TOWARDS A FORENSIC PARAPSYCHOLOGY IN THE OT PARADIGM

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Abstract

In parapsychology, unlike psychology, there is no known way to determine whether any measured effect comes from the (so-called) subjects - S, or the (nominal) experimenter - E. In each generation parapsychology has been dominated by a mere handful of experimenters who report regular extra-chance results, while the great mass of experimenters encounter, at best, only sporadic success. This paper pursues the view that parapsychology's elite are themselves particularly endowed with psi ability: they attribute scoring to subjects, in line with the psychological tradition, while it actually comes from their own psi. The Geiger counter of the radioactive man ticks everywhere! It is suggested that the experiment using subjects is a ritual tailored to maximize experimenter-psi. Today the experiment with subjects seems to replace the Juju of former times. The E-centric view explains why parapsychological experiments can rarely be repeated by different experimenters. Skeptics maintain that many parapsychology results are due to the experimenter affecting his results in an "improper" way: the E-centric model agrees, but suggests that he does so with his mind rather than with his hands.

What has been lacking until now are systematic methods to measure how much of a psi effect is due to subjects, and how much to the experimenter. The physics-based Observational Theories break this impasse. An outline is given of the author's take on these theories as Minimalistic Observational Theory (MOT). The parapsychological experiment is physically no more than a source of (informational) entropy which is acted on by psi. If the subject is the source of psi then he affects the result at the level of the trial. On the other hand, if the experimenter is the source of psi he (typically) affects the experiment as a single global unit. The consequences of this elementary dichotomy are remarkably wide-reaching and testable. Only two examples are noted here: for E-psi a small experiment is as good as a big one and the usual measure of effect size (based on the trial) is inappropriate; another is that the "error" mean squares in ANOVA will be depressed by E-psi. The effects predicted by the MOT allow partitioning of the effect into S and E components. Typically Psi effects are small and differences between models are smaller still; so a great deal of data is required. Recourse must be taken to meta-analysis as well as the modern ultra-large experiment.

Keywords

experimenter psi, E-centric, minimalistic observational theory, WS-coupling, information, repeatability, ritual, trickster

1. PREFACE

Some consequences of the Psi hypothesis are explored. The question at issue is *who* produces the Psi effect in successful parapsychology experiments: the Subjects (Ss), as almost universally assumed, or the Experimenter (E)? Rather than opting for an exclusively S-centric or E-centric view, the more general question is what fraction of the Psi effect is due to each participant ?

Psi-Experimenter effects have been amply documented (White 1976a, 1976b, Kennedy, 1976). They have also been examined from a variety of theoretical viewpoints (Kennedy 2001, 2003).

No definitive treatment can be expected without a theory of Psi. Section 2 explores the topic in general terms, with the Observational Theory (OT) paradigm in the background. Within this context it is natural that E is often the senior Psi partner . Moreover, the use of Ss in an experiment can be regarded as a ritual designed to optimize E-Psi. If E-Psi is involved to any considerable degree, the results of process-oriented research cannot be trusted to indicate anything about the process and plausibly reflect only E's current idiosyncratic state of mind. All this may seem quite alien to parapsychologists raised within the Rhine tradition.

Section 3 is a more formal overview of OT. Since there are a number of different OTs, a Minimalistic OT (MOT) is sketched here: the emphasis lies on what is necessary to understand the basis for the predictions. To the MOT groundwork is added in Section 4 a short recapitulation of Information Theory.

The main burden is Section 5, in which the OTs are used to yield differential predictions for S- and E-Psi. The methods used to make the predictions are outlined without going into mathematical detail. So far as the author is aware all but one (part of 5.1.1) are new to parapsychology.

If the Observational Principle at the core of the OTs is false then the predictions made here will not be fulfilled. If the predictions do hold up then this lends empirical support to the OTs, as well as illuminating the Psi-roles of S and E.

The purview of the current paper is a general outline of the E-centric view plus a listing of theory axioms and deductions. The latter may be used to determine E and S components of Psi. The predictions generated (mostly presented in graphical form) are many and curious and for the most part they are directly testable by the psychologist, though in some cases appropriate statistical methods must be developed. In this way those who are interested can prepare to test their truth or falsity.

2. E-PSI IN THE OT PARADIGM

2.1 BASICS

Parapsychology as currently practiced is based on the *unwarranted* assumption that the methods of psychology can be applied without modification to parapsychology. The physically characteristic feature of Psi is that it couples together space-time regions which otherwise seem independent. Psi can work its way around any known barrier. If such a faculty is once entertained it undermines the foundations of psychology, which assumes such barriers can be set up. It is inconsistent to suppose it possible to separate S and E as is done in psychology. This logical problem is borne out in practice by observations (at first incidental and later experimental) that, in some cases at least, Psi scoring is more influenced by E than S.

Parapsychologists have, nonetheless, persisted in the traditional S/E distinction. Psychologists apparently do (para)psychology as they learned it and leave philosophy alone - "theirs not to reason why". E-Psi has hitherto given rise to more words than action. There is a tendency (especially among

those who regularly obtain significant results) to ignore E-Psi in the hope that it is an aberration which will somehow go away.

Interest in E-Psi tends to erupt sporadically only when some result fairly cries out for such explanation. In the 70's Schmidt (1970) reported work with cockroaches, which apparently used PK to get MORE electric shocks than chance. To many this seemed less likely than E-Psi by Schmidt (who disliked them). In the Global Consciousness Project (GCP), Ss who get feedback directly from the RG are dispensed with and a world-wide network of free-running RGs is employed (Bancel and Nelson, 2008). Significant correlation is reported between these spatially separated RGs during events which emotionally involve many people (e.g. disasters and celebrations). There are those who feel that the idea of "Global Consciousness" is on par with Schmidt's "masochistic cockroaches". It is more parsimonious to suppose the chief experimenter, who has a distinguished history of successful experiments, himself produces the effect by Psi; in other words - he is the real source of the "Psi waves".

Maybe S-Psi generally predominates and E-Psi is evident only in occasional exceptional cases? Common sense lends support to this view: there is only one E but many Ss: it seems unreasonable that the Psi of E on his own outweighs all these Ss put together. The OTs (Section 3) involve a very different view. A Random Generator (RG) becomes Psi-biased by observers of its output (FN1). The RG can be anything at all, provided that the output is truly random. A whole experiment, with all 100 Ss, is just another RG: the output is the final score for the experiment. A complex RG seems to be as susceptible to Psi as a simple one (Schmidt's [RG] equivalence hypothesis). The OTs imply that a whole experiment is just as easy to influence by E-Psi as a simple Schmidt machine (Millar, 1979).

There are two psychological arguments that suggest the contribution of E is relatively strong. E's (unlike Ss) are not randomly selected: typically those who get no evidence of Psi in their early experiments tend to take Rhine's advice and "go do something else". The population of parapsychological Es is self-selected for success. Furthermore the emotional investment of E in his experiments heavily outweighs that of S.

A third argument may be added, based on the OTs. If E-Psi "enters" the system via the feedback of the overall result of an experiment this distributes over sub-units, such as Ss, fairly equally. The E-Psi contribution is consistent (coherent) over all Ss. The S contribution is sampled at each of many trials and if this varies with time (incoherent) it tends to cancel out. There is usually safety in numbers but paradoxically here the reverse may be the case: the more Ss and trials the more likely is E-Psi to predominate. The almost inevitable result of larger experiments is that the E contribution dominates.

