ON THE MATHEMATICS OF SCIENTIFIC BELIEF SYSTEMS

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Introduction

The subject of this paper embraces two general areas dealing with the overall problem of repeatability in parapsychology. There is first the question of the importance of repeatability from the perspective of the philosophy of science and especially the role of repeatability in affecting the acceptance and beliefs of people in the scientific community. Secondly, we need to search for ways to improve the ability of researchers in parapsychology to replicate experimental results. To the extent that psi phenomena represent quantum mechanical effects, this theory should shed light on just what it is that interferes with replication of experimental results.

Whatever philosophical principles may be generally acknowledged as undergirding science, there remains the pragmatic requirement that members of the scientific community must be persuaded that any new proposition in science is supported by experimental evidence. Thus, we are ultimately engaged in efforts to sway the beliefs of substantial numbers of scientists in order to effect what in the case of parapsychology amounts to a revolution in scientific thinking.

Where the beliefs of scientists are so ingrained and the evidence so challenging to the acknowledged paradigm, this change in thinking cannot be achieved by reporting the results of a single experiment. Moreover, where repeatability is a recognized difficulty, the belief system takes on a dynamics of its own. Some aspects of this dynamic can be modeled mathematically. Thus, the mechanism of scientific revolution—paradigm change—itself can be treated mathematically. The value of mathematics is that it can show what types of obstacles must be overcome if factual information is to succeed in altering attitudes and beliefs. Indeed, we may find that what is believed to be true in science is a function of the mechanism of scientific communication.

Ultimately, however, the problem of repeatability must be resolved by achieving an improved understanding of the mechanism of the phenomenon. The time has passed when we may reasonably expect to accidentally discover some relatively simple psychological test that will separate out individuals who can assure repeatability, or when we may, without an improved understanding of the underlying physical principles, prescribe experimental procedures to circumvent the current difficulties. The quantum mechanical theory of psi phenomena has had some degree of success and therefore its possible impact as to causes and cures for the repeatability problem should be of use. We will examine the elements of the theory that relate to the problem of repeatability, to identify at least some present weaknesses in psi research that may be impeding progress and to propose some new directions for future work.

Ultimately we can have only one of two attitudes on the issue of repeatability. First, one can ascribe the lack of consistent repeatability of experimental results to nonexistence of the phenomena. Second, one can take the position that the variables responsible for the phenomena are not under the control of the experimenter. The first of these is the position taken by those who do not believe in psi, or who, seeing the absence of general repeatability in parapsychological experimental research, look to discredit the remaining supportive results. This attitude would be more justified were the general scientific context in which psi exists—i.e., the states of scientific inquiry on the nature of consciousness and mind in the fields of psychology and physics—not now so untenable. If behavioral psychology had indeed fully met its goals and, moreover, in so doing resolved issues concerning the nature of consciousness and mind; or if physics as based entirely on materialism had been successful—with no measurement problem—we might feel the position taken against the evidence for psi phenomena to be more rational. The opponents of psi, by denying efforts to understand scientifically the nature of consciousness or of mind, make their task of rejecting the evidence for the secondary manifestations of mind, psi phenomena, simply an act of faith on this erroneous conception of reality. They feel justified in rejecting psi, not because of repeatability problems, but because they are committed intellectually to a belief that such claims are extraordinary. For them these claims are extraordinary because they conflict with the tenets of the physicalistic paradigm. Their arguments over repeatability are only excuses to permit them to adhere to an erroneous paradigm—the paradigm of objective physical reality.

The error of these scientists has been the belief that they can appraise the believability of psi by an objective consideration of the physical properties of the brain, the organism and the environment as material objects. In some ways these same shortcomings affect parapsychological researchers. Although they acknowledge the reality or potential reality of psi, they largely ignore the larger problem of understanding the nature of consciousness. As a consequence, much effort is devoted to exploring objective or behavioral variables that are fundamentally physical variables of other sciences tied to a conception of physical reality that is not compatible with psi phenomena. Thus, we find in much of parapsychological research too much that is borrowed. Of necessity we are left to accept the hand-me-downs of scientific research, in tools and techniques. Only occasionally do we produce ideas or techniques wholly the product of parapsychological research. These are the ones that have proved productive.

What is the role of repeatability in parapsychological research? There already exists extensive literature on this subject. Rather than being a new issue that is the concern of parapsychology alone, it reaches back to some of the beginnings of science. Of interest is a review of the communications to the Royal Society regarding Isaac Newton's experiments on the nature of color.[1] His experiments were simple. One simply needed to place a prism of clear glass in a beam of sunlight to see the resulting display of colors. Incredibly some wrote the Royal Society to report failure to replicate!

Mathematical Models of Belief System Dynamics in Science with Application to Psi Phenomena

Kuhn[2] has introduced a now widely acknowledged concept as to how science progresses in stages of successive paradigms. These paradigms, fundamental propositions whose acceptance forms the conceptual context for scientific activity, may provide the foundation for scientific work for great periods of time, until overthrown when so much contrary evidence accumulates that the paradigm can no longer guide science.

Many in parapsychology have felt Kuhn's message to be of especial significance. But is it? Can we expect the development of parapsychology to bloom from the ashes of materialistic scientific concepts as, say, the caloric theory swept away phlogiston? More than that, with arguments rising as to just how paradigms develop, what really is the mechanism of paradigm development? Can we be certain that the valid will be accepted by science? Rather than settling an issue, Kuhn's work has opened the door to a host of ideas about the development of science. But these ideas are simply too vaguely couched to provide a basis for answering our question about the future of parapsychology. In what follows we address this problem of the development of a paradigm—the mechanism of acceptance by a large portion of the scientific community of a significant and exclusive proposition—using mathematical tools. We do not here ask if the proposition is true, but, rather, assuming the given relationship between the proposition, experimental results and repeatability, what effect will various possible mechanisms by which scientists interact affect what they accept as truth?

Simple Paradigm Adoption: Let N be the total population of discourse, i.e., the scientific community of concern. Let N_A be the number of members of this community that have adopted a particular proposition A as valid. Moreover, let n be the number of experiments or tests of the proposition conducted. If we assume these are conducted at a constant rate, we have

$$dn/dt = \text{const} = c \tag{1}$$

where t is time, so that at any time t the number of tests will be

$$n = ct + c' \tag{2}$$

We can set c' to zero by having $t = 0$ for $n = 0$.

If the rate of change in the number of individuals who accept the proposition is directly dependent on the number of individuals not yet accepting the results of prior tests, then we can write

$$\frac{dN_A}{dn} = k(N - N_A) \tag{3}$$

where k is a coefficient of proportionality and $N - N_A$ is the number of members of the community yet to be convinced of the validity of proposition A. Integrating (3) yields

$$\int_0^{N_A} \frac{dN_A}{N - N_A} = k \int_0^n dn \tag{4}$$

so,

$$N_A = N(1 - e^{-kn}) \tag{5}$$

or from Equation (2), we write

$$N_A = N(1 - e^{-\alpha t}) \tag{6}$$

where

$$\alpha = kc \tag{7}$$

Equation (6) expresses the simple concept that the number of converts to the proposition under consideration by the population will grow asymptotically to include the entire population, under these conditions.

Successive Paradigm Replacement: A more complicated situation obtains if there are two propositions, A and B, such that the second (B) can supplant the first subsequent to its having been adopted by any members of the community. We assume that the rate of growth in number of those who accept proposition A, numbering N_A, is proportional to the number available that have not yet accepted A, less the rate at which individuals replace their acceptance of A by proposition B. Thus we will have, where k is a proportionality factor,

$$\frac{dN_A}{dt} = k(N - N_A - N_B) - \frac{dN_B}{dt} \tag{8}$$

Here N_B is the number of adherents to proposition B. Equation (8) gives the build up of acceptance of proposition A. Proposition B will gain adherents according to

$$\frac{dN_B}{dt} = k'N_A \tag{9}$$

If we substitute

$$N' = N_A + N_B \tag{10}$$

in Eq. (8), it is easily integrated giving

$$N_A = N(1 - e^{-kt}) - N_B \tag{11}$$

Substituting N_A from Eq. (11) into (9) allows us to solve for N_B:

$$N_B \; N\left[\frac{k}{k' - k} e^{-kt} + \left(1 - \frac{k'}{k' - k} e^{-kt}\right)\right] \tag{12}$$

Substituting into Eq. (11) gives

$$N_A = N\left[\left(\frac{k'}{k' - k} - 1\right)e^{-kt} - \frac{k}{k' - k} e^{-k't}\right] \tag{13}$$

An example of the change in N_A and N_B with time is shown in Fig. 1.

Research Rate Dependent Paradigm Replacement: The above does not consider a realistic relationship between time and experimental work to investigate the validity of the respective propositions. Indeed, Eqs. (8) and (9) might more reasonably be applied to philosophical systems

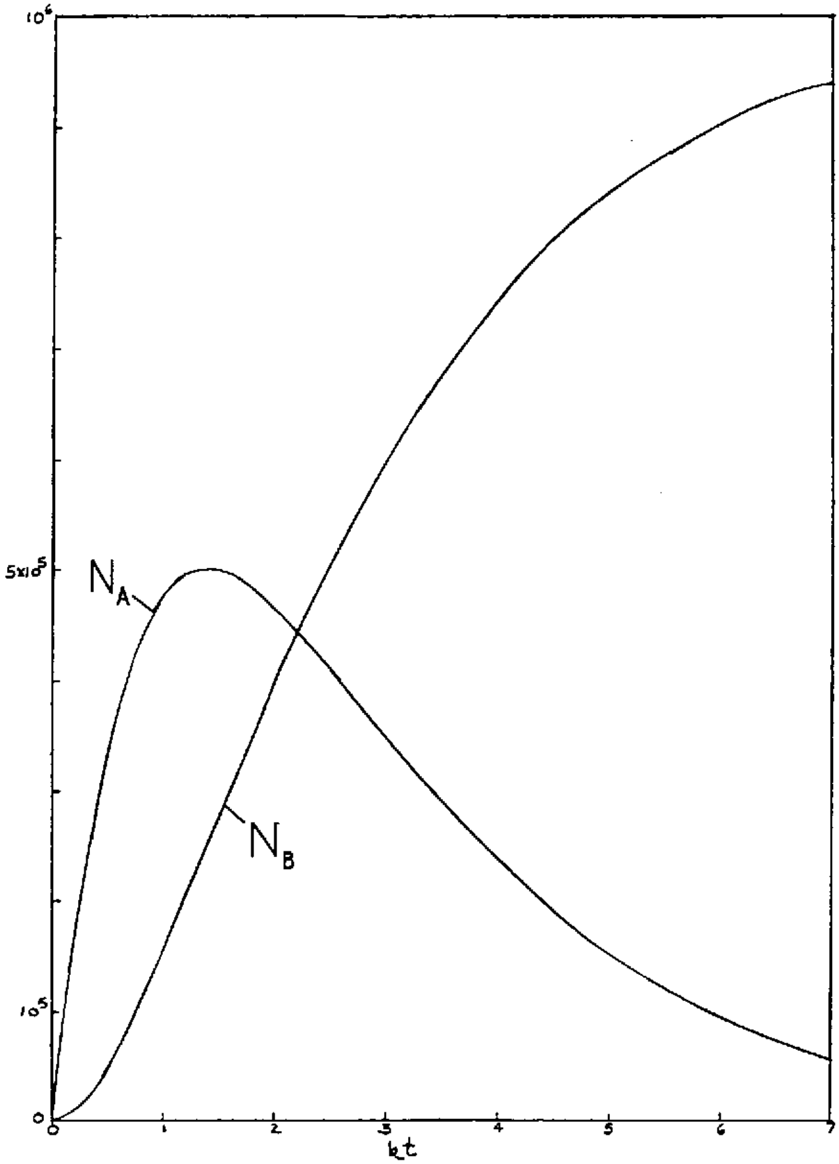

Figure 1. Successive paradigm replacement. Here we plot the number of adherents to proposition A, N_A, and the number of adherents to proposition B, N_B, as a function of time as given by Eqs. (12) and (13) with $k' = k/2$ and t is given in units of $1/k$.

that require nothing, but for the flaws in proposition A to become recognized with the passage of time, rather than that this change be the result of research. Let us, therefore, consider the equations we obtain if we assume the changes in numbers of adherents to result from experimental research conducted by adherents to the respective propositions. We have:

$$\frac{dN_A}{dn_a} = k(N - N_A - N_B) - \frac{dN_B}{dn_a} \qquad (14)$$

where n_a is the number of experiments conducted by the community of adherents to proposition A to show the validity of A. The rate at which these experiments are conducted will be proportional to the size of that community. We have

$$\frac{dn_a}{dt} = cN_A \qquad (15)$$

where c is a proportionality constant.

