Glaser

Comments and Suggestions Regarding the ANSI C-95.4 Subcommittee Report on Research Needs for Radiofrequency and Microwave Standards Setting*

by Bill Hauk, Beischer(?), (and others?)

*Referencing system for report critique: the page of the report is given and the line number on that page, as numbered from the top line consecutively.

Title — Suggest correction to use "Radiofrequency and Microwave Radiation" vice "Electromagnetic Radiation." The latter refers to the whole electromagnetic spectrum whereas the committee has responsibility only for radiofrequency and microwave portions of this spectrum.

Regarding the use of the term "electromagnetic" throughout the report, we strongly suggest that the committee use "electromagnetic" only when referring to the entire electromagnetic spectrum. In some instances, particularly in the sections where other electromagnetic radiant energies are included in the discussion, to use "electromagnetic" as the author(s) use it is overtly incorrect. It may be that the authors wish to emphasize the electric- and magnetic-field nature of microwave and radiofrequency radiations, but we feel that this is not the correct way to do it. This error in usage occurs so frequently, we will refer to it only once.

Preface P. 1, L 11 — "This standard was formulated primarily from research results on tissue heating considerations." In reference to the origins of the U. S. Standard, this statement is not really true. The standard originated from a calculation based on maximum and minimum metabolic heat production by the body of an "average" 70 kg man, and the amount of microwave radiation that could cause, or not cause, a whole-body temperature rise. Interestingly, Schwan came up with a figure of 100 mW/cm^2 for the U. S. Navy that was used for a while, and the number was later reduced to 10 mW/cm^2 using both maximum body heat production characteristics, and the minimum microwave energy input that could be tolerated without elevating the body temperature. It is also based on the fact that in our limited occupational surveys no particular deleterious effects had been observed

in personnel exposed to these radiant energies. Standards of neither the Soviet nor the U. S. are based primarily on research data per se, but are based principally on a continuing accumulation of occupational health survey information and applied and basic research information.

P. 2, L 4 Suggest the addition of the word expand to the phrase "...modify, refine and improve the present ANSI standard."

Definition and Classification of Terms

Thermal versus Nonthermal Effects

P. 4 The subcommittee may want to look very closely at these definitions. They do lie at the root of much controversy. Perhaps an attempt should be made to correct semantic problems and treat the whole problem with rigor.

Low-level fields definition:

P. 4a We suggest omitting the "arbitrarily chosen" 10 mW/cm^2 effective power density, unless accompanied by the phrase "in man." These contentious divisions of incident energy rate of exposure are not true or uniform for all species.

We suggest adding definitions to the glossary of the following: hazard, safety, and biological effect. The usage of these terms by the author(s) of the report indicate this is necessary. Specific examples noted by us will be given as the critique progresses.

In general, we suggest the committee advocate the use of international units wherever possible to ensure commonality of usage and concept in this complicated field of endeavor.

P. 4a & Section called "Synonyms."

P. 5 The listing as presented is somewhat confusing as is later manifested by the author(s)' use of these terms in the sections on dosimetry and instrumentation.

Units of Incident Energy	Units of Absorbed Energy
The measurement of this is called densitometry.	The measurement of this is called dosimetry.
I. EXPOSURE RATE (Time is inherent in the expression)	I. (ABSORBED) DOSE RATE (Time inherent in the expression)
Energy fluence rate	Absorbed power density
Energy flux density	J/kg/sec
Power density	W/kg
Intensity	
W/m^2, mW/cm^2, $J/cm^2/sec$	
If one multiplies exposure rate by time, one gets exposure parameters.	
II. EXPOSURE	II. (ABSORBED) DOSE
Energy fluence	Absorbed energy
Energy incidence	J/kg
J/m^2	

Listing these terms in this suggested manner is quite necessary for proper usage of terminology and units. If the committee desires to define watts (W), joules/second, joules, etc., this should be put in the glossary and not in this section.

Section on Biological Effects

P. 5, L 16-19 "The power deposition manifests...." Power deposition is manifested by local and general tissue heating (not "manifests itself in terms of localized tissue heating.") This heating is redistributed dynamically throughout the biological organism by the circulatory system under direct and indirect control of the thermoregulatory system (not, "which is redistributed throughout the biological system according to the thermal regulatory system").

