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Comments on Bauer's "Loch Ness Monster" Volume 1, Number 1, 1987

I read with interest Professor Bauer's paper on Loch Ness, as well as his interesting book, *The Enigma of Loch Ness,* which expands on the theme in his paper. By coincidence, I attended a cryptozoology conference this summer where Dr. Bauer also talked about the reluctance of editors to give space to the topic. My response to Bauer's ideas is that it is true that editors, and the public, have become a bit jaundiced about the famed LNM, and the reason is that no solid results have surfaced that give us anything more to go on than did St. Columba, over 1400 years ago. The creature, whatever it may be, still remains a mystery, and the recent sonar sweep of Loch Ness by 20 boats moving side-by-side only produced a single significant reading—after three days of sweeping the loch. The media responded by calling the effort a failure.

Yet, according to the Inverness *Press and Journal,* a retired garbageman (Mr. Alex Crosbie, age 65) from Edinburgh managed to take some eight photos of the creature in late July, using a camera borrowed from another U.S. expedition that was there prior to the Lowrance effort. Supposedly, this creature is 48 feet long, twice as long as several boats in the photos, and has a head resembling a feline of some unknown sort. If true, this photo was taken just one mile from the spot where the sonar reading was achieved.

The *JSE* is interested in "phenomena outside the current paradigm of one or more of the sciences"; in this case, biology is at question. Perhaps biologists, and newspaper editors, are losing interest in the LNM because it is outside the standard paradigm of biology, that is, it may not be biological per se. Perhaps the answer lies in the writings of F. W. Holiday, Jacques Vallé, John Keel, Walter Andrus, and others, who have suggested a non-biological explanation for the LNM. Could it be that this creature is a form of energy, not matter? If this is true, then perhaps astro-physicists, not biologists, may be the proper scientists to study this phenomenon. We have quarks, Black Holes, the speed of light, alternative worlds and parallel dimensions as theories. Why not add the Loch Ness anomaly to the list? If this is done, editors and scientists will show a renewed interest, for the old paradigm has failed.

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The Strange Properties of Psychokinesis*

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Abstract—This paper discusses evidence for a psychokinetic effect acting on chance events. Emphasis is laid on psychokinetic action on pre-recorded random processes and its interpretation in terms of two general hypotheses, the weak violation hypothesis, and the equivalence hypothesis. These hypotheses imply that psychokinesis can act on the outcome of indeterministic quantum events only, and that, basically, all such events are affected to the same degree.

Introduction

Most of this paper will emphasize the interesting, challenging aspects of parapsychology, pointing out how a study of psychic effects may lead to a more fundamental understanding of nature, and perhaps of quantum theory. Indeed, parapsychology appears as a dynamically progressing field of research. Good statistical methods for observing the effects in the laboratory have been developed. Modern electronics has been utilized to gather data efficiently and reliably, and even though there is not yet a final theory, there are theoretical models to provide a lively interaction between ongoing experiments and theoretical ideas.

And yet, there are a number of disquieting questions, ranging from very practical matters to basic conceptual issues. Most perplexing for the experimenter is the elusive nature of the phenomena. Although the results are too persistent and statistically too significant to be seen as mere flukes of chance, it is often difficult to replicate previous results on demand (Edge, Morris, Palmer, & Rush, 1986).

One may argue that psychic performance, like creativity, depends on subtle psychological factors that are difficult to identify and to control; and one may expect progress from ongoing experiments using motivational techniques, meditation methods or electronic feedback devices to help the subject's mental effort.

On the other hand, the replicability problem may be deeply rooted in the exotic nature of the psychic mechanism. The experiments to be discussed will use the psychokinetic effect (PK) as an example to emphasize these conceptually perplexing features.

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* This is an expanded version of a paper presented at the annual meeting of the Society for Scientific Exploration in 1985.

The psychokinesis experiments to be discussed developed out of Rhine's experiments with dice (Rhine, 1970; Rhine & Rhine, 1943). Rhine reported that some people could mentally influence the outcome of dice falls even when they had no physical contact with the dice. In this context, it does not matter whether Rhine's experiments were good or bad. What matters is that subsequent work with improved methods and new ideas has confirmed the existence of PK.

A great surprise of the early work was that PK affected only rolling dice, but could not be measured as a force acting on a stationary die on a sensitive scale. PK seemed to act only where chance processes were involved. This suggested that PK could not be considered as a force, comparable to electric or magnetic forces.

At the early stages of psychical research it seemed appropriate to introduce labels like telepathy, clairvoyance, precognition and psychokinesis to distinguish different psychic "mechanisms." Today, such a clear distinction between different psychic mechanisms seems impossible, experimentally as well as conceptually, because all our naive intuitive notions about the underlying mechanism seem misleading. It is still convenient to use some labels to specify experimental test arrangements. We will use the term psychokinesis, for example, to describe a setting where a subject mentally tries to affect the outcome of some random process. If there is an anomalous, persistent correlation between the subject's effort and the random events, then we will call this a PK effect. But it might be meaningless to argue whether the observed correlation was "really" produced by psychokinesis or some other psychic mechanism.

PK Effects on Quantum Processes

With PK acting on the outcome of dice falls, it seemed natural to explore PK action on other chance events, and in particular on the outcome of indeterministic quantum jumps.

Beloff and Evans (1961) were the first to look for a PK effect on radioactive decays, without success. But a few years later Chauvin and Genthon (1965) reported success in a similar experiment, where the participants tried to speed up or slow down the counting rate of a Geiger tube.

Subsequently, a more stable and more convenient PK test arrangement came into use (Schmidt, 1970), based on a digital random generator. To obtain such an "electronic coinflipper," we can program a computer to count upward at a high rate of 100,000 or more steps per second, and make an arrangement such that the counting process is stopped whenever a signal from a Geiger tube is registered. With these signals arriving at truly random times, there is an equal probability for the computer counter to stop at an even or an odd number. Therefore, we obtain a truly random decision for even/odd or head/tail. This and similar devices have been used in much of the recent PK work.

Figure 1 shows the setup for a typical PK experiment. The box at the left contains the basic electronic coinflipper in combination with counters for the heads and tails. When a pushbutton is pressed, the system generates 128 binary decisions at the rate of one per second. The decisions are internally counted and transmitted through two output lines to an external recorder. A most simple display can be provided by a red and a green lamp, indicating the generated heads and tails respectively.

For a PK test, one instructs the subjects to concentrate, for example, on the red lamp, trying to make this lamp flash more often than the green one. This goal corresponds to an increased generation rate of heads. At the end, one can assess the success from the two counters and from the detailed external record.

For my own experiments, I found it inefficient to gather data from a very large number of unselected people, because the poor scores of the majority tend to dilute the effect of the successful performers. Therefore, I pre-selected promising subjects, and then used these subjects immediately in a subsequent formal experiment with a previously specified number of trials. Unfortunately, the process of locating and pre-selecting promising subjects is time consuming and often frustrating.

Table 1 shows the result of my first experiment (Schmidt, 1971) with a group of 15 selected subjects. We see that 50.9% of the electronic coinflips went in the expected direction. With the large number of binary decisions involved, this small effect is statistically significant with odds against chance of 1000:1.

For my second experiment, I had found two unusually promising performers. The subsequent formal experiment could confirm their strange ability. With a scoring rate of 52.4% hits, the result is statistically significant with odds against chance of 10 million to one.

These results confirm the existence of PK as a weak but real effect. More

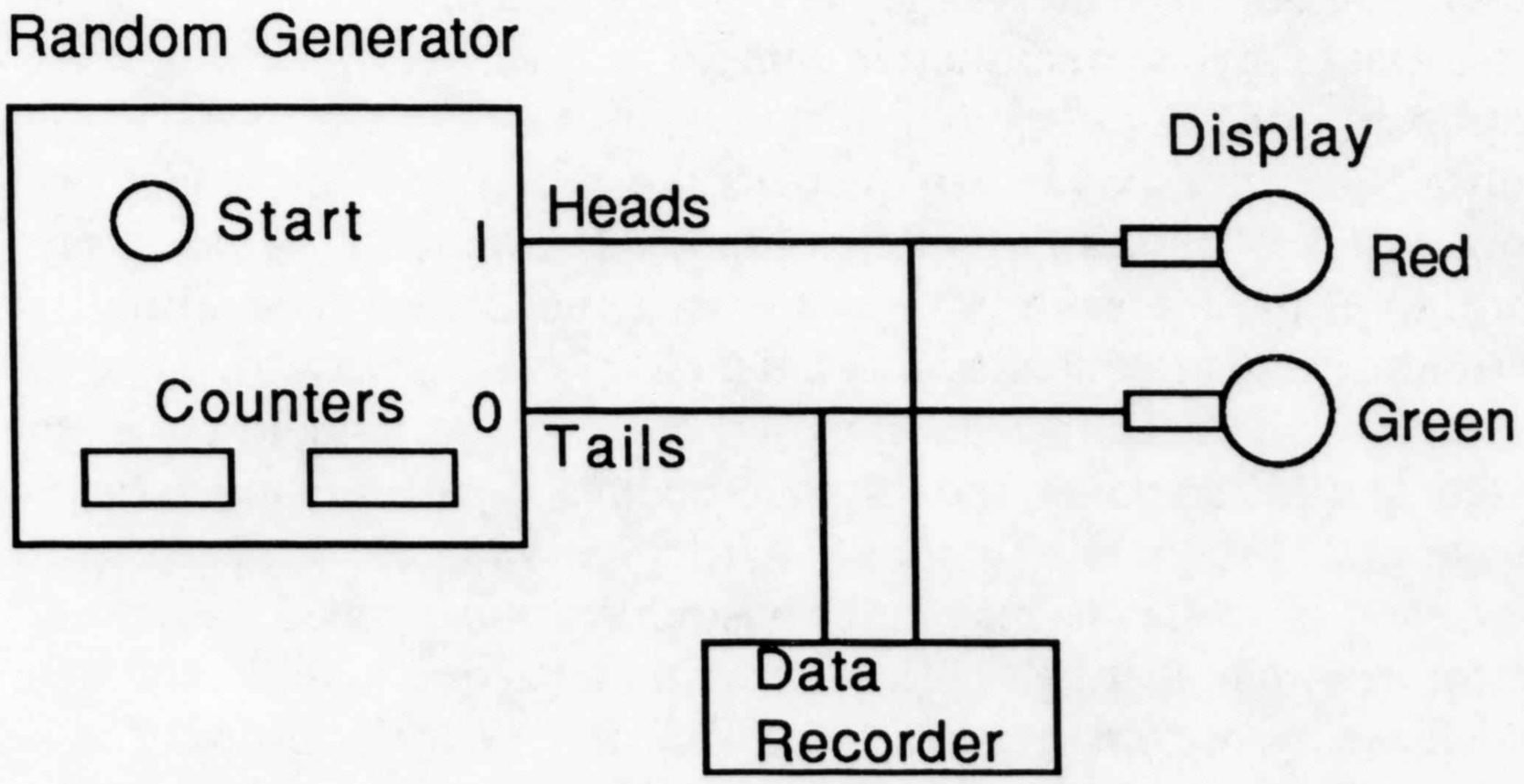

Fig. 1. Test arrangement for a psychokinesis experiment.

TABLE 1
PK tests with binary random generators

First experiment (Schmidt, 1971) 50.9% success on the 32768 bits in 256 runs of 128 decisions each	$z = 3.3$ (1000:1)
Second experiment (Schmidt, 1971) 52.4% success on the 12800 bits in 100 runs of 128 decisions each	$z = 5.3$ (10^7:1)
Total for 27 other researchers (Radin et al., 1985)	$z = 6.2$ (10^9:1)
Dean Radin at Bell Labs (Radin, 1981)	$z = 4.3$ (10^5:1)
Princeton Group (Nelson et al., 1984)	$z = 4.1$ (10^4:1)

details, including the precautions taken against errors are described in the references.

So far, 27 other researchers at many different institutions have published PK studies with binary random generators (Radin, May, & Thomson, 1985). The overall significance of these data appears high, but there may be a bias insofar as researchers with negative results might be less likely to publish their findings. Two of the studies may appear particularly interesting because they were conducted at well known laboratories.

Dean Radin (1981) at Bell Labs reported highly significant PK effects, using my equipment, which he had re-tested for randomness and reliability.

A large scale approach to PK was started by Robert Jahn at Princeton University (Nelson, Dunne, & Jahn, 1984). Working with a limited number of unselected subjects, his group studies personal patterns in PK performance. The overall positive results suggest that, with psychologically skilled experimenters and sufficient patience, PK can be produced in a routine manner.

These results and the results to be discussed later leave, I think, no doubt that a PK effect exists, in discord to currently accepted laws of physics.

Certainly, the observed effects are disappointingly weak, so that it still takes many man-hours to merely confirm the existence of PK.

This makes precise quantitative comparisons often impractical. If we want to compare, for example, the PK action in two different settings, then the number N of trials required to establish the existence of PK under one condition is already unpleasantly large. But if there were only a small, say 10%, difference in the PK action on the two systems, then we would need about 100 times N trials to establish this difference.

It remains to be seen whether, with increased research effort and growing theoretical understanding, the PK effects could be raised to a practically useful level.

For the moment, it seems best to postpone some detailed questions. If we find, for example, that PK works approximately equally well over large and short distances, we might bypass the enormous effort required for a high precision comparison and tentatively assume the simple hypothesis that PK is distance independent.

Similarly, I will introduce in the following two hypotheses that, so far, seem consistent with experimental results, but are open to future scrutiny by more precise measurements. These hypotheses stand out through their conceptual simplicity and their far reaching implications.

The "Weak Violation" Hypothesis

Laboratory experiments suggest that PK might violate the conventional laws of physics only in the weak sense, in that only the outcome of chance processes is affected, whereas the non-statistical laws of physics like the conservation laws for energy, momentum, symmetry, etc. are upheld.

This hypothesis of such a weak violation of conventional physics has great intellectual appeal; and it is a practically fruitful working hypothesis in stimulating new experiments.

If our world were governed by classical physics, with the motion of all atoms ruled by deterministic laws, then an element of randomness, and with it psychic effects, might enter only through the statistical initial conditions. We will not pursue this possibility, however, because, in the real world, quantum effects provide a new source of randomness. In the framework of quantum theory, the weak violation hypothesis can be interpreted in the sense that PK affect only the outcome of the random quantum jumps for which quantum theory makes no unique prediction. Such a hypothesis provides a specific link between PK effects and quantum theory, and leads to rather exotic implications.

Consider as example a situation, often discussed in connection with the Einstein–Podolski–Rosen experiment, where we have two systems A and B that are quantum-correlated but spatially far apart. In the framework of quantum theory, the correlation between observations at A and B can not be used for information transmission. Assuming, however, that we have a PK subject at A, able to bias the outcomes of the A measurement on demand, the situation changes. According to the weak violation hypothesis, the correlation between the observations at A and B (which is given by a conservation law rather than a statistical law) is not affected by PK. Therefore, a PK induced bias in the events at A is instantly observable in a corresponding bias of the events at B. If the PK subject could still perform in such a setting, we would have an instant telegraph.

The beauty and also the technical difficulties of an EPR experiment lie in the fact that, before the measurement, the branches at A and B are still simply correlated (if we want, we can still observe interference effects). Barring non-local interactions, this supports the interpretation that the decision for the outcome of the A-measurement occurs only at the time of the observation, when the subject makes the PK effort.

One might wonder, however, whether the full EPR arrangement is necessary for PK success. If we need only quantum correlated and spatially far separated

systems, we can provide that very easily. Consider the following nearly trivial case:

Let a quantum mechanical random generator make a decision to light a red or a green lamp, and let an automatic polaroid camera take two (identical) pictures of the resulting color. Without looking, insert the pictures into two envelopes to be mailed to distant locations A and B. Then the pictures at A and B are correlated: they both show a red or a green lamp.

The vital difference to the EPR situation is that, with the outcome of the random event macroscopically recorded on film, we can no longer observe an interference between the red and the green state. The previous argument that nature decides for the outcome only at the observation at A is no longer valid. But neither do we have solid support for the counter hypothesis that the decision for red or green is made before the human observation. If, therefore, we instruct a subject at A to open the envelope while wishing for a red color, this PK effort might still succeed. This kind of PK test, being technically much simpler, seemed as a reasonable beginning. Before going into specific experiments, however, let me present a different approach that leads to the same end.

The Equivalence Hypothesis

Experiments with different types of "electronic coinflippers" as well as experiments with dice report PK effects of the same order of magnitude. In particular, nobody has been able to produce a random generator that is noticeably more sensitive to mental efforts than other generators.

For a systematic comparison of PK action on different random generators, one has to present the generators so that the subject and the experimenter cannot tell them apart, because we want the comparison made under psychologically identical conditions. Such studies (Schmidt, 1974) suggest (within the limited power of comparison tests mentioned earlier) that the nature of the random generator does not affect the PK scores. (Psychological factors like the subject's motivation, the form of display, and the rate at which the random events are presented may make a great difference for the scores.)

This leads us, tentatively, to the following "Equivalence Hypothesis":

If we have two truly random binary generators, operating such that the generators are from the outside physically indistinguishable, then a PK effort affects the systems to the same degree, i.e., the systems are also undistinguishable in their response to a PK effort.

This hypothesis appears particularly appealing because, like the earlier hypothesis about the weak violation of physics, it is very simple and universal. And no matter whether or not these hypotheses will finally turn out to be quite correct, they can stimulate novel and provocative experiments.

Causality Violation

Most participants in PK experiments use, subjectively, a goal oriented approach, focusing on the desired outcome like the lighting of a red lamp rather than on the internal operation of the random generator. The finding that the success rate is rather independent of the physical structure of the generator, furthermore, suggests that goal orientation may be a feature of the underlying mechanism rather than a matter of mere psychological attitude. It appears as if the subject, by concentrating on the final outcome, could induce nature to let the previous random events properly fall into place such as to lead to the desired outcome. This suggests an element of non-causality in the sense that the subject's present mental effort could affect the previous decisions of the random generator.

To study this explicitly, consider the two "black boxes" in Figure 2. The boxes contain similar internal random generators, producing continuous streams of binary decisions. The difference is that the left system displays these decisions immediately, whereas the right system displays each decision only after a 24-hour delay.

Both boxes with their continuously randomly flashing lights being physically undistinguishable from the outside, the equivalence hypothesis implies that a PK effort should affect the right box as well as the left one. This is, indeed, confirmed by the experiments to be discussed below. But success of a PK effort on the right box seems to violate our common concept of causality. We feel that the subject's effort at the time of the display, 24 hours after the random events, comes too late to be effective.

On the other hand, physicists have toyed occasionally with non-causal systems (Schmidt, 1966, 1981; Wheeler & Feynman, 1945) and shown that causality is not a logical necessity. Perhaps, physicists were just lucky in that

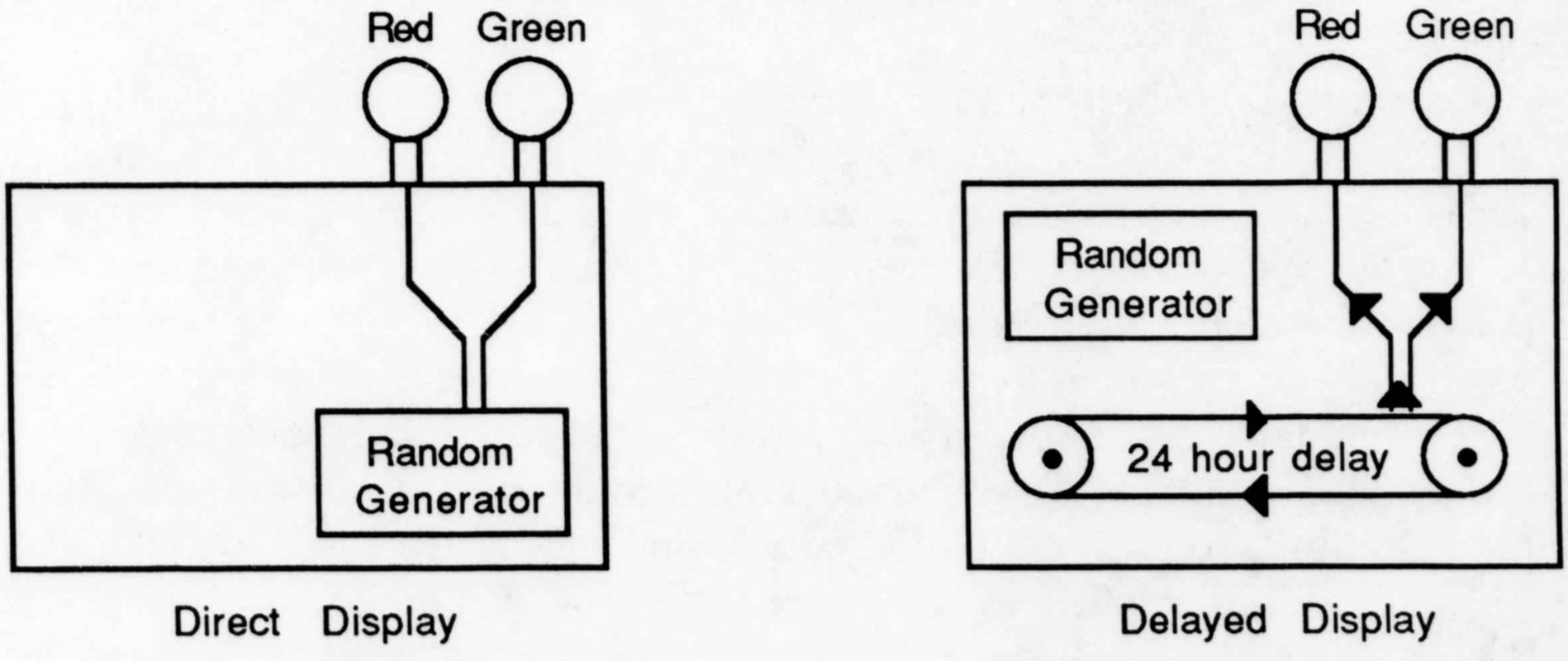

Fig. 2. PK effect on pre-recorded random events. Each of these "black boxes" displays two lamps, flashing in random sequence. For the physicist, the boxes are from the outside indistinguishable. The equivalence hypothesis requires that the two systems are also undistinguishable in their response to PK efforts.

most of this world can be described in terms of causal mechanisms so that the conceptual and mathematical complexities of non-causal systems could be avoided. And perhaps non-causality is the feature that makes psychic effects appear so intuitively implausible and "unphysical."

Let us look next at the experimental evidence that PK efforts can still succeed in a setting similar to the right side of Figure 2, where the random events are generated and recorded long before the subject makes a PK effort.

PK Effects with Pre-Recorded Random Events

Consider the following test arrangement (Schmidt, 1976). A binary generator produces blocks of 201 random decisions. These are recorded simultaneously on two cassette tapes, with the heads and tails as signals in the right or left stereo channels respectively. At the generation speed of 20 signals per second, the tapes can hold typically 140 data blocks, with 20 second pauses between the blocks.

When the recording is completed, one of the identical tapes is placed into a safe, while the other tape is used in the PK sessions. During these sessions, the subject listens through headphones to the tape, perceiving the signals as clicks in the right or left ear. The subject's PK goal is an increased rate of clicks in the right ear, corresponding to an excess of generated heads. Whenever the subject gets tired, he can stop at any of the 20 second pauses on the tape and adjourn the test.

For the experiment, six tape pairs were pre-recorded in this manner, with a total of 832 blocks of 201 signals each. After the subject had worked on all tapes, the tapes were evaluated to see whether there was, indeed, a bias towards heads as aimed for by the subject. The simple but reliable evaluation method counted the number of successful blocks with more heads than tails and the number of unsuccessful blocks. The result showed that 54.6% of the 832 blocks were successful. This is statistically significant with odds against chance of 100:1.

A series of subsequent similar experiments (Schmidt 1976; Schmidt, Morris, & Rudolph, 1986) confirmed the PK effect with pre-recorded events, at high levels of statistical significance (z-value of 3.1, 4.2, 2.0, and 2.7).

To complete our discussion, remember that the initially generated random events were identically recorded on two cassette tapes, with one tape given to the subject and the other one kept locked in a safe. The correspondence of the two tape copies was guaranteed by a macroscopic recording process not subject to chance factors. According to our weak violation hypothesis, the PK effort should not alter the correspondence between the two records. This was, indeed, verified: the two records still agreed at the end, both showing the same bias towards heads.

Thus, the results suggest a non-local interaction in the sense that a PK effort on one tape is observable on the other tape also. This may remind us of the quantum correlations between far separated systems as discussed in

connection with the Eistein–Podolski–Rosen paradox. But there is the vital difference that the quantum correlation alone, no matter how puzzling and exotic it may appear, cannot be practically used for information transmission.

The exotic features of PK, on the other hand, can be made practically useful. As a side benefit, we can make PK testing particularly convenient and safe. We can let the subject work with the tape unsupervised at his home. We do not have to worry about the possibility of fraud, because we can evaluate the experiment from the tape in the safe, which never was near the subject.

We can carry this one step further and make the experiment safe even against errors and fraud by the experimenter, by channeling first hand evidence for psychic effects to an outsider, as follows.

First, we prepare a pair of tapes with identical sequences of random numbers. Next, we give one sealed tape copy to the outsider, letting the outsider decide whether he wants an excess of signals in the right or the left tape channel. We communicate this target assignment to the subject, who can now start the test sessions, playing the tape back while trying to get more signals in the specified channel. At the end, the outsider counts the numbers of right and left signals on his tape. And if the PK subject was successful, the outsider finds that, indeed, there are more signals in the channel he specified. If we repeat this procedure with a sufficient number of tapes, the outsider cannot but acknowledge the anomalous correlation between what he specified and what he later found on his tape.

We have used this basic method, with minor modifications, for an actual experiment, with myself as the experimenter and Bob Morris and Lu Rudolph at Syracuse University in the role of outsiders (Schmidt et al., 1986). Morris is an active parapsychology researcher and Rudolph a professor in communication engineering. Both felt rather skeptical with regard to PK effects on pre-recorded events. We used two outside observers supervising each other, because we also wanted to minimize the possibility of fraud by the outsiders.

The whole experiment was structured into ten sections, with each section to be evaluated by its z-value (the observed deviation in units of one standard deviation), positive values indicating success and negative values indicating failure. Figure 3 shows that only one of the ten sections gave a negative score. The total result, as seen by the outsiders, is significant with odds against chance of 100:1 ($z = 2.7$).

The Problem of Interpretation

Psychic phenomena being at odds with current physics as well as with naive intuition, we cannot explain the effects in terms of more familiar concepts. For the theorist, the ultimate "explanation" might consist of an abstract formalism that, like the formalisms of quantum theory or relativity theory, describes the observed phenomena consistently, teaching us to adjust our intuition accordingly. For the more practically inclined experimenter, "under-

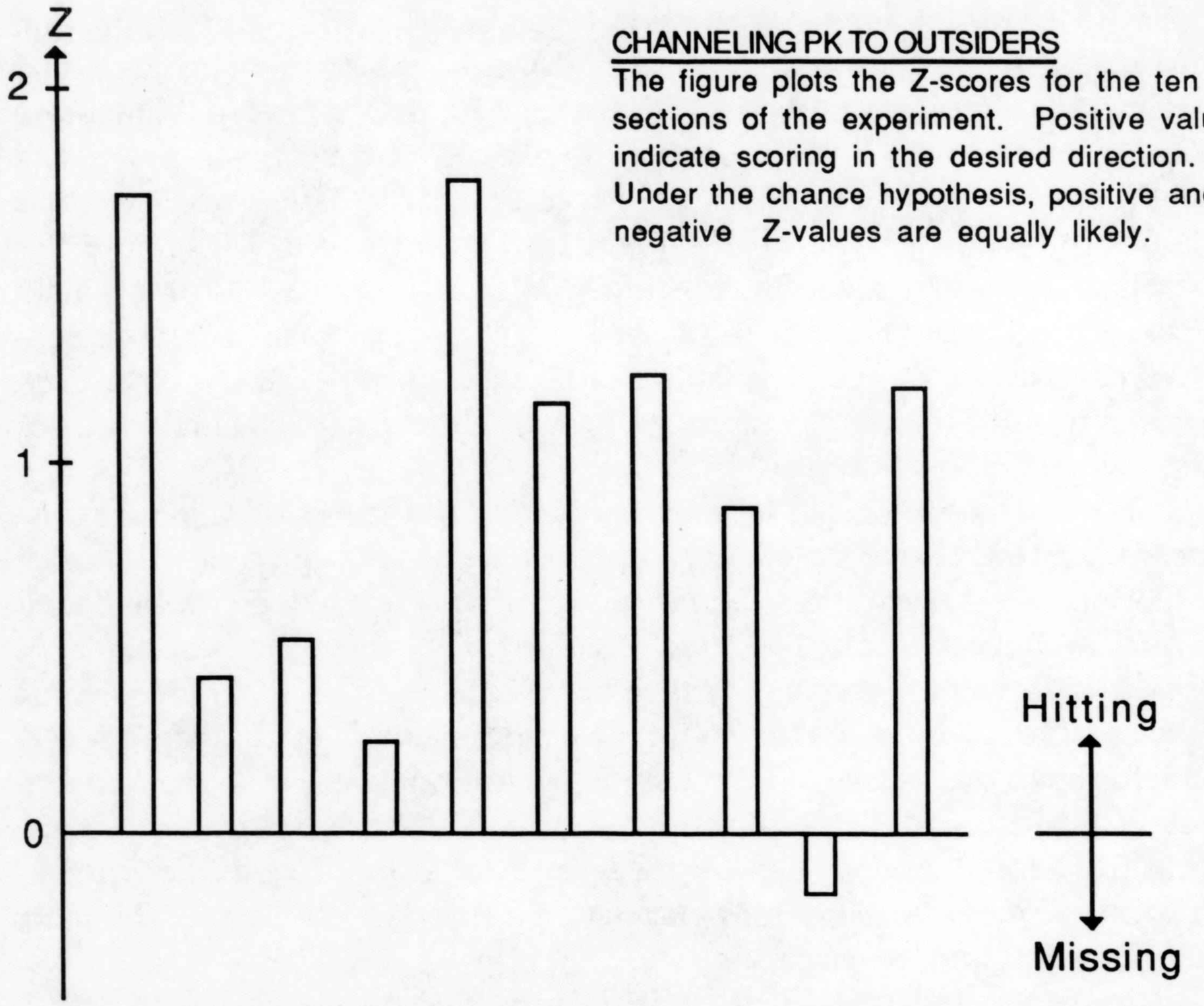

Fig. 3. Results of an experiment in channeling PK to outsiders. The figure plots the *z*-score obtained for the ten sections of the experiment. Positive values indicate scoring in the desired direction. Under the chance hypothesis, positive and negative *z*-values are equally likely.

standing" might be equivalent to sufficient hand's on experience and full information on what psychic mechanisms can and can not do.

The two hypothesis we have introduced might, if confirmed, serve as stepping stones for a future theory. The results of the experiments with pre-recorded random events were consistent with the hypotheses. But there are details that one might want to check more thoroughly.

The most basic question is whether it was really the subject in his time delayed PK effort that produced the effects. Let us consider some alternatives.

In view of the goal oriented operation of PK, the experimenter might have sensed the outsider's future target assignments and might have (unwittingly) activated his PK powers accordingly, at the time the random events were generated. While such a possibility cannot yet be ruled out completely, earlier experiments (Schmidt, 1976) in which subjects selected by their high success in regular PK experiment performed also particularly well in tests with pre-recorded PK targets point strongly to the subject as the PK source.

More information on this question of whether the subject's effort, indeed, is the source of the PK effect might come from experiments comparing the subject's mental state (during the PK session) with the resulting scores.

Another possibility to consider is that the outsider who generated the random target assignments could have been the source of the success. The outsider might have sensed the previously generated random numbers and influenced the generator for the random assignments accordingly. This argument would apply, however, only to the last experiment, because the earlier tests with pre-recorded targets did not involve such an outsider.

The weak violation hypothesis and the equivalence hypothesis agree in suggesting that PK tests with pre-recorded events should work as well as other PK tests. The weak violation hypothesis with its specific reference to quantum theory, however, introduces an additional element that we should discuss in some detail.

The Reality Problem and the Observer in Physics

Quantum theory brought changes in our view of physical reality and the role of the observer in physics. These changes may be relevant for the interpretation of PK tests with pre-recorded events.

The reality problem appears most directly at the microscopic level. Considering an electron or a photon, for example, we can measure the position of the particle, but we must not conclude that the particle always has a well specified position. Otherwise we get into conflict with the results of interference experiments. Thus, the location of a particle before a measurement does not have the kind of absolute physical reality that we customarily assign to the outside world. With quantum theory making no basic distinction between large and small systems, however, a reality question arises also for the macroscopic realm. This has been emphasized by recent experimental work related to Bell's theorem and the Einstein–Podolsky–Rosen paradox (d'Espagnat, 1976, 1979).

Consider, for example, the case where a decision from a binary random generator is recorded as a signal in the right or left channel of a cassette recorder. Then, prior to any observation of the outcome, quantum theory represents the state vector of the system as a superposition

$$|\text{STATE}\rangle = |\text{R-STATE}\rangle + |\text{L-STATE}\rangle \tag{1}$$

of two macroscopically different physical states, where $|\text{R-STATE}\rangle$ corresponds to a situation with a "head" generated and signals recorded in the right channels of each tape, while $|\text{L-STATE}\rangle$ corresponds to the other possibility that a "tail" was generated and signals were recorded in the left channels of the two tapes.

Intuitively, we feel that nature, at this stage, must have decided on one or the other outcome. But the formalism of quantum theory contains no parameter which could tell us when such a decision is "really" made. Rather, the formalism suggests that the appearance of a head or a tail remains as unreal as the position of a particle until the outcome has been observed.

This leaves the question of the nature of the mechanism that transforms the |STATE⟩ of equation (1) into either one of the macroscopically unambiguous states, |L-STATE⟩ or |R-STATE⟩.

Conventional quantum theory answers this question very simply by interpreting the state vector not as a measure for some absolute physical reality but only as a measure of the observer's information about the system. Then the macroscopically ambiguous state of equation (1) reflects only the observers ignorance. And the sudden change of the observer's knowledge during a measurement is naturally accompanied by a sudden jump of the state vector (the collapse of the state vector) into the |L-STATE⟩ or the |R-STATE⟩.

The mathematical elegance and logical consistency of quantum theory in this interpretation has suggested to many physicists that quantum theory has universal validity, giving the best possible description of nature, for small and large systems, and even for systems that include human subjects.

From the described experiments we can conclude, however, that such an universality claim must be wrong. Systems that contain a human subject can show features, like the discussed anomalous correlation between the subject's wish and an external random event, that are inconsistent with quantum theory.

Independent of the results of parapsychology, the early pioneers of quantum theory had already felt uneasy about this theory which admits reality only with regard to an observer, but does not explain if or in which respect the human observer is special (London & Bauer, 1939; von Neumann, 1932/1955; Wigner, 1962). This has led to many attempts at modifying or re-interpreting quantum theory. Let me mention only a few of these attempts, not because they should be taken particularly seriously, but because they might stimulate PK experimenters.

Eugene Wigner suggested that current quantum theory is not quite complete, that it lacks a specific description of the role of the experimenter. Wigner proposes that in the absence of an observer the state vector follows Schroedinger's equation, but that in the act of observation, some still unknown mechanism takes over, transforming the state of Eq. (1) into either the |R-STATE⟩ or the |L-STATE⟩. Thus, the collapse of the state vector assumes some physical reality of its own.

The active role played the observer in collapsing the state vector might provide an opening for PK effects to enter. This possibility and its experimental implications have been pursued by the author (Schmidt, 1981), and a specific model for the underlying mechanism, based on a hidden variable model by Bohm and Bub, has been studied by Mattuck and Walker (1979). Walker has also proposed several other interesting original ideas on the relationship between parapsychology and quantum theory. (Walker, 1979).

Wigner's model provides for minimal modifications quantum theory; only at the last stage where the observer (with his elusive element of consciousness) enters. In this model, there can still occur macroscopically ambiguous states like the one of Eq. (1), such as if physical reality before an observation could consist of two branches of reality, with nature still undecided.

As an alternative, one could consider modifications of quantum theory that provide for an automatic reduction of the state vector at some lower level, such that macroscopically ambiguous states could not arise. Such a model could still be consistent with all obtainable laboratory evidence, and, nevertheless, permit the concept of a macroscopically absolutely real world in which the observer is not basically different from other recording and computing equipment.

A New Look at PK Tests with Pre-Recorded Events

In our previous discussion of PK tests with pre-recorded events, we had implicitly assumed that macroscopic phenomena have an absolute reality, independent of any observation. From this viewpoint, nature made a final decision on the outcome of a random event, the event "really" happened, when the outcome was recorded on the cassette tapes. This led us to talk about causality violation in the sense that the subject's later mental effort affected the outcome of a previous random event.