2.2 REPEATABILITY

There is something fundamentally wrong with parapsychology. Blackmore (1985) quips that its only repeatable finding is unrepeatability. If E is the major source of Psi then unrepeatability by others is inevitable.

A recent example of failure to replicate is afforded by the PEAR research. Decades-long one sophisticated experiment after the other on RG-PK produced significant results (Jahn et al 1997). It is true that the scoring rate was quite a few orders of magnitude less than Schmidt's originals; nonetheless because of the extremely large number of subjects and trials, when everything was added up the chance probability was minuscule: there was evidently something odd happening. However, attempted replication (Jahn et al, 2000), largely by Europeans, produced no comparable evidence of extra-chance scoring.

In every generation parapsychology has been dominated by a mere handful of Es. The traditional wisdom is that their high rate of significant results is the consequence of greater application. But it

may be the other way round: those who get significant results carry out more experiments.

At first glance it looks like Psi reports fall into two groups (though no formal analysis seems to have been made). Type I comprises experiments where there is definitely something unusual going on: the P-values are impressively small and the distinctive feature is that such results are obtained *time after time*: type I experiments have been reported by a rather small and exclusive cadre of Es over the years. The prototype of Type I is the work of Helmut Schmidt. While there may be no repeatable experiment there may be at least a few repeatable Experimenters (until they too decline).

The domain of Type II is the less than convincing: the P-values tend to be less impressive and, in contrast to Type I, attempts to repeat may just as easily produce a *reversal or a total loss of effect*. Type II experiments are produced by the great mass of Es and the journals are full of such reports.

Type II includes a group of experimenters who despite much effort *never* see convincing evidence of Psi in their experiments. Blackmore and the present author are among this group. There are quite a few more who understandably keep quiet about being "Psi Challenged Experimenters" (PCE). In Britain as a whole until the 80's no-one was able to get extra-chance results like those of Rhine.

A criticism often expressed by mainline psychologists as well as by a few parapsychologists is that outcomes just do not look like any other psychological effect. A perpetual source of unrest is that results seem to depend strongly on WHO does the experiment.

Belief in the primacy of one's own experience has evident (evolutionary) survival value. Sometimes this mechanism goes awry, as with Typhoid Mary, who vehemently denied any role in spreading the disease. The attitude of an experimenter of any shade is to regard his own experience as normative. If a successful E hears of Blackmore it is almost reflexive to suspect she must be incompetent in dealing with Ss. An E who finds no Psi in his own experiments looks at Schmidt's results pondering just what was the source of error. The E-centric view may go some way to reconcile such viewpoints.

The E-centric model also affords a considerable degree of rapprochement between parapsychology and the more general skeptical view. In both cases E is affecting his results in an "improper" way. In both cases it is E who should be investigated rather than S. The fundamental difference is that in one case E does it with his hands, in the other with his mind.

2.3 EXPERIMENT AS RITUAL

In the E-centric view Ss are not necessary: it would indeed save a great deal of trouble and expense just to let E work directly on a RG. Unfortunately this does not work nearly as well: it is suggested that the use of S's is part of a RITUAL, which brings E into a suitable psychological state to exert Psi on the feedback (FN2).

In most traditional cultures the "Psi practitioner" claims it is not he who does it, rather it is an external God or Spirit working through him. In the more scientific age of today Subjects are invoked in place of Jujus. At least one of the functions of the ritual is explicitly to deny any personal responsibility for the Psi. It is as if the process is divided into two phases, a slow build up of psychic tension during the experiment proper, followed by an expectancy (confidently and unselfconsciously waiting for it to happen) at feedback. The experiment itself elicits a gradual working up of excitation followed by explosive release at the moment of feedback (FN3).

If this has any truth it may be expected that Es differ in the amount of excitation required in the first phase. For some, simply leaving the laboratory and testing people on campus may be sufficient, while others need a more elaborate ritual such as the red lamps and ping-pong balls of the Ganzfeld. There is likely a great deal of individual variability in just what is effective for a given E. However Eysenck's theory suggests that extraverts have lower resting levels of cortical activation and so require

greater stimulation from the environment. On this basis it may be expected that, in broad lines, the more extraverted E is, the more elaborate the ritual required.

Presented here is only a minimal introduction to the Ritual hypothesis. It is, however, potentially a fertile source of testable ideas for the psychologist.

2.4 PSI: TRICKSTER?

What does it matter whether Psi comes from S or E? So far as evidence for the existence of Psi is concerned this may indeed be unimportant. But for process-oriented work the E-centric view is potentially devastating: E (unlike S) knows from the outset the hypothesis he is testing and usually has a strong conviction of how it will turn out. Unconscious processing makes it implausible that Psi abides by the psychologist's "Marquis of Queensbury" rules. If Psi is an unconscious process akin to dreaming this possibility seems remote in the extreme: Augustine thanked God that he was not responsible for his dreams!

Perhaps the most successful PK Experimenter of modern times was Haakon Forwald who in the '60s reported one successful series of displacement PK after another. He was also OPENLY his own Subject. He gave McConnell (McConnell & Forwald, 1967, p206) an example of the kind of psychological strategy he constantly improvised: " Just in advance of each release (an assistant) would playfully push (a toy plastic motor car) on the apparatus table in the direction in which they desired the cubes to go. This seemed to work well for a few releases before the effect disappeared."
The sheer triviality of the measures employed is striking. It is as if the stage manager of Psi had something of the mentality of a little boy.

Many of the old psychical researchers who studied mediums considered that the manifestation of Psi fell under the direction of a semi-autonomous "sub-personality". According to Parker (2010, Section "Conclusions") this kind of idea is being revived in modern psychology under such terms as the "cognitive unconscious". Hansen (2001) has characterized Psi as a product of the Jungian "Trickster" archetype. Instead of Lord Psi parapsychologists have to deal with Coyote.

A worst-case scenario, consistent with the current state of the art, is that the results of successful Es represent little more than their individual dream-worlds objectified (to a tiny degree) by Psi.

3. MINIMALISTIC OBSERVATIONAL THEORY

There are a number of disparate theories which are classified as Observational and each of these is subject to different interpretations (Millar,1978, 1979) . Consequently it becomes necessary to detail what is meant here by the current writer. While it is intended that it encompass the essential ideas of the OTs, in many particulars this formulation has diverged from the originals. This is done under the rubric Minimalistic Observational Theory (MOT).

3.1 HYPOTHETICAL PHYSICAL BASIS

In the very beginning of Quantum Mechanics, Bohr's people tried to make sense of the field and their loose consensus has become known as the Copenhagen interpretation(s). The central problem was at what point does a system cease to be described as a wave function and classical laws take over. The wave changes to an event with the "collapse" of the wave function. Most were content with the notion that the collapse occurs in the measuring instrument and made (vague) appeal to thermodynamic irreversibility. But others, such as Wigner, suggested that the "buck stops" only when the system is observed by a human.

The Copenhagen "collapse" is instantaneous and is basically a "bookkeeping" device. While some physicists still cling to it this has been superseded by decoherence theory. According to decoherence

theory if a quantum process is coupled to any sufficiently large (and hot) system the off-diagonal elements of the density matrix get "randomized" out of existence leaving only the diagonal elements over. In other words quantum behavior is lost and purely classical physics results. This gives the appearance of "collapse of the wave function". Decoherence is a real physical phenomenon which can be followed over time in the laboratory. Even within this more elastic framework there is no known way that an observer can influence the collapse to produce, say, a "0" rather than a "1".

