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ALLEGED HEALTH EFFECTS OF ELECTRIC OR MAGNETIC FIELDS: ADDITIONAL MISCONCEPTIONS IN THE LITERATURE

J. Jauchem

Residential or occupational exposures to electric or magnetic fields have been reported to be associated with health problems, particularly cancer and reproductive mishaps. Misconceptions about these alleged effects continue to be published in the medical and scientific literature. Invalid statements relating to these effects are challenged in this paper. Case reports and studies dealing with exposures to video display terminals, magnetic resonance imaging, microwaves from television transmitter facilities, ceiling cable electric heat, electromagnetic pulse, power lines, traffic radar units, and other occupational exposures are analyzed.

Key Words:

Electromagnetic fields, Non-ionizing radiation, Magnetic fields.

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Two years ago, I reviewed articles and editorials in the medical and scientific literature that contained misconceptions about alleged hazards of exposure to electric or magnetic fields (EMFs), including microwaves [Jauchem, 1991a]. Since that article appeared, additional misinterpretations have been presented by other authors. Some of these fallacies will be discussed here. In addition to the scientific and medical literature, articles in the popular press will be discussed also.

Case Reports

De la Fuente [1991] reported a claim of "magnetic resonance imaging (MRI)-induced headache." This consisted of one patient experiencing a migraine headache during a conventional abdominal MRI examination. Importantly, the patient had a history of classic migraine. In fact, the patient had experienced a spontaneous migraine crisis 11 days before the MRI scan. In spite of this, the author reasoned that: (1) the patient had a headache while undergoing the scan; (2) magnetic fields may affect melatonin levels; (3) melatonin may be linked to headaches; and, therefore, (4) the headache was "MRI-induced." Considering the available evidence, this conclusion must be questioned.

Another case report by Davanipour et al. [1991] dealt with one patient who had amyotrophic lateral sclerosis. The authors reported that the patient's first symptom was "foot dragging and loss of control of the toes." On the basis of spot measurements of EMF levels in a clothing plant where the patient had worked, the authors suggested that the patient's symptoms were related to placement of his foot next to a transformer located on the floor. Even though the authors stated that "this is perhaps an isolated event and due entirely to chance," they also proposed that "investigation of EMF in the search for the etiology of this uniformly fatal and progressive motor disability may be fruitful." The reasoning behind this proposal is spurious.

Omura et al. [1991] presented several cases in New York City and Japan of patients with "various intractable medical problems," including stroke, edema, proteinuria, circulatory disturbance with necrosis of the toes, severe backache, adenocarcinomas of the colon, pancreas, and stomach. Each of these was attributed to exposure to abnormal EMFs, in-

cluding, for example, "sleeping ... close to a passenger elevator and a large service elevator." The severe backache, which occurred while the patient was in bed, was ascribed to "a small pocket notebook with a coiled wire bail located directly under the bed corresponding to the most painful area" and "several telephone credit cards with magnetic strips... The magnetic field coming from these magnetized strips was concentrated and aimed directly up through the bed at the center of the area where the subject had the maximum back pain." The authors performed measurements of EMFs in New York and concluded that "most New Yorkers would need to consider moving to a safer area." While other areas may be safer than New York, I would hesitate to relate this to EMF levels.

Papandreou et al. [1992] reported a case of mediastinal fibrosis in a military officer "exposed for a long period to radio-frequency radiation" and remarked that the case was "unique in the literature in English." The authors' hypothesis of an association was certainly unique; it was not based on any evidence from other studies.

Isa and Noor [1991] proposed that non-ionizing radiation caused ill health, including alopecia, in three workers (33 to 38 years of age) at a television transmitter facility. The authors stated that "the adverse effects of non-ionizing radiation are mostly inconclusive and contradictory." This did not prevent them, however, from claiming that "although the evident (sic) is circumstantial, the authors feel that there is a casual link between chronic exposure to radio-frequency and microwave radiation to alopecia areate (sic)." The presumed exposures were defined as follows: The first worker was "directed to perform maintenance (sic) work while the alternative transmission was on during the past 1 year." Regarding another worker, "during the last year or so, his tasks were performed mostly during active transmission." Exposure to the third subject was described as: "since 4 months ago, he has to perform maintenance tasks on the tower while the transmission was on." No measurements of field strengths in the vicinity of the work area were performed.

Isa and Noor [1991] cited Michaelson [1982a] when noting that "small animals exposed to microwave showed neuronal degeneration in the brain, tissue damages in the kidneys and myocardium (sic)." The power density levels at which these changes occur, however, are associated with gross thermal effects. If, in fact, the workers discussed in Isa and Noor [1991] were exposed on towers during high-power TV transmission, the exposures could have been "thermal." At the minimum, Isa and Noor should have reported the transmitter power (in one case, 1000 watt electrical strength was stated), type of antenna, and location of workers relative to the antenna.

In an earlier case report, Archimbaud [1990] stated that Michaelson [1982b] "acknowledges that hematologic effects

of microwaves have been reported by himself and others." Archimbaud insinuated that these hematologic effects occurred "regardless of thermal effect." Michaelson [1982b], however, pointed out that the effects were dependent on induced hyperthermia, and noted that "in evaluating reports of hematological changes one must be aware of ... the susceptibility to thermal influences."

