The Origins of U.S. Safety Standards for Microwave Radiation

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Summary. An analysis is made of the scientific research and values influencing the policy decisions that led to the adoption of the 1966 U.S. standard for exposure to microwave radiation. This analysis is used as a tool for understanding the problems faced by those who set standards. An effort is made to unravel the complex motivations that lay behind the adoption of the microwave standard. Based on the past record, it is suggested that standard setting remain distinct from basic scientific research and that adversary procedures be used only as a last resort in seeking consensus over a proposed standard.

The procedures by which standards are set to regulate human exposure to foreign substances and radiation rarely conform to any ideal exemplar. The scientific data used in the decision-making process seldom lead to one set of interpretations, nor do they provide the clear lines of demarcation (as between hazardous and safe exposure levels) that standards are taken to imply. The procedures used to reconcile scientific data and social demands—such as risk-benefit analysis or conferences—are themselves the subject of dispute. In sum, standard setting is a complex process that frequently raises as many difficulties as it solves; seldom does it eliminate the problems it was intended to resolve.

Given the problematic nature of procedures used to set biological exposure standards, it is instructive to look at past experiences and draw generalizations that may apply to the present. To this end we investigated the history of the process that led to the adoption in 1966 of 10 milliwatts per square centimeter as the standard for maximum safe exposure to microwave radiation (U.S. Standard C95.1-1966, which we will refer to simply as C95.1) (*1*).

The microwave case is an ideal one to study for several reasons. First, it embodies most of the elements that make standard setting problematic, such as disputed or insufficient scientific data, vested interests, ill-defined political mechanisms, unrepresented values, and so on. Second, the events involved are recent enough to allow consultation with most of the principal parties and clear access to the surviving written record. Third, since the controversy over microwave exposure continues today, a survey of its history has direct and real links with the present. Finally, since historical considerations are influencing decision-making in the microwave area (*2*), we believe that this article will aid in the current reappraisal of the microwave standard and perhaps of other standards as well.

We began our study by locating all relevant published literature. Then, through telephone conversations, questionnaires, and personal interviews, we pieced together the steps by which the scientific information presented in the literature was used to support the standard. Our efforts were aided greatly by extensive unpublished documentation that we unearthed during the course of our research.

Establishing the Standard

The main events leading to the adoption of C95.1 had their origin in the early 1940's. In response to morale problems during World War II that were brought on by popular fears about the effects of radar, the Navy's Bureau of Ships, as early as mid-1942, directed the Naval Research Laboratory (NRL) to furnish information on possible harmful effects of microwave radiation. Subsequent studies at NRL (*3*), the National Defense Research Council (Division 14), the Aero Medical Laboratory of the Air Technical Service Command, and the Army Air Field at Boca Raton, Florida (*4*), revealed no such effects. There was, in the opinion of those investigating the problem, no cause for alarm. Accordingly, during the remainder of the war numerous directives were issued that recommended caution in cases of prolonged overexposure, but no general guidelines were established. Within the context of the war effort, radar microwaves were universally regarded as beneficial.

When the war came to an end, microwave equipment developed during the war, such as the Raytheon microtherm, became available to medical researchers for studying and improving diathermy (treatment by selectively heating the body with radio-frequency radiation). As a result, the prewar interest in the therapeutic use of radio waves—now as microwaves—replaced the search for hazards, and the need for a standard was ignored. Well into the 1950's, most medical researchers believed that microwaves, if used with caution, were "apparently a safe, convenient and comfortable form of heating for local application to tissues" (*5*).

Although there was very little active research on microwave hazards after the war, some were discovered. In 1948, researchers at the Mayo Clinic reported the first confirmed deleterious effects resulting solely from microwave exposure—cataract formation in dogs (*6*). Simultaneously, researchers supported by military sources also reported a possible link between microwaves, cataracts, and testicular degeneration in dogs (*7*). This work was conducted at the University of Iowa at the request of Collins Radio in Cedar Rapids, Iowa, which in turn was a subcontractor for the Rand Corporation. However, there was little interest in these studies, especially by funding agencies. The Rand Corporation, for example, withdrew its support from the Iowa project in 1949 (*8*).

Interest in the biological effects of microwaves was rekindled in 1953 by concerns over reported ill effects suffered by radar workers. In February 1953, John T. McLaughlin, a medical consultant to the Hughes Aircraft Corporation, drew up and sent to the military a report that listed purpura hemorrhagica (internal bleeding), leukemia, cataracts, head-

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aches, brain tumors, heart conditions, and jaundice as possible effects (*9*, pp. 5–6). Response to this report was almost immediate. The Air Research and Development Command (ARDC) quickly convened a meeting and, on 28 April 1953, sent a directive to the Cambridge Research Center requesting that its mission be expanded "to include research and development in the biological aspects of microwave energy." One part of this research was to be aimed at "the determination of permissible dosages of microwave radiation to include single as well as repeated exposures." One day later the Navy convened a meeting under Commander David E. Goldman to discuss, among other problems, the establishment of "tolerance dosages" (*10*).

Faced with little empirical data to establish tolerance dosages, the participants at the Navy conference attempted to determine the amount of radiant energy the body could handle under normal conditions, basing their calculations on assumptions about the manner in which microwaves interact with living tissues. Kenneth S. Cole, director of the Naval Medical Research Institute, made a first step toward this end by suggesting that, "if I haven't misplaced a decimal point," 1 W/cm^2 is a dangerous exposure level since a 70-kilogram man having a surface area of about 3000 cm^2 (Cole's figure) and absorbing about one-third of the radiation coming from a source would absorb nearly as much energy as he can eliminate through normal body functions under normal conditions (*11*). After considering this estimate and the implications that could be drawn from the few experiments that had been conducted, the group agreed that if a safety factor of 10 were built in, then 0.1 W/cm^2 represented a reasonable first approximation of the dividing line between safe and hazardous exposures. With some dissent [several members sought higher or lower first approximations (*12*)], this guideline was adopted.

