictory findings

They reproduce result tables from recent mice and rat experiments that they claim show few effects. These have various shortcomings that they do not comment on. The most obvious is that many of the groups had an almost 100% chemically initiated cancer rate in all animals. This would completely mask most subtle effects and certainly be irrelevant as far as naturally susceptible animals were concerned. Also most of the groups show only one exposure level in the range 0.3 to 100 μT (usually 2 μT) but a number of higher levels up to 5000 μT. Given the apparent dose-response relationship commented on above in earlier (1991 to 1996) studies occurring in the lower flux range it seems most puzzling that exactly this range seems to have been avoided in the more recent studies. Instead of highlighting this problem, the AGNIR report praises these recent studies for their better quality assurance procedures.

There is quite a good section on pineal melatonin and cancer, but though it mentions linear and circularly polarised magnetic flux, it doesn't discuss the significant differences that such fields might have on living beings. There is no mention of some very important studies on the action of Tamoxifen (a commonly used breast cancer treatment drug) and how this is sometimes inactivated at levels above 1.2 μT. Although this is a 'secondary' effect of the fields, it is a vitally important one as women patients being treated with this drug might respond very poorly if they lived in 50 Hz fields above this level.

Chapter 5, "Recent residential epi studies" could do with a whole article on its own. Suffice it to say that in their 'Conclusions' they admit: *"recent pooled studies ... indicate a relative risk of nearly 2 in those exposed to more than 0.4 μT compare to those exposed to less than 0.1 μT. This excess is unlikely to be due to chance."*

Chapter 6 is "Occupational Exposures". Here a key 1999 positive paper by Floderus et al is completely missing. Quite extraordinary - were the NRPB Secretariat really unaware of it? Even without it there is plenty of evidence of an association of both electric and magnetic fields exposure with, and increasing incidence of, many cancers, though the largest increases are for leukaemia and for brain cancer. Having listed pages of high relative risks/odds ratios some of which rise to over 10 fold, they then surprisingly conclude that no risk has been established with any confidence.

Chapter 7, "Conclusions" seems to weaken, excuse and dismiss most of the positive findings reported in the body of the Report.

Chapter 8, "Recommendations" discusses further work that could be justified. However it does not raise many of the important considerations that I have raised in this commentary. It does not conclude by suggesting that precautionary action is taken in any way. So what use is this report? Very little, in my opinion.

The way forward?
Disband AGNIR (and maybe the NRPB) and set up an independent group like the IEGMP under Sir William Stewart. This would be much better value for money and would be likely to provide much better 'advice'.

Even with its failings (partly due to the NRPB Secretariat not producing all the relevant papers, once again) he managed to come out, within a short time period, with a reasonably good report on Mobile phone technology that did outline the important issues and did go on to give appropriate precautionary advice. He should have his own Secretariat next time, though, so that he does get all the papers.

Good leadership can work wonders. The money saved should be set aside as a budget for this committee to award to appropriate institutions of excellence to fund unbiased research into the key areas where they identify need for further studies.