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COMMENTS ON "BIOLOGICAL EFFECTS OF POWER LINE FIELDS"[1]

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INTRODUCTION

While serving on the US Navy's Sanguine Study Commission in 1973, I was provided with scientific information that indicated a potential health problem associated with the electromagnetic fields from ultra-high voltage powerlines (HVPLs). Since the information was not classified, and since the New York State Public Service Commission (PSC) was then considering a request from the state owned utility for approval of such a line, I notified the PSC of this information and referred them to the Navy. The Navy refused to provide this data and the PSC requested my associate, Dr. Andrew Marino, and myself to serve as expert witnesses in public hearings on this question. The hearings began in 1976 and in their course I suggested that no such lines be built until a scientific study was done to confirm or deny the presumptive risk to the human population, and to quantitate it. The study was purposed to run for five years, be funded with a minimum of one million dollars per year, and be performed under the direction of an independent entity such as the Department of Health and overseen by a panel of outside experts. These suggestions were ultimately accepted by

[1]A report of the scientific advisory panel to the New York State Power Lines Project, issued July 1, 1987.

the PSC and, after legal challenges from the utilities and other delays, the study actually began in 1981. This article is a review of the final report of the study (Report). The full story of the public hearings may be found elsewhere (1,2).

From its inception, the study was flawed in a number of ways. The utility companies that funded the project assumed an unanticipated level of control over the program. It was evident from the beginning that the persons in administrative control had a preconceived bias that biological effects could not occur, and that no health problems would be found. Rather than fund research projects to determine the level of actual health hazards, projects that essentially reduplicated existing scientific work were funded. As a result, while the existence of major biological effects were substantiated, we are still unable to provide an accurate risk assessment regarding the electromagnetic fields to which a majority of United States citizens are exposed.

THE NEW YORK REPORT

The Report presents the administrative and operational details of the program, the results of the specific research projects directly funded, and reviews of the scientific literature on the subject. Unfortunately, the Report is neither a complete nor an unbiased scientific document. While the recommendations for further research are adequate, the failure to properly quantitate the health hazards to the human population prevents any assessment of the present risk. The recommendation that further research be conducted by a federal agency "which is credible by virtue of being clearly independent of partisan influence" neglects the past record of federal actions in this matter.

The Report demonstrates the same characteristics as many previous reports on the subject.

A) Studies showing positive bioeffects were subjected to a much more rigorous requirement for scientific validity than were negative reports, which were often accepted at face value.

B) Statements made about positive reports were frequently misleading and apparently deliberately designed to cast doubt upon the scientific validity of these reports.

C) Negative reports that had no scientific relationship or applicability to the question of the safety of 60-Hz fields were quoted in support of the overall contention that negligible hazards exist.

D) The Report included reviews of the pertinent scientific literature, but substantive and important positive scientific reports were omitted from mention. For example, the report by Liboff et al. (3), indicating a direct relationship between low strength ELF-VLF magnetic fields and DNA synthesis, was not referenced.

E) Recent positive reports of importance were not noted in favor of noting prior, less positive or preliminary reports from the same project even though the more complete report was issued prior to the completion of the New York study.

F) The Report does not contain the actual final reports or abstracts thereof from any of the project directors supported by the study. Each report is filtered through whoever authored and revised the Report.

G) Important details of projects such as type of field, intensity and duration of exposure were omitted from the Report.

H) The Report contains many statements amounting to actual distortions of fact that appear designed to influence the opinion of the reader in favor of a negligible hazard. For example,

- Page 1, "Most of the research studies reported no effects of concern. Of the few effects, some warrant further consideration. No effects were found on reproduction, growth or development." Many researchers who have followed this study and are familiar with the additional scientific literature available seriously question this entire statement.

- Page 4 (in discussing Winter's result with cancer cells), "Even if this observation were confirmed, however, extrapolation to the behavior of cancer cells in humans is not justified because behavior of cells in soft agar is not predictive of their behavior in the whole organism. Furthermore, there is no basis to extrapolate between growth of cells which are already malignant and initiation of promotion of cancer in the whole organism." Despite the awkward wording the intent of the statement is clear -- Winter's observation is of no concern whether

whether it is replicated or not. It appears self-evident that any agent that increases the growth rate or malignant characteristics of cancer cells in vitro may have the same effect in vivo. One simply cannot know until one does the experiment in a scientifically valid fashion.