Video Display Terminals

The American Medical Association's Council on Scientific Affairs reviewed previous investigations in the United States, Canada, Japan, and Scandinavian countries, and noted that a casual link between video display terminal (VDT) use and spontaneous abortions and birth defects had not been established [Council on Scientific Affairs, 1987]. Recently, however, there have been a number of references to alleged hazards of VDTs in popular publications. According to some, "computer terminals have recently been found to leak potentially hazardous EMFs" [Piller, 1991]. In a recent survey of office workers [Louis Harris & Associates, Inc., 1991], the percentage of workers naming health risks from VDT emissions as a serious concern increased from 27% in 1989 to 36% in 1991. Yet, as mentioned previously [Jauchem, 1991b], recent reports have indicated no VDT-associated reproductive mishaps (I listed four examples published in 1990 or 1991; Dlugosz et al. [1992] also mentioned earlier ones). I responded earlier [Jauchem, 1991c] to another suggestion of VDT hazards [Dougherty, 1991]. Bentur and Koren [1991] have also commented on the lack of hazardous effects of exposures to VDTs. In a more recent study, Roman et al. [1992] reported no evidence of increased rates of spontaneous abortions in women working with VDTs. The authors noted the consistency of these findings with other recent studies.

Wiley et al. [1992] exposed pregnant mice to three field strengths of magnetic fields similar to those associated with VDTs. There were at least 140 animals in each group, and they were continuously exposed throughout pregnancy. The endpoints that were analyzed included numbers of implantations, fetal deaths and resorptions, gross external, visceral and skeletal malformations, and fetal weights. There were no significant differences between any of the exposure groups and a sham group. The results do not support the hypothesis that these fields are teratogenic or embryotoxic.

An article by Greiner [1991] contained several misconceptions concerning alleged hazards of VDTs. The author stated that "VLF and ELF fields emanate from every side of a VDT, so a sea of terminals exposes workers many times over." The phrase "many times over" seems to imply that workers are exposed to hazardous levels of the fields. In fact, for ELF (extremely low frequency) fields of 50-60 Hz. maxi-

To suggest that publishers have suppressed stories on VDT hazards, as Greiner did, is extremely misleading. Advocates of this cover-up theory only foster the "electrophobia" incited by the unsubstantiated claims of VDT hazards.

As Wertheimer and Leeper [1991] noted, "the overall fetal loss rate is approximately the same in the exposed and unexposed groups... However,... because of unassessed confounders, it is impossible to interpret such overall comparison of

Sweeney et al. [1989] have pointed out that results of retrospective studies (such as Wertheimer and Leeper's) of spontaneous abortions are contradictory and ambiguous. These investigators noted that the only practical way to accurately estimate spontaneous abortion rates is by the use of prospective studies. The high cost of these studies, however, should be noted.

In another paper, Eckert [1992] postulated that since "pulsations of the geomagnetic fields (GMF) are in the same range as the breathing frequency, 30-35 cycles per min, of an infant," one can assume "that such pulsations are able to influ-

ence the breathing control system of an infant." After this tenuous assumption, Eckert then hypothesized that: (Part One) "a disturbed GMF in the residence or surroundings of the pregnant woman interrupts the normal development of the... brain stem," and that: (Part Two) subsequent exposure to GMF or EMF "inverted in phase, value, form etc" could produce sudden infant death syndrome. The author then stated: "It is not necessary for me to prove part 1 of the hypothesis... because many researchers have reported such findings." The four articles cited in support of this statement, however, made no mention of GMF or EMF, but simply showed that the brain stem is involved in sudden infant death syndrome. Although the purpose of the journal that published the article is to present hypotheses rather than data, a scientific basis for Eckert's hypothesis is lacking. Eckert further stated: "the magnetic shielding effect of the ambulance... decreased the harmful effect of the pulsations of the GMF and the condition of the infants improved... During the rest in the hospital and under medical attendance, the effect of the pulsations returned and... death occurred." Evidence for a causal relationship between these factors was not presented.

Electromagnetic Pulse

In Muhm's [1992] investigation of mortality in men who were employed in an electromagnetic pulse (EMP) test program, one underlying cause of death due to leukemia was observed, compared with 0.2 expected. The low number of cases observed makes the author's claim that "the study suggested an association between death due to leukemia and employment in the EMP test program" questionable. In addition, there is no evidence of exposure to significant EMFs in the subjects of Muhm's study. The Board of Trustees of the American Medical Association [1992], noted the absence of apparent health effects of EMP.

Breast Cancer

I commented earlier [Jauchem, 1992a] on Demers et al.'s [1991] linkage of occupational EMF exposure to male breast cancer. In part of their subsequent response, Demers et al. [1992] stated: "the bias resulting from this nondifferential misclassification" (of occupation by using job titles) "would likely be in the direction of obscuring any true difference in exposure between cases and controls and, thus, would be unlikely to cause a spurious positive association between occupational exposure to EMF and breast cancer." Although underestimation of a true risk gradient may be the most likely outcome, under some circumstances nondifferential misclassification in case-control studies can result in an overestimate of relative risk [Diamond and Lilienfeld, 1962;

Dosemeci et al., 1990].

Some researchers have reported a correlation between breast cancer and decreased melatonin production, and others have reported that EMFs may result in decreased levels of melatonin in the blood and pineal gland of animals. To automatically conclude that the evidence for these two associations confirm the alleged link between EMFs and cancer, however, may be somewhat simplistic. Stevens et al. [1992] proposed that "the use of electrical power accounts, in part, for the higher risks of breast cancer in industrialized societies." I responded, in a letter-to-the-editor, that the abstract of the paper (which will probably be seen by more people and will receive more attention than the complete paper) did not state that this was simply a hypothesis based on uncertain data [Jauchem, 1992b]. I also noted the importance of considering the views of scientists who have questioned the strength of the alleged relationship between EMF exposure and the development of cancer.