Quantum theory, in the conventional form or in Wigner's version, however, permits a different interpretation: With the outcome of the random decision not observed before the PK session, nature had made no previous decision yet. The PK subject encountered a physical reality composed of two equally real potential branches. Therefore, the subject's PK effort did not have to reach into the past. Rather, the subject's observation transformed the still ambiguous reality into one of the macroscopically unique branches; and in this process of state vector collapse the subject also produced a PK effect, a slight change in the conventional transition probabilities.

From this viewpoint, the PK mechanism should no longer work if the outcome of the random decisions were observed by someone else before the subject made the PK effort. The previous observation should collapse the state vector, forcing nature into a final decision, so that there would be nothing left to change for the PK subject.

The only experiment in this direction done so far (Schmidt, 1985), indicates, indeed, such an inhibition of the PK effect by previous observation.

For this experiment, 80 sequences of 128 random bits were initially pre-recorded. Each bit sequence could be displayed in a one minute test run by a pendulum swinging at a computer screen with slowly varying amplitude. (Basically a "1" in the bit sequence increased and a "0" decreased the amplitude of the next cycle).

At the start of each run, according to a fixed schedule, the subject was instructed either to aim for a high or a low pendulum amplitude. The resulting score measured the deviation (from chance expectancy) of the average amplitude in the specified direction.

Each sequence was displayed subsequently to two subjects, a test subject with a record of good PK performance, and a control subject with no demonstrable PK ability.

The sessions were timed such that in half of the runs the PK subject was the first to make the PK effort and in the other half the control subject. Furthermore, for each condition, the subjects aimed in half of the runs in the same and in the other half in opposite directions. The subjects were kept ignorant of these conditions.

Figure 4 shows that the PK test subject was successful only when being the first to see the random events in a run ($Z = 3.1$), whereas the previous observation by the control subject brought the score of the PK test subjects close to chance ($Z = -0.7$). The control subject did not show a significant PK effect, even when acting as the first observer.

With only one experiment of this kind available, one might not want to draw a final conclusion but rather wait for future confirmations. And if, indeed, an initial human observation can block a subsequent PK effort, then we would want to know whether observation by a cat or even some lower animal might have the same effect. We might even be able to operationally define a "conscious observation" in terms of its effect on a subsequent PK effort.

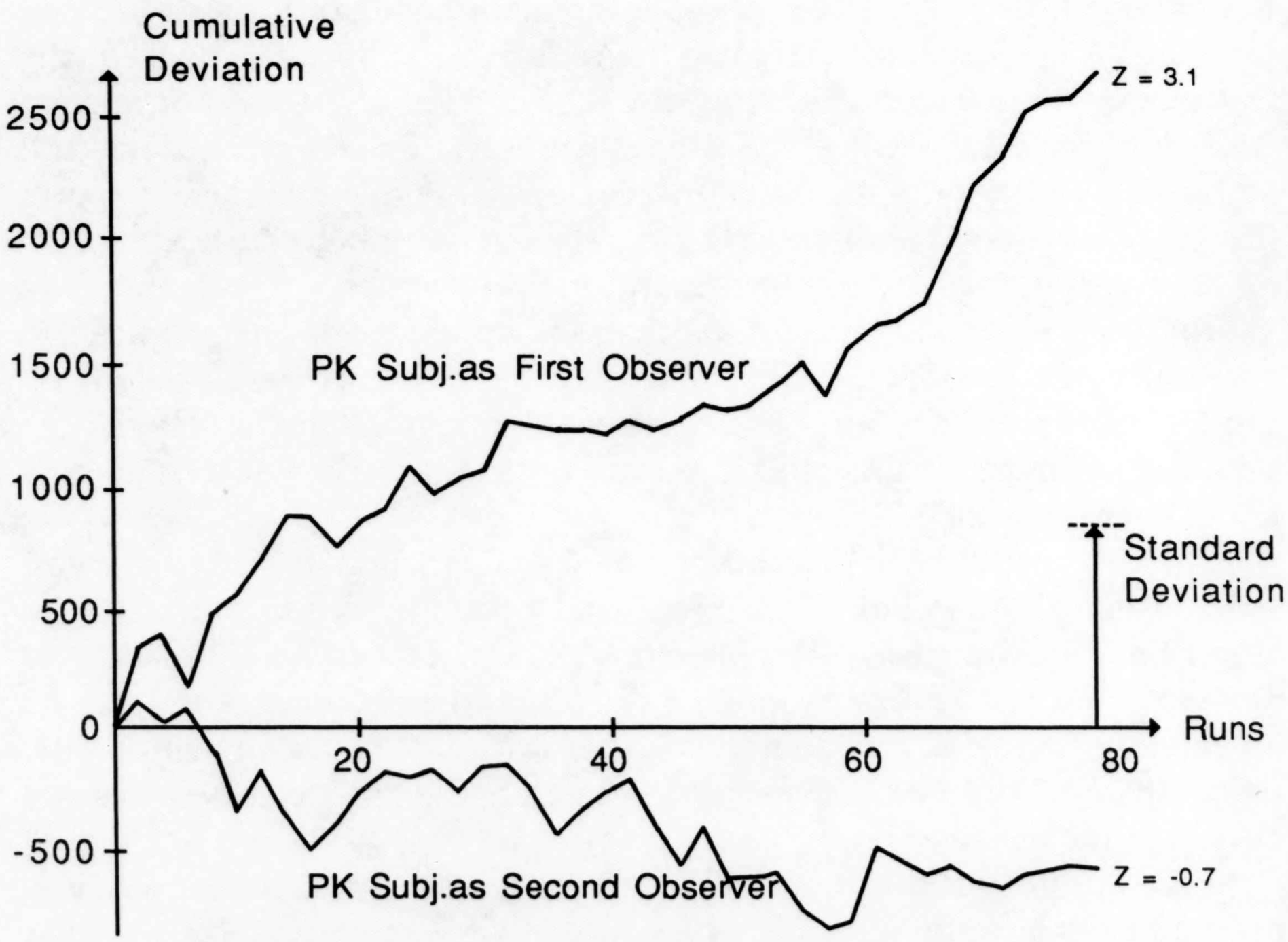

Fig. 4. Result of an experiment in which a PK subject (with a history of PK success) and a control subject (with no apparent PK ability) made consecutive PK efforts on the same pre-recorded events. In the half of the runs where the PK subject made the first effort, his cumulative score (upper curve) reached a significant level. But in the other half of the runs, where the control subject had seen the outcome of the random events first, the PK subject's effort was no longer successful (lower curve).

Conclusion

Quantum theory has raised some still puzzling questions with regard to the role of the observer and the nature of reality. At the same time, the mathematical elegance and the logical consistency of quantum theory has suggested that the theory may have universal validity, being applicable even to systems that include human subjects. And with conventional quantum theory apparently experimentally correct, there seemed little demand for modifications with their inherent mathematical complications.

The results of parapsychology, on the other hand, indicate that quantum theory can be experimentally wrong when applied to systems that include a human subject. Experiments like the reported ones, point to specific links between the quantum formalism and psychic effects, suggesting a wealth of further interesting experiments. It remains to be seen whether the results will lead to a new quantum theory that includes psychic effects within its mathematical formalism, or whether they will merely outline some final limitation of the quantum formalism.

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What Do We Mean by "Scientific?"*

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Abstract—There exists no simple and satisfactory definition of "science." Such terms as "scientific" are used for rhetorical effect rather than with descriptive accuracy. The virtues associated with science—reliability, for instance—stem from the functioning of the scientific community.

When a question is used as the title of a talk (Bauer, 1981), it is a reasonable assumption that the question is being put rhetorically: the speaker intends to provide the answer. But that is not exactly my intention here. I will say something about answers that have been suggested: but I shall maintain that the question remains open. I shall argue that no answer exists that is both short and empirically accurate: and none that is consensually accepted among those who discuss such matters. I shall also argue that the term "scientific" is itself most commonly used as a rhetorical device, to add force rather than substance to the point being pressed.

It is philosophers, of course, who have thought for the longest time about the nature of scientific knowledge. I was fortunate to hear, a few weeks ago, a splendid summary (Laudan, 1983) of that philosophical quest: Larry Laudan, himself a contemporary philosopher of science of note (Laudan, 1977, 1981), concluded that there is no epistemological criterion by which one can characterize science. Let me spend a little time on that, since the conclusion usually seems unpalatable to practising scientists when they understand its implications.

Until the middle-to-end of the 19th century, science was acknowledged to be characterized by the certainty of its knowledge. Philosophers, natural philosophers, scientists—and often the same individuals could properly be called any one of these—agreed that science equalled infallible knowledge. But that stance has proved to be untenable. For example, as science progresses, one can look back and note that the purportedly infallible science of yesterday has been replaced: making it plausible, indeed likely in the extreme, that today's science will also be found wanting in the future.

It is not just that theories change while certified facts and definite knowledge accumulate. Facts turn out to be slippery rather than graspable, and they change significantly as theories change—as our ways of looking at things change. Facts are theory-laden is how the philosophers put it (for example, Brown, 1979). Consider, for example, the "facts" about the structures and

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properties of atoms and nuclei: hardly had the existence of atoms become "hard fact" than the fact turned out not to be atoms as indivisible entities: nor were all atoms of a given element the same—not even in their chemical behavior. And what facts about elementary particles would one care to regard as infallibly established? As not subject to change in the future?

So philosophers came to realize that the facts and theories of science are not synonymous with certainty; and practising scientists and philosophers came more and more to go their separate ways, learning from one another less and less. As one result, practising scientists as a class still believe that science is synonymous with truth and certainty; a belief that humanists and social scientists commonly and pejoratively describe as "scientism."

Then came attempts to show that the scientific method (if not facts or theories) has some special virtue, leading progressively to greater certainty, closer to truth if not actually getting there. Those attempts continue, but face great difficulties: first, because there exists no agreement on what a satisfactory characterization of "the scientific method" might be: second, because the suggested attributes of "the scientific method" are not accurately descriptive of what scientists actually do: third, because none of the empirically accurate descriptions of what scientists do can be logically proven to have epistemological significance.

Consider the sort of things said about the scientific method: rational, impartial, strict regard for accuracy and controlled experiment, empirically based, looking to reproducibility and verification; careful, consistent, cautious, and so on. And those presumed attributes also define the popular stereotype of the good scientist, Martin Arrowsmith (Lewis, 1925)—careful, objective, disinterested, modest, shy of publicity, unconcerned with personal advancement or possessions, naively idealistic.

Then think about the way scientists describe the best examples of successful practice in science: creative, original, new; daring, surprising; splendidly correct; elegant. . . . Does work of that sort really come from carefully disinterested people obsessed with strict accuracy and reproducibility? For my part, I think it happens only rarely. I have known some first-rate scientists, and they were usually ready to ignore many results in favor of others about which they had hunches, ready to defend their theories vigorously despite the lack of sufficient proof, ready to jump intuitively to conclusions . . . and I have known a few Arrowsmiths, marvelously educated and conversant with the specialist literature, consulted by all on account of their judiciousness—who published hardly at all, and never anything of note. The Nobel prize went to Watson and Crick, not to the systematic, judicious, erudite Erwin Chargaff—which seemed unfair to a number of people, including Chargaff himself, who has written in very bitter tones about the manner in which Science nowadays is not as it ought to be (Chargaff, 1963, 1977, 1978).

Confidently will I wager to find examples of competent science that contradict any of the suggested attributes of "the scientific method." For instance, there is the use of hypotheses. Twenty-five years ago, having just begun post-

doctoral research in chemistry, I happened to encounter a political scientist who told me that the scientific method consisted in setting up hypotheses and testing them. That shocked me: my own mentor in research had never said anything like that. His style had been, "Let's try this and see what happens"; or, "If we look at many substances by this technique, something interesting will turn up"; and the mentor, Bruno Breyer, attained a very respectable international reputation, primarily as the pioneer of an important technique in electrochemistry (Breyer & Bauer, 1963).

Is the scientific method a matter of careful experimental control of the variables? No, excellent science has come from those who are simple observers: taxonomists, paleontologists, field biologists, astronomers. Is the scientific method a matter of being systematic in collecting and categorizing? Hardly, unless one wishes to include philologists and historians and philatelists and numismatists and many others under the rubric of "science." And too many sciences are non-mathematical to permit rigorous quantification to serve as a criterion.

One hears it said that the scientific method consists in going where the data lead one; that it consists in discarding theories when the evidence is contradictory. But is there anyone here, anyone in the world of science, who has not put aside data that did not fit the purpose at hand? We all make judgments about which data to keep and use as meaningful, and which to discard as probably misleading; and those judgements, as Polanyi (1967) has forcefully argued, are largely tacit judgements which cannot be reduced to a formula derived from some precise specification of a scientific method.

I hope that I have aroused a suspicion that we use the terms "science" and "scientific" without knowing precisely what we mean. But we do use those terms, and I think *some* meaning does attach to them. Let me speculate about what that meaning is.

Some disciplines are universally agreed to be sciences—physics, for example, or chemistry, or biology. A chemist knows a good deal about such things as atoms and molecules, about their properties, about chemical reactions; he knows how to obtain more such information in reliable ways; he has useful theories; he knows how chemists work, and often he knows something of how chemistry has developed over the years and decades, even centuries. I think that a chemist also assumes that chemistry is an exemplar of "Science," and that "Science" has the same characteristics that he has learned to associate with chemistry. But the practical characteristics with which a chemist is most familiar are not all the matters of methodology and epistemology that concern the philosophers of science and the others who seek to understand "Science" as an entity in itself. Thus when a chemist (or any other practising scientist) talks about the characteristics of Science, he is talking about a different thing than the philosopher or historian. I suggest, for instance, that when a scientist describes a fact or theory as having been certainly established, he means a different sort of certainty—a more limited one—than the philosopher who discusses what "certainty" involves.

Even beyond that, not all chemists have the same notion of what "science" is, because different sorts of chemists have learned rather different things—and have learned rather different things to be important. I still remember the seminar given 30 years ago by one of my fellow students, a theoretician. His widely acclaimed dissertation consisted of bringing under consideration a previously neglected factor in the calculating of certain properties of molecules. His calculations gave results that differed from the measured values by a greater margin than did previous calculations: but his work was nevertheless seen as an advance, because he had made one of several previously neglected factors amenable to calculation, and everyone had faith that theory and experiment would eventually come to agreement when all the other significant factors had also been brought to mathematical heel. For the moment, however, experimental verification was simply not seen as of importance: the abilities to conceptualize, to quantify, and to solve technical problems of calculation were the significant "scientific" virtues. I, on the other hand, was seeking reliable values for the quantum yields of certain reactions. My ability to rationalize any given experimental result by a suggested reaction mechanism found no favor with my mentor, who reminded me that the essence of my job in "science" was to be sure of every aspect of technique and apparatus—I must achieve precise reproducibility, and precise concordance with previous results on model systems. Surely, then, I learned that accuracy and careful experimentation are paramount in "science," while my friend learned that theoretical understanding and mathematical virtuosity are paramount. Such differences, I suggest, occur in every sub-field and sub-sub-field of what we call "science," and lead every scientist to have a somewhat different notion of what science "actually" is.

So there are multiple meanings associated with the terms "science" and "scientific" and "scientific method" and "scientific knowledge." Most or all of those meanings have some truth to them, but no single one of them represents the whole truth, and, most important perhaps, all of those meanings are seriously erroneous and significantly misleading if they are used without qualification—if one speaks of "science" globally instead of making clear that one speaks of but one aspect of some part of science. But in practice this mistake of over-generalization is almost always made. We take our particular understanding of the sub-discipline in which we work, or of philosophy or of history, and generalize that as referring to "Science" *in toto.*

Analogous situations are common enough. We talk of "civil rights," and of "economic justice," and of "democracy," and of "patriotism," and so on; and there is general agreement that those are very worthwhile things. But when specific applications are proposed, it turns out that different people have rather different conceptions of what those words mean. To some, for example, "economic justice" means a flat rate of income tax, whereas to others it means a steeply graduated tax. Both sides talk of their proposals as embodying "justice" because that is universally seen as a good thing: it is a useful rhetorical device, to clothe one's personal opinions in generalities that have wide approval.

It seems to me that something similar occurs when we talk publicly about "Science." In our society, "Science" symbolizes man's power over Nature, and "Science" symbolizes Truth. Even those who attack "Science" believe those things: they attack Science for using its power incorrectly or unethically, not for being without that power; they attack Science for neglecting or rejecting some supposed truths, not for incorrectly claiming to embody truth. And so "science" and "scientific" are immensely powerful rhetorical and polemic devices, and they are so used—misused and abused.

I will hardly be contradicted, I take it, if I criticize such usage in the domain of everyday life and popular culture—when I point to the advertisements that seek to sell toothpaste by talking of "scientific proof" obtained through "scientific tests." (Note, incidentally, the power of the word. Would not the same sense be conveyed by simply talking of *tests* that *prove* something? No, the adjective "scientific" carries a supposed guarantee of certainty.) But I aim to take no such cheap shots: my target is this rhetorical use *by scientists,* on subjects in or close to science.

Consider the public arguments over the nature and place of so-called "creation science." Note, first, how the creationists seek intellectual respectability by attempting to bend the term "science" to their own ideological, political, social ends. But then ask, is the scientific community well served when leading figures in it claim that evolution is not a theory but a scientific fact? (Gould, 1981; Kornberg, 1981; Leakey, 1982; Sagan, n.d.) I think not. Leaving aside the technicality that the claim is incorrect, I deplore these attempts to invoke the authority of Science to settle arguments. After all, the public is often told that science is openminded and holds no theories as unquestionable—often by the same individuals who are thus dogmatic on specific issues.

Some wise things have been said and written about the proper role of scientists in public arguments. Alvin Weinberg (1972) pointed out that such arguments are often carried on as though purely technical or scientific issues were at stake, and that scientists on both sides of the issues attempt to invoke the authority of science on their side: when, in point of fact, the questions are actually ones of values—what Weinberg calls "trans-scientific" rather than scientific questions. Relevant examples are the debate between Oppenheimer and Teller about the feasibility of a crash program to construct a hydrogen bomb; or the controversy about the effects on human beings of exposure over a long time to very low levels of radiation; or, about the designing of an anti-missile missile-system. Weinberg argued cogently that the experts should restrict themselves to offering the best possible elucidation of the technical matters without coupling that to the question of what course of action the society should take. A similar point has been made by advocates of a so-called Science Court (Anonymous, 1976). Michael Polanyi (1967) put the matter thus: "Laymen normally accept the teachings of science not because they share its conception of reality, but because they submit to the authority of science. Hence, if they ever venture seriously to dissent from scientific opinion, a regular argument may not prove feasible. It will almost certainly prove impracticable when the question at issue is whether a certain set of evidence is

to be taken seriously or not." (By "a regular argument," Polanyi meant the disciplined way in which technical disagreements within science are settled among experts—through controlled experiment, logical argument, refereeing of published contributions, and so on.)

That brings me to what is most germane to the present occasion: controversies about claimed anomalies which are different from the sorts of anomalous results that are routinely obtained and routinely handled within the various disciplines. For our present purpose, "anomalous phenomena" connotes claims usually made by individuals who are not necessarily expert in the relevant technical disciplines: and the claims themselves tend to be ones that seem quite incompatible with existing disciplinary knowledge. In the arguments about such claims, examples abound of *ex cathedra* statements designed to discredit a particular claim not through reasoned discussion but by invoking the authority of science. I will spare you the most outrageous instances in order to emphasize how insidiously we can fall into this sort of behavior.

In a quite moderately phrased, well referenced, soundly argued piece (Krupp, 1981) criticizing the pyramidologists, Atlantists, and gods-from-outer-spacers, I found the following sentence: "The archeologist tries . . . to avoid subjectivity and misinterpretation through systematic, scientific acquisition of information." I wondered what role "scientific" played there; other than to emphasize that archeologists are scientists, and their work scientific—to be believed, true.

The foreword (Kurtz, 1981) to a recent anthology debunking pseudo-science is entitled, "Believing the Unbelievable: The Scientific Response." And the text begins thus: "A dispassionate observer of the current scene can only be astonished by the rapid growth of bizarre belief in recent years among wide sectors of the public." The author, who happens not to be a dispassionate observer, and who is also not a scientist, seeks here to serve as a spokesman for Science; and talks of "the scientific outlook in which knowledge is based upon careful methods of inquiry and verification." Yet in that very article, he makes undocumented and unproven assertions about a host of subjects. I see here the same technique as that employed by the creationists: the use of the terms "science" and "scientific" as rhetorical devices to serve ends that are ideological, and therefore personal.

Now if science is anything at all, it surely is not a personal matter. I said earlier that no epistemological or methodological generalizations properly characterize science; now I shall suggest that some sociological principles do apply. I tried to present a paradox: that the best science—original, daring, elegant, and so on—is not the result of strict adherence to the scientific method as usually described; that those who do adhere to that method most usually produce only banalities and trivialities. The apparent paradox dissolves if one recognizes a distinction between individual behavior and organized or institutional behavior.

For the present purpose, this distinction parallels that made by historians and philosophers of science who noted that discovery and justification occur

in different contexts. Individuals who do scientific work can employ any style they choose, and success or failure are not necessarily determined by that style: wild hunches sometimes pay off, and the most routine measuring of parameters sometimes results in a marvelously unexpected novelty; and the individuals themselves may be flamboyant, immodest, overambitious, unethical, and so forth—not at all Arrowsmithian. But the work that is done does not really become Science until it is acceptable to the scientific community. The tests applied before work is so accepted serve to weed out the unreliable, unsupported, and erroneous: other individuals, with other styles, examine what has been done—and what is finally accepted thereby becomes relatively independent of style, having run the gauntlet of several different styles along the way. And, of course, the scientific community agrees that, in the testing of candidates for entry into science, it is proper to employ logic, to demand clear evidence, to avoid unexplained contradiction, and to respect the validity of existing knowledge, methods, and theories. A criticism commonly made of Science is that it resists strikingly new claims. That, of course, is to be expected under the scheme just described. Candidates for entry must be judged against what has already been incorporated; the more strikingly the claimed novelty contradicts existing scientific knowledge, the more overwhelmingly must the novelty be supported by the evidence if it is to be communally accepted (Barber, 1961).

So what is accepted into science, and particularly what remains accepted for long periods of time, *seems as though* it has been obtained by work carried out objectively, impartially, carefully, reproducibly, and the rest. But the virtues reside in the system: of awarding of degrees, of refereeing, of awarding tenure—of using communally enforced judgements and standards. No individual working outside that system could produce science as good as he can produce within the system.

Unfortunately, we are not always conscious of that. It is easy for me to assume that my expertise as a chemist makes me also an expert scientist, and that I can be "scientific" about anything to which I turn my attention. And so it is easy for me to label "unscientific" or "pseudo-scientific" anything that appears to be badly done or wrong. This is really the point to which my talk was intended to lead. We have chosen to call ourselves the Society for Scientific Exploration, and I hope that we will be conscious of the virtues that the term "scientific" can connote: the proffering to others of evidence and ideas and approaches in a manner that can be convincing to those others on grounds of logic, careful examining of assumptions, documentation, and so on; and the willingness to have criticisms made in similar fashion, and to respond to those criticisms through the re-working of arguments and the re-examining of evidence and re-thinking of ideas and approaches.

The virtues I have just enumerated are characteristic not only of good science, but of good work in any intellectual endeavor—historical scholarship, for example. Marcello Truzzi (1971, 1977) has suggested that the investigation of anomalous phenomena ought to be referred to as proto-scientific, since it is analogous to what transpired in the early days of what are now established

sciences. The point is a good one. Established disciplines, it seems to me, deal in three kinds of things: method, knowledge, and theory. Typically, research is carried on by accepting orthodoxy in two of those and seeking novelty in the third. Quantum theory, for example, was a respectable new theoretical venture because it was firmly grounded in well-established orthodox knowledge—the spectral distribution of black-body radiation—obtained by well-established orthodox spectroscopic method. By contrast, anomalous phenomena in our sense are not grounded in orthodoxy in any of the three aspects of method, knowledge, or theory. Typically, there is disagreement about the claimed knowledge: mistaken identifications, hoaxes, or fraud are often suggested as explanations. And typically the method depends initially on human testimony, which is given short shrift in the natural sciences and treated with the greatest skepticism in the social sciences. And, of course, orthodox theory typically offers no obvious explanation for anomalous phenomena.

So we must be clear about the fact that we are cut loose from the safeguards that are usual in established disciplines, and in the natural sciences in particular. That in our own disciplines our judgments and intuitions are likely to be quite good does not assure that they will be good over anomalous matters. In expressly pursuing the exploration of anomalies, we are attending to matters which the most relevant disciplines have—at least for the moment—judged to be so implausible as to be not worth pursuing; yet we stand ready to pursue them. Clearly, we cannot use our disciplinary knowledge, methods, and theories in the ways to which we have been trained. We will succeed, I believe, only if we can learn to deploy the virtues by which earlier proto-sciences became the well-established scientific disciplines of today, the virtues I enumerated earlier. We should seek to make the Society for Scientific Exploration an effective community, behaving institutionally *as though* we were individually impersonal, objective, openminded, and logical.

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Analysis of a UFO Photograph

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Abstract—This report reviews various investigative activities and analyses surrounding a photograph of a purported unidentified flying object (UFO) taken on October 8, 1981 at about 11:00 a.m. local time on Vancouver Island, British Columbia. The evidence consisted of a single frame of 35 mm color film which showed a sharply focused disc-like object against a clear blue sky with wooded mountain peak nearby. Analyses of the original negative included micro-densitometry, computer enhancements, and other measurements intent upon showing a support thread, atmospheric disturbance, or other evidences of a hoax. These analyses suggest that the disc was a three dimensional object located at a distance of at least 30 feet from the camera; the object's surface albedo was diffuse and of lower luminance than sunlit cloud. Extensive interviews with the photographer (who never saw the aerial object), her husband and daughter and site survey tended to support the entire narrative account. The identity of the disc object remains unidentified.

Introduction

Contrary to common belief there are many photographs of alleged UFOs. Of course the problem lies not so much with the details of the photograph and its negative as with the photographer and the equipment used. It is for this reason that one must be careful to fully document seemingly unimportant details concerning the person taking the photograph, the social situation which surrounded the photograph(s), the camera-lens-film data, the developing-printing-enlarging activities and the manner in which the photograph came to the attention of the investigator. Since such a photograph image is only as credible as the photographer who took it, one must exercise "due diligence" in each of the above areas. Many older UFO photographs remain useless artifacts of the UFO enigma because the investigator did not or could not obtain all of the relevant background information. As will be made clear, the author attempted to consider all of the above factors. Length restrictions of this paper impose certain practical limits upon the depth to which these facts can be documented, however.

The remainder of this report will cover the following topics: (a) the photograph and negative, (b) the camera and lens, (c) the film and its processing, (d) the results of image analysis, (e) the site visit results, (f) credibility of the photographer, and (g) a brief review of Frisbee characteristics.

The Photograph and Negative

The author received a color negative strip of two frames. The higher numbered frame showed a small child standing in front of a fireplace inside a

home. The lower numbered frame showed a mountain whose top was very nearly centered within the frame. The foreground detail was in sharp focus indicating that either a fairly high shutter speed had been used or the camera had been stabilized or both. Figure 1 is an enlargement of the full negative reproduced as a positive print. A sharply focused disc-like object is seen above and to the right of the mountain top. The tip of the mountain was located close to the geometric center of the 35 mm frame. This tends to support the statement made by the photographer that she was intent upon photographing the mountains and never saw the aerial object. The presence of a cloud directly illuminated by sunlight through extremely clear air provides a useful upper exposure limit in later densitometry measurements. Dark shadows seen within a stand of trees in the left foreground provides a lower end to the exposure. Thus, a maximum of 12,500 ft-L is assumed for the luminance of the cloud and approximately one ft-L for the shadow area. The atmospheric clarity makes it almost impossible to judge separation distance between the camera and object by means of an extinction coefficient calculation.

The negative measured 36 × 24 mm. A photographic enlargement of the disc image provided for linear measurements. Its major axis on this print was 5.70 cm while its minor axis was 1.60 cm for a width/height ratio of 3.56. The width of the "dome" protruding from the upper surface was 1.3 cm and

Fig. 1. Photograph of mountain and disc.

its height about 0.7 cm (ratio of 1.86). Finally, the dome to disc width ratio was 4.38 and the dome to disc height ratio was 2.29. While these ratios are a function of the viewing aspect angle, they may, in general, be compared with corresponding values given elsewhere (Haines, 1978, 1979) of similarly shaped aerial objects of unknown origin that have been reported over the years. This previous work shows that the present photographic image of a disc is not uncommon; there is a larger context of purported UFO drawings into which this object fits.

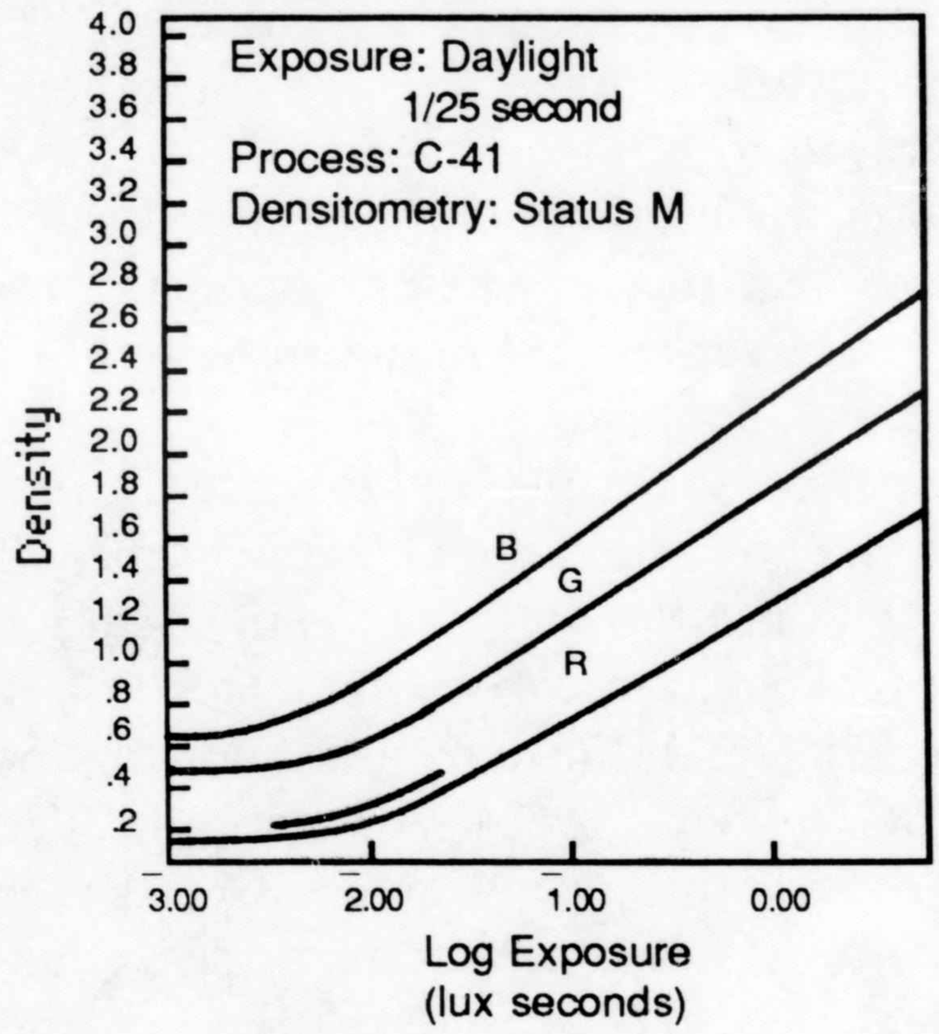
The *hyperfocal distance* is the nearest distance from an object being photographed to the camera at which the object is in sharp focus when the lens is focused on infinity. Since the mountain is in sharp focus and the photographer said that she took great care to manually focus the camera it is possible to determine the hyperfocal distance by also knowing the focal length of the lens and its aperture (Neblette, 1965). For a 50 mm focal length lens and f-11 aperture the hyperfocal distance is 15 feet. Corresponding distances for f-8 and f-16 apertures are 20 and 10 feet, respectively. Thus, the airborne object in question must have been farther than ten feet (and probably farther than 20 feet) from the camera since it also was in sharp focus.

The actual camera settings used can only be inferred. A film with an ASA 100 exposure index (as this was) and a lens with the optical quality of this one would have very likely automatically pre-set a shutter speed of 1/125th second at an aperture of f-11 for the ambient scene luminance which was present.

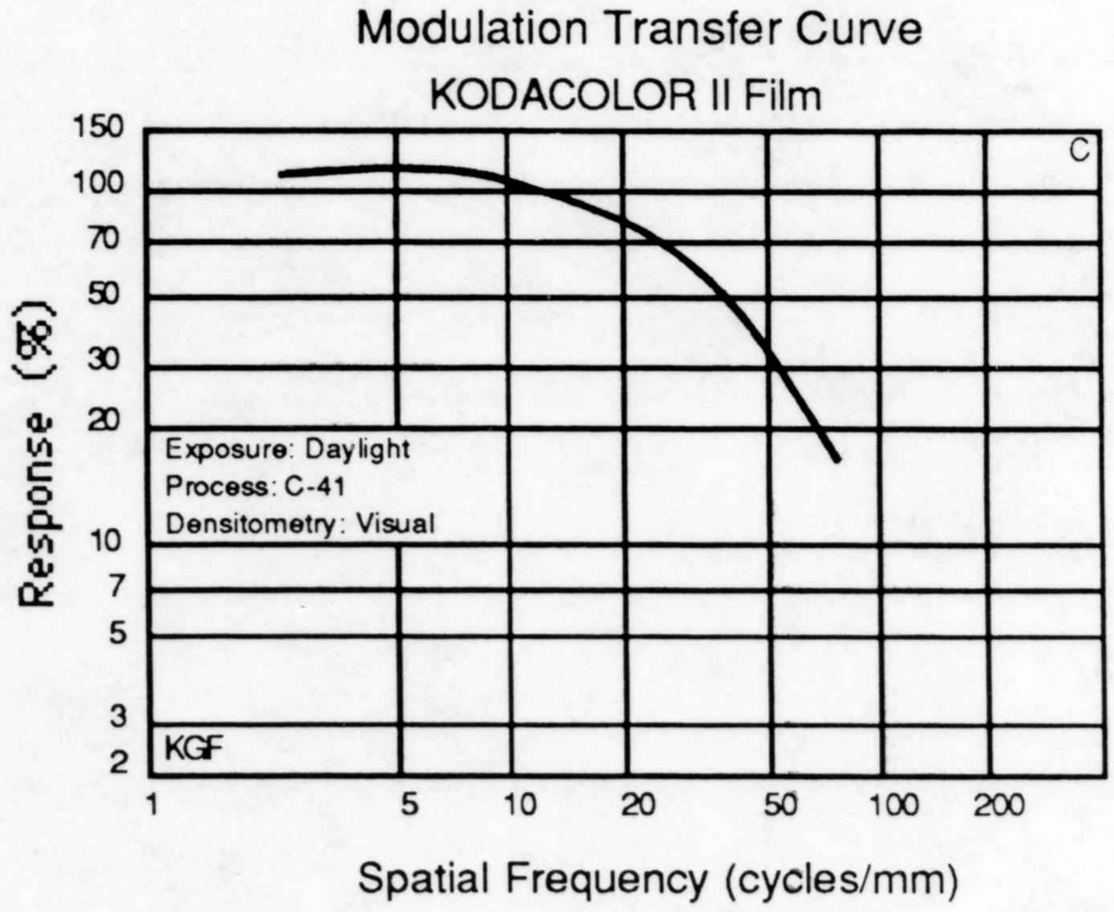
Finally, the negative was in very good condition and did not have any visible scratches, major blemishes, or other defects. An invisible vertical scratch was noted in one of the dye layers as will be described later, however, this scratch was well away from the disc image.

The Camera and Lens

The camera was a Mamiya, model 528AL, single lens reflex (SLR) type with permanently attached Mamiya/Sekor 48 mm lens with 1:2.8 aperture. The serial number on the lens was M197535. The camera is of the automatic exposure type meaning that all one needs to do to take a photograph is insert the film, pre-set the correct ASA number for the film used on the camera, aim and manually focus the lens for best focus, and press the shutter release. Both shutter speed and aperture adjust automatically for "best" exposure. The shutter actuation lever is of the standard top-mounted type which requires a downward finger depression. Once the shutter has opened and closed another exposure cannot be taken without first advancing the film to the next frame. The author borrowed this camera and took a series of photographs with it under closely similar sun angle, sky brightness, and other conditions to check on possible lens and/or shutter related image artifacts. None were found. The lens was coated with the standard anti-scattering material. No scratches or other flaws could be seen in the lens elements or surface coatings.



