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REPETITION OF THE FRENCH
PRECOGNITION EXPERIMENTS WITH MICE¹

By WALTER J. LEVY, JR., L. ALLEN MAYO, JR.,
EVE ANDRÉ, AND ANITA McRAE²

ABSTRACT: Six series of exploratory precognition tests with mice and birds were carried out with the purpose of confirming the work of Duval and Montredon in France. The experiment tested the ability of the animals to use their psi to avoid jumping into whichever side of their cage would be electrified on each forthcoming five-second trial. Out of 1,154 random-behavior trials, the animals made a hit (avoided shock) 612 times ($CR = 2.06$; $P < .05$, two-tailed).

Since the apparatus monitored the animal's position all the time it was in the cage (in the French work there was only selective monitoring), it was possible to reanalyze the data for evidence of psi in the animal behavior patterns. It was found that the animals significantly avoided shocks when the just-previous trial had been a nonshock trial ($P < .015$). If the just-previous trials were shock trials, however, the number of hits was not significant. Another test effect was that trials following a period of low activity (fewer than three jumps back and forth during the intertrial period) were also significant ($P = .015$); trials following three or more jumps were not. When both successful conditions (nonshock and low activity) were combined, they gave an even more significant separation ($P = .006$).

Mr. Levy is a second-year medical student at the Medical College of Georgia; Mr. Mayo is a graduate student in zoology at Duke University; Miss André is employed as a librarian in Victoria, Australia; and Miss McRae is a research assistant at the Institute for Parapsychology.—Ed.

I. THE PRELIMINARY SERIES

In 1968, two French biologists, Duval and Montredon,³ published a report on experiments which gave evidence that mice were able to

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³ These are assumed names.

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use precognition to avoid shock as they jumped back and forth in the two sections of a mildly electrified cage (1).

This work was outstanding in a number of ways. It won the McDougall Award as an important scientific contribution; it came from a respected biological laboratory; and it was based on work with a species not hitherto tested by researchers, who had already worked with cats, dogs, and pigeons. The French work was also the first precognition test with any kind of animal and—what is more important perhaps—it was the first completely automated test. At least, it went further than any previous work toward eliminating psi on the part of the experimenter which, as Rhine and Feather had pointed out (3), stood in the way of a conclusive interpretation of the earlier evidence for psi in animals.

From the point of view of methodology in psi research, however, the most effective advance in the French research was its reliance on the technique for selecting “random-behavior” trials, an idea first introduced by Osis and Foster (2) but not applied as a primary experimental device hitherto. This is a technique for sifting out those trials in which it is obvious that ESP probably could not have been operating. Osis and Foster had observed that when side habits dominated the behavior of the cats being tested, the ESP scores were lower than on the other trials. Duval and Montredon applied the same principle to the mouse work; by eliminating those trials in which the animal was obviously responding to non-psi factors—such as shock—they obtained more significant results than when these trials were included in the total. This procedure was reliable and efficient, and it was an important step forward for parapsychology.

Over and above these specific points of importance in the French work was the experimenters' own expressed hope that they had made some progress toward discovering the “guinea pig” that is needed for the greater effectiveness of the research program in parapsychology. Their anticipation played a large part in motivating the authors of the present work to attempt a replication. The automation in the French tests seemed to have eliminated the experimenter as a source of psi and to have centered on the animal as the producer of the results; and it appeared reasonable that replication with other rodents could be expected to follow.

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The individual differences of the French mice were not a conspicuous factor in the results. Since different strains had been used, it seemed likely that there would be no great experimental risk in using various strains of American mice in the attempted replication. Similarly, there were modifications in the construction of the apparatus, which was built with the aid of information provided by the French authors.

The first experiment was begun and largely completed during the summer of 1969 at the Institute for Parapsychology, although a complete analysis of the results had to be left for a later time, since the two senior authors had enrolled in graduate school.

The general procedure of this experiment was to put the test animal into a box which was divided by a low partition into two equal parts. The floor grid of the box was wired electrically and there was an electronic selector which randomly chose one side or the other as the side of the box which would receive an electric charge for each trial. The animal was free to jump from side to side. If it chose the "wrong" one, it received a shock; if it chose the "right" one, it did not.