P. 6, L 3 "...Total EM absorbed power in the body approaches metabolic power output, the body senses a thermal load." "Sensing" occurs long before a "load" is present. Load implies stress, therefore the term "input" is preferred. It is not a "load" until extraordinary compensatory mechanisms are invoked by the body. At what point this "sensing" of microwave power input occurs with a resulting regulatory response is not precisely worked out, nor are all the mechanisms.

P. 7, add to L 2 You may wish to amplify these considerations by considering the effects of low temperature changes in specific temperature-sensitive structures, thus producing profound alterations in normal physiological functions. Also, the rate of temperature change and/or the magnitude of rise achieved in specific sensitive structures has not been thoroughly investigated. Examples: McAfee's work; sensory afferent system of the body responding to superficial heating; the response of local heat production to thermosensitive structures and areas in the hypothalamus and midbrain nuclei. In the light of recent articles by Kritikos and Schwan, Shapiro et al., Guy and Johnson and Guy concerning energy deposition and distribution in spherical multilayered objects like the skull, these considerations do become quite important.

P. 7, L 10 "Although recent experiments indicate possible effects...adverse consequences due to heating...." This is the first instance of using the word "effects," and it appears in the same sentence with a discussion of adverse consequences. Though the author(s) does not seem to make the erroneous jump from an effect due to microwave radiation (biological response) to damage to the organism, neither does he make nor has he previously made, a clear-cut distinction between normal responses, and effects that are deleterious to the organism.

P. 7, L 14-26 Must be revised. Virtually every statement is erroneous. While it is true that tissues with poor circulation cannot compensate so well to local heating, the listing of gall bladder and gastrointestinal tracts as sensitive due to the relative absence of vascularity is inaccurate. These two structures are among the more vascular in the body. The testes, which have an excellent blood supply, are functionally impaired by maintenance at normal body temperature (37 to 37.5° C) in many mammals. If this condition is maintained for long periods of time (many months to years), oligospermia and atrophy eventually result. A controllable temperature increase, 29 to 37° C, dependent on intensity used, occurred in Sprague-Dawley rats (Imig et al., 1948) when they were exposed to 2450 MHz CW microwave illumination. Degenerative changes were observed in 50% of the testes examined histologically, where central testicular temperature as measured by an implanted thermocouple was raised from 29° to 31-35° (ΔT = 2 to 6° C).

The statements regarding the testicular pathology, reported by Imig and his co-workers, contained in the report are not precise and could be misconstrued. The species involved was not mentioned, and "as little as 35° C" is actually representing an experimental

increase in ΔT° C of 6° over a 10-minute period. Human subjects, when loosely clothed, maintain about a 2° C (average) temperature differential between the scrotal sac and the body core. With a normal body temperature of 37.5° C, 2° C less is 35.5° C. If the numbers in the report are used without mentioning species, this means that human subjects should show damage under normal conditions.

The globe of the eye has an excellent blood supply via the optic foramen which supplies various extrinsic and intrinsic structures of the eye itself, with the exception of the lens of the eye. The report indicates that it is the eye (line 22) that "has a very poor circulation."

Certain hollow body organs (gall bladder and GI tract) are rendered vulnerable to microwave heating because heating of their luminal contents (air/fluid) is difficult to dissipate. The walls have an excellent blood supply, but this cannot cool the contents of the organ. This problem is amplified by the long storage/transit times of these organs. Other "hollow" structures such as the trachea and heart are not so vulnerable because their contents are moved very rapidly. The report does not make these distinctions.

P. 8, L 1-3 Reference this statement that subthreshold exposures to microwave radiation "will not produce cataracts no matter how long the field is applied."

P. 8, L3-6 "Cataractogenesis is...and it is thus not dependent on the total radiation dose received by the eye." While the conclusion as stated is apparently correct, at least insofar as current knowledge indicates, it probably is incorrect to state it this way. Dose in this sense is subject to confusion since most data on this are related to exposure rate (mW/cm^2). Dose and dose rate are not known precisely because of lack of detailed information on absorption parameters.

There are data, however, that would allow some estimates of absorbed energy to be made.

P. 8, L 6-8 Without realizing it the author(s) has just contradicted himself in a sense. He just spoke of cataract production as a threshold phenomenon, in spite of how long the field is applied, and now he introduces a statement about how it is unknown about the effects of lower incident power densities on cataract formation at a much later time. This statement deserves a paragraph in itself or would be better if deleted entirely. A reader has no way of interpreting what they mean; low exposure rate, interrupted or continuous exposure, one exposure, or what. There is considerable occupational information on this as well as some laboratory data.