(a)



(b)

Fig. 2. Kodacolor II film characteristics: (a) Characteristic curves, (b) Modulation transfer curve, (c) Spectral sensitivity curves.

The Film and its Processing

The film used was Kodak Safety Film 5035, 35 mm, commonly known as Kodacolor II. Its ASA rating is 100. The photograph in question was located at frame 11. The so-called "characteristic curve" (i.e., exposure vs. optical density), modulation transfer function, and spectral sensitivity for this film are presented in Figure 2(a), (b), and (c), respectively (Eastman Kodak Co. Manual, 1980).

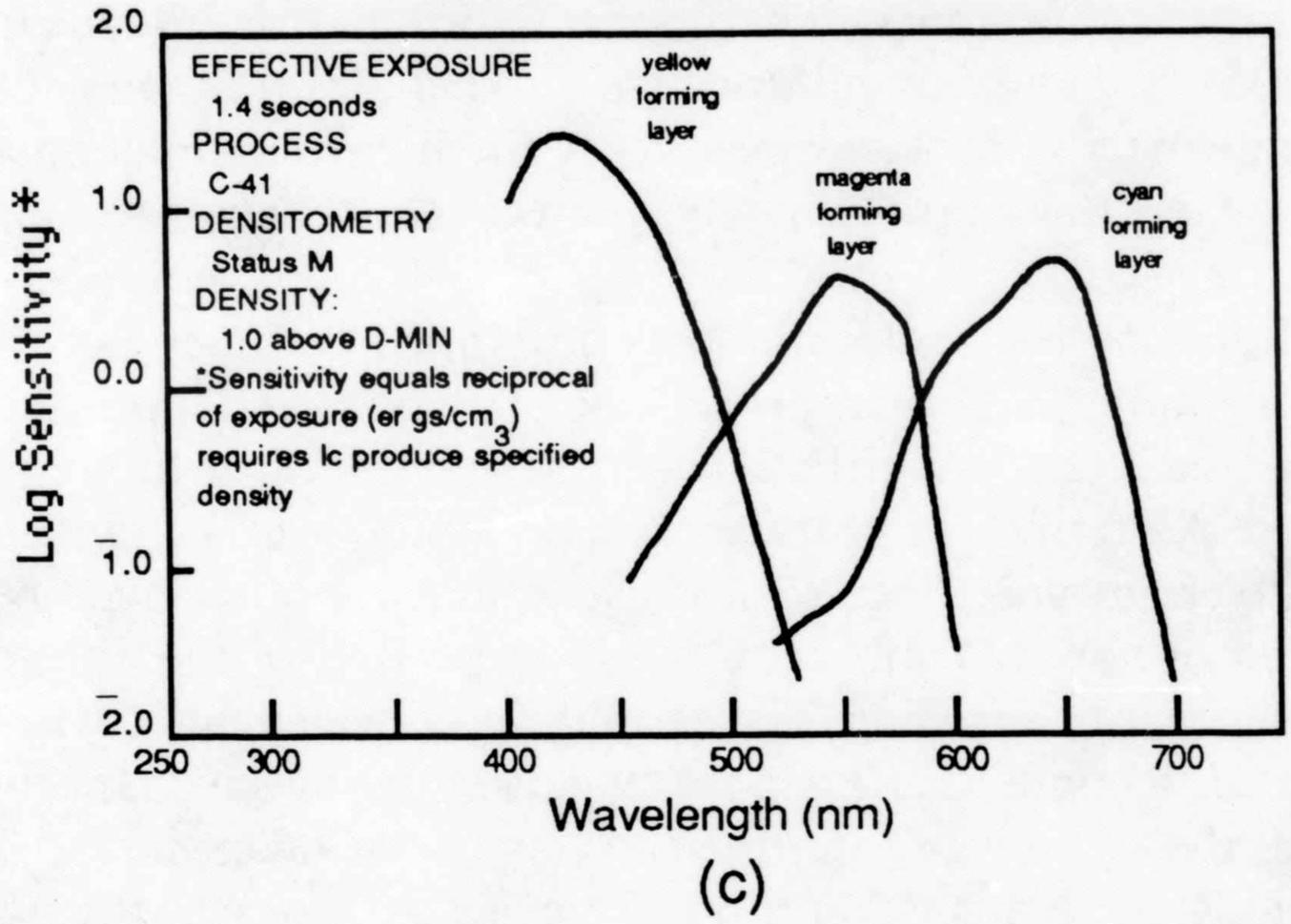


Fig. 2. Continued.

The concept of "graininess" and "granularity" of photographic film is important here. Graininess is defined as the subjective sensation one gets when viewing an enlargement of a photograph of a random pattern of variations in texture, color, or both in regions of homogeneous luminance and color exposure. Granularity is the result of an objective measurement of the film using an instrument known as a densitometer which measures the local density variations that give rise to the sensation of graininess (Kodak Publications F-20, 1973). Most silver halide crystals that make up photographic film are dispersed in a gelatin and coated in thin layers on a supporting (paper, etc.) base. Importantly, these crystals vary in size, shape, and sensitivity to light energy. In general, they are also randomly distributed. As the Kodak manual states, "Within an area of uniform exposure, *some* of the crystals will be made developable by exposure; others will not. The location of the developable crystals is random" (Kodak Publication F-20, 1973, p. 3; italics mine).

One result of this random distribution of light-sensitive crystals (grains) is that patterns can be produced which have nothing to do with the object that was originally photographed. If such a pattern is perceived as having a recognized shape, it is possible to conclude that the shape represents an object somehow related to the primary object when, in fact, there is no functional correlation with the object.

The diameter of the film's crystals is also important. The Kodak film manual indicates that a typical crystal diameter ranges from 0.2 to 2.0 micrometers. At a normal viewing distance of 25 to 35 cm the human eye can just discriminate a crystal (grain) on the order of 0.05 mm diameter. Normally the eye does not perceive the granular structure at low magnifications. The finer the mean diameter of the crystals the higher the magnification can be before graininess is noticed. It is the random nature of the exposed crystals, each

possessing a different spectral reflectivity in white light, which is a necessary condition for the appearance of graininess to occur. Also, the mean diameter of film crystals becomes a significant factor when deciding on a diameter for a scanning digitizer's entrance aperture. A six micron diameter aperture was used here which was approximately $\frac{1}{2}$ mean crystal diameter of the present film.

The roll of exposed Kodak film was developed commercially. According to the photographer, a normal processing delay occurred (approximately 1.5 weeks). She did not specify that special development or any enlargements were desired. A set of color positive prints ("jumbo size") and the developed color negatives cut apart into sets of two frames were received by the photographer on or about October 26, 1981. Inspection of the attached second frame to the one in question by the author showed that it was of a child standing inside a home. This child was the daughter of the couple who owned the camera; the scene was confirmed to have been taken inside their home in Campbell River, British Columbia. This fact agreed with the story told by the couple concerning the sequence of events of their automobile trip to the north end of Vancouver Island and return and the photographs they remembered taking.

It was not until the couple had received the set of color prints from the Vancouver processing laboratory that they noticed the strange aerial disc in the clear blue sky near the mountain top.

Image Analysis Results

The analysis of the negative (and also first generation positive black and white and color prints) included the following steps: (a) linear and angular measurements, (b) micro-densitometry scans to establish optical density range, (c) black and white photographic enlargements using papers having different spectral sensitivities, and (d) computer-based contrast enhancements.

Linear and Angular Image Measurements. Selected linear measurements of the disc's image on the enlarged print are given earlier (cf. The Photograph and Negative). The angular measurements shown in Figure 3 were determined on the basis of the linear measurements of the image, the camera lens' focal length, and on-site survey which is described below. Referring to Figure 3 it may be pointed out that the photograph's center was elevated about nine degrees arc above the horizontal and the tip of the mountain was very close to the geometric center of the photograph. The disc subtended 1.3 degrees arc. Elevation angles from the local horizontal were measured with a surveyor's transit to the top of the mountain, the location of the photographer, and the sun. These latter measurements were obtained at the same time of day, day of the year, and location as that of the original photograph but two years later. These results are presented in Figure 4 with the disc shown in side view. The exact outline of the disc is not known; also, the dome-like protruberance is not shown here.

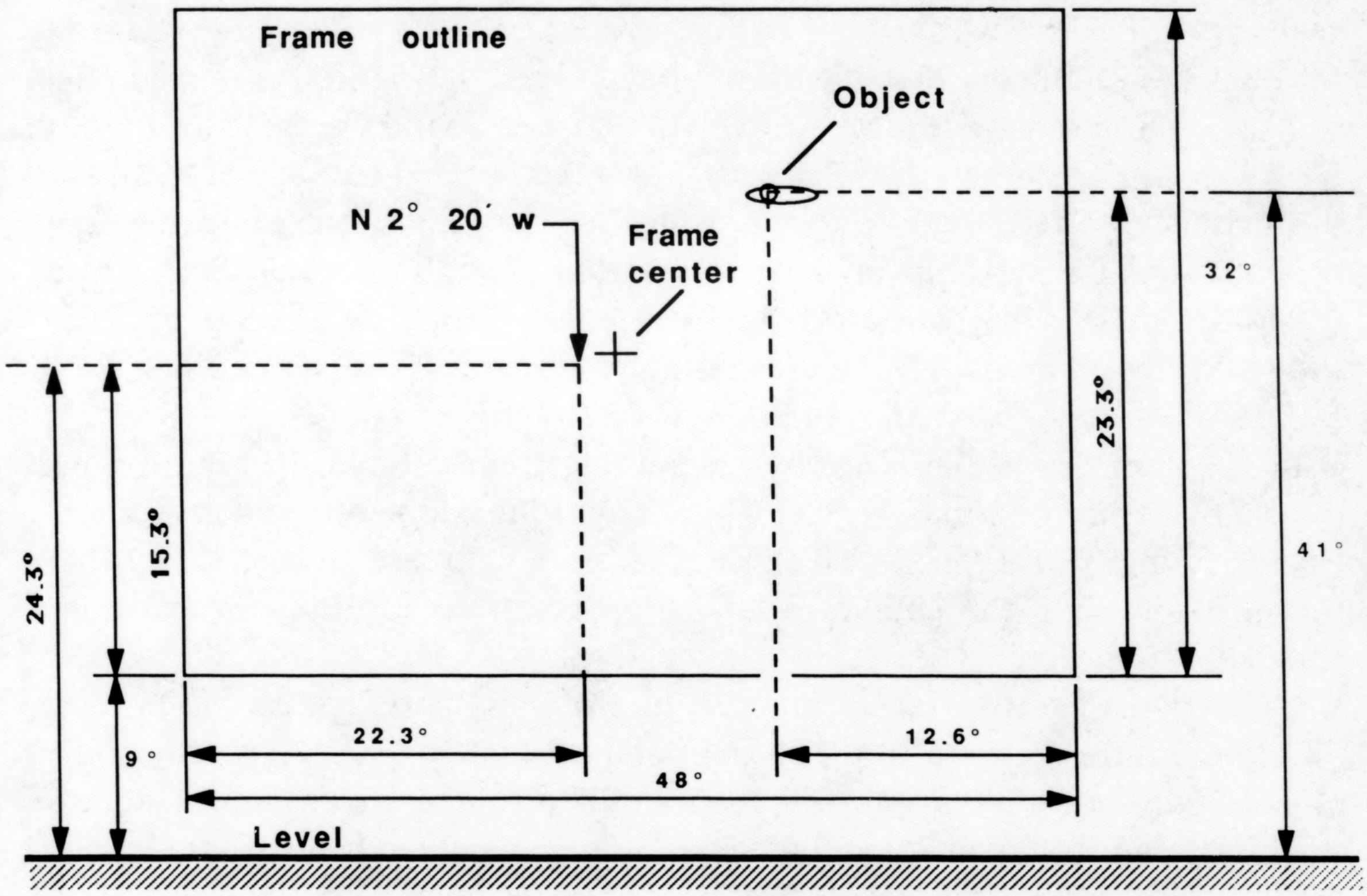


Fig. 3. Horizontal and vertical angles in the photograph.

Micro-densitometry Scan Results. A Joyce Loebl Recording Micro-densitometer with ×20 power objective, 50:1 linear magnification ratio, slit width of 0.02 inches and vertical slit height of 1 mm was used on the original negative.

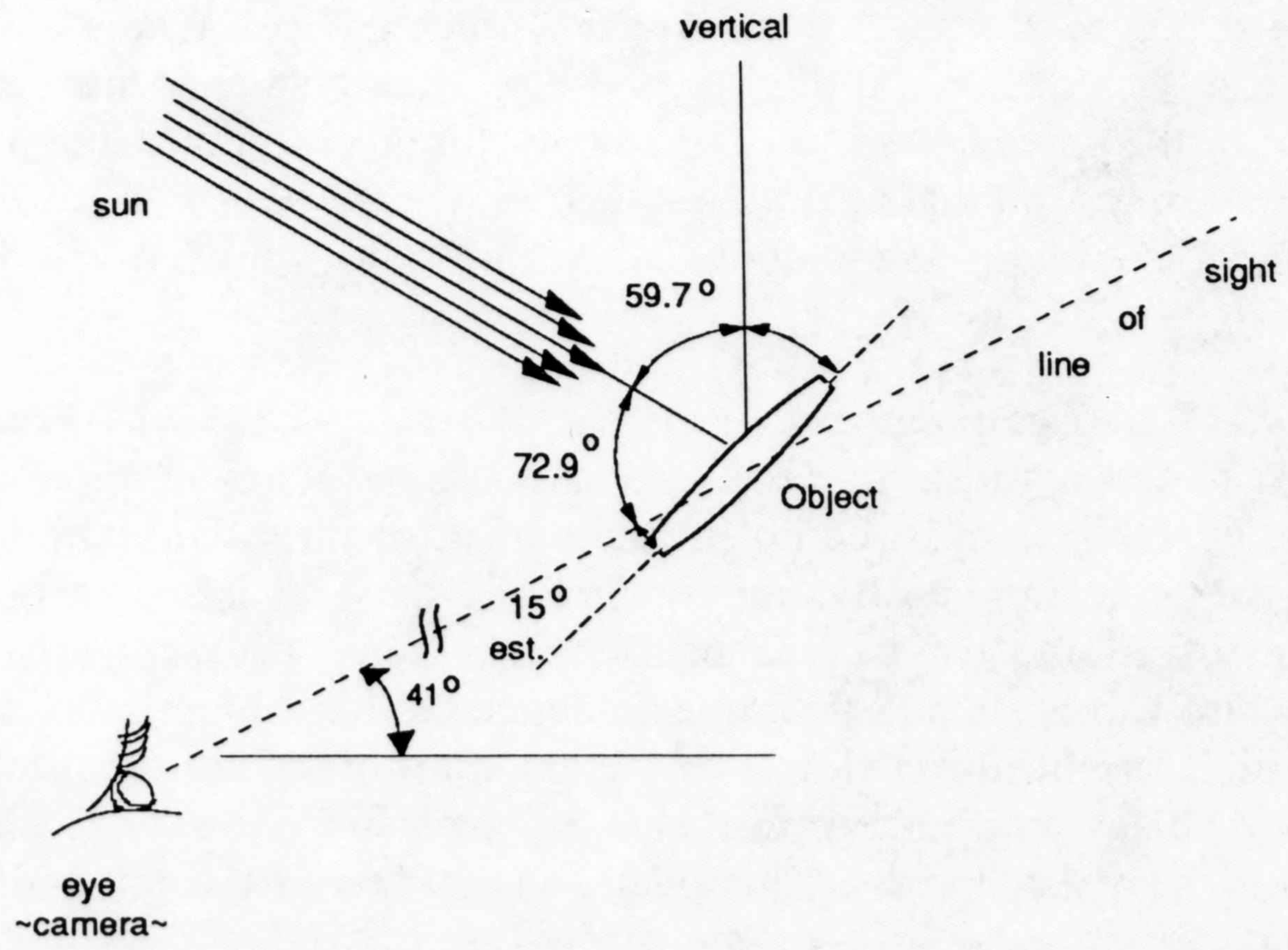


Fig. 4. Summary of elevation angles.

Density calibration was carried out using a Kodak ND step wedge spanning the densities on the negative. Greatest optical density (brightest positive image) was approximately 0.65 to 0.7 $\log_{10}$ ND and was found on the sunlit cloud. This is equivalent to about 12,500 ft-L luminance. Figure 5 is a vertical scan through the disc's two brightest areas using the micro-densitometer. The tracing peak marked T represents the upper-most (dome) bright area and B represents the lower area of brightness on the front edge of the disc. This scan line is shown in Figure 6 which is an enlargement of the disc's image. Points T and B both have optical densities of about 0.55 to 0.6.

Optical density of the blue sky on the negative is shown in Figure 5 and has a value of approximately 0.4 $\log_{10}$. The gradual slope of this densitometry tracing is due to the progressive sky brightness increase from the zenith to the horizon while the smaller amplitude deviations are due to single and grouped film grains.

Of particular note is the fact that the brightest area on the disc was of lower brightness than the cloud by approximately 0.15 $\log_{10}$ unit. According to a physics handbook (Allen, 1963), a smooth, polished silver surface reflects (within the visible spectrum) increasingly higher percentages of incident radiation with increasing wavelength. An average reflectance value of about 90% is found. Polished aluminum reflects about 85% regardless of wavelength of the incident radiation; this is also true for nickle (reflectance of about 60%), silicon (about 30%), and steel (about 54%). This comparison of dark areas on the negative suggests that the surface of the disc is very likely *not* a polished surface of any of the above metals. If direct sunlight has a brightness of about 750,000 ft-L and a 90% reflectance is assumed for the disc's surface, the brightest area should produce a brightness of about 675,000 ft-L which is more than an order of magnitude greater than what was found on the negative.

A horizontal scan using the micro-densitometer also was made to see if there was any evidence of a double exposure. A double exposure might be indicated by the presence of double edges if the film registration is not precisely the same during a manual rewind. No such evidence was found. In addition, this camera could not take double exposures due to its frame locking mechanism.

Black and White Enlargements on Different Wavelength Sensitive Paper. The disc area of the negative was enlarged and printed on panchromatic film which provided a relatively complete and undistorted translation of the three primary colors in the negative into shades of gray. This is shown in Figure 7(a). The top "dome" protruberance is clearly visible. The same area on the negative also was printed at the same enlargement using blue-green sensitive paper which significantly reduces the contribution of the red emulsion layer to the final black and white print. This is shown in Figure 7(b). The blue-green sensitive paper increases the overall brightness of the sky and causes the "dome" area on the disc to almost disappear. Apparently, the dome is not reflecting or emitting radiation in the red end of the spectrum.

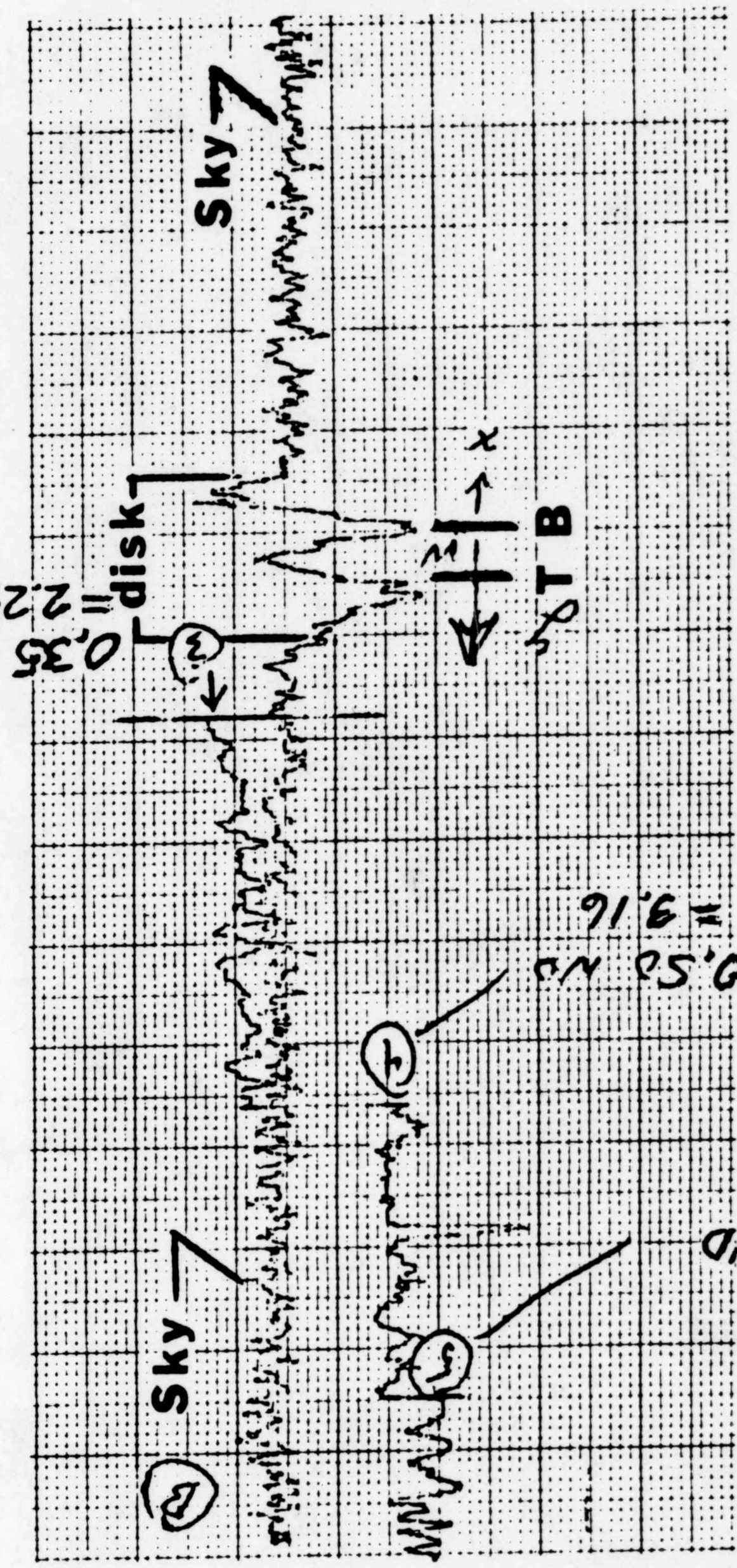


Fig. 5. Micro-densitometer vertical scan through disc image.

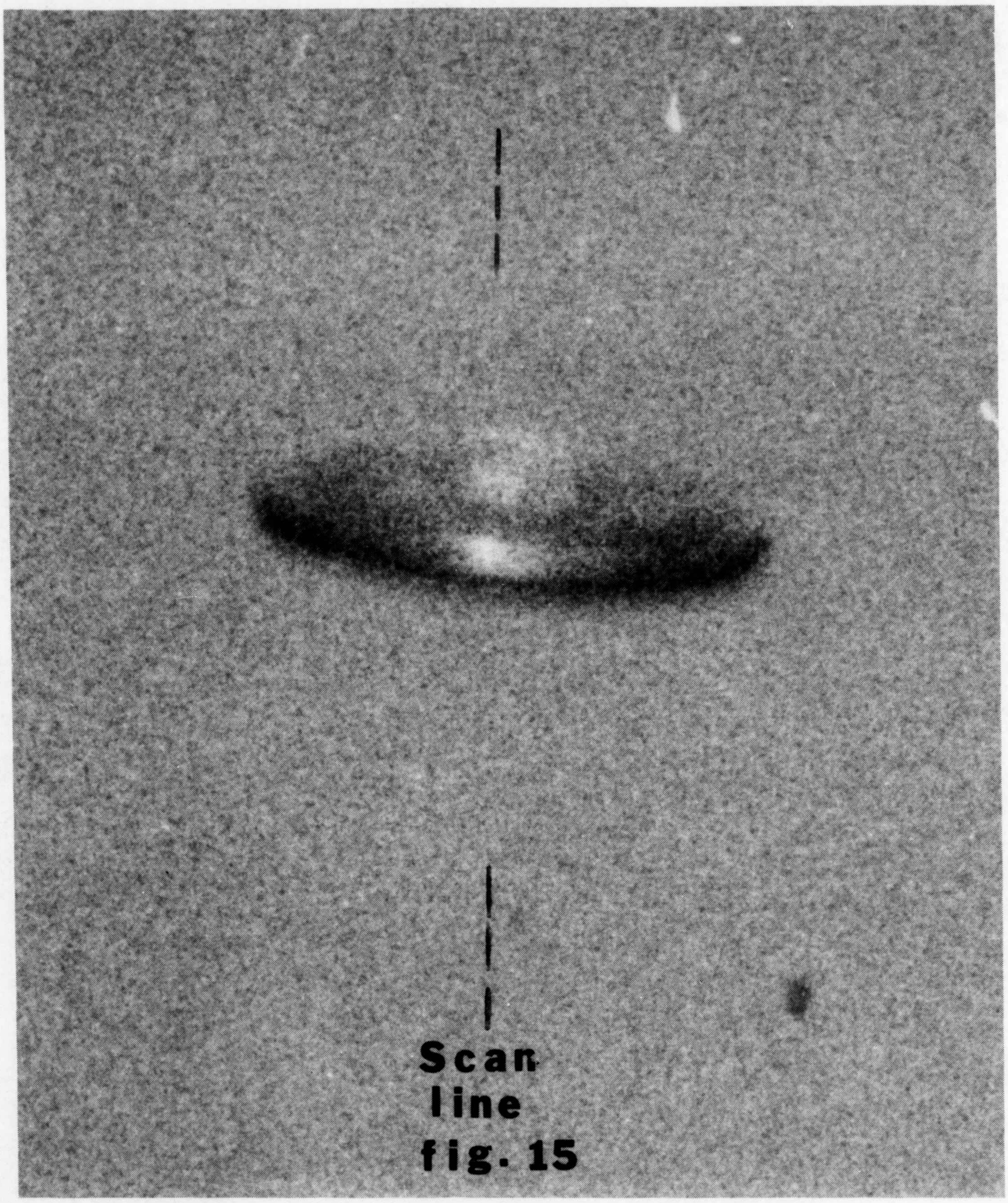
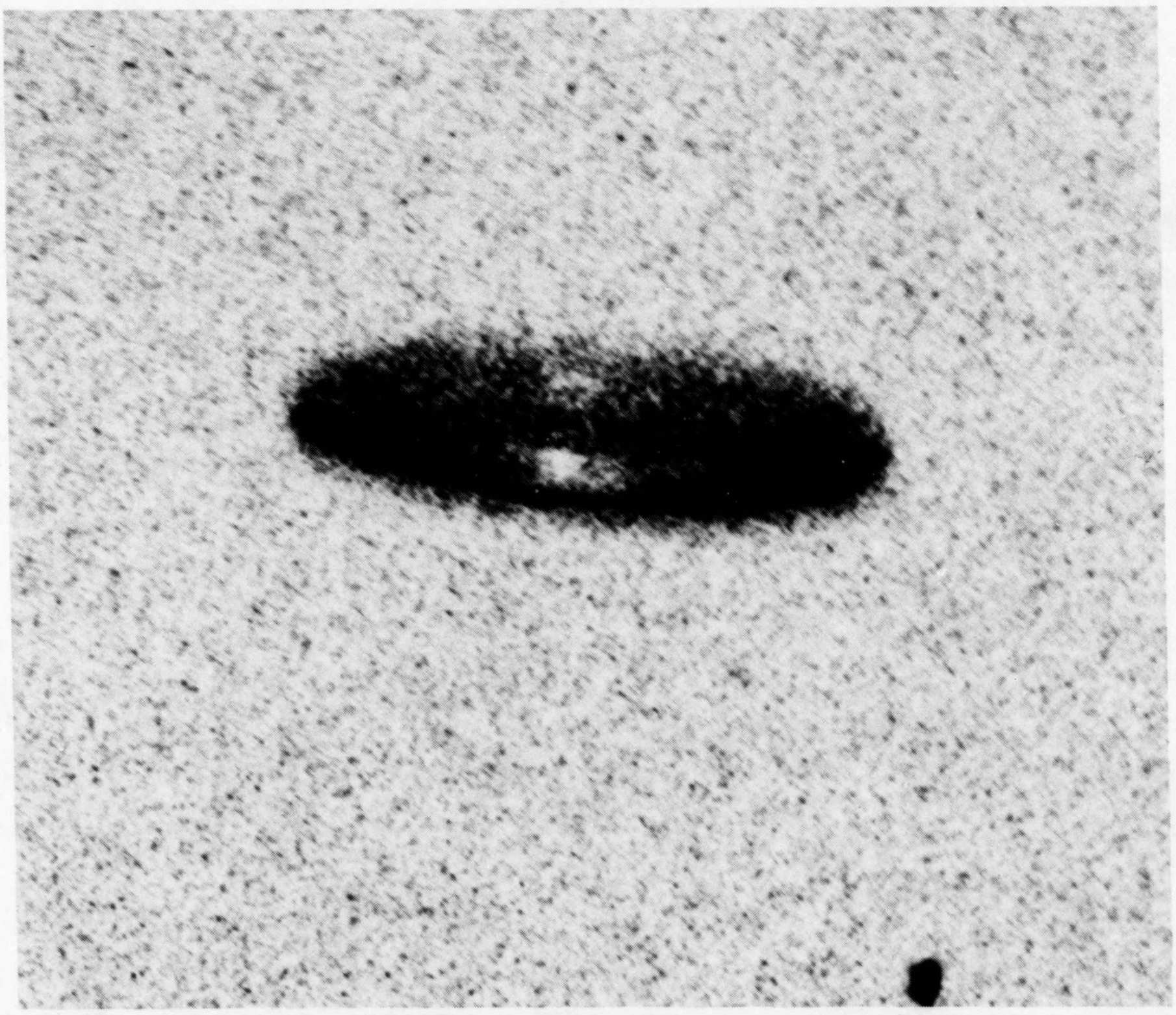


Fig. 6. Photo enlargement of disc.

Computer-Based Contrast Enhancement Results. The negative was digitized using a Perkin–Elmer Scanning Densitometer set to six microns diameter entrance aperture. Optical density was achieved to 16 bit resolution. The negative was scanned and digitized three times; each scan was made using a color filter having the approximate spectral distribution as the dye layers in the negative.[1] System output was recorded on $\frac{1}{2}$ inch magnetic tape at 1,600 bits per inch density for processing and display by a digital computer. The region studied was only slightly larger than that of the disc in order to conserve

(a)



(b)

Fig. 7. Black and white image enlargement: (a) Panchromatic paper, (b) Blue–green sensitive paper.

memory and processing time. The range of optical densities found within this image area ranged from five to 400. Since the optical densities extended only from about 200 to 400 for the disc's image, the computational range was truncated by dropping the top 8 bits. Thus, the 8 bits from zero to 255 levels of density are presented in all of the following computer color enhancements.

Figures 8 and 9 are enhancements obtained using only the blue filter scan, that is, there is almost no contribution to this image from green or red wavelengths. A very high contrast color enhancement is shown in Figure 8 using blue picture elements (pixels) on the cathode ray tube monitor to represent film densities associated with the image of the disc and orange and red pixels to represent film densities characteristic of the surrounding sky density. It must be emphasized that there is *no* particular significance to the colors seen in these computer-enhanced photographs.

Figure 9 shows a black and white enhancement using an undistorted contrast. Much of the top surface detail becomes invisible in both Figures 8 and 9. Presumably this is due to the particular range of wave-lengths that are being reflected or emitted by the disc. Both of these figures show that the sky is relatively homogeneous, the film's crystals (each possessing different sensitivity to light) are approximately random in their spatial distribution as expected. Also shown is a relatively sharply defined bottom edge of the disc relative to its upper edge. The shadow seen under the disc's lower edge in Figure 6 is barely evident here.

Figure 10 presents an enhancement made using a green filter where purples and blues are assigned to densities which predominate within the image of the disc and yellows are assigned to densities characterizing the background sky. A long vertical scratch exists to the left of the disc's image within this particular emulsion layer. Not only is the sky's density relatively homogeneous in its density but the two regions of greater brightness on the disc become much more apparent. The overall shape of the disc is symmetrical but has more pointed ends. The significance of this is unclear.

A red filter was used to generate the enhancement shown in Figure 11. As was noted in enhancements using a green filter, the dome is missing in this enhancement suggesting that the protruberance on top of the disc is reflecting or emitting wavelengths mainly in the blue-green portion of the spectrum.

Finally, a three-color composite enhancement including blue, green, and red wavelengths was made. Figure 12 presents one such enhancement to illustrate the homogeneity of the sky as well as the emulsion flaw and highlights on the image of the disc. None of the above enhancements show any evidence for a suspension line or thread above the disc.

Site Visit Results

A comprehensive site visit was made by the author on October 7–8, 1983.[2] Photographs, measurements, and general inspections were made of the entire

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[1] The film's yellow forming layer, magenta layer, and cyan layer peaked in sensitivity at 425, 545, and 650 nm, respectively. This technique permitted the information content in each layer to be analyzed separately.

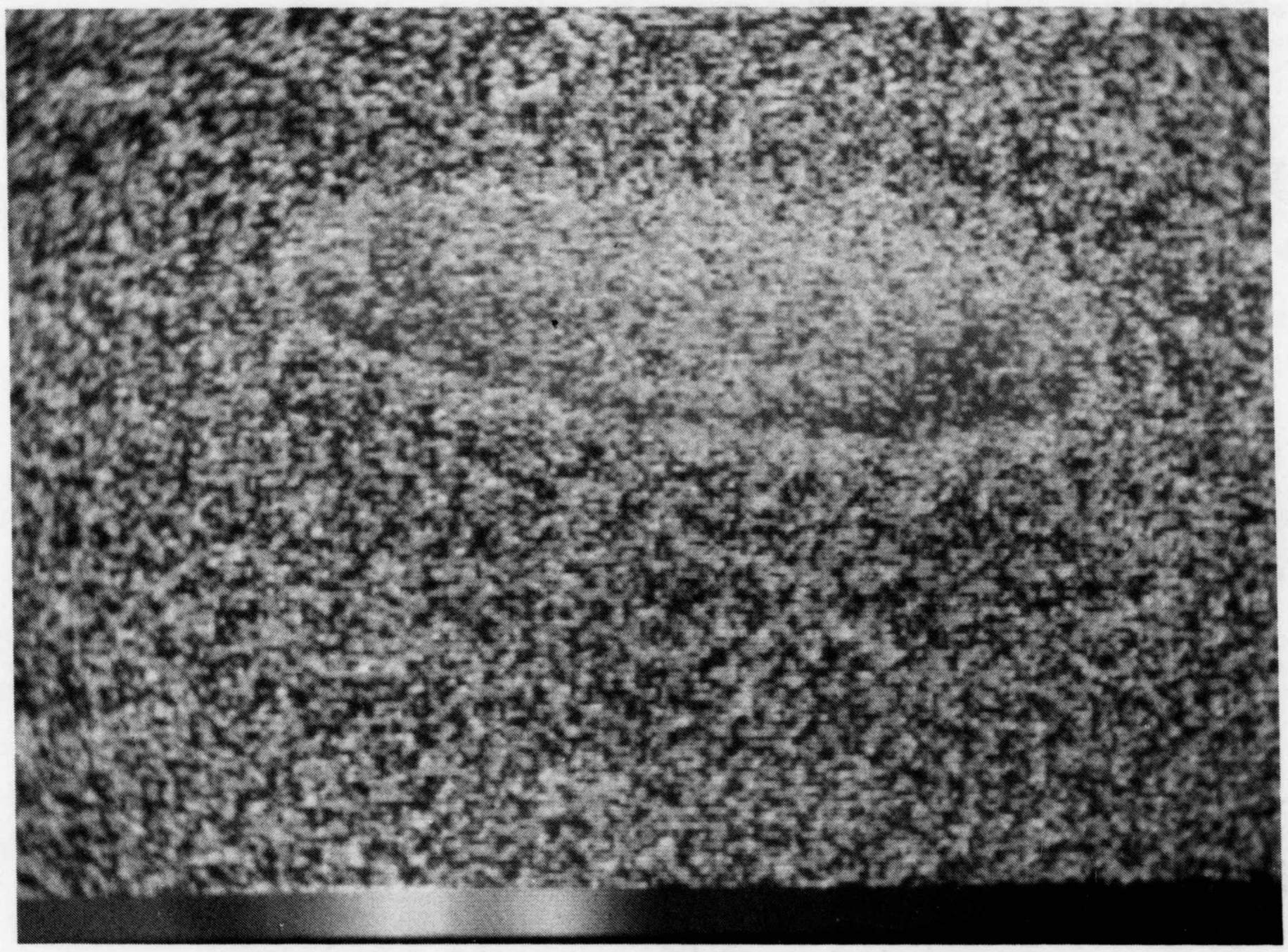

Fig. 8. Color enhancement of disc image using a blue filter.

vicinity of the Provincial park where the photograph had been taken. Figure 13 is a topographic chart of the area with the mountain darkened. This location is just east of highway 19 at the Eve River bridge, about 49 miles west northwest of Campbell River, British Columbia on Vancouver Island. Area X on this chart marks the approximate location of the photographer. This spot is located at 126°14′ West and 50°19.4′ North. The photographer and her family had stopped at this park for a rest when the photograph was taken. The rectangular area marked with a C is property operated by the McMillan–Bloedel Lumber Co. Ltd. camp which was unoccupied when the author was there. The clearing in the forested area was gravel covered with a few buildings, fuel pumps, a dynamite shack, and vehicles of various kinds. Intense white yard lights illuminated the area at night (in 1983). A registered civil engineer[3] conducted a site survey and determined that the photographer stood approximately 4,240 feet north of highway 19 (within the Provincial park) and faced 3°18′ north by west toward the peak of the mountain. The slant distance to the mountain peak was 7,580 feet for an elevation angle of 24°17′. The height of the moun-

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[2] I am greatly indebted to Donald H. Haines, my father and registered civil engineer who accompanied me and who performed the survey of the site. I am also grateful to the Fund for UFO Research, Maryland who supported a part of the travel costs to British Columbia from California.