An important aspect of the experiment, as in the French work, was the elimination of those trials in which psi probably was not operating. This "nonrandom" behavior was of two kinds: static and mechanical. A "static" trial occurred when the animal, even though shocked, continued to stay in the same side of the box, possibly because it did not feel the shock, or because it was too tired or too frightened to jump, or for some similar reason. If the animal was shocked and immediately jumped in response, this was to be expected; it was therefore called "mechanical" behavior. The "random" trials, on which the evaluations were based, were the ones in which the animal jumped for no apparent reason. Any change of side after the five-second shock period qualified the following trial as random.

The selection of random-behavior trials in the present experiment differed from that in the French work in the following way: the French monitored the animal's position only at the start of each five-second trial. If the animal jumped once after the trial, their equipment recorded a change of sides; but if it jumped twice (or any even

number of times) before the next five-second trial, it would be in the original side of the cage and would appear not to have jumped at all in the interval between trials. Our apparatus recorded the animal's position continually and therefore made it possible to sift out many more random-behavior trials from the total data; and if the scoring rate was high, a greater incidence of psi would be indicated in the data.

A further difference caused by their selective monitoring was that their animal was in the dark except immediately before and during the five-second trial. With our apparatus, the light beams, which were the animal's source of light, remained on constantly.

Apparatus⁴

The testing apparatus comprised six components: a shock box, a position indicator, a random number generator, a logic circuit, a shock control, and a recording apparatus.

A. The shock box, or testing cage, was a clear, bottomless lucite box measuring $6\frac{1}{2}$ " x $6\frac{1}{8}$ " x 6". It rested on an electrically wired grid. A low barrier $1\frac{1}{8}$ " high and $\frac{1}{4}$ " thick divided the cage into two halves (A and B in Figure 1), each half wired independently. (See Figure 1.)

B. On those sides which were at right angles to the barrier, there were mirrors, used in conjunction with a light source and a photocell in each half of the box to indicate the animal's position. If the reflection of the light beam in the mirror was cut, the animal was considered to be present; if it remained intact to shine on the photocell, the animal was considered not there. The shock box, with the position-indicating apparatus, was in a dimly lit room separate from the remainder of the equipment. The separation varied from an adjacent room, at first, to one that was two floors distant later in the experiment.

C. The random target generator was an electronic device which randomly picked side A or B as the target side for each trial. There was one target selection (and therefore one trial) every minute. The random number generator produced a rapidly oscillating sine wave

⁴The main work of designing and constructing the apparatus, including the random number generator, was done by Allen Mayo at the Institute for Parapsychology.

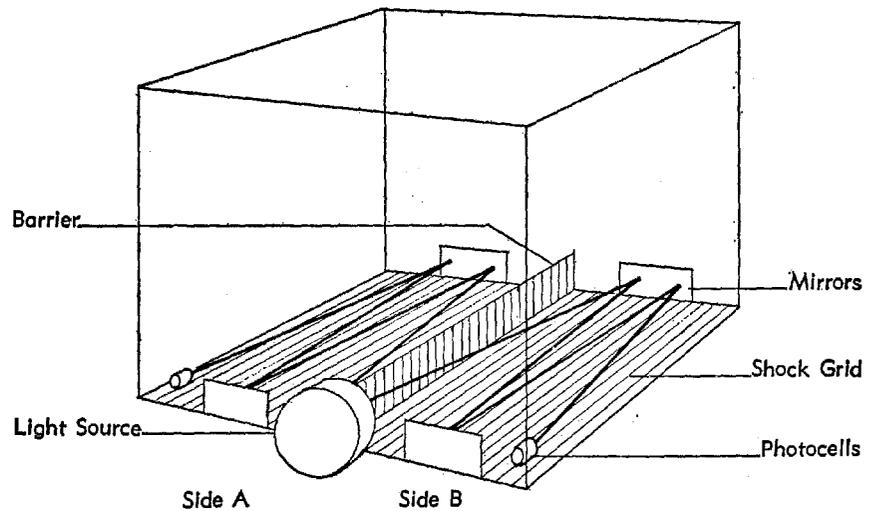


FIG. 1. Diagram of testing cage.

