B. It was agreed that there is at present insufficient evidence to draw conclusions. In answer to questions about whether or not other studies (aside from those supported by ARPA) are likely to or have shed light on the problem, it was concluded that the only known study not included herein was that of Dr. Jacobson (George Washington School of Medicine) on young women exposed to Moscow Signal. Findings may indicate abnormal genetic activities in some of the women. Significance is not established. There was general discussion of chromosomal aberrations and its causes.

C. It was recommended that further studies be developed: (1) Walter Reed facility and program advancement be encouraged. Extend animal studies and initiate human studies. Suggestions for protocols were made, e.g., study include four men involved for six to eight months. Study to be in two groups and double-blind. (2) Programs to be developed to take advantage of land-based radar installations.

4. It was recommended that the Walter Reed group prepare and present a detailed review of the field, i.e., their activities with reports of their findings, protocols, etc., since projects were begun. Also, a review of relative contract work supported by ARPA and related to Walter Reed efforts as well as any related studies of others in field.

Respectfully submitted by Lysle Peterson, Chairman.

DETAILED MINUTES OF PANDORA MEETING OF APRIL 21, 1969

Present.—Science Advisory Committee: Dr. Joseph E. Barmack, Dr. H. Allen Ecker, General Frederic J. Hughes, Jr., Dr. Joseph F. Kubis, Dr. Lysle H. Peterson, Chairman, Dr. Herbert Pollack.

Walter Reed Army Institute of Research: Colonel Joseph V. Brady, Dr. Thomas W. Frazier, Mr. T. Daryl Hawkins,[1] Colonel Merrill C. Johnson, Major James T. McIlwain.

Mr. John F. Collins (CNO), USN.[1] Mr. H. Mark Grove, Wright-Patterson AFB. Mr. Albert Rubenstein, ARPA. Mr. Harris B. Stone (CNO), USN.

Dr. Pollack reviewed events since previous meeting. He noted the formation and meeting of a new committee (ERMAC**) from the Office of Emergency Planning and chaired by General James D. O'Connell to consider microwave radiation. This panel resulted from legislation setting HEW as the responsible agent for microwave radiation health considerations. He also noted that a document had appeared by Mr. Rexford Daniels under contract to the Office of Telecommunications Management. This document has been classified. He then noted that "Big Boy" shipboard exercise had been completed insofar as the dock-side and shakedown cruise activities aboard the *Saratoga*. Dr. Kubis would be submitting a report of activities and findings to date.

Dr. Kubis reviewed "Big Boy" objectives and events to date:

A. *Objective:* To study certain behavioral and physical functions of selected crew aboard the *Saratoga* in order to ascertain effects of microwave radiation on man regarding shedding light on effects of Moscow Signal, i.e., nonthermal effects (if any) of radiation of radar origin.

B. Procedures: 1. Three groups of ship's crew were selected:

(*a*) Flight deck crew (eight in number). Highest levels of exposure expected.

(*b*) Hangared deck crew (fiifteen in number). Low levels expected.

(*c*) Look-out crews (eight in number). No exposure expected.

2. Dock-side control tests conducted from Jan. 27 through Jan. 30, 1969. Five-man team under Kubis with excellent cooperation of naval medical personnel. (Details will be included in Dr. Kubis' report.) Batteries of tests included performance (e.g., aiming, depth perception, etc.) and written procedures. Also, base-line physical exams were given.

3. *Seagoing* tests were performed while ship was under way for shakedown cruise Jan. 30, 1969, through Feb. 10, 1969.

4. *Summary.*—Sixty-seven tests were performed in three days at dock-side, and ninety-two tests were performed at sea. Forty-seven tests represented retesting of control material. There were a number of disturbances regarding shipboard routine, etc., e.g., high noise levels, P-A system interrupted activities in "quiet room," general quarters intrusions into test areas due to routines variable

[1] First attendance.

line voltage which affected equipment. Three dock-side test days conducted by five-man team; eight sea test days conducted by three-man team. It was regarded by Dr. Kubis that the testing was satisfactory and that the interruptions were not significant. Most test procedures were good (0.9); some were poor (greater than 0.1).

5. *Findings.*—There were no significant differences in the dock-side tests, i.e., among groups, and there were no significant differences among groups in the under-way tests, i.e., about 50 percent showed some increases in performance scores and about 50 percent showed some decreases. It was recommended that isolated power supply for instrumentation be developed if further work is to be done aboard ship.

Mr. Mark Grove reviewed the measurements and monitoring of shipboard radar levels. The ship was swept re two primary radiation sources, i.e., SPS–30 (S-band radar) and SPS–43 (UHF) search radar. It was expected from a naval electronics lab report that there might be greater than 10 mw/cm^2 on at least 80 percent of the surface of the deck (data from destroyers). Mr. Grove and Dr. Kubis were on ship at the same time but worked independently. Used RAMCORE dosimeter, HP power meter with thermoistor bridge (re S-band) from island to bow, 9 decks.

Findings.—In no case did measured levels exceed 1 mw/cm^2 with radar operations at 80 to 90 percent of utilization rate. Ranges of findings were in most cases less than 0.3 to 0.03 mw/cm^2. It was noted that verbal reports from shipboard engineers, i.e., engineering center and ship's hazards group, had indicated these findings. It was suggested that such reports had been made to the SEC.

C. *Discussion:*

1. Cover story considered appropriate and worked well (Robert Stone).

2. Aircraft radar and HERO effects were discussed. Effects and indications were insignificant.

3. Radiation from radar generators considered, i.e., 50 to 100 KV X-ray generation from tubes. Considered that lead shielding was adequate.

4. Mr. Rubenstein indicated that there were several excellent land-based radar sites which might be appropriate as study sites.

5. Blood studies from Saratoga crew were discussed. Colonel Johnson reported that twenty-one samples were recovered at San Juan. Seventeen successful cultures obtained with 288 spreads photographed. These were coded for double-blind studies. Code not yet revealed. Although two abnormalities were found, they were regarded as in normal range.

6. Several discussions of genetic aberrations of leucocyte nuclei covered several areas:

(*a*) Colonel Brady's three monkeys exposed to date at Walter Reed (thirty days) to special signals. One of these was the initial one showing abnormal chromosomal changes. Plan now is to get samples during exposure and with larger group of animals.

(b) Dr. Pollack reported the studies of Dr. Jacobson at George Washington University who has studied young women returning from embassy in Moscow (State Dept. contract). One hundred forty blood samples were examined over a four-year period of time. These specimens were identified only by code numbers. Four of these reportedly showed serious chromosomal abnormalities. Colonel Johnson reviewed these reported findings with Dr. Bender of Oakridge. The latter expressed the opinion that the evidence was based upon weighted data which may not be acceptable to all experts in the field.

(*c*) Dr. Johnson described the general character of chromosomal abnormalities re probabilities, stillborns, mongolism, chemicals, and drugs, etc. It was described that while bone marrow testicular tissue etc. might be better tissue to study (higher rates of replication), most knowledge is based upon leucocyte studies.

(*d*) The older Lilienfeld (John Hopkins) studies were of Korean and WW II veterans relative to the incidence of mongoloid children born to them. It was concluded that the earlier study was not well designed to reveal data regarding current interest. Study indicated that eighteen of twenty-five mongoloid children had fathers who had been exposed to radar. It is now proposed to expand the study in Baltimore and possibly in Washington. It is likely from the incidence and population of mongolism that the number cannot be increased beyond 50 percent, i.e., from twenty-five to about thirty-six. It was suggested that the original twenty-five and additional cases should be studied in detail, i.e., cytogenetic studies of testicles and lymphocytes. It is also concluded that the study

may not answer the question. It was proposed that the study should cost $100,000; $50,000 from ARPA and $50,000 from NIH. The objective should be to validate the earlier study; i.e., cross-validation seems appropriate. The study may be regarded as in three phases. Phase 1 may be supported. Later phases should not be funded unless Phase 1 defines an appropriate study, i.e., a milestone decision should be made.

General discussion concluded that additional work is required to investigate whether or not appropriate radiation levels and type have genetic effects on man. Shipload versus land-based studies were discussed. It was concluded that land-based radar studies should be seriously considered and planned if appropriate. Details of George Washington University study were lacking to the group.

8. Dr. Brazies' (New Orleans) studies on material sent from Walter Reed are not completed as yet. Contractor does not know what the exposure is nor in which animals. (Does know that it is microwave.) He reported that one monkey (exposed to special signal) showed significant changes in the auditory and visual cortex but not in deeper structures. He is now studying two other monkeys and four dogs. These reports are due this year.

It was concluded that the contractor should be urged to proceed as rapidly as possible, i.e., with urgency.

9. It was concluded that more animals should be exposed and studied. There was discussion of the new facilities developing at Walter Reed. They are expected to be completed soon.

10. Contract with Dr. Ross Adey reviewed. Adey's studies have been concerned with modulated A.M. (3 to 10 cps) and C. W (4.6, 2 v/meter) and S-band radar modulated with EEG. He is continuing monkey studies regarding EEG and reaction-time. He has a contract with Northrup for the study of reaction times in electrostatic fields. It is felt that, although Dr. Adey's work is not directly concerned with the important questions of the effects of VHF on CNS function and that of excluding the electrical effects as artifacts, his work is related to the general field of the effects of radiation and CNS function. Also, he is assisting Walter Reed with EEG evaluation and data processing. Current level of support is about $135,000 annually. It was concluded that Dr. Adey should be informed that his own priorities and work trends are not entirely matched with those of ARPA. Although his work is related to the general field and is of considerable assistance to the Walter Reed effort, it is thought that his support might be phased out in a year or two after the Walter Reed facility is better developed.

11. A contract already funded by ONR with New England Institute for Medical Research (Dr. Heller) was reviewed. It was suggested that the content of this work is not directly appropriate to this subject although that institute has a microwave facility.

12. A contract with the Milton Zaret Foundation of Scarsdale, New York, was reviewed. Contractor also uses facilities of Brooklyn Polytechnical Institute. Work primarily in 700 p.p.s. (1–10 u. sec.) range. Is attempting to confirm or reproduce Myrra, Czechoslovakia, work, i.e., production of differences in heart rate at subthermal levels, This review was for information.

13. Other contracts were also reviewed: (*a*) Dr. Dordano of Johns Hopkins in A. M., F. M., C. W., S-band frequency work on monkeys. (*b*) Dr. Sol Snyder of Johns Hopkins is studying neuropharmacological effects, e.g., turn-over rates of norepinephrine and serotonin. (*c*) Dr. Justison (Kansas City, Missouri) using microwave Tappin oven is studying hypnotic and soporific effects of low power level microwaves. It is thought that the geometry of the oven may provide higher power levels than predicted.

14. It was estimated that current funding in the area is $500,000 outside and $200,000 in-house (does not include reconstruction and development costs at Walter Reed or computer facility).

15. It is regarded that the new facility providing three chambers and data processing including (Hewell Packard) general purpose computing, record and reproduce capability for time series, coherence, cross-and auto-correlations, etc., real time capability will be ready between mid-June and August.

16. The priority of questions of interest to ARPA was discussed. It was reiterated that the elucidation of the Moscow Signal remains as a high priority question with the general field of the effects of microwave radiation on man in order that safety standards may be rationally developed. It was also noted that, aside from the work of Dr. Jacobson, there was apparently no other relative work being conducted by Federal agencies.

17. The need for information on humans in addition to the accelerated animal studies at Walter Reed was emphasized. Therefore, it was urged that the Walter Reed facility develop a human program and start immediately to develop Phase 1, i.e., to develop a plan and protocols.

(*a*) Suggested putting the human aspect into the Pandora program rather than in the Walter Reed stress program.

(*b*) Estimated that human subjects would be required for six to eight months and that they could be obtained from Ft. Dietrich.

(*c*) Controls should also include as many variables as possible including IQ, memory and performance testing. Control period should be less than sixty days. One or more should go through the entire procedure without exposure, and one or more with alternating exposure plan. Study should be double-blind with protection of eyes and gonads. Shielding of testicles is recommended.

(*d*) Panel would like to review protocol before enactment.

18. Land-based radar station human study program should be developed to replace or supplement any onboard studies at sea.

Respectfully submitted by Lysle Peterson, Chairman.

MINUTES OF PANDORA MEETING OF MAY 12, 1969

Present.—Science Advisory Committee: Dr. Joseph E. Barmack, Dr. H. Allen Ecker, General Frederic J. Hughes, Jr., Dr. Joseph F. Kubis, Dr. Lysle H. Peterson, Chairman, Dr. Herbert Pollack.