Stevens [1992] claimed that I took issue only "with the tone of our paper, as opposed to its substance..." While I did object to the "tone" of the abstract, several substantive points in the rest of the text were also challenged. Rather than implying that Stevens et al.'s whole paper could not be taken seriously (as he implied in his letter), I simply strived for clarification of a few points. I did not mention the animal studies cited by Stevens, but rather challenged his relatively uncritical acceptance of some other studies, particularly those relating to the epidemiologic evidence.

The admitted skepticism of many investigators concerning the alleged link between EMFs and cancer is based on an examination of the whole body of scientific evidence on this matter. Stevens [1992] wants to be neither a "skeptic" nor a "believer." Yet one of Stevens' former colleagues [Severson, 1991] has criticized him and his more recent co-authors for not being skeptical. Stevens would rather label himself as being "neutral"; it sounds more scientifically objective to readers not familiar with this area of research. The attack on personality traits of researchers who disagree with any of his statements [Stevens, 1992] may have been inappropriate for inclusion in a scientific journal.

Stevens [1987] was one of the first researchers to hypothesize that EMFs could promote breast cancer by affecting pineal melatonin production. As Marshall [1992] has explained, scientists "often begin their work with a hypothesis and become deeply invested in it, long before peers regard it as credible."

Stevens [1992] suggested that citing a letter that was in press was deceptive since "very few have access to the letter." It was likely, however, that this letter [Demers et al., 1992] would be published close to the time when mine citing it [Jauchem, 1992b] would be published. (In fact, the Demers

et al. letter was published before mine. Incidentally, Stevens also cited a reference that was in press and that readers would not have access to immediately.)

The few quotes that I selected were not taken out of context. Stevens [1992] noted that, in my letter, "the reference cited for a caution about melatonin in the yet-to-be-published letter in question" [Demers et al., 1992] "is our (Stevens et al.'s) own *FASEB J.* paper." This fact simply reinforces my point that the abstract of the paper did not accurately reflect the uncertainty of the hypothesis.

Stevens [1992] mentioned that Dogliotti et al. [1990] "found higher melatonin levels at 800 and at 2400 hours in patients with any... cancers than in controls." Rather than supporting a link between electric power and cancer, however, the "results support the view that melatonin secretion in cancer patients is modified... as a consequence of metabolic changes due to the worsening of the host/tumor relationship" [Dogliotti et al., 1990] (i.e., an altered melatonin level is a "result" of the cancer rather than a "cause.")

Stevens noted that some results suggest a protective role for melatonin against mammary carcinogenesis in animals. Foster [1992], however, pointed out that EMF-induced changes in pineal melatonin concentration have been observed at field strengths much greater than those normally present in the environment, and no clear dose response relationship has been found. Also, attempts to replicate these findings have not been successful (e.g., the work of Grota et al. [1991], dealing with electric fields). Although there have been recent reports of weak magnetic field effects on the pineal gland under certain conditions, the physiological significance of these effects is unknown [Foster, 1992]. Sagan [1993] also noted that "if the melatonin response to light is any guide to the retinal response to EMF, rodents appear to be far more sensitive than are humans, and therefore may not be a useful model for humans."

Stevens proposed using the scheme of Lin et al. [1985] to define "possible, probable, and definite EMF exposure of workers." Occupational titles, however, as used in this design, are woefully inadequate for determining possible exposure. The fact still remains that experiments suggesting EMF suppression of pineal melatonin content in rats have not been duplicated in studies of humans. As Stevens [1987] himself stated earlier, "since breast cancer risk is highest in the most industrialized nations, a strong correlation with electric power would not be surprising but may not have etiologic significance." Stevens [1992] did not mention that some of the increased incidence of breast cancer in industrialized nations could be related to higher concentrations of aromatic hydrocarbons. Benzene [Maltoni et al., 1989], benz(a)pyrene [Huggins and Yang, 1962], dibenz(ah)anthracene [Snell and Stewart, 1962], 1-nitropyrene [Imaida et al., 1991a], and 4-

nitropyrene [Imaida et al., 1991b] are all mammary carcinogens.

Stevens and some of his co-authors [Severson et al., 1989] had earlier indicated that a "somewhat inconsistent finding made us reticent to place a strong emphasis on exposure to electric blankets in our study." These and other inconsistent findings, however, did not prevent Stevens et al. [1992] from including electric blankets in their list of hazardous sources of EMFs.

Occupational EMF Exposures and Other Cancers

A report from the US Environmental Protection Agency (EPA) [1990] reviewed studies linking exposure to EMFs with increased incidences of cancer. (Although this was released as a "workshop review draft," it [and a subsequent "review draft"] has been cited repeatedly in both scientific and popular forums. A final version still has not been released at this date. Considering the numerous citations of the report that have already appeared and the significant attention that has been given to the report, I now consider additional citations appropriate. For other comments on this report, see: Jauchem [1990a, 1990b]; Jauchem and Merritt [1991].) In this report, a number of journal articles were either cited incorrectly or used in contexts that were misleading. As Feinstein and Spitzer [1988] have mentioned: "The error is grievous if the source statement is either unsupportive or contradictory to what has been claimed for it." Although the EPA cited articles from many sources, this discussion will focus on articles that appeared in the *British Journal of Industrial Medicine* in recent years.