That more than a decimal point had indeed been misplaced was soon discovered by one of the participants, Herman Schwan, a biophysicist at the Moore School of Engineering. In a memorandum sent to the Office of Naval Research, Schwan estimated that the amount of heat the body dissipates under normal conditions is 100 W, not 150 W; and that the absorbing surface of the body was actually 20,000 cm^2. He also discounted the one-third absorption factor. Putting these figures together, he estimated normal heat loss to be 0.005 W/cm^2 and concluded that "a UHF-radiation intensity of 0.1 W/cm^2 supplies per cm^2 [of] irradiated area 20 times as much energy as the body sets free under normal circumstances. It appears that the suggested figure [0.1 W/cm^2] cannot be regarded as a safe tolerance dosage. A more conservative figure seems to be 0.01 W/cm^2" (*13*). In the wake of this suggestion, 0.1 W/cm^2 was quickly abandoned and replaced in all official communications by 10 mW/cm^2, a figure that would, more than a decade later, form the basis of C95.1.

At about the same time that initial guidelines were being established by the military, two major military contractors, Bell Telephone Laboratories and General Electric, convened meetings for the purpose of setting guidelines governing exposure of their personnel to microwaves. Unlike their military counterparts, the participants at the industry-sponsored meetings placed more weight on empirical data, paying particular attention to a 1952 report by Frederic Hirsch of the Sandia Corporation that described the formation of lenticular opacities in the eyes of a laboratory technician who had regularly been exposed to microwaves at power levels estimated at about 0.1 W/cm^2 (*14*). Reflecting on these data, which had been mentioned in passing at the Navy conference, the researchers at General Electric decided, on 1 June 1954, that if damage could occur at 0.1 W/cm^2, then a safety factor of 100 should be built in and the guidelines for exposure set at 1 mW/cm^2 (*15*). In November 1953, the Central Safety Committee of Bell Telephone had taken an even more conservative stand and adopted a 0.1 mW/cm^2 guideline based on a safety factor of 1000 (*16*). Thus by late 1954, both industry and the military agreed that 0.1 W/cm^2 represented a known point at which injury might occur. Where opinion differed was over the margin of safety that should be adopted.

The guidelines set by the military and industry after the renewal of concern in early 1953 were not intended to provide long-range solutions to the problem of determining safe levels of exposure to microwaves. Participants at the Navy conference stressed on several occasions the need for more data (*17*), a need that has been reiterated ever since. Instead, the initial guidelines were intended to provide the best "conservative" estimates of safe exposure levels that could be used to set policy until sufficient data were assembled to confirm or deny them. As a consequence, the major power behind the development of microwave technology, the military, began sponsoring research on the biological effects of microwave radiation.

During the mid-1950's, most of the resulting research was conducted at military establishments, with the primary responsibility for direction being assigned to the Air Force. The rationale for this assignment was straightforward: research on these effects, it was felt, was best carried out in conjunction with the development of the newest microwave technology. Within the Air Force, principal responsibility passed from the Cambridge Research Center (which had been ordered to investigate the problem in 1953) to the School of Aviation Medicine, Randolph Field, in late 1954; and from there to ARDC's Rome Air Development Center at Rome, New York, where Colonel George M. Knauf initiated in 1956 the massive 4-year research effort that came to be known as the Tri-Service program (*18*).

The objective of this program was to clear up as many unknowns about microwave radiation as possible. This meant (i) studying the mechanisms of microwave-tissue interaction, (ii) searching as broadly as possible to determine the extent of the biological effects involved, and (iii) attempting to collect empirical data on the level of exposure that could be deemed safe (or hazardous). Through the Tri-Service program, the military hoped to realize a goal proposed by J. W. Clark (*19*) of Collins Radio in 1950. In response to early reports of hazards, he had suggested that "it would be highly desirable in the light of these observations to set about establishing standards for the protection of personnel exposed to intense microwave radiation before anyone is injured. We have here a most unusual opportunity to lock the barn door before, rather than after, the horse is stolen" (*19*, p. 3). However, such was not the course of events that followed. Despite claims to the contrary, the Tri-Service program did not in any formal way address itself to the problem of the standard. The information it obtained remained simply that, information, as the burden of the standard-setting process came to rest more and more on the Navy and, to lesser extent, on some industrial factions.

As early as 31 August 1957—less than a year after the first Tri-Service conference—the Chief of Naval Operations, following the orders of an "ad hoc working group" within the Department of Defense, ordered the Bureau of Ships to conduct hazards tests for microwave exposure. By 4 June 1958, this order was confirmed by the Department of Defense and broadened to include responsibility for setting a standard. In December 1958 the microwave problem was divided into

three subfields—fuels, personnel, and equipment—each of which remained under the control of the Bureau of Ships. The subfield of personnel, which included the actual setting of the standard, was then once again assigned to ARDC at Rome, New York, thus bringing at least this portion of the Tri-Service bio-hazards program loosely under Navy control. On 4 May 1959, the Bureau of Ships expanded the base of its standard-setting operation by turning to the American Standards Association (ASA) for help, thereby ensuring the involvement of industry. One month later, ASA (now the U.S. Standards Institute) formally agreed to aid in the standard-setting process by establishing a sectional committee, designated C95, under the joint sponsorship of the Bureau of Ships and the American Institute of Electrical Engineers (AIEE) (*20*).

Despite Navy prodding, ASA committee C95 pursued its objectives very slowly and sometimes with a good deal of contention. It took them 6 months to choose a chairman (Herman Schwan), and his appointment was only reluctantly accepted by AIEE (*21*). At the first meeting (15 February 1960), it was planned that within a year initial reports would be presented by the six subcommittees that had been set up. However, it was not until 24 April 1962 that the committee reassembled to discuss progress. In the interim, the Bureau of Ships had taken over primary responsibility for running C95, with AIEE becoming cosponsor and "advisor." Thereafter, due mostly to the efforts of chairman Schwan, C95 began to meet more regularly. By 1966, three of its subcommittees' reports, one of which was the report of Subcommittee IV on the standard, were submitted to the members for a vote.

After 1960, Subcommittee IV became the most important and visible group concerned with setting microwave standards. Like the parent committee, it proceeded to its goal very slowly. During the first 2 years of its existence (at which time it was under the chairmanship of Colonel Knauf), very little was accomplished. At one point the members discussed subcontracting out (to W. B. Deichmann of the University of Miami) the task of performing an extensive literature search that would be used as the basis for setting a standard. The project was never funded and came to naught. By mid-1962 it was apparent to Schwan and C95's secretary, Glenn Heimer, that Subcommittee IV was in danger of collapsing, since Knauf's duties at Cape Canaveral prevented him from providing the active leadership needed to produce a standard. As a result, at the third C95 meeting, on 20 November 1962, Schwan assumed the chairmanship of Subcommittee IV (in addition to that of C95) and staffed it with a small but active group that included Thomas Ely, David Goldman, William Mumford, and R. D. Lighty. Less than a year after Schwan took over the subcommittee, the first draft of C95.1 had been drawn up and was being circulated for comments and suggestions prior to the adoption of final wording and submission to the entire C95 membership for formal approval.

With the drafting of C95.1, the work of Subcommittee IV was still far from over. The process of getting the proposed standard accepted proved difficult, not because there were objections but because some of the members of C95 were dilatory in voting. (The rules of ASA require that its standards be approved by a three-fourths vote of the active membership of the committees that are set up to establish them.) By January 1965, 8 months after ballots had been mailed and 7 months after the deadline for submitting votes, only 31 of the 52 members had voted. Finally, by limiting the voting membership of C95 to 41, the committee tallied a final vote of 38 yes and 3 "not returned," and forwarded to ASA its report, which was accepted as U.S. Standard C95.1-1966 on 9 November 1966—1 year and a few days after Schwan relinquished the chairmanship of C95 itself.

That the adoption of C95.1 would not settle the problem of the microwave-exposure standard was evident almost immediately. In December 1966, Glenn Heimer wrote to John Gerling, president of the newly formed International Microwave Power Institute, informing him that at the last C95 meeting it had been suggested that a second standard would be needed for the general public; that is, for those who might have regular, nonoccupational exposure to microwaves. Heimer suggested that his second standard might fall "in the neighborhood of 1 mW/cm^2 for continuous exposure" (*22*). At roughly the same time, the U.S. Army Electronics Command at Fort Monmouth, New Jersey, sent a letter to the Naval Ship Engineering Center indicating that "C95.1-1966 is not concurred in" primarily because of difficulties in implementation (*23*).

Thus adoption of C95.1 did not represent the end of the debate. Instead, it was the beginning of an era during which the debate for the first time became publicly focused on a particular estimate of the "safe" exposure level.

The Scientific Basis for Concern

As drawn up and adopted, C95.1 was intended to reflect the best approximation of safe microwave exposure levels for those exposed occupationally (not the general public). The scientific data upon which it was based were of three types: (i) clinical studies and personnel surveys, (ii) animal experiments, and (iii) research on anomalistic effects. The major question that arises concerning these data is, was there sufficient evidence available at the time C95.1 was set to justify its acceptance or cast doubt on its validity (*24*)?

The most elusive and yet potentially the most useful of the three groups of data are surveys of personnel who were exposed to radar microwaves. This fact was recognized early in World War II and led to two studies. These studies gave no cause for concern. In his 1943 survey, Daily (*3*) concluded that "there has been no clinical evidence of damage to these personnel"; and in their 1945 survey, Lidman and Cohn (*25*) found "no evidence" of abnormalities. In sum, the personnel studies conducted during World War II led most to dismiss the rumors that associated radar with health hazards.

The influence these studies exerted in subsequent years certainly cannot be attributed to the weight of their scientific evidence. They were extremely superficial. The Daily study reported no urinalysis or blood chemistry data of any kind. Moreover, its conclusion that there were "no significant changes" was not justifiable on the basis of the data presented. A statistically significant increase in the concentration of immature red blood cells was found in exposed workers, as was a high incidence of headaches. Therefore, had researchers been interested in finding grounds to conduct more extensive surveys of personnel, it is clear that a case could have been made.

The inadequacies of the early personnel studies were recognized as soon as the microwave problem reemerged in the early 1950's. McLaughlin noted that "the early work done by the Navy and the AAF [Army Air Force] was not extensive, the power used was very small, the work was not quantitative in character, and the controls were inadequate. Therefore, this work cannot be relied upon as scientific background to establish the possible health hazards of microwave radiation" (*9*, p. 6). However, despite McLaughlin's prodding, the quantity and quality of personnel studies did not increase very much. The topic came

up on several occasions at the Navy conference, always with the clear understanding that more data had to be forthcoming (*26*). The industry representatives at the conference indicated that "Philco, General Electric, RCA Victor, were all anxious to cooperate to do whatever they can, but they are all standing by waiting for someone else here to make the move" (*27*). This willingness notwithstanding, the "Cook's Tour" approach to the hazards problem called for at the Navy conference seems never to have been launched, since only a handful of personnel studies appeared in print over the next decade.

The few personnel studies made during the middle and late 1950's presented contradictory conclusions. Beginning in 1954, researchers at Lockheed Aircraft reported some blood abnormalities, but these findings were dismissed in a later article as "due to a variation of interpretation by a laboratory technician." With this error eliminated, the conclusion of the final Lockheed study was that "there appears to be no justification for public concern about the effects of greatly attenuated microwave energy in the environment" (*28*). Milton Zaret, who conducted a controlled search for ocular damage, was far less certain about the safety of microwaves. While failing to uncover any "reduction of visual acuity due to cataracts," Zaret did report "statistically significant increases in the occurrence of posterior polar defects, luminescence, and early opacification" (*29, 30*). These changes were sufficient to prompt Zaret to continue his work by surveying larger populations.

In combination, the early personnel studies led to no clear course for further action. Such "meager human data," as Michaelson characterized the findings, raised as many questions as they settled and were of little use in setting a standard (*31, 32*). As a result, most of the evidence eventually used to set C95.1 was drawn from animal studies and related biophysical calculations.

The animal studies, although more extensive and better controlled than the personnel surveys (especially during the Tri-Service era), also did not provide conclusive evidence either in support of or against the 10 mW/cm^2 guideline. Animals were exposed to radiation under controlled conditions and studied for any ill effects. The experimental parameters, such as frequency and animal species, were specified by the Tri-Service program personnel, and the work was subcontracted out to university researchers. Ideally, from such controlled experiments one should have been able to determine the level at which injury began to appear. However, biological systems do not always submit conveniently to the experimental method.

There were numerous studies in which animals were exposed to radiation in excess of 10 mW/cm^2 without showing evidence of irreversible injury. Researchers at the State University of New York, Buffalo (then Buffalo University), working with 200-millicycle microwaves at 100 mW/cm^2, found no ocular changes in guinea pigs, dogs, sheep, or mice and were able to breed four generations of mice in a chamber continuously irradiated with 50 to 200 mW/cm^2. Researchers at the University of California, Berkeley, working with 3-cm microwaves, found that below 60 mW/cm^2 the temperature rise in rats stabilized and the animals recovered without any noticeable ill effects. Researchers at the University of Miami subjected rats to 24,000-millicycle microwaves and reported no blood abnormalities at 6 to 10 mW/cm^2 and moderate but apparently reversible changes in male hormone circulation at 300 mW/cm^2. These and other experiments supported the position that animals, and therefore presumably humans, could tolerate exposures well in excess of 10 mW/cm^2 without suffering any serious or permanent damage. Some studies even suggested that animals could adapt to repeated exposures (*33*).

The weight of these experiments in support of the safety of the proposed guideline led some to conclude that the Tri-Service program had settled the issue. Colonel Knauf had reached this conclusion by the beginning of the fourth and final Tri-Service conference, when he noted that "up to today we have not seen any research data which shakes our faith in the validity of this arbitrary safe exposure level which we sponsored some five years ago" (*34*). Knauf repeated this opinion many times during the course of the deliberations leading to the establishment of C95.1. A similar view was set forth by Michaelson in his retrospective appraisal of the Tri-Service era, noting that the most important contribution of the program was "the validation of the 10 mW/cm^2 safety standard" (*35*). Others who looked back on the research of the 1950's made more extensive claims. In a letter from the Raytheon Company to Senator Warren G. Magnuson, dated 31 August 1967, it was contended that the Tri-Service program had led to "three basic conclusions": the biological effects involved were (i) thermal, (ii) noncumulative, and (iii) of little concern since "man has a built-in alarm system coupled with his threshold of pain that protects him from thermal injury" (*36*).

Although widely accepted, this view of conditions at the end of the Tri-Service era was not unanimous. There were doubts about the current state of microwave research. First, the experimental techniques used were such that findings were seldom duplicated and frequently questioned. Second, although not as numerous as the studies that reported no irreversible effects above 10 mW/cm^2, experiments had been conducted in which ill effects at or near this level were found. Third, although almost all of the research conducted before 1966 was based on the assumption that only thermal effects should be expected, anomalistic findings were reported that supported the view that mechanisms other than thermal ones could be involved.

The technical shortcomings of the early animal studies were particularly apparent in two areas. First, dose levels were not reported in many of the earliest studies, and even when they were, it was often on the basis of source output and not field intensities or absorption by tissues. Further confusion arose from possible differences between pulsed and continuous-wave radiation at similar intensities. Second, even though exposure of personnel under field conditions was of long duration and very low intensity, few animal studies involving long-term exposure to low-level radiation were undertaken. In most of the early experiments, intensities well above 10 mW/cm^2 and exposure periods of a few hours or days were used.

These problems did not go unrecognized. As late as the fourth Tri-Service conference, papers were presented that discussed "experimental instrumentation considerations . . . which are intended to result in more reproducible and more quantitative measurements of biological effects of microwave energy" (*37*). The unmistakable message of these papers was that defects in procedure stood in the way of a full understanding of the exposure levels at which damage appeared.

However, it is not simply disagreement over the reliability of data that has led some to be skeptical about the safety of exposure to 10 mW/cm^2. By 1960, when the Tri-Service program came to an end, there were studies in which deleterious effects at or near 10 mW/cm^2 were reported. Researchers at the School of Aerospace Medicine found testicular damage in rats at levels as low

as 30 to 40 mW/cm², a figure that was lowered by Ely and Goldman (*38*) to 5 to 10 mW/cm². Bach and Lewis (*39*) reported brain responses at levels between 12 and 64 mW/cm², and Bach (*40*) later reported changes in blood counts at about 13 mW/cm². This, combined with the fact that most of the exposures to which animals were subjected in research before 1960 were near 100 mW/cm² or above, left at best a paucity of evidence on the effects of low-level exposure and at worst definite doubts about the safety of such exposure (*41*).

The third type of evidence against the prevailing view was that anomalistic (nonthermal) effects might result from microwave exposure. This had been indicated in the 1920's in work by Scherechewsky (*42*) on the effects of ultrashortwave radiation on malignant tumors in mice and by Schliephake (*43*) on the effects of condenser fields on flies, rats, and mice; and in the 1930's in work by Szymonowski and Hicks (*44*) on the attentuation of bacterial toxins. However, by the 1940's much of this evidence had been discounted or retracted by the authors themselves. Nonetheless, throughout the 1950's and 1960's there were occasional mentions of nonthermal responses that some felt bore looking into. Questions raised by Bach at the third Tri-Service conference, for example, led David Goldman, chairman of the 1953 Navy conference, to comment that "the circumstances suggest the possibility that these effects may not be due simply to the generation of heat. Clearly, the work will have to be continued and extended" (*45*).

But more work was not done, at least not at the same intense level as during the Tri-Service era, even when doubt and calls for more research emerged from the best studies of the day. In one such study (on cataract formation), Carpenter *et al.* (*46*) brought all of the strands of doubt together. Evidence from the literature, Carpenter's group demonstrated, suggested that the existing "data on power densities are not valid for comparison," largely because measurement techniques varied so markedly. There simply was no replicated scientific evidence available to decide whether or not microwaves cause cataracts, or at what exposure levels. Moreover, important details—such as differences between pulsed and continuous radiation or the extent of cumulative effects—had not been worked out. Finally, Carpenter *et al.* established hazardous thresholds well below 100 mW/cm². Their results, they wrote, "lead us to question whether the cataractogenic effect of microwave radiation is entirely a thermal effect" (*46*, p. 157). As a result, Carpenter, for one, planned to continue his work. However, the sponsors of his and other projects viewed the state of research differently, and in the end C95.1 was set without the benefit of additional work. Why was the standard set just when scientific research was beginning to reveal how much work remained to be done?

Implications

The simplest answer, and the one advanced by Paul Brodeur, a major critic of past policy, is that many people who were involved in setting C95.1 "felt obliged to protect the 10-mW level at all costs and to ignore, deny, or, if worst came to worst, suppress any information about adverse effects of low-intensity microwave radiation" (*2*, p. 39). For Brodeur, the values behind the events were easily accounted for. Above all, there was the belief in military preparedness and the presumption that a standard below 10 mW/cm² would interfere with national defense. This being the case, all else was ignored, including truthfulness and public welfare, presumably the two principal opposing values. The historical record, however, does not admit to such a simple characterization of the values that produced the standard.

During the years that led to the setting of the standard, there was frequent division within the military between those whose primary concern was operations and those who were more closely aligned with research activities. Within industry, there were decided differences between those who represented defense contractors and those who had ties with the medical community. Research scientists approached the microwave problem from at least three different perspectives: clinical, biological, and engineering-physics. Finally, there was the public, which prior to the first hearings on microwaves in 1967 had few active proponents. Thus, at the very minimum, the standard-setting process represents the interplay of seven or eight different interest groups.

There is ample evidence of these vying interests prior to 1966. At the same time that such military contractors as Hughes Aircraft and Lockheed were anxious to have active research on biohazards begun, the Raytheon Company, which produced diathermy equipment, was reported to have attempted to persuade the military to terminate its sponsorship of the Iowa research on cataracts (*9*, p. 26). Similarly, within the scientific community, at the same time that several research groups were attempting to determine the dosage response for the well-documented connection between microwaves and cataracts, a physician who was a proponent of diathermy confidently proclaimed that "radar waves are completely absorbed by the cornea and have not been reported to be a cause of cataracts" (*47*). The division between the research and operation sides of the military was bared at the fourth Tri-Service conference when Colonel Knauf commented on the notion that the 10 mW/cm² guideline had been selected to please operations rather than to ensure safety: "Could you have heard the protests of our operational colleagues when they first were told to live with this level, I am sure you would have concluded that operational suitability was not the basis for selecting 0.01 W/cm²" (*48*). The military, like industry and the basic sciences, did not assume one set of values in the standard-setting process.

As for the public, the lack of any organized effort on their behalf was more than evident during the years leading to the adoption of C95.1. The few concerns that were raised about the public came not from those who were affected but from those who would have to deal with any problems that might arise from the exposure of workers to microwave radiation. This concern did, in turn, ensure that some debate followed and that standards and safety practices were adopted. But it did not ensure that safety-related decisions would err on the side of the exposed public, much less the general public. Those who set the standard in 1966 still viewed microwaves as radar and radar as a military and industrial problem, and it was within this context that funding decisions were made and C95.1 set.

The presence of competing interest groups within the military-industrial-scientific framework meant that even without the public faction there was still disagreement over how to proceed. To overcome this and the widespread disinterest in public-oriented environmental issues that existed before 1966, the time and energy of a few dedicated persons were required. It took the efforts of McLaughlin in 1952 to call attention to the possibility that a problem existed, the efforts of Knauf in the mid-1950's to initiate the Tri-Service program, and, most importantly, the extraordinary efforts of Schwan in the early 1960's to have the standard set. Schwan's determination

became evident as the negotiations leading to C95.1 dragged on. As he wrote to John Anderson of the Institute of Electrical and Electronic Engineers on 16 June 1963, "I want you and the IEEE Committee to be assured that this documentation has required considerable effort and negotiation. We feel that a fine standard has been achieved and that insistence on the replacement of the word 'recommendation' by 'standard' will probably indefinitely delay a microwave health standard" (*49*). It had taken more than 7 years to progress from the initial orders to the draft proposal of 1963. That only three more years were required for the draft to be adopted can only be accounted for by the conviction and energy of Schwan and others like him.

But having persons of unusual conviction and energy in key decision-making roles has counterproductive as well as productive consequences. In this case, the most important counterproductive consequence was the neglect of competing points of view (the anomalistic and other contradictory scientific evidence) during the push to get the standard set. Schwan voiced dissatisfaction about these restrictions at the end of the first Tri-Service conference, when he suggested that "there was no opportunity to thrash things out" (*50*). The same frustration was felt by many who attended the Tri-Service conferences and continued with the effort thereafter. Allan Frey's (*51*) recollection of an early Subcommittee IV meeting chaired by Schwan (who was now in an organizational role) is one of a brief, pro forma get-together at which the claim was repeated that no evidence had yet been found to cast doubt on the guideline of 10 mW/cm^2. At other Subcommittee IV meetings, Schwan repeatedly reminded members that "it is not the function of C95 and its working committees to undertake research in order to correct deficiencies in knowledge. Hence the working committees' primary task must be evaluation of pertinent information and formulation of standards which can be well supported by pertinent literature" (*52*). The pressing need to set a standard did not leave time for extended debate over the issues.

In the push to set the standard, there can be no doubt that possible evidence against its safety was ignored and that research that might have clarified certain details was not undertaken. The actions of those who had the energy and conviction to pursue the problem displaced contradictory evidence that others, viewing the problem in retrospect, have deemed important. However, to point to these shortcomings and label them the consequence of a conspiracy—the secret planning of an unlawful act—fails to appreciate the conditions that prevailed nearly two decades ago and at the same time misses some important lessons that can be learned through retrospective analysis.

Comments

In the social climate that prevailed in the United States for 20 years after the development of radar (and radar was virtually the only source of concern over microwaves until the marketing of microwave ovens), the decision-making process was not placed in the hands of persons whose primary responsibility was public health or environmental monitoring. The decision-makers were preoccupied with winning a major war, and then, during the first Cold War, with erecting a strong defense. Thus their decisions concerning microwave technology were necessarily made in the context of global or national security rather than that of individual welfare. Under extreme pressure, during war, lives are sacrificed for larger objectives. Under lesser pressure, during years of defense planning, lives may not be sacrificed, but neither is military preparation eliminated to reduce risk to zero. Basic training is not stopped when training accidents occur; the Air Force is not disbanded when crashes occur; and radar systems, which had not killed anyone who followed minimum security regulations, were not dismantled when headaches, blood disorders, and other problems appeared. No one was dying, and the persons who drew up and accepted the 10 mW/cm^2 standard did not believe that this level of exposure was likely to cause any serious consequences in the foreseeable future.

The fact that C95.1 was based on decisions made in good conscience does not dictate that it was the best standard that could have been established or that it was valid. Since C95.1 is being revised, there apparently was room for improvement. Still, those who set standards must base their decisions on the knowledge and values of the time, and to condemn them later serves little purpose. Instead, one should attempt to learn from their example—in the case of C95.1, to discover how the decision-making processes might have been changed to avoid the resulting controversy and at the same time to allow decisions that have broader bases of support.

The fact that relatively few persons, with similar points of view, made most of the early decisions about the development of research and policy on the biological effects of microwave radiation obviously minimized and may even have eliminated input from other perspectives. To counter this tendency, decision-making processes are needed that will institutionalize mechanisms for dealing with many points of view. Not only the public, but scientists whose work falls outside the domain of normal science and persons of nonscientific persuasions, such as humanists, have a legitimate role to play in reaching decisions about standards that can affect human history.

Throughout the years that led to the setting of C95.1, the motivation for microwave safety research derived mostly from the need for a standard; conversely, the need for a standard stemmed largely from the results appearing in the scientific literature. The research and standard-setting procedures were clearly intertwined, with most of the personnel involved in one activity also engaged in the other. This intertwining of activities ultimately does disservice to both communities.

When science becomes involved too deeply in the procedures of standard setting, it runs the risk of being diverted from its primary objective—understanding nature. Standard setting does not require detailed knowledge about mechanisms or explanations about anomalistic phenomena. To set a standard one simply needs to know at what level—for whatever reasons—harmful effects appear. To be sure, this is information that is properly supplied by basic science, but if research stops here, as it seems to have during the early years of microwave radiation research, then science is not well served by its involvement in applied research. The benefits of increased funding, which stem from involvement in mission-oriented research, must be carefully weighed against the risks of scientific misdirection that can follow.

At the same time, policy-setters are not well served if they rely too heavily on the scientific community for direction. Scientists are not trained to anticipate the social and values issues that may emerge when the need for a standard arises. Science deals with what can be determined with reasonable certainty; for example, whether 10-mW/cm^2 exposure to microwaves causes cataracts. They are not trained to handle questions that have no one correct answer—such as, if 10-mW/cm^2 exposure does cause an

increase in cataract incidence of x percent in an exposed population, should exposure at that level be legislated against? Nor can scientists raise all of the questions that are important for policy decisions.

So, not only must different points of view be integrated into planning in areas such as this, they must be integrated in ways that are appropriate. This delicate task is, however, not easily achieved, and it is plagued by two classical dilemmas that must be pointed out in closing.

One dilemma that has plagued the microwave field from the beginning is that of organization versus control. In 1953, when industry came to government seeking answers, organization was lacking and control nonexistent. Faced with this situation, the military organized the field and came thereby to control it. As a result, by the height of the Tri-Service era the pendulum had swung in the opposite direction, with a highly efficient organization coming into being and also a plan of action that left little room for other ways of proceeding. Microwave researchers were looking for well-defined effects, and in the process ignored others and alternative ways of proceeding. It was in this atmosphere of control that the 10-mW/cm^2 standard was set.

The renewed public interest in the microwave problem during the past decade has led to a situation analogous to that of the early 1950's. Once again, calls are being sounded for organization and increased funding in this area. We now understand, most would agree, the problems that need to be solved. We have only to start the work. Admittedly, much more is known today than 25 years ago, and the procedures for dealing with the problems are much more sophisticated; but at the same time there are still uncertainties about how microwaves interact with living systems and about what the demands of society will be two decades from now. This being the case, it is essential to leave room for and in fact to encourage work in "unproductive" related areas of research and to take this research seriously. If this is not done, the organized effort of the 1980's could turn into the "conspiracy" of the 1990's, not only in the area of microwave radiation research but in other areas as well (*53*).

The second major dilemma that has plagued this field from the start involves the flow of information. From the moment radar was deployed in the field, a communication gap appeared between the technical community and the exposed public (initially, military personnel) that has widened with time. The reason for this is easily understood. The technical community (including those who set the policies) has always feared popular misconceptions about microwave technology. Moreover, the threat of legal action stemming from unanticipated future effects has led both industry and the military to limit the flow of information about microwaves even though both feel that their past actions are defensible. The public, sensing that the technologists have held back on them, has resorted to the very actions that the technical community fears—one-sided reporting and appeals to public anxiety—in an effort to get the information flowing. And as fears and paranoia have grown on each side, the communication gap has widened.

Two paths are being taken in an effort to force release of information related to microwave safety. One is the legal path (making damage claims for alleged microwave-related injuries), the other the political path (introducing bills at both the federal and local levels to regulate exposure to microwaves). Both promise to make a great deal of information available to the public. But neither path is designed to close the communication gap. Both the legal and the political decision-making processes are based on adversary relationships (either prosecutor versus defendant or advocate versus opponent); they are designed to compel each side to cling as tenaciously to its own values as possible—not to try to understand or appreciate the values of the other. Perhaps adversary proceedings are the only route by which conflicts between interest groups can be resolved. However, until this is proven to be the case, we hope that every effort will be made to engage in honest dialogue in an attempt to reach rational solutions to the problems we face.