Now for proposition B, the rate in growth in these adherents will depend on the number N_A of adherents available to change their ideas. Where k' is the proportionality constant, we have:

$$\frac{dN_B}{dn_a} = k'N_A \qquad (16)$$

The quantity of n_b is the number of experiments conducted by the adherents to proposition B to show the validity of B. The rate at which these experiments are conducted will be proportional to the size of the community of adherents to proposition B, N_B. We have:

$$\frac{dn_b}{dt} = c'N_B \qquad (17)$$

Substituting from Eqs. (15) and (17) into Eqs. (14) and (16) we can express the dependence of N_A and N_B on time explicitly. We have:

$$\frac{dN_A}{dt} = \alpha N_A(N - N_A - N_B) - \alpha' N_A N_B \qquad (18)$$

and

$$\frac{dN_B}{dt} = \alpha' N_A N_B \qquad (19)$$

where α and α' are

$$\alpha = kc \qquad (20)$$

and

$$\alpha = k'c' \tag{21}$$

Figure 2 shows an example of the temporal development of N_A and N_B with time. The process is straightforward, and can be extended to a succession of propositions or paradigms. Equations (14) to (17) typify the Kuhnian conception of the development of scientific thought. The dynamics involved in the growth of acceptance in psi phenomena is rather different, however, because of the repeatability question.

Repeatability Modified Paradigm Replacement: For psi phenomena, we do not have the kind of experimental success that leads directly to an ever increasing number of adherents. We have the added problem of "unsuccessful" (non-replicating) experiments. For the most elementary case we assume an initially uncommitted population of N members. Assume experiments are conducted at some constant rate,

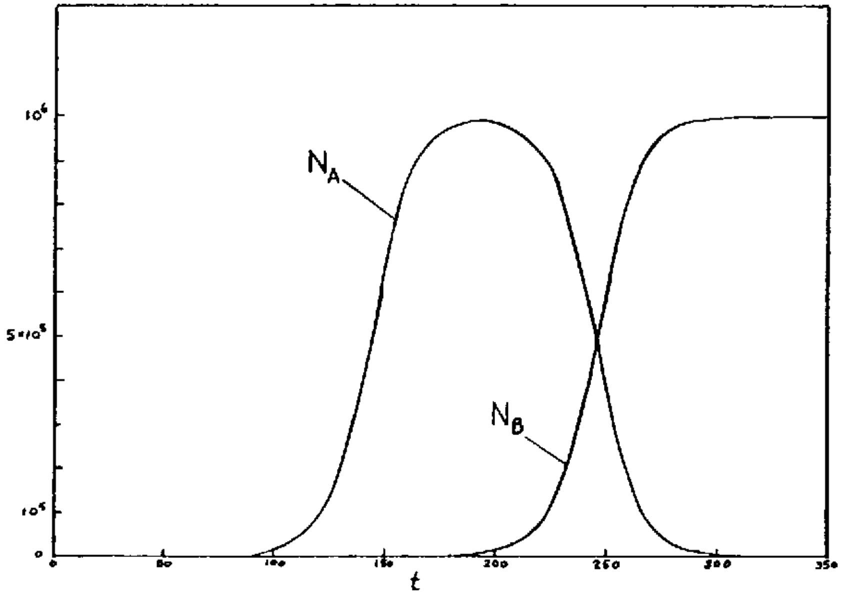

Figure 2. Research rate dependent paradigm replacement. Computer evaluation of Eqs. (18) and (19) for growth of the population of adherents to proposition A, N_A, and the growth of the population of adherents to proposition B, N_B where conversions are a function of the rate of research work. Here we have set $\alpha' = \alpha = 10^{-6}$, set the total initial population $N = 10^6$, and set the time for the introduction of proposition B to be $t' = 100$.

yielding positive results a fraction s of the time so as to convince some of the uncommitted population to accept the phenomenon to join the N_+ population of "believers," while $1 - s$ yield negative results to enhance the N_- population of those who reject the proposition. We can write

$$\frac{dN_+}{dt} = sk(N - N_+ - N_-) \tag{22}$$

and

$$\frac{dN_-}{dt} = (1 - s)k(N - N_+ - N_-) \tag{23}$$

By writing $N' = N_+ + N_-$ we obtain an equation

$$\frac{dN'}{dt} = k(N - N') \tag{24}$$

which integrates to

$$N_+ + N_- = N(1 - e^{-kt}) \tag{25}$$

Substituting for N_- in Eq. (22) yields on integrating

$$N_+ = sN(1 - e^{-kt}) \tag{26}$$

and for N_-

$$N_- = (1 - s)N(1 - e^{-kt}) \tag{27}$$

So that the populations grow along an exponential curve to a maximum number of adherents:

$$N_{+max} = sN \tag{28}$$

and

$$N_{-max} = (1 - s)N \tag{29}$$

That is, the respective numbers of adherents divide up in proportion to the success or failure of experiments in support of the respective propositions.

Repeatability Modified Research Rate Dependent Paradigm Replacement: For parapsychology, the number of experiments conducted will depend on the number of adherents. In that case the equations become:

$$\frac{dN_+}{dn} = sk(N - N_+ - N_-) \tag{30}$$

$$\frac{dN_-}{dn} = (1 - s)k(N - N_+ - N_-) \tag{31}$$

and for the rate at which experiments are carried out

$$\frac{dn}{dt} = cN_+ \tag{32}$$

We have for solutions in terms of n,

$$N_+ = sN(1 - e^{-kn}) \tag{33}$$

and

$$N_- = (1 - s)N(1 - e^{-kn}) \tag{34}$$

Substituting from Eq. (33) into Eq. (32) we obtain

$$\int_0^n \frac{dn}{1 - e^{-kn}} = csNt \tag{35}$$

which integrates to

$$n = \frac{1}{k} \ln [1 + e^{\gamma t}/(sN - 1)] \tag{36}$$

where $\gamma = kcsN$ and where we set the initial value of n to be that corresponding to $N_+ = 1$. Therefore, we have:

$$N_+ = sN\{1 - [1 + e^{\gamma t}/(sN - 1)]^{-1}\} \tag{37}$$

and

$$N_- = (1 - s)N\{1 - [1 + e^{\gamma t}/(sN - 1)]^{-1}\} \tag{38}$$

Figure 3 shows an example of the resulting growth of N_+ and N_- with time.

Paradigm Acceptance in a Polarized Population: Now let us assume that we actually begin with no uncommitted individuals in the population. We assume

$$N = N_+ + N_- \tag{39}$$

That is, the entire population of N individuals consists of N_+ number of those accepting the proposition (that psi is real) and N_- who do not. Assume they change their conviction as the result of experiments for which successful replication occurs in a fraction s of all tests.

$$\frac{dN_+}{dt} = skN_- - (1 - s)k'N_+ \tag{40}$$

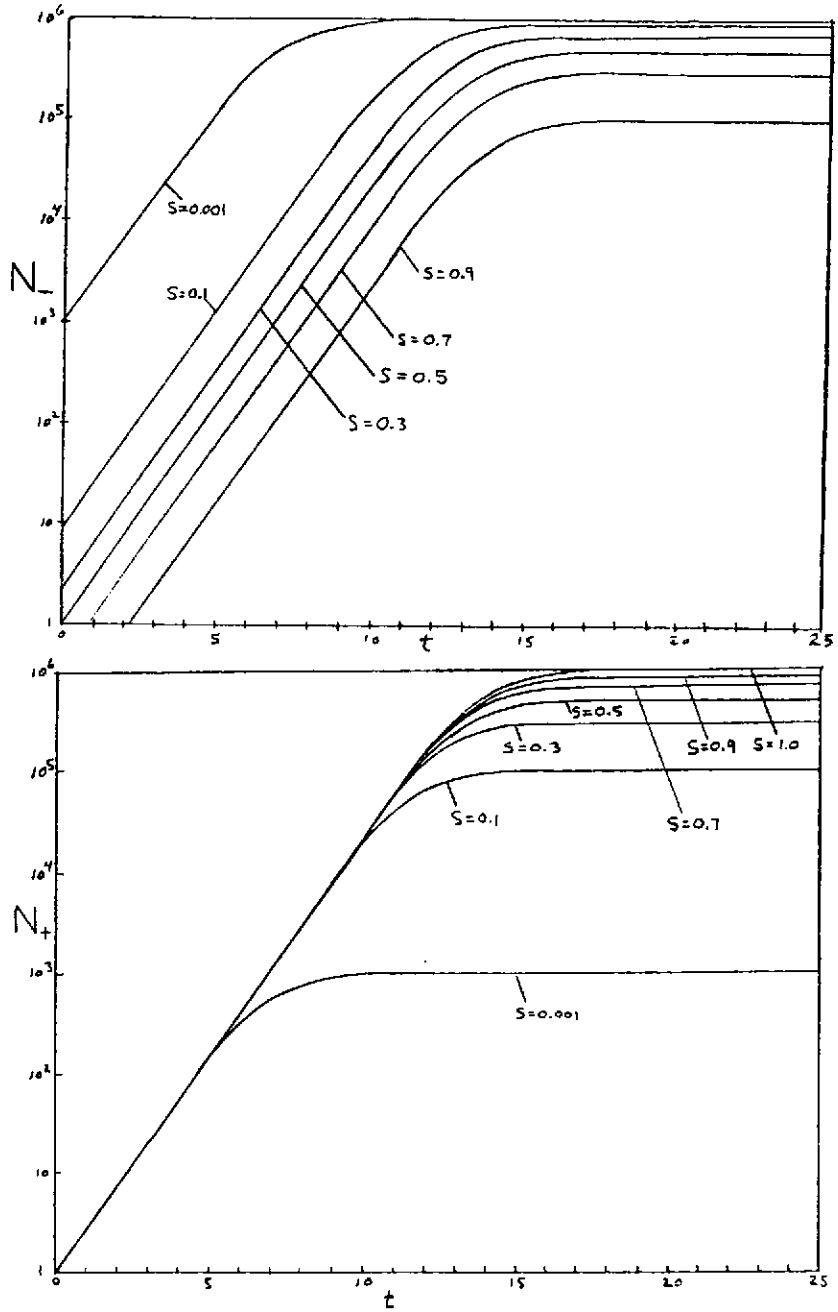

Figure 3. Repeatability modified research rate dependent paradigm replacement. Plot of Eqs. (37) and (38) with $N = 10^6$, and $\gamma = 1$ for various values of s.

where the first term on the right hand side of Eq. (40) gives the rate at which disbelievers accept psi as a result of positive experimental results and the second term on the right hand side gives the rate of loss of believers on seeing negative experimental results. Substituting for N_- from Eq. (39) gives

$$\frac{dN_+}{dt} = \alpha N - \beta N_+ \tag{41}$$

where

$$\alpha = sk \tag{42}$$

and

$$\beta = s(k - k') + k' \tag{43}$$

Equation (41) integrates to

$$N_+ = \frac{\alpha}{\beta} N(1 - e^{\beta t}) \tag{44}$$

Thus the ratio α/β gives the limiting case fraction of the total population accepting psi. If k' is zero (i.e. we never lose adherents) eventually $N_+ \rightarrow N$, a complicated way to express the obvious! But we do see here that the fraction of people accepting psi will be strongly tied to the rate of successful replication of results.