The fundamental idea behind the OTs is that there nonetheless exists some as yet unknown way in which (at least a few) observers can cause the collapse to proceed one way rather than another. Envisioned is an effect analogous to the non-local coupling described in Einstein's EPR, in which two particles which have interacted in the past continue to be coupled and enjoy "spooky action at a distance" until decoherence takes place by further interaction with the environment. The state in which the two particles are not separate but act (in some respects) as a single system is termed "entanglement". It is important to note that no information is transferred between the particles: the effect observable is limited to a mere correlation.

What is proposed in the OTs is a new kind of non-local coupling induced not by a past but by a FUTURE interaction. For convenience this hypothetical non-locality is dubbed here the Walker-Schmidt or WS-coupling. WS-coupling differs from EPR in important particulars as well as mere time-reversal, not least in that the scale of WS-coupling is typically MACROSCOPIC in both space and time. WS-coupling has not (yet) been observed at any scale in pure physics experiments and remains purely theoretical (FN4).

It is true that the EPR process is theoretically time symmetric but in practice this does not help since systems which split into two (anti-) correlated particles are two a penny, but the reverse process is unknown.

The core ordering scheme looks at first sight like reversed causation: an event in the future affects an event in the past. It is important to note that it is nothing so simple. At best the OTs may be consistent with a Transactional interpretation of QM.

While the idea that Observation (sometimes) has a special physical status is part of the common background of the OTs this is not always explicit. The concept of Observation is unfortunately nebulous. What constitutes an observation in the OTs - e.g. does it have to be conscious or is subliminal stimulation sufficient? The imprecision of the term makes it unsuitable as a basis for building a more formal theory. On the other hand a great deal is known about feedback or more generally communication channels (Section 4). The strategy employed here is to eschew Observation as an explanatory term and in its stead introduce the Feedback Channel (FC). When this is done the properties of effective Observation become a matter for empirical investigation. While parapsychologists are historically saddled with the term Observational Theories it might have been better to describe them from the outset as Feedback-Channel Theories (FCTs).

In the original formulation it may be said that Observation of a quantum system is a necessary, but not sufficient, condition for Psi. This may equivalently be stated as a Principle of Impotence: without Observation of a quantum system there can be no Psi influence on that quantum system.

To express this in Channel terms it is necessary to introduce an idealized special S which exerts constant PK: this is termed a Psi Source (PS). The PS is discussed in more detail later in this section. It is now possible to state the core OT Principle: *a quantum system must be coupled by an ordinary one-way information channel to the Psi-source as pre-condition for any Psi influence on the quantum system.*

This formulation has some elegance and while it can still be expressed in terms of Observation the back translation is labored. Effective Observation occurs when "The Psi source acts on information

that is brought to it via a Feedback Channel that links the RG and the PS" (FN5). In the author's view this conveys so little that is well to drop reference to Observation in any formal sense. It is, however, retained for its didactic value.

3.2 THE MOT TRINITY

Having duly discussed the philosophical and QM background above the working parapsychologist may forget most of it with impunity. What is important to the parapsychologist is that the OTs specify a particular physical LIMITATION (FN6) on the ambit of Psi: a Feedback Channel (FC) must exist between random generator (RG) (in the past) and Psi source (PS) (in the future): no feedback channel, no Psi (FN7).

It is useful at this point to describe the basic Schmidt type RG-PK experiment. This is centered around a hardware device, conventionally called a random (event or number) generator, which produces outputs 0 and 1 with equal probability, as determined by radioactive decay. The machine is connected by a pair of wires to the display. This consists of two lamps R(ed) and G(reen). S is faced with the display and his task is to make the G lamp light more frequently. The 1 output is initially connected to the G lamp (and 0 to R) and under this circumstance success means that more 1s are produced at the machine. (But note that if the wires are switched the 0 output becomes (Psi-) favored instead).

It is the Schmidt set-up which provides the real-world basis for the theoretical generalizations (Random Generator (RG), Feedback Channel (FC) and Psi Source (PS)) which are the MOT trinity of components in the Feedback diagram (Figure 1). (The feedback diagram is sometimes loosely called the Psi circuit.) The component terms have already been informally introduced and each is now examined in detail.

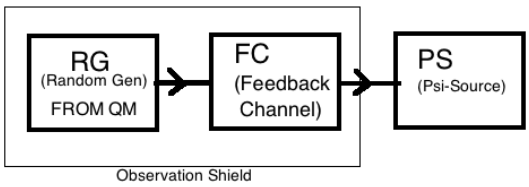


Figure 1. BASIC OT

3.2.1 RG (Random Generator)

The theoretical version of the hardware device is the RG. This is not limited to binary output: important is that it produces a PROBABILITY DISTRIBUTION on the basis of some QM process. In other words the RG generates informational entropy.

That Psi works basically on quantum uncertainty is, it seems to the current author, not due to some unknown exotic property of QM but this is logically entailed. Psi effects are operationally defined by the change of (empirically measured) probabilities. The only known processes in nature which

generate true (as opposed to pseudo-) randomness are quantum in nature. Unless one is willing to entertain some big-time additions to physics, such as a new force (with associated exchange particles etc.) then quantum uncertainty is THE candidate for susceptibility to Psi.

Most of the purely physical systems used by parapsychologists e.g the fall of dice or a random cascade (large Galton Board) are not cleanly derived from some well defined QM process. Walker (1975) carried out an analysis of the placement PK experiments of Forwald in which he claimed that the initial Heisenberg uncertainty of position and momentum is sufficient to account for the PK effect. Walker's pioneering work was so far ahead of its time that errors of detail were inevitable (Burns 2002); but his basic idea is sound. In modern terms the fall of dice is a deterministic chaotic process. One of the characteristics of chaos is sensitive dependence on initial conditions: uncertainty tends to take off exponentially. After the Lyapunov time initial (quantum) uncertainty dominates the output.

The technical issue is to determine the Lyapunov time for a given physical system. Burns' conclusion is that there are too few bounces in the Forwald experiments for initial QM uncertainty to play a role and unpublished work by this writer agrees. However, the initial position of the cubes has itself a history which includes interaction with the giant (and enormously fast) "random walk" of air molecules. Burns has presented a specific model involving a micro-puff of air at the very sensitive beginning of the cube trajectory.

There is at least some (though no compelling) reason to suppose with Walker that the physical systems used by parapsychologists may indeed be ultimately dependent on QM processes, though to what degree remains to be determined.

There is another way of looking at the RG. A quantum event, by the definition of "random", blocks information coming from the past. On the MOT Psi effects "propagate" into the past and cannot penetrate the RG barrier. The RG acts as insulation which ensures that any Psi effect is exerted in the limited period between the signal for target generation and feedback to the PS. Psi cannot pass the RG and dissipate itself in the earlier period back to the end of the inflation phase of the universe.