P. 8, L 11-12 "Direct and indirect effects on the CNS...." Since the author(s) has chosen to reference this Soviet viewpoint, he is going to have to develop it further. What the Soviets view as direct and indirect influences on the CNS, as well as the resultant effects, has no real Western counterparts.

P. 8, L 12 "This literature reports...." This would be better stated if the word "reports" were changed to "reflects their view that the CNS is the most sensitive structure to microwave radiation." It could also be mentioned that they take this stand in all of their biology and medicine for the effects induced by all physical and chemical agents.

P. 8, L 19 The reference to Gordon (author(s)' number 16) should be Tolgskaya and Gordon.

P. 8, L 19-21 "It is unfortunate that...are not known...." This could be amplified.

engineering considerations	restraint techniques
measurement techniques	physiological and chemical methodology
exposure levels	statistical analysis of data
animal handling techniques	etc.

These are often not reported in their literature. The committee might want to insert here, that in spite of the obvious voluminous Soviet literature on the biological effects of microwave and r-f radiation, their mode of presenting data is quite unacceptable by Western standards.

P. 8 to P. 9

"Very little of this type of data...for two reasons:...." The principal reason that we have generated so little data of this type is because we simply have not seen the effects that they report. The question has been asked as far back as 1947, through the late 50's and early 60's (Tri-Service Conferences). We still come up with zeros. The author(s) would be well advised to consider current reactions of the medical community to presentations on this subject. This community is impressed by the absence of consistent findings in exposed personnel that could not be attributed to normal aging, other pathological processes, and the like. Early investigators most certainly did not assume "that the only hazardous effect" was tissue heating. It was the only effect that had meaning in an organopathological context. This status still holds true for many who look at the problem. By the way, this is an improper use of the term hazardous. A hazard represents a state or condition of threat to health and safety. More precisely "detrimental or deleterious" could be used here. The second reason for the lack of research data is not a reason, really. The lively debate that is continuing over "thermal versus nonthermal" effects is probably a stimulus for much of the work that is going on. It didn't hamper work in the past, just the interpretation of it. A reason the committee might want to consider is the problem of "proving" a negative result. Microwave biological effect research has been hampered by the possibility of negative results, something that no researcher enjoys, and the reluctance of funding agencies to spend money for a higher probability of negative results. One of the authors of this critique (Houk) thinks this is one great reason for our lack of data on low-level, long-term studies. Higher power densities cause startling and interesting biological phenomena. Moreover, most health- and safety-applied investigation begins with threshold and tolerance level studies and acute phase studies.

P. 9, L 18-20

"Effects similar to these can cause denaturation or coagulation of biological molecules as has been confirmed experimentally by Fleming, et al."[18] This is a misrepresentation as the original authors pointed out recently in J. Microwave Power, 8(1):111-116, 1973) (McAfee, Braus, & Fleming).

Schwan's statement regarding dielectric saturation effects is simply a hypothesis and remains to be confirmed. Note, also, that Schwan made this statement as far back as 1958.

P. 9, L 20-22 Of what import would the "well-known phenomenon of resonance absorption in molecules and relaxation effects in biological tissues..." have, especially with respect to research involved in standard setting? Simply listing effects that have occurred or are postulated is somewhat specious and of little value unless they are discussed regarding their meaning and possible significance.

P. 9, L 22-24 In this experiment of Lords et al. involving an isolated amphibian (frog) heart which is a modified three-chamber heart with a unique conduction system, the significant point to be learned is that an isolated, artificially perfused muscle pump preparation can have the usual degradation rate of its beat increased by exposure to microwave radiation. This means, if it appears in a normal intact frog, possibly, that slowing of the heart rate has occurred. What it means relative to effects and hazard analysis in an intact human subject will remain indeterminate. This is not true bradycardia since the only thing the curves that Lords et al. have presented are intrinsic rates of decay of the frog heart, not intrinsic heart rates per se that stabilize at a certain value. Unless one plans to microwave-illuminate a human subject during a left thoracotomy, it has no real-world counterpart or significance. We are not aware that the presentation delved into the question of neuro-transmitter substances on the isolated frog heart preparation, at least as presented in the Symposium Proceedings referenced. Neither do the authors claim a nonthermal effect.

