From: Martin Gledhill
To: Tim Hopley
Cc: Jo Dones
Subject: Chat with Emilie van Deventer
Date: Thursday, 29 January 2026 9:43:44 am

Hi Tim

I had a chat this morning with Emilie van Deventer (she suggested a call when I replied to her message about the forthcoming EMF meeting), who has looked after the WHO EMF Project for the past 15 or more years. Some of the points coming up:

* Lots of people leaving WHO and people retiring are not being replaced (no surprises there). One consequence is that as well as looking after WHO's radiation programme she now covers Chemicals as well. She will be retiring in late October.

* A lot of work is going on at WHO to make sure that there is no duplication of effort. A resolution for presentation at May's World Health Assembly asks WHO to do a gap analysis as to what is being covered. This includes mention of the radiation and health area. There is a meeting next week to decide whether this resolution will go forward for consideration at the WHA. Emilie commented that the NZ Health attache had been vocal on this and made comments on the resolution (but she didn't say what the comments were). As I said in my previous email, I think it is useful for NZ to have radiation, especially non-ionising radiation, covered at WHO. May be worth trying to find out a bit more about this, although time is short.

* She hopes that the EHC monograph on RF will be published before September. However there is still a lot of work to do, including peer review once a document is ready and WHO approval. She is after suggestions for peer reviewers if you have any - she is thinking along the lines of institutions more than individuals but I guess suggestions for either would be welcome. Any thoughts? It will be a big document.

* One agenda item at the forthcoming EMF meeting is discussion of the future of the EMF Project. Once the RF monograph is published it will have completed most of the initial objectives set out in 1995 (for a very optimistic 5 year time scale!!).

One continuing project (at least, I think it is continuing but I haven't heard much about it for a while) are the "Basic Safety Standards" (BSS) for non-ionising radiation (NIR). These are intended to mirror the IAEA's Basic Safety Standards for ionising Radiation (IR). Sally and I have long thought that this is not worth pursuing, because unlike the BSS-IR the NIR version would not recommend limits and largely be telling health agencies and governments to look after public and occupational health, without providing concrete guidance on how to do this and which is a government job anyway. We have always thought that the most useful thing WHO can do is identify areas of potential concern (eg cosmetic applications that use high exposures/doses of NIR) and then let governments decide what to do about them.

It would be good to have a continuing update of the existing publications - the EHC on low frequency fields is now 19 years old - but given the huge time and effort for the RF ENC I'm not sure that this is realistic.

The EMF project also covers optical radiation (light, UV) and there is perhaps more scope here for manageable tasks that can be completed.

* Emilie asked whether she should have a chat with you Tim, to introduce herself and the EMF Project. I said that I would put that to you - her email is xxxxxxxxxxx@xxx.xx if you would like to take up the offer. I suggested that it might also be worthwhile to talk to Richard Jaine as he would be able to influence decisions about funding and would ask you what you think.

Martin

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Martin Gledhill

EMF Services
P O Box 17
Clyde 9341

Tel: s9(2)(a)
Email: xxxxxx.xxxxxxxx@xxxxxxxxxxx.xx.xx
www.emfservices.co.nz
EMF Services is a division of Monitoring and Advisory Services NZ Ltd

Assessment of WHO-commissioned systematic reviews on health effects of RF-EMF

Background and Rationale

Between 2023 and 2025, a coordinated series of systematic reviews (SRs) examining the health effects of radiofrequency electromagnetic fields (RF-EMF; frequencies from 100 kHz to 300 GHz) were published in a special issue of "Environmental International" (https://www.sciencedirect.com/special-issue/1092DR596MG). Conducted by over 80 international scientists and accompanied by detailed protocols, the SRs provide a transparent evidence base to inform global health risk assessments and policy development. The SRs were commissioned and overseen by the World Health Organization (WHO) as part of its ongoing assessment of health risks from human exposure to electromagnetic fields (EMF). This work builds on the WHO's Environmental Health Criteria (EHC) Monograph series with the last comprehensive update on RF-EMF relevant to mobile communication systems published in 1993 (WHO EHC Monograph No. 137). Following the establishment of the "International EMF Project" (https://www.who.int/initiatives/the-international-emf-project) and the reintroduction of RF-EMF to the WHO's research agenda in 2010 (WHO research agenda for radiofrequency fields, 2010), this SR project was initiated to update the evidence base. Conducted in line with WHO's *Handbook for Guideline Development*, the SRs adhere to high standards of methodological rigour, transparency, and independence.

The approach to develop the scientific basis for the updated EHC Monograph and the development of these SRs followed a structured, three-step approach, designed to ensure comprehensive and focused evidence synthesis (Verbeek et al., 2025). The prioritisation of relevant health topics for assessment by SRs was informed by a survey conducted by the WHO in 2018, which polled over 300 invited researchers on their anticipated public concerns (Verbeek et al., 2021). The most relevant health-related topics for an evaluation by SRs were identified: RF-EMF experts rated cancer, heat-related effects, male fertility and reproductive outcomes, adverse birth outcomes, electromagnetic hypersensitivity, cognitive impairment, adverse pregnancy outcomes and oxidative stress as outcomes most critical regarding RF EMF exposure (Table 1). This prioritisation of health topics was the basis for the 10 SRs subjects published as an open call by the WHO. They comprise the assessment of human observational and experimental studies as well as in experimental animal and cell studies, for which international research teams were able to apply for conducting SRs on these topics. It resulted in the publication of twelve SRs, which are now available in Environment International's special edition, accompanied by an overview of strategic considerations, methodological procedures and findings by the organisers and editors (Verbeek et al., 2025). These SRs provide an up-to-date evidence synthesis on RF-EMF health effects, serving as the scientific foundation for WHO's ongoing risk assessment and the forthcoming EHC Monograph update.

Table 1: Overview of health subjects of the WHO-commissioned SRs

Topics of WHO survey	**WHO-commissioned SRs**	**SR in Environmental International**
Cancer	SR1 – Cancer (human observational studies)	Karipidis et al., 2024 Karipidis et al., 2025
	SR2 – Cancer (animal studies)	Mevissen et al., 2025
Adverse pregnancy and birth outcome	SR3 – Adverse reproductive outcomes (human observational studies)	Johnson et al., 2024 Kenny et al., 2024

	SR4 – Adverse reproductive outcomes (animal and in vitro studies)	Cordelli et al., 2023 Cordelli et al., 2024
Cognitive impairment	SR5 – Cognitive impairment (human observational studies)	Benke et al., 2024
	SR6 – Cognitive impairment (human experimental studies)	Pophof et al., 2024
Electromagnetic hypersensitivity	SR7 – Symptoms (human observational studies)	Röösli et al., 2024
	SR8 – Symptoms (human experimental studies)	Bosch-Capblanch et al., 2024
Oxidative stress	SR9 – Effect of exposure to RF on biomarkers of oxidative stress	Meyer et al., 2024
Heat-related effects	SR10 – Effect of exposure to heat from any source on pain, burns, cataract and heat-related illnesses	Commissioned but not completed

Summary, conclusions, and relevance for human health

The twelve SRs evaluated the scientific evidence of numerous endpoints related to human health topics of concern (Table 1). The number of studies included in each SR varied substantially, ranging from five studies on cognitive function in human observational research to 215 studies on fertility in animals. Carcinogenicity of RF-EMF exposure was addressed in two SRs on human observational studies (Karipidis et al., 2024, Karipidis et al., 2025), and in one SR in laboratory animals (Mevissen et al., 2025). Four SRs addressed the topic of fertility and reproduction, either evaluating human observational studies from the female (Johnson et al., 2024) or male (Kenny et al., 2024) perspective or experimental data on animals and *ex vivo* human sperm (Cordelli et al., 2023, Cordelli et al., 2024). The impact of RF-EMF exposure on human cognition was analysed in two SRs, focusing on observational (Benke et al., 2024) and experimental (Pophof et al., 2024) studies. Two SRs reported on subjective symptoms related to human well-being, based on observational (Röösli et al., 2024) and experimental (Bosch-Capblanch et al., 2024) human studies. In relation to cancer, and also associated to other health topics, oxidative stress markers were at last systematically evaluated for experimental *in vivo* and *in vitro* data (Meyer et al., 2024).

The main findings and conclusions of these SRs are summarised elsewhere (BAFU, BfS). In a nutshell, the SRs on the available human observational studies on cancer, cognition, reproduction and symptoms have not hinted towards potential adverse health impacts of exposure. For the majority of endpoints, however, the available data are limited, and the confidence in the evidence is generally low (Karipidis et al., 2024, Karipidis et al., 2025, Benke et al., 2024, Johnson et al., 2024, Kenny et al., 2024, Röösli et al., 2024). However, for tumours of the brain and head region associated with near-field exposure, the evidence was judged to provide moderate confidence in an absence of an effect. Thus, adverse health impacts of RF-EMFs are not readily discernible in epidemiological studies, reflecting real-life exposure of the general and working populations.

The conclusions of the SRs on observations from human experimental studies and animal studies were more ambivalent, which might be due to prolonged and higher levels of exposure than for the general population typically assessed in observational studies. RF-EMF exposure in humans did not impact volunteers' symptoms, such as headache and most cognitive parameters, with moderate or high confidence in the evidence (Bosch-Capblanch et al., 2024, Pophof et al., 2024). Nevertheless, there were some indications of effects on specific cognitive tasks, albeit with low or very low confidence in the evidence. Similarly, *ex vivo* exposure of human semen has been observed to result in an inconsistent negative impact on sperm quality, yet having assigned low confidence in the evidence (Cordelli et al., 2024). The results of animal experiments on reproduction revealed some evidence, assigned with moderate confidence, for an increased risk that male animals may fail to reproduce and for reduced birth weight following exposure of females. With regard to other reproductive parameters, including fertility and brain development of the offspring, no adverse effects were reported with high

or moderate confidence (Cordelli et al., 2023, Cordelli et al., 2024). However, for a considerable number of the analysed endpoints, the available data is limited, and the confidence in the evidence is low. This renders a firm conclusion about RF-EMF effects on animal reproduction challenging and hampers also the translation of the findings in animals to human reproduction, for which no adverse effects were put forward in observational studies on the general and working population (Johnson et al., 2024, Kenny et al., 2024).

The findings and conclusions of the cancer-related SRs differ between observational human and animal experimental studies, thereby leaving some uncertainties regarding the carcinogenic impact of RF-EMF. As demonstrated by studies conducted in laboratory animals, an elevated incidence of heart schwannomas and gliomas, in addition to tumours of other organs, has been documented. The evidence supporting these findings has been categorised as either high or moderate confidence, as outlined in the SR by Mevissen et al. (2025). It is important to note that these conclusions are based on the findings of two large chronic bioassays conducted by the NTP and the Ramazzini institute (see BERENIS special NL, November 2018). This topic requires further consideration to evaluate potential human health impact, taking into account the direct translation of animal cancer to humans and the utilisation of experimental RF-EMF doses commonly used in toxicological approaches. There is an absence of compelling evidence to suggest that animal models are not a rational basis for potential effects in humans. However, it is imperative to consider the nature of exposure (local or whole body), as well as the duration and intensity of exposure, when translating the effect sizes into cancer risk in humans. In this regard, a mechanistic understanding of the mode of action of RF-EMF, which is typically investigated in cell studies, would be advantageous in evaluating its impact on human health. Yet, a recent SR conducted independently from the WHO initiative pointed with moderate confidence towards no impact of RF-EMF exposure on genotoxicity, which is a well-established driver of mutagenesis and thereby cause of carcinogenesis (Romeo et al., 2024). Concerning carcinogenesis, the SR on oxidative stress markers in animal and cell studies provides evidence that is difficult to rely on. This is because it was mostly rated with low and very low confidence for both a trend towards oxidative stress, for instance in the blood, testis and thymus of rodents, and no consistent changes in other tissues and cell types (Meyer et al., 2024). However, it is important to acknowledge the potential limitations of the included studies, which may contribute to the observed low confidence in the evidence. These limitations include the presence of studies with numerous restrictions, as well as the extensive variety of experimental models and protocols. These studies were grouped and combined in accordance with the protocol of the SR and meta-analysis for experimental outcomes. Levels of oxidative stress markers are subject to alteration in a variety of pathologies as a consequence of response to external stimuli and the action of key cellular mechanisms. The conceptual foundation of the approach was oriented towards molecular damage in the context of cancer (DNA damage); however, it did not encompass considerations such as the experimental purpose, the persistence of oxidative stress, and the functional consequences even though the latter ones were assessed in several studies. It is imperative to acknowledge the significance of these points in determining the biological relevance. However, it is equally crucial to recognise the challenges associated with their incorporation into a SR.