[3] *Ibid.*

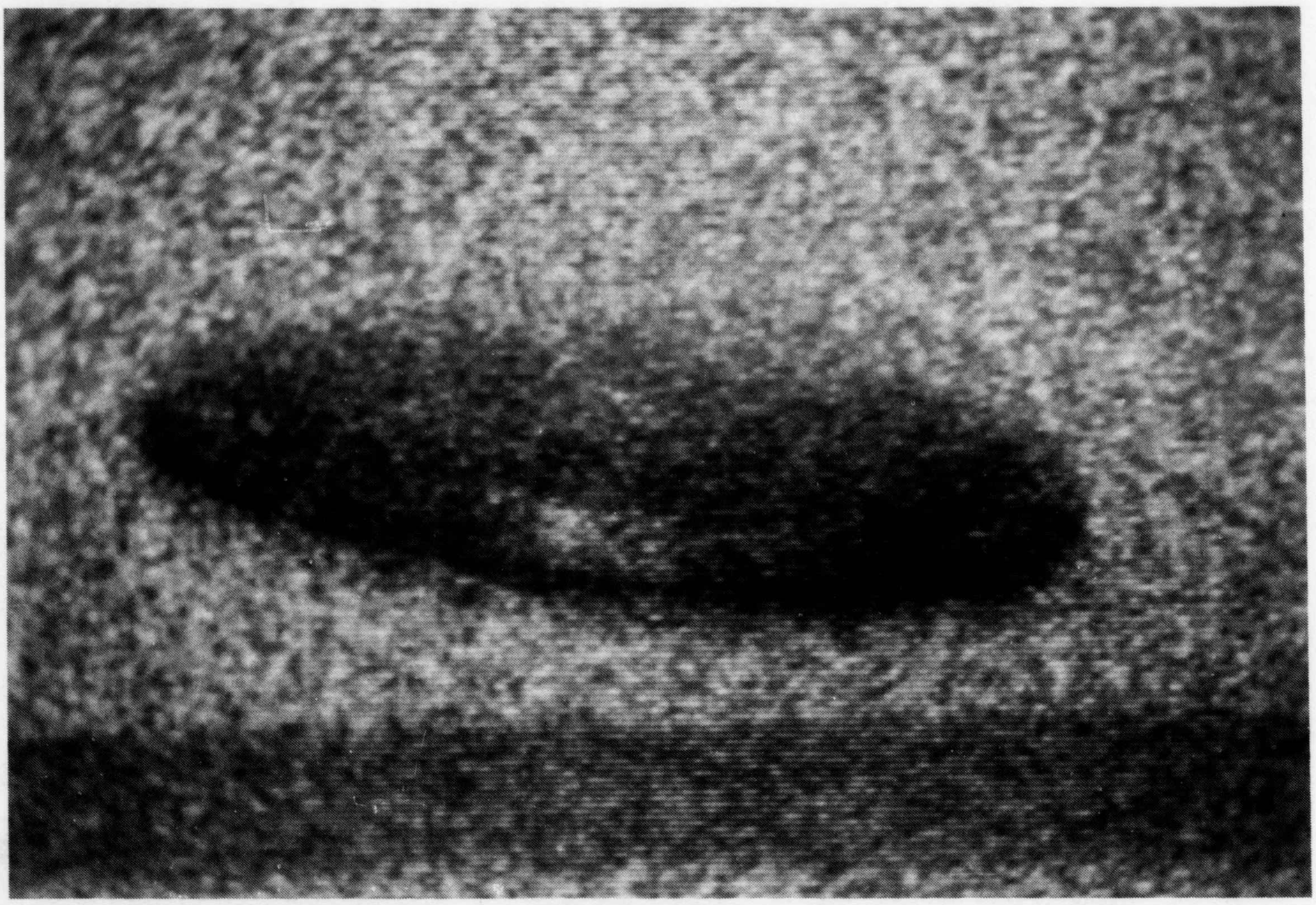

Fig. 9. Black and white enhancement with undistorted contrast.

tain from the local horizontal was 3,117 feet while the elevation of the photographer's position was 984 feet above mean sea level.

Although the Provincial park was located within an area cleared of evergreen trees, second growth timber extended from the base of the mountain almost to its top. After inspecting the site it was clear that there was sufficient flat ground to have flown a model airplane or thrown a Frisbee[4] into the air. Neither the photographer nor her husband admit to doing this. There are no buildings or stores within a radius of 15 miles of this spot. The photographer does not remember passing any vehicles on the morning she took the photo other than a few logging trucks with loads of logs.

Credibility of the Photographer

In cases such as this it is essential to establish the credibility of the persons involved. Mrs. D.M. (age 26) was the photographer. She was accompanied by her husband (age approximately 30) and their 18 month-old daughter and

[4] Both Mr. and Mrs. D.M. stated that there were no other persons at the Provincial park before, during, or while they were leaving the camp site. The large open area would have afforded a wide and unobstructed view of the surrounding terrain had there been someone else there. They also stated that no sounds were heard from the direction of the mountain while they were at the camp site.

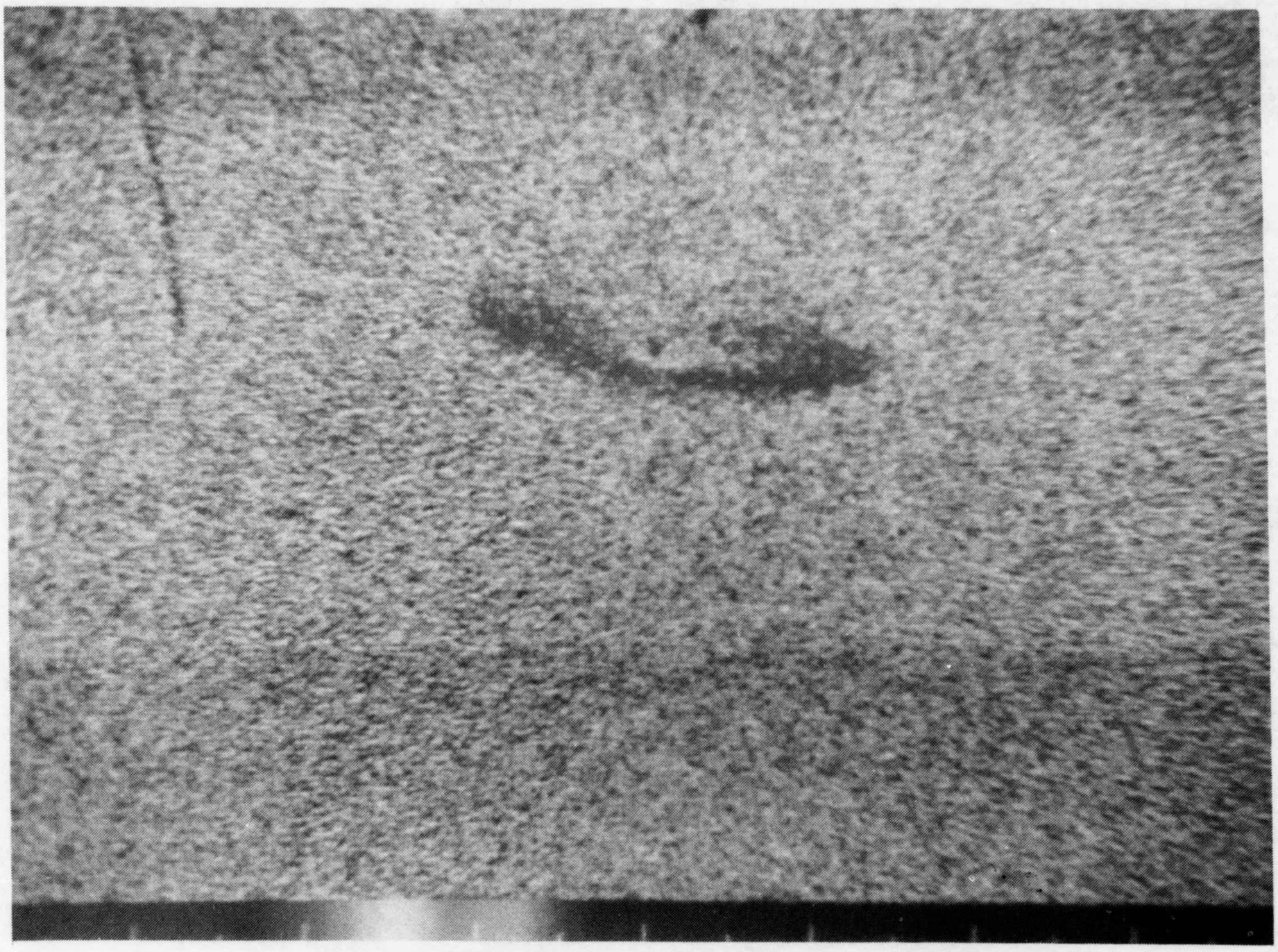

Fig. 10. Color enhancement of disc image using a green filter.

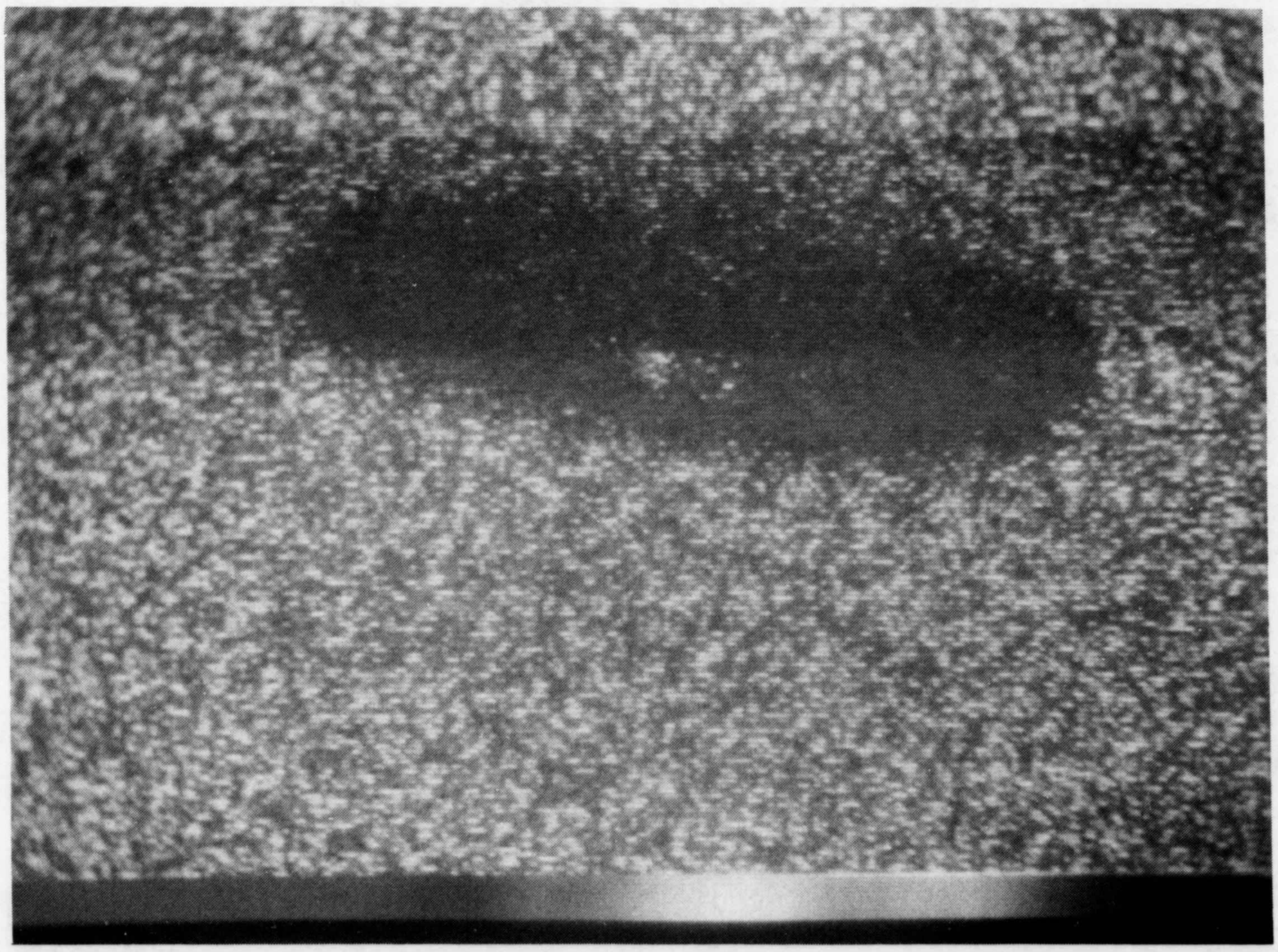

Fig. 11. Color enhancement of disc image using a red filter.

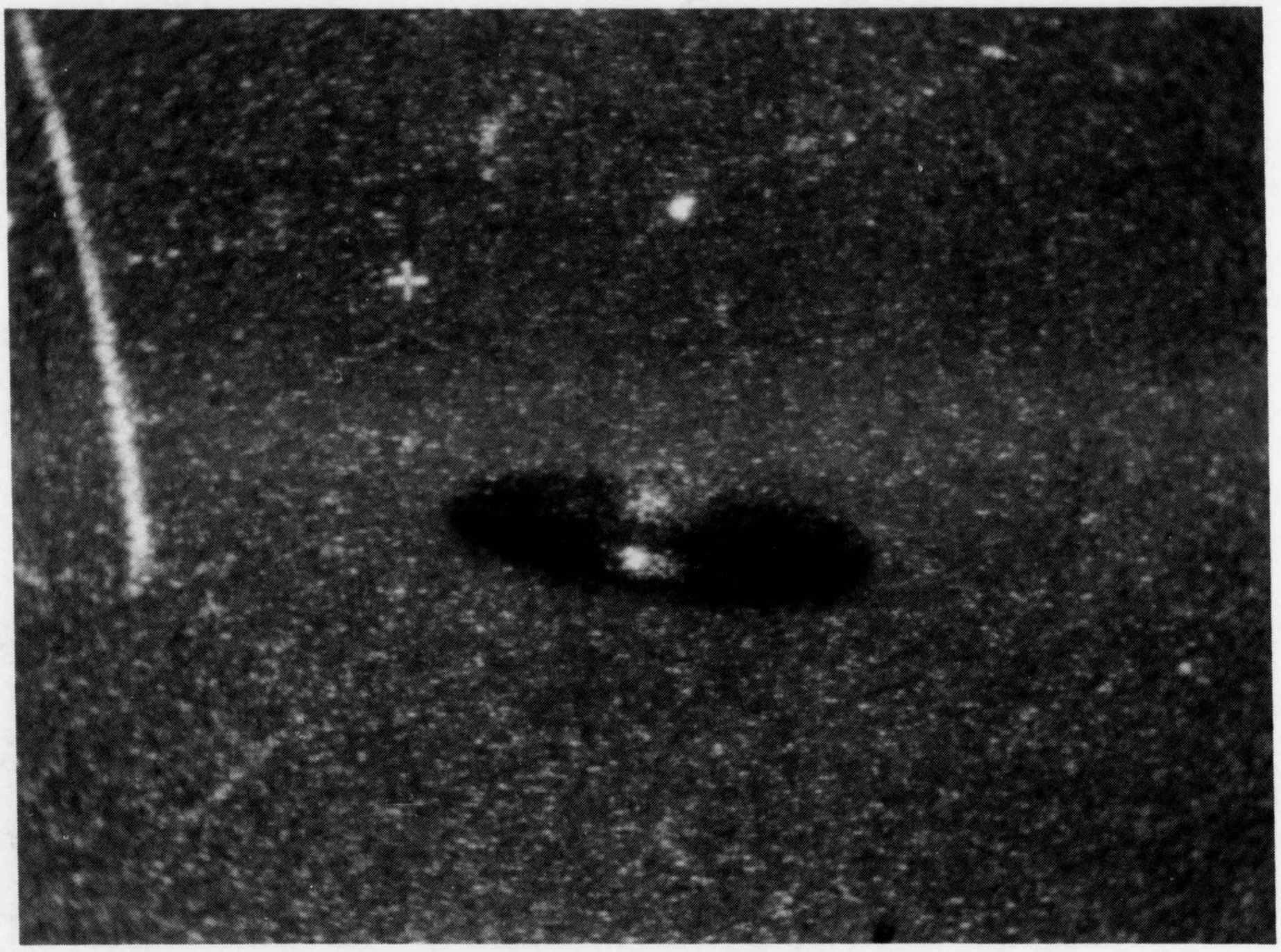

Fig. 12. Three color enhancement of disc image.

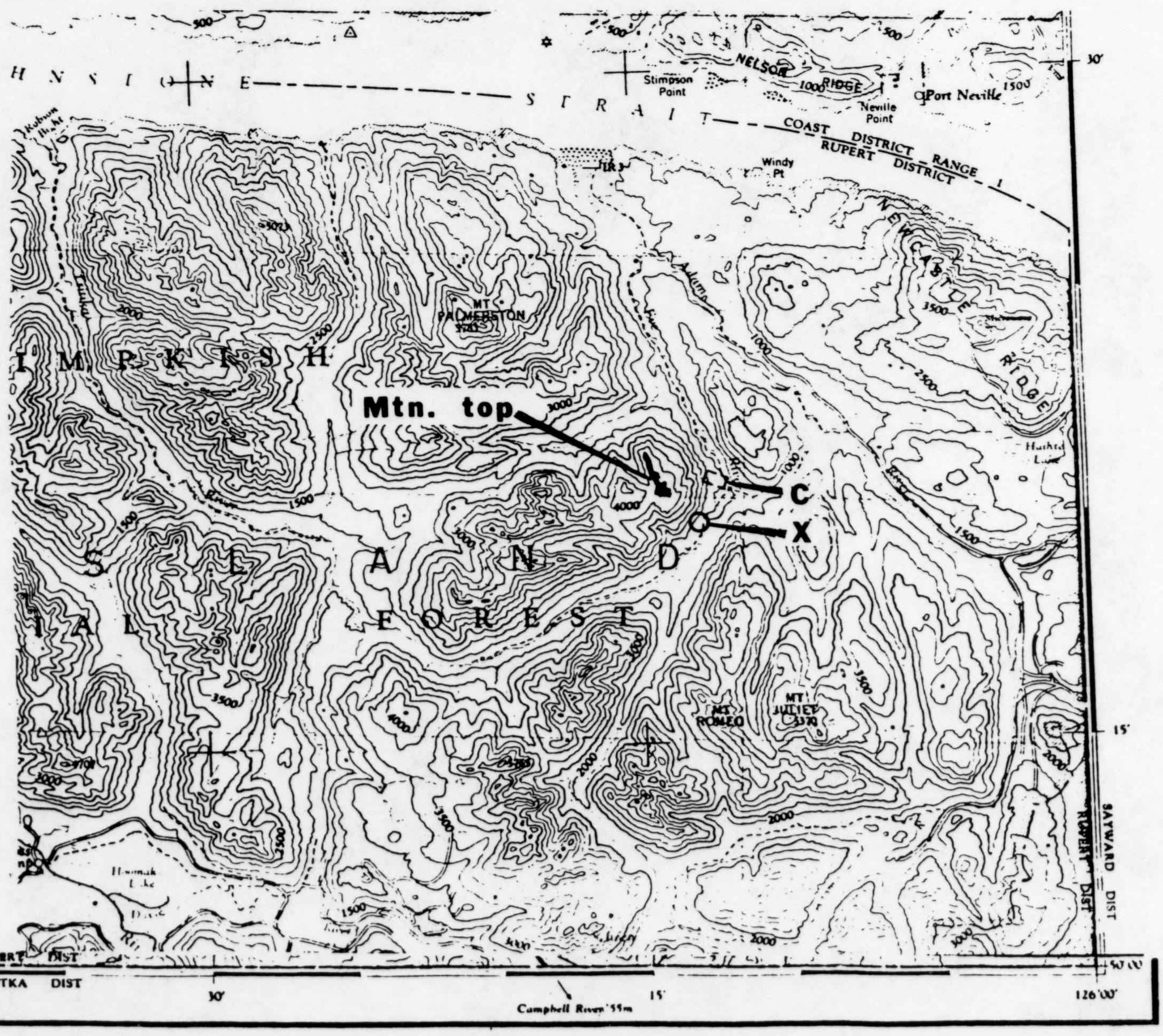

Fig. 13. Topographic chart of photographic site on Northern Vancouver Island, British Columbia.

the family dog. The family was on their way to visit her sister at Holberg, located at the northwest tip of Vancouver Island. Mrs. D.M. was an outgoing, pleasant person with a casual interest in UFOs. Inspection of their home did not indicate any interest at all in the occult, the psychic realm, or related subjects. Mr. D.M. worked at the lumber mill in Campbell River. Neither person claimed to have read any books specifically on UFOs, but had seen the movie *Close Encounters of the Third Kind.* The husband was an avid science fiction fan in earlier years.

When asked what they had done immediately after noticing the disc on the photograph (some 18 days later on October 26, 1981), Mrs. D.M. replied, "Well, we didn't know what to do. Eventually we showed it to our neighbors and Mr. and Mrs. M. Sr. (husband's parents)." Mrs. D.M. phoned the Canadian Forces Base at Comox in mid-November 1981 concerning their possible interest in seeing the photograph. She ". . . just wanted to see if they were interested in it and if they knew anything about what the object could be." An air force representative (allegedly) said they were not interested in viewing it, but did take her name and address. It was not until the summer of 1982 that the family travelled to Vancouver, B.C. bringing one 4″ × 5″ color print with them. They visited the Vancouver Planetarium and spoke with the Director, David Dodge who called in David Powell who was interested in UFO phenomena. The couple were pursuaded to lend the original negative to them to make enlarged copies. The negatives were delivered to Mr. Powell in June 1982 and were returned to Mr. and Mrs. D.M. on January 28, 1983. These dates may be significant since they suggest that the photographer was willing to wait a long time before pursuing an explanation for the disc-like image on her photograph. If this event had been a deliberate hoax it is more likely that some overt action to capitalize on it might have been taken soon after the disc had been discovered and not almost a year later. Of course this is not a conclusive argument to support this contention.[5]

The author found the photographer and her husband to be middle-class, hard-working people. Their property was well kept. Nothing could be found which pointed to a deliberate hoax. Both displayed genuine puzzlement about the origin of the disc on the photograph. Mr. and Mrs. D.M. were not defensive nor did they ever attempt to cover up anything as far as could be determined. For example, when asked if he owned a Frisbee, Mr. D.M. said yes and located it immediately for the author's inspection. It was a 9″ diameter, dull black, "Professional FIFI model." He claimed to have been proficient in throwing it in the past, but had not done so in some time. There was no indication that some type of dome-like structure had been attached to it. The suspicion lingered throughout the investigation that a Frisbee or other similar object had merely been tossed up into the air and photographed. It became important to learn more about the subject of Frisbees and their "flight" qualities.

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[5] While it is true that Mr. and Mrs. D.M. have had color enlargements made of their photograph and have sold some, this was done as a courtesy to their friends and to others who wrote asking for copies. Almost no profit has been made from the sale of these photographs.

A Review of Frisbee Characteristics

Three topics are briefly reviewed here (a) surface characteristics, (b) flight records in competition, and (c) subtended angles and related distances. The author consulted with a person[6] who had previously worked for a well known manufacturer of Frisbees. He explained the necessity of having a smoothly curved leading edge at the circumference of the disc and tiny microgrooves in the top surface in order to create a lifting force during its spinning flight. He suggested that the addition of a dome-like structure to the top would probably reduce or destroy this aerodynamic lift. The author (later) proved that this was indeed true. The author also contacted various toy stores to inspect various Frisbee models. A total of seven different models were inspected. All possessed a glossy (specular) outer surface. Most had reflectances of about 80% or less. Of the six models produced before 1981, only two had paper labels, the other four[6] had colorful embossed drawings centered across the top surface.

Men's and women's world records for throwing Frisbees were obtained from the International Frisbee disc Association (IFA). This organization has hosted tournaments which have become qualifying events for the World Frisbee Championship. It was discovered that the men's outdoor distance record is 166 m (540 feet) and the women's record is 122 m (397 feet). These records were set in 1983 and 1980, respectively and are meant only to indicate the general range of human capability for this skilled activity. The men's world record for maximum time aloft is 15.5 seconds (1981); the women's record is 11.4 seconds (1980).

The linear width of the disc's image on the negative was 0.98 mm. The width of the 36 mm frame was equivalent to a horizontal angle of 48°. The useful ratio can be formed:

$$36/48 : 0.98/X$$

where: X = the angle subtended by the disc. This angle is 1.307°. Therefore, Tan 1.307/2 = 0.0114 = (W/D)2 where: W = the assumed object width and D = the separation distance between the camera and object. Letting W = 9 inches, D = 32.88 feet which exceeds the hyperfocal distance. If the disc object was 10 or 50 feet in width it would have been 438 feet or 2,192 feet from the camera, respectively. And if the disc had been hovering directly over the mountain (i.e., 7,580 feet away) it would have been 173 feet in width.

Assuming that the camera shutter speed was 1/125th second and the disc image was produced by a typical Frisbee travelling at 10 feet per second, a 9 inch diameter disc moving normal to the line of sight would move 0.96 inches in his duration. Approximately 9.3% of the Frisbee's diameter would show up as a blur on the leading and trailing edge of the Frisbee's photographic image. There is virtually no blur visible on the photograph in question which strongly argues that the disc was not travelling normal to the line of sight. If it was motionless it would be far more difficult to perceive, particularly if the

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[6] I wish to thank Mr. Gordon Holt for his professional assistance in this phase of the analysis.

photographer was (a) looking through a camera's optics and (b) was not expecting to see anything hanging motionless in the air.

It is highly unlikely that the object photographed was a commercially available Frisbee. There are significant top surface contour differences between a Frisbee and the photographed disc. This was shown by a careful comparison of photographs of a Frisbee model with scale dome oriented the same as the photographed disc and illuminated by sunlight under the same angular conditions. The surface reflections were markedly different in each case. In addition, the presence of the tiny, concentric micro-grooves on all Frisbees would not be expected to yield a sharp contrast gradient as is seen in Figures 7(b), 8, and 9. When the author attached a light-weight dome to a Frisbee it would not fly very far nor very high. It is problematical whether another person could have achieved such a feat. The author inspected the frame immediately following the frame in question and found that it had been taken in Campbell River following the trip north. The immediately preceeding frame was also located. It showed Mr. D.M. and their daughter standing in front of a small pond at the Provincial park on the day the photograph had been taken, exactly as stated by the photographer. If someone had tossed a model up into the air in order to photograph it, only one photo was taken. It is fortuitous that such a clearly focused image was obtained on the first try, if this is what happened. Furthermore, this explanation does not stand up under scrutiny of the author's in-depth interviews and site visit. The fact that the photographer stated that she was taking a photograph of the mountain (and not of a UFO disc or model) is further supported by the fact that the top of the mountain was well centered in the photograph. The object was not centered. The lack of any image blur suggests that the disc was nearly motionless which would make it more difficult to see, other factors equal.

In summary, this investigation has shown that a mature adult with high credibility and little or no interest in UFO phenomena obtained a single, colored, sharp imaged photograph of an unidentified aerial disc-like object. Her subsequent reactions to seeing the disc's image on her photograph produced surprise and dismay as well as the normal array of "answer-seeking" behavior. She has not capitalized on having such a photo[7] and still acts somewhat embarrassed at having taken it without seeing the disc. The disc's identity has not been identified to date.

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[7] See footnote 5.

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Analysis and Discussion of the Images of a Cluster of Periodically Flashing Lights Filmed Off the Coast of New Zealand

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Abstract—The New Zealand UFO sightings of December 31, 1978 are unique among civilian UFO reports because there is a large amount of the documentary evidence which includes the recollections of seven witnesses, two tape recordings made during the sightings, the detection of some unusual ground and airplane radar targets and a 16 mm color movie made with a professional camera. Of the several unidentified light sources that were filmed, one of the more interesting is the cluster of lights that oscillated periodically in intensity at a rate of about once per second. An analysis of the 279 frames of film which show about 30 cycles of the oscillation indicates that there were three lights which formed an isosceles triangle. The color of the light source at the apex was pale yellow or very pale orange (the exact shade is difficult to determine). The base of the triangle was formed by two red lights, side by side. The light at the apex oscillated over an intensity range which went from zero (no image) to such a large value that it greatly overexposed the film. The red lights also oscillated, but they were generally out of phase with the light at the apex and they did not get bright enough to overexpose the film.

This paper presents some of the physical characteristics of the film images and a discussion of the visual and radar sighting, which, it is argued, took place at essentially the same time (i.e., within a minute or so) as the filming.

A number of explanations have been suggested for the film of the flashing light. The explanations are analyzed and reasons for rejecting them are presented. As yet no explanation in terms of known phenomena has been proposed that satisfactorily explains the film.

I. Introduction

A number of unusual lights were seen and filmed during a series of UFO sightings that took place off the coast of the South Island of New Zealand during the early morning hours of December 31, 1978 (Fogarty, 1982; Ireland, 1979; Maccabee, 1981; Startup & Illingworth, 1980). The witnesses were aboard an Argosy freighter aircraft and were flying east of the coast of the South Island. The first sightings occurred between 0010 hours (12:10 a.m.) local (Daylight Savings) time and 0100 hours (1:00 a.m.) while the plane flew southward from Wellington to Christchurch. More sightings occurred between 0218 and 0315 hours while the plane flew northward to Blenheim. The sighting that is the subject of this paper occurred roughly between 0245 and 0255 (exact times will be given later in the paper). The witnesses on the airplane

were the pilot (William Startup), the copilot (Robert Guard), two TV news reporters (Quentin Fogarty from Melbourne, Australia, and Dennis Grant from Christchurch, N.Z.), and a professional TV cameraman (David Crockett). An unusual radar target was detected by the Wellington Air Traffic Control (WATCC) search radar in the apparent vicinity of the visually sighted cluster of lights. The witness to the radar sighting was the air traffic controller (Geoffrey Causer).

These sightings are unique in the annals of civilian UFO sightings for two basic reasons. The first reason is that there is a large amount of available information, including tape recordings and film made during the sightings. The second reason is that the sightings were immediately publicized worldwide by major news media. They were publicized because several of the witnesses were a news crew of a major Australian TV station. This section of the paper first presents a very brief history of the circumstances which led to the unusually large amount of information and publicity and then summarizes the results of the technical analysis.

Quentin Fogarty, a reporter for a TV station (formerly Channel 0, now Channel 10) in Melbourne, Australia, was responsible for the presence of a news crew on board the plane. He intended to obtain background film footage for a news story about previous visual and radar UFO sightings that had occurred off the East Coast of New Zealand about two weeks earlier. (The witnesses had been pilots who were flying Argosy freighter aircraft and air traffic controllers at Wellington.) Not expecting to see anything unusual himself, Fogarty planned to have most of the background filming done in the cargo bay of the aircraft. This background film would show him talking about the previous sightings. However, during the early part of the flight the air crew observed a number of unusual lights that they could not identify. Coincidentally, the WATCC reported a number of unexpected radar returns that were correlated in time and direction (from the plane) with the unusual lights. Because of these sightings the captain suggested that the news crew leave the cargo bay and come into the cockpit. The crew then pointed out the unusual lights and Fogarty recorded his impressions of the sightings and radar reports as they occurred. Because the air crew could not identify the unusual lights (although they did identify for the news crew numerous town and city lights, stars, coastal beacons, and ship lights) Fogarty subsequently made the public claim that the news crew had seen and filmed UFOs.

The Melbourne TV station publicized the sightings, claiming that Fogarty had the world's first film of UFOs. The film was the highlight of a short, hastily produced documentary that was shown completely or in part around the world. Even Walter Cronkite was impressed. He devoted five minutes of the CBS Evening News to the sightings (January 2, 1979).

In spite of the fact that no investigation had yet been carried out and that the only information generally available was contained in newspaper stories, scientists throughout the world were quick to offer explanations. Within a week of the sightings newspapers had published explanations proposed by

"experts" from many countries. These explanations included the planet Venus, the planet Jupiter, drug runners, secret military activities, "unburned meteorites," mirages or reflections of squid fleet lights, ball lightning, earthquake lights, swarms of glowing bugs and light reflected from flocks of birds. It was also suggested that the film was a hoax by Fogarty and the cameraman or that the whole event was an attempt by the TV station to improve its ratings in the Melbourne area.

There were hundreds of small, bright, well-focused images for the producer of the documentary to choose from. However, in order to create the greatest visual impact, the producer decided to emphasize the largest images, which are round and have horizontal lines going through them. Much later it was shown that these are defocused images. However, at the time they were thought to accurately represent the structure of the unknown light source. Newspapers all over the world published several of these peculiar images. One astronomer publically identified the lines as the rings of Jupiter, in spite of the fact that the film was taken on board a vibrating airplane with lens of only 240 mm focal length. (The actual images are about 2 mm in diameter. It would require a lens with about 10 times the focal length and a stable platform to get images as large as these which show the rings of Jupiter.) In one newspaper story these images were also compared with images of Venus photographed (by a spacecraft) from a distance of only several hundred thousand miles, images which showed for the first time banding structure of the Venusian clouds. This comparison was completely ludicrous since no earth telescope had showed such structure. (It was subsequently shown that the horizontal lines across the defocused images were caused by refractive effects due to non-uniformities in the airplane window glass.)

To add to the public confusion surrounding the December 31 sightings, a second film was obtained from the coast of New Zealand on January 2, 1979. The film was obtained by a N.Z. TV news crew that resented the "scoop" by the Australian TV station. The N.Z. crew rented the largest lens available (600 mm focal length), set up its camera on the coast and waited for the UFOs to appear. Subsequently a bright light was observed on the horizon in the east and the crew filmed it. The film showed a large round image with a black dot in the center, similar to a donut shape, but with no horizontal lines. Needless to say the TV crew claimed it had filmed a UFO, and this second film was also discussed throughout the world. However, an analysis of the sighting showed that the light was Venus rising and that the unusually large round images were a result of the failure of the cameraman to correctly focus the large lens.

The proliferation of explanations for the December 31 sightings embarrassed the Melbourne TV station managers because they had publicized their UFO claim without carrying out an investigation. They had based their claim only upon the reporter's opinion that he had seen UFOs. Therefore the management publicly promised to investigate the sightings. The station subsequently contacted a UFO investigating group in the USA which then contacted this author.

I investigated the sightings by traveling to New Zealand and Australia to interview all the witnesses, by analyzing the film, and by discussing the sightings with numerous other scientists.

The main reason for the uniqueness of these sightings is the amount of information that is available for analysis. The information that is available for most other sightings is only that which is extracted from the memories of the witness(es). A relatively small fraction of all other sightings involve photographs or "landing traces" and a few have associated radar contacts ("radar-visual" sightings). However, there is no sighting (by civilians, at least), other than the N.Z. sightings, which has (a) two independent tape recordings made at the time of the events, (b) reports of unusual ground-based (search) and airborne (weather) radar targets that were coincident with visual sightings, (c) a color movie (16 mm professional camera and film), as well as (d) the memories of a sizeable number of credible witnesses (five).

The unusual amount of publicity that attended these sightings and the large amount of information which was recorded on tape and film during the sightings justifies a careful analysis of the events to determine just what did happen. This paper reports on the results of the analysis of one portion of the sightings. The information contained in this paper is based on a study, carried out over the last eight years, of the film and the testimony of the witnesses.