P. 9, L 25 to P. 10, L3 Last sentences: All these effects reported should be referenced.

Generally speaking, the effects listed in paragraph 2, pages 9-10 are of contentious origin, apparently due to interactions not requiring the heating of the organism in question (which some call "nonthermal"). Also, mention of the species involved and whether the effect is acquired in vivo or in vitro are extremely important.

P. 10, L 18 The "Kerr effect" referenced? It should be described whether it has ever been observed in biological systems or materials as such. If not, it should be deleted.

P. 10, L 20-23 What are the data and responses so important in the Tobacco Mosaic Virus at E field intensities of <5000 V/m? What are the references to these effects? What similar effect apparently can be produced with magnetic fields and what is its reference?

P. 10, L24 "very sensitive differentiating cell, as found in developing embryos." Is the author(s) sure that he means "differentiating" cell, and not dividing cell? This should read: "very sensitive dividing (not differentiating) cell, and tissue/organ differentiation as found in developing embryos."

P. 11, L 2 "...and in chromosomes." Does this report talk about chromosome damage (aberrations) or actual gene alterations (mutations)? Check reference 39 also; we believe the title is in error.

P. 11, L 3-5 The author(s) should make a clear distinction in his own mind about genetic and somatic effects of microwave and r-f radiation. The last sentence should read "...due to the sensitivity of the cell during fetal (sic should be embryonic stage or embryonal) with malformation and genetic (sic developmental) consequences." The alteration of a population's gene pool as a result of embryonic radiation exposure has not been worked out. To be sure more research in this area is needed, but great care should be exercised in applying the criteria of an effect produced in flies, plants, birds and other species to man for purposes of hazards evaluation and standard setting. This is a carry-over from ionizing radiation and the teratology/genetic problems associated with the use of various pharmacologic agents. In spite of enormous efforts in these areas, cause-effect relationships are still in an indeterminate stage.

Field Variables

P 11, L 21-23 "The square of the tissue electric and magnetic fields, E_t^2 and H_t^2 generate forces in the tissues,...." Is this an appropriate way to say this? Or is it more proper to say, "Forces in the tissues exposed to an incident radio frequency field are related to the square of tissue electric and magnetic fields, E_t^2 and H_t^2."

P 12, L8 The word "radiofrequencies" should be microwave frequencies.

P 12, L 9-10 "...skin effect protection mechanism...." This should be changed to say, "microwave frequencies higher than 10 GHz are essentially absorbed completely in the skin layers." This energy exposure causes changes in the skin, just like any other tissue, and if certain levels are exceeded, the skin can be harmed like any other tissue (burning, etc.) so,

"protection mechanism" is, perhaps, an ill-advised use of the term.

P 12, L 11 "Protection also exists...." What "protects" what from what? This needs to be amplified.

P 12, L 13 "The greatest concern...." We suggest adding the term "current" concern. We have little biological information at lower radiofrequencies.

P 12, L 18 "For example, very little relative energy...." We suggest changing this to read "...relatively very little energy...." If no change is made, my question is, what is relative energy?

P 12, L 24-27 Definition of radiation field? Lines 25-26 phrased in this way seem to contradict the inverse square law. We think there are better ways of describing the radiation field or the near field. This is very confusing.

P 13 In the description of the near field, we believe some discussion of the relationships and indefinable characteristics of the E and H fields would be of benefit, since these are the relationships that make the near field differ from the far field.

Dosimetry and Instrumentation

P 14, L 13-15 We would say that knowledge of the amount and distribution of radio frequency and microwave energy deposition is still quite rudimentary.

P 14, L 17-20 "The investigation of long-term, low-level radiation effects, which may ultimately lead to a modification of the electromagnetic (sic) radiation protection standard (sic, guide), will require a well understood and quantified body of dosimetric knowledge." The term "dosimetry" as it is used throughout the paper is the origin of quite a bit of

confusion which is also related to the author(s) use of terms such as "dose" and "absorbed power density." If by dosimetry the author(s) includes both measurement of exposure, exposure rate and dose and dose rate, then no issue is really taken by us with what is contained in the previous quotation from page 14. However, as the author(s) uses the term in the whole report it seems that he is referring to the measurement of what energy is absorbed only. Our preference is to refer to exposure and exposure-rate measurements as densitometry, and we indicate the units in question in our suggested amendment to page 5. We reserve the term dosimetry for the measurement of dose and dose rate (or energy absorbed integrated over time), as well as knowledge of energy distribution and absorption patterns within tissue. From the text, we repeat, we believe the author(s) is tending to use the latter meaning for the most part, though he does not make this distinction clear. More will be developed on this issue as the points come forth in the report.