Does the data and study quality allow for firm conclusions about health impact?

While the overall SR project demonstrated scientific quality and transparency, the limitations of the existing primary studies meant that the certainty of evidence for many key outcomes remained low or very low. The methodological quality and completeness of available data generally did not permit firm conclusions regarding the health impacts of RF-EMF exposure across most investigated endpoints. All

twelve SRs reported constraints related to either an insufficient number of studies or methodological weaknesses in existing research, both of which limit the strength of the conclusions that can be drawn. BERENIS concurs with the authors' observations that the frequent methodological limitations documented for the included studies, often involving failures across two or more quality criteria, challenge the certainty of the assessment and the formulation of firm conclusions regarding the potential health effects of RF-EMF exposure. Consequently, despite the existences of numerous studies on RF-EMF effects, many of them are found to be lacking scientific rigour. Such limitations are not only confined to the body of literature assessed in these SRs, but are a pervasive issue in research, particularly in the field of EMF-related topics, where the distinction between thermal and non-thermal effects, along with the establishment of appropriate exposure metrics, are paramount. The reliability of observational studies may be compromised by misclassification resulting from retrospective exposure assessment by proxies. Conversely, insufficiency in the study design and exposure characterisation is a prevalent limitation of experimental studies, resulting in downgrading of the confidence in the evidence (GRADE assessment).

Besides the limitations of the existing primary research, it is necessary to consider the constraints of the SR approach regarding the assessment of the certainty and quality of a body of evidence (GRADE assessment). For this SR project a harmonised approach was used to assess the level of evidence, to ensure that the methods were as similar as possible across the different SRs. In a 2016 publication in Environment International, experts in SR methodology asserted that while the GRADE framework offers many advantages for evidence assessment, it requires further refinement and methodological adaptions to be fully applicable in environmental research. Particularly for studies of cancer in animals, meta-analysis was deemed inappropriate primarily because of substantial methodological and biological heterogeneity between the studies, including differences in animal models (species, genetic modifications, diet, housing conditions), exposure characteristics (far- versus near-field, modulation), and key experimental parameters (onset, timing and duration of exposure, and type of exposure system). Hence, the SR methodology needs refinement to include the evaluation and integration of evidence from human, animal, *in vitro*, and *in silico* studies when determining whether an environmental factor represents a potential health risk. To date, the guidelines have not yet been adapted. However, there are proposals on changing the assessments in the "*Report on Carcinogens",* which is part of the National Toxicology Program. These proposals include incorporating sensitivity issues in the risk of bias evaluation.

The SRs also dealt differently with studies judged to suffer from biases and limitations when it came to meta-analyses, which were performed in eleven out of the twelve SRs. A meta-analysis is recommended when studies included in a SR address a similar question, use comparable interventions and outcomes, and provide sufficient data for meaningful statistical synthesis. However, as stated above, a meta-analysis should not be performed when there is substantial heterogeneity in study design, populations, or outcomes that cannot be satisfactorily explained, or when methodological differences and biases make summary estimates misleading. The quality and number of studies included in most of the SRs were limited, and the way in which biased studies were handled may have influenced the overall conclusions. This issue must be given due consideration in the forthcoming evaluation of the health implications for humans, which will be informed by the systematic collection and assessment of the current body of literature by these SRs.

BERENIS's overall evaluation of the extant evidence, as presented and analysed in the SRs, is that it is insufficient and too ambivalent to draw firm conclusions about human health impacts of RF-EMF. This is partly due to the SR methodology employed, which has been designed for clinical studies to assess the advantage of a new treatment. Conversely, although high-confidence findings in the SRs of human

observational studies are lacking, there is hardly any indication for substantial health impacts by RF-EMF exposure in healthy individuals, suggesting that the regulatory measures offer a precautionary level of protection.

Are there more vulnerable people?

The current scientific data available does not allow for the drawing conclusions about the existence of more sensitive and vulnerable individuals when compared to the general population. To date, the analysis of observational and experimental data from electromagnetic hypersensitive and multiple sclerosis patients did not advance our understanding (Bosch-Capblanch et al., 2024, Röösli et al., 2024). Observational studies generally include data from the entire population. It is therefore unlikely that a small, highly vulnerable subpopulation would have a significant impact on the overall public health, especially if the genetic, physiological or disease-based predispositions are not identified. *Vice versa*, an effect in this subpopulation may have been masked, using this study design. SRs on observational human studies did predominantly not allow for stratification by vulnerability or demography (Karipidis et al., 2024, Kenny et al., 2024, Karipidis et al., 2025), while the SR on cognition included predominantly studies on children and only one study on elderly people (Benke et al., 2024). Similarly, human experimental studies on cognition mostly assessed children and adolescents, who are widely regarded as more vulnerable. Notably, only a single study on elderly people has been described (Pophof et al., 2024). Thus, it is challenging to draw conclusions about the vulnerability of subgroups, and there is a particular lack of data for older individuals. It is important to note that the insights derived from human experimental studies often lack generalisability, as these studies typically involve healthy and young volunteers.

In toxicology, developing organisms, such as foetuses and children, are recognised as being particularly vulnerable. In this context, the assessment of birth outcomes in human observational (Johnson et al., 2024) and experimental animal (Cordelli et al., 2023) studies is meaningful. A number of studies have been conducted on the effects of RF-EMF exposure on the general public and in the occupational settings. These studies have not identified any impact on foetal development, birth weight or premature birth. In contrast, there is evidence with moderate confidence for a reduced birth weight in laboratory mammals and with low confidence for an impact on the development of embryos and the neural system of the offspring. Yet, it remains unclear whether the RF-EMF exposure directly affects foetal development or acts indirectly through maternal changes. At this time, the possibility cannot be discounted that RF-EMF exposure has an impact on foetuses. Consequently, pregnant females and their unborn offspring should be considered a potentially vulnerable group.

Knowledge gaps and directions of future research

The authors of each SR provided commentary on the implications of their results and conclusions for future research. Overall, there was a common demand that more research is needed, especially studies of higher quality. BERENIS concurs with these statements, which concern not only the health topics addressed in these WHO-SRs but are generally observed in the body of literature on EMF-related research, being a main cause for the existing uncertainties. Although this series of SRs supports the evaluation of potential human health impacts on cancer, reproduction, cognition and well-being, many health topics remain insufficiently explored and require further research on both the experimental level and evidence evaluation by SRs. For instance, there is some, but hardly compelling evidence, for effects on the cardiovascular and immune system as well as for influences on the metabolism and

neurodegenerative processes. However, there is a need for adaption and refinement of the approach for future SRs on health-related EMF effects. This is crucial for the meaningful assessments of potential environmental health impacts, especially when including cell and animal studies but also human studies lacking homogeneity. The combination of data in a meta-analysis without consideration of homogeneity is questionable; nevertheless, this practice has been adopted by most of SRs, despite the possibility of a narrative synthesis approach in accordance with the OHAT SR methodology. It is evident that SRs serve as a potent instrument for evaluating the body of evidence. Yet, it is essential to recognize the fundamental principles and the knowledge of toxicology, which remain highly relevant and should not be overlooked.

BERENIS emphasised that for the majority of research and health topics, there is a necessity for well-conducted mechanistic investigations to facilitate a more profound comprehension of the impact of RF-EMF exposure on the molecular level. This is ultimately required to achieve a comprehensive understanding of the potential impact on human health. This also holds true for the health topic "cancer", for which the conclusions of the WHO-SRs differ between human observational and animal studies (Karipidis et al., 2024, Karipidis et al., 2025, Mevissen et al., 2025). This discrepancy necessitates heightened scrutiny and further research to elucidate the underlying implications. However, in order to advance our understanding, it is essential that these investigations are conducted in a way that minimises study limitations and biases, thus avoiding further uncertainties and concerns as raised in the WHO-SRs. Apart from mechanistic and toxicological investigations that address both thermal and non-thermal influences, as well as current and forthcoming technologies, the health impacts of RF-EMF exposure need to be continuously monitored by observational studies on cancer but also other health topics. The primary focus should be on the conduct of prospective studies of long-term exposure of the general public, as well as the working population. These studies should involve improved exposure assessments, as recommended in several SRs (Johnson et al., 2024, Karipidis et al., 2024, Kenny et al., 2024, Röösli et al., 2024, Karipidis et al., 2025).

BERENIS recommendations for precautionary and regulatory measures

As previously stated in special newsletters, BERENIS underpins the importance of the precautionary principle, as specified in Switzerland by the "installation limit value" for emissions from stationary transmitters (e.g., mobile phone base stations and radio transmitters) in the Ordinance on Protection against Non-Ionising Radiation (NISV). Despite the tremendous effort expended on the WHO-commissioned SRs, it remains impossible to draw definitive conclusions on potential health effects of RF-EMF exposure. For a considerable proportion of the assessed endpoints, the confidence in the supporting evidence was mainly categorised as low or very low. This conclusion was drawn in particular for health impacts investigated by the SR on human experimental and observational studies. The authors posit that the prevailing low confidence in the accumulated scientific evidence is attributable to a combination of different factors. These include the paucity and/or inconsistency of findings, as well as the lack of reliable studies with few limitations and potential risk of biases. This notion is in line with the evaluations and judgement set out by BERENIS, overseeing the scientific literature of the last decade. Consequently, within the established regulatory limits, it proved impossible to definitively ascertain putative health impacts of RF-EMF exposure with high confidence. Nevertheless, a systematic collection and evaluation of the body of literature is an important step towards the health risk assessment of RF-EMF. It is evident that the multitude of biological and experimental disparities appears to be incongruent with the methodological constraints of the highly standardised SR approach, which has been designed for evaluation of clinical studies. It is a common occurrence, particularly in the context of experimental studies, that meta-analyses exhibit a paucity of biological or statistical

rationale. A wide variety of factors are combined including, but not limited to, the combination of different animal species, different strains, different sexes, different experimental models, studies targeting specific organs or cells with toxicity studies, different exposures such as pulsed with continuous fields, different frequencies, intensities and exposure duration, different study length and endpoints, different statistical analysis methods, and many more. In addition, experimental groups within studies were treated as independent when in fact there is a dependency based on a common control group, violating a fundamental assumption of the meta-analysis methods.

In respect to precautionary and regulatory measures, it also important to note that the WHO-SRs only cover a selection of previously proposed potential biological effects regarding health impacts. In addition, pooled analyses of a broad frequency range were performed in some instances, especially when occupational exposure was involved. However, the included data is dominated by RF-EMF exposure in the frequency range of 0.8-2.5 GHz, which is related to older mobile communication standards. Whether the conclusions of the SRs are extendable on the forthcoming higher frequency bands of future mobile communication standards remains speculative and poorly explored (see BERENIS special NL, May 2025). There is also a knowledge gap when it comes to potential combinatory effects with other environmental factors as well as genetic or physiological pre-conditions, which may not be readily discernible in observational and experimental studies. Despite the absence of a definitive mechanistic concept for RF-EMF impact, the potential for this to be a contributing factor within a vulnerability-stress model is conceivable. This model delineates the manner in which genetic, biological, and environmental factors interact and influence the risk and extent of stress reactions. The ability to cope with stress is determined by a combination of innate and acquired vulnerabilities, in addition to further stressful events. Consequently, in certain individuals, the most trivial overload or negligible pressure to perform can induce symptoms, while in others, it remains asymptomatic.

It is worthy to note that the authors of the WHO-commissioned SRs were encouraged to discuss the implications of their findings for practice and policy. It has been asserted by some authors that there is no need to adjust the regulatory guidelines, on the grounds that the evidence is too uncertain for informed decisions to be made at the regulatory level. Furthermore, the limitations of translating the findings from animal and cell studies to humans were emphasised. Overall, BERENIS can relate to these appraisals and recommends the consequent application of the precautionary principle and current guidelines.