In a lengthy discussion, the EPA referred to studies by Vågerö et al. [1985], Törnqvist et al. [1986], and De Guire et al. [1988] as showing excesses of skin cancer in workers exposed to EMFs. In the study by Vågerö et al. [1985], an excess risk of malignant melanoma of the skin was associated with work environments where soldering was practiced. However, no unique exposure to EMFs in telecommunication workers was mentioned by any of the authors of these studies. In the study by Törnqvist et al. [1986], there was not even any mention of skin malignant melanoma. The EPA summarized these studies by stating that "the effect has been seen in different jobs with different primary EMF exposures — it seems that exposure to EM fields... may present some risk for developing malignant melanomas of the skin." Since exposure to EMFs was not measured in these studies, these comments were unfounded. Studies that supposedly assess EMF exposure levels in occupational categories are, at best, questionable and plagued with problems. In the cases reported above there were not even any attempts to assess exposure, and, in fact, the studies were not designed to investigate EMF

effects. The inclusion of these reports in the EPA's review on EMFs was invalid.

Regarding a study by Olin et al. [1985], the EPA stated: "Exposures from soldering can involve several potentially important agents, including EM fields." Again, there was no mention of EMFs by Olin et al. To stress alleged exposure to EMFs over known exposure to chemicals and solvents is spurious.

In its report on EMFs and cancer, the EPA included a lengthy discussion of work by Gubéran et al. [1989], who examined cancer incidence among painters and electricians. In contrast to EPA's focus on EMFs, Gubéran et al. included no mention of EMF exposures in the electrical workers in their study, but rather pointed out another factor which may have been important in these workers — piercing and sawing asbestos plates used for thermal insulation. This factor was not even mentioned by the EPA. Again, inclusion of this study in a review of EMF effects was unwarranted.

The above cases found in the EPA report are not the only instances of incorrect citations of articles in the *British Journal of Industrial Medicine* relating to EMF effects. Byus et al. [1987] cited a study by Vågerö and Olin [1983] supposedly indicating hazardous effects of EMFs. Byus et al. stated that "epidemiology studies have shown correlations with EM field exposure and... pharyngeal cancer," and cited Vågerö and Olin in support of this statement. The research did show that workers in the electronics industry had an increased incidence of tumors of the pharynx; but this was not thought to be due to EMF exposure. Reference to the work of Vågerö and Olin [1983] was also erroneously included in a review by Marino and Morris [1985] and a book by Coghill [1990], both claiming to show an association between EMF exposure and cancer. Delpizzo et al. [1991] also cited some of these studies [Vågerö et al., 1985; Olin et al., 1985; Vågerö and Olin, 1983] incorrectly, indicating that they involved subjects "exposed to power frequency magnetic fields." Marino [1993] referred to the work of Vågerö and Olin [1983] and De Guire et al. [1988] as studies of "EMF exposure." Aldrich et al. [1992] actually included the studies of Olin et al. [1985], Törnqvist et al. [1986], Gubéran et al. [1989], and De Guire et al. [1988] in a meta-analysis of the epidemiological evidence regarding human health risk associated with exposure to electromagnetic fields. Hopefully, more recent work of Vågerö et al. [1990], (who, again, did not investigate EMF exposure) will not be cited incorrectly by authors in this field.

Childhood Cancer in Relation to Modified Residential Wire Codes

Savitz and Kaune [1993] reanalyzed data originally reported

by Savitz et al. [1988] from a study of magnetic fields and childhood cancer. The use of a dichotomous wire configuration code was said to have "yielded much more precise evidence of elevated risks for all cancers except lymphomas" than the original five-level wire code being referenced. In this case, it is true that the confidence intervals were smaller, or "more precise." The new odds ratios for highest wire configurations, however, were 1.3 to 2.1, whereas the odds ratios from use of the original code were 1.6 to 2.8. Thus, while it may be correct to say that the evidence is "more precise," the risk estimates were not larger. (For lymphomas, the new ratio was 0.8, compared with the original value of 3.3.) Savitz and Kaune [1993] then used a modified three-level wire code and found odds ratios that were "more markedly elevated than the results based on the dichotomous codes." The odds ratios, however, were not elevated in comparison with the original wire code (for total cancers, 1.9 with the modified code versus 2.2 with the original code). This paper appears to be another version in an unending series of reports of "modified" wire codes. The number of possible permutations of these codes, with multiple rearrangements of data to obtain the desired categories, could be endless.

"Cardiac Deficits and Abnormal Vascular Response" due to Microwaves: Evidence from Animal Experimentation

A recent report by Lu et al. [1992], regarding cardiovascular responses to microwave exposure, contained faulty explanations of basic physiological processes and illogical conclusions. The authors characterized "sudden decreases in heart rate of short duration" as a "typical respiratory arrhythmia which is related to changing depth of respiration." Yet the irregular pattern shown in several figures did not resemble the arrhythmia normally seen during the various phases of the respiratory cycle [Saul et al., 1991].

According to the materials and methods section, "the means from each microwave treatment were plotted along with the means of the sham exposed group." Yet, rather than means of heart rate in each group, "incidence of abnormal heart rate" was presented instead. By defining "normal" as a change of less than 20 beats per minute, heart rate in rats exposed to higher power microwaves either increased or decreased. A change in the means was not reported; we do know there was a greater variance. The biological significance of this finding is unknown.

The high incidence of outliers should be noted (e.g., five out of eleven animals in one group concerning mean arterial blood pressure). This, once again, indicated a large variance.