P 14, L 21 to P 15, L 19 "...dose and absorbed power density..." This is quite confusing. Are these concepts contrasting, opposites, similar, or what? According to the definitions on page 5, absorbed power density (mW/cm^3) and dose (J/kg) are easily interconvertible if time of exposure is known and the density of the tissue involved. Using our amendments, these terms are referred to as dose rate (mW/cm^2) and dose (J/kg). We are not familiar with any "considerable controversy over the concepts" of dose and dose rate in connection with the "electromagnetic (sic, microwave and radiofrequency) RPG's. It could be more edifying to state the relating of exposure rate (mW/cm^2) to the energy absorbed (dose) per unit time (dose rate).

P 14, L 25 Health officials in ionizing radiation are quite familiar with all these terms so this statement is not quite correct.

P 15, L 1-3 "It has been well established...." Ionizing radiation protection guides are expressed as dose rates (rems per year) not doses. Ionizing radiation experts are familiar with exposure (roentgens) and dose (rads) and certain relationships between the two. We still do not measure dose, but the exposure or exposure rate an individual receives. With knowledge of the radiant energies involved (LET, RBE, etc.) and their behavior, dose is calculated and integrated over time, then compared to existing standards which are expressed in still another unit (rems per year). This unit is equivalent to dose in rads times various qualifying factors integrated over time. Much discussion and problems still exist in the ionizing radiation field regarding dose and exposure relationships. The guides are expressed as the dose equivalent per unit time (calculated), not just dose, and all the information comes from (1) exposure measurements, (2) length of time exposed, and (3) knowledge of qualities of radiation in question. The ionizing radiation guides could easily be written in exposure terms for ionizing radiation since this is what is measured anyway in the occupational situation.

P 15, L 3-6 "Apparently many biological effects resulting from electromagnetic radiation are not dose dependent,...." This is totally inaccurate. It is an unproved, unreferenced assertion that has little to do with microwave and r-f standards and research needs at present. First of all, we do not currently measure "dose" but the exposure rate related to the far field characteristics of microwave radiation. The quotation is later contradicted by the author(s) listing of effects, even the so-called "nonthermal" ones, most of which

have exposure rate thresholds defined. What this quotation uncovers is the same controversy that has occurred in ionizing radiation on which an overwhelming amount of literature exists--that is, the linear, no-threshold (Lewis model) dose-response relationships versus the threshold dose-response relationship. No evidence exists now for accepting a linear curve in microwave and r-f radiation standards criteria.

P 15, L 6-7 "Hazards which are...can occur over a wide range of dose." Really a meaningless statement. Further, the relationships indicated are compared against exposure rate measurements, not dose. I'm not sure that the author is making a careful distinction between exposure, exposure rate dose and dose rate.

P 15, L 8-10 Kramer, et al.: Their work indicates a threshold response for cataract formation to an exposure of rabbits eyes. This unverified report, as nicely done as it is, has not established anything.

P 15, L 10-12 "No cataracts are formed...to accumulate an extremely high dose." Expressing the results this way raises the question of cumulative damage versus an accumulation of damage. The 150 mW/cm^2 figure given by the author(s) is an exposure rate anyway, not a dose, so the matter is rendered somewhat difficult to understand, at least insofar as what the point is that they are trying to make.

P 15, L 12-14 "It appears that as far as thermal effects are concerned that the absorbed power density (W) is the field parameter which should be controlled." W = watts! Absorbed power density = W/kg (dose rate). We would prefer some consistency in using these terms.

This is the first time that the concept of dose, as the author(s) term it, is the parameter upon which the standard should be based. We do not categorically agree (besides, dose

rate would be more meaningful) with this concept. Absorbed power density (W/kg) can be controlled by controlling exposure rate (W/m^2) can it not? It is what will be measured since dose measurements require invasive techniques in the intact functioning human organism. This discourse will continue in the critique of page 16.

P 15, L 17 "However, insufficient research...controlled." What does this statement mean? The case has not been made yet for the fact that dose (absorbed energy) "as a possible controllable factor cannot be ruled out." Why not exposure or exposure rate? Does energy really <u>have</u> to be absorbed to produce a biological effect? There are many ways of looking at this problem. What about field-induced force effects?