The second sentence, in particular, is patently incorrect. "Promotion of cancer" means an increase in the growth rate of an already established cancer with accompanying greater difficulty in treatment and an accelerated clinical course. In order to "promote" cancer, one must start with cancer cells. Winter's (and other workers') data clearly indicated an increased rate of growth of malignant cells when exposed to such fields.

- Page 10, "The variety of behavioral and nervous system effects may not constitute a major hazard because most appear to be reversible, but they may impact temporarily on human function." The statement obviously neglects the effect of continuous residential exposure which should have been the primary objective of the study. Persons so exposed have little, if any, chance of reversal of the effect unlike the laboratory animals exposed for a finite time. It is noteworthy that several projects supported by the program identified neural system effects of importance which, if continued, would indicate major health effects.

- Page 54, "If damage is shown in laboratory studies of exposed cells in tissue culture, it does not necessarily mean that exposed humans will also show evidence of chromosomal damage. It would mean that further studies on whole organisms are needed. Conversely, if no chromosomal damage is detected in vitro these results are reassuring."

- Page 56, "Effects on isolated DNA were not pursued because ELF fields are not expected to produce enough energy to break DNA bonds and even if DNA damage occurred, there are interpretive difficulties in extrapolating to human health hazards." The implication is clear that such studies do not mean anything whether they are positive or negative. This is clearly a scientifically untenable position. The insertion of such statements into a purported unbiased report raises serious questions.

- Page 66, "In summary, proof of human fetal effects of electric or magnetic fields does not exist and such effects are

unlikely, based upon what information is available from studies of non-human organisms, and from knowledge of the mechanisms of actin of established teratogens. There are no standard epidemiologic investigations of the effects of electromagnetic fields on fertility and birth defects. The few studies purporting to examine these questions show no convincing deliterious association with exposure. Indeed it would be difficult to conduct appropriate epidemiological studies which would answer this question because of the very large samples that would need to be studied. Clearly defined endpoints would have to be determined in standard fashion to remove bias. Such studies, if done properly, are unlikely to be informative given the difficulties of obtaining 'controls' free of exposure in a modern population, or of estimating dose." The first sentence is true as written, however, it depends upon one's definition of proof, and further it ignores the fact that such evidence does exist in both the human population and in laboratory animals. Several studies indicate that such exposure apparently produces some genetic alterations which then result in developmental defects in subsequent generations. In the second sentence, the word "standard" is unusual and its exact meaning in the context of the paragraph is obscure. In the third sentence the truth depends again upon the definition of "convincing". Again the scientific literature is ignored and the reader is provided only with the opinion of whomever wrote the sentence. The fourth sentence is also "true" but implies that it would be "impossible" to conduct an adequate epidemiological study because of the population size involved. The final sentence implies that even if such a study was done it would not be "informative". Again, the Report implies that such studies would be of no consequence whether they were positive or negative. Most independent scientists knowledgeable in the field would challenge both statements. This is a poor excuse for the failure of the program to perform an adequate prospective study. The strong bias towards downplaying the potential hazards is clearly evident in the entire paragraph. Such statements have no place in a purported scientific document.

The Report is not scientific and it shows a clear and unmistakable bias in the direction of minimizing the real risks that were identified by the project itself, or which were extant in the scientific literature. One can only interpret the Report

as an attempt to influence public opinion in favor of the industry continuing and expanding its operations while "studying" further the "potential risks". Further, the nature of the Report issued casts serious doubt upon the integrity of the project itself. While the Report clearly states that "The chapters of this final report were prepared by individual Panel members in their areas of expertise, and reviewed and revised by the entire Panel", the uniform bias noted above would seem to indicate that revision by a few individuals was extensive, or, far less likely, that all panel members shared the same viewpoint.

PANEL MEMBERSHIP AND ACTIONS

The Report states that the panel members were selected for their "outstanding reputations" and "professional expertise". A brief review of the panel members and the two specifically selected consultants (Trent and Buick) who reviewed that part of Winter's project performed "outside of his contractual workscope", indicated that of these 10 individuals only 3 (Albert, Marron and Shelanski) were listed in American Men and Women of Science, and none were listed in the latest bibliographic data base, Who's Who in Frontier Science and Technology. Under the guise of impartiality, no scientists who held the position that health effects were possible were included on the panel.