The RG has to be truly random and not pseudo-random like the "Random" function of an ordinary computer. This is a very long list of numbers conveniently coded into a short algorithm. According to MOT it is impossible to exert any Psi effect on this. However, as Schmidt pointed out, the entry point to the list must be chosen somehow. If this is done by truly random means then Psi targets this random process, and the algorithm is merely a (rather convoluted) Feedback Channel. If there is but a single entry point to an extremely long string which is used in an experiment then this is virtually deterministic and (practically) no Psi effect is to be expected. If, on the other hand, a random entry point is chosen for each feedback unit then this is just the same as working directly on a truly random RG. The interesting region is between these two extremes: here the Psi susceptibility can be calculated and compared with experiment.

The RG may produce discrete or continuous probability distributions. Any RG which produces a given probability distribution is as susceptible to Psi as any other RG with a totally different design but the same output probability distribution. This is a consequence of the MOT assumption that Psi works on the underlying quantum level and has become known as Schmidt's Hypothesis of [RG] Equivalence (SHE).

Of particular interest in the tightly controlled world of experiment is the overall result of a whole experiment. The RG output here may be represented (under the null hypothesis) as a Normal Probability Distribution (a Z-score). This is subject to Psi just like any other probability distribution.

In the real world things are complicated by deterministic elements. As noted above even in such simple cases as bouncing dice the quantum element has not yet been disentangled from the deterministic. Local weather or stock market prices lie beyond all practical hope of analysis.

Nonetheless in (MO) Theory such cases may in principle be treated as a combination of RG and FC.

In general terms the RG represents ALL the indeterministic elements in the experiment and not just the hardware device upon which hope is pinned. If there is anything at all which is not "nailed down" by experimental design and which can be used to get a better score it will automatically be influenced by Psi. Thus Psi-gifted Ss tend to be selected as a side-effect of E-Psi. Likewise gifted associates tend to be Psi-selected out of the general population. In the same way if it comes to pressing a button, the moment selected is such as to get a good score.

3.2.2 PS (Psi Source)
The PS is the theoretical counterpart of the successful S. More precisely it is the part of the S which does the actual "Psi-ing". Unlike the human its Psi function is constant over time. For convenience it is discussed here as a symmetrical two-terminal device and the inputs are marked + and -. The PS is fed by a discrete +/- probability distribution. The single primitive function of the PS is to unbalance its own inputs in favor of +. It is important to note that the PS acts on the probabilities (or information) directly and not via the accompanying events. In the world of MOT, as in QM, the probability distribution is real and events are mere shadows. On this picture (though "magic"), the Psi source is a dumb "device",

3.2.3 FC (Feedback Channel)
The FC is the theoretical counterpart of the pair of wires connecting the hardware random generator and the display. Despite its humble origins the FC is without doubt the single most important MOT component for the working parapsychologist. This is because it is the part of the Psi circuit which is most amenable to experimental manipulation. The power of the MOT rests primarily in its ability to predict the change in scoring that results if the FC is modified.

The feedback channel is not usually a simple pair of wires. It is generally of sufficient complexity that it is an information processing unit in its own right and (part) may conveniently be replaced by a computer: in order to influence a RG it is necessary only to program the FC computer to select the operator-specified one of the myriad random generators and produce a + pulse when an operator-specified condition is met at the RG (- when not met).

In this way the PK machine can be converted for ESP duty: Schmidt calls this a mechanical "paragnost". An auxiliary RG is used to generate the targets 0/1. If the target is 0 then the 0 output of the RG is connected to the + input of the RG and if the target is 1 then the switch is set the other way round. In either case the current target symbol is connected to the + PS input and the probability of a match is Psi enhanced.

Part of the feedback channel lies within the human observer with his in-built Psi source: the complex information network involved in processing sensory input is the special province of (cognitive) psychologists. In part of this internal network the desired outcome is decoded from the raw sensory stream so that a series of +/- pulses is fed to the internal Psi source. The function of the PS is "magically" to unbalance its own inputs: this is exploited by S for his own ends by modifying the programming of (part of) the internal feedback channel. This critical part is here designated the PS "driver software". This is unlikely to be simple and may sometimes even be associated with a rudimentary "personality". The default driver is fairly immune, but not always completely impervious to modification by conscious effort.

The FC is the AIMING MACHINE for Psi, the "ballistics computer" for the PS "cannon". Selectivity and sensitivity are taken care of without fuss by the feedback channel and its connection.

Psi effects are determined primarily by the internal feedback channel rather than the machine-like repertoire of the Psi source itself. The intelligence displayed by Psi effects has the same basis as intelligence shown by the hands.

3.3 GLOBAL PROPERTIES

Since the ordering scheme of the OTs looks a little like retroaction the "grandfather paradox" naturally springs to mind. Is it possible to use Psi to prevent mother and father from ever meeting so you are never born? This was investigated in detail by Schmidt and the answer is NO. Basically the math indicates that with increasing success the less probable you are to exist and the more you "fade away" the less Psi you exert. On the OTs past and future remain consistent.

From its philosophic background the formulation of the OTs should display similar characteristics to EPR. In particular while correlations can be observed no message can be sent. In a simple case of ESP the correlation is present ONLY if the guesses and targets are compared. For radio no such verification is required: there is not just a correlation (with information formally associated) but information which is actually TRANSMITTED.

In the rest of science an experiment is done when it is published. But the OTs are formulated without reference to time (and space) so that an observation a year later can be as effective as sitting in front of the random generator. All the later observers of the results such as those who read the journals also have some effect. Schmidt opened up a mathematical tangle (divergence) when he tried to take these future observers into account. This may be the primary reason he finally abandoned his OT brainchild.

The MOT account, with its emphasis on the feedback channel, potentially takes care of this problem. No feedback channel is perfect: there are always errors (added noise) along the way. Nor are actual errors in transmission the only factor: any averaging operation reduces the information and as time goes on any particular experiment tends to be reported in less and less detail: before it is finally forgotten it may figure only as a very minor part of large meta-analysis. The consequence is that although OT is theoretically time independent, in practice it is usually the case that the longer the elapsed period the less information about the experiment the feedback contains and so the susceptibility to future Psi influence declines with time.

The influence of future observers depends strongly too on how different PSs combine when they work together on the same RG. Schmidt postulated that there is always enhancement. To the current writer it seems much more plausible that they rather tend to cancel out. The normal laws of probability are then the limit expected with an infinite number of observers. If this is the case the results of Psi experiments should be strongest for work carried out in secret and never publicly reported. Starships based on an OT "improbability drive" surely cease to function on publication of a passenger schedule!

In the OTs Psi information input is to be expected at every level at which observation takes place, from the individual trial, run, experiment, up to meta-analysis (FN8). How many terms must be taken into account in the sum is an empirical matter.

The MOT presented above is particularly minimalistic in the sense that it describes only the qualitative framework. Both Schmidt and Walker have presented quantitative speculations of how exactly the Psi-altered probability changes with the output probability of the RG and of how PSs combine. Such matters lie largely outside the scope of the current exposition, though this is partly remedied in Section 5.2 .

4. INFORMATION THEORY

As long ago as 1948 Shannon published his mathematical theory of communication, usually called Information Theory (IT). He showed how information can be measured (the now ubiquitous bit). His own work was devoted primarily to technical communication but was of such a fundamental nature that it was rapidly seized on in fields as disparate as statistics and physics, particularly (statistical)

thermodynamics. Information is basically an alternate representation of probability (the limit of Mean[-Log[Pi]] as the sample size tends to infinity): this is often expressed as the average "surprisal". From any given set of data all sorts of Ps may be calculated and each has a corresponding Information.