P 16, L 1-3 "Thus the dosimetry...ionizing radiation." This judgment is unnecessary. It does not resemble the case with ionizing radiation; it is different, so different approaches are required. The experience with ionizing radiations and standard setting is useful, but the agents are different and, therefore, the considerations will bear their own characteristics, unique unto themselves. This is an irrelevant statement.

P 16, L 14-18 Two sentences. The first, "However...possible." is a truism. The same can be said of exposure to a degree. The variety of exposure conditions, position, area of body irradiated, orientation, environment, co-stressors, etc. render statically determined dose and dose rate data limited by any given exposure conditions. For a given set of exposure conditions certain dosimetric parameters will exist. Each time these are altered, the dose and dose rate will change. The exposure parameters will be just as important as dose, even more so relative to standards setting because it is <u>what will be measured</u>. Given (1) a set of exposure conditions, (2) the effect produced, (3) the extent to which

the effect constitutes damage or threat of damage, (4) the control that can be exercised over the source and the exposed subject, very realistic standards can be evolved. Analogies exist everywhere for this. Knowledge of energy distribution and absorption characteristics are important, to be sure. They will help elucidate mechanisms of biological response and they, perhaps, will identify certain critical problems for consideration by standard setting bodies, but they are not a sine qua non of standard setting. The mechanisms of what is occurring are not of principal importance--these constitute lifetimes of work. What effects were produced is, and should be, duly emphasized. This, then, is our response to the second statement ("To be of value...field conditions.") This is much-needed knowledge, and will take a long time to develop. We require standards now. The rest of the paragraph tells why it will take a long time to develop this body of knowledge. This is the whole problem, we measure exposure, we can have a variety of exposure conditions of a finite but very large number. Dose is going to vary also as exposure varies. We measure exposure rate--we calculate dose and dose rate, and no matter how it is calculated, it will be limited in value and constrained by the same parameters that exposure is. There is a lack of emphasis throughout this report on the effects themselves and developing criteria for assessing their potential for harm in the human subject. This dosimetry section emphasizes this lack.

P 17, L 11-15 There is Soviet literature on this subject as well that could be referenced.

P 17, L 15-18 "...inasmuch as the hazard at low radio frequencies comes from the external magnetic fields...." A conclusion has been reached here about the state or condition of threat or potential threat to health and safety that we are not sure is warranted. Further

explanation and referencing would be required to make this viewpoint tenable. Earlier in the report the impression was given that lower frequency radiofrequency radiation presented less of a hazard to man since it was not absorbed. Without biological effect data, a re-examination of the standard would not be very fruitful. This is an area for further research.

P 17, L 24 What are "more realistic models" of animals and man? What constitutes them?

P 18, L 8 "For interpreting animal research...experimentally useful animals." What dictates which animals are "experimentally useful"?

P 17-18 This whole paragraph describes many, many years of work and states quite clearly the author(s) view of the requirement for absorption data on models, etc. for standard setting. It presents in a very biased manner a very unrealistic approach to standard setting. The author is using this report as a vehicle for proposing a particular viewpoint which is incorrect and unjust. Regarding the last sentence of that paragraph on page 18, biological effect research has not been handicapped in the least by lack of absorbed power density knowledge. Exposure-response parameters exist, or they do not exist. All of the mechanistic steps in between are important for understanding how it happened but doesn't change whether or not the effect or response occurs.

P 19, L 2-5 "Research results reported...." This apparently relates to one occurrence--Korbel's work. It didn't change the results either, just the exposure parameters. Reference the "several instances" (line 5). No one questions the need for someone trained in electromagnetic theory and practice as the author states. Indeed, the presence of such a person is essential to the success of experimentation in this area of interest. The

converse is true also, the engineering and biophysics community are in need of trained biological expertise for similar reasons.

P 19, L 21-24 "Biological research...inside the tissues." It is true that dosimetry research would be accelerated by such a probe, and possibly various mechanistically oriented biological effect studies, but not biological effect research in general, necessarily. I would believe that this would depend more on biological technical methodology and not on different internally deposited probes which involve a basic alteration of biological integrity--a so-called "invasive" measuring device. The corollary problem of developing externally applied measuring probes for physiological parameters will probably be of greater interest and practical importance in the immediate (5-10 years) future.

P 21, L 18 Appropriate densitometric techniques, not "dosimetric."