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Note: All WHO-commissioned SRs, as well as the respective protocols and additional articles, can be found in the corresponding special edition of *Environment International*: https://www.sciencedirect.com/special-issue/1092DR596MG

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Contact

Dr Stefan Dongus
BERENIS Secretariat
Swiss Tropical and Public Health Institute
Department of Epidemiology and Public Health
Environmental Exposures and Health Unit
Kreuzstrasse 2, CH-4123 Allschwil, Switzerland
Tel: s9(2)(a)
Email: stefan.dongus@swisstph.ch

Additional information:

BERENIS - Swiss expert group on electromagnetic fields and non-ionising radiation

BERENIS newsletter search tool

List of abbreviations (pdf)

From:	Martin Gledhill
To:	Jo Dones; Tim Hopley
Subject:	Public Health data limitations
Date:	Friday, 9 January 2026 2:40:14 pm
Attachments:	Newsletter BERENIS Special Issue - December 2025.pdf

Hi Jo, Tim

Happy New Year, hope you both had a good break.

I have been having one of my periodic trawls through the EMF research in preparation for the next interagency meeting, and have had a look at a recent Swiss commentary on the WHO systematic reviews of the RF literature that have been published over the past couple of years and will form the basis for the WHO review of the RF research.

The Swiss commentary was prepared by "BERENIS", a consultative group of Swiss experts from various disciplines with scientific competence regarding electromagnetic fields nominated by the Swiss Federal Office for the Environment. I have attached the commentary and you will see that it highlights the poor quality of much of the research data (a factor noted by the authors of the systematic reviews - and many others.) They say:

"BERENIS's overall evaluation of the extant evidence, as presented and analysed in the SRs, is that it is insufficient and too ambivalent to draw firm conclusions about human health impacts of RF-EMF. This is partly due to the SR methodology employed, which has been designed for clinical studies to assess the advantage of a new treatment. Conversely, although high-confidence findings in the SRs of human observational studies are lacking, there is hardly any indication for substantial health impacts by RF-EMF exposure in healthy individuals, suggesting that the regulatory measures offer a precautionary level of protection."

Elsewhere they say:

"While the overall SR project demonstrated scientific quality and transparency, the limitations of the existing primary studies meant that the certainty of evidence for many key outcomes remained low or very low. The methodological quality and completeness of available data generally did not permit firm conclusions regarding the health impacts of RF-EMF exposure across most investigated endpoints. All twelve SRs reported constraints related to either an insufficient number of studies or methodological weaknesses in existing research, both of which limit the strength of the conclusions that can be drawn. BERENIS concurs with the authors' observations that the frequent methodological limitations documented for the included studies, often involving failures across two or more quality criteria, challenge the certainty of the assessment and the formulation of firm conclusions regarding the potential health effects of RF-EMF exposure. Consequently, despite the existence of numerous studies on RF-EMF effects, many of them are found to be lacking scientific rigour. Such limitations are not only confined to the body of literature assessed in these SRs, but are a pervasive issue in research, particularly in the field of EMF-related topics ..."

I don't think that there is anything new here, BERENIS are not the first people to make these types of observations (but I do find it depressing that after more than 40 years of research in this area people have still not managed to get it right. On the other hand, I think that some of the BERENIS comments are a little exaggerated.). However, I think we also have to note their comment that techniques developed for the evaluation of well controlled clinical trials may not be the most appropriate for evaluating environmental agents, but at the moment they are all we have.

I think that it always helps to have a bit of context when reading this kind of stuff, so you can understand whether EMF research is unique in this respect or whether other areas of environmental health suffer similar limitations in the quality of the data available, but nevertheless we still have to make a call on what sort of controls might be necessary to protect public health. For that reason I was wondering whether one or other of you, or maybe someone else, might be able to give a brief presentation at the February Interagency meeting to provide that context of the wider field of public health. Do you think that would be possible?

Thanks

Martin

--
Martin Gledhill
EMF Services
P O Box 17
Clyde 9341

Tel: s9(2)(a)
Email: xxxxxx.xxxxxxxx@xxxxxxxxxxx.xx.xx
www.emfservices.co.nz
EMF Services is a division of Monitoring and Advisory Services NZ Ltd

From: VAN DEVENTER, Tahera Emilie
To: Tim Hopley
Subject: RE: [EXT] EMF project update
Date: Monday, 2 February 2026 7:10:02 pm
Attachments: image001.png

Dear Tim,

Thanks for reaching out. I would be pleased to have a call with you next week. I am based at WHO headquarters (Geneva, Switzerland), which is 12 hours behind NZ time. Would a 8:30am slot (NZ time) anytime next week work for you?

Kind regards,
Emilie

Dr VAN DEVENTER Emilie
Unit Head (Chemicals, Radiation and Health)

Department of Environment, Climate Change, One Health and Migration
Division of Health Promotion, Disease Prevention & Care

World Health Organization

HQ/PPC/ECO/CRH
Geneva
Switzerland

Office: s9(2)(a)

From: Tim Hopley <.@..>
Sent: Monday, February 2, 2026 5:15 AM
To: VAN DEVENTER, Tahera Emilie <@.>
Subject: [EXT] EMF project update

Hello Emilie,

I hope you are well?

Martin Gledhill reached out and suggested we have a catchup regarding the EMF project. My time is limited this week as I'm taking some leave. But would you have some time available next week for a catchup? I'm not to sure where you are based but I'm happy to hop online in the evening or early morning NZ time to accommodate the time difference.

Look forward to hearing from you.

Noho ora mai

Tim Hopley
Ngāti Kahungunu

Manager – Environmental Health
National Public Health Service, Health New Zealand - Te Whatu Ora
waea pūkoro: s9(2)(a) **| īmēra:** xx@xx

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MINISTRY OF HEALTH
MANATŪ HAUORA

133 Molesworth St
P.O. Box 5013
Wellington
New Zealand
Phone (04) 496 2000
Fax (04) 496 2340

15 January 2001

Ref. No PH20-19-1

Dear Colleague

Institute of Environmental Science Ltd (ESR)'s *Review of the Report 'Criticism Of The Proposal To Adopt The ICNIRP Guidelines For Cellsites In New Zealand'*

As you may be aware, in March 1998 the Government directed the Ministry for the Environment, in partnership with the Ministry of Health, to draft *National Guidelines for Managing the Health Effects of Radiofrequency Transmissions* (the Guidelines).

In July 1999, *Towards national guidelines for managing the effects of radiofrequency transmitters: A discussion document* was released for public consultation. The drafting of the discussion document included consultation with community groups in Auckland, Wellington and Christchurch and with industry groups, as well as a formal peer review process. Dr Neil Cherry was one of the peer reviewers.

Environment and Health officials met with community representatives in Auckland and Christchurch prior to the public release of the discussion document. At the Christchurch meeting, Dr Cherry, supported by the community members present at the meeting, requested that officials advise why his comments were not considered appropriate and valid, and that a review of his report be undertaken.

As a result, the Ministry of Health commissioned an independent review of Dr Cherry's report *'Criticism of the Proposal to Adopt the ICNIRP Guidelines for Cellsites in New Zealand'* by the Institute of Environmental Science Ltd (ESR).

A copy of the *Review of the Report 'Criticism Of The Proposal To Adopt The ICNIRP Guidelines For Cellsites In New Zealand'* is enclosed for your information. The review focused on Dr Cherry's assessment of cancer epidemiology studies and their implications. Dr Cherry's evaluations of nine cancer epidemiology studies concerning possible exposure to radiofrequency fields were critically examined.

The review found that Dr Cherry's critique suffered from a number of problems. Most seriously, there was an apparently limited awareness of the potential for bias that rendered most of the re-analyses and reinterpretations of studies invalid, or at least highly suspect.

It was not possible for the reviewer to pass an authoritative judgement on the other, non-epidemiological, aspects of Dr Cherry's critique. However, Dr Cherry's main basis for recommending a much lower level of exposure to radiofrequency fields than did ICNIRP was his reinterpretation of the epidemiology studies. The reviewer felt that, generally speaking, the other material in the Cherry critique seemed to be present mainly to buttress the conclusions derived from the re-evaluation of the epidemiology studies.

The reviewer concluded that he could not recommend that Dr Cherry's critique of the ICNIRP guidelines be accorded weight in determining the final shape of the Guidelines.

For further information on radiofrequency fields, including a copy of the *National Guidelines for Managing the Health Effects of Radiofrequency Transmissions* (the Guidelines), please visit the Ministry's website at www.moh.govt.nz. The World Health Organization also has information on electromagnetic fields, including radiofrequency fields and information on the international project on electromagnetic fields at www.who.int/emf.

Yours sincerely

Dr Don Matheson
Deputy Director-General
Public Health Directorate

ESR

FW: 0088

REVIEW OF THE REPORT "CRITICISM OF THE PROPOSAL TO ADOPT THE ICNIRP GUIDELINES FOR CELLSITES IN NEW ZEALAND" BY DR NEIL CHERRY

Michael Bates
Project Leader

Dominique Noiton
Programme Manager

Julia Carr
Peer Review

Jeff Fowles
Peer Review

Institute of Environmental Science & Research Limited
Kenepuru Science Centre
Kenepuru Drive, PO Box 50-348, Porirua, New Zealand
Telephone: (64-4) 237-0149, Facsimile: (64-4) 237-2370

A CROWN RESEARCH INSTITUTE

REVIEW OF THE REPORT "CRITICISM OF THE PROPOSAL TO ADOPT THE ICNIRP GUIDELINES FOR CELLSITES IN NEW ZEALAND" BY DR NEIL CHERRY

Prepared as part of a Ministry of Health contract for scientific services (Project PH1a)

by

Michael Bates, Ph.D.
Epidemiology Group
ESR Kenepuru Science Centre
PO Box 50-348
Porirua
Wellington

July 2000

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Contents

Summary

This report provides a critical review of the undated document produced by Dr Neil Cherry, titled "Criticism of the proposal to adopt the ICNIRP Guidelines for cellsites in New Zealand" [No. DR 98627PuCo- 039]. Dr Cherry has been a major critic of the guidelines for radio-frequency (RF) radiation exposure limits produced by ICNIRP (International Commission on Non-Ionising Radiation Protection) and believes that equivalent New Zealand guidelines should be set at a much lower level than ICNIRP proposes. This review focused on Dr Cherry's assessment of cancer epidemiology studies and their implications. Dr Cherry's evaluations of nine cancer epidemiology studies concerning possible exposure to RF radiation were critically examined.

The review found that Dr Cherry's critique suffered from a number of problems. Most seriously there was an apparently limited awareness of the potential for bias (confounding, selection bias, and information bias) that rendered most of Cherry's re-analyses and reinterpretations of studies invalid, or, at least, highly suspect.

It was not possible for the reviewer to pass an authoritative judgement on the other, non-epidemiological, aspects of Dr Cherry's critique. However, Dr Cherry's main basis for recommending a much lower level of exposure to RF radiation than did ICNIRP was his reinterpretation of the epidemiology studies. Generally speaking, the other material in the Cherry critique seemed to be presented mainly to buttress the conclusions derived from Dr Cherry's re-evaluation of the epidemiology studies.

The reviewer concluded that he could not recommend that Dr Cherry's critique of the ICNIRP Guidelines be accorded weight in determining the final shape of the New Zealand guidelines for the siting of radiofrequency transmission sites.

Introduction

This report is intended to provide a critical review of the undated document produced by Dr Neil Cherry of Lincoln University, titled "Criticism of the proposal to adopt the ICNIRP Guidelines for cellsites in New Zealand" [No. DR 98627PuCo- 039]. Dr Cherry's critique was submitted to the Ministry for the Environment in early 1999 in response to a request from the Ministry for comment on the publication jointly from the Ministries of Health and Environment, titled "Towards National Guidelines for Managing the Effects of Radiofrequency Transmitters". Amongst other things, the latter document proposed that New Zealand adopt the guidelines proposed by the International Commission on Non-Ionizing Radiation Protection (ICNIRP) as a basis for the control of exposures to radiofrequency (RF) radiation.