Walter Reed Army Institute of Research: Colonel Joseph V. Brady, Dr. Thomas W. Frazier, Mr. T. Daryl Hawkins, Colonel Merrill C. Johnson, Major James T. McIlwain.

Dr. John F. Collins (CNO), USN. Mr. H. Mark Grove, Wright-Patterson AFB. Mr. Albert Rubenstein, ARPA.

(S) The primary order of business was the preliminary protocol proposal for human studies which was requested at the previous meeting on April 21, 1969. The protocol had been distributed toward the end of the previous week and, therefore, had not been received by many of the panel. Time to pursue the proposal was taken before discussion began.

(S) Dr. Brady noted that the proposal had been the combined effort of himself, Thomas Frazier, Merrill Johnson, and Daryl Hawkins and desired the advice of the committee on the ninety-day protocol. Dr. Brady noted that they had considered two basic strategies: (i) assumes that there is an effect of the signal (based upon previous experience) and the protocol is designed to maximize the yield and (ii) assumes that there may or may not be an effect (nue hypothesis) and the protocol would include "extreme" operations, i.e., high-forcing functions and large "n"s. If an effect is seen, then fine responses are defined.

(S) In view of previous experiences and evidence available, the first alternative was chosen, i.e., based upon the assumption that there is an effect. Therefore, protocol is an attempt to optimize economic considerations, use small "n"s and primarily to define the effects of the signal.

(S) The panel discussed the over-all strategy and alternatives and agreed with Dr. Brady. Also, re human experiments, this approach regarded most defensible as a prerequisite to more demanding studies, if needed.

(S) It was noted that a major question regarding any such study relates to the evaluation of behavioral effects since the spectrum of possibiilties is so broad compared to physical evaluation. Thus, the major part of the discussion related to behavioral aspects of the program. It was also noted that any energy form, if large enough, will produce biological effects. It was agreed that the signal used would be the special signal at the levels developed and used with the primates, i.e., between 4.5 and 5 mw/cm^2. Discussion revealed several distinct questions:

(i) Because the "n" is small (eight subjects) there, the question was raised as to whether the protocol will permit the characterization of the group, i.e., significance of the findings in individuals in a small "n" group.

(ii) To what extent is the instrumentation appropriate to carry out the objectives of the experiment, e.g., signal beam incidence, range of power levels, polarization, etc. The protocol had not detailed the electromagnetic aspects of the experimental design. Also, what are the effects, if any, of the signal on the instrumentation, e.g., EEG electrodes?

(iii) What are the dependent variables re behavior?

(iv) What are the considerations relative to monitoring the physical (biomedical) parameters re two purposes: as a monitor of the subject's general health and as scientific data re effects of signal?

(v) What are the classification considerations of the program re its management and scientific effectiveness?

(S) The discussion provided consensus regarding these points as follows:

DOD regards the general line of effort to aquire human-based data on effects of the signal, with appropriate safeguards, as a high priority. ARPA believes that the entire effort should be classified for several reasons. It was urged that DOD provide written security specifications and guide for the program.

(S) An appropriate cover relates to the purposes of the program to evaluate the validity of U.S.S.R. reports that nonthermal effects of nonionizing radiation are significant.

(S) It is urged that the special signal (or any improved signal, i.e., to better simulate the Moscow signal) be used. Currently, special signal-producing available equipment can develop less than 10 or 20 mw/cm^2. Monkey studies have been done at 4.6 mw/cm^2. Also recommendation to use same carrier frequency. While polarization can be varied, it was urged that the same polarization (radiation beam toward back of animal and vertical) be used in humans but that absorbent seat and gonadal protection be provided. While posterior presentation is utilized, protection of eyes should be considered.

(S) It was recommended that a medical examination function be established as a separate entity from the research function. Thus, the physical well-being of the subjects would be ascertained and reviewed periodically by medical expertise, which is not directly associated with the purpose of the effort. This medical examining function would not be privy to Pandora but would be given the cover story. It was noted that this separate examination procedure, if properly defined, could provide useful data as well as a safety check for the program. General Hughes thought that such a medical evaluation function could be arranged through the new commander of Walter Reed. In view of the fact that the morphological changes (cytological) which have been found in the CNS of animals exposed to the signal appeared in the visual cortex (as well as other areas), flicker fusion studies should be incorporated into the medical examination. Also, slit lamp and visual field checks should be made and audiograms done. It was also recommended that a separate psychiatric evaluation should be accomplished before and after the study. It was not resolved as to whether there should be separate psychiatric screening in addition to the research program screening procedures. This separate medical function or task force may be referred to as the "medical monitoring task force." It was recommended that a specific chain-of-command be established to be certain that in the changing personnel structure of Walter Reed. the appropriate responsibiilties are established, and thus, the research team will know whom to work through re the medical monitoring function. It was recommended that the medical monitoring procedure include:

Slit lamp examination: initially 90 days, 180 days.

Visual fields examination: initially 90 days, 180 days.

*Audiogram: initially 90 days, 180 days.

ECG: once per week.

**Physician perform general check-up once per week.

*At end of day.

**Have responsibility to be certain that all data are entered on record.

(S) Although the panel expressed some concern that the experiment, as defined by the protocol, might result in findings which would be difficult to characterize as definite indications of effects of the signal on man, i.e., the problems of characterizing changes in each individual and as his own control and of satisfying criteria of group statistics. It was recommended that at least one of the subjects and perhaps two go through the entire procedure except that they not be exposed to the signal. The fact that there are to be two rooms would facilities this approach.

(S) There was also continuing concern expressed regarding the effects of the signal on the measuring system itself, i.e., signal artifacts. The panel recommended continued scrutiny of this problem. The panel requested an up-to-date bibliography of the effects of microwaves on biological systems. It was noted that there is a recent article in the Canadian Journal of Microwaves.

Respectfully submitted by Lysle Peterson, Chairman.

MINUTES OF PANDORA MEETING OF JUNE 18, 1969

Present.—Science Advisory Committee: Dr. Joseph E. Barmack, Dr. James N. Brown[1], Dr. H. Allen Ecker, Dr. Lysle H. Peterson, Chairman, Dr. Herbert Pollack, Dr. Lawrence Sher.[1]

Walter Reed Army Institute of Research: Colonel Joseph V. Brady, Dr. Thomas W. Frazier, Mr. T. Daryl Hawkins, Colonel Merrill C. Johnson, Major James T. McIlwain.

Mr. Richard S. Cesaro, ARPA. Dr. John J. Collins, (CNO), USN. Mr. H. Mark Grove, Wright-Patterson AFB. Mr. Albert Rubenstein, ARPA.

Absent.—Dr. Joseph Kubis, Science Advisory Committee, Dr. Joseph C. Sharp, N.Y. State Dept. of Health, Mr. Harris B. Stone, (CNO), USN.

(S) The minutes of the previous meeting (May 12, 1969) were discussed. Dr. Peterson explained that the questions regarding the WRAIR protocol were stated as those which were raised for discussion at that meeting. The discussion associated with these questions resulted in a general agreement that the protocol in general provided an appropriate initial approach with human subjects. Furthermore, the nature of the problem together with constraints of available time, facilities, personnel and funds, and the scope of the approach result in statistical problems from the use of a small "n." A protocol using significantly more subjects would be more desirable but would delay the program considerably. It was agreed that for the first approach, the small "n" would be acceptable. The primary purpose of this meeting (June 18, 1969) was to consider the approach in more detail and to hear reports from ARPA and WRAIR regarding broad approaches to the solution of the Pandora and related problems:

(S) Mr. Cesaro reminded the Committee of the urgency and high priority which ARPA assigns to the Pandora program; it is important to make major advances in solving the problem in FY 70; the Advisory Committee was invited to give as much attention and creative thinking to the problem as it can. He defined the Advisory Committee's role and that its effectiveness would be enhanced by maintaining a close and continuing association with the Walter Reed group. His view of the overall problem was summarized:

1. Investigative programs should be designed to take major bites at the problem to achieve definite indications of whether or not there are effects on humans of microwaves under conditions simulating, as closely as appropriate, the Moscow signal. Furthermore, he urged that the experimental programs be relevant to the Pandora problem and provide significant results, negative or positive. He reminded the Committee that, conversely, it would be inappropriate to follow paths which while they might be interesting scientifically, would not be relevant to the problem.

2. While there is evidence that low energy (less than 10 mw/cm^2) R.F. radiation does penetrate to the CNS in primates, a significant and relevant question is how the penetration occurs, i.e., the mechanism re microwave characteristic and biomedical engineering principles. Such questions relate to whether or not the Moscow signal is unique and to whether the Soviets have special insight into the effects and use of athermal microwave radiation on man. The WRAIR experience indicates that C. W. has no effect, but modulation does.

3. There appears to be interrelationships of signal time and biological effects, which should be evaluated, i.e., biological on-off effects phased to the on-off character of the signal, i.e., short response times, intermediate and long time effects together with intermittent and long exposures, i.e., at least nine combinations possible.

4. The biomedical effects may include (i) physiological, (ii) behavioral, and (iii) genetic. Each general class re short and long time effects should be evaluated.

5. Man should be emphasized as the object in the investigations both because he is the obvious recipient and because he is in many ways a more effective reporter of effects.

6. Behavioral investigations should include attitudinal (subjective) considerations and measures.

(S) Discussion centered next on details of the WRAIR protocol. There was a consensus among the WRAIR and Committee members that the general approach of the protocol is appropriate and that in the initial stages of the effort, many "on-the-spot" decisions would have to be made because of the uncertainty of

[1] First attendance.

aspects of the program. As these uncertainties are clarified, the protocol would become better defined. The areas considered which require initial exploration to provide better definition are:

1. Exposure. It was the concensus that the aim should be to duplicate or simulate the Moscow environment as much as appropriate to solve the problem. Since exposure to the Moscow signal tended to be over an eight- to ten-hour period, it is thought that the exposure of subjects to he Special Signal should approximate that time frame. Furthermore, the primate findings were based on a seven-day, ten hours each day, exposure. It is suggested that the protocol indicate exposures up to eight to ten hours each day rather than the one-hour cycle providing four hours total per day.

2. The instrumentation artifact question re possible E. E. G. recordings be explored vigorously. The differences between primate and man with respect to head dimensions, effect of beam incidence, etc., must be reviewed and analyzed.

3. The physiological and medical monitoring include the array noted in the minutes of May 12, the matrix presented by Dr. McIlwain, portable ECG for continuous monitoring and the utilization of *lower-body, negative-pressure* plethysmography (LBNPP). It is recognized that the Soviet literature refers extensively to cardiovascular effects of microwave radiation. The nature of the reported behavior of the cardiovascular system requires relatively continuous monitoring to characterize significant effects from normal variations. Also, it is recognized that LBNPP is an accepted and relatively sensitive measure of cardiovascular "deconditioning" (in the physiological sense). In addition to the special tests, the medical monitor would be responsible for the routine physical examination, regular daily inspection of subjects and to adhere to the responsibilities associated with AR 70–25. These procedures would be worked out through the Office of the Surgeon General. Dept. of the Army, and jointly with WRAIR operational group and relative to security requirements defined by ARPA. ARPA agreed to present in writing, the security requirements for the program.

(S) The Committee recognizes that a period of uncertainty exists in the early phases of developing such a program and that the program director will require a degree of attitude in conducting pilot experiments before finalizing a detailed protocol. The Advisory Committee agreed to work closely with WRAIR in considerations of refinements of the protocol.

(S) The Committee agreed to conduct "brainstorming" sessions relative to the general Pandora problems in the context of ARPA's interests and responsibilities. The Committee expressed a need for more, yet manageable, literature review and information relative to the problem especially re the Soviet literature and from whatever sources useful information could be obtained. It was agreed that Dr. Pollack and Dr. McIlwain would assemble a "package" of useful documents to be distributed to the Committee. There are general discussion about various documents known to the attendees. It was agreed that several documents were relevant and appropriate, e.g., the IDA analysis due out soon, a document by Dr. Robert D. Turner, U.S.A Standards document, the ERMAC report, a report by Castlr and Dodge, one by Castle, one by Thompson, and the Presman summary. It was also agreed that ARPA would make available, to the Committee, any information that is relevant to the problem. It was also agreed that the agenda of the Advisory Committee would automatically include a call for a progress report and new, relevant information from Committee members, WRAIR, and ARPA.

(S) Dr. McIlwain and Data Corporation representatives presented a demonstration of the BEER project and application of a computer-based information storage and retrieval system to provide rapid access to files on microwave related information. It is expected that user needs and responses will soon be tested by ARPA and WRAIR.

(S) A correction of the minutes of May 12 is noted, i.e., that Dr. McIlwain participated as a principal in developing the WRAIR protocol.

(u) The next meeting is scheduled for July 16, 1969.

Respectfully submitted by Lysle Peterson, Chairman.

MINUTES OF PANDORA MEETING OF JULY 16, 1969

Present.—Science Advisory Committee: Dr. Joseph E. Barmack, Dr. James N. Brown, Dr. H. Allen Ecker, Dr. Joseph Kubis, Dr. Lysle H. Peterson, Chairman, Dr. Herbert Pollack, Dr. Lawrence Sher.

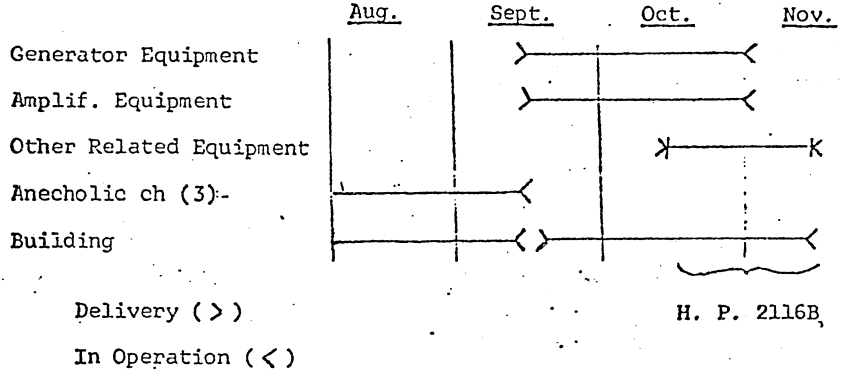
Walter Reed Army Institute of Research: Colonel Joseph V. Brady, Dr. Thomas W. Frazier, Mr. T. Daryl Hawkins, Colonel Merrill C. Johnson, Major James T. McIlwain.

Mr. Richrad S. Cesaro, ARPA; Dr. John J. Collins, (CNO), USN; Mr. H. Mark Grove, Wright-Patterson AFB; Mr. Albert Rubenstein, ARPA; Dr. Stanley Marder[1], IDA; Dr. Joseph C. Sharp, N.Y. State Department. of Health.

Absent: Mr. Harris B. Stone, (CNO), USN.

(S) The meeting was convened at 0930 at Building 503, Forrest Glen Annex, Walter Reed Hospital. The purpose of the meeting at Walter Reed was to review the developments of the facilities and programs re site, to review schedule for completion of the program and to give new Committee members the opportunity to see the laboratories.

(S) Major McIlwain and Mr. Grove led a presentation of schedules to complete the development of the facilities:



(S) Discussion included estimates of signal stability, flexibility, and obsolescence of system. Mr. Grove indicated that satisfactory stability could be expected re 15 ft. cube, that the system was modular and could be expanded, and that range extended from 250 megahertz to 10 gigahertz.

(S) Discussion then focused upon the relevance of the signals to the Moscow signal. It was stressed again that the signal used in the forthcoming experiments should (i) be comparable to previous experiments at the laboratory, (ii) be relevant to the Pandora problem, (iii) the signal characteristics be calibrated periodically as referenced to a standardization technique and (iv) possible signal artifacts (i.e., signals recorded from biomedical transducers due to direct effects of the microwave input in contrast to microwave input effects on the biomedical system) should be elucidated and evaluated.

(S) Discussion also focused upon the software program, i.e., the evaluation of data via computer system. Mr. Grove and Major McIlwain defined three software areas: (i) that developed by Ross Adey at U.C.L.A., (ii) that developed for the internal logic of the equipment to be delivered (H.P. 2116B and related system), and (iii) software needed to extend and interface with (i) and (ii). The U.C.L.A. program had been developed to test an array of statistical parameters re EEG signals on the equipment at U.C.L.A. WRAIR has selected from the total U.C.L.A. program certain aspects which were felt necessary for the WRAIR needs, e.g., auto- and cross-correlation, auto- and cross-power spectra, average transients and certain standard statistical processing. These were packaged into the logic of the equipment being delivered by Hewlett-Packard.

WRAIR felt that there were aspects of the U.C.L.A. programs (e.g., conference functions) that were not needed. To duplicate the U.C.L.A. software program would have been too costly and would require too extensive a hardware package. Thus, the special computer package approach was taken. It was not anticipated

[1] First attendance.

that extensive, additional software would be needed but that if so, contracting would be more appropriate than developing extensive in-house programming capability.

(S) In summary, the WRAIR presentation concluded that:

1. The facility (at least two chambers operational) would be ready for experimental work by mid-October unless unexpected delays occurred.

2. The signal characteristics would be comparable to previous work and that a calibration procedure would be adopted.

(Note: Discussion indicated that the calibration question related to defining the characteristics to be calibrated rather than to capability to calibrate, i.e., to define spectral and energy characteristics to be measured for calibration and standardization.)

3. With the planned program and facilities, the experimental artifact question could be approached within the inherent uncertainty levels. It is known that EEG experimental artifacts do occur and that the question remains as that in a given experiment how much of the recorded response is artifact, i.e., in levels at different EEG frequencies, to what extent is the living brain required. It is expected that the autospectral analogies will be helpful. A two- or three-month program using monkeys is planned.

(S) The Committee reconvened in executive session after lunch at Room 10K5 in IDA to review the present status of the program in the light of Mr. Cesaro's charge to the Commitee. It was agreed that the Committee would participate in certain working sessions devoted to specific problem areas and, whenever appropriate, do so in subgroups relative to the particular expertise involved, would conduct "brainstorming" sessions relative to the overall Pandora question and the various components of the program, and structure agenda for periodic, approximately monthly, meetings such that orderly progress should occur. Furthermore, contributors to the agenda would be invited as early as possible and notified of the material requested. A list of possible agenda items for the future was developed, and it was expected that this list would be considered appraised by Committee members and an agenda for the next meeting defined on or about August 1. It was agreed that the next meeting would be scheduled for August 12 and 13.

(U) The meeting was adjourned at 1700.

Respectfully submitted by Lysle H. Peterson, Chairman.

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MINUTES OF PANDORA MEETINGS OF AUGUST 12 AND 13, 1969 (U)

Present.—Science Advisory Committee: Dr. Joseph E. Barmack, Dr. James N. Brown, Dr. H. Allen Ecker, Dr. Joseph Kubis, Dr. Lyle H. Peterson, Chairman, Dr. Herbert Pollack, Executive Secretary, Dr. Lawrence Sher.

The following attended Aug. 12 only: Mr. T. Daryl Hawkins, WRAIR; Major James T. McIlwain, WRAIR; Mr. Daniel Sullivan, ARPA.[1]

Absent.—Colonel Joseph V. Brady, WRAIR; Mr. Richard S. Cesaro, ARPA; Dr. John J. Collins, (CNO), USN; Dr. Thomas W. Frazier, WRAIR: Mr. H. Mark Grove, Wright-Patterson AFB; Colonel Merrill C. Johnson, WRAIR; Mr. Albert Rubenstein, ARPA; Dr. Joseph C. Sharp, N.Y. State Dept. of Health; Mr. Harris B. Stone, (CNO), USN.

(S) I. The minutes of the July 16 meeting were approved with minor modifications, such as the omission of the fact that the Committee had requested a written summary of the WRAIR program to date and an oral presentation of the basic data.

(S) II. Dr. Peterson reviewed the purpose of the meetings (August 12 and 13) as principally that defined at the previous meeting.

A. To have a detailed, full review presented by WRAIR of the background and findings of the Pandora/WRAIR primate studies from their inception to present. Specifically: (i) A review of the background for the development of the program. (ii) Considerations of the expected sensitivity and relevance of the tests used in the program. (iii) Review all findings from all experiments. (iv) Review current protocol for future program.

[1] First attendance.

B. To hear Dr. Ecker's report on the work of his subcommittee, which was asked to look into the hardware systems being developed for future work in the program at WRAIR.

C. To discuss follow-up of Big Boy project. The Committee was then, in the remaining time, available to conduct "brain sessions," as requested by Mr. Cesaro.

(S) III. Major McIlwain presented his analysis and conclusions of the WRAIR/Pandora program. It is noted that although Colonel Joseph Brady, Dr. Joseph Sharp, and Mr. Mark Grove were invited, they were unable to attend this meeting. Also, Messrs. Cesaro and Rubenstein were unable to attend because of conflicting duties; however, they were represented by Mr. Dan Sullivan. The Committee's summary of Major McIlwain's review is as follows:

The experimental approach was defined early in 1966 mainly by Drs. Brady and Sharp with Dr. Sharp being assigned primary, direct responsibility for the operational conduct of the experimental program. The earliest studies were pilot studies. The same signal has been used throughout the study except for varied strength. This signal was selected from the "Moscow signal" tapes by a group representing various agencies. Mr. Mark Grove has had a major responsibility for providing the hardware to generate the selected "special" signal for WRAIR. Primate behavior measures were chosen by WRAIR as the primary indicators of an effect of the special signal. Studies comparing exposed and unexposed behavior were divided into three groups: 1. Reaction times, 2. Detection, and 3. Multiple schedule studies.

1. Reaction Time.—Two types of experiments were performed on two monkeys at 4.6 mw/cm^2: (i) *Shock avoidance* (No. 682) 250 trials/day in midday period and (Animal No. 683) 500 trials/day; both a.m. and p.m. periods.

Results: (a) No significant differences between experimental (exposed) and nonexposed periods, and no significant differences between a.m. and p.m. trials. was exposed to 4.6 mw/cm^2 at 11-second intervals. The second monkey was exposed to 20 mw/cm^2 at 60-second intervals.

Results: No significant detection elicited by either monkey.

Conclusion: The animals showed no response to the microwave signal at least in this regimen.

3. *Multiple Schedule.*—This was regarded as the most critical aspect of the experimental program and involved learning performances for nine sessions on four monkeys, using a progressive, fixed ratio reward (PRL) and alternating with a differential reinforcement of low rates (DRL). Six measures were examined using 4.6 mw/cm^2 as average field strength of the signal: (i) DRL distributions. (ii) Latency of the DRL. (iii) Average number of time-out responses. (iv) Average time per time-out. (v) Average pause time for each ratio step. (vi) Average running time within each ratio step. Of these, the only measure that showed any possible significant effect was the DRL distribution measure, but the data were internally contradictory. A further analysis was made of (i) the number of uninterpreted pauses exceding ten minutes, (ii) work stoppages for any reason, including equipment malfunction, and (iii) total number of pellets earned.

Results: In two of the nine sessions in which the full-power special signal was used, uninterpretable data were obtained either because of equipment malfunction or the use of reinforcement schedules (which reward certain types of stoppages) were recognized as contributing themselves to work stoppages. In two of the nine sessions, there was no effect of the signal; in three (or four) of the nine sessions, there were effects, but they were not cumulative; in one or possibly two sessions, there were unexplained work stoppages which, in the light of the total experience, appear to be random variations found as often in control as in exposed sessions.

Conclusions: There is no convincing evidence of an effect of the special signal on the performance of monkeys.

In addition, Dr. McIlwain acknowledged that the studies had primarily involved behavioral analysis with little effort made to analyze physiological functions, e.g., cardiovascular.

The Committee was told that Dr. McIlwain had not had the opportunity to present his analysis and conclusions to Drs. Sharp and Brady although he had

briefly and informally conferred with Dr. Brady. Although the Commitee had not previously had any other detailed analysis of the WRAIR program findings, it was apparent that the conclusions presented earlier by Dr. Sharp were significantly different from those of Dr. McIlwain.

The Committee felt that because of the importance of these differences, the WRAIR program principals, Drs. Brady, Sharp, McIlwain, etc., should attempt to resolve the inconsistencies of their conclusions. The significant differences are, of course, whether or not there are established effects of the special signal on primate behavior as evidenced from the WRAIR program. It was also noted that no technical report had been written regarding the program to date. Dr. McIlwain agreed to furnish the Committee with a written summary of his presentation. The Committee expressed the desire to analyze the raw data themselves.

(S) IV. Dr. H. Allen Ecker presented the findings of his subcommittee on the functions and characteristics of the instrumentation being developed for the expanded WRAIR program. (See minutes of July 16.) A written summary of Dr. Ecker's report is appended (Exhibit A). In discussion, it was concluded that the capacity of the instrumentation exceeds present requirements, but the rational that if the program is expanded and extended, that added requirements will develop, is probably sound.

Discussion regarding the special signal was withheld until after Dr. Ecker's presentation although this major attempt to duplicate it must start from a thorough analysis. To date, the analysis has not, apparently, been thorough; a deficiency in the Pandora effort.

Mr. Daniel Sullivan of ARPA was requested to see whether the signal analysis could be made available to the Committee.

(S) V. A brief review of Big Boy reminded the Committee that the results of the Saratoga study were essentially inconclusive although the testing technology and programs were worked out and proved feasible and reliable with certain suggested improvements. (A report has been written of the Saratoga effort.) A possible reason for the inconclusiveness of the Saratoga study was that the levels of microwave radiation measured at sites where tested personnel were located were found to be extremely low and the exposure time very slight. The Committee recommends that the approach of using established, operational radar stations to test the effects of microwaves on humans be followed-up. Dr. McIlwain indicated that he and Mark Grove had looked at the possibilities of several ground-based establishments, such as those at Monmouth, White Sands, Fort Sill, Hawk missile systems (e.g., El Paso), etc. Their conclusions are that populations of men exposed to radiation are too unstable to be effective for tests. It was also noted that the Navy has at least seven communications * * *

INSTRUMENTATION REVIEW

On 31 July 1969 Dr. James N. Brown, Mr. Mark Grove and Dr. H. Allen Ecker met at Georgia Tech to review the planned instrumentation for the new facility at WRAIR. Since the schedule for completion of equipment was discussed at some length at the last Pandora Panel Meeting and is recorded in the minutes of that meeting, further discussion of the schedule did not appear necessary. The two major items that were reviewed are (1) the Signal Generation Equipment and (2) the Data Recording and Processing Equipment. Neither the variables to be measured, the method of their measurement nor software requirements was discussed.

Data recording and processing equipment

A simplified block diagram of the Data Recording and Processing Equipment for the new facility is shown in Figure 1. Hewlett-Packard is the prime contractor for design and installation of this system and a very flexible and well-coordinated system which offers excellent data analysis capability has been designed. The use of pre-programmed analyzers such as the new Hewlett-Packard 5450 Fourier Analyzer provides pre-programmed analysis techniques and therefore, reduces the initial software requirements. Both analog and digital data storage equipment is included. Care has been taken to provide calibration and monitoring capability throughout the system. Since the variables to be

measured and analyzed were not defined at the time of equipment design, it appears that the money available for this instrumentation was used wisely to provide a very flexible data recording and processing system.

A suggestion for increased capability is the use of a Honeywell Visicorder in place of the currently planned HP 7878 Ink Oscillograph. The visicorder offers a bandwidth of 5 KHz with an extension to 10 KHz with reduced amplitude; whereas the HP Ink Recorder has a maximum bandwidth of 150 Hz. In any system in which input variables are unknown, bandwidth can be a limiting factor on flexibility of the processing system.

Signal generation equipment

The task of designating the system to generate the desired RF signal was accomplished in house with Litton Industries providing the special high voltage power supply. A preliminary block diagram of the basic components of the system is shown in Figure 2. The basic RF oscillator is an HP 8690 Sweep Oscillator. The frequency modulation signal is generated by summing the output from two Hewlett-Packard 3300 Function Generators and a General Radio 1390 Noise Generator; current plans do not call for amplitude modulation. However, a PIN modulator could be inserted between the basic oscillator and the first amplifier as indicated by X on the block diagram. A PIN modulator is an easy and flexible way to provide amplitude modulation with minimum interaction with the desired frequency modulation.

A two stage drive section in the form of sequential 1 watt and 10 watt TWT amplifiers is part of the present design. TWT's with 30 dB gain are common; therefore, in a conventional system, drivers separated by only 10 dB in output would not be an efficient system; the desire for extreme linearity prompted the choice of this configuration.

Provisions are made to monitor the power level and the frequency spectrum after the 10 watt TWT. The results of this monitoring will be available to the Data Processing System.

The present plans call for Klystrons to amplify the signal to a 1-to-12 kilowatt level. An amplifier of this type in the frequency range of interest would have only approximately 5 MHz bandwidth with 40 dB of gain. Since only about 23 dB of gain would be required to produce 2 kilowatts from a 10 watt drive, it would be possible to stagger tune the Klystron amplifier sections and increase the bandwidth at the expense of gain; perhaps as much as 15 or 20 MHz bandwidths could be achieved. A major design item in the Signal Generation System is the high voltage power supply and associated protective circuitry for the Klystron amplifier. At present, a 10 kilowatt-1 amp power supply is being designed.

After the Klystron amplifier, monitoring of the output power will be provided as well as isolation from undesired or reflected signals. The transmitting antenna will be a Scientific Atlanta Standard Gain Horn with approximately 18 dB gain. The three dB beamwidths in the E and H planes are 23° and 22° respectively. A probe antenna and associated thermistor mount and power meter are provided down range for monitoring power levels at that point.

The signal generation system as proposed provides a flexible source for a frequency and amplitude modulated RF signal with the exception of the final Klystron amplifier. Octave bandwidths are common practice in TWT amplifiers whereas Klystrons usually exhibit less than a one percent bandwidth. If one is assured that the signal to be simulated will never exceed the bandwidth limitation of the Klystron amplifier, then the proposed system will be adequate. However, if the possibility of a larger bandwidth requirement exists, the search for broader bandwidth and perhaps higher powered output tubes should be made. Both Hughes and Varian advertise 1 KW CW TWT amplifiers in the frequency range of interest. A recent advertisement by Varian in the March 1969 Microwave Journal indicates that 10 KW CW TWT's have been built and delivered. Also, recent developments in CW cross field amplifiers could provide a very efficient and reasonably broadband final transmitter stage. Both Ratheon and SFD have made significant advances in the development of cross field amplifiers. It should be emphasized that the system was originally conceived before the above tubes were catalog items. Also, cost and scheduling can be limiting factors.

The choice of a standard gain horn for the transmitting antenna provides a simple method of coupling to free space. However, the far-field criterion must be satisfied to establish a uniform plane wave over a region in space. If a larger area and a more uniform wave is desired, a large collimating reflector type

antenna could be substituted for the standard gain horn. Of course, a large reflector antenna would be much more expensive.

Concluding remarks

The results of the review of instrumentation indicate that at this point both the Data Recording and Processing System and the Signal Generation System have been well planned. However, two pressing issues must be resolved. (1) It is imperative that rapid decisions be made on the variables to be measured and on the desired methods of analyses to permit determination of required software. (2) A more thorough analysis of the RF signal to be simulated is necessary. The bandwidth requirements of the output amplifier hinge on the results of that analysis.

Respectfully submitted,

H. ALLEN ECKER.

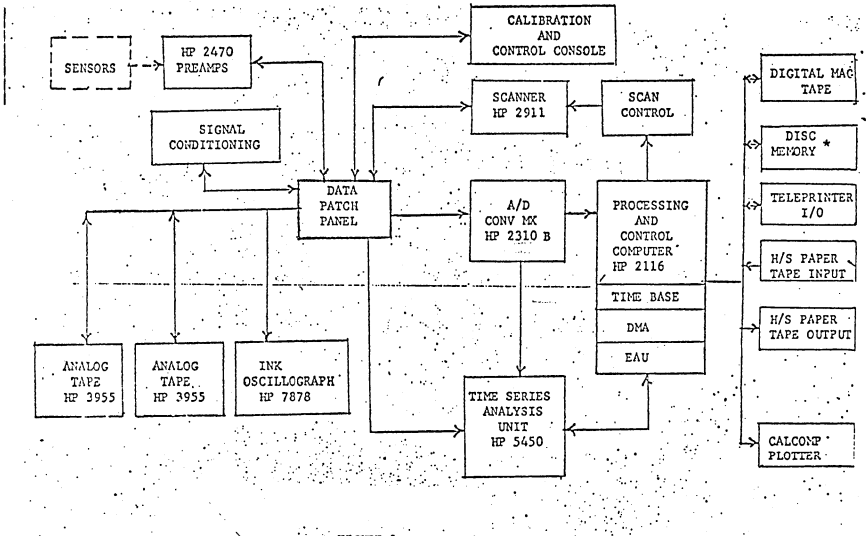

FIGURE 1

9 August 1969

* Not part of original purchase

On the Evaluation of Data Associated With Pandora

Preliminary Report by Joseph A. Kubis 12/4/69

SECTION 1: INTRODUCTION

Found

In a letter dated October 29, 1969, Dr. Lysle H. Peterson invited me to help in "developing criteria for the evaluation of data associated with Pandora." I agreed to talk with the project investigators, examine whatever records were available, and prepare a report of my findings and recommendations.

My purpose in meeting with those intimately connected with the project was to solicit ideas on how they felt the data should be evaluated. The examination of basic and derived data was not only to gain an understanding of the analyses already made but also to recommend additional procedures the structure of the data might suggest.

Finally, I suggested that whatever findings and recommendations I would make be reviewed by Dr. Peterson, who with the assistance of Dr. Pollack, would decide whether further effort on my part would be desirable.

Problem

The underlying problem which led to this report was the existence of two different approaches to the analysis of Pandora data—one by Dr. J. Sharp, the other by Maj. J. McIlwain. In Dr. Sharp's view, although most of the experiments produced negative results, certain aspects of the data (IRT changes and work stoppages) are suggestive of an exposure effect due to the WRAIR signal. Admitting that some changes in the animal's behavior could not be explained by artifacts, Maj. McIlwain believes that there is insufficient consistency in the data to seriously entertain the notion of an exposure effect due to the special WRAIR signal.

The evidence adduced by Dr. Sharp was a series of graphs and a cataloguing of the instances of work stoppages attributed to the special signal. Maj. McIlwain's position is that there are clerical errors in one of the graphs and that the sequence showing a shift in the IRT distribution is a function of the days selected for graphing. He has presented graphs from baseline periods that are similar to the graphs in the IRT sequence. As for work stoppages, Maj. McIlwain noted that a number of them could be associated with mechanical and other failures. Acknowledging that such difficulties limit interpretations, Dr. Sharp believes that there are some runs that are artifact-free and that these should undergo intensive analysis.

Appointments were made with the following individuals associated with Pandora: Col. J. Brady, Col. E. Buescher, Mr. R. Cesaro, Maj. J. McIwain, Col. W. Meroncy, Mr. A. Rubenstein, Dr. J. Sharp.

During these visits a number of reports were examined as well as the basic and derived data from the experiments. A description of the visits made and the materials examined may be found in Appendixes A and D.

SECTION 2: COMPONENTS OF THE PROBLEM

As stated in Section 1, the problem seems simple enough. However, it is embedded in a mosaic of components, which, if clearly understood, will place the problem in proper perspective.

Project as a whole

Essential to a proper evaluation of the problem is the consideration of how the project as a whole was viewed. Dr. Sharp interpreted his task as exploratory in nature—a series of pilot experiments to search for leads that might prove fruitful in a more extended research effort. An examination of the experiment protocols does support this interpretation: many experiments were tried, many conditions were tested, and many changes in procedure were introduced during the experimental program.

The inevitable outcome of a pilot program is an accumulation of a wide diversity of data, fractionated into blocks obtained under various combinations of conditions. Experimental outcomes, uncontaminated by changes in condition or possible influence of several factors, are hard to come by. A particularly pertinent example from the Pandora experiments may be found in Appendix C.

A second consequence of a pilot program is that conclusions cannot be asserted with great confidence because of the limited number of observations that are available. Statistical tests performed on even promising segments of data might very well lead to insignificant results because the sample size is small.

From the nature of the case, then, the conclusions generated from the Pandora experiments at WRAIR must be considered as tentative and suggestive except for segments of data sufficiently numerous and free from the contaminating effects of changes in condition or intrusion of uncontrolled factors.

The WRAIR experimental program contains an imaginative complex of procedures which tap various behavioral functions that are uniquely packaged into a well-motivated work day for the monkey. A description of the various "jobs" comprising the monkey's work day may be found in Appendix B.

As structured at present, the experimental program generates data from the same monkey exposed to a variety of conditions. At times he is exposed to a variety of conditions. At times he is exposed to a signal, at times not. Each monkey is his own control. This is an efficient design provided that a minimally varying baseline can be achieved, that the baseline is stable over relatively long periods of time, that the behavior observed is sensitive to the experimental "intrusion," and that strict control over most of the confounding conditions can be maintained throughout the experiment. When these conditions cannot be easily met, the design can benefit appreciably from the inclusion of control subjects.

From an interpretative point of view, it is felt that the results of the Pandora experiments would have been more discriminative with the addition of control animals. In particular, it would have been desirable to control for the effects of "isolation" and "confinement." Over a long period of time, say several months, it is possible that continued isolation and confinement could bring about an erratic temporal pattern in the monkey's performance.

One further point about procedure. The complexity of the present task routine makes inevitable a long turn-around-time for a single animal. He requires a long training period and is subjected for a long time to a stimulus whose measurable effect may be long delayed. Inevitably this means that the experiment is based on a small sample with an effect that may be weak or ephemeral. In turn, this leads either to very limited generalizeability or to inconclusive statistical results. At worst, it could lead to accepting as significant results that reflect long-range and unknown fluctuations in the animal's condition in no way related to the experimental signal. Expanded facilities, of course, would help since more experimental and more control animals could be used. In addition, the use of tasks or animals with a shorter turn-around-time should also be considered.

Flaws in test conditions and baseline

As mentioned earlier, test conditions were changed relatively frequently. Some changes were due to limitations of test facilities, for example, having the animal tested or trained in the "ice box" and then tested in the anechoic chamber. In other cases, changes were introduced to determine if a facilitating or debilitating effect would ensue, as, for example, changing the character or strength of signal exposure. In any event, the greater the number of changes in conditions (especially if used in combination), the smaller the chances that an adequate baseline would be obtained to test for the specific effect under consideration.

As illustrated in Appendix C, the animal was exposed to a number of differing conditions in various combinations within a relatively short period of time. These were: night vs. day work, alone vs. with another animal, "ice box" vs. anechoic chamber. WRAIR signal on vs. WRAIR signal off, new chair, new lever, new speaker vs. old chair, old lever, old speaker. Good baselines for testing the effects of all these changes might prove impossible of attainment.

Adequacy of baseline is a serious but not hopeless problem in the WRAIR experiments. The basic shortcoming is that the number of observations for the appropriate baseline may be small.

Functioning system

The usual mechanical difficulties were experienced during the course of the Pandora experiments: feeder jam, empty feeder, ink failure, etc. Power fluctuations and circuit malfunction also occurred.

One of the obvious results of such malfunction is loss of data. The problem, however, is more complicated. It is very likely that the finely trained animal may have had his habit patterns disturbed because of the inappropriate cues or inadequate food rewards. His "confusion" and altered motivation may have affected his behavior for the remainder of the day and possibly for a day or more after the malfunction. The fundamental questions are: Subsequent to the identified

malfunction, how much data should be discarded as unreliable? How reliable is the data immediately before the difficulty was identified?

Relevant criteria for these situations may have been developed by the laboratory in the course of its experience. In any event, an informed opinion to corroborate the adequacy of such criteria should be obtained from an outstanding expert in the field of animal experimentation. It is suggested that such information be obtained from Col. J. Brady.

Analytic procedures, etc.

Interpretation of data is strongly connected to the model assumed to underly the data. Similarly, the model will tend to determine the character and direction of the statistical procedures one would employ to analyze the data. As a simple example, the model may specify that the exposure signal enhances the functioning of the organism, or that it causes a deterioration in its functioning, or that it enhances some functions and brings about a deterioration in others, or that it has no observable effect at all.

The analysis and interpretation of the Pandora experiments will depend, then, on the characteristics of the model one considers appropriate for the data. For example: Is the effect cumulative (or non-cumulative)? Is the effect reversible (or irreversible)? Is the effect ephemeral, so that it is generally observable only under direct exposure?

One must take into account that the level of significance for the statistical tests is no longer the conventional one agreed upon. In a similar vein, as one tests more and more relatively independent variables within the same experiment, one is likely to obtain some results that seem to point to a statistically significant effect when, in reality, there is none. This is analogous to the traditional urn problem (containing, for example, 95 white and 5 red balls) in which the probability of picking a red ball in one draw is .05. Yet the probability of obtaining a red ball increases rapidly as I make many successive draws from the urn (replacement model).

The Pandora experiments reflect a multivariate model embedded in a time series. Although the multivariate time series model is not recommended for the present set of Pandora experiments, some thought might be given to its use when a critical and definitive set of experiments is decided upon.

SECTION 3: RECOMMENDATIONS

Basic issues

Work stoppage.—A relevant scale needs to be developed for the application of this concept. In Maj. McIlwain's analyses an animal who "stopped work and did not recommence within ten minutes of the end of the day" is said to have engaged in a work stoppage. As used in a nominal-scale situation, a work stoppage of 11 minutes is equivalent to one of several hours.

It is recommended that the distribution of the lengths of work stoppages be compiled so that the parameters of the distribution can be estimated. This should help in establishing a weighting procedure which would give more weight to longer than to shorter work stoppages. (Often in the case of time variables, a logarithmic transformation provides an adequate solution.)

Distribution shift.—In his presentation of a series of overlays (exposure data) compared to a distribution of baseline data, Dr. Sharp can be challenged concerning the criterion he used to select his time points. If this criterion had no theoretical jurisdiction, then distributions at other time points could have been taken just as well. What Dr. Sharp wanted to show is a global trend of the distributions without the necessity of processing all the data. Maj. McIlwain's "counter examples" taken from baseline data, though pictorially effective, may suffer from a similar selection bias.

Two analytic approaches seem to be feasible: (1) studying the change in the distribution as a whole over time; (2) utilizing several appropriate parameters of the distributions and studying their trend over time.

A first approach towards evaluating the change in the distribution as a whole over time might consider fractionating a time period into successive segments and then constructing an "average" distribution for each segment. The differences between distributions could be tested by a Kolmogorov-Smirnov type of statistic. This test procedure could be improved by the development of a Studentized range type of statistic for total distributions analogous * * *

In the second approach mentioned above, several basic parameters would be estimated for each distribution at each time point. The median and interquartile range would be appropriate. The distribution of these statistics over time would be available for analysis. Often enough, the hypothesis under test is relatively simple, as, for example, that the median (or interquartile range) increases over time. Curve fitting procedures could be used to test this or more complex functions. Orthogonal polynomials also could be utilized in testing for trend.

Either of the two approaches should be used. Of these, the trend analysis would seem more feasible, at least initially. The statistics (median and interquartile range) are available and the test procedure is characterized by ease of computation and interpretation.

Baseline problems

From an examination of the data and the graphs based on the data, it would be a questionable procedure to use many of the original baselines to evaluate the effects of the WRAIR signal. For example, the baseline developed in the "icebox" is not relevant to test for the effect of the signal in the anechoic chamber. Similarly, a baseline developed on a night "shift" cannot be used to test the effects of the WRAIR signal given during the day. Discouraged by the problem of an adequate baseline, Maj McIlwain has refrained from using baselines in his analyses of the exposure periods.

It is recommended that the initial period of orientation to any change in condition be considered as a source for baseline data. For example, after transfer from the "icebox" to the anechoic chamber, the animal is usually tested for a number of days without exposure to the WRAIR signal. A portion of this adaptation period, once the perturbations settle down, could be used as the base against which the effects of the signal should be evaluated. Despite the fewer number of points available, they should be used as baseline data for the particular comparison under consideration.

The issue pointed up in this discussion is that there is no general baseline for all combinations of test situations. Changes in test situations seem to produce more pronounced effects than exposure stimuli.

Division of exposure period

In the extant analyses, Maj. McIlwain has divided the exposure period into two equal segments. He has reasoned that if the effect is cumulative, the average value of the two segments should differ—the greater effect being observed in the second half of the exposure period. This analysis loses some valuable information. In the first place it gives no information about the comparison of the exposure period with the * * *

It is recommended that the exposure period be divided into more than two segments. In particular, since it is anticipated that the new baseline periods will not contain many points (confer discussion in the previous section), it would be advantageous to divide the exposure period into segments equal in size to the baseline period. The analyses, nonparametric analogues of analysis of variance, will have the following advantages: (1) a comparison with baseline data will be available, and (2) a trend would be more discernible and easily tested.

Typical considerations

Where hypothesis are specific and directional, one-tailed tests should be used. The possibility of an enhancement effect on some functions under exposure should not be discounted. Two-tailed tests in these situations would leave the issue open ended.

It is recommended that the personnel at WRAIR continue using nonparametric statistics wherever feasible. These are the statistics of choice since the distributions of many of the statistics used in behavioral measurements have no known parametric representation. Further, they have good efficiency and they are simply and easily calculated.

It is also recommended that these calculations be done by clerical personnel trained and supervised by Maj. McIlwain.

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It is urged that the operational definitions of relevant concepts (ex. pause), the specification of models, evaluation of effects of malfunction, rejection of inadequate data, and other such problems be discussed with the expertise available in-house at WRAIR. In particular, the services of Col. J. Brady should be secured, since he is an outstanding authority in behavioral matters, especially those pertaining to animal behavior.

STATE OF ALABAMA,
DEPARTMENT OF PUBLIC HEALTH,
Montgomery, Ala., June 15, 1977.

U.S. SENATE,
Committee on Commerce, Science, and Transportation,
U.S. Capitol, Washington, D.C.

GENTLEMEN: This letter is in response to your questions at the Conference of Radiation Control Program Directors as part of their testimony to be given on June 29, 1977.

(I) We have reviewed the program and conclude that there are several areas which need resolutions or are of concern to the consuming public. Some of these issues may not require legislation but either changes in regulations or an approporiate policy decision. These areas are as follows:

(*a*) *Radium and Other Non-Agreement Radioactive Materials.*—As the Committee is aware, all source material, special nuclear material, and byproduct materials are covered by the Atomic Energy Act of 1954, as amended. Regulatory control of these materials is currently vested in the U.S. Nuclear Regulatory Commission or by special agreement in Agreement States. There are other radioactive materials such as radium which are not presently regulated by any federal agency; however, most, if not all, of the Agreement States regulate these materials similarly to source, special nuclear, and byproduct materials. The fact that some twenty (20) states do not license the users of these non-agreement radioactive materials is a gap in their control. Each Agreement State can cite several instances where these non-agreement materials have been transferred to unlicensed users from Non-Agreement States. In some cases the equipment being transferred did not meet minimal standards or give any indication that it contained radioactive material. Currently the Bureau of Radiological Health of the BDA has a voluntary program to identify equipment containing these materials. While this is an improvement over the situation that existed several years ago, we still are having materials distributed to non-licensees, improperly labeled, or with other questionable manufacturing practices.

We suggest that the Atomic Energy Act be amended to include these products so that they are regulated like byproduct, source, and special nuclear material. As an alternative, you may wish to amend the Radiation Safety Act of 1968.

(b) *OSHA.*—The Occupational Safety and Health Administration has comprehensive regulations for all health results in the work environment. It has been our experience with OSHA that, in general, they do not have sufficient trained personnel to adequately protect the worker from ionizing radiation. Indeed, virtually every state is doing as many inspections for ionizing radiation alone as OSHA is doing for all other safety and health matters. In addition, most states have specifically trained personnel to do their ionizing radiation inspections. For example, in Alabama everyone inspecting radioactive materials has a Masters Degree. Those inspecting x-ray equipment are registered x-ray technologists and have an additional month and a half specialized radiation training plus three (3) months on the job training.

Presently OSHA's regulations do not totally recognize good health physics practices. Further, in attempting to work with OSHA, they are totally against any state applying to control a single standard such as ionizing radiation. They would accept an application to control a group of standards but not a single standard. In view of the limited resources of individuals trained in the regulation of ionizing radiation as well as the recognition that most states have an ongoing program in ionizing radiation, it would be well for the Committee to consider designating one agency for all regulatory items involving ionizing radiation: and such an agency should utilize to the greatest possible state capabilities similarly to the NRC Agreement State program. As a philosophical point, it would probably be wise to have a different Agency establishing radiation exposure standards.

(c) *Disaster Planning.*—This area of radiation operation has been subject to much public comment, most of which does not leave a true impression of the state of emergency planning. In general most communities can take the necessary protective actions on a timely basis from any hazardous threat normally encountered at peace time; however, that is not to say that improvements should not be made. It has been our experience that the local and state governments are prepared and will take the necessary actions of clear lines of communications and authority are established between the various concerned agencies.