which caused a flip-flop circuit to go from state A to state B, or the reverse, with the start of each full cycle it produced. Thus, when a target was called for, the flip-flop stopped in either state A or B. The randomness came from the fact that the error of the timing device calling for a target was tens of thousands of times greater than the period of the wave. Thus, the side at which the flip-flop stopped was not predictable other than by a chance distribution.

The randomness of the random number generator was checked before the first series started. The results of several thousand trials showed only a chance variation ($CR = .7$), with slightly more targets for side B than side A. A further randomness check was carried out later on the actual targets generated during the experiment. The CR of .87 indicates that the distribution between the A and B sides was well within the range of chance variation, with slightly more targets on side A than side B.

D. The logic circuit integrated the information from the target generator and the position indicator and sent a shock to the proper side if one was needed. If the position of the animal and the selected target side matched, a five-second shock was sent to the side the animal was in at the initiation of the trial, and no shock was sent

to the other side. If the target and position did not match, no shock was sent. Thus, the animal could be shocked only if he was in the target side at the start of a trial, and only as long as he remained in that side. However, at times the animal might have his body on one side and his tail on the other, or he might be changing sides, so that his position was ambiguous. In such a case both beams or neither beam would be cut and no target would be chosen. When such a "mistrial" occurred, no shock would be sent to either side. This part of the apparatus provided safeguards against the possibility that mechanical failure, such as a burned-out bulb, might give spurious results. It was difficult for an animal to jump into a shock, since he would have to jump at least twice within the five seconds to land in the side of the cage that was given a charge.

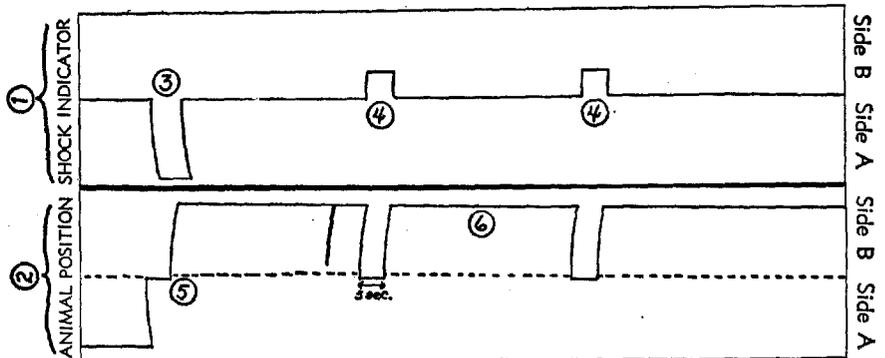
E. The shock-control device determined the intensity of the electric charge given to the animal. The shocking voltage was individually adjusted for each animal at each run to give a voltage sufficient to make him move away but not react strongly. During the first part of the series the shock grid was charged by alternating current; later, the voltage was supplied by a battery in order to provide better control of the intensity of the shock.

F. The recording device was a Grass two-channel polygraph, one channel of which continuously recorded the animal's position and the other, his position at the time of the trial as well as whether or not a shock had been sent to the animal. From this information the target chosen could be determined.

The lower channel on the chart was the position indicator. (See Figure 2.) Side A of the cage was recorded at the bottom of the channel; side B, just above it. The position of the pen indicated the animal's position. For a move of the pen to count as a change of position, it had to go at least 1 cm. past the midline into the opposite half of the area. Each minute, the pen automatically centered for the five-second trial period, and in checking the data this allowed a machine judgment (rather than a human judgment) to be made about what position the animal was in at the initiation of the trial and whether a jump occurred during the trial period.