Dr Cherry has been a major critic of the ICNIRP Guidelines and believes that guidelines or standards for non-ionising radiation should be set at a much lower level than ICNIRP proposes. At a meeting in Christchurch in August 1999 with the team that developed the New Zealand proposal for national guidelines[1] Dr Cherry invited a critical appraisal of his views. It is partly in response to that invitation that this document has been prepared.

Dr Cherry's complete document is very extensive and cites a wide range of studies. In considering how most appropriately to take into account Dr Cherry's views, the key initial issue for the team preparing the draft New Zealand Guidelines has been to establish the scientific quality of Dr Cherry's critique. It is on this aspect that the present review is focused. For purely practical reasons this review did not seek to examine the entirety of the Cherry critique, but focused mainly on the sections dealing with epidemiologic studies of cancer in relation to RF radiation exposure. The main reasons for focusing on Cherry's evaluation of such studies are (1) the question of whether exposure to RF radiation could cause cancer is one of the most important issues concerning such radiation; and (2) criticism of the ICNIRP evaluation of the available cancer epidemiology studies forms the major part of Dr Cherry's review. Dr

[1] The author of this review is a technical advisor to the joint Ministry of Health/ Ministry for the Environment team developing the New Zealand Guidelines.

Cherry has quoted and re-evaluated some of these studies in considerable detail. It follows that if the Cherry critique is worthy of detailed consideration, then the most substantial part of the critique should be of high scientific quality and stand close scrutiny. If the cancer epidemiology section of the critique were found generally to be robust then it would be appropriate to consider the Cherry critique in its entirety, with a mind to seriously reconsidering whether the ICNIRP guidelines would be appropriate as a basis for the proposed New Zealand Guidelines.

It needs to be clearly understood that the present review is not intended to be a critique of the ICNIRP guidelines, nor is it intended to be a comprehensive evaluation of whether there are adverse effects associated with low level exposure to RF or other non-ionising radiation. Both these tasks would require very much more extensive efforts than could be accommodated by the resources available for this review, and would, to a large extent, duplicate other more recent work, in particular, the recent review carried out in the UK by the Independent Expert Group on Mobile Phones (IEGMP, 2000).[2] Instead, the present review is solely focused on evaluation of the scientific quality of Dr Cherry's critique. As such, this review does not deal with Dr Cherry's interpretation of parts of the Resource Management Act.

Method

Pages 35 to 78 of Dr Cherry's critique discuss and, to a large extent, reinterpret key epidemiological studies that have examined the possibility of an association between cancer and RF radiation. These are studies that Cherry considers important to the issue, and in some cases are studies where he has substantial disagreement with the conclusions of the authors, or of ICNIRP's evaluation. The focus of this review of the scientific quality of Dr Cherry's critique is how he has interpreted, and in some cases reanalysed, these studies.

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[2] The IEGMP includes members expert in epidemiology, experimental biology related to electromagnetic fields and radiofrequencies, social science, risk perception and legal issues. Among the 12 members of the IEGMP are two of Britain's most experienced epidemiologists, Professor David Coggon and Professor Anthony Swerdlow, and also one of the world's leading statisticians, Professor Sir David Cox.

The studies considered (in the order in which they appear in the Cherry critique) are:

- Robinette et al. (1980) Effects upon health of occupational exposure to microwave radiation (radar)

- Lilienfeld et al. (1978) Foreign Service Health Status Study – evaluation of health status of foreign service employees from selected eastern European posts.

- Selvin et al. (1992). Distance and risk measures for the analysis of spatial data: a study of childhood cancers.

- Beall et al. (1996) Brain tumors among electronics industry workers.

- Grayson (1996). Radiation exposure, socio-economic status and brain tumour risks in the US Air Force: A nested case-control study.

- Szmgielski (1996). Cancer morbidity in subjects occupationally exposed to high frequency (radiofrequency and microwave) electromagnetic radiation.

- Hocking et al. (1996). Cancer incidence and mortality and proximity to TV towers.

- Dolk et al. (1997) Cancer incidence near radio and television transmitters in Great Britain. I. Sutton Coldfield Transmitter; II. All high power transmitters.

There are several other cancer-related studies cited by Dr Cherry. However, this review has not included them, for the following reasons:

- Barron and Barraff (1958). Medical considerations of exposure to microwaves (radar).

This is an old study, of limited value, and everyone, including ICNIRP and Dr Cherry, appears to be agreed that it has little contribution to make to resolving questions of whether RF radiation is carcinogenic or not.

- Rothman et al. (1996). Assessment of cellular telephone and other radiofrequency exposure for epidemiologic research.

- Rothman et al. (1996). Overall mortality of cellular telephone customers.

Again, there is general agreement that these publications are not particularly useful in contributing to the debate, because the length of follow-up of cellular phone users in the study was fairly short.

Complete citations for the above-listed studies are provided in the Reference section at the end of this report.

For the purposes of this review, copies of each of the studies referred to above were obtained and compared directly with the corresponding comment in the Cherry critique. For each study, a similar approach was used: First the study is briefly summarised, Dr Cherry's criticisms or comments are outlined, and then a substantiated opinion on the appropriateness of Dr Cherry's views or re-analysis is set out. This study-by-study analysis is followed by a discussion on the appropriateness of the critical methodology applied in the Cherry critique.

Results

Robinette et al. (1980)

Effects upon health of occupational exposure to microwave radiation (radar)

Outline of study

This is a retrospective cohort study of about 40,000 US Navy personnel, who served during the Korean War, half of whom were considered to be highly exposed to microwave radiation (radar), and the other half less exposed. High exposure groups were electronics technicians (ET), fire control technicians (FT), and aviation electronics technicians (AT); low exposure groups were radiomen (RM), radarmen (RD), and aviation electricians mates (AE). The study report considered that there were no adverse effects (as indicated by death, hospitalization, or disability pension award) that could be attributed to potential microwave exposures during the period 1950-54.

Dr Cherry's criticisms

Dr Cherry considers that there has been exposure misclassification, such that one low exposure group (AE) should actually be classified as high exposure. He argues that the AE group has high rates of deaths and malignancies in line with the rates in the FT and AT groups. On the basis of the argument that there has been exposure misclassification in the low exposure group, any distinction between the high and low exposure groups would be attenuated. Cherry, therefore, largely dismisses the comparison between the high and low exposure groups in favour of a comparison within the high exposure group, between the FT and AT groups combined (FT+AT) and the ET group. His justification for this is the results of a "hazard number" assessment by Robinette et al., which suggests that the ET group is relatively less exposed than the FT and AT groups. On that basis, Cherry calculates risk ratios for the (FT+AT) versus ET groups, showing a number of instances when there are statistically significant risk ratios. He concludes that "the original data shows (sic) a significant increase in morbidity and mortality incidence for the high exposure group"

[Cherry: page 41] and that the original authors are wrong when they conclude that the study showed no adverse effects that could be attributed to potential microwave radiation exposures.

Finally, Cherry [page 41] makes a comparison between cancer mortality rates in this study and New Zealand male population (ages 25-49) cancer mortality rates, and concludes that all the groups in the study (who would all have had more exposure to radar signals than the general NZ male population) had higher mortality rates than New Zealand males.

Comment on the Cherry critique

A major plank in Dr Cherry's argument is that the AE group has been misclassified in terms of exposure, and, therefore, inclusion of the AE group in the low exposure group is sufficient to invalidate that group for comparison purposes. It is, therefore, important to examine that argument closely. Cherry puts forward two grounds for his viewpoint:

- An opinion that AE "work with aircraft electronics technicians in repairing equipment on planes and spend a great deal of time on the flight deck exposed to radar, and exposed to radar while it is tested under repair" [Cherry: page 37]. Cherry argues that "the two aviation technician groups AE and AT are linked by Robinette et al. "through their common high incidence of aircraft accidents" [Cherry: page 37].

- The AE group has rates of deaths and malignancies in line with the highly exposed FT and AT groups.

Robinette et al. explain in their paper that the basis for classification of occupational groups as high or low exposure was actual measurements on ships by the US Navy and a consensus by Navy personnel involved in training and operations. On the face of it, this seems like a reasonable approach to exposure classification, and it is not clear why Cherry's contrary opinion should carry more weight than the opinion of

those actually in the US Navy, familiar with operations, and having access to actual measurements of exposure. Certainly, the high rates of deaths in aircraft accidents of both AE and AT personnel is not a basis for concluding similarity of exposure to microwave radiation.

Dr Cherry's argument that there is a high rate of deaths in the AE group, more akin to the mortality rate in the high exposure AT and FT groups than the rate in other low exposure groups is apparently based on crude death rates calculated from data in Table 5 of Robinette. This calculation ignores the fact that the AE group is older on average than the RM and the RD groups also in the low exposure category (see Table 4 of Robinette). Older groups would be likely to have higher death rates on the basis of age alone. To do an appropriate comparison would require a more complex calculation, taking into account the age distribution of the populations being compared and the person-time of follow-up. Such a calculation is not possible from the data in the paper. In any case, it is inappropriate to work back from disease or mortality rates to argue that this confirms that a group belongs in a particular exposure category.

It is instructive to consider the consequences if Cherry were right – that the AE group had been incorrectly classified as low exposure and really should have been in the high exposure group. The AE group comprises only 6.8% of the low exposure group. Therefore, even if it had been incorrectly classified it is unlikely that it could have more than a minor impact on the comparison between the high and low exposure groups. Certainly, it is unlikely that use of the (probably relatively highly exposed) ET group would make a more appropriate comparison group than a low exposure group with a small misclassified AE group within it.

Cherry carried out a considerable number of comparisons of the (AT+FT) group versus the ET group. It is usually not clear from his text how he did these comparisons. However, an attempt by the reviewer to recalculate some of the figures adduced by Cherry confirmed that they had often (and probably in all cases), been produced first by calculating crude rates (number of deaths or hospitalizations divided by the total number of subjects in the group). Risk ratios were then calculated by dividing the rate for (AT+FT) by the rate for ET. This approach is inappropriate and

likely to lead to misleading results. It is necessary to take into account the differing age structures and person-times of follow-up of the two groups, or the comparison (and associated calculation of confidence intervals) will not be valid.

Cherry makes much of a comparison of data abstracted from Table 11 of Robinette et al., showing that for a wide range of conditions (FT+AT) have statistically significantly higher rates of hospitalisations than the ET group. However he ignores the fact that if (FT+AT) is compared directly with the low exposure group (RM, RD and AE) then it is the low exposure group that has statistically significantly higher rates of hospitalisations. It is inconceivable that the small numbers in the (allegedly misclassified) AE group could have caused such a result.

Finally, Cherry's direct comparison of the US Navy personnel all-cause cancer mortality rate with the corresponding New Zealand population rate is inappropriate and uninterpretable. The data for the two groups would have been collected under totally different circumstances. For a number of reasons, mortality rates, particularly for all cancers combined, vary markedly across time and place, and it is always inappropriate to compare two groups from different eras and base populations, and to draw conclusions in this way.

Overall, in the opinion of the author of this review, Cherry's critique of the Robinette study suffers from several serious methodological problems. These mean that little weight should be given to the reanalysis that Cherry has carried out. These problems include an unsubstantiated re-assessment of exposures, and an inappropriate and selective use of comparison groups.

Lilienfeld et al. (1978)

Evaluation of health status of foreign service employees from selected eastern European posts.

Outline of the study

In the early 1960s the Soviet Union directed focused microwave beams at the United States Embassy in Moscow, presumably in an attempt to penetrate embassy security.

The Lilienfeld study was an attempt to investigate whether there were effects induced by the microwave transmissions, either on embassy staff or their dependants living in Moscow. A comparison group, comprising staff and their dependants from embassies in other Eastern European countries, was used. After extensive investigations, the report concluded "No convincing evidence was discovered that would directly implicate the exposure to microwave radiation experienced by the employees at the Moscow embassy in the causation of any adverse health effects as of the time of this analysis".

Dr Cherry's criticisms

Cherry has carried out his own assessment based on the data in the report and concluded [page 42]: "the data presented in the Lilienfeld contract report is (sic) contrary to that stated in the report's conclusions.

"The Lilienfeld data shows (sic) a significant increase in

- Neurological symptoms
- Blood cell counts
- Chromosome aberrations, and
- Cancer in children and adults."

Comment on the Cherry critique

This comment will focus on Dr Cherry's re-evaluation of the data on neurological symptoms and cancer. His analysis of the information on blood cell counts and chromosome aberrations involved access to other publications, which were not available to this reviewer at the time of carrying out this review.

1. Neurological symptoms

Cherry [page 44] partially reproduces Table 6.31 of Lilienfeld et al. (1978). The original table shows the number and percent of neurological symptoms (e.g., depresssion, sleepiness, anxiety, insomnia, etc.) ever present, and rates of occurrence

after first tours of duty, for male employees in the Moscow and comparison groups. Cherry has selected out those symptoms that occur more frequently in the Moscow employees than in the comparison group employees, and presented relative risks and *p*-values. On the face of it, the rates of symptoms appear quite a lot higher in the Moscow employees than in the comparison group and four of the comparisons are quite statistically significant. What Cherry has not mentioned is that on page 156 of Lilienfeld et al. (1978), it is stated that the rates of these four symptoms were higher in Moscow embassy employees judged not to have been exposed to the microwave radiation than in employees judged to have been exposed to the radiation. Therefore, it seems unlikely that the symptom rates would have been related to the microwave radiation.

2. Cancer rates

Cherry has extracted data from three tables of the Lilienfeld report and combined them in a table [Cherry: page 45]. He concludes that "adult foreign service workers and their spouses showed marked increases in a number of cancers compared with the number expected for the same age-adjusted population". Cherry's reconstructed table shows standardised mortality ratios (SMRs) exceeding 1.0 for all cancers, adult leukaemias, female genital cancers, adult brain tumours, female breast cancers, and childhood leukaemias. According to Cherry's analysis, the latter three categories have SMRs that are statistically significant. Cherry's table is reproduced in its entirety below.

In the opinion of this reviewer, in constructing this table Cherry has made a basic error that renders his analysis invalid. The table has been constructed by combining data from Lilienfeld Table 5.6 (male and female embassy employees combined); Table 7.12 (adult dependants of embassy employees), and Table 7.16 (dependent children of embassy employees). In Tables 7.12 and 7.16, separate data are presented for employee's dependants who lived in the embassy building and those who did not live there.

Table 1. Table from page 45 of the Cherry critique

Table: Cancer mortality rates for employees and dependents (sic) at the U.S. Embassy in Moscow				
Symptom	Moscow	Expected	SMR	p-value
All cancer	33	24.83	1.33	N.S.
Adult leukaemia	2	0.8	2.5	NS
Genital cancer, female	4	0.8	5.0	
Adult brain tumour	2	0.10	20.0	< 0.05
Female breast cancer	2	0.50	4.0	< 0.05
Childhood leukaemia	4	1.33	3.0	< 0.05

The way Cherry has combined the data can be illustrated by looking at the table row for all cancers. The number of observed cases has been summed by adding 17 (Table 5.6), 12 (Table 7.12), and 4 (Table 7.16) to give 33. Similarly, the expected number of all cancers has been obtained by adding 19 (Table 5.6), 4.5 (Table 7.12), and 1.3 (Table 7.16) to give 24.8. Division of the observed by the expected gives 1.33, the SMR.

The error Cherry has made becomes apparent when looking at the individual cancer types. When the observed number of cancer cases was zero in a Lilienfeld table Cherry has not included the corresponding expected number in his calculation of the total expected number, even though all expected numbers are greater than zero. Inclusion in the calculation of these expected numbers is required because the observed numbers of cancers from these other tables (i.e., zeroes) have implicitly been included in the calculations of the total observed numbers of cancers. Inclusion in the calculations of all observed and expected numbers is required for consistency with the corresponding calculation for all cancers combined. The bias Cherry's omission induces is to increase the SMRs, giving the impression of a stronger association between exposure and cancer.

This point is illustrated by looking at how Cherry has calculated an SMR of 20.0 for adult brain cancer. The two brain cancers in the Moscow embassy group occurred in

adult dependants not living in the embassy itself (Lilienfeld: Table 7.16). For this group the expected number was 0.10, which is shown in the table above and used in the calculation of the SMR of 20. However, the correct value of the expected number would include both the expected numbers of brain cancers for embassy employees, shown in Table 5.6 (i.e., 0.9) and the expected number for adult dependants living in the embassy, from Table 6.12 (i.e., 0.05), giving a total expected number of 1.05 (i.e., 0.9+0.05+0.10). On that basis, the SMR reduces from 20 to 1.9.

It could not unreasonably be argued that, to be consistent with the calculation for all cancers, a calculation for all brain cancer (i.e., including children) would be more appropriate. From Table 7.16 there were no child brain cancers, although the expected number was 0.5. Adding together the expected and observed numbers for adults and children gives an observed number of two brain cancers and an expected number of 1.55, with a corresponding SMR of 1.3. Comparing this with Cherry's original estimate of an SMR of 20 illustrates the ease with which bias may be introduced by the inappropriate selection of data for comparison purposes.

Similar calculations may be carried out for the other cancer types in the table. Table 2 shows a comparison of Cherry's data and values that have been calculated by this reviewer using the correct method. For two cancers, Cherry's observed numbers of cancer cases were also at variance with what was actually in the Lilienfeld tables. In the absence of any other obvious explanation, these appear to be transcription errors.

In summary, Cherry's method of calculation is inappropriate and leads to serious bias. No weight should be given to his table [Cherry: page 45].

Table 2: Comparison of data from Cherry (NC) and calculations by the reviewer (MB)

Cancer	Observed		Expected		SMR	
	NC	MB	NC	MB	NC	MB
All cancer	33	33	24.83	24.83	1.33	1.33
Adult leukaemia	2	2	0.8	0.98	2.5	2.0
Genital cancer, female†	4		0.8		5.0	
Adult brain tumour	2	2	0.10	1.05	20.0	1.9
Female breast cancer	2	3	0.50	1.4	4.0	2.1
Childhood leukaemia	4	2	1.33	0.5	3.0	4.0

† It is unclear what data Cherry used for female genital cancer and, therefore, no recalculation has been attempted.

Selvin et al. (1992).

Distance and risk measures for the analysis of spatial data: a study of childhood cancers

Outline of the study

In this study, three statistical methods were used to assess whether there was spatial clustering of childhood cancers around a large microwave tower (the Sutro Tower) situated in San Francisco. A total of 123 cases of cancer in 50,686 white individuals under 21 years during the years 1973-88 were used in the analysis. Two of the analytical methods were based on analysis of distances of case residences from the tower; one of these methods involved maps that had been transformed so that the underlying population density was equalised across the map. The third method involved calculation of relative risks for the exposed children compared with unexposed children, The division between "exposed" and "unexposed" was a 3.5 km radius around the Sutro Tower. The choice of radius was based upon statistical power considerations applied to San Francisco disease and population distribution data.

Dr Cherry's criticisms

Dr Cherry has conducted his own analyses of the data in this paper taking into account information on levels of actual exposure to electromagnetic radiation from the tower. He has concluded the opposite of the finding in the published paper, namely that "the data in Selvin et al. (1992) show a highly significant dose response relationship..." [Cherry: page 54]. This conclusion is based on two related analyses that Cherry has carried out:

1. A comparison of the relative risks for childhood cancer for those living less than 4.5 km from the tower with those living outside that radius.
2. Calculation of risk ratios and cumulative risk ratios for successive radial rings around the Sutro Tower.

From the first analysis, Cherry calculated a series of relative risk estimates, ranging between 2 and 3, all statistically significant (i.e., $p < 0.05$) for brain cancer, leukaemia, leukaemia and lymphoma combined, and all childhood cancers.

From the second analysis, Cherry produced for each of the cancer types and all cancers combined a series of risk ratios for the individual cancer types and all cancers combined. He claimed that these show an exposure-response relationship inversely associated with distance from the Sutro Tower (i.e., decreasing risk with increasing distance from the tower).

Comment on the Cherry critique

There are three main problems with Cherry's review of Selvin et al. (1992). The first and over-riding problem is that it is very unclear how Cherry obtained his data, both numbers of cancer cases and underlying populations, for calculating radial-band cancer rates and relative risk estimates. It is clear from Cherry's tables that he has used exactly the same overall under-21 population as Selvin et al. (i.e., 50,686 individuals). However, how Cherry has apportioned this population between those living inside and outside the 4.5 km radius, or within the various radial bands he has chosen, is completely unclear and unexplained. There are insufficient data published

in the paper by Selvin et al. for such calculations to be carried out. That Selvin et al., themselves, had sufficient data to compute such populations is clear, as they had area-specific population data adequate for map transformations to equalise residential population densities. This demonstrated that the underlying population density is not uniform and such heterogeneity would need to be taken into account in assigning populations to radial areas. It is not clear that Cherry has been able to do this appropriately. Certainly, a simple proportionation of population on the basis of radial area alone would be inadequate. In any case, a clear explanation of the method, so that figures may be checked or replicated, is essential for a critique that purports to present a more appropriate analysis than that of the original authors.

It is also unclear how Cherry calculated the number of cancer cases in each of the radial areas that he uses for his relative risk calculations.[3]

Clearly the rates and relative risks calculated by Cherry are very sensitive to the methods used to derive the numerators and denominators. The methods Cherry has used are obscure. Overall, then, in the absence of a justification for the radial band-specific cancer numbers and underlying populations, it would be inappropriate to give weight to Cherry's relative risk calculations.

The second problem with Cherry's critique is how he decided that a radius of 4.5 km from Sutro Tower would be appropriate for delineating "exposed" and "unexposed" populations. Selvin et al. (1992) use a radius of 3.5 km for this purpose, and provide a clear and detailed rationale for this choice. However, Cherry simply states "the exposure data suggests (sic) that a 4.5 km cutoff is more appropriate" [Cherry: page 52]. To avoid bias in the analysis of data, spatial boundaries to define exposure groups need to be clearly justified (Rothman & Greenland, 1998, page 206). This avoids concerns and possible allegations that boundaries have been gerrymandered to fit pre-existing causal hypotheses. In that regard, the explanation put forward by Cherry is inadequate and unconvincing.

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[3] One method that might have been used is to take Figure 2(a) published by Selvin et al., to draw upon it concentric circles around the Sutro Tower, and to count the number of cancer cases within each circle. This method (if it was used) would rely heavily on the precision of the published diagram and would be likely to introduce considerable errors.

Thirdly, Cherry makes no attempt to rebut the findings of Selvin et al. which, using three separate methods, showed no association between cancer risks and the Sutro Tower. Selvin et al. had available more accurate and detailed cancer and population data than Cherry. To provide a convincing argument that his analysis should take precedence over that of Selvin et al., Cherry would need at least to rebut their analysis. He has not done that.

In conclusion, in the opinion of this reviewer, Cherry has presented an alternative analysis that is unsubstantiated, relying on data that have been generated using unknown methods, and using a delineation of exposed and unexposed that has not been appropriately justified. He has failed to refute the original analysis by Selvin et al., which remains the most appropriate interpretation of the data.

Beall et al. (1996)

Brain tumors among electronics industry workers.

Outline of the study

This study was a nested case-control study of brain tumor mortality cases from among the employee mortality cohort (1975-89) from a large electronics company developing and manufacturing video display terminals (VDTs). Controls were selected from among other causes of death, matched on gender and year of birth. Work histories were obtained from corporate records. No job-specific evaluation of exposure to electromagnetic fields or chemical agents was carried out. Overall, the study found "no meaningful association between VDT development work and brain tumour mortality". However, in some job and employment length categories, particularly engineering/technical workers, statistically significant elevated odds ratios were found.

Dr Cherry's criticisms

Dr Cherry claims that the study shows "statistically significantly increases (sic) of brain tumours for those using VDTs in their work for more than a decade" [Cherry:

page 56]. He considers that the study has been misrepresented by ICNIRP as a "no effect study".