Moreover, if we assume the two populations, N_+ and N_-, are of the same character so that $k = k'$ we have that the limiting case for the number of adherents will be:

$$\lim_{t \to \infty} N_+ = sN; \qquad k' = k \tag{45}$$

Since positive results can be obtained by chance, we might be tempted to interpret the present low population of psi researchers as stemming from just this mechanism. However, Eq. (45) represents a continually rotating population, which is rather different from the nature of the actual population in parapsychological research.

Paradigm Growth with Publication Bias: In the discussion above we assumed that the acceptance of a scientific proposition depends on exposure to the results of experimental findings. Unfortunately, there often exist biases against concepts that run counter to the accepted paradigm. As a result there can exist significant bias regarding what is published in the scientific literature. If we look at the effect of a publication bias that is proportional to the respective number of adherents and detractors of the proposition, we will have for the rate of change in the number N_+ of individuals accepting the proposition

$$\frac{dN_+}{dt} = k_1 s \frac{dn_+}{dt} N_- - k_2(1 - s) \frac{dn_-}{dt} N_+ \tag{46}$$

where k_1 and k_2 are proportionality constants, s is the success rate of experiments, dn_+/dt is the rate at which successful experiments supporting the proposition are accepted, and dn_-/dt is the rate at which unsuccessful or negative findings are published. Again we assume a total population N divided between those who accept the proposition and those opposed to it.

We will assume the rates of publication to be proportional to the respective numbers of supporters and detractors as mentioned above, so that this bias yields

$$\frac{dn_+}{dt} = a_1 N_+ \tag{47}$$

and

$$\frac{dn_-}{dt} = a_2 N_- \tag{48}$$

where a_1 and a_2 are proportionally constants.
With

$$\alpha = a_1 k_1 \tag{49}$$

and

$$\alpha' = a_2 k_2 \tag{50}$$

we have

$$\frac{dN_+}{dt} = [s(\alpha + \alpha') - \alpha']N_+N_- \tag{51}$$

If we assume there is no difference in the level of bias in the two populations or in their tendencies relative to the propositions other than as indicated by the equation, we can set

$$\alpha = \alpha' \tag{52}$$

Substituting Eq. (52) into (51) and integrating we find the population N_+ at time t will be

$$N_+ = \frac{NN_i}{N - N_i} e^{\alpha N(2s-1)t} \bigg/ \left[1 + \frac{N_i}{N - N_i} e^{\alpha N(2s-1)t}\right] \tag{53}$$

where N_i is an initial or seed population that initiates the proposition.

Figure 4 shows how the population N_+ changes with time. In

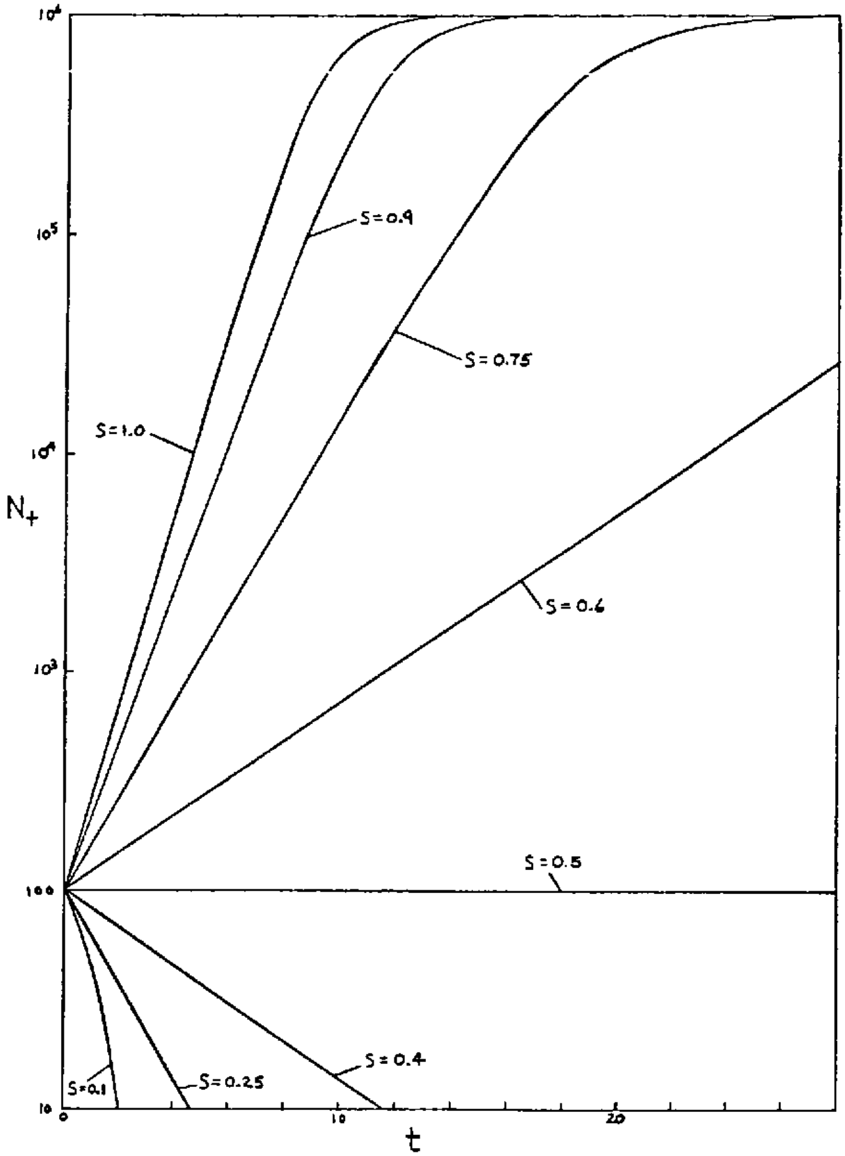

Figure 4. Paradigm growth with publication bias. Plot of N_+ as a function of non-dimensional time (i.e. αNt) as given by Eq. (53) for various values of s and for a seed population of $N_i = 100$ at $t = 0$. Note that for values of $s < 0.5$ the population of adherents decreases with time. The effect of low repeatability is the eventual loss of the population of adherents.

particular it will now be seen that the effect of a publication bias may not be overcome with time. The fact that the phenomenon being studied yields stochastic results, that is, yields experimental results that are not always successful in replication together with the effect that publication bias can have on science, can lead to a failure to discover scientific truth. Where bias was not present, such a result was not obtained. There the problem of repeatability merely lengthened the time required for acceptance. Thus the mechanism of publication bias can be successful in distorting our scientific knowledge, not merely temporarily, but permanently.

Publication Biased Paradigm Acceptance with Organized Opposition: Now let us look at what happens when there also exists an organization established to propagandize against the proposition. We assume publication bias to also be a factor here. The basic equation for N_+ is then

$$\frac{dN_+}{dt} = k_1 s \frac{dn_+}{dt} N_- - k_2(1 - s) \frac{dn_-}{dt} N_+ - k_3 N_- \tag{54}$$

where we have assumed the propagandists to have an effect proportional to the population of those opposing the proposition; k_3 is the proportionality coefficient. As before we assume Eqs. (47) through (50) and (52) to hold. Writing

$$\beta = \alpha(2s - 1) \tag{55}$$

and

$$\gamma = k_3/\beta \tag{56}$$

we have for (54)

$$dN_+/dt = \beta(N_+ - \gamma)(N - N_+) \tag{57}$$

Integrating Eq. (57) yields

$$N_+ = \{(\gamma + N) + Q + [Q - (\gamma + N)]Ke^{-\beta Qt}\}/2[1 - Ke^{-\beta Qt}] \tag{58}$$

where

$$Q = \sqrt{(\gamma + N)^2 - 4\gamma N} \tag{59}$$

and

$$K = \frac{2N_i - (\gamma + N) - Q}{2N_i - (\gamma + N) + Q} \tag{60}$$

and where N_i is, as before, the initial or seed population of advocates for the proposition.

The results for typical numbers are shown in Fig. 5. We see that such a propagandist effort can successfully and permanently alter what is accepted as scientific fact even in the face of scientific experimental evidence that would otherwise have shown the proposition to be true.

Of course, we know that in the case of parapsychology such an organization exists. It appears to be having a detrimental effect, moreover. The effect can prove to be more than merely one of depressing activity in parapsychology. The effect can be one that succeeds in detrimentally altering the course of science. Science is a filter of facts and the facts obtained depend on the design of that filter. If the design is wrong, truth will escape.

Effect of Belief Biased Experimentation on Paradigm Acceptance: We obtain some interesting results if we assume that the success rate in experimental replication depends on the relative numbers of believers and skeptics. Ignoring other effects we will have again (see Eq. (40))

$$\frac{dN_+}{dt} = skN_- - (1 - s)k'N_+ \tag{61}$$

but where

$$s = N_+/N \tag{62}$$

Equation (61) becomes

$$\frac{dN_+}{dt} = (k - k')N_+N_-/N \tag{63}$$

If we assume both adherents and antagonists to be equally able to affect the outcome, we must set

$$k = k' \tag{64}$$

and, as a result, Eq. (63) becomes

$$dN_+/dt = 0 \tag{65}$$

This assumption would imply that a change in the level of acceptance of psi would not be possible!

Publication Biased-Belief Biased Paradigm Growth: Now let us assume that there exists both a publication bias and an effect such that the success rate in experiments depends on the proportion of believers (we omit the term: representing propaganda effects). We write as before (see Eq. (46))

$$\frac{dN_+}{dt} = k_1 s \frac{dn_+}{dt} N_- - k_2(1 - s) \frac{dn_-}{dt} N_+ \tag{66}$$

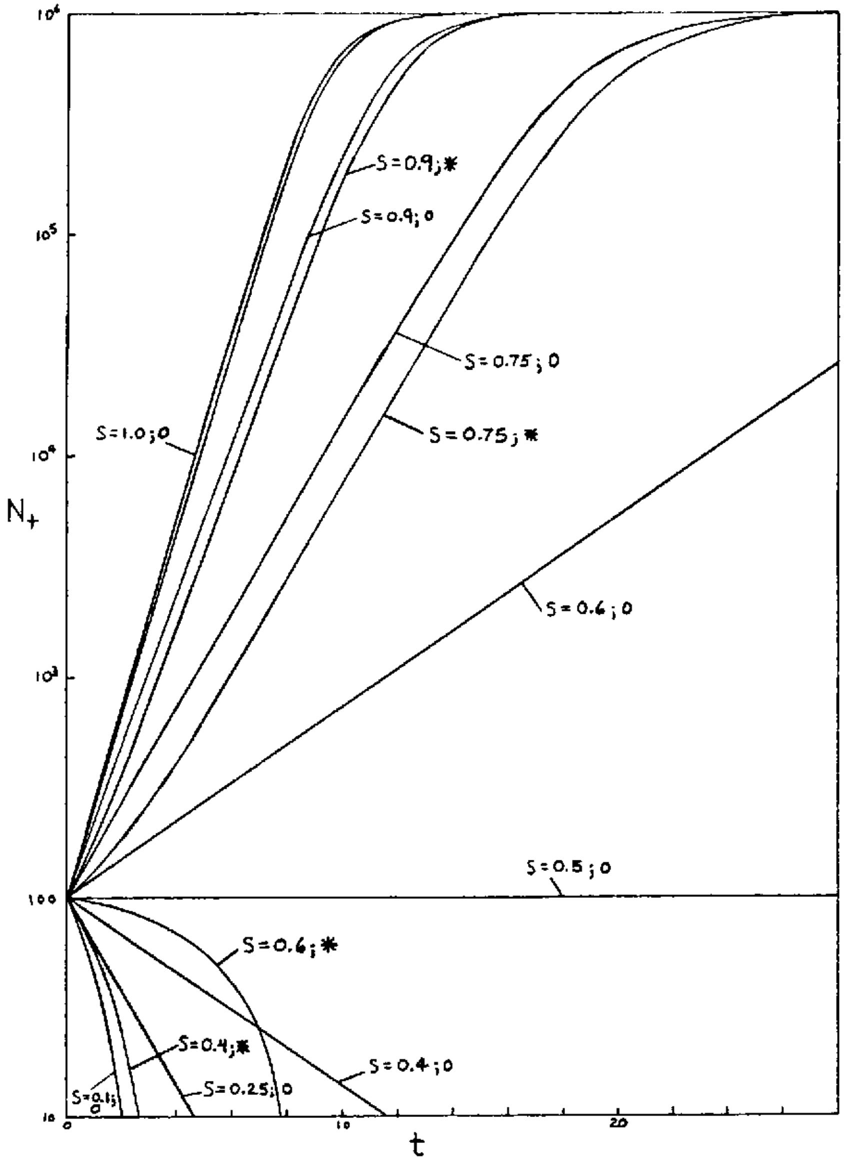

Figure 5. Publication biased paradigm acceptance with organized opposition. Plot of N_+ as a function of time as given by Eq. (58) with various values of s, $\alpha = 10^{-6}$ and $k = 0$ (compare Fig. 4), and $k = 2.5 \times 10^{-5}$ (*). It will be seen that the effect of an organized opposition is to totally block acceptance of the proposition even at moderately high levels of repeatability.

and assume Eqs. (39), (47) through (50), and (62) to hold. This gives us

$$\frac{dN_+}{dt} = -\alpha' N^2 N_+ + (\alpha + 2\alpha')NN_+^2 - (\alpha + \alpha')N_+^3 \tag{67}$$

We integrate Eq. (67) to obtain

$$t = \frac{1}{2\alpha' N^2}\left\{\ln \frac{N_+^2}{N_i^2}\cdot\frac{\alpha' N^2 - (\alpha + 2\alpha')NN_i + (\alpha + \alpha')N_i^2}{\alpha' N^2 - (\alpha + 2\alpha')NN_+ + (\alpha + \alpha')N_+^2}\right.$$
$$\times\left(\frac{-2(\alpha + \alpha')N_+ + (\alpha + 2\alpha')N + QN}{-2(\alpha + \alpha')N_i + (\alpha + 2\alpha')N + QN}\right.$$
$$\left.\left.\times\frac{2(\alpha + \alpha')N - (\alpha 2\alpha')N + QN}{2(\alpha + \alpha')N_+ - (\alpha + 2\alpha')N + QN}\right)^{\frac{\alpha + 2\alpha'}{Q}}\right\} \tag{68}$$

where

$$Q = \sqrt{(\alpha + 2\alpha')^2 - 4\alpha'(\alpha + \alpha')} \tag{69}$$

With

$$\alpha = \alpha' \tag{70}$$

and

$$\alpha = \alpha_0/N \tag{71}$$

we have

$$t = \frac{1}{2\alpha_0 N}\ln\left\{\frac{N_+^2}{N_i^2}\cdot\frac{N^2 - 3NN_i + 2N_i^2}{N^2 - 3NN_+ + N_+^2}\left(\frac{N - N}{N - N_i}\cdot\frac{N - N/2}{N_+ - N/2}\right)^3\right\} \tag{72}$$

Figure 6 shows the result obtained with Eq. (72). Here we see an initial population of 100 proponents of the proposition growing in number until the population is half of the total population. Indeed, this is the result of Eq. (72) regardless of the "seed" population. It is difficult to judge at present whether this result reflects anything realistic. It does somewhat look like what is found in the lay public where belief is the result of personal experience.

Evidential Adoption of Paradigm: Finally, let us consider what *should* be the relationship between numbers of adherents and experimental results. There should be a criterion for the acceptance of a proposition such that (a) an as yet unproved proposition should have some modest chance of attracting one or a few adherents, and (b) the chance that any significant number of individuals in the population are wrong should be small.

More specifically, let us say that for a sequence of n experiments having individual chance probabilities (for the null hypothesis) P_k; k

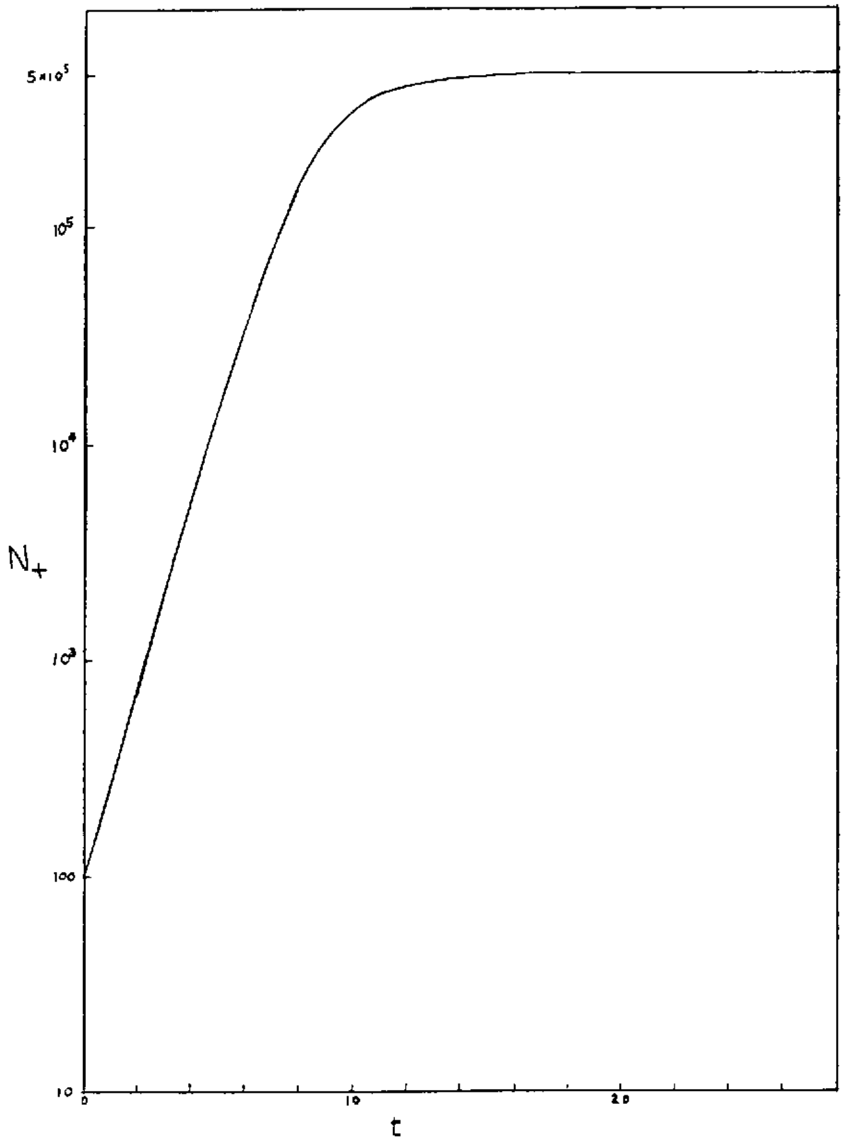

Figure 6. Effect of belief biased experimentation on paradigm acceptance: Plot of N_+ versus t from Eq. (72) with $N_i = 100$ and $\alpha_0 = 10^{-6}$. The effect of belief biased experimental results leads to a bifurcated population.

= 1, n, the overall probability that the results are due to chance is P. In that case we should require

$$N_+ = \begin{cases} P^{-1} & P^{-1} < N \\ N & \text{otherwise} \end{cases} \tag{73}$$

with N being the total number of scientists. This will give N_+ equal to one for the proposition when it is as yet untested. If all tests simply use the same criterion P_c (for example $P_c = 0.05$ as frequently employed), we have

$$P(n_+, n) = \sum_{k=n_+}^{n} f(k) \tag{74}$$

n_+ being the number of positive tests of the proposition, and

$$f(k) = \binom{n}{k} P_c^k (1 - P_c)^{n-k} \tag{75}$$

Figure 7 shows how this criterion works out for various values of the success rate s, where $s = n_+/n$, and $p_c = 0.05$. We see that as the repeatability increases toward unity, the number of experiments required to convince most individuals becomes fairly modest. The result of five experiments for $s = 1.0$ under the present assumptions is close to what is generally required of significant propositions in much of science. But parapsychology is not like other sciences in that regard. In parapsychology we find s is perhaps less than ½. Thus, we should be looking toward providing on the order of at least 15 (for $s = 0.5$) and perhaps more like 50 plus (for $s = 0.25$) experiments of such quality and of such independent sources as to satisfy a reasonable requirement for psi to be generally accepted. It is not clear that this standard has been fully met.

In what we have considered above, we see that it is important that we distinguish the issue of repeatability in psychic research from the issue of the reality of psi as an objective phenomenon.

The two points are closely entwined, but their separation is important to avoid diverting efforts in unproductive directions. The lack of replication of results when objective conditions for replication are met does not mean the phenomenon is not real, despite our tendency to be so discouraged. But when replication does not yield significant results, such negative results surely must be weighed to assess the overall standard of significance confirming or rejecting the reality of psi.

The reality of psi, as based on direct experimental tests, must be judged on some finite set of tests that either do or do not yield

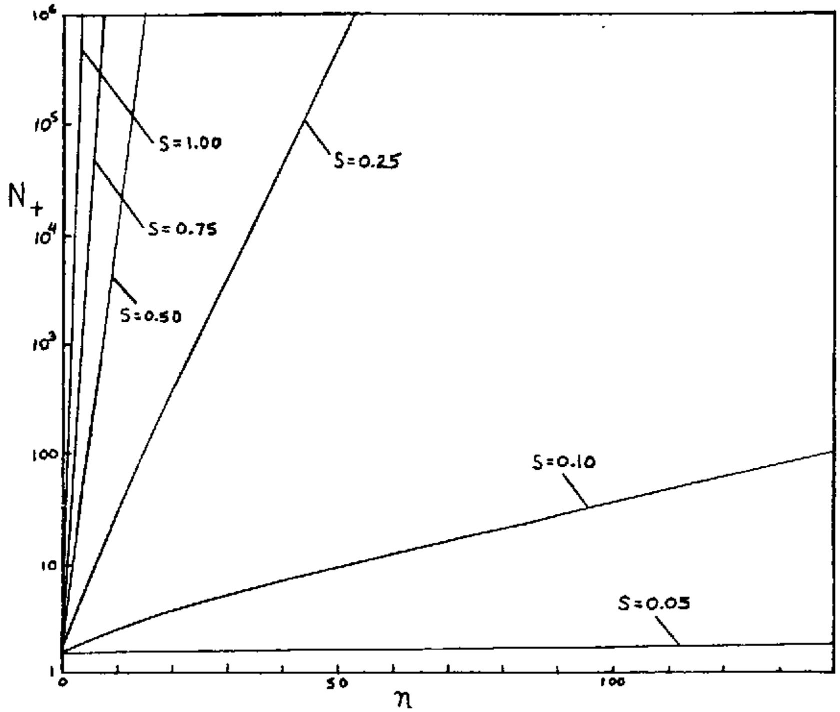

Figure 7. Evidential paradigm adoption: Plot of the numbers of adherents N_+ of a proposition versus the number of experiments performed as given by Eq. (73) for various levels of the success rate s. Note that for s = 1.0, i.e. all experiments yielding the same result on replication, about 5 experiments at the 0.05 level suffice to convince most of the population. For success rates of 0.5, about 15 such experiments are needed, and for a success rate of 0.25, we need more than 50.

statistics that satisfy one's individual requirements concerning the risk vs. advantage of investing one's time and reputation in the pursuit of knowledge in that field. It is hard to conceive of achieving significant success in the repeatability of psi phenomena without improved understanding of psi. Moreover, such success does not of itself mean we would understand the phenomena better. Repeatability must be looked upon as an expected product of understanding the mechanism of psi phenomena. The science will come first and then its reliable utilization.