The upper channel was a shock indicator. A full-scale deflection of the pen indicated that the animal had received a shock; a half-

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1. Shock Indicator: Shows which side of the cage the animal is on at the time of the trial and whether it was shocked or not.
2. Animal Position Pen: Records the animal's position at all times, except during the 5-second trial period. The pen moves from side A to side B and back as the animal moves. During the 5-second trial period the pen moves to the center to indicate the trial.
3. Miss: When the animal makes a miss, it is shocked and the shock is recorded on the Shock Indicator by a long stroke of the pen to the side where the animal is located at the time of the trial.
4. Hit: When the animal makes a hit, there is no shock and this fact is recorded on the Shock Indicator by a short stroke of the pen to the side where the animal is located at the time of the trial.
5. Mechanical Behavior: Results when the animal jumps only in immediate response to a shock.
6. Static Behavior: Indicates no jumping activity between trials.
7. Random Behavior: Results when the animal jumps from side to side without an apparent reason; i.e., not in immediate response to shock.
8. Mistrial: Indicates that at the time of the trial either both light beams reached the photocell unbroken or that neither reached it. The animal may have been on the barrier in this case, and no shock was sent to either side of the cage.

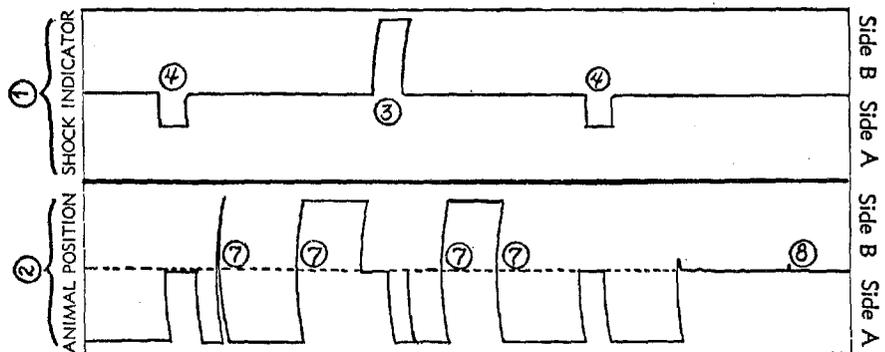


FIG. 2. Facsimiles of two polygraph charts.

scale deflection indicated no shock. If the pen deviated toward the center line (downward) it meant that the animal was on side A at the beginning of the five-second trial; if the pen deviated away from the center line (upward) it meant that the animal was on side B. From this information it could be deduced which side had been the target. During a mistrial no deflections of the upper pen occurred, since no target and shock decisions were made. This also allowed a machine judgment as to the occurrence of a mistrial.

Except for the recorder and the testing cage, the equipment was enclosed in a box surrounded by an inch-thick layer of insulation.

The animal species used in the experiment were 14 white mice (*Mus musculus* Swiss Webster) and 9 jirds (*Meriones unguiculatus*).⁵ They were initially housed in homemade cages; but as work progressed, they were moved twice into new cages. They received as much personal attention by the experimenter as possible and were frequently handled. A 12-hour light-12-hour dark cycle was used except for the first two series, during which a 15-hour light-9-hour dark cycle was used.

General Procedure

There were six series in the experiment, with 25 trials in each run. Since the work was mainly exploratory, there was no preset length for the series; they varied from 7 to 41 runs. The breaks between the series were arbitrarily set by such events as relocation of equipment and changes in personnel, but the series were basically similar in design.

The experiment was terminated partly by the departures of the senior authors and partly by the dismantling of the equipment—a decision which was made by someone other than the experimenters and which therefore eliminates the question of optional stopping. The scoring of the data was completed during the winter, but the results were not evaluated at that time. The following spring, when the authors were again free to resume work on the experiment and before the results were tabulated, the decision was made to combine the series into a unit because of the lack of any clear basis for de-

⁵ This animal is commonly but erroneously referred to as a gerbil. True gerbils are closely related but different species.

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marcation and to examine the degree to which the results gave preliminary indications that we could replicate the French work.

Before each experimental session, the experimenter checked the apparatus to be sure it was functioning properly. He then selected an animal and recorded on the polygraph sheet the experimenter's name, the date, time, and the animal's number. He then put the animal in the testing cage and adjusted the shock level to the animal's tolerance—just enough to make the animal move to avoid the shock. The shock level was recorded on the polygraph and the apparatus was run until one trial had taken place. Then the polygraph sheet was marked and the experimenter left the room for at least 26 minutes in order to minimize the possibility that his own psi might affect the results. (Trials in excess of 25 were not to be included in the experiment.) After the run was completed, the experimenter removed the polygraph sheet from the machine and numbered the trials from 1 to 25, including mistrials. The sheet was then placed in a file.

Later, an independent checker removed the chart from the file and scored the data, transcribing the information onto another record sheet. He noted which of the 25 trials indicated random behavior. (See Figure 2.) Hits and misses were determined by the length of the shock-indicator pen stroke. A miss (shock) was indicated by a long vertical stroke; a hit (nonshock), by a short stroke. The number of hits and misses was noted, the score was recorded, and the polygraph chart was returned to the folder to be rescored eventually by a second checker who was unaware of the first checker's results and who scored the data directly on the chart itself. Both sets of scores were then compared by a third independent checker.

Results

Altogether there were 140 runs of 25 trials each, a total of 3,500 trials. There were 416 occasions when a mistrial occurred or the light source failed and the animal's position could not be monitored. This left a total of 3,084 trials, of which 1,154 were random-behavior trials. When evaluated by chi-square, the results were not significant.

However, the experimenters were interested in finding out whether there was any evidence that ESP was present or operating

Table 1
GENERAL RESULTS

Series	No. Trials in Series	No. Random-Behavior Trials	No. Hits on Random-Behavior Trials	Percentage Random-Behavior Hits
1.....	478	201	104	51.7
2.....	986	423	227	53.7
3.....	437	210	105	50.0
4.....	224	139	78	56.1
5.....	165	62	33	53.2
6.....	794	119	65	54.6
Total	3,084	1,154	612*	52.9

* $CR = 2.06$; $P < .05$ (two-tailed).

at all in these series. Therefore an overall CR was computed. (See Table 1.) In 1,154 random-behavior trials where mean chance expectation was 577 hits there were 612 times when the animal avoided the shock (made a hit). This gives $CR = 2.06$, ($P < .05$, two-tailed).

Discussion

The experiment reported here was an exploratory attempt to repeat the findings of the French work. It was terminated when the authors returned to school in the fall semester, and as a first effort, with suggestive results, it is a sufficiently successful beginning to warrant the further studies, reports of which are to follow.

One of the most crucial questions in this research is the randomness of the targets—whether or not there was some favoring factor. The empirical check preceding the experiment and the check on the targets actually generated during the experiment showed only a chance variation between the two target sides, thus eliminating non-randomness as a factor in the results.

The likelihood that the results could be accounted for by psi on the part of the experimenter appears to have been at least partially reduced by the use of automatic equipment and the fact that the experimenter was out of the room and otherwise occupied while the trials were being run. However, the theory of psi could easily be extended to allow it to work in the absence of the experimenter; and,

unlikely though it seems, it can still be considered a conceivable alternative. This reasoning suggests further measures to put it to test. The second section of the paper goes further into this problem of making a study of the differential responses of the animals that produced the successes.

II. ANALYSES OF THE EFFECT OF THE TEST SITUATION ON THE TEST ANIMALS

The analyses which follow are based on the results of Section I. They were carried out by J. L. to explore the effect of the test procedure upon the ESP responses of the test animals.

Obviously, only a very inefficient demonstration of psi ability was given by the animals, as the pooled totals show. The 1,154 random-behavior trials gave a *CR* of 2.06, which represents a psi quotient (*PQ*) of 3.68 as a measure of efficiency, whereas the comparable figure for the Duval-Montredon evidence is a *PQ* of 29.93.

It seemed reasonable to suppose that there might be differences in the distribution of the small scattering of hits in our data that would reveal a more meaningful pattern. This, after all, was the type of reasoning that led to the original French findings and the principle of random behavior as a method of concentrating the successes in the analysis of the data. The selection of random-behavior trials before the checkup was made was intended by the French authors to eliminate trials on which psi was not to be expected under the conditions of the test. This selection concentrated the successful trials and produced the high *PQ* mentioned above. However, the attempt to repeat the French experiments, with all the subtle variables involved in such attempted replication, may have introduced differences in method that weakened the confirmation, and it seemed possible that there might be some areas of the test situation that were more conducive to the operation of psi than others.

In following this line of thought, J. L. saw an opportunity for a more complete analysis of the animal behavior than had been possible in the French study. It has been pointed out that our apparatus monitored the behavior of the animal continuously and gave readings of jumps back and forth between trials that would not have been recorded by the French apparatus, which monitored the mouse only

at the start of the five-second trial. On inspection, our records gave indications that the relative jumpiness, or nervousness, on the part of the animals was noticeably greater right after a shock trial; that is, a trial in which the animal made a miss and therefore received a shock. Considering the probable psychology of the animal, it seemed likely that right after a shock trial, his sensory and motor systems would be more dominantly activated and that this might interfere with the spontaneous operation of his precognition. J. L. planned, on this basis, to divide the trials following shock from the trials following nonshock. This breakdown makes up the first step in the present study.

Comparison of Results Following Shock and Nonshock Trials

The after-shock vs. after-nonshock breakdown was made by two checkers, who rechecked the original polygraph charts. They totaled the number of times a shock trial was followed on the next trial by a hit (avoidance of shock) or a miss (shock); and the number of times a nonshock trial^a was followed by a hit or a miss. These data were later rechecked by computer. The results are shown in Table 2.

Table 2
COMPARISON OF RESULTS FOLLOWING SHOCK AND NONSHOCK TRIALS

Condition	No. Random-Behavior Trials	Hits	% Hits	CR	P ^a
After-shock trials	506	257	50.79	.36	.72
After-nonshock trials	648	355	54.8	2.44	< .015

^aTwo-tailed.

There were 1,154 random-behavior trials, 648 of which followed a nonshock trial. (See Table 2.) Of these, 355 were hits (54.8%) with $CR = 2.44$ ($P < .015$, two-tailed). The $PQ = 9.19$. The remaining 506 random-behavior trials were trials which followed a shock. They contained only 257 hits (50.79% success); $PQ = .25$.

^a A nonshock trial could be preceded either by a hit or a mistrial, as defined in Section I.

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Analysis of Intertrial Jumps

Encouraged by the results of the after-shock vs. after-nonshock analysis, J. L. undertook a further breakdown of the same results on the basis of the apparent "nervousness" of the animals as indicated by the number of times they jumped back and forth during the period between trials when the two compartments, of course, were not electrified. This was a mere behavioral count of activity as recorded by the apparatus.

Since the animals could jump into a shock only with difficulty (by jumping at least twice within a five-second period), one could say they were reinforced for jumping. In jumping they eluded shock almost exclusively; hence it was a beneficial behavior pattern for them to adopt. As with the random-behavior selection, one might expect those cases where the animals jumped less to show more evidence of psi, since low-jumping is an activity less accountable for by non-psi factors. Also, one would expect less jumping after nonshock than after shock. So this was logically complementary to both the "random-behavior" principle and the shock-nonshock idea.

From a rough inspection of the same original data, it appeared that the best separation of the total trials would be to group those in which the animal jumped back and forth not more than two times between trials and compare them with those in which it jumped three or more times.⁷ This analysis was carried out on the entire group

⁷The choice of this point to break the low-jump vs. high-jump trials was, in addition to the one given, a mathematical one stemming from the difference between selective monitoring (the French method) and continuous monitoring. As pointed out, the selective monitoring recorded the animal's position only at the time of the trial. Thus, if the animal jumped once between trials, a position change was recorded; but if he jumped twice, he appeared not to have moved at all. All trials with even numbers of jumps, therefore, were lost and only trials with odd numbers of jumps were counted. Again, since mechanical jumps were excluded, any trial after a shock, whether the number of jumps was odd or even, was excluded. It was apparent that, while the scoring in our experiment gave marginally significant results on the basis of continuous monitoring, it was at chance on the basis of selective monitoring. This suggested the occurrence of an uneven distribution of psi; i.e., that more psi hits occurred in trials with certain types of behavior than with others. Since the trials that are excluded in selective monitoring necessarily have an even number of jumps, it would follow that in continuous monitoring the trials with two, four, etc. jumps must be the ones that contributed the positive scoring.

It was assumed that as the number of jumps increased, the number of trials with that number of jumps would decrease. Thus there would be fewer trials with four and five jumps than with one or two. This, together with the reasoning about

Table 3
COMPARISON OF RESULTS FOLLOWING HIGH AND
LOW NUMBER OF INTERTRIAL JUMPS

Condition	No. Random-Behavior Trials	Hits	% Hits	CR	P^a
Three or more jumps	395	199	50.5	.15	.88
Two or fewer jumps	759	413	54.5	2.43	.015

^aTwo-tailed.

of data, again following essentially the same procedure as before, first by hand and then by computer.

Out of 1,154 random-behavior trials, there were 759 which were preceded by two or fewer jumps before the trial. (See Table 3.) On that next trial there were 413 hits (54.5% success) with $CR = 2.43$; $P = .015$, two-tailed. $PQ = 7.78$. The remaining 395 random-behavior trials, which were preceded by three or more intertrial jumps, gave only 199 hits on the next trial. The scoring rate has a $CR = .15$ and a $PQ = .06$.

The Two Analyses Combined

Finally, on the discovery that both of the breakdowns did show a meaningful concentration of the successes in a way that clearly improved the efficiency level as measured by the PQ of the selected category, the obvious next step was to combine the two analyses for the still further improved concentration that would have to follow.

There were 422 trials which were preceded by a nonshock trial and before which the animal had jumped only once or twice. Of these, 239 were hits, 56.7% success, and a $CR = 2.73$ ($P = .006$, two-tailed). This has a $PQ = 17.66$. Most of the overall scoring was from this type of trial.

jumping activity and shock mentioned above, made it seem that trials with one and two jumps were likely to be scoring better than those with three or more. The one-jump trials' positive scoring would be brought to chance by the scoring of three-jump and five-jump trials, giving the result obtained with odd-jump trials. Since the two-jump trials greatly outnumber four-jump trials, the overall result with even-jump trials would be positive, as seen. This division of one- and two- versus three- and more jump trials also gave roughly equivalent numbers of trials in each group, and it was therefore predicted that the data would separate along those lines.

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Table 4
RESULTS OF TRIALS FOLLOWING NONSHOCK
TRIALS AND TWO OR FEWER JUMPS

	After Shock	After Nonshock	Total
Three or more jumps	169 trials 83 hits 49.1%	226 trials 116 hits 51.4%	395 trials 199 hits 50.4%
Two or fewer jumps	337 trials 174 hits 51.7%	422 trials 239 hits 56.7% <i>CR = 2.73*</i>	759 trials 413 hits 54.5%
Total	506 trials 257 hits 50.8%	648 trials 355 hits 54.8%	1,154 trials 612 hits 52.9%

**P* = .006 (two-tailed).

As can be seen from Table 4, the other three conditions (after shock, three or more jumps; after nonshock, three or more jumps; and after shock, two or fewer jumps) gave lower rates of scoring, as was expected.

Discussion

This is, in effect, a pilot study in which the method itself was used for the first time and the decisions concerning certain points had to be made with some arbitrariness. The comparison of trials following shock and nonshock left little latitude for choice of procedure unless the breakdown might have been based on the results of two trials (instead of one) following a shock trial vs. two trials following a nonshock trial. This alternative method might, in fact, be worth trying in the future. The selection of the dividing point for the number of jumps made by the mouse between trials had to be an arbitrary one, but there was little choice that would have left a suitable division of numbers of trials for the two groups.