Comment on the Cherry critique

Dr Cherry's view appears to be based on the observation in the study that engineering/technical workers with more than 10 years of work in that category had an odds ratio of 1.7 (95% confidence interval [CI]: 1.0-3.0); and subjects with 10 or more years of work as a programmer had an odds ratio of 2.8 (95% CI: 1.1-7.0). However, Cherry's interpretation that these two groups of workers were "using VDTs in their work for more than a decade" [Cherry: page 56] is not substantiated. In particular, the Beall paper states (page 130) that before 1975 it would have been uncommon for programmers to use VDTs and there was no evidence of elevated odds ratios among systems analysts, who would have been comparably exposed to VDTs over the same period of time. The paper also states that there was no evidence of an increased risk among VDT development workers.

In his critique Cherry reproduces a table from Beall et al., which shows odds ratios based on years of employment. Cherry states that this shows, for men, "increasing risk of all brain tumours and gliomas with the increasing work time with VDTs..." [Cherry: page 55]. This statement misrepresents the data. The table refers to employment across all subjects in the study and is not related to years of work with VDTs.

The study by Beall is of limited value in relation to the issue of whether radiofrequency radiation is associated with increased cancer risk. It has little information on job-related exposures and it is likely that exposures would have included both extremely low frequency electromagnetic fields and chemicals. Thus, even if it did produce evidence of associations between brain tumours and cancer, it would be difficult, if not impossible, to interpret them. Certainly, it would not be the case that radiofrequency radiation could be implicated. No evidence has been put forward that the workers for this company would even have had appreciable exposure to RF radiation. It is even unclear the extent to which specific occupational groups worked with VDTs. Cherry has over-interpreted the data in this regard.

Apart from the issue of exposure misclassification, this is a study in which a large number of statistical comparisons were made. In such circumstances, at least a few comparisons between cases and controls are likely to produce false positive results. For that reason, it is appropriate to be cautious when interpreting data and not to jump to conclusions on the basis of a few selected results that marginally achieve statistical significance.

Grayson (1996).

Radiation exposure, socio-economic status and brain tumour risks in the US Air Force: A nested case-control study.

Outline of the study

This was a nested case-control study to examine the relationship between electromagnetic field exposures and brain tumour risk in the US Air Force. The strongest risk factor found was higher military rank. However, elevated risks were found for estimated exposures to radiofrequency and microwave radiation (OR = 1.39, 95% CI: 1.01-1.90), and for exposure to extremely low frequency non-ionising radiation (OR = 1.28, 95% CI: 0.95-1.74).

Dr Cherrys' criticisms

Dr Cherry is mainly critical of the ICNIRP statement that the paper did not show significant increases in nervous tissue tumours. He postulates that the association with rank may be a consequence of having had early exposure to electromagnetic radiation and then remaining in the Air Force (and eventually reaching high rank).

He concludes that "the study does show statistically significant increases in brain tumours from RF/MW radiation" [Cherry: page 56].

Comment on Cherry's criticisms

Dr Cherry is correct insofar as his criticism of the ICNIRP statement is concerned. However, he over-interprets the data when he concludes that the increase in brain tumours is from RF/MW radiation. The study only shows a weak association, which is marginally statistically significant. It would be inappropriate to reach any such conclusions on the basis of such a result. Cherry is also incorrect in his hypothesis about the association between brain tumour risk and higher rank being a consequence of early exposure and remaining in the Air Force. This possibility was considered and the association with rank was found not to be confounded by age or length of service in the Air Force (Grayson, page 484).

Overall, the Grayson study provided some weak evidence of an association between RF/MW radiation and brain tumour risk. However, Cherry has over-interpreted it, even though the brief statement in the ICNIRP (1998) report (page 11) may indeed be incorrect. Possibly of more importance in this study was the stronger result that higher rank is associated with brain tumour risk. This is consistent with other studies that have shown an association between brain tumour risk and higher socio-economic status (Demers et al., 1991).

Szmgielski (1996).

Cancer morbidity in subjects occupationally exposed to high frequency (radiofrequency and microwave) electromagnetic radiation

Outline of the study

This is an update of an earlier study of Polish military personnel during 1971-1985. There was an annual average of 128,000 military personnel, of which about 3,700 were considered to be occupationally exposed to RF/MW. Expected rates of cancers were calculated from the "unexposed" portion of the cohort and compared with the observed rates in the "exposed" group. Study results showed an excess of all cancers in the exposed group (relative risk = 2.07) and increased risks for a number of specific cancer sites, including the gastrointestinal tract, the brain and nervous system, and the haematopoietic and lymphatic systems.

Dr Cherry's criticisms

Cherry is supportive of the published results of the study and critical of ICNIRP, which he considers unreasonably dismissive of it. Cherry considers the Szmigelski study to be "the largest, most carefully identified and classified study of its kind yet carried out" [Cherry: page 62].

Cherry then makes an attempt to estimate mean work day exposures for the exposed group, based upon assumptions of compliance with the Polish standards for electromagnetic field exposure then in place. He calculates that the effects in the Polish exposed group are associated with mean lifetime RF/MW exposures in the range 3.5-10 $\mu W/cm^2$.

Cherry cites a further, 1998, study by Szmgielski which he says contains a dose-response relationship as part of a prospective study. However, he does not provide a reference for this additional study, and the author of this review has not been able to find reference to it in other sources, including Medline.

Comment on Cherry's criticisms

The Szmgielski study (with its 1988 predecessor) stands alone in its findings of increased risks of quite high magnitude for a wide range of cancers. Its significance and importance is diminished by the lack of clarity about the methods that were used. For example, it is not clear how exposure status was defined, except that it was stated to have been "easily established". Given the lack of information on how the exposed group was selected, the possibility of other confounding exposures, or some form of selection bias, cannot be ruled out. Also, it is unclear how cancer cases were identified and whether different cancer ascertainment methods were used for exposed and unexposed members of the cohort. It is conceivable that, if a greater effort was made to find all cancer cases in the exposed group than in the unexposed group (which was used to calculate baseline cancer rates), this, in itself, could have led to

the increased relative risks.[4] There is also a question about how the statistical analysis was performed. It does not appear to have been appropriately carried out using person-time of follow-up, as would normally be done for a retrospective cohort study. Instead the cohort has been divided into age-groups and rates calculated for each of those age groups. How person-time was allocated as the cohort aged over the 15-year duration of the study is not clear.

It is not clear whether there was a minimum exposure criterion for entry into the exposed group, or whether members of the cohort were followed after they left the military. In addition, no analysis by latency or by length of exposure has been carried out. This could strengthen findings if the cancers were associated with exposure to electromagnetic fields.

In summary, despite Cherry's claims about the value of the Szmgielski study, as published this study does not stand up to scrutiny. This is probably why it generally has not carried much weight in expert assessments of the possible effects of RF/MW radiation.

Hocking et al. (1996).

Cancer incidence and mortality and proximity to TV towers.

Outline of the study

This study investigated whether there were higher rates of brain cancer and leukaemia incidence and mortality in three municipalities (Lane Cove, Willoughby, and North Sydney – the "inner area") surrounding a site with three closely located TV towers, compared with six more distant municipalities (the "outer area"). Rate ratios were adjusted for age, sex, and calendar period (1979-84 and 1985-1990). Elevated rate ratios (inner vs outer areas) were found for total leukaemia incidence and mortality

[4] It is of note that the recent IEGMP (2000) review noted, in regard to the Szmigielski study, that "it appears that the exposures of cancer cases were obtained from a different source (medical records) than those of the study population as a whole (provided by safety staff), and this could have seriously biased risk estimates". The IEGMP review concluded that the Szmigielski study was "unsatisfactory, and can be given little, if any, weight".

(1.24 and 1.17, respectively) and lymphatic leukaemia incidence and mortality (1.32 and 1.39, respectively). Also, incidence and mortality rates for total leukaemia, lymphatic leukaemia and myeloid leukaemia were elevated for children aged 0-14 years. No elevations were found for brain cancer risks. The authors concluded that they had found an association between increased childhood leukaemia incidence and mortality and proximity to TV towers.

Dr Cherry's views on the study by Hocking et al.

Cherry largely cites and reproduces results from this study, without criticism. He regards it as providing support for a no-observed adverse-effect-level of 0.02 μW/cm2, based on his estimates of exposures.

Comment on Dr Cherry's analysis

Since the publication of their original study report, there has been reanalysis of the data and criticism of the original conclusions (McKenzie et al., 1998). Cherry does not refer to this subsequent paper, although it was published by the time that he submitted his critique. McKenzie et al. (1998) reanalysed the data used in the original study by Hocking et al. (1996) and found that the high rates of leukaemia in the "inner area" were entirely attributable to high rates of leukaemia in the Lane Cove municipal area. The other two municipalities that made up the inner area had leukaemia rates similar to those in the outer area. The leukaemia rates in Lane Cove were also higher in an earlier period, when exposure to radiation from the TV towers would have been lower, than in the later period, because of the extension to 24-hour broadcasting by one of the TV stations. These findings were contrary to what would be expected if there was a causal association between the TV towers and leukaemia.

This led to a debate in the literature between the two sets of authors. Hocking et al. (1999) accused McKenzie et al. (1998) of carrying out "*post hoc* analyses which are not scientifically justifiable". They argued that it was not appropriate to look separately at the three municipalities comprising the inner area, as their statistical testing had not shown them to be heterogeneous in terms of leukaemia rates.

Therefore, they argued, it was justified to combine results for the three areas. McKenzie and Morrell (1999) rebutted this argument by carrying out their own statistical tests, showing the inner area municipalities were indeed heterogeneous in terms of leukaemia rates. Hocking et al. (2000) responded that it was inappropriate to carry out statistical tests of heterogeneity for the inner and outer areas separately and that one such test should be carried out across all inner and outer areas combined.

In this author's view the recent debate has become inappropriately focused on issues of statistical significance testing, ignoring the underlying epidemiological issue of whether there is really an association between the TV towers and leukaemia rates. Viewed in that light it is appropriate to separately examine the heterogeneity of risks in the areas close to the TV tower. If non-ionising radiation from the tower is indeed causing increased leukaemia risks then the risks in correspondingly exposed areas should be similar. That appears not to be the case. Some of the later correspondence on this issue may not have been published at the time Dr Cherry prepared his critique. Nonetheless, it is summarised here in the interests of presenting a reasonably complete picture of the debate surrounding this issue. In regard to Cherry's critique, the main point to be made is that it is incomplete and takes no account of more recent analysis and debate regarding the original paper of Hocking et al. (1996).

Dolk et al. (1997)

Cancer incidence near radio and television transmitters in Great Britain. I. Sutton Coldfield Transmitter; II. All high power transmitters.

Outline of studies

These are two companion papers, and are outlined together here.

The first paper was a study of selected cancers in relation to proximity to the Sutton Coldfield FM radio and television transmitter in the West Midlands, England. The study area was defined by a 10 km radius around the transmitter, with 10 concentric radii at various distances inside this. An inner 2 km radius band was also defined. Expected numbers of cancers were derived from national rates. The relative risk of adult leukaemia within the 2 km band was 1.83 (95% CI: 1.22-2.74) and across the defined radial bands there was a significant decline in risk moving away from the

transmitter ($p = 0.001$). Significant declines in cancer incidence moving away from the transmitter were also found for bladder cancer and skin melanoma.

The second paper was a study of 20 British high power television and radio (FM) transmitters, excluding the Sutton Coldfield transmitter (except for the examination of childhood cancers). The transmitter with by far the largest surrounding population was at Crystal Palace, and this tended to dominate the results. The Crystal Palace transmitter broadcast only TV and not FM radio. Although there was a decline in risk for leukaemia when moving further out from the transmitters, this decline began at the 2-3 km band and there was no excess risk within the 2 km radii. No patterns similar to that found with Sutton Coldfield were evident for skin melanoma or bladder cancer. Nor was any pattern of risk found for childhood leukaemia. The authors concluded that "the results, at most, give no more than very weak support to the Sutton Coldfield findings."

Dr Cherry's criticisms

Cherry argues that the results in the two papers by Dolk et al. are, in fact, consistent with each other. This is because the other transmitters, particularly that at Crystal Palace, are limited to UHF television broadcasting, and the direction of the Crystal Palace beam is such that it would create a peak ground level strength at about 2 km from the transmitter. This would be consistent with the result for the combined transmitters of a peak in leukaemia rates occurring in the 2-3 km band and declining with distance from there.

Comments on the Cherry critique

Cherry cites no source for his opinion that UHF signals would be low near to the transmission towers. However, if his view is correct, then this would represent a potentially plausible hypothesis for reconciling the results of the two studies of Dolk et al.

To investigate this hypothesis further, the author of this review contacted the primary author of the UK transmitter studies. She advised that extensive measurements of

both FM and TV transmissions from the Sutton Coldfield transmitter, going out further than 5 km, showed maximum power density for both transmissions to be highest within one kilometre of the transmitter [Pers comm: Dr Helen Dolk]. This conflicts with Cherry's statement (page 74), in relation to Sutton Coldfield, that "the UHF [TV] signal is relatively low inside about 1.5 km because it is strongly directed outwards in the main beam ...".

Further contact was made with Ms Frances Pollitt of the UK Department of Health (Environmental Chemicals Unit) to find out about more about beam strength measurements around the Crystal Palace transmitter. In response to this enquiry, Ms Pollitt contacted Crown Castle UK Ltd., the company that now owns and operates the transmission services for the BBC. In response, Crown Castle advised, *inter alia*, in relation to the UHF (television) transmission beam from the Crystal Palace transmitter "There is no peak or local maximum at or around 2km from the site. This is confirmed by theoretical studies of the antenna performance and measurements we have taken at this and many similar locations."

The full text of the letter from Crown Castle is included in the Appendix to this report. In short, no support is provided for Dr Cherry's hypothesis.

Discussion and conclusions.

This review was carried out both at the behest of Dr Neil Cherry and for the purposes of assessing the extent to which Dr Cherry's extensive criticisms of the ICNIRP (1998) Guidelines should be fully considered in the development of New Zealand guidelines for the siting of radiofrequency transmission sites. It is appropriate to re-emphasise the limitations of this review. First, it did not seek to critique the ICNIRP Guidelines, nor could it adequately address the issue of whether there is any association between radiofrequency radiation and health effects. Thirdly, this review does not purport to be, by any means, an exhaustive review of Dr Cherry's critique. It attempted to focus on the major points of his critique, with particular emphasis on the interpretation of cancer epidemiology studies. Re-evaluation of the cancer epidemiology studies forms the major part of Cherry's critique. The premise behind

this approach was that if the Cherry critique was worthy of full consideration, then the most substantive part of it should withstand close examination.

By their nature, observational epidemiological studies, such as those considered above, because they do not permit the investigator to control the conditions of the experiment, are potentially liable to biases, in turn leading to difficulties in interpretation. To combat such problems epidemiologists have become adept at analysing the limitations of particular studies, and also at combining and drawing inferences from the results of studies carried out in different settings, and possibly with different study designs. In general, it is true for most epidemiological issues, particularly those for which the underlying relative risks are fairly small, that the results of one (or even a few) epidemiological study are insufficient for firm conclusions about causal relationships to be drawn. This is because any one epidemiological study will have its limitations, and the possibility of unknown biases can never be completely discounted.

Although there are a number of widely accepted criteria for considering together the results of epidemiological studies, most notably the criteria set out by Hill (1965), these do not obviate the need to closely evaluate each study separately. The two main considerations when considering individual studies are validity and precision. Validity is a measure of systematic error, introduced by bias, and precision is an issue of random error, affected mainly by study sample size. Validity is paramount over precision. A study with little validity is of little value, whereas a study of low precision, but high validity, can be of value in at least putting upper and lower bounds around the size of the underlying risk.

In his analysis Dr Cherry is clearly aware of the criteria set out by Hill (1965). He periodically cites a number of Hill's criteria, such as the presence of a dose-response relationship, consistency of results between studies, and biological plausibility. When it comes to the more critical issue of the quality of individual studies, Cherry has some awareness of the importance of precision of results (as instanced by his frequent citing of the importance of statistically significant results). However, he shows only limited awareness of the potential for bias (confounding, selection bias, and information bias), not only in his assessment of the published results of studies, but

also in his own reanalyses and reinterpretations of the data of others. That, in this reviewer's opinion, is the most fundamental problem with Cherry's analysis of the epidemiological literature. It is a pervasive issue that renders most of his re-analyses and reinterpretations invalid, or, at least, highly suspect.

Some examples to illustrate this point are appropriate: Cherry's apparently limited awareness of the problem of confounding is illustrated by his willingness to recalculate results using crude data, without even acknowledging the potential confounding by age, as in his reassessment of the data from Robinette et al. (1980). Selection bias is illustrated by Cherry's arbitrary selection of a 4.5 km cutoff to delineate "exposed" and "unexposed" in his reassessment of the data from Selvin et al. (1992); information bias is illustrated by Cherry's willingness to consider workers in the study by Beall et al. (1996) as having spent particular periods of time working with VDTs, when the published paper does not support that view.

In several places Cherry makes clear that he considers statistically significant results to be of paramount importance. For example, on page 6 of his critique, he states that "public health protection ... should be based on epidemiological studies which show statistically significant results." On page 7 he states "once epidemiological studies find statistically significant increases in cancer from chemicals at a given mean concentration, safety factors of 1 to 10,000 are applied." And on page 35 he states, *inter alia*, "in epidemiology it is agreed that a statistically significant result is one which reaches the 1-in-20 or 5% threshold for statistical probability". In the same paragraph, in reference to a two-tailed test of significance, he states that it "requires half the population to achieve statistical significance when searching for an adverse effect than when the hypothesis involves the possibility of a positive and a negative effect".

In this reviewer's opinion, it is doubtful that any properly trained epidemiologist or biostatistician would agree with any of these statements.

Cherry's emphasis on the importance of statistical significance testing is out of step with most modern epidemiological thinking. Statistical significance is an arbitrary criterion, strongly influenced by sample size. For some time now, consideration of

actual *p*-values and, more usefully, confidence intervals, has been regarded as much more appropriate than statistical significance testing (Gardner & Altman, 1986; Thompson, 1987). Also, Cherry's emphasis on statistical significance testing shows little, if any, understanding of how bias can strongly influence the size of the *p*-value associated with a particular statistical test. Cherry's view of bias seems to be focused on bias associated with personal, pre-conceived views of investigators and interpreters of data, rather than bias resulting from study design and data collection methods. For example, he suggests (page 34) that analysis of data is subject to bias in its use and interpretation and that subjective bias can influence the choice of statistical method used (page 35). Ironically, Cherry's own analysis of data contains striking examples of how such bias can operate. This is illustrated, for example, by the way in which the data from the Lilienfeld et al. (1978) report were re-presented by Cherry, with inappropriate calculation of expected numbers of cancer deaths.

It is possible to have some sympathy with Cherry's view that the ICNIRP (1998) treatment of the available epidemiology studies is brief and, in some instances, appears wrong or, at least, rather carelessly worded. However, one assumes that the published ICNIRP Guidelines are a summary of more substantive underlying documents. In any case, the ICNIRP assessment of the epidemiology is broadly consistent with other expert reviews of these studies, such as the IEGMP (2000) review.

Overall, one is left with the impression that Cherry either has not had the skills to properly evaluate and extend the published cancer epidemiology analyses, or has not applied an objective approach, or possibly both. This review engendered no confidence that Cherry has the expertise or the objectivity to evaluate the other areas of radiofrequency radiation epidemiology covered in his critique. It is not possible for this reviewer to pass an authoritative judgement on the other, non-epidemiological, aspects of the critique. However, it would appear that Dr Cherry's main basis for recommending a much lower level of exposure to RF radiation than does ICNIRP is his reinterpretation of the epidemiology studies. Generally speaking, the other material in the Cherry critique seems to be presented mainly to buttress the conclusions derived from Cherry's re-evaluation of the epidemiology studies.

In conclusion, based on the assessment set out above, this reviewer could not recommend that Dr Cherry's critique of the ICNIRP Guidelines be accorded weight in determining the final shape of the New Zealand Guidelines for the siting of radiofrequency transmission sites.

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Appendix: Letter from Nigel Turner, Director of Field Operations, Crown Castle UK Ltd

CROWN CASTLE INTERNATIONAL

Crown Castle UK Ltd
PO Box 98
Warwick
CV34 6TN

Tel: 01926 416000
Fax 01926 416600

www.crowncastle.com

Frances Pollitt
Department of Health
Room 685D Skipton House
80 London Road
LONDON
SE1 6LH

DDI Tel 01926 416008
DDI Fax 01926 416400

Our Ref: NT/DJB

3 July 2000

Dear Ms Pollitt

Radio Frequency Field Strengths from Crystal Palace

Your query regarding UHF field strengths around Crystal Palace transmitting station has been passed to Crown Castle UK Ltd as we are the company that now own and operate the transmission services for the BBC following privatisation in 1997.

UHF main station antennas are designed to cover a service area up to a distance of typically 80km from the station, the field strength falling off with distance.

The antennas used radiate a narrow beam, the main lobe of which is at an angle of less than 1° below the horizon. At larger angles below the horizon, the antenna radiates a series of minor lobes, which decrease in amplitude as the angle increases. A technique known as gap filling is used to fill in the curve and provide a more uniform field close to the site.

There is no peak or local maximum at or around 2km from the site. This is confirmed by theoretical studies of the antenna performance and measurements we have taken at this and many similar locations.

The station at Crystal Palace radiates 4 analogue TV services in the UHF band, with a peak synchronising pulse power of 1MW per service. Other transmissions from this station are FM and AM radio, digital TV and digital radio services, but all at much lower power levels than the analogue TV. None of the antenna systems for these services produce a local peak or maximum at or around 2km from the site.

We note that Dr Helen Dolk's 1997 study is referred to in your enquiry: you are probably aware that the study of the Sutton Coldfield area was re-visited by the Birmingham Institute of Public and Occupational Health in December 1999, using more up to date cancer information. Their conclusion was "this study has found no credible evidence that living close to the TV mast is associated with an increased risk of leukaemia".

I trust the above information is of use to you.

Yours sincerely

Nigel Turner
Director of Field Operations

Copy to: Bob Giles

From:	Martin Gledhill
To:	Richard Jaine (MoH)
Cc:	Chris Procter (MoH); Tim Hopley; Jo Dones
Subject:	Re: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068
Date:	Tuesday, 10 March 2026 9:58:37 am
Attachments:	image001.png image002.png ESR review of Cherry on RF MoH cover letter.pdf ESR review of Cherry on RF.pdf

---

Hi Richard

I have had a quick look at the paper and while the details of biology are well outside my expertise I note a couple of things:

1) There is barely a passing reference to how the RF exposure was achieved and nothing on what the actual RF exposure was - just a single diagram in the supplementarymaterial which is not informative. Poor dosimetry is one of the main weaknesses in much RF/health effects research, and without knowing what the exposure was it is impossible for anyone to try and replicate findings (important feature of any science - it should be replicable) and we have no idea whether the exposure could have heated the cells under test, which could have induced its own effects.

2) At the end of the paper there is quite a list of "notable" (their word) study limitations that mean you cannot extrapolate from this study on isolated cells to what might happen in the real world.

I see that the email to Minister Docey mentions Neil Cherry's criticism of ICNIRP (dating from 1999 or early 2000) - attached is a review commissioned by the Ministry of Neil Cherry's critique. The review concludes that Dr Cherry's critique should not be afforded any weight in determining the shape of NZ Guidelines for siting RF transmitters.

We can include this paper in the material presented to the next Interagency meeting.

Martin

On Mon, 9 Mar 2026 at 12:58, Richard Jaine <xxxxxxx.xxxxx@xxxxxx.xxxx> wrote:

> Kia ora,
>
> Original correspondence and another attachment that was sent is attached. I hope that helps.
>
> Cheers,
>
> Richard
>
> ---
>
> **From:** Martin Gledhill <.@..>
> **Sent:** Monday, 9 March 2026 12:47 pm
> **To:** Richard Jaine <.@..>
> **Cc:** Chris Procter <.@..>; Tim Hopley <.@..>; Jo Dones <.@..>
> **Subject:** Re: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Hi Richard

No, I can't access the link, are you able to send me the correspondence?