The flight of the aircraft, and the corresponding sightings and film, can be roughly divided into three parts: the flight from Wellington to Christchurch, the flight northeast from Christchurch, and, finally, the flight north from Kaikoura East toward Cape Campbell, which is illustrated in Figure 1. The film shot during the second part, the flight northeast from Christchurch, includes the large defocused images which were shown worldwide. However, it is the third part which is the subject of this paper. It shows a cluster of lights which oscillated regularly in intensity during the 27.9 second section of the film. Although it is known that the film was shot between 0230 and 0315, the exact time of the filming cannot be determined independently of other events since the camera was not synchronized with Fogarty's tape recorder. However, a reconstruction of the sighting events using the two tape recordings (one made by Quentin Fogarty on the plane, and one made by the WATCC) strongly suggests that the plane was at the location marked "radar-visual (0251)" on the map (Fig. 1) when the film was shot. When it was at that location the copilot, Robert Guard, reported to WATCC that he saw a "collection of lights" suddenly appear ahead of the plane. Quentin Fogarty, recorded a description of several lights which suddenly appeared and flashed a number of times. Because the tape recordings prove that other occupants of the plane saw flashing lights ahead of the plane, because Crockett was also watching the skies ahead of the plane for unusual lights, and because Fogarty's real time description contains features (the colors and the number of lights) which match features of the film images, it is herein argued that Crockett filmed lights that were described by Fogarty and Guard. Thus it is argued that the oscillating lights were filmed within a minute or so of 0251.

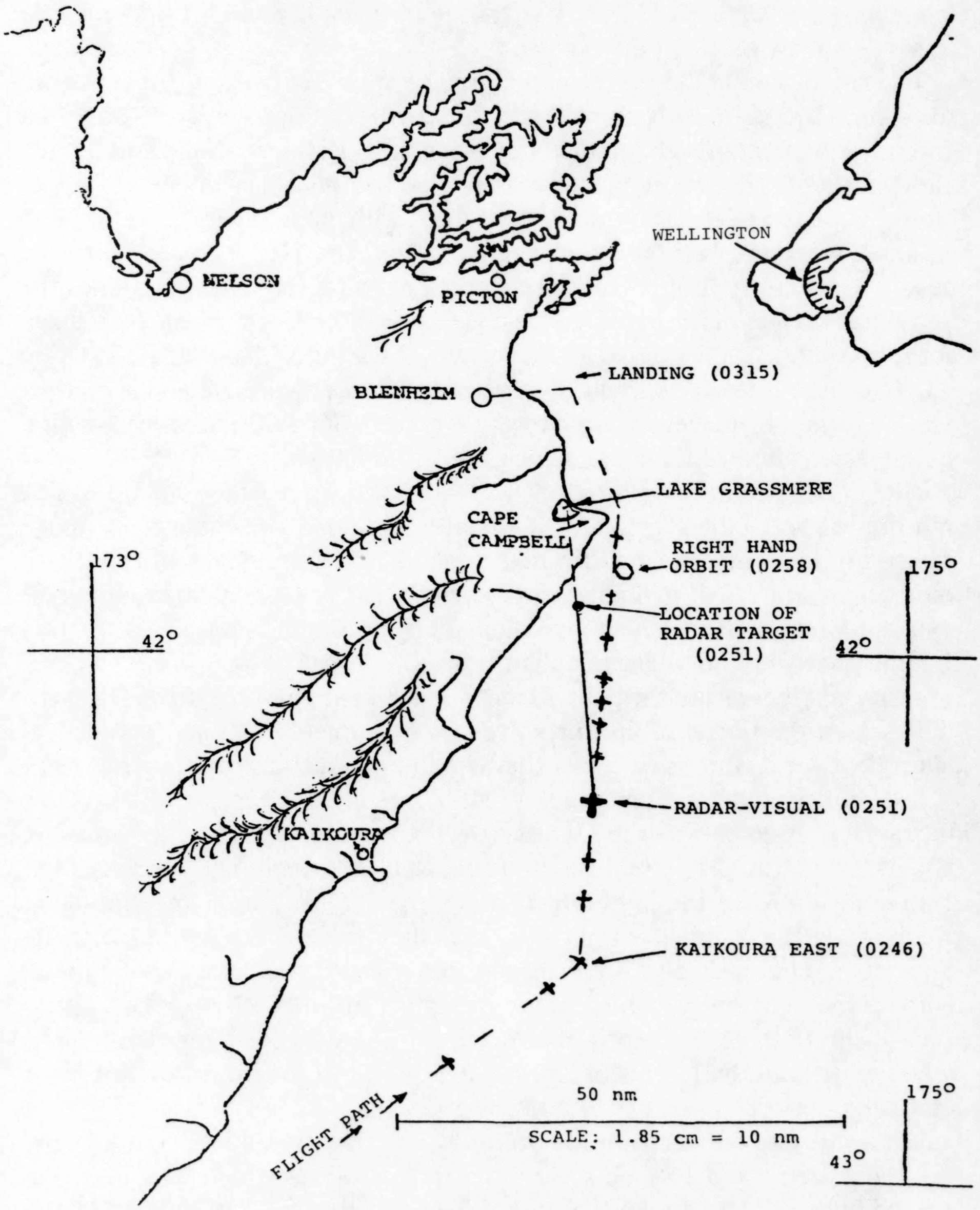

Fig. 1. Flight path from Kaikoura East to Cape Campbell.

A frame-by-frame analysis of the film shows that the images of the lights oscillate in size and color. The oscillation rate is about 1.16 cycles/sec for the full 32 cycles that were recorded. The images vary from overexposed "white" (transparent film base) with a yellowish tinge to dimmer combinations of pale yellow/orange (PY/O) and red (R), or just to red. Although all of the R images were made by a source which definitely was red, the PY/O images and the

overexposed images could have been made by a single oscillating source that was pale yellow or very pale orange.

The camera was held on the cameraman's shoulder because there was no room for a tripod in the cockpit. Therefore it vibrated randomly in horizontal and vertical directions about an average position. This vibration caused most of the images to become elongated making them elliptical or "hot dog" shaped. However, a considerable number of frames with images (about 10%) were obtained when the camera was not moving, for example, at or near the time when the vibratory motion reversed direction. In several of these frames the individual images of the lights are arranged in a triangle, with the PY/O image at the apex and two R images at the corners of the base. The triangle baseline is horizontal or nearly so in all but one of the frames with triangular images. The PY/O and R images are close together (within one milliradian) suggesting a close association of the lights which made the images.

The image oscillation referred to above actually is a periodic change in size and brightness of the PY/O and R images. Although the changes in image size could, in principle, be attributed to actual changes in size of the PY/O and R light sources, it is argued in this paper that the actual sizes remained constant while the intensities oscillated. This argument is based on the fact that the photographic image of a light source increases in size with increasing intensity whether or not the light is large enough in angular size to be resolved. (The size of the image of an unresolved light source—a "point" source—is determined by its intensity, a fact that is well known to astrophotographers.)

A careful study has been made of the "transition images" which occur during each cycle between the largest (overexposed) images and the smallest (R and PY/O) images. The study indicates that there were no changes in the relative positions of the light which made the PY/O images and the lights which made the R images, indicating that they maintained a triangular arrangement. The study also indicates that the overexposed images were made by the same light source which made the much dimmer PY/O image at the apex of the triangle. Evidently the intensity of that light changed by many orders of magnitude (factors of ten, not astronomical magnitudes) during an oscillation cycle.

Red images do not occur in the frames with overexposed PY/O images. In fact, there are no red colored areas at all in the frames which contain overexposed images. This could be because either or both of the following occurred: (a) the R lights dimmed and went out as the PY/O light increased in intensity (there is some evidence for this), or (b) as the PY/O light intensity increased its image grew so big and overexposed that it "covered up" the much weaker red images, or (c) a combination of (a) and (b).

The red lights also oscillated in intensity, and occasionally they "went out" at the same time that the PY/O light was "out", thus creating film frames with no images. About nine percent of the frames have no image.

This portion of the film, showing the oscillating lights, was not publicized by the Melbourne TV station because Fogarty decided, without talking to the

cameraman, that the cameraman had photographed a beacon. (The cameraman subsequently told me that he had not photographed a beacon.[1]) Consequently none of the publicized explanations were intended to apply to this section of the film. However, I and others whom I have contacted have proposed, in private communications with me and in books, eight different hypotheses to explain the film. The hypotheses and their originators are: (1) a chance alignment of ground navigation beacons (Maccabee; referee of this paper); (2) a reflection of light from within the aircraft cockpit (Maccabee); (3) lights on another aircraft (Maccabee); (4) a specific marine beacon in the entrance to Wellington Harbor (Ireland, 1979); (5) a bright non-astronomical source on the horizon affected by atmospheric propagation (Rackham, personal communication, 1980); (6) earthquake lights (Brady; see Pye, 1981); (7) an emergency vehicle on the ground (Maccabee; Sheaffer, 1981); and (8) the reflection off a propellor of the red flashing beacon on the top of the aircraft (Klass, 1983). These hypotheses are discussed in detail in a later section of this paper. Each of these hypotheses has logical consequences which have been analyzed and found to contradict the photographic evidence. Therefore it is argued here that the nature of the filmed lights has not been explained. It is also argued that the visual and radar sightings have anomalous characteristics that have not been explained. Furthermore it seems unlikely that conventional explanations that are consistent with the photographic data and testimony can be found. If this turns out to be true, then the lights filmed here can be logically called a TRue UFO (TRUFO): a phenomenon for which no conventional explanation exists. The film images and witness testimony can then be used to ascertain some of the physical properties of the TRUFO.

The following sections of the paper present technical characteristics of the lights based on the analysis of the film, supplementary information provided by the witnesses, discussions of the suggested explanations, and a discussion of the implications of this UFO sighting.

II. The Nature of the Oscillation

The periodicity of the overall image brightness is illustrated in Figures 2 and 3. Figure 2 shows that there is a linear relation between the cycle number and the number of the frame in which the image size is maximum. It will be shown later in this paper that the maximum image size corresponds to the maximum light source intensity in each cycle, assuming that the light source itself did not change its physical size. Thus the straight line means that the number of frames between the maximum intensity point in each cycle is constant. The slope of the straight line corresponds to an average of 8.62

[1] Information about the sightings and the camera operation was obtained during several interviews with David Crockett. This author personally inspected the camera and lenses during a visit to New Zealand approximately five weeks after the sighting. Further technical information was provided by Dr. Richard Haines.

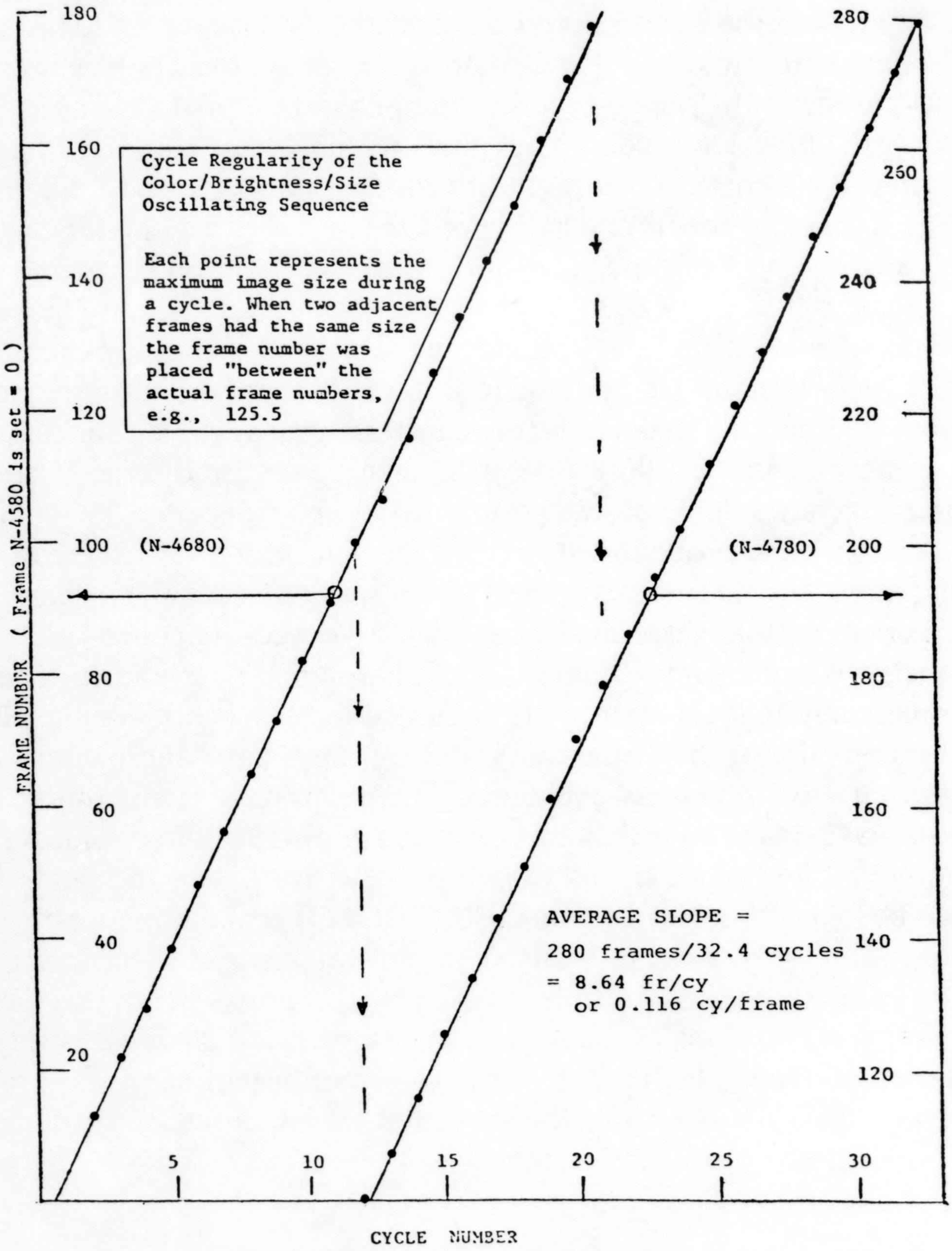

Fig. 2. New Zealand film of December 31, 1978,

frames/cycle or 0.116 cycle/frame. The Bolex H10 EBM electric camera was operated at the 10 frame/sec setting, which should have been accurate to within 10%.[2] Thus the nominal flash rate was 1.16 cycle/sec.

Figure 3 is a more direct illustration of the periodicity and also of the intensity variation of the lights, assuming that the image size variations were largely caused by intensity variations. Figure 3 is a graph of the image widths from frame to frame without regard to the color(s) of the image(s) in each frame. The image widths were plotted, rather than the maximum image dimensions or mean dimensions, in order to minimize the effect on these mea-

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[2] *Ibid.*

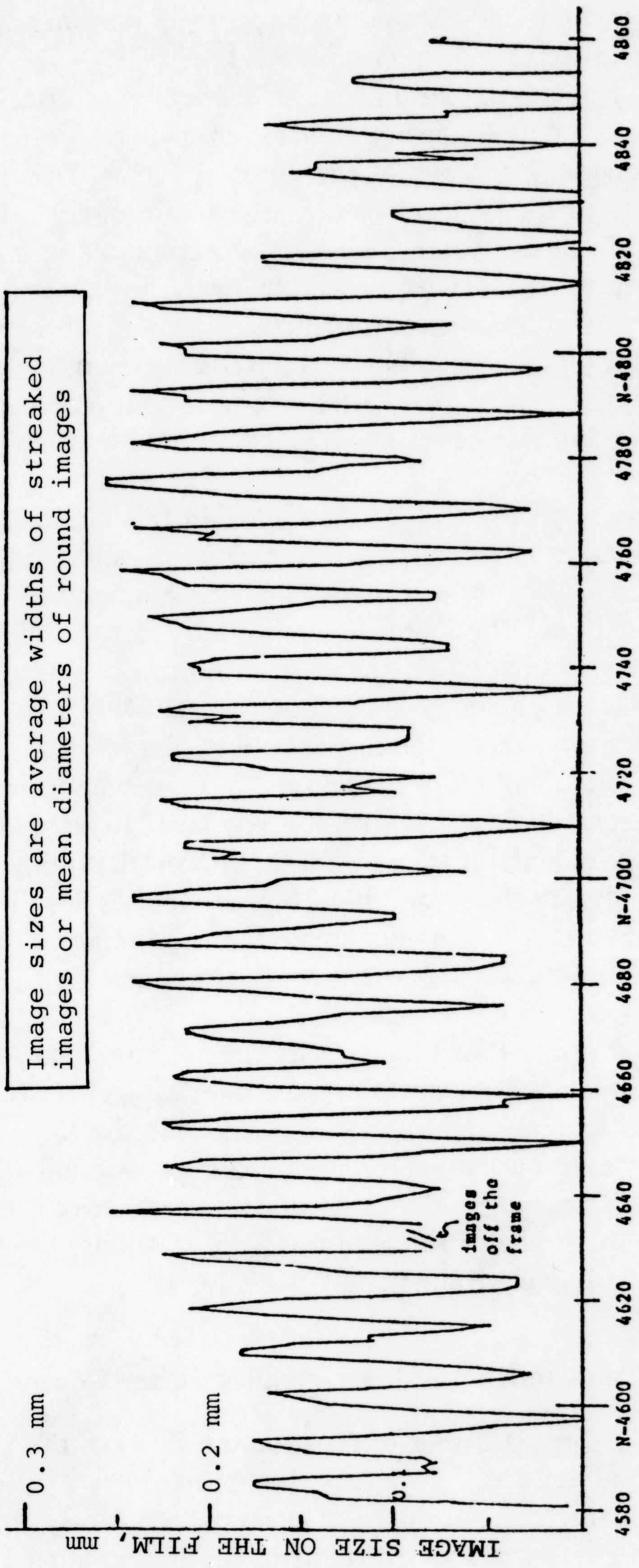

Fig. 3. Temporal dependence of the image sizes during the oscillating light sequence in the film.

surements of random camera vibration (which occurred because the cameraman held the camera on his shoulder). (The width of an image, measured transverse to the direction of the instantaneous camera motion or image elongation, is largely unaffected by camera motion.)

Figure 3 shows that the maximum image width changed during the film sequence. There was a definite overall increase during the first several cycles and a definite decrease at the end of the series of cycles. The increase and decrease in maximum image width is consistent with either (a) a decrease and then an increase in the distance between the camera and a light source of constant maximum intensity in each cycle, or (b) an increase and then a decrease in the maximum intrinsic intensity of a light source which is at constant distance, or (c) changes in the optical transmission of the atmosphere, or (d) a combination of these. (Changes in the camera optics could also produce such an effect but there is no evidence to suggest that any such changes occurred.)

Figure 4 is a close-up of several of the cycles. In this figure the widths of the R and PY/O images have been treated separately, rather than in combination as in Figure 3, in order to illustrate the phase difference between the R and PY/O oscillations. The R images very often reached their maximum widths when the PY/O images were at their minimum size or zero (no PY/O light). However, a careful study of the graph will show that the R and PY/O light sources were not exactly 180 degrees out of phase since very often the R image size reached a maximum before the PY/O image size reached zero. Moreover, there seems to have been some irregularity in the red oscillation since in 10 of the cycles both the R and PY/O lights were at minimum intensity (no image on the film) at the same time. In nine other cycles the minimum intensity frame contained only a very small, faint red image. Some of these features of the red and PY/O oscillations are illustrated in Figure 4. (Only 23 of the 32 cycles are shown for illustration.)

Figure 4 shows that the PY/O light cycle was not completely symmetric: the rise to a peak intensity (maximum image size) was generally faster than the subsequent fall to zero intensity. Occasionally the rise to peak intensity took only two frames but usually it took three, and occasionally four frames. On the other hand, the fall to zero intensity from the peak generally took four or five frames, although in several instances it required only three, or even two frames. This effect is illustrated in Figure 4.

III. Characteristics of the Triangular Image Cluster

The Appendix presents tracings of most (about 73%) of the images. The boundaries between regions of decidedly different color within each image are indicated in the tracings, but the tracings do not accurately reproduce the amount of color structure that is apparent in the movie film. The tracings also accurately depict the shapes of the outer boundaries of the images. However, the relative positions of the tracings on the pages in the Appendix are

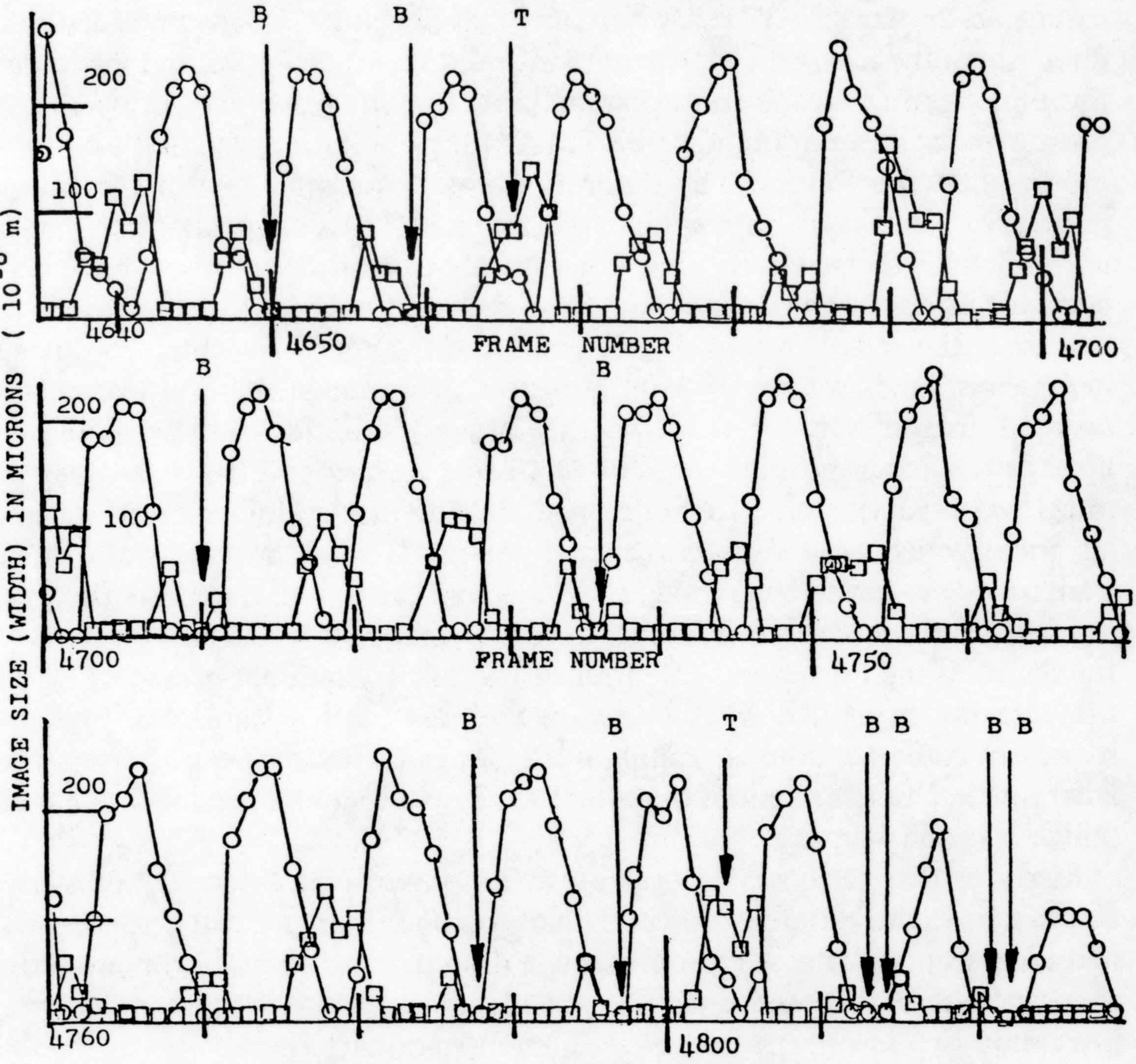

Fig. 4. Size of the red and PY/O film images. The size of the smeared or streaked image is its width.

not related to the relative positions of the images as they occur within the film frames. The introduction to the Appendix describes how the tracings were made, the magnification factor that was used, and it also defines the notations used to describe the image colors. The reader should refer to the Appendix at this time in order to understand the following discussion of specific images.

When viewing the film one sees that the location of the image usually shifts, sometimes by a rather large amount (several millimeters), from frame to frame. The shift from frame to frame is attributed to random camera motion that occurred because the cameraman held the camera on his shoulder. The camera generally vibrated randomly up, down, left and right over an angular range of several degrees. Whenever the shutter was open to create a frame of the film the camera motion during that time caused the true image shape to be

elongated or "stretched" by an amount that depended upon the speed and direction of the motion. Furthermore the random vibration caused the image position (relative to the frame boundaries) to shift from one frame to the next, with the amount of image shift depending upon the speed of the camera motion between frames. The random vibration caused most of the image positions to shift from one frame to the next and it caused most of the images to be elongated. However, any vibration about a mean position has short periods of time during which there is no motion (analogous to the pauses in motion at the ends of the swing of a pendulum). The images obtained during these short times were only slightly distorted or elongated. The minimally distorted images were located using the following criteria: (a) there should be little or no image position shift in successive frames and (b) in a series of images with varying amounts of elongation, the minimally distorted images are those which are the most compact. About 10% of the total number of frames were found to have images that are consistent with these criteria. The frames which contain minimally distorted images are called "stationary frames" and the images in these frames are called "stationary images" since they are the most like what one would obtain with a stationary (tripod-mounted) camera. Although many of the elongated images are important for a complete understanding of the film, the key photographic results are based on the stationary images.

Many of the stationary images are overexposed and a few are properly exposed or slightly underexposed. The overexposed images will be discussed in a later section. This section presents a discussion of some of the properly exposed or underexposed images in the stationary frames. It also includes a discussion of a few of the images in the non-stationary frames.

Of particular interest are each of the stationary frames which contains three small round "dot" images arranged in a triangular cluster. These frames are numbered 4666, 4752, 4804, 4806, and 4838 in the numbering scheme of the Appendix. Non-stationary frames which are also worthy of study are those which contain a pale yellow-orange (PY/O) elongated image that lies parallel to and above a similarly elongated red (R) image. These frame numbers are 4613, 4638, 4639, 4673, 4674, and 4699, 4717 and 4725.

A straightforward interpretation of the triangular cluster of "dot" images in frame 4838, for example, is that the cameraman filmed a triangular cluster of lights (!) and that the camera and film resolved the spacings between the lights. In frame #4838 the spacing between PY/O round "dot" image at the apex of the traingle and the center of a line joining the lower two R "dot" images is about 0.084 mm (84 microns). The center-to-center spacing between the R images is about 0.049 mm (49 microns). Since the focal length of the camera lens was 100 mm the angular separations of the images were 0.84 mr (milliradians) and 0.49 mr, respectively. The actual separations cannot be determined unless the distances of the lights from the camera are known. What can be determined from the angular separations are the spacings as projected onto a plane perpendicular to the line of sight (i.e., parallel to the

film plane) if the mean distance to the lights is known or assumed. Assuming that they were all at the same distance, i.e., lying in a plane parallel to the film plane, and assuming that they were 10 km away then the separations would have been 8.4 m and 4.9 m respectively. If they were at the distance of the radar target that was reported at 0251 (see map), that is, at 20 nm (37 km), the spacings would have been about 30 m and 18 m respectively. (The radar target will be discussed later.) Of course, if not all of the lights were at the same distance from the camera then the separations would be greater than calculated here.

The first triangular cluster in the film appears in frame #4666. In this frame the PY/O and R images are not distinctly separated, as they are in later clusters. From the center of the PY/O image to the center of the baseline joining the centers of the R images is about 0.07 mm (70 microns). The spacing of the centers of the R images is also about 0.07 mm. The next triangular cluster is found in frame #4804. In this frame the vertical spacing is 0.085 mm and the horizontal spacing (of the R images) is 0.065 mm. The most clearly resolved cluster is in frame #4838. It was described in the preceding paragraph.

Each of the non-stationary frames listed above contains a horizontal or nearly horizontal elongated PY/O image above a similarly elongated R image. These elongated images are *consistent* with what would be expected if the camera line of sight were moved laterally (i.e., the camera rotated) while the camera filmed a triangular array of lights with a PY/O light above two R lights (forming a horizontal base). The first such frame is #4613. In this frame the center-to-center vertical spacing of the streaks is about 0.058 mm. The next frames are #4638 and 4639. In these frames the vertical spacing is about 0.056 mm. The next frames are #4673 and 4674, for which the vertical spacing is about 0.07 mm. Finally there are parallel (bent) streaks in frames #4699, 4717 and 4725 in which the vertical spacing is estimated at 0.077 mm.

Figure 5 illustrates the changes with frame number (i.e., with time) of the

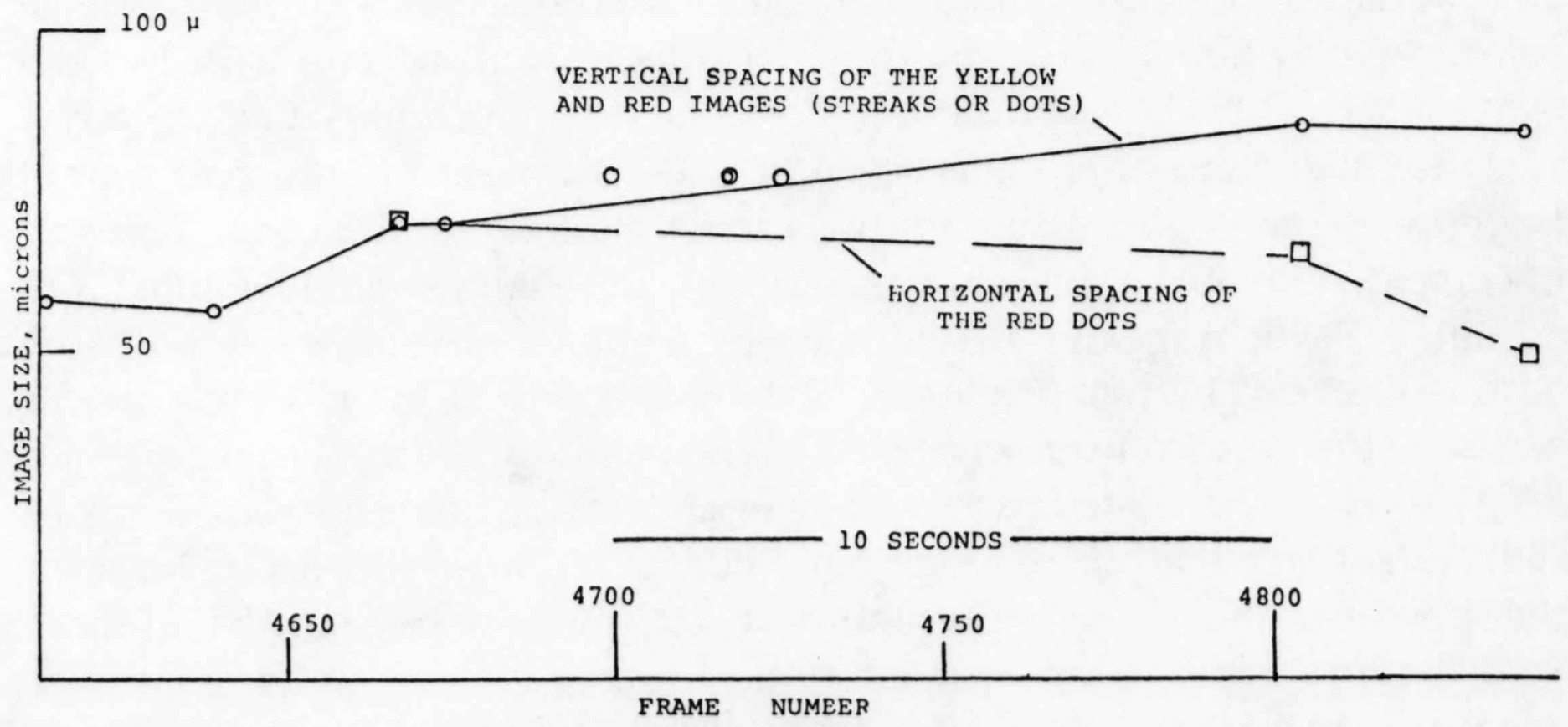

Fig. 5. Dimensions of the triangular images.

vertical and horizontal spacings of the images based on the measurements given above. The graph in Figure 5 shows that the vertical spacing of the PY/O image from the R images (circles; upper line) increased as time went on, while the horizontal spacing of the R images decreased (squares; there are only three data points).

The increase in angular separation between the apex and the base of the trangular cluster is consistent with the testimonial information that indicates that the plane was approaching the lights, assuming that the vertical spacing of the lights was constant. The fact that the vertical and horizontal spacings changed differently suggests that there may have been a slight change in the viewing aspect. If the three lights were attached to a single object, the decrease in spacing between the R lights could be attributed to a slight rotation of the object about a vertical axis. Another possibility is that the light cluster remained fixed in space and the distance decrease (as the plane approached) was accompanied by the rotation of the line of sight to the lights, such as would happen if the plane did not fly directly toward the cluster. Unfortunately the film does not provide sufficient information to determine whether the viewing aspect changed or the light cluster itself rotated. However, the descriptive information on Fogarty's tape, which will be discussed in a later section, suggests that rotation of the cluster itself could have actually occurred.

A general discussion of proposed explanations will be presented in a later section of this paper. However, a brief discussion will be given here of those explanations which to apply to the triangular image clusters.

The initial explanation for this section of film was proposed by Quentin Fogarty when he first saw the film: the cameraman had filmed a flashing beacon. However, a study of the nautical and aircraft beacons within view of the airplane at any place along its flight path failed to turn up any beacon with the appropriate basic characteristics (a triangular arrangement of one white and two red lights and a flash rate of about once per second). There were beacons found which flash red and white, but the periods of these beacons are four seconds or longer and each such beacon flashes with only a single color, red or white, at any one time. Also there is no beacon with two red lights side by side. This author then considered the possibility that there was a chance alignment of beacons, say a rapidly flashing (i.e., once per second) bright white beacon aligned with two rapidly flashing red beacons. This sort of coincidence could explain the periodicity, although it would be difficult to explain the synchronism (i.e., maintaining a phase difference of about 180°) during 32 cycles by independently flashing beacons. If the red beacons did not flash, the synchronism between the bright white and the red images could be explained if the white beacon got so bright that its image increased in size and blotted out the red beacons once per cycle. However, again no such alignment was found. Then this author investigated the possibility that there were flashing lights on another aircraft. However, the air traffic controller stated that there were no other aircraft anywhere near the Argosy freighter. Finally this author considered the possibility that some emergency vehicle

near Blenheim had been filmed. However, a check with the local police and fire departments showed that there were no emergency vehicles out that night. Furthermore, it was learned that emergency vehicles in New Zealand use blue lights. Thus no conventional explanation was found for the triangular image clusters.

IV. Discussion of the Transition and Red Images

The previous section contains a discussion of the frames with triangular clusters and parallel elongated R and PY/O images. The frames with bright overexposed PY/O images will be discussed in the next section. This section contains a discussion of those frames in which the R and PY/O colors are mixed within a single, usually distorted, image. These are the "transition images" which lie between the minimum and maximum intensity images in each cycle. This section also contains a discussion of the frames which contain only R images.

A study of the transition images shows that the PY/O light and the R lights remained adjacent to one another (at least from the point of view of the camera) throughout each cycle. The study also shows that the PY/O source intensity decreased uniformly from the maximum value in a cycle, generally taking four or more frames to reach zero, but that it increased from zero very rapidly, generally taking only two frames to reach a value close to the maximum (see, for example, Figure 4). Thus the intensity did not change discontinuously, such as would happen if the light had been quickly switched on and off, like a strobe light, or abruptly occulted by some opaque object. The transition images also show that the R intensity increased and decreased uniformly rather than discontinuously (see Figure 4). Unfortunately the camera motion distorted most of the transition images making a more detailed interpretation quite difficult.

There are also a number of frames with only R images. These occurred when the PY/O intensity was zero. At their brightest the R light sources were much dimmer than the PY/O light at its brightest. At their dimmest the R lights either barely made images (as in frames #4606, 4623, 4624, 4649, 4658, 4676, 4684, 4685, 4709, 4735, 4762, 4770, 4787, 4796, 4797, 4812, 4820, and 4839) or were not even bright enough to make detectable images on the film (as in frames #4597, 4598, 4630, 4631, 4632, 4633, 4650, 4659, 4710, 4736, 4788, 4813, 4814, 4821, 4822, 4823, 4829, 4830, 4831, 4847, 4848, 4849, 4854, 4855, 4856, 4857, and 4858). A study of the above frame numbers will show that the frames with faint red images or with no images at all occurred most often at the end of the film sequence, and next most often at the beginning of the sequence, when the maximum brightness in each cycle of the overexposed images was lower than the maximum reached in the middle of the sequence of cycles. Referring to Figure 3 we see that the tendency for the red images to be very dim near the beginning and end of the film correlates with the tendency for the maximum image brightness to be lower at the beginning and end of the film.