References and Notes

1. *Safety Level of Electromagnetic Radiation with Respect to Personnel* (United States of America Standards Institute, New York, 1966). To avoid confusion, we use the term "guideline" to refer to recommended exposure levels published prior to C95.1 and "standard" to refer specifically to C95.1. It should be noted that C95.1 is not legally binding. It serves only as a guideline for voluntary compliance.
2. The most widely circulated history of the development of the current microwave controversy is by P. Brodeur [*The Zapping of America: Microwaves, Their Deadly Risk, and the Cover-Up* (Norton, New York, 1977)].
3. L. E. Daily, *U.S. Nav. Med. Bull.* **41**, 1052 (1943).
4. Summaries of these studies are given in R. H. Follis, Jr., *Am. J. Physiol.* **147**, 281 (1946).
5. K. G. Wakim, J. F. Herrick, G. M. Martin, F. H. Krusen, *J. Am. Med. Assoc.* **139**, 992 (1949).
6. L. E. Daily, K. G. Wakim, J. F. Herrick, E. M. Parkhill, *Am. J. Physiol.* **155**, 432 (1948).
7. A. W. Richardson, T. D. Duane, H. M. Hines, *Arch. Phys. Med.* **29**, 765 (1948); C. J. Imig, J. D. Thomson, H. M. Hines, *Proc. Soc. Exp. Biol. Med.* **69**, 382 (1948); W. W. Salisbury, J. W. Clark, H. M. Hines, unpublished report.
8. With regard to the funding of research at Iowa, H. M. Hines noted at the 1953 Navy conference that "we haven't had any [funding] for four or five years. This was stopped cold" [in (*10*), p. 133]. It should be noted that the Rand Corporation was not necessarily taking specific action against this project since it withdrew most subcontracts at the time.
9. J. R. McLaughlin, *A Survey of Possible Health Hazards from Exposure to Microwave Radiation* (Hughes Aircraft Corp., Culver City, Calif., 1953).
10. *Biological Effects of Microwaves* (Office of Naval Research, Washington, D.C., 1953). A summary of ARDC activities begins on page 18.
11. K. S. Cole, in (*10*), p. 77.
12. See (*10*), pp. 114–115.
13. Cited in W. B. Deichmann and F. H. Stephens, Jr., *Ind. Med. Surg.* **30**, 221 (1961).
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15. B. L. Vosburgh, *IRE Trans. Med. Electron.* **4**, 5 (1956). For the full report of the General Electric deliberations, see H. R. Meahl, "Using microwaves safely: 750 to 30,000 mc," General Electric Technical Information Series, Schenectady, N.Y., 1956.
16. W. Mumford, *Hazards to Personnel Near High Power UHF Transmitting Antennas* (Proj. Rep. 717, Bell Laboratories, New York, 1956). The early disagreement between the military and industry is discussed by G. M. Knauf, *Arch. Ind. Hyg. Occup. Med.* **17**, 48 (1958).
17. See (*10*), pp. 73, 105–107.
18. Developments between the 1953 Navy conference and the beginning of the Tri-Service program are discussed in *Proceedings of the Tri-Service Conference on the Biological Hazards of Microwave Radiation* (AF 18(600)-1180, U.S. Air Force, 1957), pp. i–ii. This interpretation of events was disputed by some of the sources we contacted, but we were unable to uncover documentation to support another view.
19. J. W. Clark, *Rand Rep. P-122* (7 March 1950).
20. These orders are discussed by J. J. Fisher, "Standardization of procedures for electromagnetic environmental hazards to personnel, fuels, and weapons," memorandum, Navy Bureau of Ships, 1 August 1958; in a series of memoranda by J. J. Dunn dated 2 December 1958; in "Report of the general conference on standardization in the field of radio-frequency electromagnetic radiation hazards," American Standards Association, 4 May 1959; and in the follow-up letter from M. A. Pisciolta to J. J. Anderson and E. F. Seaman dated 8 July 1959.
21. The summary that follows of the activities of C95 and Subcommittee IV is based on unpublished minutes and official correspondence. When possible, we confirmed details with persons who were active on C95 and Subcommittee IV.
22. Undated letter from G. M. Heimer to J. E. Gerling. The distinction made in Heimer's letter is too often ignored by modern critics of microwave policy decisions. Personnel guidelines, by virtue of the fact that they apply to controlled conditions—known exposure levels for known periods of time—tend to be higher than general guidelines, which are for exposures of longer duration and undetermined levels. Thus the suggestion by Heimer that a lower level may be needed for the general public is in no way indicative that he felt the personnel standard established was too high.
23. R. Ullman to Commander, Naval Ship Engineering Center, memorandum dated 3 November 1966.
24. Past research on the biological effects of microwave radiation is surveyed in detail by authors, *Ann. Sci.*, in press.
25. B. I. Lidman and C. Cohn, *Air Surg. Bull.* **2**, 448 (1945).
26. See (*10*), p. 119.
27. J. R. McLaughlin, in (*10*), p. 145.
28. C. I. Barron, A. A. Love, A. A. Baraff, *J. Avia. Med.* **22**, 442 (1955); *IRE Trans. Med. Electron.* **4**, 44 (1956); C. I. Barron and A. A. Baraff, *J. Am. Med. Assoc.* **168**, 1194 (1958).
29. M. Zaret, in (*30*), p. 302.
30. *Proceedings of the Fourth Tri-Science Conference on the Biological Hazards of Microwave Radiation* (Plenum, New York, 1961).
31. S. M. Michaelson, in (*32*), p. 162.
32. *Proceedings of the Third Tri-Service Conference on the Biological Hazards of Microwave Radiation* (University of California, 1959).
33. See the summaries of research in (*32*). In pre-