In IT the emphasis lies on the channel and its characteristics. It turns out that using Shannon's logarithmic measures a channel has a very well defined capacity to transmit information: if fed by an increasingly entropic source the channel transmits increasing information up to its capacity and at that point it saturates. Shannon's primary concern is the technical one of how the raw signal can best be coded (and decoded) to take optimal advantage of a given channel. Of particular interest is the information received.

Although originally conceived within a transmission model IT actually makes no assumption of causality and may more generally be regarded as a treatment of correlation.

Parapsychologists have traditionally calculated P-values but these have been used simply as mere indicator of whether there is any Psi effect and have not typically been converted to information estimates. There are early references to IT in the parapsychology literature (e.g. Chari, 1966) but these had no discernible influence on the mainstream of parapsychological research. In 1969 Schmidt developed the measure PQ (which received some ephemeral attention) but he did not emphasize that this is a direct measure of information. In Walker's (1975) theory Omega is explicitly information but he made only the most rudimentary use of it.

Information is defined for asymptotic N and yields simple results in this case. For finite N however, if a plug-in formula is used, the sampling distribution is not simple: it is both biased and skewed and is often nearly chi-square distributed. There is available a battery of better estimators: for parapsychology Nelson suggested transformation of information to standard Z-scores for statistical purposes. This move is to be applauded when used as an adjunct for statistics: for theoretical purposes it is more useful to think in terms of information itself. Information estimates have a direct physical meaning. They may be expressed in units such as bits per trial or bits per experiment and if time is available the corresponding bits per hour etc (FN9).

It is little realized that the most common measure of effect-size used in meta-analysis is also an information measure, namely (to within a constant factor) Sqrt [information per TRIAL].

5. TESTABLE PREDICTIONS

5.1 FEEDBACK DIAGRAMS

If the Psi effect "enters" the system via the feedback channel it is necessary to determine the appropriate feedback diagram for each observer (Figure 2). For the (nominal) S the feedback unit is usually the TRIAL and for PK the goal is fixed. (For ESP the goal is generated bit by bit by an internal RG).

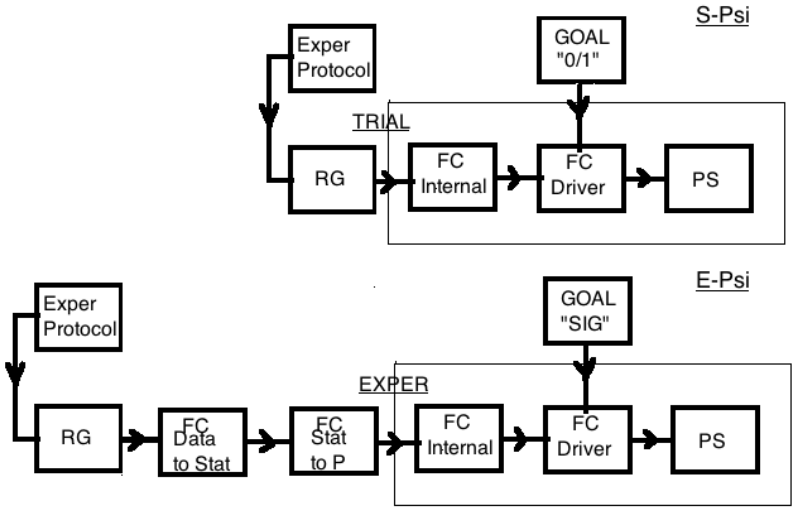


Figure 2. FEEDBACK DIAGRAMS

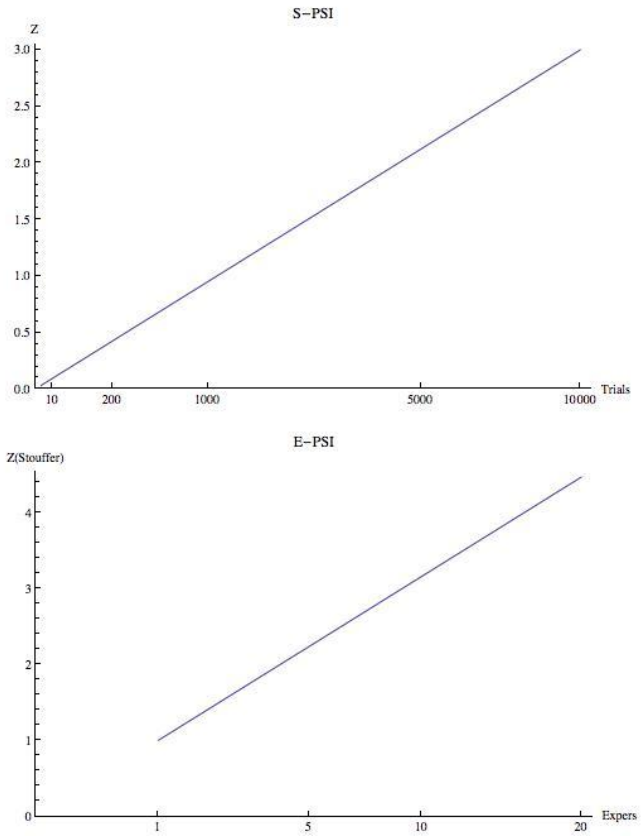
For E the most psychologically salient unit is the EXPERIMENT and his goal is statistical SIGNIFICANCE. At least two processing steps more are involved than for S: a single STATISTIC (such as t or F) is computed from all the raw data and the statistic is then converted to a P-value. In IT terms computing the statistic is regarded as encoding: if no information is lost in the process the statistic is said to be "sufficient" and this applies to most statistics used in parapsychology.

From the OT point of view the statistic used defines part of the feedback channel and as the feedback information flows through it impresses characteristic features, like water takes on the color of the earth through which it flows: the corresponding PK effect displays these same characteristics.

5.1.1 Unit: Trial for S, Experiment for E
The feedback unit is critical within the context of the OTs. This leads immediately to differential predictions for S-Psi and E-Psi. If the conventional S-centric model holds then the information per EXPERIMENT increases linearly with the number of TRIALS (Nt). This may be equivalently expressed in different (approximate) ways: Z increases linearly with Sqrt[Nt] OR the conventional effect-size is independent of Nt.

On the other hand, for the E-centric model only the number of experiments is important and the number of trials is irrelevant. The linear relations noted for S-Psi should not be found. This is counter-intuitive: if E is the major source of Psi then a small experiment is (almost) as good as a big one. Linear relations should however be visible in the chronological results of successive experiments by a given experimenter: in this case the information per EXPERIMENT is expected be constant so the combined Z-score of his experiments increases (approx.) linearly with the root of the number of experiments (Sqrt [Ne]) (OR equivalently Z^2 increases linearly with Ne) (Figure 3).

FIGURE 3. GROWTH OF Z WITH (RELEVANT) N FOR S–PSI and E–Psi
x scaled as Sqrt[n]



The principle is illustrated above by considering E-Psi and S-Psi separately. In practice these presumably occur together and it is necessary to estimate the relative contributions of S and E.

Meta-analyses make much use of the effect size. But if E is the main source of Psi the conventional effect size based on the numbers of TRIALS in an experiment is not relevant. If several experiments are done by the same E then the proper measure of effect size is Z(Stouffer)/Sqrt[Nr. of EXPERIMENTS], which for a single experiment reduces to Z(exper). This is (to within a constant) approx. the square root of the information per EXPERIMENT.