V. Estimates of the Maximum Intensities of the PY/O and R Light Sources

The PY/O images present the greatest possible range in film exposure, varying from zero (no image) to completely overexposed. The central portions of the bright images are completely devoid of color (overexposed) and appear to be "white" or the color of the incandescent projection bulb. Around the central portion of each image is a highly exposed, pale yellow band or annular region. Surrounding the pale yellow annular region is a very narrow (10 to 20 microns wide) "color gradient region" (CGR) which forms the boundary between the highly exposed area and the surrounding unexposed (black) area. The CGR exists because of the variations of exposure level in the three layers of film emulsion. It is visible around the edges of overexposed images of known incandescent light sources (e.g., airport lights) as well around the edges of the overexposed images of the unusual light. The width of the CGR is roughly independent of the image exposure level, growing to a maximum of about 20 microns wide around overexposed images that are about 200 microns in diameter. The CGR is surrounded by "perfect" blackness (no exposure). (A discussion in Section VII shows that this fact by itself is sufficient reason to reject the anticollision beacon hypothesis that has been proposed to explain the unusual flashing light images.)

Because the central portions of the images are overexposed it is difficult to determine the exact color of the PY/O light source. It could have been pale yellow or very pale orange, hence the notation PY/O. In the Appendix the notation used is BYW (bright yellowish white), rather than PY/O, to convey in words the impression one gets from looking at the images. When the overall image is relatively dim, the PY/O portion of the image is found at the apex of a triangular cluster of images (described in Section III). At this time it looks pale orange or pale, *very slightly* reddish, yellow. By way of comparison, the R images which form the base of a triangular cluster are "pure" red and none of them are overexposed.

There are several possible explanations for the periodic changes in the sizes of the PY/O images, only two of which are worthy of some discussion. These are (a) periodic changes in the size of the light source while the source intensity remained substantially constant and (b) periodic changes of intensity while the size remained constant. The reason for the first possibility is that the size of an image increases with increases in the size of an object, all else being constant. The reason for the second possibility is that the size of an image on film increases with increases in the intensity of the object or light source, all else being constant. (This is a fact well known to astrophotographers.) More specifically, the image size generally increases beyond the geometric size, with the amount of increase (the image "growth") being proportional to the logarithm of the *exposure level*[3] (Mees, 1944). (The size of the geometric image

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[3] This author has conducted numerous experiments which confirm that color reversal film of the type under discussion obeys the logarithmic image growth law over a wide range of exposure levels starting with the lowest level which will produce a visible image and ranging upward for four or more decades in exposure level.

of an object or a light is equal to the actual size of the object, projected onto the line of sight to the camera, multiplied by the focal length and divided by the distance from the camera to the light source.) Therefore, since the exposure level is proportional to the intensity (all else being constant), the amount of growth in the size of the image is generally proportional to the logarithm of the intensity.

Although the first possibility (a periodic size change by a factor of ten or more) cannot be positively ruled out, it is more reasonable to assume that the intrinsic size of the PY/O light source reamined constant. Therefore the analysis in this paper assumes that the PY/O and R lights which form the triangular arrangement were effectively point sources of constant size and that their intensities changed periodically.

One can calculate the luminous intensity in lumens/steradian (lm/st), or candelas (cd), of a point source using the following equation:

$$(1) \qquad I = \frac{Q \times R^2 \times e^{(b \times R)}}{T \times A \times t}, \qquad \text{cd (=lm/st).}$$

This equation can be found by inverting standard photometric equations which give the image exposure in terms of the source intensity. In this equation, Q (lm $\times$ sec) is the photometric energy deposited within the boundary of the image (within the CGR) and t (sec) is the exposure duration (the "frame time"). The value of Q is determined from the image in a manner to be described. Also in the equation, R is the range from the camera to the light source in meters (m), b, is the atmospheric extinction coefficient in m^{-1}, T is the transmission of the camera lens and the aircraft window, and A (cm^2) is the area of the lens aperture. The area is given by the following equation:

$$(2) \qquad A = (\pi/4) \times (F/f\#)^2.$$

where $\pi = 3.1416$, F is the focal length of the lens (10 cm in this case) and $f\#$ is the aperture setting on the camera (1.9 in this case).

Of the quantities in the above equations only R is completely unknown. Since R is completely unknown the procedure for using the photographic data is as follows: (a) for a given image determine Q in the manner to be presented; (b) using Q along with the camera coefficients (t, A, T) and an estimate for b (see below), calculate a set of intensity values corresponding to various R values and produce a graph of I versus R; (c) choose a value of R and read from the graph the light intensity *which is required at that distance to make the film image.* Besides providing intensity levels that correspond to various assumed distances this graph allows one to compare the intensity of a known light source at known distance with the intensity at that distance as *required by the film image.*

The camera coefficients in Eq. (1) are $t = 0.044$ sec (0.1 sec/frame with a rotating shutter efficiency of 44%: $0.1 \times 0.44 = 0.044$) and $A = 0.00196$ m^2 (from Eq. (2) using $F = 100$ mm and $f\# = 1.9$). The transmission factor, T, is estimated to be 0.8 (80%).

The atmospheric extinction, b, has been estimated from weather data which include upper altitude data on temperature, wind direction and the relative humidity. Unfortunately these data were measured several hours before the sighting and at a location about a hundred miles from the sighting area. Nevertheless, using atmospheric models (LOWTRAN code), using knowledge about the weather patterns preceding the sighting (e.g., a cold front passed through the area several hours before the sighting) and using estimates of the aerosol content of the atmosphere, it has been possible to estimate reasonable maximum and minimum values of b at the time and location of the sighting.[4] Values of b were found for a slant (visual) path from 4 km (the airplane altitude) to the ground and for a horizontal path at 4 km.[5] The reason for considering a slant path to the ground is to compare the calculated maximum intensity with the maximum intensities of various (ground level) beacons. There are two reasons for considering a horizontal path. One is that the witnesses claimed that the unusual lights were above ground. The second reason for considering a horizontal, or even an upward, path is that the film shows no lights other than the flashing light, although at ground level there were numerous lights, both flashing and steady. The absence of other images therefore implies that the field of view of the camera was totally above the horizon (ground level).

The maximum and minimum b values for the slant path to the ground are 1×10^{-4}/m and 5×10^{-5}/m, respectively. The estimated value for the horizontal path is $b = 1 \times 10^{-5}$/m. If the visual path had been upward from the plane the value of b would be even lower (Menat, 1980) but not zero ($b = 0$ corresponds to the situation with no atmosphere).

Because the value of b is not precisely known there are three curves of I versus R corresponding to the above three values of b. To use the graph one should decide first upon the value of b, and then use the curve for that value of b to find I at some chosen value of R.

The final quantity needed to calculate the source intensity is the quantity Q, which is the spectrally weighted (according to the color response of the film) energy collected by the camera lens and deposited within the boundary of the film image. Q is the product of two other quantities which can be measured or estimated: the film exposure level, H, in lm $\times$ sec/cm^2, and the image area, A_i in cm^2. More accurately, Q is the summation (or integral) of the product of H and A_i over small subareas of the image. Unfortunately densitometry equipment that would conveniently measure H in a large number of small subareas of the image was not available for use in this investigation. Therefore the summation has been approximated by using an estimate of the average H over the whole image.

The average value of H has been estimated in two ways. The first way is the "traditional" method which makes use of the empirical relation between

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[4] An unpublished paper on the estimate of the extinction coefficient at the time of the New Zealand sightings is available from the author.

[5] *Ibid.*

the image density and the exposure level, H, for the particular type of film. The second method, which provides a very convenient way to estimate H values, makes use of the image size to estimate H.[6] Since both methods yield the same value of Q only the traditional method will be discussed.

The relation known as an "H & D" curve is provided by the film manufacturer. On this curve (or family of curves) image density, D, the negative of the logarithm to the base ten of the film optical transmission, is plotted as a function of the logarithm of H. For positive transparency (color reversal) film, such as slides and movie film, the density is high in unexposed regions and low in exposed regions. A densitometer is a device which directly measures D. A scanning densitometer was used to measure the average densities of overexposed images on Crockett's film. Then the H & D curves for the film (Fujicolor 8425 color reversal type) were used to find the corresponding H values. Since the curves supplied by the Fuji Film company do not provide accurate information about the density of overexposed images, the curves were supplemented by experimental data obtained by this author.

A large number of overexposed images are candidates for this type of analysis. The densities of these images lie in the range 0.12 to 0.16, with the clear leader having a density of about 0.10. A conservative choice that leads to a lower bound on the maximum image exposure is D = 0.16, which, according to the commercial H & D curves supplemented by experiments carried out by the author, corresponds to an exposure level of about 5×10^{-5} lm × sec/cm^2. If a lower density value were assumed, say 0.12, the exposure level would be considerably greater since, in the region of overexposure (film saturation), the relationship between exposure and density is highly nonlinear.

The maximum intensity of the PY/O light source can be estimated by combining the above value of H with the area of the largest overexposed image which has a density of 0.16 (or less) and which was not smeared by camera motion. (Smearing can increase the area of the image and cause the image to have a distorted shape, while decreasing the exposure level slightly, making it difficult to calculate Q). Assuming that the light was a point source, an unsmeared, overexposed image should be round. Therefore, only images which are round or very nearly so have been considered for this calculation. There is a considerable number of large images of very similar areas which are nearly round (in frames #4618, 4628, 4636, 4644, 4645, 4646, 4652, 4653, 4662, 4694, 4748, 4774, 4775, 4782, and 4792). There are also two large images that are almost perfectly round (in frames #4687 and 4758). Although Q could have been calculated using the area of any of these, it has been calculated only for #4758 because of its simple round shape. It has a diameter inside the CGR of about 250 microns so its area, A_i, is about 4.9×10^{-4} cm^2. Multiplying this by the average value of H given above yields $Q = H A_i = 2.5 \times 10^{-8}$ lm × sec. This is actually a lower bound on Q since the exposure level used corresponded to a film density of 0.16, whereas some of the large overexposed images had densities as low as 0.12.

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[6] *Ibid.*

Figure 6 has been created using this value of Q along with the previously listed values of the other quantities in Eq. (1). The figure shows that for any assumed distance greater than 5000 m the source intensity needed to make an image as large and bright as #4758 must be greater than 10^4 cd. Similarly, if the source were at a distance greater than 50 km the intensity must have been greater than 10^7 cd if the PY/O light were on the earth's surface, or greater than 10^6 cd if the source were at the altitude of the plane.

Similar estimates of the maximum intensity of the red light source show that it was, at its brightest, about one one thousandth as bright as the PY/O light at its brightest.

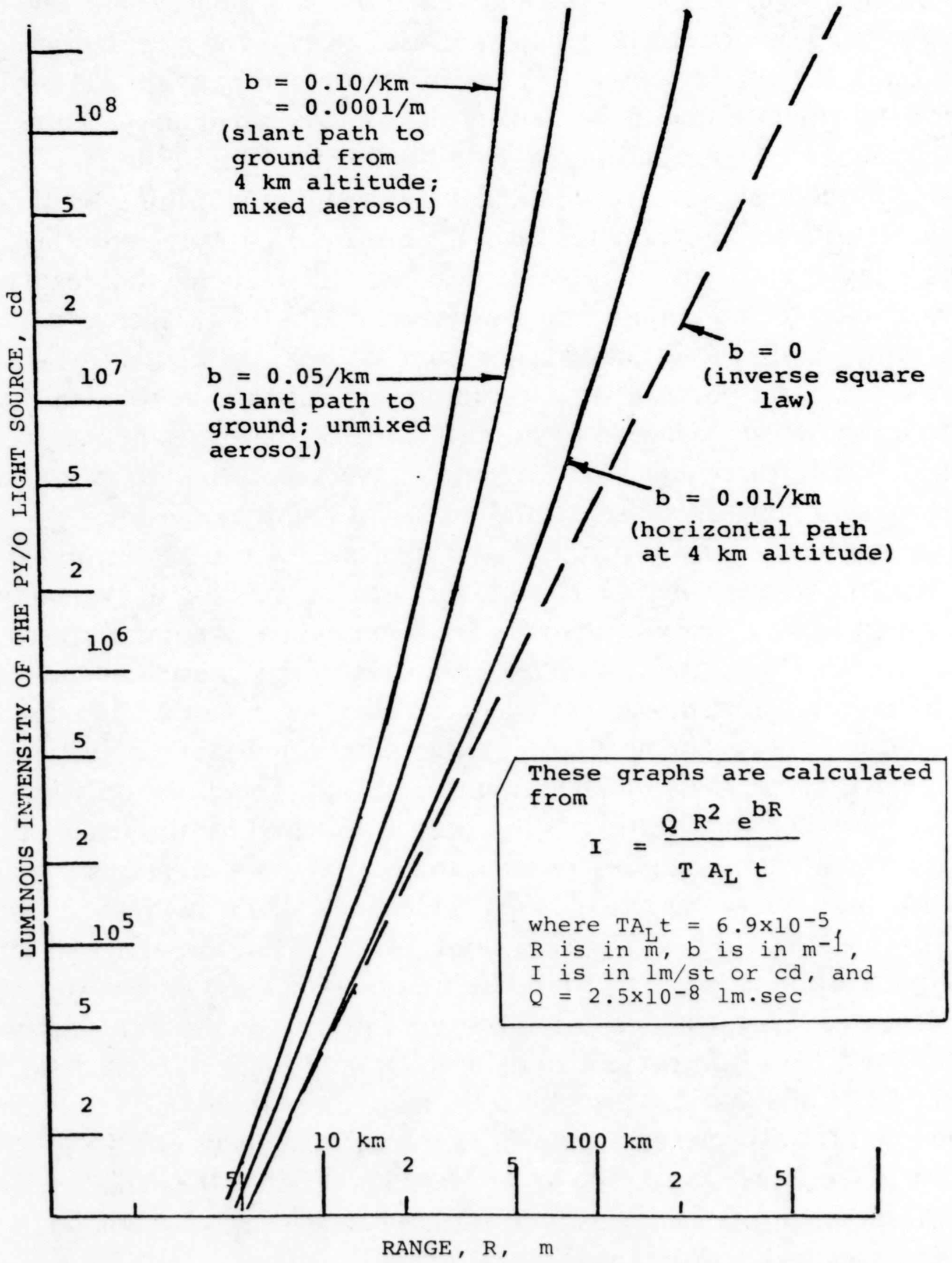

Fig. 6. Source intensity for various distances.

VI. A Discussion of the Witness Testimony

The information provided thus far has been derived from the analysis of "hard" photographic data. This section presents a summary of the "soft" information supplied by the witnesses. This information is worthy of serious consideration because it comes from tape recorded statements made at the time of the sighting supplemented by the recollections of the witnesses. Thus, unlike the situation with most UFO sightings, it is not necessary to totally rely on the memories of the witnesses.

The exact time (to within half a minute) of the visual sighting, which, it is argued here, was coincident with the previously described film, has been determined from the tape recording made at the Wellington Air Traffic Control Center[7] (WATCC). The tape shows that, about five minutes previous to the sighting, the plane turned westward at Kaikoura East and headed toward Cape Campbell (see Fig. 1). The time was about 0246:30 (30 seconds after 0246). In the following two minutes the WATCC gave a weather report for Blenheim which included the statements that the visibility was 60 km and that the cloud coverage was $\frac{1}{8}$ of the sky over the Blenheim airport. The WATCC also reported several radar returns (radar "targets") near the South Island which were showing on the search radar that was monitoring the flight of the plane. The WATCC reported the unidentified radar targets at this time because in several instances earlier that morning there had been correlations in the appearance, disappearance and apparent location of visual objects (lights seen from the plane) and radar targets. However, the crew on the plane did not see lights associated with the radar targets that were reported between 0246 and 0248.

Then, at 0251, the copilot contacted the WATCC to report a visual sighting. The portion of the tape transcript which is pertinent to this sighting is given below. Times are given in 24 hour notation, with seconds following the colon, and directions are given in "clock notation" with 12:00 straight ahead, 3:00 at 90° to the right, 9:00 at 90° to the left, etc. Words in parentheses have been added by this author for clarification.

TIME	STATEMENT
0251	—(plane) Wellington, do you have (a target) in my 12:00 position (i.e., straight ahead), probably somewhere near Grassmere or perhaps a little east of Grassmere?
	—(WATCC) Affirmative. I have a strong target at 12:00 to you at 20 (nautical) miles and, uh, that's 2 miles off the coast, 10 miles south of Cape Campbell.
	—(plane) Roger. We have that one, also, and quite a good visual display at the moment.
	—(WATCC) It's showing lights?
	—(plane) Say again?
	—(WATCC) It's showing light, is it?
	—(plane) Affirmative. It looks like a collection of lights. I wonder, can you

[7] A copy of the Wellington Air Traffic Control Center tape record was supplied to this author by Geoffrey Causer, the air traffic controller who was on duty that night.

establish (contact) and ask the flight service man (at Blenheim airport) to turn his rotating beacon off just in case we're mixing it up with that?

0252 —(WATCC) OK

—(At this point the WATCC called Blenheim and asked for the beacon to be shut off.)

0252:20 —(WATCC) The beacon is going off now.

—(plane) Thank you.

0253 —(WATCC) Two further targets showing well on radar, one at 9:00 at 8 miles and one at 10:00 at 10 miles.

—(plane) Roger.

0253:20 —(WATCC) The one just south of Cape Campbell is now gone off the radar.

—(plane) Roger.

Although the statements by the copilot give only a frustratingly brief, description of the events occurring at the time, a significant amount of information can be derived from the above transcript. First and foremost is the apparently simultaneous appearance of a radar target in the same direction from the plane as the visual display. Figure 1 shows that the location of the radar target was in the same direction as the copilot indicated (toward Lake Grassmere) but about 10 miles closer to the plane. The copilot's error in estimating the distance is not surprising since this was a nighttime sighting of lights of unknown intrinsic size.

The copilot described what he saw as a "collection of lights." This implies several lights in close spatial association. The copilot then asked for the Blenheim Beacon to be turned off "in case we're mixing it up with that." This was a reasonable precautionary measure since the direction to the Blenheim beacon (see Fig. 1) was close to the direction to Lake Grassmere. The beacon is a single white light that flashes once every 3.5 seconds so that the implication of this request is that the lights he saw were flashing. In a tape recorded free-recall statement made four days after the sighting the copilot recalled seeing "two big orange lights" as the plane headed toward Cape Campbell. "One of them was flashing a little, so we got a message through Wellington to Blenheim flight service to turn the aerodrome beacon off. We were told it was off, but this thing was still flashing." (Subsequently the unusual flashing light did disappear, as will be described). In this statement, which was made before he heard the WATCC tape, the copilot inadvertently combined some details of the sighting of the "collection of lights" with the details of a subsequent sighting of two bright lights that appeared in the sky ahead of the plane after the flashing "collection of lights" had disappeared. (Crockett remembered seeing these two lights appear but he was not able to film them because the airplane started turning into an orbit at about 0258 to lose altitude in preparation for landing before he could align his camera properly.)

It is interesting to note the copilot's statement that the "collection of lights" was still flashing after the beacon went off. It is interesting because, according to the WATCC tape, the beacon went off at about 0252:20 or 0252:30 and about 45 seconds *later* the large target south of Cape Campbell disappeared from the radar screen. Thus when the WATCC tape is compared with the copilot's statement it appears that the flashing "collection of lights" and the

radar target disappeared at about the same time, perhaps even simultaneously, although simultaneity cannot be proven with the available information.

The second major source of testimonial information is the tape recording made by reporter Quentin Fogarty.[8] Fogarty had tape recorded his impressions of the previous sightings that night. He had taped several descriptions of what he saw during the sighting that took place between about 0218 and 0235 while the plane was within about 50 miles of Christchurch. After that sighting was over Fogarty, along with the air crew and the other passengers, continually scanned the skies for unusual lights. A few minutes after the plane passed Kaikoura East he recorded the following statement:

> We've now just passed Kaikoura East and, uh, there's been no further activity. There are pinpoints of light in the sky but nothing's been confirmed on radar. I, for one, am hoping really that, uh, we've seen enough, and, uh, the rest of our journey back to Blenheim will be uneventful. I've had just about enough of UFOs for one night.

Fogarty's next taped message is reproduced below. It is broken into individual statements to facilitate the following discussion. Elapsed times, accurate to within a second or so, are given in parentheses.

1. "About 30 seconds after that last message and, of course, we've got another one . . . right in front of us (8 sec)
2. very bright . . . it seems to be quite a long way away (12.5 sec)
3. and another one flashed just to the left of it. (14 sec.)
4. That one flashed extremely brightly. (17.5 sec)
5. They've now both faded. The other one's flashing again. (20 sec)
6. It's . . . giving off . . . an orange flashing light (25.5 sec)
7. It looks like an aircraft beacon . . . and it's moving . . . off (32 sec)
8. It's extremely bright. (34.5 sec)
9. It fades and it's dropped. (37 sec)
10. It seems to have just dropped at an incredible speed and it's . . . (40 sec)
11. It seems to be rolling and turning. (41 sec)
12. In fact there's one light, there's another light beside it. (46 sec)
13. Oh, I don't know . . . I really don't known what is going on. (53.5 sec)
14. It appears to be over the hills. (58 sec)
15. There appears to be a whole cluster of them in fact. (In the background Crockett yells "I can't see anything.") (65 sec)
16. You can see orange and red among the lights. There's one particular one that keeps flashing to the right hand side of it. (74 sec)
17. You can see three distinct lights . . . In fact it looks very much like the same sort of pattern we saw . . . when we came down over the Kaikoura

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[8] A copy of the tape recording made during the flight by Quentin Fogarty was supplied by TV Channel 0 (now channel 10) in Melbourne. This author transcribed the tape and Quentin Fogarty reviewed and corrected the transcription.

coast on the way down, but, um, there wasn't quite as much flashing . . . It really is, uh, . . . quite strange," (98 sec)

Because Fogarty did not keep track of the exact times of his taped messages, the time of the message quoted above can only be determined by correlation of its content with the content of the WATCC tape. Since it is very likely that Fogarty saw what the copilot reported directly ahead of the plane at 0251 (the sudden appearance of a collection of lights), it is the contention of this author that this message describes the 0251 sighting. This contention is supported by the comparison of Fogarty's next (and last) message, which reports what happened after the beacon was turned off, with the copilot's statement at 0255, as recorded on the WATCC tape. First consider Fogarty's last recorded statement (elapsed times are measured from the beginning of his last statement):

18. "Well, you can't be right all the time. It appears that the last flashing light that we saw was a beacon at Blenheim and they asked . . . the pilots asked for the beacon to be turned off and we're no longer seeing that light. (15 sec)
19. But at the same time as they turned the beacon off, Wellington radar told us that he had targets coming over to the left of us. In fact, as I speak now, we have another one right above Blenheim. Extremely bright. (30 sec)
20. And that's not a beacon because it's not in the same positions as the lights were before and these sightings at the moment are right in the position were Wellington radar says they should be" (46 sec)

To recapitulate, according to the Wellington tape the Blenheim beacon went off at about 0252:20, but, according to the copilot the "collection of lights" was still flashing. At about 0253 WATCC reported "Two further targets showing well on radar" at the left of the plane (9:00 and 10:00). (These do not appear to have been related to the sighting. They were probably weather "angels," spurious temporary ground returns caused by atmospheric refraction of the radar beam. Such spurious returns are common along the Kaikoura coast and had been reported earlier in the evening.) At about 0253:30, the "strong target" south of Cape Campbell disappeared from the radar screen. Presumably the "collection of lights" disappeared at the same time as the "strong target," but this is only conjecture. About 45 seconds later Wellington reported four targets at 9:00, 9:30, 10:00, and 10:30 to the plane (probably more spurious returns). Then, shortly after 0255 the copilot reported as follows:

We had a pretty bright light. We have it again now. It appears to be behind Woodbourne from where we are. Probably towards, uh, between us and Nelson North in that direction. Do you have anything at all in that direction?

Woodbourne is the name of the airfield at Blenheim and Nelson North is

a non-geographic aircraft reporting point somewhat north of the town of Nelson (logical!), so the copilot was indicating that the "pretty bright light" was also in the direction of Blenheim. Although the WATCC had no target that would correlate with the copilot's report, the light described in Fogarty's last message does correlate: ". . . we have another one right above Blenheim. Extremely bright." (statement 19 above). Thus it appears that Fogarty's last message was taped at about 0255. It is therefore quite likely that the previous message, which describes the orange and red flashing lights, was recorded at 0251, as claimed by this author.

In his last taped message Fogarty identified the "last flashing light" (statements 6 through 12) as the Blenheim Beacon (see statement 18). When thinking about the sighting afterward Fogarty realized that this identification was clearly erroneous (Fogarty, 1982). He could see coastal beacons and the city lights of Blenheim, so he had numerous fixed reference points by which he could judge azimuthal directions, movements of lights and also the depression angle (the angle below horizontal) of the horizon. (At the 4 km altitude of the plane the horizon was about 242 km away, including atmospheric refraction effects, at a depression angle of about 1°. The lights of Blenheim were about 93 km away at a depression angle of 3.6°, or slightly below the true horizon.) He was therefore able to determine that a light seen "above Blenheim" was considerably above ground level. The unusual light which had been "rolling and turning" (statement 11) was apparently in the sky above ground level and perhaps "over the hills" (see statement 14 above) east of Blenheim (see Figure 1). Fogarty realized that no beacon could suddenly be "moving off" (statement 7), then drop downward "at an incredible speed" (statement 10) and then go into a "rolling and turning" maneuver (statement 11). (Fogarty described this maneuver as being a rotation in space, roughly like a ball tied to a string and swung about a center point, except that the rotation was in an elliptical rather than a circular orbit.) Furthermore, he realized that there are no orange beacons (statement 6). Therefore he realized that his last message (statement 18) was obviously wrong.

Considering all of these reasons why it could not have been a beacon, Fogarty (1982) explained his immediate "identification" in the following way. In the "heat of the moment" he was impressed by the fact that the flashing light went off at about the same time that he was told that the copilot had asked for the beacon to be turned off. He therefore accepted the beacon explanation for lack of anything better at the time, and went on to describe the next sighting which he knew could not have been the beacon because the beacon was already off (statement 20).

Thus far the relationship between the WATCC radar targets, the copilot's sightings and Fogarty's tape recorded statements have been discussed. The relationship between the film and the visual sightings will now be discussed.

Crockett's camera was not synchronized in any way with Fogarty's tape or the Wellington tape. Therefore it is impossible, without resorting to the testimony of Fogarty and the copilot, to show that Crockett filmed the lights

reported at 0251. However, it is quite reasonable to assume that he did film these lights for two important reasons. First, it is very difficult to imagine that he would not have seen the unusual lights which appeared directly ahead of the plane. Considering his state of mind and alertness it is extremely likely (certain?) that he would have seen them and tried to film them. Second, there are clear similarities between the lights described by Fogarty and the images on the film. The similarities are between the triangular cluster images on the film and Fogarty's description (statements 15, 16, and 17 above). Fogarty reported seeing a "cluster" of lights, that he saw "orange and red among the lights" (i.e., orange and red lights), that he observed flashing and that he could see "three distinct lights." (Note that the latter part of statement 17 refers to sightings that occurred several hours earlier as the plane flew from Wellington to Christchurch.) The descriptive terms "cluster," "orange and red," "flashing," and "three distinct lights" can also be applied to the images in Crockett's film. Therefore this author claims that Crockett filmed what Fogarty saw.

There are three possible objections to this claim. One objection is that Crockett could not see what Fogarty saw. This objection is based on Crockett's recorded statement (in a voice sufficiently loud to be heard over the noise of the airplane engines, indicating frustration on his part) "I can't see anything" (see statement 5 above). However, this statement probably refers to the difficulty he was having in keeping his camera pointed at the lights while looking through his 100 mm lens because of the rapid relative motion between the lights and the camera. Crockett had made a similar statement during the earlier sighting near Christchurch when he and all the other witnesses were watching a bright light as the plane turned. The turning created a rapid apparent motion of the object relative to the plane. At that time Crockett had trouble keeping the camera pointed at the light while he was viewing through his lens and, shortly after the plane started turning, he yelled "I can't see it." Yet there is no doubt that the light was clearly visible to all of the other passengers on the plane. (Moreover, analysis of that portion of the film suggests that Crockett actually did film that light during the turn.)

Even if Crockett had not started filming the flashing lights until five seconds after he yelled "I can't see anything," he still could have obtained the 28 seconds of film before Fogarty ended his taped description (see the elapsed times). If he had not started filming until more than five seconds after he yelled, then his filming would have continued after Fogarty stopped recording. However, if the flashing light cluster actually was the cause of the large radar target that appeared at 0251 and disappeared at about 0253:20, then he would have had about a minute to film it after Fogarty ended his message. Thus the fact that Crockett yelled that he could not see the lights does not mean that he could not have started filming them immediately after he yelled.

Another objection to claiming that Crockett filmed what Fogarty saw is based on the possible color difference between Fogarty's "orange flashing light" and the PY/O images in Crockett's film. However, as pointed out previously, the exact color of the light which made the PY/O images is difficult

to determine from the photographic data. A sufficiently pale shade of orange could appear orange to a viewer and at the same time cause images on color reversal film, especially overexposed images, to be pale yellow.

The third objection is based on Crockett's failure to recall, in an interview about five weeks later, exactly when he filmed the flashing light cluster. However, the final portion of the film shows the landing at the Blenheim airport. The unusual flashing light cluster was not part of the landing field display of lights. Therefore the film proves that the cluster was filmed at some time before the landing, and the most likely time was at 0251 when the others on the plane saw the unusual lights which have been discussed above.

Thus, in spite of the above objections, it appears to be very reasonable to conclude that Crockett saw and filmed the lights that Fogarty described in his tape recording.

If one accepts the claim that the copilot and Fogarty described the lights which Crockett filmed, then the following technical information can be added to that which has already been gleaned from the film: (a) the light source which made the PY/O images may have been tinted more toward orange than the film indicates, (b) the unusual lights were considerably above the ground (this is consistent with the film which shows no ground lights during the flashing light sequence of frames), and (c) the lights moved rapidly at times, even dropping downward and "rolling and turning." It may be impossible to determine whether or not some of the motion of the images on the film is related to actual motion of the lights because the camera rested on Crockett's shoulder and not on a stable tripod. However, there is a short section at the begining of the film in which the image motion seems difficult to ascribe to random camera motion alone. The image undergoes a rapid cyclic motion that requires about four frames to complete each cycle. The image motion describes a very narrow ellipse with the major axis being nearly vertical. The motion lasts for at least five cycles with an oscillation period of about 4 frames/cycle, corresponding to 2.5 cycles/sec. The peak to peak amplitude of the motion is about 11 mr (0.6 degree). This motion is similar to what Fogarty meant when he said in his taped message that the one of the lights he saw dropped downward and went into a "rolling and turning" maneuver.

If the light cluster that was filmed was also the cause of the radar target reported at 0251, then the distance to the cluster would be the distance to the radar target, 20 nautical miles or 37 km. At that distance the maximum intensity of the PY/O light (see Figure 6) would have had to have been greater than 5×10^5 cd if it had been at the altitude of the plane and greater than 2×10^6 cd if it had been near (but above) the ground. Also, at that distance the spacings between the triangularly arranged light sources, projected onto a plane perpendicular to the line of sight, would have been as follows: about 30 m between the PY/O source at the apex and line joining the red lights and about 18 m between the red lights themselves.

VII. Discussion of The Suggested Explanations

Eight hypotheses have been offered to explain the film. These hypotheses reject the conclusion that Crockett's filming was correlated with the 0251 radar target and visual sighting and assume, instead, that the film can be treated independently of the visual and radar sightings. Therefore this section will therefore concentrate on proposed explanations for the film alone and treat the visual and radar sighting information only tangentially.

Fogarty was the first person to "explain" the film because he was the first person to see the it after it had been developed (Crockett did not see it until a week or more later). Because the images flashed at a steady rate his immediate impression was that Crockett had filmed a beacon. He therefore ignored this section of the film and publicized, instead, the section of film shot earlier (between 0218 and 0235) while the plane was still near Christchurch. Had he spoken to Crockett before deciding that it showed a beacon he might not have been so hasty because Crockett was certain that he hadn't filmed a beacon at any time during the flight. Crockett stated that whenever he saw a light which he thought might be worth filming he asked the copilot to identify it, thereby avoiding beacons. The copilot confirmed that he had repeatedly been asked by Crockett and the other passengers to identify navigation beacons, stars and other ordinary lights.

In spite of Crockett's claim that he did not film a beacon the most obvious hypothesis is that he *did* film a navigation beacon or a combination of beacons. This hypothesis requires that there was, *at some point along the known path of the aircraft,* a navigation beacon or a combination of beacons that was within view of the aircraft and that had the flash rate and color structure of the film images. The hypothetical beacon or combination of beacons could be placed, for purposes of explanation, at any point along the flight path between Kaikoura East and the Blenheim airport because Crockett did not remember exactly when he filmed the flashing light cluster. The *New Zealand Nautical Almanac* (1979) lists all the marine and aviation beacons and gives the intensities (in cd) of the marine beacons. There are no yellow or orange beacons. There are beacons which flash white only, beacons which flash red only, and beacons which alternate white and red, with the white color being much brighter than the red, but both colors are not visible at the same time. Allowing for the possibility that an incandescent white light source could make the overexposed images in the film, a search of white-red flashing beacons was made. The fastest white-red flashing marine beacon that could have been seen from the plane flashes at a rate of 4 sec/cycle (1/4 Hz). It's peak white intensity is only about 1×10^3 cd which is much too low for the beacon to have created overexposed images even at the closest distance of the airplane to the beacon. All the other, more powerful, white-red flashing beacons flash much at slower rates. Furthermore, each of these beacons has only a single red light, not two side-by-side.

It has been suggested that the red images were made by two steady red lights that at one time during the flight happened to be aligned with a bright

white beacon in such a way as to produce the triangular arrangment. The pulsation of the red images would then be a photographic artifact of the large change in size of the image of a white beacon as its intensity oscillated. However, as Figure 4 shows, there are a number of frames (labelled "B") in which there is neither a PY/O nor an R image. This means that decrease in the brightness and size of R images could not always have been caused by increases in the PY/O intensity. Instead the pulsation of the R images must have been caused by pulsation in the red lights themselves.

The only beacons that flash at rates around 1 Hz are strictly red or strictly white "quick flashing" lights. Therefore the following possibility was investigated: at some point on the aircraft flight path there happened to be a triangular alignment of one white and two red quick flashing marine or aircraft beacons. However, no such configuration was found. Furthermore, the *New Zealand Nautical Almanac* (1979) indicates that the quick flashing beacons have intensities lower than 1×10^4 cd. These intensities are too low because at all times during the flight the airplane was so distant from each quick flashing white marine beacon that even the intensity of the closest beacon would have had to have been greater than 1×10^6 cd to produce the overexposed images. Only the quick flashing lights which are part of the Blenheim airport lighting would have been close enough to the airplane, during the landing, to cause overexposed images of the sort found on the film. However, there is no triangular arrangement of white and red lights at the airport, nor was there any possibility of a temporary triangular alignment that could explain both the flash rate and the duration of the film segment (28 seconds). Furthermore, because of the several degree field of view of the camera lens, any film of those lights would also have shown numerous other airport lights and even some city lights of Blenheim, because the landing pattern took the airplane directly over Blenheim. For these reasons the beacon hypothesis was ruled out.

The possibility that an internal (in the cockpit) flashing light caused the images was considered and ruled out because (a) there are no such flashing lights in the cockpit, and (b) the pilot turned off all the lights except steady dim red panel lights to make it easier to see lights outside the plane.

The possibility that the flashing lights were on another airplane in the area was ruled out by the WATCC radar operator who stated that there were no other scheduled aircraft in the vicinity of the sighting area, nor were any unscheduled aircraft detected by the search radar. The only conventional aircraft radar target detected at WATCC during the sighting period was that of the Argosy freighter which carried Crockett, Fogarty, and the other witnesses.

Ireland (1979) suggested that while the plane was at the location indicated by 0251 on the map Crockett filmed one particular quick flashing white beacon in the entrance to Wellington Harbor (see Fig. 1). In making this suggestion Ireland completely ignored the red images in the film, he did not fully appreciate the implications of the degree of overexposure of the PY/O images, and he did not consider the consequences of the fact that the camera had a field of view of about 4° by 6°.