P 22, L 7-17 This entire paragraph is subject to much criticism not to speak of conflict of interest--look at the makeup of the committee and reread this paragraph. We object emphatically to the way the author(s) is trying to standardize everything here: protocol, exposure chambers, densitometry and dosimetry, specific animals, and few facilities. This could lead to stifling of interest, and many errors due to lack of interpretative experience. We agree that standardization of terms and concepts, dosimetry and densitometry, and other objective parametric details should be standardized. One gets the impression, however, that the experimental results will end up standardized also by making the animal fit experimental designs and not vice versa. It is the effect in the intact real animal and in man that we are interested in, after all, is it not? It sounds like the authors, being members of the in group, are trying to acquire hegemony over

such research. This is grossly inappropriate and will be detrimental to this field of interest in the future.

P 22, L 18-20 "In view...placed in this area." We agree with this statement, but must urge great caution to the research community on chronic low-level effect studies. The very nature of such studies fosters the production of experimental artifacts, false-negative and false-positive data. These experiments are extremely difficult to perform and control properly.

P 22, L 20-22 In view of its importance, references on this should be included.

P 22, L 22-23 A discussion of these effects, however brief, with interpretive statements should be included.

P 23, L 3-5 This section demonstrates lack of understanding about problems in performing CNS studies (electrophysiological, biophysical, and behavioral). It hardly identifies better developed techniques for interpreting CNS effects. Biophysical analyses would require extensive development of neurobiophysics--a field that has not been really developed as yet. We doubt that the highest priority should be assigned here. Before such statements are made, an effort by the committee should be made to explain the problem to physicians and biologists in the fields of neurophysiology and experimental psychology and ask for their suggestions on approaches.

P 23, L 6-8 "Since biological effects...specimen understudy." What is the author(s) trying to convey here. Does he refer to differences in tolerances to thermal input in different species, differences in thermoregulatory responses? It really is not an appropriate statement in this paragraph.

P 23, L 14 We prefer densitometry to dosimetry when measuring exposure. More accurate term.

P 23, L 23-25; L 25 to P 24, L 3 "Studies should be conducted on small animals...." While a true statement in itself, to link this appropriately with the following sentence it should read: "Studies on small animals are required when statistical validity, large numbers, and cost considerations predominate." A large experience base and other related information on mice, rats and rabbits already exists. In the interest of scaling and to amplify our knowledge and experience base, work with dogs, cats and subhuman primates is also necessary to permit better extrapolation parameters. Finally, human research and investigation on larger-headed mammals is of paramount need and interest for ultimate analysis of effect-response production and to validate predictive models. Further, it is required if appropriate scaling parameters and extrapolation data are to be evolved for this particular discipline.

P 24, L 1-3 With the availability of facilities for human research, simulation of absorbed energy in man is rendered to a secondary position of importance. The importance of simulation will be only insofar as it reflects, predicts, or verifies real, live human data, and not vice versa.

P 24, L 4-5 We think biological research needs are clearly discernible. Certain end points have not been defined, especially in long-term, low-level studies. This is a research need in itself. This is a question of appraoch.

P 24, L 7-9 "Threshold levels for biological hazards...." Do you mean biological response? Hazard is a state or condition of threat to health and safety. Many effects or responses

can occur that don't represent a hazardous situation. Make this distinction.

What does "threshold for interaction" mean? Interaction with microwave or r-f field?

P 24, L 9-11 What is a stress variable? What does "performance of a physiological task" mean? Sweating and belching are physiological tasks. Pressing a lever for a reward is an operant task.

P 24, L 19-20 Electrochemical and neurochemical? We think you mean electrochemical refers to end plate neurotransmitting agents; and we believe, the way you use neurochemical indicates gross measures of particular areas of the brain and their content of various agents, serotonin, norepinepherine, gamma *GABA isoaminobutyric acid, etc. We suggest you avoid such terminology and refer to the whole area as neurophysiology.

P 24, L 23-24 We agree about observing test animals for prolonged periods of time, but don't use the reference in line 24 to cataract production. These are completely unverifiable reports (no published case reports). The author(s) is lending credence to highly suspect reports. This entire paragraph needs extensive revision under the guidance of biologically oriented persons.

P 25, L 2-3 "...biological reproduction...." Leave out biological. Better stated, "reproduction, cell division and differentiation, embryonic development, fetal malformation, and alterations in genotype and phenotype in exposed populations...." It would be advisable if the author(s) makes the distinctions among reproduction, development (embryology) and genetics much clearer.