It is likely that as long as we place repeatability as the first objective, we will be frustrated. We must instead use indications of relative changes in observed effects where we find them, to glean what facts we can until a consistent picture has been achieved. To some degree I feel this has been achieved already. The idea that quantum mechanics plays a role in psi phenomena has begun to enjoy some modest consideration in the community. Ultimately, the test of that theory will be its ability to shed light on the problem of repeatability. But here, as with other areas of psi research, repeatability cannot be pushed ahead of progress. We must first develop and then refine the tools we use to study the important parameters that control the phenomenon.

The QM Theory of Psi as Regards Repeatability

This writer has previously written extensively about the tie-in between quantum mechanics in physics and psi phenomena. If indeed psi phenomena do arise out of the stochastic machinery that underlies quantum mechanics, it surely cannot be surprising that there is a problem replicating results in psi experiments. One cannot do this in physics—indeed, the basic hypothesis there is that the selection of events is totally stochastic within the range of probabilities designated by the Schrödinger equation. The quantum mechanics hypothesis in regard to psychic research is that some bias in state selection can arise and that bias arises as a correlation with aspects of the observer *that are not subject to direct measurement themselves.* Only associable quantities may be tied to parameters that would correlate to high psi performance. We are, therefore, limited to characteristics of consciousness and "will." All other operants we know of depend on the body or brain functions, and these can be represented as terms in the Hamiltonian of the Schrödinger equation which describe what states can occur in quantum mechanics and not how states vector collapse occurs. If this is so, repeatability depends on determining what goes on in the mind as something apart from brain processes. Our own efforts to adhere to what we may take as scientific objectivity, restricting our research to manifestly physical and objectively measurable constraints are, from this vantage, the problem we must surmount to achieve repeatability. We must look for those things *associated* with brain functioning that are not physical parameters. We must not only identify these, but find ways to "control" these subjective states of mind or consciousness as though they were objective parameters.

The Measurement of Internal States

Perhaps because of traditional psychology's emphasis on behaviorism, much of the effort in parapsychology has been directed toward finding behavioral and physiological correlates of successful psi performance. My feeling is that this effort has met with very limited success. Such a result should not be surprising. It is not difficult to show[3] that ideas such as mental radio (or radar or any type of transmission based on the known physical forces, or even hypothetical forces having the character of known fields) cannot explain psi phenomena short of dismissing the major part of what we know about these phenomena. Since the mechanism of psi cannot lie in ordinary (or classical) physical processes, searching among ordinary physical and behavioral characteristics should not yield significant results. The objective behavioral measures simply provide tools for distinguishing differences among the types of "machine" or "instrument" on which psi effects can be manifested. One can employ a Schmidt random event generator, look for psi effects using a high gain magnetometer, or even look for voices in the noise of a tape recorder. The effect is not to be found in the type of mechanism we employ in psi experiments. The effect is not in the machine, according to the quantum mechanical theory, but upon the machine in the manner of selecting or biasing the occurrence of allowed quantum mechanical states for the machine. We must instead look for our correlates of psi amid the parameters that characterize "subjective" perception, consciousness and will. Let us, therefore, consider the subject of imagery from this vantage.

Stanley Krippner and Leonard George[4] have recently written a review of imagery studies in parapsychology. These studies embrace a wide variety of literature in psychic studies, covering imagery of apparitions, hallucinations and dreams, as well as more common visual and auditory imagery. Specific experimental work has treated eidetic imagery, passive versus active imagery, vividness of imagery, predication association in imagery, visual imagery control, presence of imagery versus absence of imagery, process versus goal-oriented imagery, imagery conducive conditions, target characteristics in imagery and the effect of practice in imagery evocation. It would not be correct to say that efforts to find correlations among these parameters have been unproductive. Quite the contrary. Some of this work has yielded promising results. But, for the most part, efforts in the direction of psychological measurements to characterize high scoring psi subjects have had limited success.

One shortcoming of this research, however, seems to be that little has been done to discriminate imagery types in terms of the internal state differences. Much of the imagery work involves efforts to find psychological characteristics distinguishing high scoring subjects. But differences among subjects that are found are likely to stem from the fact that one subject employs one type of imagery while another employs another type. If instructions in imagery experiments are not specific as to imagery type, experimental results may not exhibit differences in abilities, but merely uncontrolled differences in the selection of imagery procedures employed by the subjects.

Three imagery types are to be noted here:

1. iridescent
2. luminous
3. ideational

Iridescent imagery is imagery having a sparkling glowing quality (regardless of intensity) as, for example, can be produced mechanically by pressure applied to the eyeball (eyelid) when the eyes are closed. Such imagery is seen immediately when the eye is closed and some of this type of imagery likely originates from sporadic neuronal activity in the retina. Nevertheless, iridescent imagery may also originate cortically. If this is indeed its origin, this type of imagery is connected to sources of quantum mechanical noise in the neurovisual pathways of the central nervous system. As such, images having such an origin could have a tie to ESP phenomena.

Luminous imagery is that having a character like ordinary vision, except that the intensity can vary from the rather low level encountered in dreaming, through that of ordinary visual experiences (awake, eyes open), to super-luminous experiences (possibly as in the "blinding lights" of extraordinary religious experiences).

Ideational imagery is imagery that is not actually experienced as a visual image, but that is a "conceptual" image having spatial structure. With your eyes open and while watching any activity, if one says "visualize a dog," a conceptual image of a dog is easily formed, yet is non-visual. This image has no luminous character to it, yet it has pictorial content; one can sensibly ask if the dog faces left or right, for example.

As mentioned, experiments have been conducted to evaluate the significance of vividness in imagery, but not to evaluate types of imagery. Moreover, imagery types should be studied from the standpoint of their source relative to the occurrence of consciousness and will.

The resolution or data capacity in imagery also has not been studied as an independent variable (except indirectly as this variable relates to psi in dreams). As we will show below, the signal to noise ratio in psi phenomena is tied to the total data capacity of the imagery. The reduced data channel capacity imagery, as encountered in dreams, results in an increased signal to noise ratio which can result in improved psi performance. A similar situation should be obtained for ideational imagery as such images have very low data content. The opposite is to be expected where eidetic imagery is employed in tasks like remote viewing. Puthoff and Targ[5] have found evidence supporting this observation. But sometimes in all this imagery there may also occur the pure psi (will) data channel with an image capacity of about 1000 bits and a data rate for the channel of about 10^4 bits/sec. Can this channel be perceived in its pure form? Refined studies on image types may bring us closer to an answer. Let us turn now to the question of signal to noise enhancement.

Signal to Noise Enhancement

In the quantum mechanical theory of psi, the signal to noise ratio is given by[6] W/C, where W is the will data rate and C is the consciousness data rate. The value of C is given by

$$C = nN/M\tau \qquad (76)$$

where N is the total number of synapses in the brain, n is the number of transmission molecules (opposite vesicle gate molecules and approximately equal in number to them) in the post synapse, M is the total number of transmission molecules (soluble RNA) in the brain and τ is the time required for electron tunneling from one transmission molecule to the next. Typical values of these are $n = 2 \times 10^5$, $N = 2.35 \times 10^{13}$, $M = 7.45 \times 10^{20}$ and τ can be calculated[7] to be about 8.4×10^{-12} sec. The value of W is given by[8]

$$W = \log^2 (Ct_s)/Ct_s \qquad (77)$$

where t_s is the synaptic transmission delay time, 3×10^{-4} sec. The signal to noise ratio R is therefore

$$R = W/C = \log_2 (Ct_s)/t_s \qquad (78)$$

Choosing a reduced C state, as in a dream state, will give an increased value of R. In the standard conscious state with $t_s = 10^{-3}$ sec and $C = 10^8$ bits/sec, we have $R = 1.7 \times 10^{-4}$. If we take $C = 10^5$ bits/sec, as a value appropriate for a dream state, we have $R = 0.07$ and W (dream state) = 7000 bits/sec.

These results show how one may expect to obtain improved results in experiments using imagery that would seem at first to have a lower potential for successful psi functioning, because the value of C is reduced. To study such issues, however, it is necessary to develop techniques for measuring data rates for these internal imagery states.

*Measurement of the Data Channel Capacity in Free Response Imagery**

As discussed above, control of the data capacity of imagery may be useful in efforts to enhance performance in psi experiments. If so, it becomes important to develop techniques to "measure" the data capacity in imagery under the conditions of psi performance, as in Ganzfeld testing. Such an experiment has been carried out by Marilyn J. Schlitz of the Mind Science Foundation and Evan Harris Walker. Let us look at how this experiment was conducted and its significance.

Theoretical studies of psi phenomena have indicated the existence of a specific data channel for psi information.[9] It has been calculated that this channel has a data handling capacity some four orders of magnitude smaller than that of our ordinary sensory channels, as mentioned. Further, according to the quantum mechanical theory of psi phenomena, psi information does not involve specialized sensory organs, but instead employs the ordinary (visual, auditory, etc.) sensory neural networks of the brain. Therefore, extrasensory imagery, as in remote viewing and Ganzfeld experiments should reflect a corresponding degradation in its information capacity as compared to ordinary sensory information. This means that where ordinary visual imagery is limited to a resolution of about 10^7 bits (at any instant, with a data rate of about 10^8 bits/second), a remote viewing image should be limited to images having about 10^3 bits. Such degraded remote viewing images would explain much about the qualitative nature of the mechanism of psi information handling processes occurring in remote viewing experiments.

Within the past decade, free response procedures have moved to the forefront of psi testing, providing a broad range of target/response possibilities. Procedures such as remote viewing and Ganzfeld are providing strong support for psi.[10] Reviews of this literature show that better than 50 percent of the experimental studies provide significant evidence for a psi mediated information transfer.[11] Perhaps the most outstanding aspect of free response procedures is the rich qualitative material that is generated within an experimental setting.

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* This next section was co-authored by Marilyn Schlitz, Research Associate with the Mind Science Foundation, San Antonio, Texas.

Viewers are typically asked to describe their impressions of a distant target, be it a picture, object or geographical location. Imagery is usually reported as the mediating vehicle by which these impressions are integrated into consciousness. Holistic information, such as shapes, forms and colors are emphasized, with sketches often supplied by the viewer when descriptions fail to provide the necessary information. Despite the richness of the experimental protocols, however, psi impressions rarely provide complete and perfect information concerning the target.

Walker[12] has suggested degraded imagery resulting from the low resolution (and noisy) psi information channel as the cause of various characteristics of remote viewing responses. Walker has pointed out the similarity between remote viewing drawings and data degraded photographs of remote viewing targets. The actual quantitative value for this degradation is important, since it can be compared to the theoretically predicted value. Confirmation would give strong support to the quantum theory of psi phenomena. Success in this area could significantly advance the status of parapsychology as a quantitative and predictive science. It was with this in mind that the following experimental effort was carried out by Schlitz and Walker.

A complete and thoroughgoing experiment to definitively establish the data content and channel capacity involved in remote viewing is a formidable task that must ultimately require the detailed study of a half dozen or more specific issues. How many images must be simulated, and how much time should the subject be permitted to examine these images? How should color degradation be handled to keep it compatible with the image degradation used to simulate low information levels of mental imagery? How should transcripts be edited? But this study must begin by considering the results obtained by the simplest and most straightforward procedure. Thus, this present experiment represents much more than an exploratory effort, though something less than the definitive determination of the channel capacity for remote viewing.

Design of the Experiment: In this experiment, preselected subjects were asked to describe their sensory impressions of target slides which had been systematically degraded in the amount of perceptual information available. These responses were then compared with responses made during psi impression periods, previously conducted with the same target images. Three judges, blind as to the order and nature of the information concerning the transcripts, were asked to read through all the transcripts (simulated Ganzfeld mixed in with the transcripts from the actual psi experiment) for a given target, and to rank order them according to the amount of information which is provided in each transcript about the target picture.

Targets: Targets for this experiment were selected from a series of Ganzfeld sessions, carried out at the Experiential Learning Laboratory, Duke University, in the summer of 1982. They were photographs, cut out from various *National Geographic* magazines. Ten targets in all were selected on the basis of the good quality psi impressions which were associated with them.[13] Each of the photographs was then rephotographed by E.H.W., who progressively defocused each one seven times. In this way, the amount of information available for each picture was 10^6, 10^5, 10^4, 10^3, 250, 100, and 10 bits.[14]

Participants: While five subjects were preselected for this study on the basis of their initial success in the Ganzfeld condition, only four were tested.[15]

Procedure: Subjects were tested on an individual basis by M.J.S. They were told that the study was a follow-up of the initial Ganzfeld project and that we were attempting to quantify the data rate in free-response experiments. They were told that they would be shown a number of slides, each at different degrees of resolution (blurriness). They were asked to sit back and relax in a small room at the Foundation for Research on the Nature of Man, and to describe their impressions of the target slides. Furthermore, they were asked to report raw impressions and not to analyze their images. Each subject was shown seven slides and was allowed to study each for several minutes. They were never shown the same picture twice. A tape recorder was provided to record the verbal responses and subjects were encouraged to make drawings of their impressions. They were shown the photographs at full resolution following completion of the experiment.

Judging Preparation: Following completion of the subject interviews, M.J.S. returned to the Mind Science Foundation for completion of the study. There, she typed the verbal reports that had been recorded on tape, coding each.[16] All of the transcripts, including the Ganzfeld transcripts, were ordered according to the target picture which they represented. For each of the ten pictures, there were four or five transcripts.

Judging: Three independent judges were asked to order the transcripts in each packet according to the amount of information relating to the target picture. They were given a judging sheet, which provided spaces to record the transcript code and the ranking. A number 1 was assigned to the transcript with the most similarity to the target picture, a 2 to the second, and so on.

Evaluation: Given the limitations of the study, the results are rather impressive. With ten target pictures and three judges, we obtained thirty rankings of the true Ganzfeld transcripts relative to the simulated transcripts taken for images of known data capacity. Fourteen of these fell below the level of 10 bits of information, two ranked between 10 and 100 bits, seven between 100 and 250 bits, five between 250 and 1000 bits and two placed above 1000 bits. The judges were very consistent in their rankings. The average value of the Kendall coefficient of concordance for the judges in the ten ranking tasks was 0.95. We obtain a better picture of the concordance if we look at the data for only those six of the ten target pictures for which two or more judges ranked the Ganzfeld above 10 bits of information, and where we retain only the rank positions for the Ganzfeld and the simulated transcripts immediately above and below the Ganzfeld position (this removes from the measure of concordance the rather trivial agreement the judges reach concerning the placement of transcripts for pictures having 10^6 bits capacity). In this case we obtain for the average Kendall coefficient of concordance 0.93 (maximum and minimum values obtained were 1.00 and 0.78; the concordance values are significant at the 0.05 level for five of the six high data targets and at the 0.07 level for the sixth). If we omit Ganzfeld transcripts measured at less than 10 bits of information, the weighted average information content (i.e., $\Sigma wn/\Sigma n$ where n is the number of judgings having w bits or more in the image) yields 248 bits. This result is an important fact. Perhaps even more significant is the fact that one of the transcripts evidenced more than 250 bits for one judge and more than 1000 bits for the other two judges (in this case, Kendall's coefficient of concordance was 0.91 and significant at the 0.05 level).

Our results here must be considered tentative, but they suggest a channel capacity ostensively in accord with the quantum mechanical theory. Note channel data capacity and image data capacity are to be measured in terms of the maximum information content the channel can hold and, therefore, the maximum (significant) measures rather than average measures are to be compared with theory.

Discussion: In this experiment support for Walker's theory was obtained. We recognize a number of limitations within the present exploratory effort. In future efforts it will be important to generate the psi data and the perceptual data within the same experimental period. The use of pre-existing data in the present work was done for the sake of availability. Several of the pictures used in the present effort were difficult to degrade, due to factors such as content, color and brightness. While we controlled for this as best as possible, a future study would involve targets which were preselected for purposes of data degradation.[17] Some interesting points were noted in the process of this study. For one, we noted that subjects made the same perceptual errors on a specific target picture as did the psi viewer. For example, one picture was of an experimental monkey smoking a hookah. Each subject described the picture as showing two faces, as did the Ganzfeld participant, when in fact there was only one face in the photo.

An interesting line of research, which has common features with this study, is that of the subliminal perception work. While such an effort manipulates the amount of time in which the target is observed, in the present investigation we have manipulated the amount of information that is being viewed. In future work, we will manipulate the time of exposure as well.

The Karnes et al. Remote Viewing Replication

Not too long ago, the writer[18] became involved in a controversy over "replication" of the remote viewing protocol as reported by Karnes et al.[19] The basic design of the experiment was as follows (we quote from their text): "Eight self-proclaimed psychic subjects, self-selected into sender-receiver pairs based on their previous experience with psychic communications, participated in several remote-viewing trials. Two pairs of subjects were each given ten remote-viewing trials. The other two pairs of subjects were each given six remote-viewing trials. Sender-receiver pairs alternated roles as sender-receiver.

"The following procedures from previous successful demonstrations of remote-viewing were used: (1) An experimenter was closeted with the receiver during the sessions; (2) A double-blind procedure was used in that the experimenters and receivers had no knowledge concerning the number or identity of the target possibilities; (3) Receivers recorded their impressions by offering narrative . . . and drew free-hand sketches of their visual impressions; (4) Feedback was provided by having the sender debrief the receiver at the target site immediately following each trial. The debriefing session was open dialogue between the sender and receiver. . . .

"Sixty-four independent judges evaluated the accuracy of remote-viewing by comparing the receivers' protocols to the actual target sites and to records of the senders' experiences. Records of senders' experiences included a color movie of the sending situation and typed narrations of the senders' tape-recorded verbal impressions of their sending situations.

"Sixteen distinctively different target sites were used for the 16 remote-viewing trials . . .

"The order of target sites to be used for the trials was determined by a table of random numbers. The principal author was the only person aware of the identity of the target sites and the order of use . . .

"Sender-receiver pairs met the principal author at his office . . . Receivers were isolated with an experimenter in a small quiet conference room. Receivers tape-recorded their impressions and drew free-hand sketches of their visual impressions . . .

"Senders were escorted by the principal author in his auto, or by walking, to the target site. They were instructed to use a Polaroid Motion Camera to visually record their impressions of the target site and to verbally record their subjective impressions by using a portable cassette tape recorder . . .

"At the end of each sending session, the receiver was escorted to the target site by the principal author where he/she met the sender . . .

"A professional transcriber typed the receivers' and the senders' narrations. . . . In preparing the receiver and sender protocols, all references to personal names, dates, times and gender references to the sender or receiver were carefully edited out to eliminate any extraneous clues that judges could use to evaluate the accuracy of the remote-viewing."

Judging: "Sixty-four independent judges evaluated the accuracy of the remote-viewing data. Judges were read a set of instructions which explained the purpose of the experiment and how they were to judge the accuracy of remote-viewing. Judges were run in groups of four or less. Each of the 16 target sites was evaluated against the entire set of 16 receiver protocols by four judges. Each judge was given one sender's description to read. After reading the narration, judges were shown the movie taken during the sending session followed by a visit to the actual target site. Judges were then given the entire set of 16 receivers' protocols and were required to separate the 8 that best matched the target from the 8 that least matched the target. Judges rank-ordered the 8 matches with 1 used for the best match through 8 for the least best match. Judges were allowed unlimited time including overnight to complete the judging."

Results: "A hit will be defined as a judge's selection of the correct receiver's protocols for a target site. Since the judges were required to select the 8 best matches from among the set of 16 receivers' protocols, the chance proportion of hits for the 64 judges was .50.

Evidence supporting remote-viewing would be obtained if the proportion of hits was reliably greater than chance expectations. The 64 judges obtained 25 hits. That proportion (25/64 = 0.39) was not significantly different from chance, $Z = -1.76$ $p > 0.08$."

Karnes et al. also obtained a rank-order statistic using half of their data and something less than the recognized method of evaluation. This statistic yields $p > 0.60$, i.e. non-significance. Karnes et al. conclude: "The results of the experiment offered no support for the existence of a remote-viewing paranormal perceptual capability in a group of experienced psychic subjects." It is not appropriate to discuss the shortcomings of the Karnes et al. study here. The details have been given elsewhere.[20] It is appropriate to point out the principal flaws as they bear on the issue of repeatability.

First, if one intends to replicate an experiment, then why depart from the specific successful protocol in ways neither necessary nor justified by any presentation of facts to establish a deficiency in the original protocol. The embellishment of Puthoff and Targ's protocol in which the "senders" take movies of the target site and make tape recordings of their impressions, while seemingly more "scientific" can easily adversely affect the ability of the judges to cope with the extensive data to be judged. If the reader has never been a judge in such an experiment, this may appear to be an over-statement. Transcripts of 16 sessions, each containing three or four pages of dialogue, an assortment of pages the judge must use to score targets if records are to be maintained in a complete fashion, movies, tape recordings and trips, *repeated* trips to the sites must be handled by the judges. And buried in all this material, if remote viewing is indeed a real phenomenon, there may exist no more than 5 bits (corresponding to a significance level of slightly better than the 0.05 level of confidence) that hold the difference between success and failure to replicate. The use of large numbers of judges constituted a further deviation from the practice of Puthoff and Targ. While the use of more judges can reduce the chance of a type 2 error, this requires that the judges be unbiased. Moreover, there is the clear hazard that the use of 64 judges will trivialize the experiment, which again Puthoff and Targ have stressed must be avoided.

Second, it is likely that the judges had been motivated to give biased results. Although never specifically pointed out, it appears that the 64 judges were Karnes' psychology students at Metropolitan State College (Denver, Colo.). It is likely these judges had some awareness of their professor's expectations. That Karnes et al. harbored and conveyed particular expectations is evidenced by the authors' presentation of their material. In their first paragraph, the proponent literature cited is a mishmash of both the scientific literature and such as *Occult Medicine Can Save Your Life* (1977) and Vallee's *The Invisible College: What a Group of Scientists Has Discovered About UFO Influences on the Human Race* (1975), etc. The opposition side is characterized by "on the other hand by the development of societies and journals concerned with critical evaluation of the paranormal . . ." It has long been a source of irritation among parapsychologists that their critics will not present legitimate parapsychological references, but rather go to the drug store to seek out their opponents' definitive literature. Their paragraph two sees "scientific proof" set off in quotes to point up that the term "scientific proof" is not a term to apply to the paranormal. Paragraph three begins, "the scientific respectability claimed for remote-viewing . . ." And these cues to the reader as to the kind of objectivity with which Karnes et al. approach their task of experimental replication continues throughout their report. To have stated that they did not obtain results corresponding to those obtained by Puthoff and Targ would have spoken to the issue clearly. The fact that an objective report was not presented by Karnes et al. in any part of their paper, strongly argues that such bias probably existed during their experiment. As there was the opportunity for these experimenters to have influenced the subjects with information leakage, it must be concluded that the experiment reported by Karnes et al. does not represent uncontaminated results.

Third, the statistical procedures employed by Karnes et al. are not valid. The statistics are rendered invalid by a fault that has for quite some time become recognized by parapsychologists as a subtle but very serious hazard in conducting parapsychological research. This hazard enters whenever a less than optimum, other than standard, or multiplicity of statistical analyses are carried out on a single set of data. In the experiment of Karnes et al., the authors have altered the procedure for handling the statistical evaluation of the experiment from that used by Puthoff and Targ and thus open their results to the criticism of having *sifted* their data, as when they throw away and thus do not rank-order half of all the target-transcript sets.

Why, we may ask, select out the eight best? We could also calculate statistics in which we select only the best 4, or the best 6, 10, 12 or retain all 16. Each procedure will yield a different statistic. The more ways in which there exist *degrees of freedom* in the selection of our statistic, the less meaning one can attach to that statistical result. Suppose one were given a $p = 0.60$ statistic and told that it had been selected as the largest result out of ten possible ways of constructing a statistic. What should be taken as the probable smallest statistic? In the extreme case where each calculation is taken to be independent this would yield a corrected value of $p_C < 0.05$! In the above example clearly no one can accept the 0.60 statistic as valid. Because of Karnes et al.'s handling of the statistics one cannot ascertain what the correct statistic should be. If Karnes et al. were biased in their selection, the true statistic might be 0.05. One can state what the correct procedure for handling the statistics should have been. It should have been a complete rank-ordering.

Fourth, a remote viewing experiment of the type Puthoff and Targ have conducted seldom yields results that read like an eyewitness account. Instead it is found that the transcripts must be evaluated by a judging procedure. This procedure and the overall experiment frequently yield results that are at the $p \approx 0.05$ level of confidence for unselected subjects. Puthoff and Targ[21] have reported insignificant results having been obtained with novice subjects. The experiment is designed to achieve a measure of the information transfer under the conditions of the remote viewing protocol. While frequently expressed in terms of the probability level of confidence, this is equivalent to a measure of the information transfer in units of *bits of information.* Thus, a 0.05 level of significance corresponds to somewhat less than 5 bits of information, more precisely 4.32 bits. Now, this is the point. For an experiment having a mean value of 4.32 bits, the Poisson distribution yields a chance of 0.57 that replication will not yield significance. While Karnes et al. give us their statistic, they fail to calculate the probability that *assuming* the existence of remote viewing as indicated by the results reported by Puthoff and Targ, their experiment would have failed to have detected its existence (under the assumption that it is the statistical variability that is to be taken into account in the calculation). As several experimenters have replicated the remote viewing results as obtained by Puthoff and Targ, to be acceptable as contrary proof Karnes et al. should have to obtain a counter-proof statistic less than that of the null hypothesis statistic established by the composite of the prior research work.

Fifth, we should further demand that such a calculation be carried out under the additional constraint that Karnes et al.'s results are to be compared to those of Puthoff and Targ in which task naive subjects have been employed. The fact that Karnes et al. used subjects who *said* they were psychics is quite without significance. The proper testing procedure as employed in parapsychological research is to treat subjects as novices unless experimental results have been obtained to indicate otherwise. Proper testing procedure would require prior screening tests before accepting the subjects' self designation as valid. Their experiment should, therefore, be considered as an exhibition of results to be expected under the constraint of task naive subjects being employed. Since Puthoff and Targ obtained more nearly marginal results working with task naive subjects, the Karnes et al. results are less surprising.

Finally (this criticism is perhaps of the most critical importance in efforts to replicate the experiments of Puthoff and Targ) one must design the experiment so that a *bias* on the part of the experimenter or on the part of the judges will *automatically* yield results that exhibit this particular flaw. In the experiment of Karnes et al. it is clear that this failure to replicate can easily be attributed to the *ability* or the *inclination* of the judges to incorrectly pair targets with transcripts, even assuming that sufficient information does exist in the transcript that would allow for correct pairing. Now, when Puthoff and Targ conducted their experiments which yielded significant results, these results provided statistical evidence not only that remote viewing was operating, but that the judges employed were capable of detecting information cues in the transcripts designating the corresponding targets. No *control* trials were necessary under these conditions as the *a priori* null hypothesis statistics are known. However, Karnes et al. do not obtain significance. This can mean *either* an absence of remote viewing information transfer *or* poor judging.

Poor judging must be eliminated by additional *control* tests. For a proper experiment, Karnes et al. would have to insert a number of transcripts under *double blind* procedures that have been specifically altered so as to contain adequate information to just correspond to the 0.05 level of significance (or about 5 bits of information) for an equal run of the simulated transcript-target pairs. As this was not done we may conclude that the judges could have been inadequate to the task. Indeed, the judges, confronted with so much material and a hostile attitude toward results pointing to a reality of psi could easily have randomly rank ordered the transcripts. I have encountered this being done by a judge in a similar test situation. Here we have looked at one "failure to replicate" in some detail. This, indeed, may be one of the better such studies. But it is seriously flawed from the standpoint of anyone wishing to know of the reality of psi. Yet this is the kind of experiment done by critics to show that psi fails to replicate. This is the only failure to replicate I have examined in detail. Yet it is flawed and it draws conclusions that go well beyond the bounds of the evidence presented by the experimenter. The reason these researchers get away with this is the repeated admonition that "claims of the extraordinary demand extraordinary proof." As Karnes et al.[22] note, "the demand for replication is particularly crucial in an area where the findings appear to violate well-established physical laws." Yet this simply is no longer a valid argument. Physical law, specifically quantum mechanics, already has established that the physicalistic picture of reality (the actual basis for the contention that physical laws are violated) is not tenable. The structure of the universe includes observer interaction with the state description of the physical world. As such, the state of mind of the observer must play a role in physical events. The real issue is simply one of showing when, where and how much.

Conclusion

In addressing this whole issue of repeatability of experimental results, it has seemed the issue is to a great extent simply not in focus and indeed may be entirely bogus. Certainly, we all know experimenters who have not achieved significant results. And of course it is not surprising to find that a phenomenon dependent on participant intent will yield poor results at the hands of severe skeptics or those having little intent to conduct meaningful tests. But what is the real status of repeatability? If an experiment conducted in a particular fashion with unscreened subjects is expected to turn in a result at the 0.05 level, should there be great surprise if half the experiments fail to yield statistical significance? Should we be apologetic for investigating a subject for which we must carefully sift our data to bring to light evidence of its existence? Should we be reticent about our conception of nature because it differs from that materialistic paradigm already experimentally disproved? To the contrary, we must become more aggressive advocates.

Now, if our concern in regard to repeatability is the difficulty of verifying experimental findings, this is a serious and valid issue. But it is one that will yield to the straightforward application of time and effort. If, however, the issue is a perception that critics cannot verify our findings or that some of our colleagues do not find statistical significance in their efforts, the issue is largely bogus. Such fluctuations in experimental findings are to be expected, even experimenter to experimenter deviations, especially in those ill-prepared to achieve significance or those seriously disinclined toward a positive outcome because of attitude, inclination or training.

There is such frustration for so many parapsychologists. Yet I find it incredible that what seems so universally to be beyond belief has proved to be so readily knowable, to be so easily seen as something that must be so. It is rather all the other disciplines that really suffer. Philosophy grapples in frustration over consciousness and reality; physics puzzles over the measurement problem, confessing[23] "quantum mechanics seems to reject our intuitive notion of realism . . . that physical systems have well defined properties whether anyone measures them or not;" psychology is caught between rejecting consciousness and the clear need to deal with this fundamental nature of humans and animals—consciousness surely exists, but what function does it serve in modifying behavior? But in parapsychology, we have the examples of many who have already shown the phenomena to exist; we have contemporaries who continue to provide ample evidence. We do have procedures that permit of a very satisfactory success rate in replication; we do have a theory that provides an acceptable rationale to tie psi phenomena to quantum mechanics; we do, in fact, have phenomena predicted by theory and found experimentally. If that were not enough, many of us have also had the opportunity to see it.

We presently live in a rare state of grace. This incredible field of scientific research is still left to us to explore fully and carefully. Treasure these days for they may be few in number. And when they are gone, you may find we have all been swallowed up by the true professionals—the "industrialists" of scientific research.

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13. Each of the transcripts had been assigned a rank of 1 or 2 in a blind judging procedure. While the overall results of the experiment did not yield statistical significance, the five subjects who were selected were independently significant when we took away their first direct hit. We recognize this data selection as an experimental limitation, but justified for purposes of this exploratory effort.
14. The slides were degraded by projecting the image and then photographing it again with a different setting. This was done in a very careful and methodical fashion by E.H.W. Not all slides were used as some degraded better than others. Each target picture was represented, however.
15. One subject was unable to participate as he was in the hospital during the testing period.
16. Ideally, typing should be done by a third person, blind as to the correct targets. This was not possible in the present effort due to a lack of clerical assistance. For future studies, this extra step would be taken. However, the tapes are available for anyone interested.
17. Since the photographs were selected for their artistic quality and strong sensory impression, the image content tends to remain high until the image is severely degraded. This renders some of the photographs less suitable for discriminating differences at the upper end of the information content scale.
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DISCUSSION

HONORTON: As usual, Harris, I am totally fascinated by the torture you put us through with all these equations. And as usual I don't have the slightest idea where you get your numerical estimates from. Where does 10^8 bits for "awake" come from, where do you get 10^5 bits for "dreaming"? Is this intended to be purely heuristic or is this to be taken seriously?

WALKER: No, this has been derived and the results published. It is in the literature. On those viewgraphs that I have shown the data rate or C is the product of the number of synapses divided by the number of soluble RNA molecules in the brain and also divided by the time for quantum mechanical tunneling between neighboring molecules. This result comes from the quantum theory of consciousness. You can get an experimental confirmation of this value fairly easily just by knowing the data rates for sensory data input required in order to just saturate visual imagery. That takes better than 10^7 bits per second. For example, the data rate for a movie (cinema) is about 10^7 bits per second.

HONORTON: Does the quantum mechanical theory of psi or consciousness say that the nature of the device makes no difference in terms of the psi detection system? Then is there a contradiction here where you are deriving differences on the basis of the neuro-machinery?

WALKER: People have psychological attitudes with regard to different devices or procedures and that certainly is going to affect things. There really isn't evidence that there is a particular type of machine that you can use that will elicit psi and other machines that won't. There is reason for it in the equations, as given by the theory.

HONORTON: This is an important enough issue that we can't accept that broad a conclusion on the basis of what has been done so far with a very small number of systematic comparisons with different types of physical systems. Your theory says that the nature of the device isn't really important. And yet what you are saying is that the information rate coming out of consciousness is an important factor. You are deriving that information rate from estimates of how the neural apparatus is working and so it seems to me there might be a contradiction there.

WALKER: No, remember that one device you speak of is a machine, the other has a special property, consciousness. The conscious mind is something very different. Now, as for the machines used in ESP experiments I said differences relate to probability and there are differences that come in there; the actual machinery doesn't matter. The brain, however, is a source of consciousness; these physical devices used in psi experiments are not. Changes in conscious attitudes in the brain can affect psi performance, but changing devices—as such—will not.

SCHMIDT: One comment to the first part of your talk, where you seem to apply statistical methods that have been very successful in economics to predict future trends. I think the big problem is that we don't have large numbers of participants. We depend very much on singular events, like the existence of J. B. Rhine, the existence of Uri Geller and perhaps the invention of the Ganzfeld method. There are just too few events. Continuous differential equations don't seem very applicable.

WALKER: If I were a much, much better mathematician or a master at programming the computer, I would have expressed this work in terms of discrete mathematics. We are trying to address a population that is on the order of a million scientists overall. We do have a community in the hundreds and we do have a fairly good steady stream of experimental results that are read by a scattered sampling of people. I don't want to make it seem that what I have done is really precise. I don't want to deceive you in that fashion at all. I simply indicated that there are some indications from what has been done that suggest that we should be aware that science may not come up with the truth no matter how long it follows the present procedures.

SCHMIDT: My second question concerns the quantum theory of psi. There is certainly not only one quantum theory of psi. We cannot categorically say that the quantum theory of psi predicts this and that. That is specifically the case with regard to the role of future observers. You hinted that this might be a very vital point for our discussion of repeatability; that perhaps the future observers might play a role. On the other hand, I think one can give models of quantum theory where the future observers do not play a role. I somewhat regret that this role of the later observer didn't come up more in our discussions.

WALKER: Time independence comes into the theory because of physical constraints on the theory. I am trying to adhere as long and as far as possible to the demands that have been established in physics, without touching anything that exists in ordinary physical science. The only thing that I can toy with to create a viable theory of psi is the fact that state vector collapse can be postulated to be caused by something associated with consciousness and observation. A part of the regular physics is the Lorentz variance. In order to make the theory work spatially, in the first place, I have to add that there is a temporal independence in psi phenomena. The Lorentz variance requires me to treat space and time on an equal footing. Now, the existence of a spatial independence for state vector collapse has been known for a long time. This has now been confirmed in the test of Bell's theorem. For the state vector collapse—whatever collapses it—you do have this characteristic of non-locality and this is established. Now, unless I postulate temporal independence, the theory will violate Lorentz variance. We are forced into an absolute temporal independence. This has some consequences, but I don't feel they are disastrous. As a matter of fact, I think we do see these effects. Experiments designed for wide dissemination among a critical scientific audience have quite a difference to them as regards the magnitude of the psi effect. My impression of the literature seems to confirm this expectation of the time independence of the theory. Others can address it from their own point of view and I would not mind going into a careful study of this point.

There is an infinity of theories that can be presented and there are an infinity of theories that you can call quantum mechanical theories. The question is what kind of a theory will you come up with if you make the fewest number of postulates that deviate from what is accepted in quantum mechanics? This is what I have tried to do and if I have deviated from that then this should be pointed out, certainly. What I have called the quantum theory of psi deviates from conventional physics only in taking at face value the implication—already noted as the cause of the measurement problem in physics—that consciousness causes state vector collapse. This is all that is needed in order to construct an adequate theory of psi.

BELOFF: While the aim of quantifying the psi process is obviously enormously ambitious, enormously exciting and challenging, one feels a bit uneasy because one wants to be sure that the quantification is valid in the first instance. When you use different pictures with successive degradations, how do you quantify the bits of information in each of them? For example, you showed a blurry picture of an insect. How did you work out how many bits of information that contained?

WALKER: The best way to do this is to put it on a computer where you have full control. We hope to do that in the future. In optics, when you defocus a spot you wind up essentially producing a pixel. It is a little bit fuzzier than what you would get if you generated the image from a computer with pixel by pixel readout, but it contains the same kind of information; that is, one bit for each black or white element of the picture. If you take into consideration the color resolution, the information for a single pixel is on the order of 10 to 30 bits. I would not stand by the accuracy of that number any better than a factor of 2, 3 or something of that sort.

BELOFF: But, anyway, it is a form of quantification that bears no relationship to information in the nontechnical sense.

WALKER: That is right, it is the technical representation of information content presentation. These are the values one obtains using conventional information theory. But this same treatment has been tied in with the conception of consciousness in prior theoretical work. Now, this is not the information measure of "insect" as in the above picture. What is that anyway if I do not specify who and when and what context is involved? But the picture itself does have information capacity that limits how much information is communicated by the picture. This is a well established, technically precise way of talking about information.

BLACKMORE: If you look at what we know about perceptual processes, it is absolutely clear that a pixel by pixel count is quite irrelevant even at the lowest level of perceptual processing. For example, if you do that kind of pixel by pixel degradation, you do not find any kind of simple monotonic relationship between the ability to recognize the picture and the pixel count. Now there are many other ways of degrading pictures which have more relevance to the kind of early processing that is going on in the visual system. For example, if you do spatial frequency analysis you find that there are certain spatial frequencies which are crucial to recognition and others which aren't. So you can measure the amount of information at different spatial frequencies and get entirely different results in terms of what the person is going to see in the picture in terms of recognition, in terms of what he'll say, or anything else.

Now, clearly it is things like recognition, like seeing things in the picture, which are going to be crucial. Perhaps it is controversial, but I believe it is reasonable to say that it is things like what we see in the picture which is ultimately going to be related to psi. I don't think anything which measures information in terms of a pixel by pixel count can be relevant at all.

WALKER: First of all, I disagree absolutely categorically with what you have said. If you make a spatial transformation or a Fourier transformation of a picture, then you know you will have something that appears different. But the Fourier transform will have the same amount of information as had the original. Simply making a Fourier transform does not either create information or destroy it so long as you have the same resolution and the operation is noise free. That is a fundamental concept in information theory.

BLACKMORE: I was talking about filtering, not transform, taking out some information.

WALKER: They are the same things. Spatial filters are a physical way of doing a Fourier transform.

BLACKMORE: But you take out information that way?

WALKER: Just by making the transform you neither create it nor take it out unless you do additional operations for that purpose. And this is something terribly misunderstood almost throughout the scientific community, I am afraid. People have talked about holograms. Here you have in a little corner the whole representation of everything that is on that hologram. That is absolutely false. The only reason that you can use a hologram to do that is that the photographic plate has such a high resolution that you can actually put a million bits of information in a little speck of the photographic plate. By using a Fourier transform you can retrieve a photographic image from that spot. I could also have stored the information without using a Fourier transform spot by spot across the photographic plate. The Fourier transform does not create or destroy information.

When I hear people talk about remote viewing or when I see the drawings that have been made by people like Hella Hammid or Ingo Swann, they seem to be saying they have a mental image. I don't know experimentally what the information content in that image really is. They seem to be saying they have a channel capacity there, a capability to resolve an image at some level or other. They seem to be making errors when they try to tell you what they are seeing—even in the best of psi experiments. They will have things placed in the right place, and yet they will misidentify things perhaps because of the resolution of their mental image. I don't know what the information is as they see it. The only way to get it is to try to do an experiment in which I give them a chunk of information, a quantifiable chunk of information, and then I see how they handle it. We look to see how they degrade information in remote viewing experiments and we compare this with information degradation that has been carried out in a controlled fashion. In the experiment performed we find a significant correlation.

STANFORD: You stated that PK is not affected by the type of device targeted.

WALKER: I will never live it down!

STANFORD: I want to draw up to you some of your own statements in the past that, at least to the uninformed observer, might appear to be a contradiction. I would like you to please explain this so that any confusion can be eliminated. You have noted in your own writings how it is that small dice thrown onto a hard surface will bounce more and produce better PK results than large dice thrown on the same surface or perhaps onto a softer surface. Is that not a physical feature of the system?

WALKER: What I said was that the differences in the devices are of value only to the degree that they give you different potential states. I hope I have expressed it that way, at least in my papers. That is, they give you various potential states with their associate probability measures. The dice, for example, are important. If they are handled in one fashion you will obtain a range of quantum mechanically dependent potential states whereas if handled in a different way the results may be deterministic and no PK will be obtained. In that aspect, yes. And this "yes" encompasses a great deal—but it can be boiled down to the number of potential states and their associated probabilities. But as for what it is that is making a state selection, that is properly scientifically designated as not a physical thing accessible to conventional scientific treatment.

STANFORD: That seems to me to be an important qualification. You said that you expect that we get correlations with seemingly subjective things, psychological variables perhaps. Now, can you explain to us a bit further about how your theory would make predictions for the psychological variables when it doesn't for the physical variables?

WALKER: I guess I would have to make an appeal to just a particular experiment that was done. I am very cautious about making lots of predictions because experiments can be handled in strange ways that deviate greatly from the way I might originally have imagined them to be. That is one in which the image that a subject has in his head that he says he is using to give you a transcript in a Ganzfeld or remote viewing setting—by all the tools of science that we know of at the moment—is inaccessible to direct measurement as we talk about it in physics. In physics I can plug in directly and show that that image is there in the conscious experience no matter what the subject may say. That is, as a consequence, a very subjective matter. But I think that as mentioned there are ways to pull out characteristics or measures of what that image is like.

HONORTON: I would like to go back to the data rate question. I have a pretty good idea that when you are talking about a picture there are a number of ways that you can think of breaking that down pixel by pixel. When you are talking about conscious experience or imagery, I am really confused and I am not trying to give you a hard time. I would like to understand this. I would like to be able to use this concept if it is valuable, but I don't understand it from the information that you have given so far. It may just be that, because it is late in the afternoon, our data rate is down to 10^7.

WALKER: I have a very visual way of thinking about things, that is why I use the viewgraph. Other people may be satisfied with only talking about the subject. Therefore, most of my arguments are reduced to how you measure information in pictures visually. For visual images the procedure is quite straightforward. One need only determine the extent of the image and the size of the minimum element present in the image as though it were in hard copy. In Marilyn's paper she was trying to tell you to do things that are not so "sciency" and here I am trying to tell you how to do really hard "sciency" stuff, get quantitative measures on mind itself. This is an area that is remote from conventional science, but yet can be reached. What I am saying is that we do need to be looking at an effort to make measurements on things that ordinarily are considered to be subjective and not accessible to measurement.

In the case of consciousness, if I want to know the visual information content, the procedure is straightforward. To begin, I know that I consciously experience at the same time that there is a white screen and a chair in here. Just that fact is enough to say I have at least one or two bits of information that are a part of that instantaneous conscious experience. Now, all one has to do is extend that procedure. What is the extent of the visually experienced conscious field and how small are the smallest pieces of that image? If you carry out that procedure you will find the conscious capacity overall to be about 100 million bits. I don't think anyone has ever counted 100 million bits in a photograph and yet you can easily get many, many engineers who will tell you how many bits of information are in the photograph, because they can go through a procedure of looking at a little portion of the picture counting a few bits in there and then extrapolating—that is the way the whole thing is carried out. I can do this for any of the photographs I used and therefore I can make a measurement of the information capacity of the images in the experiment. I can do the same thing with the conscious stream.

HONORTON: Okay, but what I am asking you is how?

WALKER: I gave an example of the initiation of that procedure. Anyone who wants to do it will have to carry it out individually. The analogy to that is where you simply look at various things in which you know the amount of information on those displays and you look to see when you no longer can fully consciously contain the information in the display. You will find that if you look at a black and white snowy TV set you will not be saturating your conscious experience and this is about 10^5 bits of information. TV channel capacity is around 10^6 plus bits of information and you will have to go to about 10^7 or 10^8 before you really saturate what you are experiencing. This procedure is trivial if you are accustomed to making information capacity measures for various systems. Determining the information capacity of a TV, telephone, computer, brain—all involve similar techniques. And these techniques can be extended to determining the information capacity or data rate capacity of the consciousness. It may help to clarify the difference between "information" as used in the field of information theory and our usual conception of information. "Information" as used in information theory means information capacity of some channel or registry. The numbers given by information theory represent the capacity of the channel to handle information. We are not talking here about how much the picture of an insect means to Chuck Honorton. We are talking about how much information the channel available for that display can communicate. As that channel is degraded, the meaning the brain obtains from that channel changes, becomes distorted. And the extent and characteristics of that distortion can be used to determine the channel capacity.