The Report also indicates the responsibilities of the panel to oversee the progress of each of the contractors, in particular "Panel members were expected to follow the assigned projects closely, to participate in site visits as often as needed and to request consultant assistance as needed." Rough calculations based upon the data provided are interesting. Of the total number of "man-visits" to contractors' laboratories, more than twice as many visits were done by non-members than by Panel members. Further, the division of this responsibility among Panel members was non-uniform, with only 3 members (Marron, Martin and Albert) performing approximately 57% of the total number of site visits and with one member, Marron, performing approximately 27% of the total Panel member site visits. One Panel member, Wolpow, apparently performed only one site visit

during the entire project and Carpenter (director of the project) apparently made only one site visit.

CRITIQUE OF PROJECTS FUNDED

Projects funded by the program were in the nature of confirming prior studies indicating that electromagnetic fields had bioeffects, rather than being designed to determine the extent of their risk to the human population. The bias of the program is evident from the statement in the report, "A review of the literature existing in 1981 on genetic, chromosomal, teratogenic and reproductive effects of 60 Hz electric and magnetic fields revealed no unassailable proof of induction of these types of biological damage." This fallacy of requiring "unassailable proof" is well addressed by a recent editorial (4). "Science is a hard taskmaster, and, in the light of mounting evidence that suggestions of toxicity are for the most part ultimately confirmed by painstaking scientific inquiry, perhaps it is time to reexamine whether scientific standards of proof of causality -- and waiting for the bodies to fall -- ought not to give way to more preventive health policies that are satisfied by more realistic conventions and that lead to action sooner."

The scientific literature on bioeffects was completely reviewed in 1977 (5). This paper includes reviews of 122 citations available at that time, and numerous other reports were published between 1977 and the start of the program in 1981. Scientifically, the problem was not one of "are there bioeffects of such fields?", but rather "what are the health hazards associated with their exposure?". The panel chose to answer a question that already had been answered rather than the one posed during the hearings.

The funds and time available to the panel were more than adequate to support a full scale, prospective epidemiological study of populations exposed along the routes of HVPLs. When asked why this was not done, Dr. David Axelrod, Commissioner of Health, State of New York, answered that the population group in New York exposed to HVPLs was too small to support such a study (6). However, at the time, there were several thousand miles of such lines operating in the Mid-West. Furthermore, since it was

not the specific HVPLs themselves that was in question, but the electromagnetic field emitted by them, many thousands of miles of 345-kV HVPLs producing an identical field were available within the State. The panel was under no constraints to use only New York State residents, and in fact, one epidemiological study funded by the panel (a replication of Wertheimer's study) was performed in Colorado. It is interesting to speculate why a repeat of Wertheimer's study was done at a cost of $355,905 (Wertheimer did her study with basically no support), rather than to fund an adequate prospective study.

At the time the study was begun there were valid concerns expressed over the potential of field exposure causing or promoting the growth of cancer cells. In addition to Wertheimer's epidemiological study published in 1979, several other pertinent studies were published. Fukada et al. (7) reported a 200% increase in growth rate of murine osteosarcoma exposed to low-level direct current. Becker (8) reported a 300% increased growth of human fibrosarcoma under the same conditions. Liboff (9) reported that ELF magnetic fields stimulated DNA synthesis in normal human cells. Akamine (10,11) reported that electromagnetic fields at 100 Hz produced a 100% increase in rate of growth and increase in malignant characteristics of embryonal carcinoma cells. Considering the fact that tissue culture of human cancer cells was a well established technique, and that such cultures can be easily and cheaply exposed to controlled 60-Hz fields, the failure of the panel to establish and fund such studies is highly questionable. When one of the contractors (Winters) did such a study by "piggy-backing" it on his originally funded study and announced a marked enhancement of cancer cell growth, the panel responded by attempting to discredit the study.

REPRODUCTION AND DEVELOPMENT STUDIES

The program supported two *in vitro* and one *in vivo* study. The Report provides no details of any of these studies other than to state that they were negative. No effects on chromosomes and no effects on cell cycle time were reported. One study in which animals were exposed for several generations revealed no effects on growth or development. We are provided

with the endpoints used in analysis in this study; frequency of dominant lethal gene mutations, litter sizes, postnatal weights, and induction of recessive lethal mutations on the X chromosome as inferred by sex-ratio deviations. No anatomical dissections were performed, and the incidence of non-lethal developmental or genetic defects is not known and was apparently not studied.