The light suggested by Ireland flashes white about once every second, which is the proper rate. Nevertheless, it can be ruled out for several reasons. First, there are no adjacent red flashing lights which are bright enough to make images on film at the distance of the airplane from the harbor entrance. Second, within the field of view of the camera there were numerous other flashing and steady white lights, including some city lights of Wellington and some lights at the Wellington airport. These lights should have made numerous faint images, but there are no images on the film except those of the flashing light cluster discussed in the previous sections. Third, at the distance of the plane at 0251 from the beacon its intensity would have had to have been greater than 1×10^9 cd in order to saturate the film, but according to the *New Zealand Nautical Almanac* (1979), the actual intensity is rated at about 7×10^3 cd. Even at the distance of closest approach of the plane to the beacon, about 60 km, its intensity would have had to have been about 2×10^7 cd in order to produce overexposed images. (See Figure 6 for b = 0.05/km, since the beacon was at ground level.) Since the publication of his paper Ireland (private communication, 1984) has claimed that by the time of the sighting in December 1978, the beacon in Wellington Harbor had been replaced with a quick flashing strobe with a rated candlepower of 1×10^6. However, as pointed out above, even this intensity would not be bright enough to create overexposed images if photographed from the point of closest approach of the aircraft to the beacon. Furthermore, a strobe creates very short flashes of light, so one might expect to have PY/O images created by the strobe in one or at most two frames per cycle, not the six to eight frames per cycle in which they actually appear. Thus, in spite of Ireland's recent claim, the harbor beacon hypothesis still fails for the above reasons.

T. W. Rackham (private communication, May 15, 1980) has suggested that atmospheric turbulence and extinction effects modified the light from some distant source on the surface of the earth. He made this suggestion because, as an astronomer at Jodrell Bank in England, he was aware that atmospheric refraction and turbulence effects can distort a light source both spatially and spectrally. He pointed out that, for example, elongated and even multiple images of Venus have been photographed (through telescopes). The elongated images tend to be whitish on top and red at the bottom, which is roughly similar to the red/white images in the film. Rackham did not suggest an astronomical source for the light, and in fact he ruled out the brightest astronomical source, Venus, because it was at or below the eastern horizon at the time, it would not have been bright enough to make images as large as the overexposed images on the film, and its intensity would not have pulsated in a regular manner.

In order to make Dr. Rackham's suggestion compatible with the film one would have to assume either a distant pulsating light source, or a steady source which was distorted by a steady pulsation of atmospheric refraction. Aside from the beacons discussed before, there were no distant pulsating sources. There were, however, in the Tasman Bay, two to four intense steady

light sources that might have been in view of the plane at the time, if they were not obscured by the known cloud cover. These sources were Japanese squid boats which carry large numbers of steady incandescent light bulbs to lure squid to the surface where they can be netted. Using information about the nature and number of the lights used on the largest squid boats one can calculate that the largest intensity expected from such a boat would be 5×10^5 cd. However, at the distance of closest approach of the plane to the squid boats, about 100 km, the intensity required to create overexposed images on the film would have been more than 1×10^8 cd (see Figure 6 for b = 0.05/km). Thus, they could not have made the overexposed images. If one nevertheless assumed that these boats somehow did create the overexposed images, then one would still have to explain the large amplitude periodic oscillation of the boat intensity and the color change. Periodic (and even transient) atmospheric effects of the magnitude required by this hypothesis are completely at odds with theory and experience related to atmospheric optics. Atmospheric turbulence causes minute refractive index variations in the atmosphere and these refractive index variations can create very rapid intensity pulsations (scintillation) and slight changes in color, but turbulence is known to be random rather than periodic. Moreover there is neither observational nor theoretical support for the idea that atmospheric reddening, which is a result of frequency selective extinction as light travels over long paths (100's of km) in the atmosphere, could "convert" white light to red over a path of only 100 km. Therefore, for these reasons atmospheric effects on distant lights can be ruled out.

Because there are geological faults running through New Zealand, Brady (Pye, 1981) has suggested that the witnesses saw earthquake lights caused by the geological stress along a fault line. Traditionally such lights are associated with imminent earthquakes. However, there were no earthquakes immediately, or even for a long time after the sighting discussed here. Unfortunately the earthquake light hypothesis is not sufficiently well developed to allow one to make quantitative predictions as to the size, color, luminosity and dynamics of any small glowing regions that might be created by earth stresses. Therefore this theory (and similar "ball lighting" theories) cannot be tested against the photographic data. However, it seems highly unlikely that this hypothesis, or a "ball lightning" hypothesis, could explain the steady pulsation of the filmed lights, the extreme intensity of the PY/O light, the presence of the red lights, and the triangular arrangement.

During the initial search for known flashing lights this author considered the possibility that an emergency vehicle or police vehicle was filmed. Sheaffer (1981) independently advanced this hypothesis. For example, one might imagine an ambulence with a flashing white light on top and two flashing red tail lights. One might further imagine that the red and white flashes were accidently out of phase and flashing at the rate 1.16 Hz. If such a vehicle were filmed from directly behind, it might make images similar to those on the film.

A rather detailed analysis of the consequences of this hypothesis showed that there are several problems related to distance of the vehicle from the plane, the alignment of the vehicle with respect to the flight path of the plane, the probable presence of other lights near the vehicle, etc. However, all of the analyses became moot when it was found, by checking with Blenheim police, that there were no police or emergency vehicles on the roads around Blenheim during the early morning of the date of the sighting, and that emergency vehicles in New Zealand have flashing blue lights.

The most recent explanation is that Crockett filmed a reflection of the red flashing anticollision beacon (AB) which is on the top of the aircraft[9] (Klass, 1983; Sheaffer, 1981). The AB was suggested as a source of the light because it flashes at a rate compatible with the flash rate of the filmed light. (There is also another AB at the bottom of the aircraft. It is not considered here, but the arguments presented apply to the lower beacon as well as the upper.) This hypothesis was published (Klass, 1983) as being an acceptable explanation in spite of the following facts: (a) the overexposed images were clearly made by a pale yellow or pale orange light, not by a red light, (b) the single red AB could not simultaneously create red and PY/O images, and (c) the single red AB could not produce a triangular arrangement of images. The reasons for rejecting the AB hypothesis are made explicit in the following paragraphs.

Crockett did film the upper AB about six hours before the 0251 sighting. At that time the plane was on the ground at Blenheim airfield and had not yet taken off on its historic flight. The AB film shows (a) that the AB is red, as expected, (b) that the AB is intense enough to overexpose the film when it is shining *directly at the camera* from a distance of forty feet or so, and (c) that it flashes at a rate of 1.3 Hz. This flash rate is quite accurately known because Crockett filmed the AB with the camera running on crystal control at 24 fr/sec.

It is of great importance in evaluating the AB hypothesis to know the following facts: (a) each *properly exposed* image of the AB is "pure" red, as is expected, and (b) each *overexposed* image of the beacon has a bright pale yellow center that is *surrounded by a wide annual region* or "fringe" that is *red.* The width of the fringe is generally comparable to or larger than the width of the central area.

Crockett's filming of the upper AB was an unintentional but very important experiment with the type of color reversal movie film he used (Fuji 8425). This author has conducted numerous similar experiments with other types of color reversal (slide) film. In these experiments a red light was photographed at various high exposure levels. The resulting overexposed images always have red fringes around the pale yellow central areas. The fact that the color of the annular fringe is the same as the color of the light which made the image is

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[9] Sheaffer (1981) discussed the hypothesis of P. J. Klass that the anticollision beacon light was reflected off a mist around the airplane or off a propellor, but provided counterarguments for that hypothesis. Instead, Sheaffer proposed that "almost any object on the ground such as an emergency vehicle could conceivably be responsible for the UFO that was captured on film but not noticed at the time."

expected since the fringe is made by light which *diffuses sideways within the film material.* As the light diffuses radially sideways from the intensely illuminated central region of an overexposed image, eventually, at some distance from the edge of the overexposed region, the intensity is reduced to a value that is too low to overexpose the film. Beyond this distance the diffusing light *properly exposes the film* and creates a fringe that is the color of the light source. Eventually the sideways-diffusing light intensity reaches a value so low that the film is not exposed at all. At this distance, and beyond, the film is unexposed (black). The clear result of these experiments is that *if* a red light overexposes a film, *then there will be a red fringe around the overexposed region.* Conversely, if there is no red fringe then the light was not red.

The first step in analyzing the possibility that the AB was the source of the flashing cluster of lights is to point out that the cluster was filmed when the camera was running *without crystal control* at a speed of about 10 fr/sec. The accuracy of the speed control when the camera is operated without crystal control is about (±)10%. Therefore the 1.16 Hz flash rate, calculated previously for the cluster by assuming a frame rate of exactly 10 fr/sec, could actually have been as high as the 1.3 Hz rate of the beacon.

The second step is to point out that there is no way of knowing, independently of other information that could be compared with the film (e.g., Fogarty's taped description), exactly when Crockett filmed the cluster or where he was looking when he filmed it. Nor is there any way of knowing in which direction the camera was pointing during the filming.

The third step is to propose a mechanism by which Crockett could have filmed light emitted by the AB. The initial suggestion was that light from the AB was reflected off a mist surrounding the airplane (Sheaffer, 1981). However, this was ruled out because such a reflection would be highly diffuse and very weak. The more recent suggestion is that light from the beacon was reflected off the rotating propellor blade (Klass, 1983). It is supposed by the author of this hypothesis, without independent proof, that such a reflection might be sufficiently specular (mirror-like, as opposed to diffuse) to create a very bright reflection of the AB. However, even granting that a sufficiently bright reflection might occur, this hypothesis fails on physical grounds for the reason cited above: the overexposed PY/O images *have no red fringes around them* even though, as pointed out above, experiments have shown that the *overexposed images of red lights always have red fringes around the overexposed central areas.*

The only way to explain the lack of red fringes is to assume that the red light was in some way "converted" to bright PY/O light as it travelled from the beacon to the camera lens. The only way for the red beacon light to be "converted" to PY/O is to have the color spectrum changed. Such a change is possible to accomplish, in principle, because the red beacon light is not spectrally "pure" red, but is actually a broad continuum of colors that is strongly biased, or "weighted," toward the red end of the color spectrum. (The beacon uses a red filter to "convert" incandescent white light by selectively transmitting colors at the red end of the spectrum while absorbing other colors.)

The spectrum could be changed by a filter that would be (nearly) the inverse of the filter that converted the white light to red light in the first place. However, since the airplane window is clear glass it could not have filtered the red light. Moreover, although the reflection of the red light off the metal of the propellor might change the spectrum very slightly, it would not change the spectrum suffiently to convert red light to PY/O light. Therefore there is no way that the spectrum could have changed in traveling from the beacon to the camera.

The AB hypothesis also fails to explain the two-colored triangular images that consist of a PY/O "dot" image above *two* red "dot" images. Neither reflection off one (or two) blades of the propellor, nor passage through the window glass, could cause light from a single red beacon to split into three parts, triangularly arranged and of different colors (PY/O and R). In short, this hypothesis fails on physical grounds to explain the overexposed images and the triangular image clusters.

This hypothesis also requires it to have been possible for Crockett to film in the direction of one of the propellors. However, the size and shape of the cockpit and the locations of the backs of the seats of the aircrew would have made it very difficult, and perhaps impossible, for Crockett, who supported the camera on his shoulder, to film one of the propellors. In order to film out the right (or left) window along a sighting line about 115° from straight ahead (to film a propellor) he would have had to sit in the copilot's (or pilot's) seat. He couldn't do this without actually displacing copilot (or pilot) from his seat. However, no such displacement occurred at any time during the flight.

To summarize, the only support for the AB hypothesis is the apparent coincidence between the flash rate of the unusual light cluster and the rate of the AB. On the other hand the AB is contradicted by photographic image data and other information and therefore fails for the following reasons: (1) neither a reflection off a propellor nor passage through the clear glass window would convert the spectrum of the red AB light to a PY/O color; (2) there is no way to explain how red light from a single beacon, after reflection from a propellor blade (or two such blades at the same time) and subsequent passage through a clear glass window, could make a triangular cluster of three images such as appears on the film; and (3) Crockett probably could not have filmed any of the propellors even if he had wanted to because of the structure of the cockpit and the locations of the side windows.

In his description of the film images Klass (1983) ignored the problem of explaining how a red beacon could make PY/O images without making red fringes as well. Instead, he concentrated on trying to explain the changes in shape of the images from frame to frame. He argued that the *only way* to explain the image shape changes was to assume that the light (from the AB) was reflected off a propellor. According to Klass one can "readily" explain how the film images change rapidly from large round or oval blobby shapes to "banana shapes," to thin parallel streaks ("string bean shaped") and back to round blobby shapes in a periodic manner if one assumes that there was a lack of synchronization between the AB flash rate and the propellor rotation rate. In making this argument Klass has ignored the much more likely ex-

planation that the natural tendency of the camera, which was held on Crockett's shoulder, to vibrate randomly would cause image shape changes similar to what he describes. Such changes are evident in all portions of Crockett's film, including portions of the film which show landing field lights. (Should one imagine that Crockett filmed the reflection of landing field lights off the propellor?) Random camera motion combined with the intensity pulsation of the cluster of lights can explain all the image shape changes in the film.

Some other explanations in terms of "natural" phenomena have been proposed for the New Zealand sightings, but these are far more bizarre than the ones mentioned already. One person suggested that a portion of the sun's corona or plasma had somehow gotten into the atmosphere where it glowed. Another suggested that volcanic vapors had something to do with the sightings. These and other bizarre hypotheses (a miniature black hole, an "anti-matter meteorite") are completely untestable. However, the photographic evidence of the flash rate and the triangular structure would seem to rule out any conceivable natural phenomenon.

VIII. Conclusion

The New Zealand UFO sightings of December 31, 1978 are unique in the annals of "ufology" in that they are multiple witness sightings that are supported by two tape recordings made at the time of the sightings and by a 16 mm color movie film. Several of the visual objects reported by the crew and passengers were correlated, in terms of the times of appearance and the directions from the plane, with radar targets reported by the WATCC. One of the unusual objects, a very bright source of light that was seen between 0218 and 0235, was also correlated with an airplane radar target.

The sighting discussed here was one of many during the early morning of December 31, 1978. It is unique because of the two color images, the triangular structure and the regular flash rate of the filmed cluster of lights. Moreover, it was the only sighting that was not discussed publically. The initial news stories mentioned the sightings which occurred during the flight from Wellington to Christchurch and concentrated on the sighting which took place just after the plane left Christchurch because Crockett had more film of that unusual light than of any of the other unusual lights. There was no mention of the periodically flashing light. After the initial news stories appeared, explanations were offered by numerous experts in the fields of astronomy, meteorology and geophysics (e.g., Venus, Jupiter, atmospheric refraction of squid boat lights, earthquake lights, unburned meteors, etc.). However, these explanations only applied to the 0218-0235 sighting and not to the sighting discussed here (Fogarty, 1982; Startup & Illingworth, 1980).

This paper has presented a discussion of some of the technical data derived from Crockett's film and of the tape recorded descriptions by Fogarty and the copilot. It has been argued that Fogarty and the copilot saw the same "collection of lights" that Crockett filmed. The coincidence between the radar target at 0251 and the visual sighting has also been discussed. Explanations

for the film itself, without reference to witness testimony, have been presented. It has been argued that, when confronted with all of the technical details derived from the film, each explanation is found to either fail completely (e.g., the anticollision beacon hypothesis) or to be highly unlikely (e.g., the earthquake light hypothesis).

The only suggested explanation for the visual sighting reported by Fogarty and the copilot is that the witnesses failed to recognize ordinary flashing navigation beacons and city lights in the area (Ireland, 1979; Klass, 1983; Sheaffer, 1981). However, this explanation is not convincing. To accept this explanation one has to assume that the air crew did not recognize the usual navigation beacons in spite of years of flying experience in the area. Nor does this explanation account for Fogarty's description of a flashing light which suddenly appeared high in the sky "over the hills," dropped to a lower altitude and then went into a rapid rolling and turning maneuver. Obviously no navigation beacon could move in such a manner. To accept this explanation one also has to assume that Crockett did not film the cluster of lights that was reported by Fogarty and the copilot in spite of the high probability that Crockett did see what the others saw and that he would have tried to film it.

This author is not aware of any explanations which have been suggested for this particular sighting other than the ones listed here. If there are any other reasonable explanations which are consistent with the data the author would appreciate learning of them.

Since this sighting falls into the general category of "UFO" sightings it can be compared with other reports. One does not have to search very far in the UFO literature to find reports of multicolored objects moving through the skies (Fowler, 1974; Story, 1980; Hall, 1964). The colors red, white and orange or yellow/orange are frequently reported, sometimes along with other colors such as green and blue. Furthermore, triangular shapes are reported, although not as often as other shapes.

Fowler (1974) lists a number of sightings of lights in geometric arrangements, including triangular. Of particular interest is the sighting late in the evening of March 9, 1967 of a triangular "cluster" consisting of a white light and two red lights with the white light at the apex above the red lights. The lights were steady in this sighting. The report of a multiple witness sighting during the late evening of April 19, 1966 states that the witnesses saw a "large disc shaped object which was accompanied by two smaller objects of the same shape. They flashed red and white lights, hovered and swung back and forth like a pendulum."

In some UFO reports the dynamical characteristics (movements) of the UFO have suggested intelligent control and even sometimes reactions to the witnesses and crude "communications." The crudest form of communication is mimicry. The cluster of lights flashed steadily at a rate comparable or equal to the rate of the anticollision beacon. Was it mimicking the flashing anticollision beacon?

This brief comparison with other UFO reports does not prove that the flashing cluster of lights discussed here are in fact related to whatever phe-

nomena have been reported as UFOs in the past. However it does establish the similarities.

It has been the intent of this paper to demonstrate that, at least to the present time (nearly nine years after the event), there is no satisfactory explanation based on known phenomena for the lights that Crockett filmed. (There has also been no satisfactory explanation for what Fogarty described, whether or not it was what Crockett filmed.) Therefore it appears that the sighting was of something truly unknown to science, i.e., it remains "unidentified." Furthermore, the verbal descriptions suggest that this phenomenon was actually an object that was moving or "flying," in which case it was a true UFO.

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Appendix
Tracings of Images in the Flashing Light Sequence of Film

This appendix presents a sample of the 279 frames within the flashing light sequence of Crockett's film. Not all frames of the film actually have images that are visible to the naked eye. Frames without visible images are marked "gone" in this set of hand sketches of the images. Visible images were projected onto white paper and traced. Thus they are presented as the viewer would have seen them while looking out through Crockett's lens if there had been no image position shift from frame to frame caused by the camera vibration. When the film is viewed at the filming speed, 10 fr/sec, the images are blurred by the motion from frame to frame and by the persistence of vision. What is most obvious under these circumstances is the pulsation of the PY/O images, which change from "zero" brightness to very bright at what appeares to be a high rate (actually about 1 Hz). The red images are also visible when the film is run at full speed but they are not as impressive as when the film is run at very slow speed or frame-by-frame. Of course, one does not detect any structure in the images when the film is viewed at full speed.

The images are depicted here as if they had infinitely thin outer boundaries, but in fact the boundaries are very slightly diffuse. The "thickness" of the

boundary of overexposed images (the "Color Gradient Region") is about 20 microns. The thickness of the boundaries of images with lower exposure is about 10 microns.

Regions of different color within a single image are indicated by clear and crosshatched areas as appropriate. However, the use of a single color designation for a particular area of an image does not necessarily mean that the color is constant throughout the area. In fact, there are generally gradations of color and brightness within any particular area of an image.

Color notations are as follows: BYW = bright yellowish white, R = red, Y = yellow or pale orange, etc. BYW areas of images appear to the naked eye to be the brightest, being very pale yellow or "pure" white (the color of the clear film base, which is maximally overexposed film). R areas are red and Y areas look pale yellow, or they could be very pale orange. Other color notations have straightforward interpretations. However, one should remember that the color rendition of the film is not perfect, since it is made up of layers that respond differently to different intensities of a particular color light. For example, tests of color reversal film show that when an incandescent white bulb is photographed at good exposure the color could be called a slightly greyish white. At high exposure the same light will cause a "pure" white image (the color of the projection bulb), but at very low exposure it will have a very slight reddish hue. A red light, on the other hand, always produces red within the film image, although when the light is so bright it overexposes the film the center of the image becomes bright yellow, and there is a wide red fringe around the central area.

These images were magnified 71 times during the projection onto the tracing paper. A length scale corresponding to 0.14 mm (140 microns) on the film plane is illustrated along with the tracings. The widest image (image width is measured transverse to the direction of maximum extension for non-round images) is about 0.25 mm (250 microns) wide and the smallest images are around 0.014 to 0.028 mm wide. The smallest images are about 2 to 4 times the film grain size, which appears to be about 0.005 to 0.010 mm.

The camera was held on the cameraman's shoulder because a tripod would not fit within the cockpit. The resulting random motions of the camera caused most of the images to be elongated and also caused the image positions to shift from frame to frame. Elongated images are a result of camera motion in the direction of elongation during the time the shutter was open. The random motions of the images from frame to frame are not apparent in the sketches, which are presented as if all the images had been shifted to the center of the frame.

Since the camera vibrated randomly about a mean pointing direction there were times when the camera moved very little while the shutter was open. Thus there are several instances for which two or three successive frames have very little or no the image position shift from frame to frame. The images in these frames, "stationary images," are not distorted by camera motion and they are what would have been obtained if a tripod had been used. Most of the analysis in this paper is based on these stationary images.

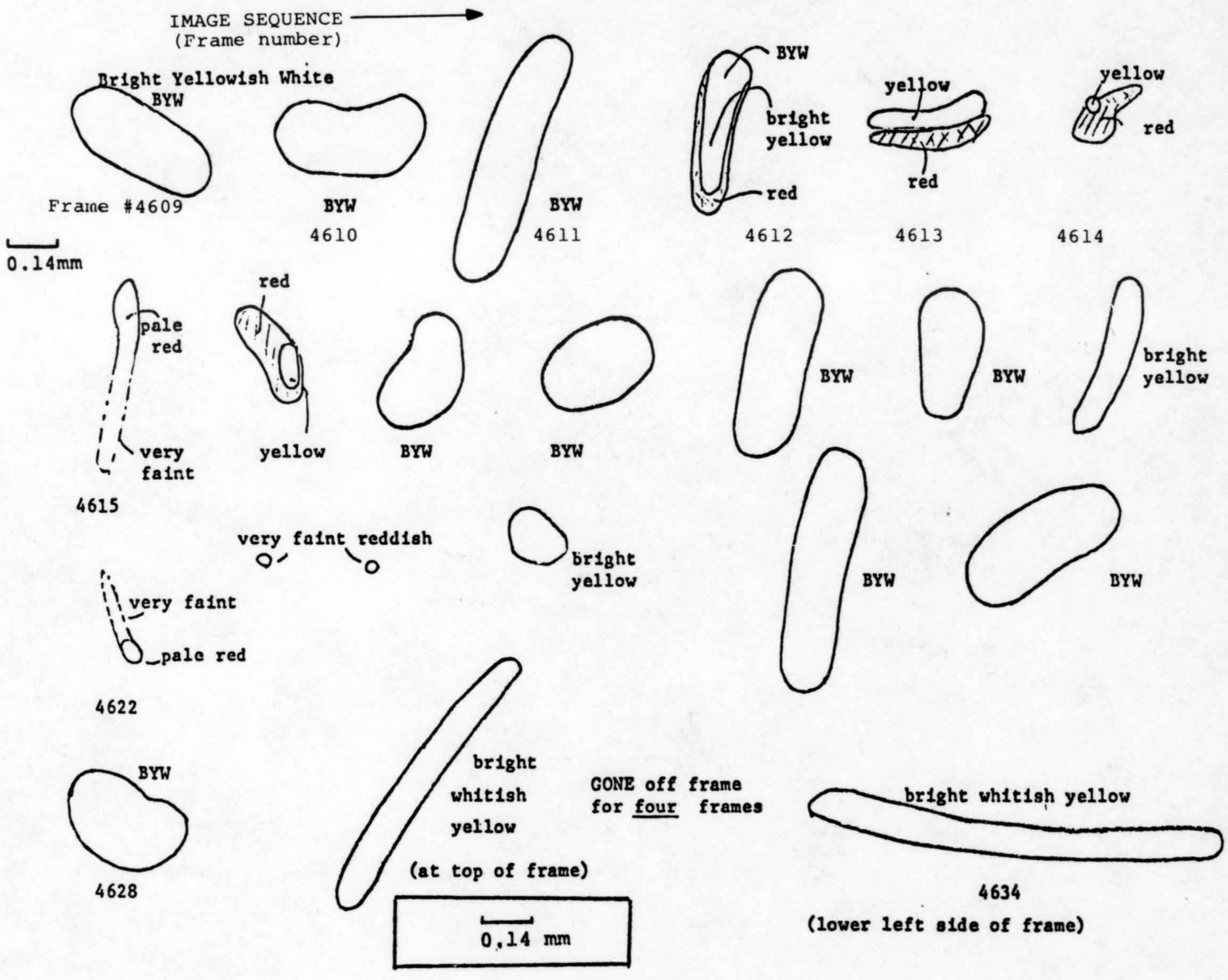


IMAGE SEQUENCE
(Frame number)
Bright Yellowish White
BYW
Frame #4609
BYW
4610
BYW
4611
BYW
bright yellow
red
4612
yellow
red
4613
yellow
red
4614
0.14mm
pale red
very faint
4615
red
yellow
BYW
BYW
BYW
BYW
bright yellow
BYW
BYW
bright yellow
very faint reddish
very faint
pale red
4622
BYW
4628
bright whitish yellow
(at top of frame)
0.14 mm
GONE off frame for four frames
bright whitish yellow
4634
(lower left side of frame)

Fig. A1.

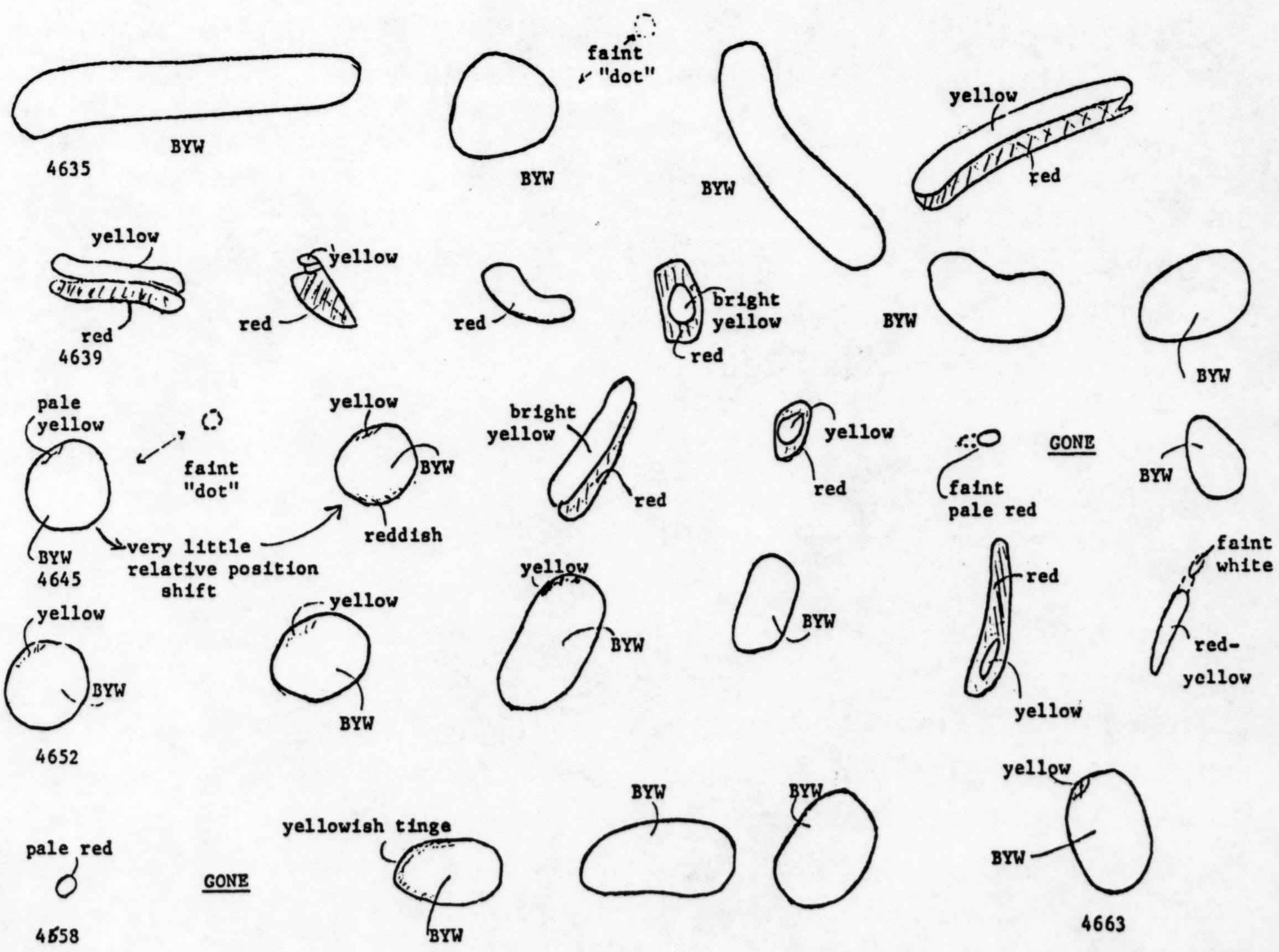

BYW
4635
faint "dot"
BYW
BYW
yellow
red
yellow
red
4639
yellow
red
red
bright yellow
red
BYW
BYW
pale yellow
faint "dot"
BYW
4645
very little relative position shift
yellow
BYW
reddish
bright yellow
red
yellow
red
faint pale red
GONE
BYW
faint white
red-yellow
yellow
yellow
BYW
BYW
4652
yellow
BYW
BYW
pale red
4658
GONE
yellowish tinge
BYW
BYW
BYW
yellow
BYW
4663

Fig. A2.

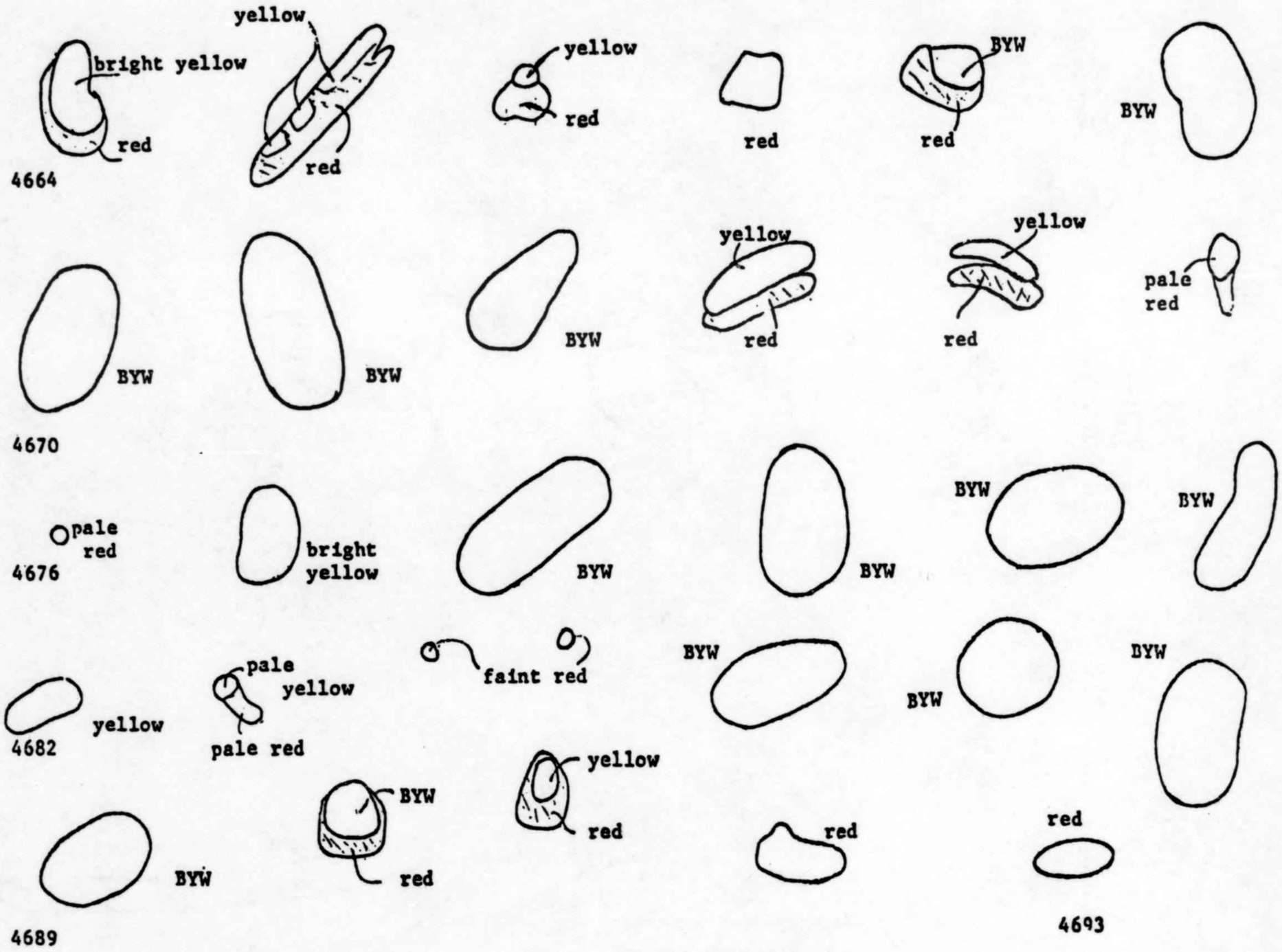

yellow
bright yellow
red
4664
yellow
red
yellow
red
red
BYW
red
BYW
BYW
BYW
BYW
yellow
red
BYW
yellow
red
pale
red
4670
pale
red
4676
bright
yellow
BYW
BYW
BYW
BYW
pale
yellow
yellow
4682
pale red
faint red
BYW
BYW
BYW
yellow
red
BYW
red
4689
red
4693

Fig. A3.

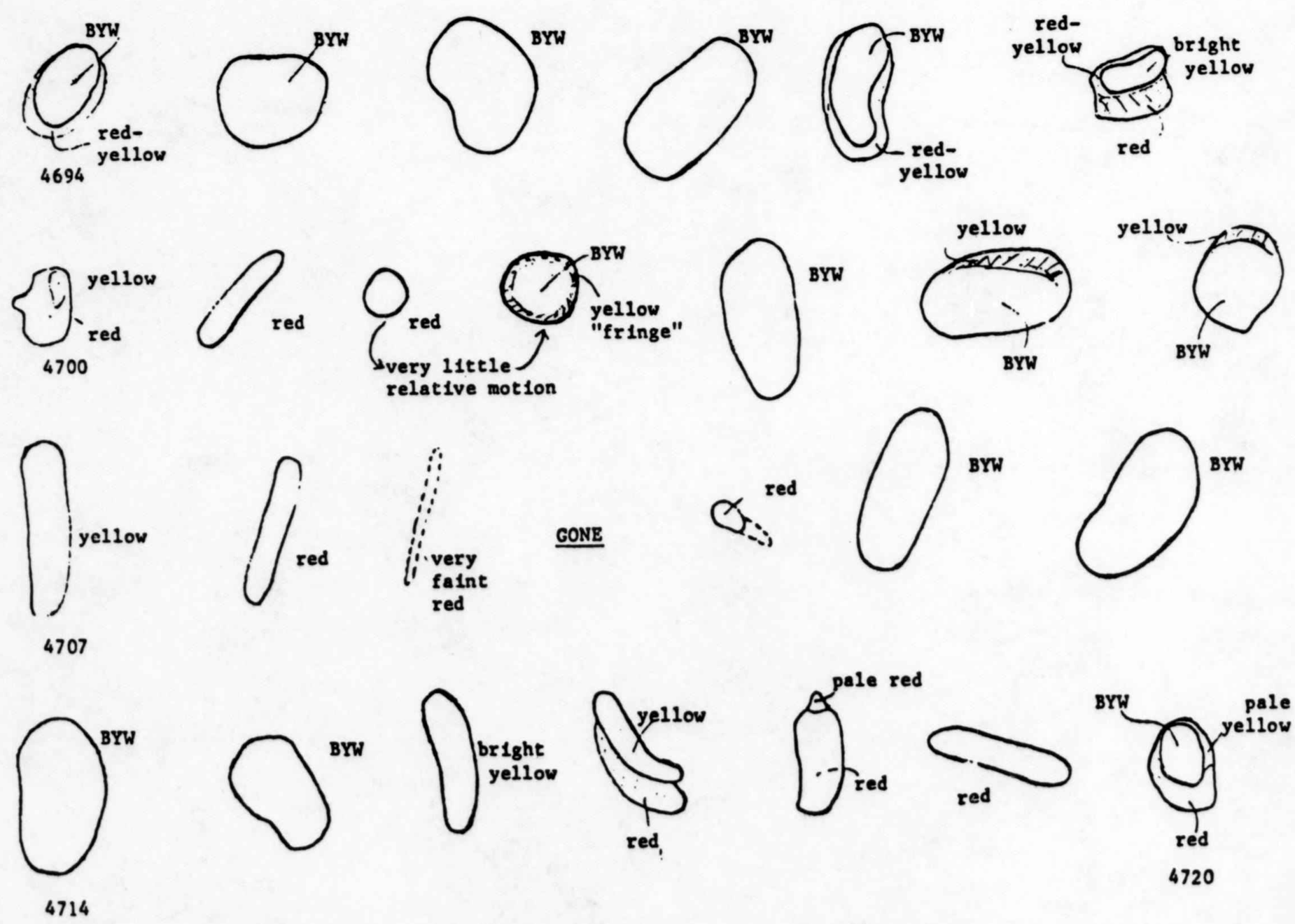

BYW
red-
yellow
4694
BYW
BYW
BYW
BYW
red-
yellow
red-
yellow
bright
yellow
red
yellow
red
4700
red
red
very little
relative motion
BYW
yellow
"fringe"
BYW
yellow
BYW
yellow
BYW
yellow
red
4707
red
very
faint
red
GONE
red
BYW
BYW
BYW
bright
yellow
yellow
red
pale red
red
red
BYW
pale
yellow
red
4714
4720

Fig. A4.