The influence of the number of Ss is of direct interest: if it turns out that the number of Ss in an experiment is irrelevant to the total information this comes close to showing that Ss have nothing to do with it.

The time is ripe for systematic meta-analysis along these lines.

5.1.2 E-distribution, E-declines and Recovery

It has been argued above that Es may be classified into (at least) types I and II. It would be useful to compile a systematic library of "biographical" (chronological) meta-analyses, one for each experimenter. One use for this "library" is to determine formally whether the E differences from Schmidt to Blackmore are the extremes of a continuum or if there are indeed distinct clusters.

The Es in the "library" are of course already selected as having published at least one experiment and are likely not representative of the general population. For this the more general population of

"rookies" must be examined. Under Morris in Edinburgh a few students per year for a decade were brought up to the current state of the parapsychological art. Most of them left the field shortly thereafter. It is not clear to what extent they merely could not find employment in parapsychology or whether they were unable to obtain sufficiently convincing results. For a start it is possible to examine and evaluate the results in their project theses. This is a topic of particular value for those contemplating a career in parapsychology. At least one of these ex-students, Thalbourne, has become publicly known as a Psi Challenged Experimenter.

Type I Es from the library are of most interest. In what degree is the information per EXPERIMENT constant for a given E? If the Psi is primarily due to E then it may be expected that information per experiment gradually declines over time. The decline is not expected to be smooth over short time-scales, when instead E's momentary state of mind dominates the picture. Temporary "recovery" is to be expected when E has a long "holiday" between experiments or if he switches to a new stimulating line of investigation.

Type II Es are not devoid of interest: do the results correspond to any particular pattern e.g. is there increased variance, what does the autocorrelation function look like etc ?

It is not expected that such investigations will immediately return hard results but are rather valuable for their exploratory value.

5.2 MINIMUM CROSS ENTROPY (MXE)

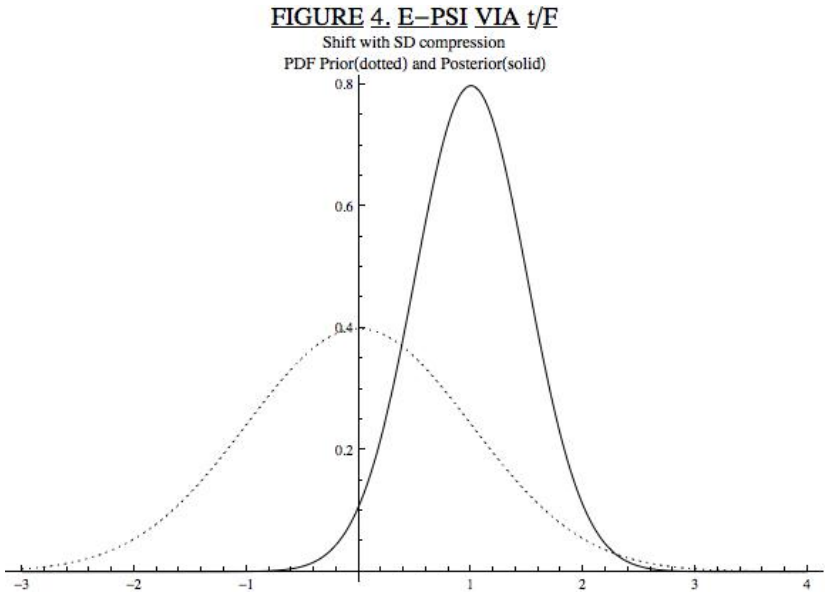
A standard tool from the IT toolbox is "minimum cross entropy" (MXE) which calculates an altered probability distribution "as near as possible" to the original distribution, given that it is subject to constraints. Another way of looking at it is that MXE computes the posterior distribution which incorporates the change wanted with the minimum expenditure of information. Mathematically it involves minimizing the Kullback-Leibler divergence (K=Sum [pdash*Log[pdash/p] or the corresponding integral for the continuous case, where p is the prior and pdash is the posterior probability). Technically this is much used for such purposes as the reconstruction of noisy MRI scans. MXE is taylor-made for parapsychology: it offers a mathematical version of Schmidt's characterization of Psi as goal-directed and hypothesis that it obtains the desired outcome in the most efficient way. In the simplest case the prior distribution is the chance (binomial p=0.5) random generator (RG) distribution; what S wants are expressed as constraints (e.g. his "as many 1's as possible" is written as a constraint): in this case the resulting PK-affected posterior distribution is the shifted binomial.

5.2.1 Experimenter error-term depression

For E-Psi the statistic used influences the Psi result. A compound random generator consists of two or more RGs together with other components, wired together according to a known "circuit diagram". From pdash at the composite output terminals MXE can be used to calculate the corresponding pdash values of the internal RGs i.e. how Psi at the output of the compound RG distributes across its internal (RG) components.

Most statistics used in parapsychology are COMPOSITE: they consist not only of a deviation but also an estimated error term and these are (asymptotically) independent random variables. In OT terms there are TWO RGs in an F etc. The consequence is that for E-Psi not only does the deviation term increase but the error term is Psi-depressed (Figure 4). This applies in general to ANOVA in its many variants. Numerical calculation for simple cases shows that most of the overall Psi information goes into the deviation term (typically having only a single or a few degrees of freedom), with only a little concomitant depression of the error. Only if the error term has rather few degrees of freedom (say df<10) does an appreciable depression result. The most powerful direct tests can be made only if the error term is known independently e.g. if it can be calculated theoretically or is available from some

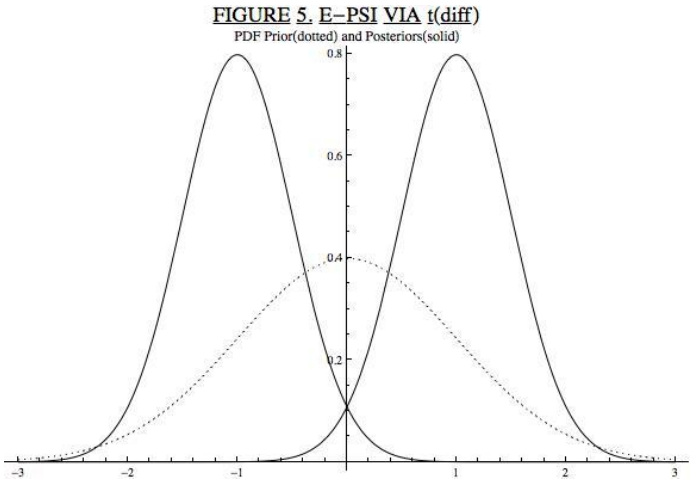
external calibration.



A statistically weaker approach is to determine if there is a negative correlation between deviation and error term: this should be more pronounced for error terms with small df. Data for such meta-analyses may be obtained from published material, either from reports which conveniently contain full ANOVA tables, or by working out the error term from the usually reported deviation and final statistic. (As noted below in 5.3.2) it may be possible to do better than just predict a negative correlation.)