To suggest that arterial pulse pressure is always propor-

tional to stroke volume (and cardiac output) was misleading. By using empirical formulas, cardiac output can be estimated from the downward slope of pressure during diastole (Lu et al. did not do this). Other characteristics of the pressure curve can be used to make the calculation even more valid. Unfortunately, these characteristics depend on the distensibility of the arteries as well as the rate of run-off of blood through the peripheral vessels, thus making the method subject to error [Guyton, 1976]. The authors mentioned that "in acute experiments... vascular compliance is not expected to change." Evidence for this assertion was lacking. The suggestion that a thermally-induced 5-6% increase in heart rate coupled with a 10% decrease in pulse pressure would automatically indicate a decreased stroke volume was not supported by the data. Although a decrease in stroke volume may have been consistent with other studies of heating responses, on the basis of the information gathered during the experiments of Lu et al., this could not be determined.

The experiments of Lu et al. did not yield results that "support Johnson's underperfusion hypothesis" that "high peak power microwave radiation may induce a subtle resetting of... baroreceptors." In contrast to the authors' claims, cardiac output and total peripheral resistance (TPR) could not be determined from the experiments. The supposed "cardiac deficits and abnormal vascular response" seen by these investigators are not based on facts.

Lu et al. noted that atropine sulfate was administered in some previous experiments of microwave exposure [Jauchem et al., 1983, 1984; Frei et al., 1988] and stated that "this muscarinic drug is known to block cholinergic nerves including the vagus nerve." While this last statement is certainly true, in the study cited by Lu et al. to support it, as much as 30 g/kg body weight was administered [Spielman and Lyman, 1971]. In the other experiments [Jauchem et al., 1983, 1984; Frei et al., 1988], much lower doses of the drug were administered prior to microwave exposure (0.04 mg/kg), which would not have affected cardiovascular responses.

Lu et al. cited the previous studies [Jauchem et al., 1983, 1984; Frei et al., 1988] and stated that "baseline heart rate... varied from experimental group to experimental group with average value in each group varied between 271 and 403 beats per minute (bpm), and the baseline mean arterial pressure also varied between 81 and 110 mm Hg or a range of 132 bpm and 29 mm Hg." Lu et al. then noted a seemingly impressive "improvement of baseline stability" in their study. In fact, the three previous papers that Lu et al. cited dealt with completely separate and unrelated studies that involved exposure to microwaves at different frequencies and power levels, in different exposure chambers, using different methods and animals of different weights, and were conducted several years apart. To compound the apparent "baseline variability" even more,

Lu et al. selected "baseline" values that were obtained at different points during the experimental procedures in the various different studies.

Regarding the previous work [Jauchem et al., 1983, 1984; Frei et al., 1988], Lu et al. asserted that "since cardiac output or stroke volume was not evaluated..., changes in TPR in these experiments could not be evaluated." This is true. The authors insinuated that they had improved the techniques by adding measurement of pulse pressure, which therefore gave them measurements of cardiac output, stroke volume, and TPR. As mentioned above, this is not true.

The suggestion that "a possible health consequence" could result from "cardiac deficits and abnormal vascular response" is not supported by the data.

Other Statements in Books, Editorials, and Reviews

Marino [1990] asserted that "failure to act coupled with the reality of EMF health risks means that some luckless subjects would have developed disease that could have been avoided." He criticized the lack of regulatory guidelines for EMFs by stating: "We do not understand the molecular mechanism of cancer induction by cigarettes, asbestos, or ionizing radiation, and yet we do not fail to regulate them." To compare EMFs with these three factors is illusory. In fact, although our knowledge is incomplete, we do have some understanding of the mechanisms of these (some of the more recent studies are, e.g.: Carbone [1992], Mossman et al. [1990], and Haranghera et al. [1992]). In contrast, there is no solid evidence of cancer induction or promotion by EMFs (regardless of which mechanisms may be postulated by some). In addition, it is important to note that there is a 10- to 30-fold difference between lung cancer death rates in cigarette smokers and non-smokers, in contrast to the low relative risks (generally between 1.2 and 2.0) for an association between EMF exposure and cancers [Silverman, 1990]. Sagan [1992] has discussed the difficulties and highly speculative nature of theories raised to explain biologic effects of EMFs.

Again quoting from Marino [1990]: "The issue of EMF health risks belongs squarely within the jurisdiction of the state agency concerned with other environmental pollutants -- not within the purview of... the Health Department (which is usually geared to study infectious disease)." Yet, it would seem more reasonable to rely on medical expertise for dealing with medical problems rather than on environmental experts.

Stevens and Savitz [1992] criticized Moore [1991] for being "unaware that a static magnetic field does not induce current in a conducting body." Stevens and Savitz, however, did not mention the important fact that a human body moving through the field will introduce additional time-varying mag-

netic fields (e.g., see Foster [1991] and Doucet [1992]).

Stevens and Savitz [1992] stated that "movement out of central cities and into small towns and suburbs may produce a net decrease in average exposure if yards are larger and homes are more distant from power lines." Even if this was true, it would not have been expected to play a major role in the Savitz et al. [1988] study of residential EMF exposure and childhood cancer, since Denver County is considered to be over 98% urban, based on the definition of urbanization used by the U.S. Bureau of the Census [Greenberg, 1983].

Moore [1992] effectively defended his position that EMF effects are greatly overstated. He noted that, in Stevens' and Savitz' reply, "not a single phrase or sentence refutes my conclusions..."

Stevens and Savitz [1992] urged readers to evaluate this area of research by attending one of the national meetings of the Bioelectromagnetics Society. Equally important presentations at other meetings of other organizations, such as the Society for Epidemiologic Research (with abstracts published in the *American Journal of Epidemiology*), the International Epidemiological Association (abstracts in the *International Journal of Epidemiology*), and the Society for Occupational and Environmental Health and International Society for Environmental Epidemiology (abstracts in *Archives of Environmental Health*), should also be considered. One of the reviews that Stevens and Savitz suggested for readers [Wilson et al., 1990] has been criticized because "none of the contributing authors belong to the skeptics group (i.e., that group of scientists who have quite vocally argued that ELF exposure has no relationship to the development of cancer and that this whole area of work is something of a wild goose chase)" [Severson, 1991].