Furthermore, unless generalizations are confirmed in the analyses of other data, they are very tentative and soon lost sight of. Fortunately, in this case additional data were immediately available in the work of other staff members of the Institute and in the results of further tests in the present series. While it is permissible to say that these later analyses confirmed the findings reported here, further

details must await publication of the other researches on which the evaluations are based.

Not all the questions raised by these results can be very fully discussed at this stage. Some will have to wait for the later confirmation, especially those having to do with the principles involved in the bases for discrimination used in this breakdown. It does look as though shock recency is a deterrent to the exercise of psi, and it does look as though the hyperactivity of the animal, which may owe something to the recency of shock itself, is a sign of unreadiness for a psi response. One view of this situation that might be relevant is that this immediacy may be a rough analogue of consciousness in man and that these results may suggest some parallels between the unconscious control of psi in man and the expression of psi in animals.

If these points add up in a rational way, and especially in an experimentally demonstrable way, the summing-up would run like this: if the French workers have found a test animal which, with automation and random-behavior selection, can produce a moderately efficient and fairly reproducible type of psi demonstration, and if, in addition, the selection of trials following nonshock in which the animals are less activated to jump around enhances the psi effect, there would appear to be some advance toward the desirable objective of dependable psi test performance.

This leaves the problem of what role the experimenters have in the experiment. With each of the steps summarized in these analyses the segment of the data in which psi is found is related more to the animal's situation and less to the experimenter's. Conceivably the experimenter could affect the apparatus controlling the frequency of shock in some such way as must be happening in the experiments described by Schmidt (4) when he had subjects try to influence the frequency of the lighting of lamps which were determined by a similar selector system. The complexity of the operations is comparable in both these experiments, and the question is still good enough to deserve attention, as indicated earlier in the paper. A succession of steps such as the ones in these analyses removes the experimenter very considerably from the suspicion that he may be involved. It was not the experimenter who experienced the shock and

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it was not the experimenter who was jumping back and forth between sections in the apparatus; but these two considerations mark off almost exclusively the area in which the efficient performance was produced. Perhaps the experimenter is as much exonerated here as is possible at this stage of psi research. However, the question may be reconsidered when the further analyses are reported. Perhaps there will be other indications of how to distinguish man from animal in the psi test.

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*Institute for Parapsychology
College Station
Durham, N. C. 27708*

LOCATION OF HIDDEN OBJECTS BY A MAN-DOG TEAM¹

By J. B. RHINE

ABSTRACT: The experiment was carried out with two German shepherds and their trainer who, as a team, attempted to locate empty land mine cases that were buried in a straight line four inches deep in sand under 6 to 12 inches of water. Each five-yard section, made up of five one-yard units, contained one buried mine, the location determined by a random number table. All surface signs except section and unit locations were removed by raking.

The test site was out of visual range of the man-dog team during the planting of the mines. The dog was led over each section as a single trial, continuing to retrace until it indicated a location by sitting. The experimenter who buried the mine remained silent and invisible to the team during the test, but from a screened position he was able to observe and record the team's responses. When an assistant was present, the recording observer did not know the target locations.

Both dogs gave significant results. The presence of steady crosswinds, surface ripples, and strong side currents in the water gave good assurance against olfactory cues. Special tests with colored liquids and streamers were made to verify the adequacy of these controls.

Two hundred and three underwater trials gave 38.9% hits where 20% is expected ($P \ll .001$), but a decline set in with both dogs, even with no change of personnel or overt conditions. Possible psychological factors are recognized that suggest an essential link in the man-dog relation.—Ed.

The report given here describes experimental tests of the ability of two man-led dogs to locate underground objects made to simulate military land mines. The purpose of the investigation was to provide conditions that afforded no sensory guidance to the dog or man but allowed the possibility of extrasensory perception.

The experiments were conducted by the Parapsychology Laboratory of Duke University in 1952 at the request of, and under contract to, the Engineer Research and Development Laboratories (ERDL), Fort Belvoir, Virginia. The reason for the delay in publication of the report is that the material was classified and that efforts to obtain declassification were unsuccessful until 1969.

The work was part of a larger program of investigation into the psi capacity in animals. Some of the other studies have been re-

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