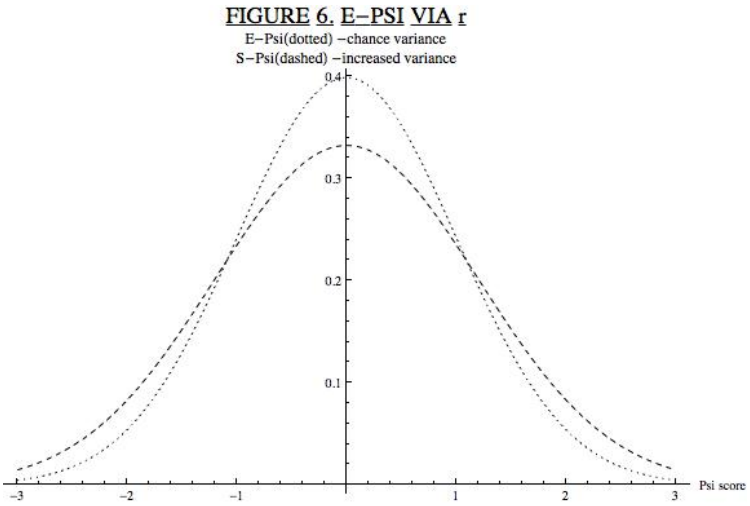
5.2.2 Symmetric splitting by E

If the feedback to E is limited to a single end-statistic, (such as t) which represents the difference between two equal groups then the Psi-influenced scoring distributions for the two groups turn out to be symmetrical mirror images around the chance level (Figure 5). It makes no difference if one group is labeled "control" and the other "experimental": the control goes down as the experimental goes up. A possible minor complication is that in practice E usually has more than just the final statistic available as feedback. It is routine to look at the group means too and additional Psi may be input at this stage. Nonetheless because it is informationally cheapest when the total deviation consists of two equal parts it is to be expected that a tendency to symmetry should still be evident. Powerful tests can be applied if independently determined values are available for a "control" condition. The reality of this effect may be investigated by appropriate meta-analysis.



5.2.3 Correlation by S and E

The result for E-Psi via the Pearson correlation coefficient (r) is that the covariance (and correlation) increases but that (surprisingly) the standard deviations of the two variables remain the same: in particular if one of the variables is a standard Psi-score, with theoretically known chance distribution the variance does not change if the Psi-input is via r (E-Psi). The classical S-level correlation is different and requires some actual Psi-scoring (even though a mixture of positive and negative) for correlation with anything else: a normal S-level correlation requires an increase of ESP-score variance over the chance level (Figure 6). How much excess variance is expected from a given correlation is available from the standard statistical model: it is quite small (and correspondingly difficult to detect) unless the correlation is high. Nonetheless the effect should be demonstrable with a sufficiently large meta-analysis. The ABSENCE of the expected excess variance is diagnostic for E-Psi via the correlation coefficient.



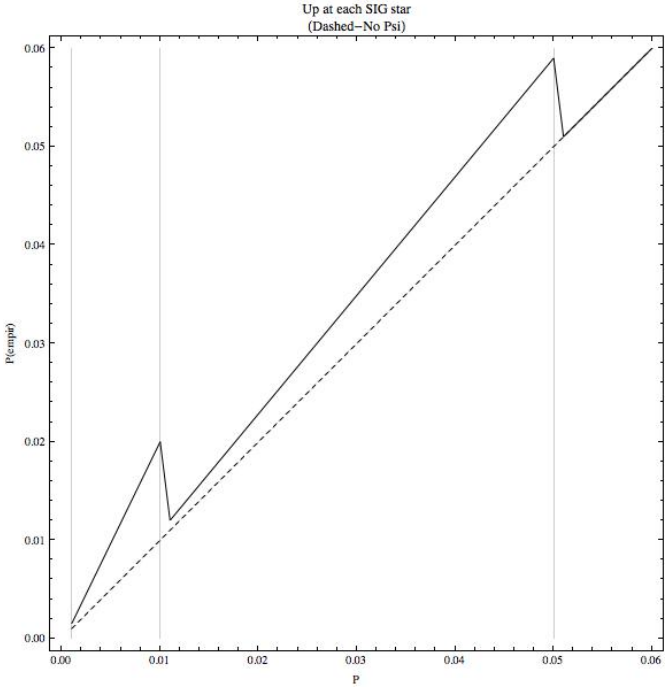
5.3 P-VALUE AND SUBJECTIVE WORTH

5.3.1 Scree Jumps

E does not (usually) react psychologically to the raw end statistic: rather it is first converted to a P-value. If this is larger than 5% (NOT significant) he is unhappy. A P-value of 0.049 is subjectively worth so very much more than a mere 0.051 that E rewards himself with a significance star, publishes the work and may apply for a new research grant. Much the same happens at the next conventional significance levels 1% (2 stars) and 0.1% (3 stars). This is the first step in constructing a scale of E subjective-worth versus P-value: a P-value is not worth much until it gets down close to 5%: its value then shoots up when it reaches the conventional level and thereafter increases only a little until it gets close to the following (1%) conventional level, where it shoots up again.

P-values are usually reported roughly (< 5% etc). and it is necessary for further analysis to reconvert the final statistic to an exact probability: the frequencies (or corresponding empirical probabilities) of these exact Ps are plotted (a scree-plot). If there is nothing other than chance in the data the frequencies give empirical estimates of P and the frequency vs P curve is a straight line. If Psi plays a role then more low P-scores will be found than expected and the curve displays an increasing upward deviation from the straight line for low P. For E-Psi it is to be expected that superimposed on this smooth curve a sudden jump (up) should appear as the P-value drops below each conventional significance level (Figure 7). A gross artifact affects the 5% level: many workers publish only if P is better than this minimum level of significance so that a jump is expected from this cause alone. The 1% level etc should, however, be free from this artifact. It is difficult to test this prediction directly because of the very large number of experiments required to obtain adequate resolution.

FIGURE 7. E–PSI SCREE JUMPS



It is noteworthy that if E-Psi enters the system via the calculated P, then the scree plots should be just the same regardless of what STATISTIC is used. Further, physically scoring should be the same regardless of the details of the experiment, so that Ganzfeld and RG-PK share a common scree-plot. Psychologically though, different experimenters, each with his own Psi-strength, favor different types of experiment. Differences between different research lines are not necessarily inherent but may well be due to E-differences. Consider the drop in scoring rate by three orders of magnitude when the PEAR group took over RG-PK from Schmidt (Section 2.2).

There is a curious exception to the standard scree plot which potentially lends itself to test. Because there is usually no way of knowing in advance whether a Psi deviation will be positive or negative it is customary to test 2-tailed (2P). On some occasions, however, parapsychologists do make planned 1-tailed predictions (1P). If the Psi-information enters via the probability CALCULATED it will have the common distribution general for all such scree-plots; but this implies that when converted to 2P (like all the rest) the transformed scree plot has a different distribution, which is just like the common one save that the x-axis is stretched out by a factor 2.