Bates [1991] suggested that results of studies showing no association between EMF exposure and cancer (at least large ones) would probably not remain unpublished, and that, therefore, publication bias would be unlikely. The National Radiological Protection Board [1992], however, concluded that publication bias was the most plausible explanation for the small overall excess mortality due to leukemia in their review of EMFs and cancer. Easterbrook et al. [1991] noted that, in general, clinical studies without significant results were less likely to be published in "high-profile" journals.

Coghill [1990] mentioned "the chilling fact that most of the world's first AIDS victims were born in the same years as radio and television began" and stated his "suspicion that many immune-related diseases are acquired from exposure to EMF... The first few AIDS sufferers lived on the American West Coast, where electromagnetic radio traffic is among the highest... The next cases appeared in New York, which is said to consume as much electricity at any one time as the whole of Africa. As the cases built up thereafter, it became apparent

that they correlated... with the density of electromagnetic traffic in the cities." The evidence to support this imaginative hypothesis is shaky, at best.

Rai [1989] claimed that "out of 17... surveys in various electronics and electrical industries in the USA as many as 15 showed a distinct co-relation between cancer and EMFs." Results of surveys published in the literature do not support this statement. Rai [1989] surmised that "one thing is clear from these researches: that apparently low-frequency, non-ionizing EMFs from power cables and electrical appliances do make cells cancerous." The evidence for this statement is lacking.

Vainio et al. [1992] stated that the International Agency for Research on Cancer (IARC) (which is assessing the risks of EMF) evaluates potential carcinogenic agents "on the basis of all published studies of cancer in humans following exposure to the agent in question." Yet, as others have pointed out, the IARC procedure "does not in general give weight to negative human evidence" [Shore et al., 1992]. According to Moolenaar [1992]: "The current system (of the IARC)... ignores much scientific research, exaggerates the level of risk in studies it does use, and communicates its findings in terms that give the public little idea of actual risk." More recently, another investigator [Shubik, 1993] noted: "The IARC has established rules for certification (of carcinogenicity) that are quite rigid and scientifically dubious."

Savitz [1993] suggested that "in regard to EMF, an entire body of empirical evidence from epidemiology and from the laboratory has been dismissed based on theoretical objections..." and "physical theory is argued to prohibit the reported empirical observations." These statements seem to imply that all of the conclusions regarding EMF hazards made by some are based on solid experimental evidence, with no confounding present, and that the "entire body" of this evidence is clear. Some of this evidence, however, has not been discounted solely on theoretical grounds. On the contrary, the "theoretical objections" that Savitz alluded to would more correctly be described as simply recognition of physical laws that discount theories of EMF hazards. The experimental evidence itself is often in question, due to problems with the techniques used.

Marino [1993] cited a study and suggested that it demonstrated the following: "The 91 counties in the United States that contained the city nearest each US Air Force base had significantly higher cancer death rates for both men and women during 1950-1969, when compared with population-matched counties without an Air Force base." Marino [1993] ignored a report [Polson and Merritt, 1985] (appearing in a journal for which he was editor) that pointed out many fallacies in the study, including: "Cancer mortality incidence for 1950-1969 was used, but electromagnetic emissions emanating from AFBs could have changed significantly over time due to

changing missions. In fact, it is possible that some of the bases did not have radar installations during some part of this period... the correlation was with the presence of an AFB and not with nonionizing electromagnetic energy per se — a tenuous secondary association... Reevaluation showed that even the primary correlation could not be substantiated... Counties with an AFB had incidences of cancer mortality that were not statistically significantly different from those of population-matched counties for the 1950-1969 period."

Popular Books and Magazines and Non-scientific Trade Publications

Although this review has concentrated on publications in the medical and scientific literature, in this and the next section several articles in non-scientific forums will be critiqued. The objective of a new consumer oriented magazine, *Health Watch*, as stated on the publisher's page of the premier issue, was to provide readers with "accurate and complete information" on health care questions. In an article about EMFs [Turner, 1991], however, there were several misconceptions that must be challenged. Many of these misinterpretations have been addressed in the scientific literature. The suggestion of a link between EMFs and cancer has not been supported by the whole body of evidence. Conclusions based on the studies by Wertheimer, Leeper, and Savitz, which form the backbone of the alleged association presented in the article, have been soundly criticized over the past several years.

Turner mentioned "one important study" suggesting a link between video display terminals and miscarriages. The many other studies showing no such association, however, were ignored. Furthermore, the authors [Goldhaber et al., 1988] of the study mentioned by Turner acknowledged that "the kinds of jobs where VDTs are heavily used might contribute to reproductive risk," independently of EMFs.

Turner referred to "proven or suspected hazards" of EMFs, when in fact there are no proven hazards at low levels of exposure. Much of the article focused on the writings of Paul Brodeur. (I commented on some of these writings earlier [Jauchem, 1991a]. Also, see comments on Brodeur's most recent articles [Jauchem, 1992c].) Brodeur's false accusations that the federal government, with its "uncaring and cynical attitudes" (this and the following quotes are from Turner), is "ignoring or trying to bury the issue" have been debunked by many scientific investigators. The notion that "the public deserves... immediate information about whatever protective measures exist" can be countered by the fact that no such measures exist for all circumstances. According to Turner, Brodeur also suggested that we should start burying power lines as one solution to the perceived problem with EMFs. Yet, in some situations, burying cables may actually cause an

increase in magnetic fields [Fitzgerald, 1990].

Sugarman [1992] made many errors when citing studies of EMFs and microwaves; these are too numerous to include here. As just one example, one study was defined as "a large case/control study" which "examined residential exposures of census tracts with (and without) broadcast towers in Honolulu." In fact, this study [Environmental Epidemiology Program, State of Hawaii Department of Health, 1986] was an ecological study. The difficulties of this study design, including inappropriate conclusions regarding cause and effect, have been discussed by Kelsey et al. [1986]. In this particular study, the census tracts that contained towers were mainly in Waikiki and downtown Honolulu. The tracts without towers were chiefly in agricultural areas and valleys in the center of the island that were less densely populated. (Marino [1993] also ignored these factors when discussing this study.) Possible confounding due to urban/rural factors has been discussed previously [Waterhouse et al., 1982; Jauchem, 1993].

Young [1992], in an updated version of what the publisher described as "a prophetic book — the first to reveal the hidden dangers of EMFs," stated: "Although all of the projects that showed significant biological effects have been attacked..., it is impossible to believe that a hundred teams of respected scientists have produced unacceptable research results." Yet, much of the criticism of these studies relates not necessarily to substandard methods on the part of investigators, but rather to faulty conclusions about the data. Many of these conclusions were not even stated by the original investigators but rather by other scientists or even the news media; this factor is often beyond the control of the investigators. Considering the inherent difficulties which may be encountered in these studies, the investigators must be commended for their effort.

Time magazine (26 October 1992; "Danger Overhead") presented information on two scientific reports (that were unpublished in the peer-reviewed literature) dealing with the possible association between EMFs and cancer. The fact that these studies had not yet been peer-reviewed is an important point. With this in mind, it is not surprising that many unanswered questions about these results were raised at a meeting where results of the studies were presented (Annual Review of Research on Biological Effects of Electric and Magnetic Fields; San Diego, 12 November 1992).

Concerning the first study (Feychting, M. and Ahlbom, A., "Magnetic fields and cancer in people residing near Swedish high voltage power lines," IMM-Rapport, Stockholm, June 1992), the *Time* article stated that "cancer risk grew in proportion to the strength of the EMF" and that "such a clear progression makes it difficult to argue that factors other than exposure to the EMF were responsible for the extra cases of leukemia." These statements are misleading and not supported by the scientific evidence. The strongest effect was related to

proximity of single family homes to power lines and not to exposure *per se*. Thus, there could be some factor other than EMF confounding the results. As *The Lancet* [Anon., 1992] noted, the reported association was valid only for children who lived in houses, not apartments. Differences between single family dwellings and apartments could reflect non-differential misclassification. At least *Time* mentioned another major problem of the study: the small number of cases registered. The relative risk in homes closest to power lines was based on only three excess cancer cases (seven observed, four expected).

The second study (Floderus, B., et al., "Occupational exposure to electromagnetic fields in relation to leukemia and brain tumors. A case-control study," National Institute of Occupational Health, Solna, Sweden, 1992) was performed using well-documented and detailed methods. Despite the commendable efforts of the investigators, however, the study was still subject to the usual problems of a case-control study of occupational exposures, including: selection bias, recall bias, potential confounding, and occupational misclassification.

The Oak Ridge Associated Universities Panel on Health Effects of Low-Frequency Electric and Magnetic Fields [1993] has made additional comments relating to the two studies. One would assume that the authors of these studies would attempt to address some of the above questions before submitting the results to peer reviewed journals. One cannot assume, however, that *Time* magazine will print another story dealing with these questions. Reporting of study results by the media before scientific review is complete can lead to a confused public. As Entman [1993] has mentioned, "dissemination in the lay press bypasses... peer scrutiny and moves the newest and flashiest findings into the public domain." (Recently, the two reports were revised and published in the scientific literature, in Swedish [Feychting and Ahlbom, 1992; Floderus et al., 1992]. English translations were not available at this time.)

Well-performed cohort studies of EMF that have not been reported by *Time* include those of Sahl et al. [1993], which focused on hematopoietic cancers and brain cancer among utility workers, and of Schreiber et al. [1993], which dealt with cancer mortality and residence near electricity transmission equipment. Both of these studies were consistent with no effects of EMFs. (Savitz [quoted in B. Richards, "Southern California Edison study finds no workplace tie between cancer, EMF," *Wall Street Journal*, March 15, 1993] stated that "this moves my thinking a little bit in the negative direction.")

In a law review journal, Kaufman [1990] suggested that courts considering "EMF cases" should "not admit the scientific testimony because it is irrelevant and unnecessary and may tend to inflame the jury." He also proposed that courts should adopt the following rule: "Evidence of lost market value due to the fear of adverse health effects may be introduced

regardless of the reasonableness of fear." This approach, while lucrative to lawyers, would be of questionable benefit to society.

Previously, Brent [1985] had coined the term "litogen" (meaning a substance that does not cause malformations but does cause lawsuits). Unfortunately, EMFs may be a litogen. Mills and Alexander [1986] discussed the disturbing trend of legal decisions in medically-related lawsuits being based on evidence unacceptable by today's scientific standards.

Traffic Radar Units and Cancer

Several articles in *Law Enforcement News* suggested an association between cancer and the use of traffic radar units by law enforcement officers. Poynter [1990a] asserted that "in thousands of research experiments, it has been shown repeatedly that long-term exposure to microwave radiation and electromagnetic fields can have potentially devastating biological effects on the exposed organism." This claim is simply not true. Studies of effects of microwaves on the development of cancer have been poorly controlled or analyzed, or could not be replicated (e.g., see Roberts and Michaelson's [1982] critique of one such study). A review of these studies indicates that there is no conclusive evidence that microwaves are carcinogenic.

In another article, Poynter [1990b] noted that standards of Eastern European countries were considerably lower than those of the U.S. As Yost [1992] has explained, differences between exposure limits "may be largely due to different viewpoints used in setting standards. In Russia, exposure limits tend to be set below the level at which any observable biological effect is found; in the U.S., exposure limits typically are set below the level of any harmful biological effects (within a margin of safety)." In addition, it should be noted that the guidelines in Russia were intended to apply only in non-military situations [McRee, 1979]. It has been postulated that "the Soviets, in practice allowed exposure above their guidelines, since they knew that it was not seriously hazardous" [Sloney and Cuellar, 1992]. Furthermore, very recently, these guidelines were relaxed enormously. (Other aspects of invalid comparisons between Soviet and U.S. standards have been discussed by Osepchuk [1987]).

Regarding the exposure of embassy personnel in Moscow to microwaves, Poynter [1990b] accused the federal government of "ignoring any connection between medical difficulties and exposure to low-level microwaves"; he said that the government "went to great lengths to avoid any connection between embassy residents in Moscow and the microwave beam." In fact, extensive studies showed no association between microwaves and any adverse health effects in embassy employees [Osepchuk, 1990]. Pollack [1979] de-

plored the media response to this question.

Poynter [1990b] quoted a professor of electrical engineering as stating that "microwaves at sufficiently high power densities... could be harmful..." This statement is certainly true. The levels at which such damage would occur, however, are many orders of magnitude higher than power densities in the vicinity of traffic radars.

To characterize the use of traffic radars as "human experimentation without informed consent" was inflammatory and unscientific. There is no factual basis for this statement. The term "life-threatening forces produced by a traffic radar unit" was also irrational and unsubstantiated.

In another article [Anon., 1991], a police spokesperson was quoted: "If this is determined not to be a safety hazard, then I'm sure we'll go right back to radar." In fact, the use of the radars has not been determined to be unsafe. In addition to the Florida Highway Patrol, which was mentioned in the article, many other police organizations (including the San Antonio Police Department) have not seen any evidence prompting them to discontinue using radar devices.

Zaret [1991] mentioned "radiant energy" cataracts in relation to traffic radar. His research has been critically discounted by many scientists who have reviewed it. Hathaway [1978], for example, noted that Zaret "misrepresents his pet theories as established facts" and proposes a mechanism that is "biophysically impossible considering the energy levels of microwave radiation." Cataractogenesis due to microwaves is a threshold phenomenon and requires exposure to levels well above standards [Petersen, 1983; Shusterman and Sheedy, 1992]. Zaret's [1990] most recent abstract supposedly added "a new category of mutagenesis, pancreatic cancer in radio and radar repairmen," but contained no data in support of this assertion.

Clark [1991] questioned the credibility of the American National Standards Institute (ANSI); this challenge was spurious. The ANSI has relied heavily on advice from university and academic medical center researchers.

Milham was quoted by Clark [1991]: "I couldn't imagine that outside light could give you a tumor on the inside of the eye. Radiation could." Yet, radiofrequency energy at the frequencies encountered in the use of traffic radars (e.g., 24 GHz) would not penetrate to any depth within the eye.

Clark's [1991] idea that the EPA draft report was altered "under pressure from the White House" was unfounded. As I've mentioned before [Jauchem, 1990b], to imply that review of the document was driven strictly by political considerations, without involving scientific analyses, is misleading. In fact, the absence of both a mechanism of interaction and a dose-response relation do not support classification of EMFs as a probable carcinogen.

Fisher [1993] determined that when traffic radar units are

operated properly, the radar operators are exposed to levels of microwaves that are less than 1% of the maximum exposure level listed in current safety standards.

Concluding Remarks

As others have mentioned (e.g., Petersen [1983]; Foster [1992]), "effects" are not necessarily "hazards." The assumption that one automatically implies the other must be questioned. Although moderate-intensity ELF's may be capable of producing biological effects, the distinction between these effects and health effects is important. Reports of effects using *in vitro* systems do not make a strong case for carcinogenicity. If there is a true relationship between EMFs and cancer, then it is a very weak one. It is important to note that, in epidemiologic studies, an association of a factor with a health outcome often does not reflect a causal relationship [Davey Smith and Phillips, 1992; McCormick, 1992]. Davey Smith and Phillips [1993] have "shown — without any need to invoke extreme or unlikely circumstances — that strong independent associations can arise solely as a result of a lack of control over confounding." Skrabanek [1992] has suggested that some of these studies are really "scaremongering made respectable by the use of sophisticated statistical methods."

Case [1991] has noted that since environmental epidemiology is a branch of clinical medicine, it should, first of all, "do no harm." (As Marks [1993] has mentioned, "such an admonition should also apply to laws and regulations" relating to environmental issues.) The potential areas of harm include: "a confused public, a welter of litigation, a plethora of artificially created problems, and exponentially increasing choices for what to study in an era of decreasing real funding" [Case, 1991]. Others have reported that "the perception of an elevated cancer risk, in the absence of a true risk, may have a substantial negative effect on the affected community, both psychologically and economically" [Guidotti and Jacobs, 1993].

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