In the case of cellular effects, such as chromosomal or cell-cycle time, no literature review was included to provide an idea of the relationship of these negative reports to the rest of the scientific literature. This is interesting in view of the fact that a number of significant reports were available. Goodman (12,13) reported significant effects on DNA-RNA activity and d'Ambrosio (14) reported significant chromosomal abnormalities. In a latter section discussing chromosomal abnormalities reported in occupationally exposed humans, a most significant paper, by Nordenson (15) is not mentioned. In this study Nordenson reported that peripheral lymphocytes from electric switchyard workers demonstrated significantly more chromosomal abnormalities than controls. In regard to cell cycle time a number of significant reports were not mentioned. Akamine (11) reported promotion of the growth rate and enhancement of malignant characteristics of embryonal carcinoma cells, and Liboff (3) reported increased DNA synthesis in human fibroblasts. The relationship between induced chromosomal abnormalities and cancer has lately become a significant question (16). This failure to adequately review the literature in regard to cellular effects of time varying electromagnetic fields, particularly chromosomal effects and cycle kinetics, is inexcusable.

The literature review of growth and development in general is indicative of the bias throughout the report. For example, on page 58, the reports of Marino et al. (17,18) were incorrectly reported, "body weights were also measured but no consistent effects were found." The report by Marino clearly indicates that body weights of both male and female animals were significantly depressed in both the first and second generation in both the horizontal and the vertical fields. In the third generation only the male body weights were significantly reduced in only the vertical fields. Therefore, of 12 endpoints, 9 were found to be statistically significant with all indicating lower weights. If one defines "consistent" with "always" or "in all

cases" then the statement in the report is true. However, in scientific research on living systems the innate variability of such systems is assumed to mitigate against such definitions. In fact, reports claiming absolute consistency are always viewed with skepticism. The report also states that Marino's results have not been confirmed; this is incorrect. Noval et al., working in a Navy supported project reported similar effects with even lower intensity fields (19). A paper by Mathewson (20) claims to have made no such observation. However, when the Mathewson report is analyzed it is apparent that the circumstances were not similar to Noval's study and in actuality, Mathewson can be considered to confirm Noval (5). The listing of scientific reports that contradict Marino et al. contains a citation for Konerman and Monig, who "found no developmental effects in rats exposed to fields." In fact Konerman and Monig (21) examined the possible effects of NMR imaging on pregnant mice. Since neither ELF frequencies, nor 50-60 Hz fields are used in NMR, this report has absolutely no relationship to the question of developmental effects of 60-Hz fields. The reader should note that the wording in the report is correct as it stands, but can only be ascribed to a deliberate attempt to mislead the reader.

The report discusses the Battelle study (an ambitious, multi-generation, multi-year, multi-dollar mammalian study designed to "replicate" the Marino study) in one paragraph only ten lines long. It also incorrectly reports the findings as "an incresed incidence of fetal malformations in the second but not in the third generation" with the implication that malformations occurred in only one of three generations. In fact they occurred in two of three generations. Further, the report, apparently quoting from a Battelle report, states, "the change in incidence of malformations between generations make it impossible to unequivocally conclude that there was a cause-and-effect relationship." Again, the report is guilty of selective quotation -- the latest published report (22) contains the identical quote but it is preceded by the statement, "There appears to be an association between the chronic exposure to a strong electric field and developmental effects in swine." The average reader not having access to the original data would conclude from the Report's review of this important study that it was essentially

negative. The Report's handling of this matter is not only non-scientific and non-factual, but egregiously misleading as well.

Finally, the Report devotes exactly the same amount of space to a destructive review of a single paper by Cameron (23) on the development of fish eggs in a 60-Hz field. It would appear obvious that the two projects are in no way of equivalent importance and one can only question the intent of the panel report's treatment of this matter. While other reviews of the literature on growth and development in the Report may be subjected to the same criticisms (particularly the treatment of Delgado's work), these comments should suffice to indicate the defects in this portion of the report.