Thanks

Martin

On Mon, 9 Mar 2026 at 11:38, Richard Jaine <xxxxxxx.xxxxx@xxxxxx.xxxx.xx> wrote:

> Kia ora koutou,
>
> Thanks for sending this through, Chris. You've ended up at the right place. The Ministry (PHA) convenes the Interagency Committee on the Health Effects of Non-ionising Fields (where I am currently chair), while HNZ (the Environmental Health team) provide secretariat support for the committee and technical advice. During my time on the committee, we have not had the public attend meetings, but we do have a consumer representative on the committee.
>
> I have cc'ed in my HNZ colleagues for their comment.
>
> @Tim Hopley, @Jo Dones and @Martin Gledhill – here is the link to the original correspondence. Please let me know if you can't access it.
>
> Case Type: Correspondence
> Case Number: H2026078866
> Case Title: Claims that Bluetooth causes autism
> Advisor: Chris Procter
> External Reference Number: MDC-MH3572
> Case Link
> Case Sharepoint Link
>
> The corresponder provides a link to this article: CELREP116238_grabs 1..1 with a request that it is taken to the committee, and they also request attendance at the committee meeting.
>
> An initial proposed response could be along the lines of:
>
> - Thanks for your letter
> - We will consider the article at the next committee meeting
> - We do not have members of the public attend the committee meeting, but we do have

a variety of stakeholders on the committee, including a consumer representative
- Further info and discussion regarding ICNIRP and standards can be found here https://www.health.govt.nz/publications/interagency-committee-on-the-health-effects-of-non-ionising-fields-report-to-ministers-2022

Happy to hear others' thoughts.

Ngā mihi,

Richard

From: Chris Procter <.@..>
Sent: Friday, 6 March 2026 11:33 am
To: Richard Jaine <.@..>
Subject: FW: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Kia ora Richard

The Ministry has received a DREP regarding concerns that bluetooth causes autism. We are having a bit of trouble finding a lead for the response. Sally Gilbert chaired the last interagency committee on health effects of non-ionising fields. However, Sally is no longer at HNZ or the Ministry. It appears you chaired the meeting the previous year (2024).

Are you able to provide a steer as to where this might sit and any direction such as does the committee have a position on this and do they engage with the public?

Note: Health NZ, Clinical SPG and Regulatory Services have already advised that this correspondence doesn't belong to them.

Ngā mihi

Chris Procter (she/her)

Senior Advisor

Government Services

Corporate Services | Te Pou Tiaki

xxxxxxxxx.xxxxxxx@xxxxxx.xxxx.xx

Ministry of Health, 133 Molesworth Street Thorndon, Wellington 6011

From: Ian Town <.@..>
Sent: Friday, 6 March 2026 11:17 am
To: SPG Office of the DDG <@..>
Cc: Chris Procter <.@..>
Subject: RE: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Hi Bridget – can you give Sally Gilbert a call (she is the contact for the Committee – and see what she thinks – does the committee have a position on this and do they engage with the public. If not we can do a short reply

Ian

From: Bridget Murphy <.@..> **On Behalf Of** SPG Office of the DDG
Sent: Friday, 6 March 2026 10:29 am
To: Ian Town <.@..>
Cc: SPG Office of the DDG <@..>; Chris Procter <.@..>
Subject: FW: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Kia ora Ian

We have received a DREP for your team. Please review the correspondence in the below SharePoint link and confirm your team can lead. Clinical leads believe MoH had an evidence group that looked at the science behind these things and that it therefore may sit with you.

The following have already advised that this correspondence doesn't belong to them: Health NZ, Clinical SPG, Regulatory Services.

Case Sharepoint Link

Due: 17 March

Please work with your EA to confirm internal sign out times. You can find sign out guidance here.

Nāku noa, nā

Bridget Murphy

From: Chris Procter <.@..>
Sent: Friday, 6 March 2026 9:35 am
To: SPG Office of the DDG <@..>
Subject: FW: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Hi SPG

HNZ have come back and advised their clinical team think this should sit with us. Are you able to please re-test with Ian Town's team?

Clinical leads believe MoH had an evidence group that looked at the science behind these things- Professor Ian Town?

Ngā mihi

Chris Procter (she/her)

Senior Advisor

Government Services

Corporate Services | Te Pou Tiaki

xxxxxxxxx.xxxxxxx@xxxxxx.xxxx.xx

Ministry of Health, 133 Molesworth Street Thorndon, Wellington 6011

From: Chris Procter
Sent: Friday, 20 February 2026 10:23 am
To: SPG Office of the DDG <@..>
Subject: RE: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Thanks for testing this one, Chanel.

Given it already has gone through RS who confirmed Andreas' area is for ionising radiation, I will query transfer with HNZ.

Have a great rest of your Friday!

Ngā mihi

Chris Procter (she/her)

Advisor

Government Services

Corporate Services | Te Pou Tiaki

xxxxxxxxx.xxxxxxx@xxxxxx.xxxx.xx

Ministry of Health, 133 Molesworth Street Thorndon, Wellington 6011

From: Sarah Key <.@..>
Sent: Friday, 20 February 2026 10:16 am
To: SPG Office of the DDG <@..>
Cc: Chris Procter <.@..>
Subject: RE: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Kia ora Chanel

I've done a search on Pātengi and believe this would best sit with Health NZ as it looks like Health NZ may chair the Interagency Committee on the Health Effects of Non-Ionising Fields (see relevant email linked below). Alternatively, I suggest contacting Andreas Markwitz, Director of Radiation Safety – who may be able to direct you to the appropriate part of MOH or HNZ.

Out of scope

Ngā mihi
Sarah

From: Chanel Williams <.@..> **On Behalf Of** SPG Office of the DDG
Sent: Thursday, 19 February 2026 5:37 pm
To: Sarah Key <.@..>
Cc: Chris Procter <.@..>; SPG Office of the DDG <@..>
Subject: RE: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Kia ora Sarah,

Can you please review this request and provide some feedback on whether this is something your team can provide input or a steer?

Thank you very much!

Chanel

From: Victoria Manning <.@..>
Sent: Thursday, 19 February 2026 5:19 pm
To: SPG Office of the DDG <@..>
Cc: Chanel Williams <.@..>; Chris Procter <.@..>
Subject: Re: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Kia ora

This requestor is asking to be invited to be a member of the "Interagency Committee on the Health Effects of Non-ionising Fields". This Committee reports to our DG and so I'd expect there's a team in the Ministry that looks after this Committee and I'm sorry that's not my team and I don't know where to refer you to. Perhaps try the Clinical team?

Victoria

From: Chanel Williams <.@..> **On Behalf Of** SPG Office of the DDG
Sent: Wednesday, 18 February 2026 3:51 pm
To: Victoria Manning <.@..>
Cc: SPG Office of the DDG <@..>; Chris Procter <.@..>
Subject: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068
Importance: High

Kia ora Victoria,

Can you please review this correspondence request regarding the effects of bluetooth on Autism and confirm if your team has any input regarding Neurodiversity?

If you can please confirm as soon as you can so that we can give some time to scope with HNZ.

Ngā mihi,

Chanel

From: Chris Procter <.@..>
Sent: Wednesday, 18 February 2026 3:15 pm
To: SPG Office of the DDG <@..>
Subject: FW: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Hi team

Before I test with HNZ, is there anything here for SPG regarding the neurodivergent aspects?

Case Type: Correspondence
Case Number: H2026078866
Case Title: Claims that Bluetooth causes autism
Advisor: Chris Procter
External Reference Number: MDC-MH3572
Case Link
Case Sharepoint Link

Ngā mihi

Chris Procter (she/her)

Advisor

Government Services

Corporate Services | Te Pou Tiaki

xxxxxxxxx.xxxxxxx@xxxxxx.xxxx.xx

Ministry of Health, 133 Molesworth Street Thorndon, Wellington 6011

From: ODDG Regulatory Services <.@..>
Sent: Wednesday, 18 February 2026 1:50 pm
To: GSWT Service Account - Prod <@..>
Cc: Chris Procter <.@..>
Subject: RE: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

Kia ora Chris

There is nothing in here that RS can respond to. Non-ionising radiation is not something we regulate, HNZ looks after that. Other parts of the Ministry might have views on the neurodivergent aspect, but not us.

Thanks

From: GSWT Service Account - Prod <@..>
Sent: Wednesday, 18 February 2026 12:35 pm
To: ODDG Regulatory Services <.@..>
Subject: Case commissioned - Waiting for Acceptance - H2026078866 Claims that Bluetooth causes autism Correspondence CRM:0001068

A case for commissioning is now ready for your review

Case Type: Correspondence
Case Number: H2026078866
Case Title: Claims that Bluetooth causes autism
Advisor: Chris Procter
External Reference Number: MDC-MH3572
Case Link
Case Sharepoint Link

Commissioning Comments: Kia ora, please see linked the draft DREP regarding concerns that bluetooth causes autism for RS input. This is due back to the writer by 17 March. If there are any issues with this deadline, please do let me know.

--

Martin Gledhill

EMF Services

P O Box 17

Clyde 9341

Tel: s9(2)(a)

Email: xxxxxx.xxxxxxxx@xxxxxxxxxxx.xx.xx

www.emfservices.co.nz

EMF Services is a division of Monitoring and Advisory Services NZ Ltd

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*****

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Martin Gledhill
EMF Services
P O Box 17
Clyde 9341

Tel: s9(2)(a)
Email: xxxxxx.xxxxxxxx@xxxxxxxxxxx.xx.xx
www.emfservices.co.nz
EMF Services is a division of Monitoring and Advisory Services NZ Ltd

From:	VAN DEVENTER, Tahera Emilie
Cc:	VAN DEVENTER, Tahera Emilie; LOVELL, Samantha
Subject:	WHO 2026 IAC meeting - 9-11 June - PLEASE REGISTER
Date:	Saturday, 4 April 2026 6:26:14 am

****** 2026 International Advisory Committee (IAC) Meeting on Non-Ionizing Radiation- 9-11 June 2026 – Geneva, Switzerland ******

Dear IAC representatives,

We are pleased to confirm that the 2026 International Advisory Committee (IAC) Meeting will take place **9–11 June 2026** at **WHO Headquarters in Geneva**, marking the **30th anniversary of the International EMF Project** and the **15th annual meeting of the INTERSUN Programme**. We very much look forward to celebrating these important milestones together.

The programme will cover **EMF on 9 June**, **cross-cutting NIR topics on 10 June**, and **optical radiation on 11 June**.
If there are topics which you would like us to include, please let us know as soon as possible. A draft agenda will be available by **9 May**.
Member States wishing to make a short **5-minute / 5-slide "open-mike" presentation** are kindly invited to inform us.

In-person participation is strongly encouraged, ideally with at least one representative per country, as this enables full engagement in discussions and break-out sessions. An online option will be available for those unable to travel, but please note that **remote participants will not be able to join break-out group work**.

All expenses related to participation are expected to be covered by your respective organizations. If you require a **formal invitation letter**, please let us know.

We kindly ask you to register using this survey form by **24 April**, indicating whether you will attend **in person**, **online**, or **not be available**. These responses will also help us update the distribution list and send on-site registration or Zoom details, as appropriate.

National reports (maximum 2 pages per topic) on **EMF** and/or **optical radiation** are requested by **29 May**.
These reports should briefly address:

(i) research activities related to EMF/optical radiation and health,
(ii) new relevant legislation or policies, and
(iii) any recent communication activities.

All reports will be **uploaded to the WHO website** under the respective initiatives (EMF and Optical Radiation) and will contribute to a **long-term public record of national activities**, forming a valuable historical resource.

We look forward to welcoming many of you **in Geneva** this June.

Kind regards,
Emilie van Deventer

Dr VAN DEVENTER Emilie
Unit Head (Chemicals, Radiation and Health)

World Health Organization
HQ/PPC/ECO/CRH
Geneva
Switzerland

s9(2)(a)