senting these results, we are not making judgments regarding their validity. Many reports presented at the Tri-Service conferences and elsewhere, both supporting and challenging the safety of exposure to 10 mW/cm², were questioned at the time. Negative discussion was apparently very lively at times. However, since little criticism of existing data was ever published, we have had to assume that all unchallenged studies were available to those who set the standard.
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35. S. M. Michaelson, *IEEE Trans. Microwave Theory Tech.* **19**, 133 (1971).
36. U.S. Senate Committee on Commerce, *Hearing on S. 2067* (90th Congress, 1st session, 1968), pp. 407–408.
37. J. H. Vogelman, in (*30*), p. 31.
38. T. S. Ely and D. E. Goldman, *IRE Trans. Bio-Med. Electron.* **11**, 38 (1956); *IEEE Trans. Biomed. Eng.* **11**, 123 (1969).
39. S. Bach and S. Lewis, in (*32*), p. 87.
40. S. Bach, in (*30*), pp. 131–132.
41. Perhaps the best evidence for a lack of research of low-level effects is the problem the military had in finding researchers to study such effects in the early 1960's, when the Moscow Embassy problem arose.
42. J. W. Schereschewsky, *Public Health Rep.* **41**, 1939 (1926); *ibid.* **43**, 927 (1928); *ibid.* **48**, 844 (1933); *Radiology* **20**, 246 (1933).
43. E. Schliephake, *Kurzwellentherapie* (Actinic Press, London, 1935).
44. W. T. Szymanowski and P. A. Hicks, *J. Infect. Dis.* **50**, 1, 466 (1932).
45. D. Goldman, in (*32*), p. 93.
46. R. L. Carpenter, D. K. Biddle, C. A. Van Ummersen, *IRE Trans. Med. Electron.* **7**, 152 (1960).
47. *J. Am. Med. Assoc.* **150**, 528 (1952).
48. G. Knauf, in (*30*), p. 10.
49. H. P. Schwan, letter to J. J. Anderson, 16 June 1965.
50. H. P. Schwan, in (*18*), p. 88.
51. A. Frey, personal communication.
52. Minutes, meeting of American Standards Committee C95, 20 November 1962, p. 5. See also the minutes for the meeting of 24 April 1962.
53. It is impossible to summarize the many developments that have taken place since 1966. Even as we write, voluminous reports are being prepared and published by numerous federal and private organizations, including newly formed ones such as the Bioelectromagnetic Society. The proliferation of reports and organizations could lead to effective problem-solving or could further fragment an already complex field and make effective problem-solving more difficult. Our objective is not to suggest that one or the other will result. Rather, our objective is (i) to suggest, on the basis of past experience, that organization or increased funding alone will not solve problems of this sort and (ii) to give some reasons why this is the case.
54. Supported by NSF grant OSS-78-06675 and by the Science, Technology, and Values program of NEH. We thank the University of Michigan Collegiate Institutes for Values and Science for their encouragement.

Court Finds That *Science Digest* Infringes on Logo of *Science*

William J. Broad

On 28 April, after 5 days of trial, the U.S. District Court for the District of Columbia ruled that a redesigned edition of *Science Digest* magazine had infringed on a registered trademark of the AAAS, the cover logo of *Science* magazine. Infringement first occurred in October 1979 when the Hearst Corporation published a "special edition" of *Science Digest* in which the word "Digest" on the cover had shrunk to 9 percent of the area occupied by the word "Science." In the regular edition of the magazine, both words are the same size. Hearst has so far published three issues of the special edition, which is slated to eventually replace the regular edition.

Throughout most of the trial, a copy of the special edition rested on a stand some 14 feet away from U.S. District Court Judge Joyce Hens Green, who in her 28-page opinion commented on the visibility of this exhibit. "Although abundantly cognitive of the true title of the magazine and the words which actually existed on the cover, and forcefully straining for a more neutralized perspective, the Court nonetheless continually found the word 'Digest' blurring into oblivion."

On the basis of this and a good deal of other evidence, Judge Green enjoined Hearst from further publication of the special edition in its present form, ruling that the word "Digest" must in the future occupy at least 75 percent of the area occupied by the word "Science." The case is one of the few in which a court had found trademark infringement on the basis of word size, rather than meaning.

During the trial, attorneys for the AAAS argued that Hearst had deliberately infringed on the cover logo of *Science* in an attempt to lure new customers. If this had been found true, it would have required Hearst to pay AAAS profits from the special edition as well as damages. The court ruled, however, that there was insufficient evidence of deliberate intent to infringe. "In Hearst's selection for the title of its revised publication a logo visually almost identical to *Science*, with 'Digest' virtually obfuscated and therefore falling from sight and mind, there is the suggestion, but just that, of an intent to capitalize deliberately on *Science*'s enviable good will, prestigious reputation, and alluring market."

Science Digest first appeared in 1931, and for years it has been published as a small monthly, about the size of *Reader's Digest*. Unlike the 14 other magazines owned by Hearst, *Science Digest* in the past 15 years has failed to appreciably increase its circulation, and in the past 5 years has suffered a financial loss. In an attempt to upgrade the magazine and treble its circulation, Hearst launched the special edition as a test of commerical success. Unlike the regular edition, it is the same size as *Science* and *Science 80*. This special edition, according to the court, "is a flashy, up-beat magazine that differs from *Science* as a Philip Roth novel differs from a Shakespeare play, as Bo Derek does from Katherine Hepburn."

The cover of the Winter 1979 issue announced articles on "Sex and Survival—Our Erotic Origins," "Fuel from Water—Science says Yes," "Plus Urgent News on Radiation, Pain, Cancer, Smoking, Pesticides, Burns." Some of the stories, the court wrote, "recall the old *Science Digest* as far as their implausibility."

At the trial, Hearst's expert witness on the design of the magazine covers testified that because *Science Digest*'s logo was designed as a "unitary title," a consumer approaching a newsstand would perceive the title correctly as "Science Digest." A key AAAS witness took issue with this, and testified as to his own confusion. Arthur Habel, a public relations consultant by profession, had become familiar with *Science* over the years, and had read it continuously for 6 months in 1974. Last fall, Habel saw a copy of the special edition of *Science Digest* in the office of a friend and thought it was a special edition of *Science*. He later wrote Hearst, asking for the "special edition of Science." All this became known to attorneys for AAAS during the discovery process, when they came up-

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