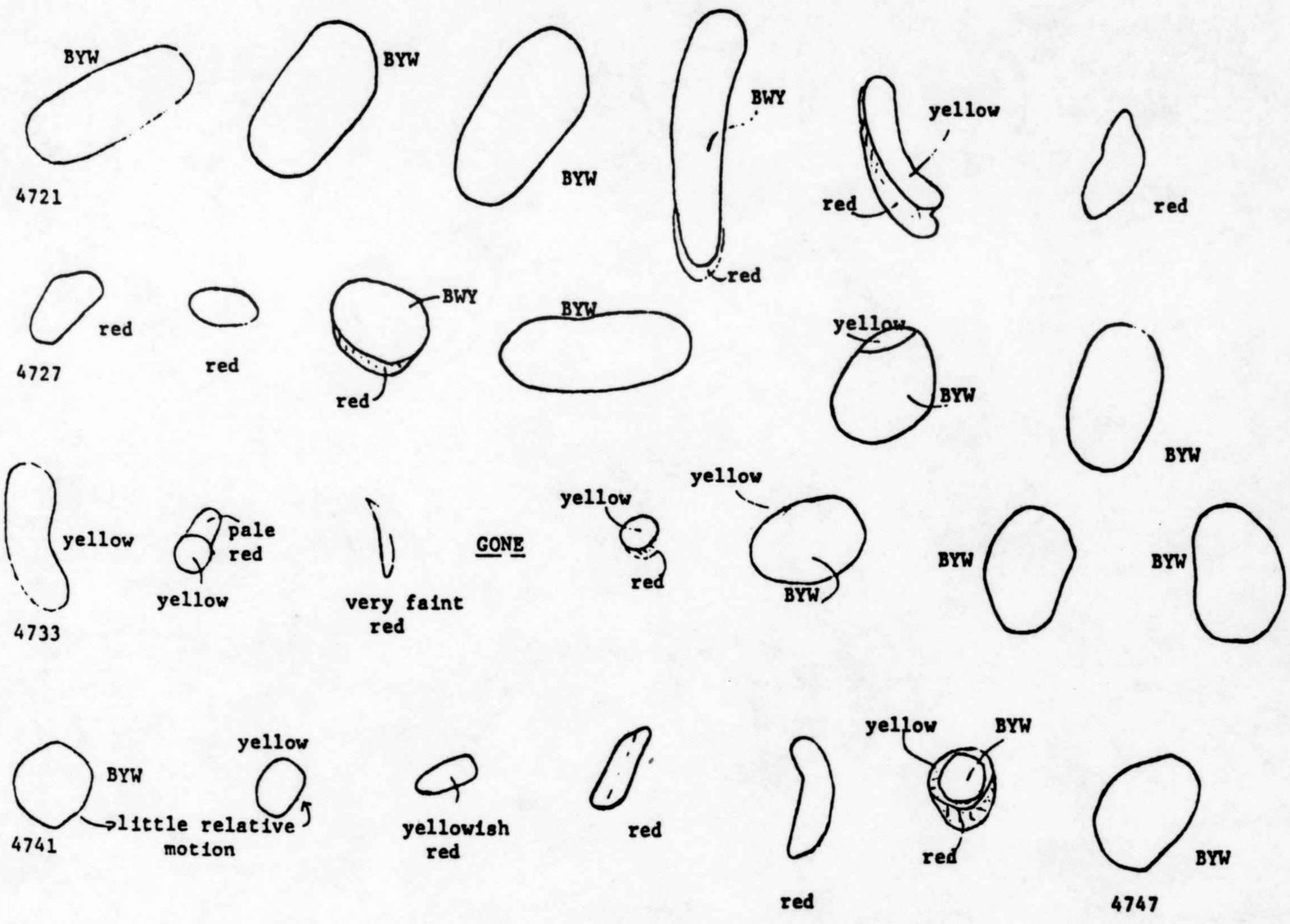

BYW
4721
BYW
BYW
BWY
red
yellow
red
red
4727
red
red
BWY
red
BYW
yellow
BYW
yellow
pale red
4733
very faint red
GONE
yellow
red
yellow
BYW
BYW
BYW
BYW
little relative motion
4741
yellowish red
red
red
yellow
BYW
red
yellow
BYW
4747

Fig. A5.

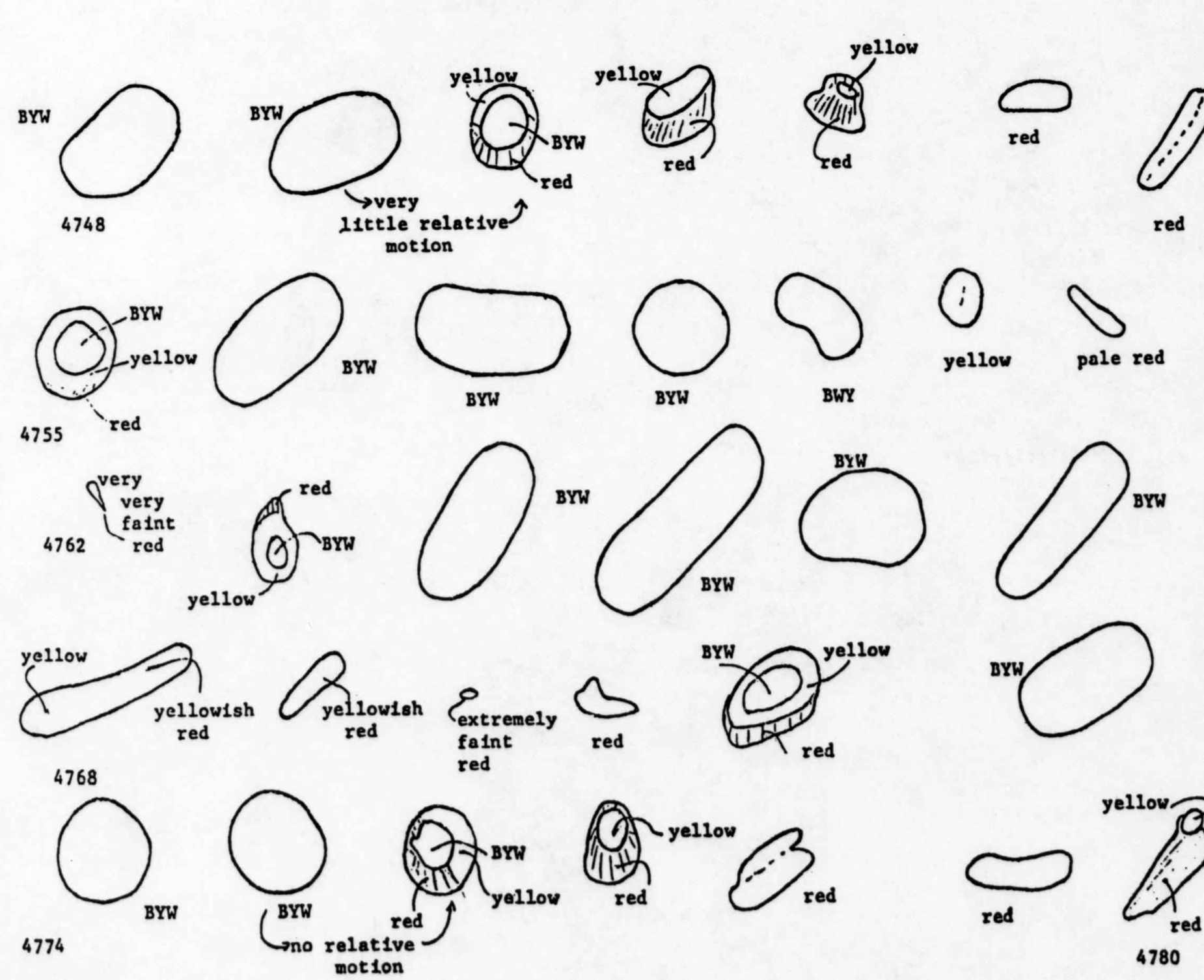
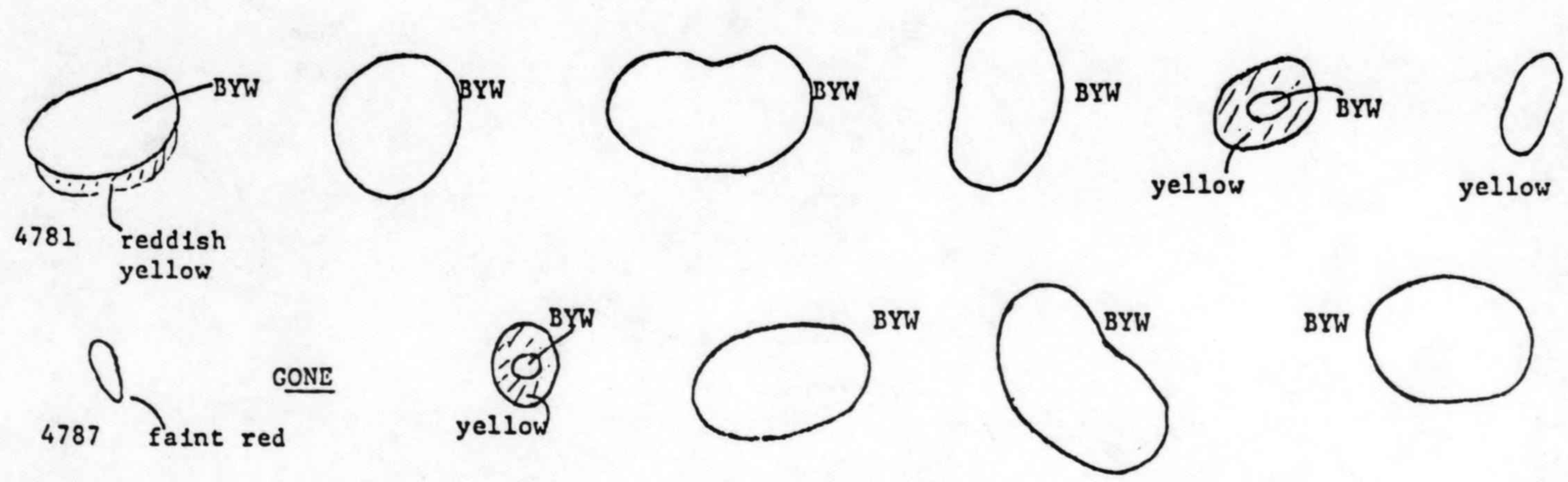

BYW
4748
BYW
very little relative motion
yellow
BYW
red
yellow
red
yellow
red
red
red
BYW
yellow
4755
BYW
BYW
BYW
BWY
yellow
pale red
very very faint red
4762
red
BYW
yellow
BYW
BYW
BYW
BYW
yellow
yellowish red
yellowish red
extremely faint red
red
BYW
yellow
red
BYW
4768
BYW
BYW
no relative motion
red
BYW
yellow
red
yellow
red
red
yellow
red
4780

Fig. A6.

B. Maccabee



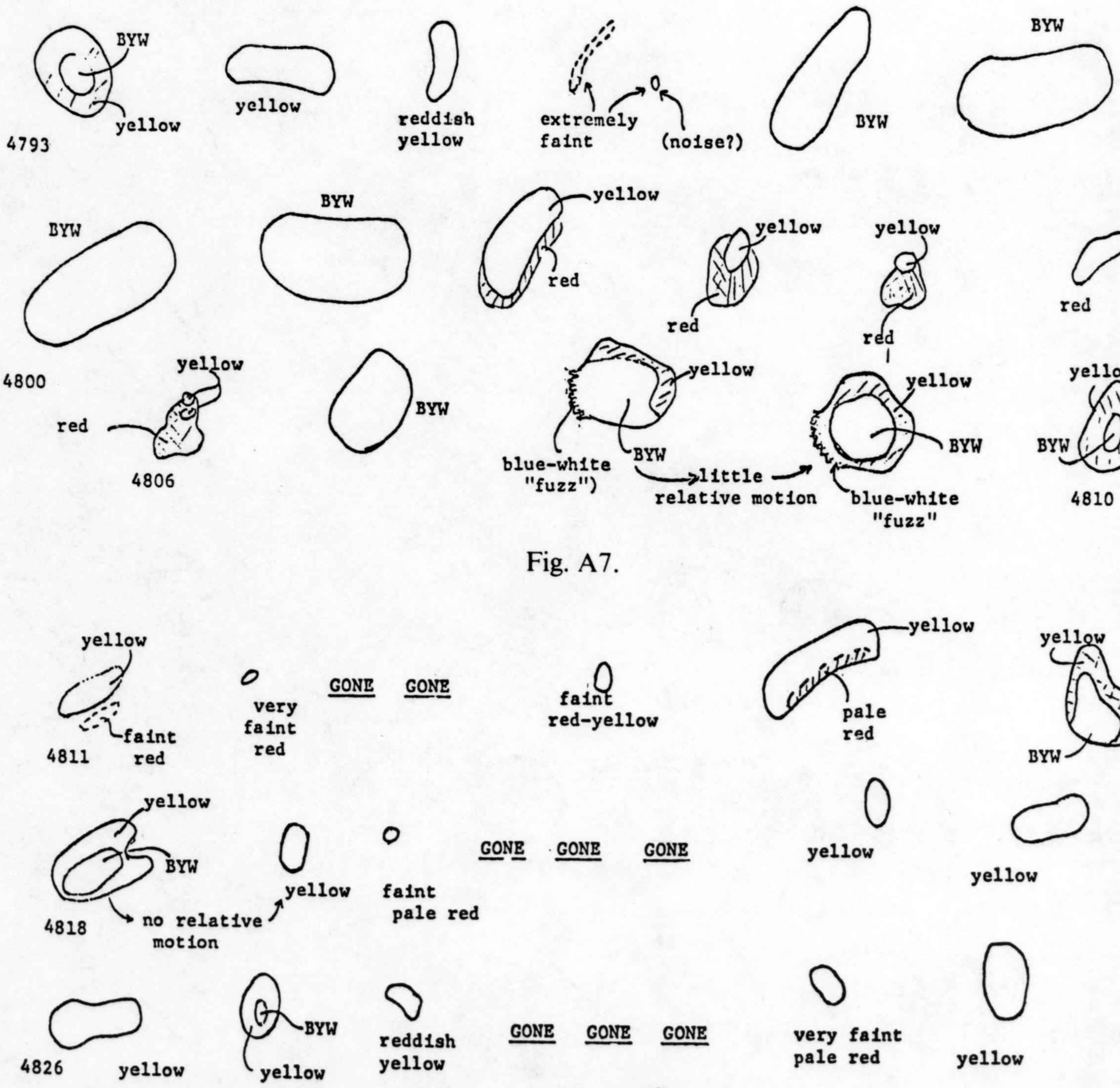


Fig. A7.

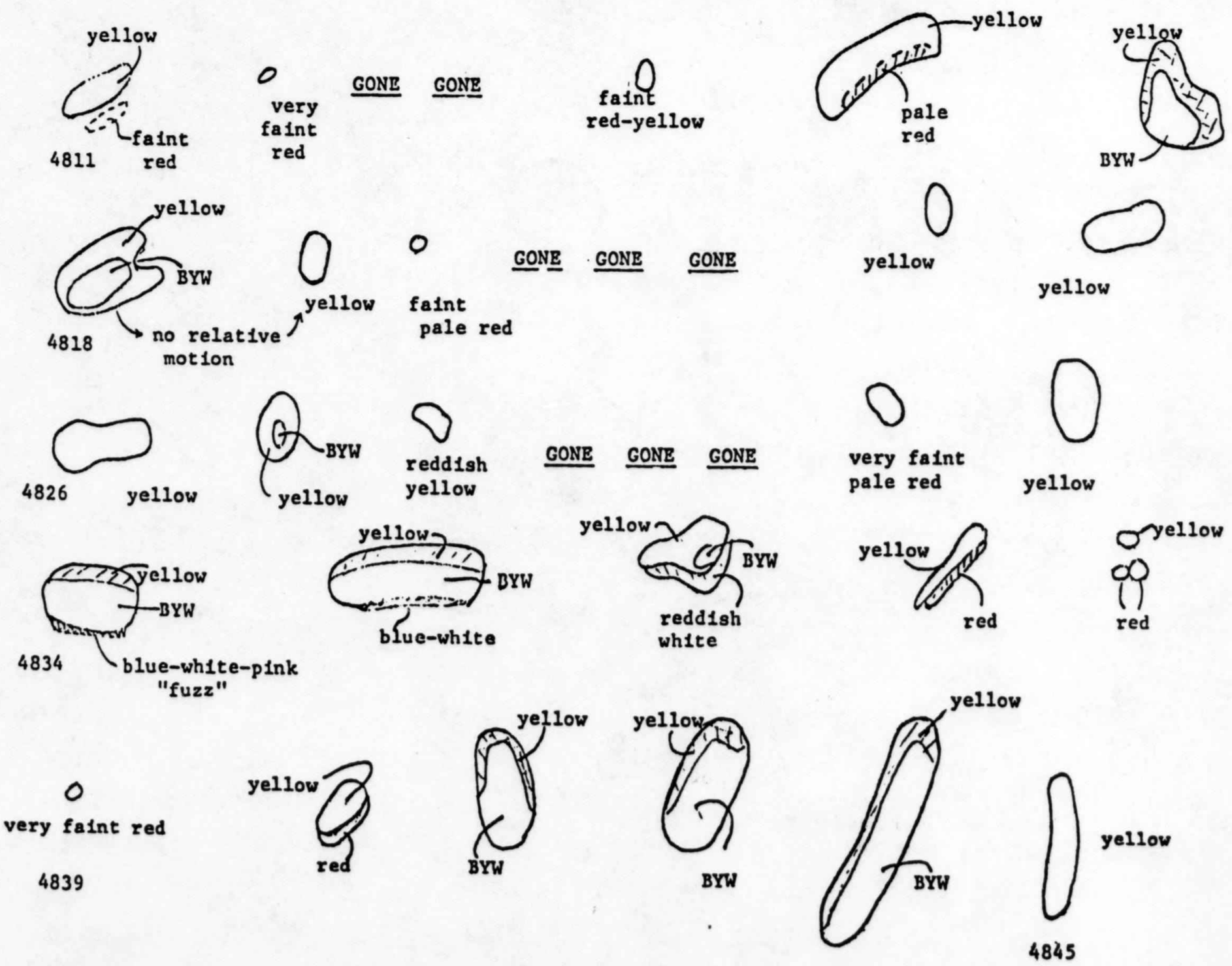


Fig. A8.

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Parapsychology: The Continuing Impasse

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Abstract—The question here at issue is whether there are any paranormal claims that deserve to be taken at face value. The two main grounds for skepticism are: (a) that they are incompatible with the prevailing scientific world view and (b) that the evidence is inadequate. Neither of these two objections, however, is fatal. Hence the impasse. If it is the case that there *are* no paranormal phenomena it is unlikely that this impasse will ever be broken if only because so many people are convinced that they have had personal experience of such phenomena. If, on the other hand, paranormal phenomena *do* exist, the impasse could well be broken provided that certain conditions were fulfilled. At present we still have no assurance that such conditions will ever be fulfilled.

That a century of psychical research should have failed to establish the existence of the phenomena it purports to study, at least to the general satisfaction of all concerned, is surely food for thought. There are two possible explanations. It could be the case that there *are* no such phenomena, that the evidence is deceptive or that those who believe in them are deluded. Alternatively, it could be that the phenomena are genuine but are of a nature which systematically eludes conclusive proof. I shall leave open here which paranormal claims should be included in the argument. Obviously these range widely both in kind and in magnitude. What is basically at issue here is whether there are *any* cases, whatsoever, of an individual acquiring information about the external world *otherwise* that via the known sensory channels (= ESP or psi-gamma) or, likewise, of an individual influencing events in the external world *otherwise* that via the known motor channels (= PK or psi-kappa). Collectively such cases exemplify "psi phenomena."

In this paper I shall argue that, in present circumstances, there is no way in which we can decide conclusively whether psi phenomena are real or illusory. Thus the objections raised by skeptics are not only understandable, they are legitimate (I shall not deign to discuss those objections—and they are plentiful—which are *not* legitimate). At the same time *some* of the evidence put forward by parapsychologists remains valid evidence that has withstood criticism. Hence the impasse. Which position we adopt will depend on which criteria we apply and what weight we attach to the different criteria. This leaves open the question as to whether the impasse will ever be resolved. If, in fact, there *are* no psi phenomena, it is most unlikely that the fact would

ever come to be universally acknowledged if only because there are, and always have been, a large number of people who claim to have had first-hand experience of such phenomena and are very unlikely to be persuaded by any abstract arguments that they must be mistaken. The most, therefore, that skeptics can hope for is that parapsychology, as a discipline, will eventually fold up for lack of any hard data as standards of evidence become increasingly stringent. If, on the other hand, psi phenomena are real, there is always the possibility that ingenuity will find a way of demonstrating this so forcibly as to overcome all reasonable reservations. I shall conclude my paper by considering some hypothetical contingencies that might have this effect although I am not optimistic about their becoming actual.

Let us start by considering the principal reasons for discounting the existing evidence. These can be conveniently discussed under two main headings: (a) the alleged incompatibility of psi phenomena with the existing scientific world view and (b) the technical inadequacies of the evidence as such. Consider the first reason. What I am here loosely calling "the scientific world view," the product of several centuries of science, is not something that we should ever take lightly. Modern science, after all, represents the most powerful and coherent body of knowledge that has ever been created. It does seem odd, therefore, that there should be an entire class of phenomena that has so far remained obdurately outside its purview. There are, however, two points to consider before jumping to the conclusion that such phenomena are self-evidently nonsensical. First, science, unlike religious dogma, does not purport to express a set of immutable truths. Science, as we know, changes as it advances and, already, there is a vocal body of informed opinion which regards a universe based on quantum mechanics as more hospitable to paranormal claims that one based on classical physics. Whether this view is sound or not, whether it relies too much on extrapolating from the subatomic level to the macroscopic world, it is still a matter of ongoing controversy and is, at least, worth noting. The second and, in my opinion, more important point is that even if we grant that science gives a fundamentally correct account of the physical world, it does not follow that it represents a comprehensive account of reality in all its aspects, in particular those aspects we may identify with the domain of mind. For example, the physical sciences have nothing to say about the existence of consciousness although it is as much a fact of nature as any other. The most that they can do is to fall back on the well worn epiphenomenalist view of mind according to which subjective experience is no more than a fortuitous byproduct of its concomitant brain states. Now, the epiphenomenalist view of mind may well be correct but one can hardly deny that it is wildly counter-intuitive. Intuitively we all believe that our wishes and intentions do, as such, have an effect on our behavior. This, alone, should put us on our guard. It could be, therefore, that psi has failed to find a place in the accepted scientific scheme of things precisely because it is a function of mind as opposed to brain. This is speculative but, granted the evidence for psi and the persistence of the mind–body problem, such speculations should not be ignored.

Let us turn, now, to the evaluation of the evidence. Evidence for psi phenomena are of three basic sorts: (a) spontaneous cases such as may occur

unexpectedly in the course of daily life, (b) observations on special individuals carried out under prearranged conditions, and (c) experiments designed to test specific hypotheses where the results can be evaluated statistically and the hypotheses can be either confirmed or rejected. It is evidence of this third kind that counts with the scientific community. Evidence of the first kind can readily be demoted as mere "anecdotal evidence," while evidence of the second kind, traditionally associated with mediumistic seances, can always be ascribed to trickery or sleight-of-hand whatever the safeguards. Experimental parapsychology, as this has been practised since the early 1930s, with the advent of J. B. Rhine at Duke University, recognized this predicament and strove to produce just the sort of evidence that is best calculated to impress the scientist (Mauskopf & McVaugh, 1980). Unfortunately this has proved far more difficult than Rhine or any of the other pioneers had envisaged. In the first place, it is never easy to obtain non-random scores on tests for ESP or PK. Secondly, even when such scores are forthcoming, it is never easy to replicate them and it is still harder to get corroboration for them from other workers at other laboratories. Thirdly, even the most successful scorers have been able to produce only very slight deviations from chance expectation however significant these may have been in the long run. The marginality of the laboratory results contrasts sharply with the often dramatic nature of the spontaneous phenomena or with cases of poltergeist disturbances or of physical mediumship. It suggests that the laboratory may well be the least favourable milieu for the production of paranormal phenomena.

In the light of such considerations some authorities now question the basic justification of the experimental approach. They like to point out that there are all sorts of facts about human behavior—that people enjoy themselves, that they fall in love, that they quarrel and compete with one another, that they suffer remorse, disappointment, grief, and so on endlessly—yet none of these facts that go to make up the very fabric of human life could be demonstrated in the laboratory situation except in the most stilted and artificial way. Now it could be, so the argument goes, that the conditions necessary to trigger a psi phenomenon, a combination, perhaps, of the right individual in the right situation, could never be deliberately produced for experimental purposes. And even if, by a stroke of luck one were to do so, the chances of this happening again, let alone regularly and consistently would be negligible. (A recent critique along these lines by an American philosopher may be found in Braude, 1986, especially in Chapter 1 "The Importance of Non Experimental Evidence.")

Suppose, then, that for this or for any other reason, psi phenomena do not lend themselves to standard experimental treatment. Does this mean that belief in their existence must remain forever optional or, like religious belief, a matter of personal faith? There are, of course, non-experimental sciences: astronomy, geology, palaeontology, and so forth but, although they are sometimes cited in this connection, they are not really comparable to parapsychology, first because the observations on which they rest are open to all concerned and, secondly, because they rest on a firm body of theory which does have experimental support. A more fitting analogy is the study of history.

Historians reconstruct the past on the basis of documents but, though the documents as such are part of the public domain, their interpretation rests ultimately on an evaluation of human testimony as does parapsychology. There is thus nothing inherently objectionable about studying spontaneous cases, it is all a matter of applying the relevant criteria of evidentiality much as an historian or a lawyer might do. Of course a confirmed Humean could still object that no testimony in the world could ever establish the reality of a miracle, on the grounds that it would always be more probable that the testimony was false than that the miracle was true. But that would be to make a dogma of skepticism and one, moreover, which could not be sustained in the special case where one was, oneself, the witness.

As for the study of exceptional human faculties of the kind we associate with so called "psychics," this represents a sort of half-way house between the strict experimental approach and the study of spontaneous phenomena. As in the experimental approach all the necessary preparations and safeguards can be made beforehand in anticipation of what is expected to happen during the session and appropriate apparatus and recording instruments can be used to supplement human observation. Yet, in the last resort, what actually *does* happen, and exactly when, remains unpredictable so that every session constitutes a unique episode. But, again, this cannot be a reason for rejecting such evidence out of hand. The serious objection to the claims of the "psychic superstar" is the difficulty of distinguishing between ostensibly paranormal manifestations and ordinary conjuring or sleight-of-hand. The fact that nearly all physical mediums who were intensively investigated were caught at one time or another faking their phenomena exacerbates this problem as does the fact that conjurers have been able to simulate their phenomena to some degree of verisimilitude. There is also the fact that substantial monetary prizes have been offered at various times for anyone who could demonstrate a paranormal phenomenon to the satisfaction of the challenger but so far no such prizes have been won. On the other hand challenges have been issued at various times to members of the magicians' fraternity to duplicate what the successful psychic is credited with doing under similar conditions and these challenges have been declined.[1] The usual excuse in these cases is that conditions never would be similar, the attitude of the investigator would be different or the self-styled psychic would have specialised in that particular trick. At all events, the question of trickery, like all the other questions concerning the paranormal, has reached a stalemate. On the one hand trickery can always be offered as a counter-explanation, if only because trickery is an open-end concept, no one can define its limits. On the other hand, to have substance as a counter-explanation it is necessary to demonstrate how trickery could have been used

[1] An example from recent times is the case of Ted Serios and his alleged psychic photographs, see Eisenbud (1967). Serios was widely dismissed as fraudulent when it was shown that similar images could be projected onto polaroid film using a small lens device held in the hand while the shutter was released. No one, however, volunteered to demonstrate how this could be done undetected in the conditions which obtained when Serios was tested by Eisenbud and his colleagues at the University of Denver despite repeated challenges which Eisenbud made to the magician, James Randi. Hence the stalemate.

in the circumstances and, unfortunately, dialog is hampered at this point by the fact that the professional secrecy of the magicians makes them reluctant to reveal the tricks of their trade.

Where, then, does this leave us? We may agree with the skeptic that the onus of proof rests with the claimant and the more extravagant the claims the stronger must be the proof. On this reckoning we may agree that proof of the paranormal has not been forthcoming. The pragmatic and interesting question, however, is whether it is worth while pursuing the problem and to this question I would emphatically say yes. Parapsychology, after all, is not a creed. What the parapsychologist asks is to be given sufficient funding to pursue further research. Given the sheer bulk of the evidence that has come to light over the past hundred years and the impressive quality of that evidence at its best, this is surely not an unreasonable demand.

On the assumption that there are such things as psi phenomena, what is the prospect of this being established unequivocally in the future? Let us look again at our three types of evidence. On the experimental front the main impediment, hitherto, has been that the effects reported were (a) too weak and (b) too inconsistent. With respect to (a) there are grounds for hope in the ever increasing sophistication of the methodology and technology of the modern experimental parapsychology laboratory. Thus, with the advent of the computer, much of the test procedure, including the randomization and scoring, can be automated thereby eliminating one of the chief sources of human error and bias. One has only to compare the typical set-up that is currently used to test PK, using an electronic binary random event generator as the target system, with the tumbling dice of yester-year to appreciate the increased scope of modern parapsychology. The sheer amount of data which can be generated using the new system can help to compensate for the weakness of the PK effect itself. (A good example is the lengthy series of PK tests carried out at the Princeton Engineering Anomalies Research Laboratory of Princeton University, see Nelson, Jahn, and Dunne, 1986.) There still remains, however, the question of consistency. Part of the problem is that psi appears to be bidirectional in its operation. That is to say, it is just as likely to reveal itself in significantly *below* chance deviation as in a significantly above chance score. To make matters worse, scoring is liable to fluctuate between the positive and the negative or "psi missing" mode. Recently, however, various strategies have been explored with some success whereby the subject can try to identify which phase is operating and to act accordingly. (The problem has recently been taken up by a group of Dutch experimental parapsychologists, see Michels, 1987.)

With respect to the spontaneous evidence, although this is still as plentiful as it has ever been, it is doubtful whether any changes can be expected with respect to its credibility, it looks as if it would always be possible to doubt it with impunity. With respect to the case of exceptional individuals, however, there is always the tantalizing possibility that another Daniel Home will emerge, that is, someone whose paranormal powers are so strong that they can withstand the most intensive investigation and the most critical scrutiny. (It is noteworthy that no one who actually attended a seance with Home

doubted that he had witnessed paranormal phenomena such as table levitations, however skeptical initially. But Home died in 1886 and, inevitably, doubts have been expressed ever since, see Hall, 1984.) In this connection one has only to recall the flurry which Uri Geller created, not only among the general public, but even in scientific circles when he first appeared on the international scene in the early 1970s. Geller, however, for whatever reason, was always more interested in publicity than in research and eventually withdrew entirely from serious investigation. He is now generally regarded, rightly or wrongly, as a pure impostor. At present there is no sign of such another psychic superstar, but it does not require much imagination to appreciate, in these days of video-recording, the impact which such an exceptional individual could make.

I would finally like to mention very tentatively one further possibility that could arise if another exceptional individual were available. This is the creation of a permanent paranormal object. The prime example of such an object would be the seamless linkage of two wooden rings each made from a different timber. There are, in fact, claims in the literature for such objects although none is now extant.[2] The virtue of such an exhibit from the evidential standpoint is that it would be independent both of the conditions in which it was produced and of those who vouched for it. It could be passed round from one laboratory to another for minute examination and, if no flaw in it could be detected this, in itself, would amount to as conclusive proof of paranormality as could be imagined.

But, at the present time, these are all hypothetical possibilities which I mention mainly to show that the impasse I have been discussing is not, in principle, impassable.

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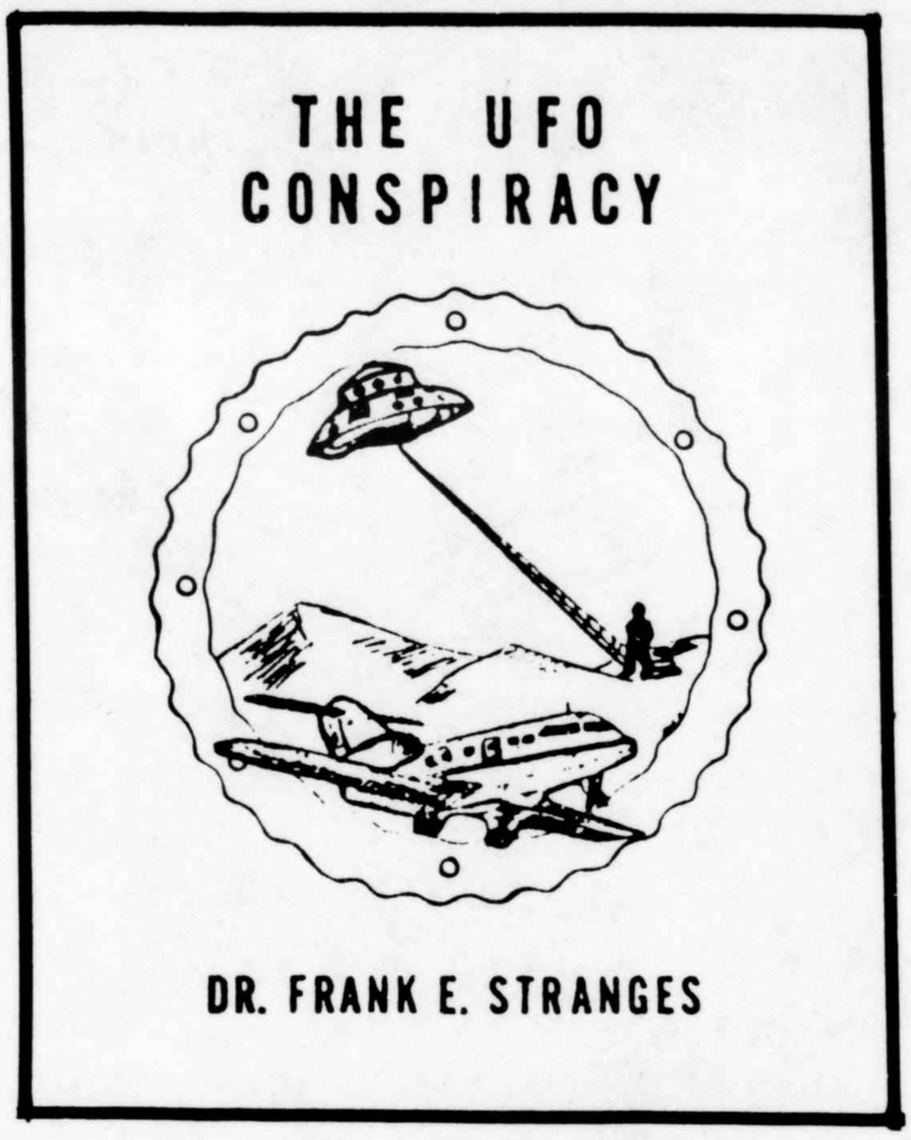
[2] Notably in the case of the medium "Margery" who flourished in the 1920s and 30s. Although today widely regarded as fraudulent, her case has recently been reassessed by Maria N. Nester (daughter of the principal investigator, Mark Richardson) who is preparing a biographical study of her. According to Nester (1985): "The paranormal linkage of wooden rings is possibly the most mysterious of the Margery phenomena. Two sturdy rings made of different kinds of wood, placed on the seance table linked and unlinked without human touch. To author's knowledge, this spectacular (and often repeated) phenomenon has never been explained." At one point, it would seem, the rings were X-rayed without any flaw being apparent and were put on exhibition at the headquarters of the American Society for Psychical Research in New York.



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