P 25, L 3-7 Egg-laying animals create enormous extrapolation problems. Human beings do not lay eggs or hatch fetuses. (Better, suggest that the research community study mammals,

rodents and other such.) You might include this material in ecological considerations.

P 25, L 9-11 This statement is unfortunately all too true. It is the reason we suggest lower priorities for such research in order to be cost- and time-effective. There are more immediate problems at hand.

P 25, L 11-13 What are "appropriate scaling rules"? One of the major points of this report should be that adequate scaling rules and extrapolation parameters do not currently exist. We prefer extrapolatability; extrapobility is not in any dictionary (line 13).

Extrapolation to Man

P 25, L 17-18 Mention here that the capability exists in human experimentation and should be developed. It is available. Don't forget occupational health data either. Remember, we in western countries have not observed what the Soviets report.

P 25, L 18-20 This is a value judgment that is to be viewed as irrational. A laboratory animal is no measure of tolerance capability in a higher organism phylogenetically, particularly man. It doesn't even give a good idea of what an equivalent wild species is capable.

P 25, L 20-24 This is just one approach. The report should offer other alternatives. What we really object to here is the value judgments made by the author(s) which are out of the scope of the purpose of this report.

P 26, L 4-13 We agree with the intent and stated objectives in this paragraph as an area of work that needs development. But we feel that the author(s) has grossly overstated its need, especially with respect to standard setting in general, and hazard evaluation in particular. Note, carefully, that this report has completely ignored effect analysis with respect to hazard evaluation.

Effects of Elevated Temperature and Environmental Conditions

P 26-28 Generally, we suggest avoiding promulgation of THI as an index in itself. NIOSH is currently using the WGBT index in its regulations on heat stress. Suggest the keeping of such records by stating what THI contains but avoid the use of the term THI, thus avoiding any conflict with what is in official usage. There is pressure for NIOSH to change their standards, phrasing them in terms that are more interpretable physiologically (i.e., THI).

P 27, L 8-9 Reference this statement about "Above 106° F...occur." Also, is this true for all species, for all mammals, or what?

P 28, L 3-4 Reference the "standard man" statements, especially the statement about the fact that it is not conservative. There are many "standard men." To which are you referring?

Population Groupings

P 29-30 You might mention here the need for surveys and surveillance just to acquire the data we do not have. It is in areas just such as this that the committee would benefit from the addition of personnel experienced in personnel health protection on your consulting staffs.

P 30, L 1-3 Under OSHA and the promulgated regulations, employers had better be equipped to recognize and control health-related factors. Most of them are for insurance reasons. This is true for military particularly. We do not believe that the author(s) is very familiar with the extent and development of occupational and industrial medicine or hygiene.

Philosophy of Radiation Protection

P 30, L 17-18 "Undoubtedly...increases." This expresses a personal point of view that all of us do not agree with. Suggest changing this to read: "Undoubtedly, more risks, benefits, and overt hazards to mankind will be developed as technology increases." The next sentence could be altered slightly, then; "Advocates of the benefit philosophy...."

P 30, L 23-24 Does the author(s) mean "hazards" or "biological effects," and to what extent these effects represent a threat to health and safety?

P 31, L 15-18 In L 17 by "...not hazards..." we think you are referring to no risk of harm to health or, if risk is accepted, it is balanced by acknowledged benefits to mankind. Omit "...due respect given to hazards." at the end.

Summary of Research Needs

P 31, L 20 This is a committee report. "Personal views" are inappropriate.

P 33, L 2 "...or are genetically expressed." Again here is a value judgment that is open to much question.

P 33, L 5 "...possible hazards...." Deleterious consequences is what you mean.

P 33, L 8-10 We disagree. Knowing incident energy parameters and the effect produced as well as the species involved, animal handling and restraining practices, etc., we can compare biological results. Absorbed energy is not a sine qua non for this at all. It does expand the concept and will possibly identify areas requiring further work.

We suggest that the entire "Summary of Research Needs" section be placed in a paragraph format to avoid any artificial priority assignments by readers of this report. We also prefer encouragement of a multidisciplinary approach as the highest priority.

If the committee feels bound to assign priorities, it is suggested that a multitrack program be listed dividing the priorities in the various categories of interest. This will avoid interdisciplinary squabbles. In the list we suggest the following revisions on page 33.

Item 3: List all items as neurophysiological effects here.

Item 4: Use psychophysiological correlates instead of neurophysiological here.

Suggested Program for Multitrack Approach to Research Needs: