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TO "UNCLASSIFIED" 24 SEP 1979  
Per: Director, DARPA / T10

Dear Steve:

REVIEW OF PROJECT PANDORA EXPERIMENTS

Following our recent discussions I have gone through the data on the Pandora Experiment as they have been presented by Major McIlwain. Maj. McIlwain has done a superb job on reassessing the material of the last few years and presenting it in an easily understood form. During the course of this review I spent approximately 8 hours (October 22, 1969) looking at the material and in related discussions. In brief, I am forced to conclude that the data do not present any evidence of a behavioral change due to the presence of the special signal within the limits of any reasonable scientific criteria. There is evidence of behavioral change in some cases but this change could be attributed to a variety of causes or systematic measurement errors all well within the limits of experimental methodology. Evidence of other effects such as EEG, histology, and chromosomal analyses have not accumulated with either adequate detail or control to tell whether effects due to radiation are present.

One should not infer from these statements that there is no value to the work done; there is unquestionably considerable value in development of protocols and facilities and the possibility of extending this to a variety of useful work which I will discuss later.

The primary experiments have been to look for the effect of the special signal on specially trained monkeys at intensity levels comparable

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to the special site environment. As I recall the data, there have been four operant conditioned animals which have been exposed in total 7 times to the special signal, 2 times to a square wave, 1 time to a triangular wave, and 1 time to cw. The intensity generally used has been  $4.6 \text{ mw/cm}^2$  which, I might point out, is probably in excess of the special site environment. Experiments are not run at higher intensity since this is the maximum possible for the equipment using two carriers. At least one animal was run at very low intensities corresponding perhaps closer to the action site ranging from  $8 \text{ mw/cm}^2$  to  $1 \text{ mw/cm}^2$  but I recall nothing particularly significant for this run compared to the others. The basic parameters measured were the PR (prompt response), DRL (differential reinforcement of low rate) and the latency time to go into DRL.

I will not attempt here to detail the various particular runs generally of 20 to 30 or more days in duration but rather give my general impressions. There were certainly individual days where differences were observed which were statistically significant in terms of the individual day's experiment. These behavioral changes, however, were well within the limits of causes other than radiation such as change of the animal from one room to another, day/night variations, or perturbations caused by malfunction of equipment. In particular there seem to be a considerable number of malfunctions in the pellet-feeding gear. In the case of one animal who was exposed at two different times approximately two years apart, it was interesting to note that the variation in his behavior during the 2nd exposure where he had the opportunity for long continued training was much smoother than the first period. It is also important to note that while a large number of performance degradations were noted most of these occurred either in the form of very small variations from a normal count (i.e., number of food pellets obtained) or occurred the day following a significant equipment malfunction. There may have been one case (animal number 673) where there was a performance time-out of some significance.

In general one would consider the unexposed animal or a period of nonexposure to be the control; I would also say that in view of the problem associated with the special signal an equally significant control would be the cw signal. However, as mentioned above there was only one case of this sort of run and this quite a few years ago. It was difficult for me to see how one can have a viable protocol for any stimulus when the stimulus intensity has not been brought to a level which creates a positive effect and this then compared to the required operational level.

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For example, such a level might be in excess of  $10 \text{ mw/cm}^2$ . The equipment used in a combined single-mode manner could certainly produce approximately this level of power.

Another type of experiment that could be classified as behavioral was the reaction time studies. Four animals were used here, two with food reward and two using shock avoidance. The basic concept here is for the animal itself to adjust his reaction time to a comfortable value and to look at changes in this as a result of various stimuli or environmental conditions. Of the four tests run three showed no effect, one did show an effect but this effect could be either eliminated or emphasized by a change of the timing program. I believe, in general, these reaction time studies have been used in the behavioral field primarily for relatively short-term changes. Certainly there was some indication of statistically valid variations over a period of months but this could not be correlated to on/off times of the signal. It might be noted also that negative results were obtained for a tone substitution versus the microwaves for shock avoidance.

In summary, you could say that there are some changes in the distribution of the various parameters at various times but there were few or none uniquely correlated with a special signal. There were certainly no trends observed, any statistically valid changes were single day, and there was certainly no evidence of anything that could be described as a catastrophic effect.

The effect of low frequency modulation on the EEG has been reported a number of times by this project. Implanted electrodes are placed into various brain regions of the monkey and the resulting EEG tapes were analyzed off-line by Dr. Adey's laboratory in California. The time delays intrinsically involved in this process may be significant in explaining some of the experimental procedures followed or not followed. If the animals are irradiated by sine wave modulated at various low frequencies in the alpha region the autocorrelated power spectrum analysis shows reinforcement of the modulation frequency in various portions of the brain. At this date there is no convincing evidence that this effect is not an electrical artifact of the procedure. There are several variations of protocol which could determine this using an on-line system. I believe a fast-Fourier transform analyser is on order for purposes of going on-line. Experiments were run with the animals' head shielded, under anesthesia, killed during the experiment, and even with a perfused brain. However, none of these were satisfactory for positive elimination

of the possibility of an artifact. In fact, variations of head position versus autocorrelation spectrum did tend to lend some evidence for an antenna action for the probes.

Additional programs are underway for chromosomal analysis using karyotyping of cultured lymphocytes and for testicular and brain histology but no substantive results have been reported yet other than a few isolated observations that cannot be considered significant until placed in the context of systematic data.

As stated earlier, the value of the behavioral protocols, procedures, and equipment should not be summarily dismissed. In addition not only does the present working facility represent a substantial capital equipment investment but also the new facility nearing completion is a magnificent laboratory indeed with three additional exposure chambers and all the various ancillary histological, biochemical, and conditioning laboratories that could be required, at least for studies in the microwave region. The issue of determining whether or not there is a biological effect at relatively low levels below the 50 to 100 mw/cm<sup>2</sup> levels which constitute directly observable hazards is not limited to the question of the special signal. Failure to have absolute scientific evidence of the presence or absence of an effect and its threshold region can leave the U.S. vulnerable to a campaign against the use of surveillance radars foreign and domestic, military and civilian, as well as highpowered communications equipment. A possible public and consequent Congressional reaction on scare material, particularly if encouraged by inimical forces, could result in a catastrophic impediment to the use of various equipments essential for the national security.

It would appear that the problem should be viewed on three security levels. First, the compartmented signal and data derived from it should be put aside under adequate security protection for the present; if there is to be any understanding of this, the present program is probably wrong to start with.

One should start with an examination of various basic wave forms and then the combinations resulting in possible intermodulations and demodulations by biological tissue. A program that might look at possible behavioral implications from the point of view of a weapon or interrogation device could be handled on a SECRET level. The more pressing issue is the safety problem and that could be handled on a CONFIDENTIAL or ORO level during acquisition of data with eventual declassification as the goal.

As an example of a protocol one might consider starting at a fairly high level, 10 or 20 mw/cm<sup>2</sup> then looking at 5 mw/cm<sup>2</sup> and 1 mw/cm<sup>2</sup> for cw, and 50%, 1%, and 0.1% pulsed duty cycles with equivalent average power. I do not mean to imply by this that either I or ARPA should design the experimenter's protocol, but rather that one should start with a level high enough to get some observable effect and then continue to look at real world levels and modulations. The new facility is certainly adequate to handle the microwave problem, still leaving currently urgent problems of ULF and HF/VHF.

The important objective now should be to determine at what level, modulation, and exposure regime (chronic, intermittent, etc.), a biological effect as distinguished from a hazard exists. These two terms should not be confused. If an effect is observed at that time an adjudication of various operational situations should be made to determine what hazard, if any, exists.

Sincerely,



Samuel Koslov  
Research Council

SK/bt

cc: Augenstein, McIlwain, Tamarkin