CANCER

The Report devotes four and a half pages to a discussion of the work by Phillips and Winters. The discussion centers about the clonogenicity (an expression of the growth rate) of the cells, and attempts to discredit their work. In several instances claims were made that the data lacked significance, that only a few experiments were positive, that there was great experiment-to-experiment variability, that this might be attributable to "lack of rigid quality control", and that the origin of the cells was unknown. These statements may be refuted by anyone who takes the time to read the published works. While two of the three papers published are listed in the bibliography of the Report, the wording and statements in the Report bear little relationship to them. The data presented in these papers clearly indicates that the human tumor cell lines used (which were obtained from the ATCC) demonstrated significant increases in colony formation following exposure to magnetic fields alone or in combination with electric fields, and that this observation was replicable. The report totally ignores several other extremely important observations. First, that a single 24-hour exposure resulted in apparently a permanent alteration in the cells, with the increased clonogenicity still being evidenced months later after such exposure. Second, that such exposure altered the expression of transferrin receptors on the cells such that the total number of these receptors was

equal to or exceeded the theoretical limiting number. The importance of this latter finding is evidenced by the work of Larrick and Cresswell (24,25) which reported a relationship between such receptors and the rate of growth of the cancer cell and even the clinical outcome. The Report does not even list Phillips' latest paper (26) which reported a decrease in the ability of NK cells to lyse the tumor cells that had been exposed to the fields. Further, while extensive use is made of the reports by the Panel's "outside experts", there is no mention of Dr. Winters' vigorous refutation of them which was sent to the panel. Finally, much is made of the failure of Cohen and Hamburger to replicate these observations. It is apparent from the report itself that the experiments were not identical to those done by Phillips and Winters. The Report ignores the report by Akamine (11), which confirmed the growth effects of Winters and Phillips, using a different cancer cell line.

In summary, the Report focused on only one finding, the increased clonogenicity of the exposed cells, and attempted to discredit it with fallacious reasoning. The other findings, of equal or even greater importance, were not even mentioned. In my opinion this "review" of Phillips and Winters work by the Panel represents an inexcusable breach of scientific principles, and can only be viewed as an example of scientific disinformation.

Virtually all of the Report may be criticized on the same basis.

RECOMMENDATIONS

The report contained 6 recommendations for further research. The majority were directed towards determining mechanisms of action, thresholds of effects and replicating some present studies. Replication or extension of *in vitro* and *in vivo* promotion of cancer is noteworthy by its absence. A recommendation for the development of animal models for "field induced carcinogenesis" is mentioned but it neglects to specify chronic exposure. In practical terms, the promotion of cancer may be considered more important than the induction of cancer in view of the multiple carcinogenic factors already present in the

environment. The only recommendation for epidemiological study is, apparently, for further replication of the same type of study as previously reported. There is no mention of a large scale, prospective epidemiological study (dealing with growth, development and neurological effects in addition to cancer incidence and characteristics) on the very large population group presently exposed to HVPLs in this country. It must be noted that many people are presently exposed, under residential circumstances, to 60-Hz fields within the field densities reported by Wertheimer and confirmed by Savitz to be related to childhood cancer. There is no recommendation for reducing the present exposure of this population group, nor is there any recommendation for mitigating further expansion of HVTLs or other facilities which will result in the exposure of additional significant population groups. There is no recommendation for further evaluation of effects upon growth and development despite the significant findings reported from the Battelle study. Finally, the recommendation was made that further research be the responsibility of some federal agency with no ties to parties of interest. This latter ignores the record of prior actions taken by federal agencies in this regard. While concerned with exposure to microwave radiation, the actions taken by several such agencies in the situation in Vernon, New Jersey is instructive (27).

The Report is not a scientifically valid document, but rather appears to be an example of scientific disinformation designed to influence public opinion towards the belief that few, if any, hazards exist and that the only steps necessary are further studies. It is this reviewer's opinion that evidence for significant hazards to major segments of the population has been identified by both the New York study and other investigators, and that while further study is definitely required, it must be aimed primarily at elucidating the level of risk to the general population. It is imperative that this step be taken as soon as possible, and that final risk-benefit determinations be made by the population at risk, rather than by engineers, the utilities themselves, or agencies of the Federal Government that are parties of interest.

REFERENCES

(1) Schiefelbein, S.: The Invisible Threat. The Stifled Story of Electric Waves, The Saturday Review, September 15, 1979.

(2) Marino, A. and Ray, J.: The Electric Wilderness, San Francisco Press, San Francisco, 1986.

(3) Liboff, A.R., Williams, T. Jr., Strong, D.M. and Wistar, R. Jr.: Time Varying Magnetic Fields: Effect on DNA Synthesis, Science 223, 818-820, 1984.

(4) Ashford, N.A.: New Scientific Evidence and Public Health Imperatives (Editorial), N.Eng. J. Med. 316, 1084-1085, 1987.

(5) Marino, A.A. and Becker, R.O.: Biological Effects of Extremely Low Frequency Electric and Magnetic Fields: A Review, Physiol. Chem. Phys. 9, 131-147, 1977.

(6) Axelrod, D.: Rome Daily Sentinel, October 27, 1982.

(7) Fukada, E., et al.: Paper #8, Japanese Committee of Electrical Enhancement of Bone Healing, 1981.

(8) Becker, R.O. and Esper, C.: Electrostimulation and Undetected Malignant Tumors (Letter to the Editor), Clin. Orthop. 161, 336-339, 1981.

(9) Liboff, A.: BRAGS paper #6, 1982.

(10) Akamine, T.: Paper #10, Japanese Committee of Electrical Enhancement of Bone Healing, 1983.

(11) Akamine, T., Muramatsu, H., Hamada, H. and Sakou, T.: Effects of Pulsed Electromagnetic Field on Growth and Differentiation of Embryonal Carcinoma Cells, J. Cell Physiol. 124, 247-254, 1985.

(12) Goodman, R., Bassett, C.A.L. and Henderson, A.S.: Pulsing Electromagnetic Fields Induce Cellular Transcription, Science 220, 1283-1285, 1983.

(13) Goodman, R., Abbott, J., Krim, A. and Henderson, A.S.: Nucleic Acid and Protein Synthesis in Cultured Chinese Hamster Ovary (CHO) Cells Exposed to the Pulsed Electromagnetic Fields, J. Bioelectricity 4, 565-575, 1985.

(14) d'Ambrosio, G., Scaglione, A., DiBerardino, D., Lioi, M.B., Iannuzzi, L., Mostacciuolo, E. and Scarfi, M.R.: Chromosomal Aberrations Induced by ELF Electric Fields, J. Bioelectricity 4, 279-284, 1985.

(15) Nordenson, I., Mild, K.H., Nordström, S., Sweins, A. and Birke, E.: Clastogenic Effects in Human Lymphocytes of Power Frequency Electric Fields: In Vivo and In Vitro Studies, Radiat. Environ. Biophys. 23, 191-201, 1984.

(16) Yunis, J.: The Sciences, November-December, 1985.

(17) Marino, A.A., Becker, R.O. and Ullrich, B.: The Effect of Continuous Exposure to Low Frequency Electric Fields on Three Generations of Mice: A Pilot Study, Experientia 32, 565-566, 1976.

(18) Marino, A.A., Reichmanis, M., Becker, R.O., Ullrich, B. and Cullen, J.M.: Power Frequency Electric Field Induces Biological Changes in Successive Generations of Mice, Experientia 36, 309-311, 1980.

(19) Noval, J.J., et al.: Compilation of Navy Sponsored ELF Biomedical and Ecological Research Reports, Vol. III, Naval Medical Research and Developmental Command, Bethesda, NTIS No. AD A035955, January, 1977.

(20) Mathewson, N.S.: Compilation of Navy Sponsored ELF Biomedical and Ecological Research Reports, Vol. III, Naval Medical Research and Developmental Command, Bethesda, NTIS No. AD A035955, January, 1977.

(21) Konerman and Monig: Radiologie 26, 490, 1986.

(22) Sikov, M.: Bioelectromagnetics 8, 229, 1987.

(23) Cameron, I.L., Hunter, K.E. and Winters, W.D.: Retardation of Embryogenesis by Extremely Low Frequency 60 Hz Electromagnetic Fields, Physiol. Chem. Phys. Med. NMR 17, 135-138, 1985.

(24) Larrick, J.W. and Cresswell, P.: Modulation of Cell Surface Ion Transferrin Receptors by Cellular Density and State of Activity, J. Supramolec. Struct. 11, 579-586, 1979.

(25) Kvaloy, S., Langholm, R., Kaalhus, O., Michaelson, T., Funderud, S., Foss Abrahamsen, A. and Godal, T.: Transferrin Receptor and B-Lymphoblast Antigen -- Their Relationship to DNA Synthesis, Histology and Survival in B-Cell Lymphomas, Int. J. Cancer 33, 173-177, 1984.

(26) Phillips: Immunol. Lett. 13, 295, 1986.

(27) Becker, R.O. and Becker, A.J.: An Analysis of the Effectiveness of Regulatory Agency Responses to a Situation Involving Perceived Health Effects from Microwave Radiation, J. Bioelectricity 5, 229-251, 1986.