Current federal efforts leave us with several basic impressions:

(1) All emergency planning must take the form of a quasi-military operation or is merely a modification of the nuclear warfare concept. We do not agree that a community will be totally unable to get advice and technical support during a radiation emergency as would occur during a nuclear war. This subtle but significant difference does not appear to be recognized by all federal agencies.

(2) There is a lack of definition of what accident criteria state and local plans should be capable of coping with. Expressed another way no one has told the state and local agencies what accidents have sufficient probability to the expenditure of public mones to design a radiation emergency plan. As an example, everyone knows that it is possible for a 747 to crash into a Super Bowl game with resulting large numbers of fatalities and injuries. We are unaware of anyone who thinks this probability is sufficient to develop a detailed disaster plan for this occurrence. The State of Alabama has since 1971 endeavored to obtain a statement from the federal agencies as to what accident conditions for which we should write a detailed radiation emergency plan. As of this date, such information has not been provided. We understand that it will be provided in the near future.

Because of this lack of accident definition, the State of Alabama and other states have recommended against participating in the NRC's concurrence program for radiation emergency plans for fixed facilities. It does not seem logical or even possible for one to write an emergency plan when the scope of the accident is different for each agency who reviews the plan; however, once there is a general agreement on the accident or accidents for which radiation emergency plans should be prepared, then we believe we can support the NRC's Concurrence Program.

Current guides prepared by the federal agencies are open to interpretation and indeed is contradictory. For example, current NRC regulations define an accident which is used to evaluate the suitability of a specific site; yet, when one takes their emergency planning document, "Guide and Checklist for Development and Evaluation of State and Local Government Radiological Emergency Response Plans in Support of Fixed Nuclear Facilities", NUREG 75/111, one finds under L3 that one is required to have maps showing the physical location of all public, private, and military hospitals or other emergency medical service facilities within the state considered capable of providing medical support for any off-site victims of a radiological incident. Further, these facilities are expected to have trained personnel to care for victims of radiological accidents. At first glance there seems to be no problem until you try to get a decision about the injuries these medical facilities will be treating. Are we to expect several hundred individuals suffering from the radiation syndrome, or are we talking about those people who may be involved in car accidents, etc., during an evacuation? If it is the former, then it is inconsistent with the NRC regulations for design based accidents (DBA), which would not require medical attention. If it is the latter, experience with evacuations shows that you will not need a map of all the medical facilities in the state, nor will you have seen unusual medical problems to require experts to be imported from all over the state. This is just one example of many in which federal agency guidance is inconsistent and confusing.

Regretfully, many federal people envision a situation in which the local community will be unable to get timely technical advice during a radiological emergency. Using this concept, many have proposed using nuclear warfare instrumentation to attempt to evaluate the radiological situation. Indeed they point with pride at how many millions of instruments have been distributed and how many have been trained in their use. Unfortunately they forget that much of the training occurred over ten (10) years ago and that it was oriented to strictly wartime conditions. Further, they conveniently forget that the instruments were designed for particulate fallout and not for gaseous releases which are the most likely releases from nuclear powerplants. One should be aware

that many police and fire departments have been given updated training with these instruments, and indeed our own Division has supplemented the training in some cases. I would not suggest that anyone should ignore the availablity of instrumentation that is available or the fact that some people are quite competent in its use. I would suggest to the federal government that rather than trying to have everybody utilize this equipment, a selective approach is desirable both for the choice of the operator of the instrument and the accidents it is used for.

I would specifically suggest that for nuclear powerplants, rather than attempting to train large number of local monitors to make the necessary conversion for civil defense instrument readings, we should insist that the nuclear power plants have adequate staffing and instrumentation to make the initial (four (4) hours) environmental assessments. Most people agree that the initial decisions for protective actions have to be made based on information supplied from the facility. Further, we believe we can get people from the facility to make the necessary environmental measurements quicker and more accurately than we could get a policeman or fireman into the area to get his readings, then make the necessary correction, and hope that he has not forgotten his training. In fact, quite possibly we will have our policemen and firefighting forces engaged in protective actions activities which may be much more important than trying to teach them to read an instrument once every six (6) months. Further, because of instrumentation limitations, the more likely accidents, i.e., those involving milk and food supplies, will not be detected and may lead to initial reports that there is no problem when in fact a significant health problem exists. With radiation accidents, as with accidents involving hazardous chemicals, you do not need a volume of data to determine when and where you need to take a protective action. If we did, many more people would have already been killed by the many chemical accidents we have had in our country.

(II) Alabama continues to support the following resolutions of the Conference:

Year:	*Resolution No.*
1971	1–5
1972	2
1973	1
1974	None
1975	1
1976	1–2
August 1976	1–2

(III) Most of the reasons for calling for more federalization of radiation safety control are given in Number I. We feel that basically one agency should be responsible for all radioactive materials. This would include the licensing of the users, equipment, and the environmental discharges. Further, we feel either the same agency or a different agency should be responsible for the use of ionizing radiation use in the machines. This regulatory control over machine produced radiation would probably be best applied at the manufacturer in the case of the federal government and the user in the case of the state government. In either case provisions should be made for a state to fully participate in the program as a compliance unit. To do otherwise would result in a needless waste of trained resources. This is one of our prime objections to the current OSHA policy.

Currently when we report problems involving machine-produced ionizing radiation, it appears to get swallowed up in the great federal bureaucracy. Perhaps months later, we receive a telegram informing us that at long last a compliance program has been initiated which may last for years. In most cases the state is left powerless to encourage prompter compliance or even know what the details of the compliance program are. We suggest that all of this secrecy is really not necessary and that the states can and should be kept aware of compliance activities regarding this equipment. In many cases states will assist in identifying units remaining in noncompliance, and in most cases have been the initiators of noncompliance activities involving *ionizing* radation.

One of the major dfficulties of any program is funding. The ability of a state to fund a radiological health program varies widely. We do not claim that all of the problems associated with state radiological health programs are funding related, however, a large number are. Generally those states which are Agreement States are better funded than Non-Agreement States. To encourage states to become Agreement States as well as to support those Agreement States cur-

rently in existence, we would suggest that legislation should be considered by Congress which would authorize and require a state to collect license fees for byproduct, source, and special nuclear licenses issued by that state as an agreement State. Such fees would be the same as those charged by the U.S. Nuclear Regulatory Commission and are considered a federal grant to the Agreement State for radiation health and safety purposes. Specifically the federal legislation should authorize the Agreement States to collect and use the fees. This would also equalize the economic disadvantage that currently exists for some operations in Non-Agreement States.

If you have any questions or wish to discuss these matters further, please contact me.

Sincerely,

AUBREY V. GODWIN,
Director, Division of Radiological Health.

COMMENTS SUBMITTED BY UNITED ACTION FOR ANIMALS, INC.

United Action for Animals, Inc. (UAA) submits these comments for inclusion in the record of the Committee's study of the biological effects of radiation. UAA is a membership organization committed to ending unneeded live animal experimentation because UAA believes that live animal experimentation is not a proper or effective way of determining health and safety standards for human beings.

As the testimony before this Committee has indicated, the public is interested in the biological effects of ionizing and non-ionizing radiation in the doses that occur in real life. Except in isolated instances, these are low-dose, chronic exposures. Yet radiation researchers continue to rely on the bombardment of animals, usually with high doses over a short term. The record of this Committee confirms that this method yields little if any information about the biological consequences of the "real life" problem.[1] The only certain product of most present radiation research is injury to thousands and thousands of animals who experience indescribable agony in the process.

In November 1975 Rep. George E. Brown, Jr., held week-long hearings on "The Costs and Effects of Chronic Exposure to Low-Level Pollutants in the Environment," including radiation. At that time, radiation research experts admitted that the cause of death in short-term high-dose radiation experiments "may not be the same" as in real-life low-dose chronic exposures. But only one witness urged the study of chronic doses relevant to the real world. Another witness expressed caution about trying to extrapolate from high to low dose, stating flatly that "we may never know the risk" from chronic low-dose exposure. But there was no consensus on the question of what is a reliable means for determining the radiation risk to humans in everyday situations.

It was the same story in hearings held a year later, in November 1976, by the House Committee on Interior and Insular Affairs, Chairman James A. Haley. There, two more witnesses testified that the biological effects of radiation at high doses are different from those resulting from reallife low-dose exposure levels. (Some scientists point out that high doses of anything overwhelm the body's metabolic pathways, rendering abnormal results.) Again, in these hearings, there was such scientific confusion about the biological effects of low-dose radiation that Dr. Roger Mattson of the Nuclear Regulatory Commission, asked, apparently in frustration: "How do we come to a consensus of scientific opinion of how to protect people from low levels of radiation?"

This, in spite of the fact that radiation research has been going on since the turn of the century.[2] Is it too much to suggest that continued use of live animals in experiments using old shop-worn techniques will not produce the breakthroughs so necessary to understanding the health problems of radiation?

Today's researchers, probably as a result of their education and training in animal research, downgrade the use of other research and testing methods, such as living cells, including human cells, growing and functioning in tissue culture.

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[1] See, e.g., testimony of Dr. Steven O. Schiff, June 16, p. 5; Dr. Erwin D. J. Bross, June 17, pp. 1–2.

[2] A review published in The Archives of Pathology (34:562) in 1942 on the biological effects of radiation on many species of animals cited references going back to 1903.

Yet in 1965, radiation experts, under sponsorship of the Atomic Energy Commission, published a book entitled "Mammalian Radiation Lethality," in which they described the use of tissue culture in radiation research as follows:

"Quantative cellular radiobiology was advanced markedly by the introduction of single cell tissue culture techniques which allowed characterization of the radiation response of populations of mammalian cells, as well as individual cells in that population. Introduction of the technique allowed the demonstration, contrary to previous thought, that mammalian cells in tissue culture are comparable in their radiosensitivity to cells irradiated in the live animal. The method is unique in that an inoculum of growing tissue culture cells can be plated out on petri dishes; and if the dilution is sufficiently great, the overwhelming majority of visible colonies that appear in a matter of days will have been derived from, and therefore represent, the original single cells. Thus, following irradiation, the fate of an individual cell and its progeny can be determined accurately by counts of grossly visible colonies." (Mammalian Radiation Lethality, Academic Press, 1965.)

Animal researchers of course know that quantitative (numerically measurable) tissue culture methods exist, but apparently prefer the trial and error of the live animal experiments which are familiar to them. For example, at Rep. Brown's hearings in 1975, a researcher from Oak Ridge (ERDA) first stated:

"If we talk about dose levels where the probability of an effect is one in a thousand or one in a hundred thousand we simply cannot obtain the information from animal experiments for technical, logistical and economic reasons. The very low dose end of the curve, then, cannot be established by empirical observations."

Then he testified:

"It is possible to maintain and grow animal and human tissue cells outside the body in much the manner that bacteria can be grown in dishes of culture media. We know that certain carcinogens (cancer causing agents) can alter the characteristics of these cells so that when they proliferate to form colonies of cells the altered or "transformed" cells form colonies with characteristics that are different from untransformed cells. Furthermore, in some cases at least, these untransformed cells will produce tumors when inoculated into suitable test animals.[3] It would be possible, though technically difficult and tedious, to try to determine if a very small dose of carcinogen caused transformation in one cell in a hundred thousand or perhaps one cell in a million. In theory, at least, dose-response curves could be obtained down to very low levels of effect. The difficulty is that at present we do not know whether this system bears any relationship to what happens in the intact animal. . . . Theories must be consistent with observations at high dose levels."

Thus, despite the inadequacy of the old methods, he is right back where he started: the administration of high doses to animals without a hope of finding any answers.

Although animal researchers profess not to know what the production of cancer in tissue culture "means in animals," the animal of concern in environmental cancer is man. Human cells, obtainable commercially or from hospital surgical services, can be kept viable and functioning in vitro as per the method described by the Oak Ridge researcher. Both radiation and environmental chemicals can be tested on these living cells. The level of radiation (or concentration of environmental chemicals, such as pesticides) that "transforms" normal human cells into cancerous ones can be determined through the use of these living cells. But it only confounds these results to inject the cancerous human cells into foreign hosts—animals (in the 90-year old method that is still being used). Results in tissue culture can be seen within days instead of years, as in the case of live animals. That the method may be "to technical" or tedious"—especially for those trained in animal research—elevates form over substance by giving priority to the experiments and experimenters rather than the knowledge that is being sought.

A Dutch scientist has forcefully summarized the advantages of in vitro research methods:

"At present, only a few investigators use in vitro methods to study X-ray induced transformation [i.e., of normal cells to cancer]. This is unfortunate, because cell culture systems can be used to investigate malignant transforma-

[3] [This is a 90-year old method. Editor's note].

tion directly at the cellular level with the exclusion of the interfering factors operating in whole animals.

"With these in vitro methods, it is possible to establish dose-response relationships for malignant transformations induced by single and split doses of X-rays and also for other types of radiation. This offers the possibility of obtaining better insight into the potentially carcinogenic effect of very low doses of radiation.

"Another important advantage of in vitro systems is that chemical and viral carcinogens can be introduced and investigated in the same system. This makes these systems suitable for the exploration of the fundamental mechanism of carcinogenesis." (Biology of Radiation Carcinogenesis, p. 301, Raven Press, N.Y., 1976.)

Additionally, there are numerous other modern research methods that are not being used. For example, scientists at Oak Ridge National Laboratory developed a mathematical model for the distribution and excretion of the radioactive element thorium (^{229}TH) and its decay product (^{224}Ra) and used the model to estimate the radiation doses to human organs and tissues from a single intake of thorium. Their model was based on data on humans and dogs reported in the experimental literature. Further, they were able to calculate radium retention in man over a 50-year period of time. (Bull. Math. Biol. 35:129, 1973). By contrast, institutions across the country are still gassing animals (mostly Beagle dogs) with radioactive materials such as plutonium to study the distribution of "hot particles" in the respiratory system.

Probably the most important advance of this century was recently reported. Researchers at private laboratories, under contract with the Environmental Protection Agency, developed a "mathematical prediction system for determining the toxicity of chemical compounds." Their method, based on "sophisticated statistical techniques," permits the predictions of toxicity of untested chemical compounds, now routinely determined in the so-called rat LD50 test (the lethal dose from which 50 percent of the animals must die). Their mathematical prediction system "could also be applied to . . . long-term tests, carcinogenicity, etc. in other species and for other routes of administration." [4]

With proper motivation and incentive, similar or related methods could—and should—be extended to radiation research; there is more than 70 years' data in the published literature to start with.

But only Congress can lead the way to the development of modern, sophisticated research methods in radiation research. We urge that the Committee recommend and prepare legislation to accomplish the following: the elimination of experiments that use old, well worn techniques or that duplicate and overlap other well documented research; the elimination of the use of laboratory animals to produce research results of dubious value and application to the study of human response to radiation of all sorts; and the promotion of research alternatives and the advancement of modern, sophisticated research methods through the use of substantial incentive awards to the research community.

Respectfully submitted,

ELEANOR SEILING, *President.*

APPENDIX TO COMMENTS OF UNITED ACTION FOR ANIMALS, INC.

EXAMPLES OF MODERN RESEARCH METHODS WHICH DO NOT REQUIRE THE USE OF LIVE ANIMALS

1. Gamma-Irradiated Heart Cells in Vitro
2. In Vitro Interactions (Synergism) of Chemical Carcinogenis and Radiation.
3. Radiation Damage and Repair.
4. Radiation Research with Nuclear "Phantoms".
5. Radiation Inhalation.
6. Biological Effects of Radioactive Material on Cultured Cells.

Gamma-irradiated heart cells in vitro.—Scientists at Harvard University used rat heart cells in vitro (outside the body) to study the effects of acute doses of gamma radiation in post-mitotic (after dividing) mamalian cell function. Hearts

[4] A Toxicity Prediction System, developed by Genesee Computer Center, Inc., Rochester, N.Y. and Franklin Inst. Research Labs. Philadelphia, Pa., under partial sponsorship of the EPA, April 1977. Other techniques directly applicable to research in the radiation field are discussed in the appendix.

from 28 new-born rats were excised and kept alive and beating in suitable nutrient solutions. Seven days after "seeding" the culture cells, the scientists irradiated them at doses ranging from 1,000 to 100,000 rads with a cobalt–60 (Co) irradiator. Following irradiation, the heart cells were incubated at 98.6 F for the 30-day period of the experiment. The beating rate of the heart cells in each of the cultures was measured daily with a "heated state" inverted light microscope. Two or three days after irradiation, beating in those cultures receiving 50,000 or 100,000 rads became arrhythmic (i.e., stopped beating for a few seconds, then beat quickly for the next few). Doses of 50,000 and 100,000 rads completed inhibited the culture from beating within 8 to 23 days, "indicating the functional limit of radiation these cells can tolerate." Doses of 10,000 rad or less did not affect the beating rate or rhythmicity, "although the overall cell density appeared sparser than in control cultures." Conclusions: The beating of heart cells in vitro allowed the physiological expression of radiation damage to be numerically measured. Loss of rhythmicity after gamma-radiation occurred much earlier than the loss of the ability of the cells to contract spontaneously. An advantage of the in vitro method of irradiating cultured heart cells is that it "eliminates the effects of vascular (blood vessel) damage," so that direct radiation injury to the cells themselves can be studied. The scientists said, "Maintenance of these cells for periods longer than 30 days may aid in identifying functional effects after irradiation with lower doses." (Int. J. Radiat. Biol., 28(1) : 99–102, 1975)

In vitro interactions (Synergism) of chemical carcinogenis and radiation.—Cancer toxicologists usually study the interaction of cancer-causing chemicals and radiation by dosing live animals with the chemicals and then irradiating them or vice versa. In contrast, a scientist at the National Cancer Institute used hamster cells to study this phenomenon. In addition to being "reproducible" and numerically measurable, the in vitro procedure for transforming normal mammalian cells into cancer cells "made possible the separation of events related to the process leading to transformation from secondary events that interfere with the early recognition of transformation." Exposure of the hamster cell strains to cancer-causing chemicals resulted in a dose-response relation which could be described mathematically by a "poisson distribution indicating that the transformation . . . is inductive [directly produced by the chemicals]." Pretreatment of the cells with ionizing radiation or "alkylating" chemicals (chemotherapeutic cancer drugs which in high doses are toxic to normal tissue) enhanced the frequency of transformation on a cell or colony basis "ordinarily obtained with known chemical carcinogens." Pretreatment with non-ionizing ultraviolet radiation did not have a similar effect. The two types of radiation and the alkylating agents reduced the efficiency of the cells to clone.

Radiation damage and repair.—Scientists at the University of Texas in Dallas used hamster lung cells in culture to study repair of radiation damage. Appropriate numbers of cells were plated onto petri dishes to yield about 200 colonies "in control and experimental dishes." The cells were than irradiated with x-rays at a dose selected to cause "potentially lethal" damage (i.e., damage which may be repairable to a non-lethal level if suitable conditions are provided). Immediately after exposure to a single radiation dose, cells were overlaid with either "FS–15" (complete growth medium) or "Earle's balanced salt solution." After a period of time in the balanced salt solution, buffer (the chemical balancing the solution) was discarded and cells were provided with FS–15 for colony formation. After 10 to 12 days of growth, cell colonies were counted. "Survival rapidly increased to a maximum after 1 hour in the balanced salt solution." This initial survival was followed by a decrease and then a second rise. "The initial increase observed . . . suggests that in the presence of buffer, mammalian cells repair potentially lethal damage." The subsequent fall in survival suggested that, during the repair process, an unstable cellular state occurred which prevented cell division in complete growth medium. Conclusion: A cellular environment which inhibits cell division is necessary if cells are to repair themselves following "potentially lethal" radiation damage. Further experiments on the cultured cells showed that their capacity for repair was also dependent on cell age. (Science, Vol. 165, Aug. 1, 1969)

Radiation Research with Nuclear "Phantoms".—Alderson Research Laboratories developed a life-like radiological test subject called "REMAB" (Railroad Equivalent Manikin-Absorption). These dummies or "phantoms" are equivalent to an average man in size and contours; they are filled with "a solution that has the same radiation-interaction properties as human soft tissues." REMAB was designed for radiation research and can be used for "depth-dose" measurements and "nuclear-accident re-creation." The dummies can be loaded with radioactive materials to determine the exposure effects to adjacent areas. ("Alderson Nuclear Phantoms," Alderson Research Laboratories, Inc., Stamford, Conn.)

Radiation Inhalation.—Over the past 30 years at institutions all across the country, animals have been forced to inhale radioactive materials. In contrast, scientists at Pacific Northwest Laboratories, under contract to the U.S. Atomic Energy Commission, designed a computer program that "permits rapid and consistent estimates of the effective radiation dose to the human respiratory tract and other organs resulting from the inhalation of radioactive aerosols." They also constructed mathematical models to describe the atomospheric dispersion of radioactive material "for the purpose of evaluating doses resulting from either accidental or chronic atmospheric releases of radionuclides." In the model, the respiratory tract was divided into three regions: the nasopharyngeal (nosepharynx); the tracheobronchial (air passages to lungs; and the pulmonary (lung). The scientists employed "complex mathematics" to design the computer program input which consisted of several variables: duration of inhalation exposure, ventilation rate, time interval within which the dose is delivered, organs of interest, quantity of radionuclide inhaled, and "solubility class" and particle size. Output from the "CDC CYBER 74" computer consisted of the effective radiation dose to "any of 18 organs and tissues" in the human body. The model was extended "to include calculating organ doses resulting from chronic inhalation," as well as acute inhalation of radioactive particles. The scientists said that most of the basic data required to complete the calculations are available at "data libraries." The data in the libraries are set up such that "additional data or changes to existing data may be made with little difficulty." (BNWL–B–389 UC41, Dec., 1974).

Biological Effects of Radioactive Material on Cultured Cells.—Researchers at Rutgers University Agricultural Experiment Station are using cultured mammalian cells to test the damaging effects of tritium and other radioactive materials. They are conducting a comparative study of the chemical effects of radiation and of high energy, heavy ions (electrically charged atoms) and gamma rays on the isolated cells. Methods will be used to 'accurately" relate deposition and absorption of radioactive energy to biological damage. Though the work is still in progress, the scientists have already found "significant chemical differences between the effects of tritium beta rays and gamma rays." More interpretation is needed, but results so far are "potentially important since the present basis for tritium maximum permissible concentration assumes little difference between these radiations." (SSIE, Notice of Research Project, No. GY–60353–4, July 1977)

UNIVERSITY OF OREGON HEALTH SCIENCES CENTER,
Portland, Oreg., June 13, 1977.

Hon. WARREN G. MAGNUSON,
Chairman, The Senate Commerce Committee,
Russell Old Senate Office Building,
Washington, D.C.

DEAR SENATOR MAGNUSON: Your hearings on the adequacy of United States safety standards for microwaves and radiowaves are timely indeed. In recent years evidence has steadily mounted concerning the likelihood of low power level microwave cumulative dose effect hazards. It has also become apparent that certain federal agencies and private contractors have vested interests in obstructing the public scrutiny and adequate research support for accurate determination of the risks of these apparent hazards. Most people do not realize that certain types of non-ionizing radiation (e.g. microwaves and radiowaves) can have similar effects to the readily recognized dangers of ionizing radiation (X rays and gamma

rays), with the principal difference being the much lower incidence of tissue damage due to the much lower energy content of the non-ionizing radiation. However, the lifetime accumulation of non-ionizing radiation dose, the damage to the posterior capsule of the ocular lens, and the suspicion of increased cancer risk are all characteristics in which the effects of non-ionizing radiation resemble those of ionizing radiation. The continuing rapid increase in use of radiowaves for military and civilian communication and sensor systems as well as the growing popularity of microwave cooking in the home, means that it is imperative to develop accurate and adequate health risk estimates from sources which cannot be accused of vested interest in health hazard information suppression. This will require epidemiologic and experimental research of a whole array of circumstances over considerable periods of time because of the intricate influence of environmental variables and the latent ueriod s between exposures and potential chronic disease manifestations.

A volunteer member of our faculty, Mr. William Bise, has been instrumental in bringing to our attention the fact that foreign scientists have gathered a great deal of evidence suggesting the existence of significant hazard from low level microwave radiation. He is an experienced radio broadcaster and has formed a non-profit corporation (Pacific Northwest Center for the Study of Non-Ionizing Radiation) for the purpose of promotion and support of investigation of these hazards. He is writing to you under separate cover. We endorse your efforts to look into this matter and anticipate the eventual facilitation of of radiowave health hazard investigation as a result.

Sincerely,

WM. E. MORTON, M.D., DrPH, *Professor*

PACIFIC NORTHWEST CENTER FOR THE STUDY OF NON-IONIZING RADIATION,
Portland, Oreg., June 6, 1977.

Hon. WARREN G. MAGNUSON,
U.S. Senate, Russell Senate Office Building, Washington, D.C.

DEAR SENATOR MAGNUSON: Enclosed is material that I would like to submit in lieu of verbal testimony in the upcoming senate subcommittee hearings on microwaves. In a telephone conversation with Sharon Nelson on Wednesday June 1, 1977, I pointed out that my limited budget does not allow me to appear in person. The material describes a pilot study of central nervous system radiofrequency (RF) effects which I conducted on ten human volunteers at a power level on the order of 67 microvolts per meter intensity, previous to founding a non-profit research corporation. As you know, Public Law 90–602 set the so called safe level for human exposure to this energy at about 195 volts per meter. The pilot study findings suggest that brain waves (and possibly behavior) are affected at specific frequencies at substantially lower power levels of RF than people can be exposed to in their daily lives. Whereas further research is necessary to verify or deny the pilot study findings, it is my opinion that a meaningful risk factor already exists for the population at large. The technical paper entitled Radiofrequency Induced Interference Responses in the Human Nervous System has been submitted for consideration in the upcoming URSI International Symposium on Biological Effects of Electromagnetic Waves to be held at Airlie, Virginia on October 30–November 4, 1977. Also enclosed is a popular technical article that I authored which describes the pilot study which appeared (without references) in the January 1977 issue of Communications/Engineering Digest.

I have worked out an experimental design using the ambient RF as a signal source for a follow up study. The design conforms to all H.E.W. guidelines for protection of human subjects. If funds can be granted for this research, many questions concerning RF biological hazards can be answered.

Electronics engineers are dedicated to designing, building and maintaining better and ever more sophisticated products for radiating and detecting all kinds of electromagnetic waves. With the exception of careful respect for high voltages, currents and power, a good engineer will insist that low power microwaves or radiowaves cannot possibly affect human behavior and/or health. As an engineer

who held that view for fourteen years, I can attest to this fact. Except for a rather dramatic incident in 1970 involving my wife, who was nearby while I was troubleshooting some gear with a low power microwave generator, I might not have done a pilot study of RF effects; and I might still have remained unconvinced that low level microwaves/radiowaves are hazardous at specific frequencies.

Since I became a clinical instructor in environmental medicine at the University of Oregon Health Sciences Center, it has become apparent to me that most physicians are unaware that low-level radiowave biological effects can mimic many symptoms of various defined diseases. I have found the entire subject of radiowave hazards very unpopular to science, business, industry and the military; perhaps because recognizing these hazards would cause so many necessary changes in these various areas.

An international radiowave conflict has been going on for some time and was intensified dramatically in July 1976 with the addition of the Soviet 10 Hz pulses which may produce serious biological effects on people regardless of the real intent of these intense pulses of radiant energy. It is not necessary for nations to declare war of this nature since it is silent and invisible to our normal senses.

Using the legal power limit of 10 milliwatt per square centimeter, a malicious person could easily build (or buy surplus) equipment that could physically and mentally damage people, over a period of time, without the victim ever knowing what was happening to him. The members of the executive, legislative and judicial branches of government may even be more vulnerable to this kind of personal harassment than the ordinary citizen.

It is conceivable that special interest groups have already resorted to tactics of this nature. I was called as a consultant in April of 1976 in a case in Seattle, Washington where the microwaves and lasers which my instruments detected possibly were the result of personal harassment. One motive could be the desire to buy property at bargain prices from people who are anxious to sell in order to leave the neighborhood because they no longer feel well in their home or business. Because I was working alone in Seattle, my findings remain inconclusive. Military radar scatter also seemed to be one of the sources of radiation in the two different areas which I checked.

To paraphrase an engineer formerly with the North American Air Defense Command, "You'd never really know the cause of death from this stuff . . . . you could hit someone, coagulate their blood and no one would ever know what happened."

I hope that there is something that your committee can elicit from the upcoming hearings to at least inform, if not protect, the people of the United States from electromagnetic hazards. The specialization of science unfortunately limits the scientists' ability to bridge the interdisciplinary gap necessary to realize the urgent need for a joint effort of investigation into low power level microwave hazards.

There is another serious matter which involved a breach of security in Seattle concerning the President's "Doomsday" airplane. If at any time you might wish to talk to me, please let me know.

Sincerely,

WILLIAM BISE.

Enclosures.

RADIOFREQUENCY INDUCED INTERFERENCE RESPONSES IN THE HUMAN NERVOUS SYSTEM

(By William Bise)

Summary: Interest in lowpower athermal RF effects on biological systems has grown in recent years among American researchers and the foreign literature on the subject is prolific. Against this background a pilot study was conducted on five men and five women volunteers. Participants in a group of eight experiments completed from July 1975 through June 1976 were, of necessity, highly preselected. They ranged in age from 18 to 48 years. Three had been occupationally exposed to RF energy; the other seven had not and all were in apparent

good health. The RF ranges covered from .1 to 960 Mhz CW and 8.5 to 9.6 Ghz pulse modulated. Power levels were varied from 10^{-16} wt/cm^2 to 10^{-13} wt/cm^2. Three variable frequency low power output test alignment generators served as signal sources. Subjects were seated and oriented so that they could not see the equipment or the operator. During data gathering they did not know to what frequencies the sources were tuned nor did they know when the signal source was off or on. A 1 meter free space impedance coupled antenna was placed parallel to and 1 meter from the upper torso and head during CW tests. The open wave guide output of the pulse modulated source was oriented 1 meter from and perpendicular to the sternum. Any changes in the EEG traces were photographed from a cathode ray tube display and/or note was taken of the frequency which produced them. Previous to irradiation, the normal EEG tracings of the subjects were noted or photographed. Experimental time for each volunteer was typically 50 minutes. Tests were done at different times of the day at various locations, both in and out of a Faraday room. Signal source accuracy and power output levels, at the subject to antenna distance, were measured with two spectrum analyzers.

The EEG instrumentation used gave sequential recording of a single trace from an electrode pair and was specially designed for use in RF fields. Artifacts were identified and controlled out of the tests. Pushbutton switchable selectivity allowed sampling from the three different cranial areas without changing electrode placement. Plastic covered skin surface type electrodes were used.

Subjects' EEG traces displayed desynchronized alpha waves of 15 to 25 percent higher than normal amplitude and slow waves appeared at certain radiofrequencies. Conversely, diminution and desynchronization of alpha wave amplitude on the order of 20 to 50 percent occurred at other radiofrequencies and 2 to 6 Hz slow waves appeared. These two anomalous patterns were found in both men and women volunteers. Mental attitudes appeared to change during the tests. CW frequencies at a power density of about 10–15 wt/cm.^{2} which produced EEG changes in males, were found between 130 and 780 Mhz. Female volunteers' EEG alterations occurred between 350 and 960 Mhz. Pulse modulation tests on two males, at a power density of about 10-12 wt/cm^2 showed EEG changes around 9.1 and 9.15 Ghz. Brain waves changed almost immediately upon tuning a generator to a frequency which produced them and then almost immediately reverted to their normal patterns when the generator frequency was altered or turned off. Various lead configurations and antenna orientations were tried during the CW tests and demonstrated that active sampling from the right frontal parietal to the right occipital, with antenna presented 1 meter from left hemisphere, yielded repeatable results. This suggests that the right hemisphere is the more responsive one to RF energy. Experiments performed near noon and midnight showed the most significant EEG pattern changes thus a circadian biorhythm dependence may be involved.

Notably, the CW RF energy of about 67 microvolts per meter which was seen to alter brain waves is characteristic of the average EEG voltage level which exists at the surface of the head. The EEG changes seen in most of the participants were produced by this RF energy level at frequencies between 130 and 960 Mhz. Since most researchers agree that the relaxation frequency of protein bound water must lie between 100 and 1,000 Mhz, it is possible that resonance absorptions occurred. The shape of the human head (spherical antenna bandwidth), the piezoelectric characteristic of bone and the possible up and down frequency shifts of radio energy entering and leaving protein bound water in near surface tissue are considerations in this phenomenon. Altered EEG patterns resulting from electrical stimuli should be expected to reflect corresponding behavior changes. It is known that both the thalamus and hypothalamus are especially sensitive in this respect. The 6 Hz waves which appeared in some subjects' EEG patterns are similar to those waves associated with annoyance seen in earlier EEG research. Brain wave changes were triggered in alert volunteers by external energies which they could not perceive. At very low RF field densities the body's neurointegrative systems evidently do not function normally or somehow may be prevented from normal functioning. RF induced stimulation of the midbrain structures may be a fundamental factor. The full impact on biological systems of long exposure to low intensity RF energy is not known, but the likelihood of cumulative dose effects means that small doses are important.

MEDICAL COLLEGE OF VIRGINIA,
VIRGINIA COMMONWEALTH UNIVERSITY,
Richmond, Va., July 25, 1977.

HON. WENDELL H. FORD,
HON. ADLAI E. STEVENSON,
Committee on Commerce, Science, and Transportation,
U.S. Senate,
Washington, D.C.

DEAR SENATORS FORD AND STEVENSON: I am writing to you in reference to the U.S. Senate Committee on Commerce, Science, and Transportation hearings concerning the current status of health protection problems from radiation exposure. I have been involved in research related to the biological effects of ionizing and nonionizing radiation for the past twenty years and much of my research has been directed toward the establishment of safe exposure levels for humans.

My particular interest is the biological effects of microwave radiation, a topic that as you know is now much in the public view and presents a number of controversial aspects. In spite of the fact that recent research in this area has revealed a number of physiological and psychological alterations in various species of experimental animals, research support for such studies has been rather drastically reduced in the past few months. I fully realize that biologic effects of microwave exposure are but one of the many problems related to radiation exposure, but it is of great concern to me personally as a scientist and as a citizen, that sustained research support does not appear to be forthcoming for the solution of problems such as this. I believe we need answers now for the questions that will inevitably arise in the near future due to the ever increasing levels of occupational and nonoccupational exposure to microwave and radiofreqency radiation. Such answers can only be obtained by sustained research efforts which are difficult, if not impossible, in the face of drastic fluctuations in research support in areas such as this.

My views are based upon the results of research being conducted in my laboratory as well as a rather extensive review I have recently completed of the literature in this area. The results of this review are detailed in the enclosed article entitled "Biological Effects of Microwave and Radiofrequency Radiation." In view of the extent of this review article I would like to summarize a few of what I feel are the most salient points regarding human exposure to microwave and radiofrequency radiation:

(*a*) a significant number of studies conducted during the past six years in this country and the Soviet-bloc nations have revealed various physiological and psychological alterations in living systems at levels in the region of 10 mW/cm^2, the human occupational exposure standard in effect in the United States,

(*b*) although microwave-induced heating appears to play a role in the induction of such effects, there are qualitative and quantitative differences in the radiation effects compared to nonradiation heating,

(*c*) the reported effects of exposure of normal, healthy animals to low-intensity (ca 10 mW/cm^2) fields by and large appear to be reversible in nature, but chronic exposure (i.e. periods of weeks or months) results in immunological alterations suggesting that exposure of animals compromised by coincident exposure to, for example, pathogenic organisms or drugs may result in significantly enhanced effects. Multiple-stress microwave interactions could present a wide spectrum of problems in human populations due to endemic viral and bacterial pathogens and to the prevalence of drug usage. Although studies to ascertain the significance of such interactions have been proposed, such investigations are currently suffering from a lack of funding.

((*d*) In spite of the evidence indicating that low-intensity microwave and modulated radiofrequency and low frequency electromagnetic fields alter biological systems, the presently available data is not sufficient to enable basic interaction mechanisms to be determined.

I believe that the biological effects of electromagnetic fields pose significant problems that must be solved if we are to preserve the quality of our environment. Research in this area is for the first time beginning to define the true nature of the problem and it is also providing the data needed for the future development of exposure guidelines that will prevent deleterious effects. I urge you, therefore, to consider the necessity of supporting efforts for the funding of sustained research in this area.

I thank you for your interest in this matter and will willingly provide any additional information at my disposal to aid in your deliberations.

Very truly yours,

STEPHEN F. CLEARY, Ph.D.,
Professor of Biophysics,
Medical College of Virginia.

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