5.3.2 Psychophysics of P-scores for E: (Jubilation)

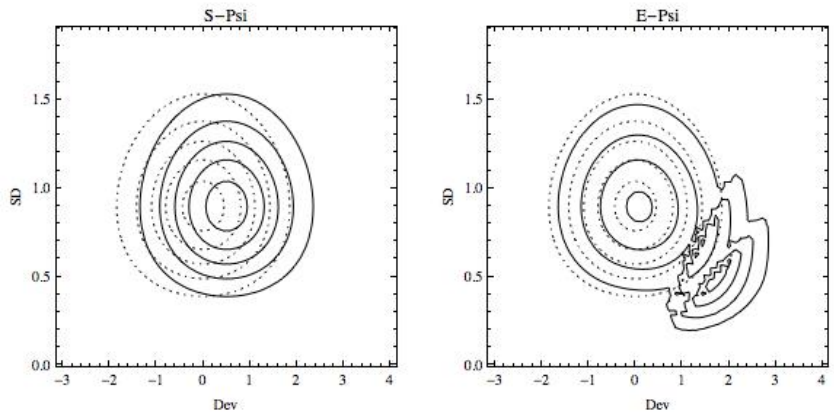
In 5.3.1 above an initial crude approach was made to specify a feature of the internal "worth" to E of a given P-score. The general problem of determining psychological transfer functions evoked by stimuli (psychometrics) has been studied extensively by generations of psychologists since the time of Fechner and has resulted in a battery of increasingly sophisticated techniques, including Stevens scaling. To progress further in parapsychology it seems desirable to make use of this expertise to determine how many units of jubilation (Jubs) are evoked in E by any given feedback P-value. This no doubt differs from E to E and for the same E at different times. Also the Jub-value evidently does not depend entirely on P. Even before this psychometric work is carried out it is possible to make informed guesses about the form of the Jub function. A Jub function is mathematically equivalent to the constraints required by MXE and so, if known, the Psi-affected distribution can be calculated.

As illustration Figure 8 shows the 2-dimensional probability distribution calculated for E-Psi (working via t with 5df) using a trial Jub function, as well as the standard shifted model for S-Psi. It will be noted that these are markedly different: for E-Psi the distribution consists of a central peak, which corresponds to the prior, plus a series of foothills of decreasing height, which correspond to successive conventional significance levels. The summits of successive foothills curve off towards increasing deviation and decreasing standard deviation.

FIGURE 8. S & E−PSI VIA t(5)−(TRIAL JUB FN)

PDF contours Prior(dotted) and Posterior(solid)

SD vs Dev are scaled



If the Jub function is actually measured it is possible to use optimally powerful statistics to distinguish E-Psi from the standard S-Psi model.

6. DISCUSSION

6.1 META-ANALYSIS

Above it has repeatedly been suggested that predictions be assessed by means of meta-analysis (MA) of existing data. As compared to new experiments meta-analysis is cheap. Furthermore on the E-centric view it is necessary to average over Es, just as on the Rhine view where S is central the average of Ss is used. Psi effects are typically small and differences between models smaller yet: a very large database is consequently needed to resolve the models discussed here.

Meta-analysis is particularly attractive for this writer since Psi is something he can approach only via the literature, all attempts to observe it at first hand having proven abortive.

MA, necessarily post hoc, has additional shortcomings: perhaps the most severe of which is that no "control" MA is commonly available. However, in the case of parapsychology, psychology itself may serve in this respect. The methods used in parapsychology are almost entirely those of experimental psychology and the experimenters are predominantly experimental psychologists. Psychology, in contrast to parapsychology, has a whole list of results which are securely established and are used in psychology practical classes. If the analyses suggested above are repeated on this data-base no suggestion of E-Psi should be found. Psychology also has a large list of effects which may or may not turn out to be real. It would be particularly interesting to examine these data for E-Psi by the methods suggested above. If comparable effects are in fact found in this purely psychological data-base then the simplest explanation is that this demonstrates the limitations of the methods used in psychology when carried out by real people, even though there is actually no effect. (Another possibility is that psychological research is as contaminated by E-Psi as parapsychology itself.)

6.2 PROSPECT

The fecundity of the OT-paradigm is only thinly sketched above; a wealth of testable predictions can be devised. It remains to be seen how the predictions fit the actual data.

If the E-Psi effects predicted here are NOT found in practice then this calls the basic truth of the OTs into question. And If the OTs prove false then for this writer the most plausible theory of Psi defaults to "error".

The reality of Psi is currently uncertain. But matters take on a whole other aspect if predictions made on theoretical grounds are found widespread in the existing literature: parapsychology may yet become a legitimate science, around the OT paradigm. If it turns out that the Experimenter is the senior Psi partner the whole face of parapsychology changes radically.

The predictions made are such that maybe hundreds of experiments must be processed in order to partition Psi into E and S components. What is needed in the future are techniques sufficiently sensitive that they can be applied to a single experiment, like a unique fingerprint in conventional forensics (Psi-prints).

When fingerprints began to be used forensically thieves started wearing gloves. When a particular method to detect E-Psi becomes known to E it may likewise be expected that his inbuilt "trickster" will desist from producing that kind of evidence of his own involvement. Psychologically this will likely go on until it is possible routinely to measure E-Psi in the small with a variety of forensic methods.

If the parapsychological experiment using Ss is indeed a ritual to elicit E-Psi then it may be disastrous to significant Psi effects if E has to accommodate to this viewpoint too rapidly. Unless some kind of desensitization is employed there is a distinct risk that the successful E of today could rapidly become as extinct as the physical medium of the 19th century.

NOTES

FN1. In the OT context the term Random Generator (RG) is preferred over either Random Number Generator (RNG) or the more popular Random Event Generator (REG). RNG is widely understood but has the drawback that Numbers are not necessarily involved in parapsychological experiment. For the OTs (as in QM) Events are merely a phenomenal by-product of probability distributions. The essential feature of the RG is that it produces a probability distribution. REG is not merely contrived but positively misleading. Random Probability Distribution Generator (RPDG) is clumsy and the point may be made more economically with the simpler RG. A two letter acronym also fits better in the OT trinity of Random Generator, Feedback Channel and Psi Source (RG, FC and PS).

FN2. The current acme of ritual is arguably the GCP (mentioned in Section 2.1): it is also one of the most successful Psi experiments ever. There are no Ss in the traditional sense i.e. people who get feedback directly from the RGs. The role of Ss is taken over by disturbance in "The Force". Mythogical elements of the powerful Star Wars theme seem to be used to enhance the ritual.

FN3. It is striking to compare this view of the successful E and the subjective reports of putative macro-PK Ss as described by Parker (2010, Section "The phenomenology of PK performance").

It would be fascinating to look at f-MRI of the brain of a successful E at the moment of feedback: on the OT picture massive activation of the major (so called) pleasure center of the Nucleus Accumbens (NAcc) seems likely. However, shutting E into a box with superconducting magnets is likely less than Psi-conducive. The NAcc is intimately involved in drug and gambling dependence and this raises the question to what degree successful Es can be regarded as "significance-dependent". Are the personality profiles of successful Experimenters similar to gamblers?

FN4. Some faint hope still remains that WS-coupling (or something sufficiently like it) may already lurk unrecognized within the existing equations of QM. Notably WS-coupling shares the same characteristic zigzag space-time diagram with quantum teleportation (QT) and for the same reason, the necessity for a classical information channel between the 2 locations involved. In other respects the two do not seem to match up but it would nonetheless be interesting to see how far one could get on the assumption that Psi is based on the exploitation by organisms of a naturally occurring QT.

FN5. I am indebted to a referee report by Burns for this translation.

FN6. Rhine toyed with the idea that mind is literally OVER matter and that Psi is directly subject to no physical constraint whatever. Such a notion seems to this author a recipe for an "Alice Through the Looking Glass" world where anything goes. Any scientific theory of Psi, whether the OTs or any other, must in principle specify some physical limit on what Psi can do, so that Psi is firmly anchored in the physical world.

In the OTs there are (further) severe restrictions on what can be Psi-affected: the randomness in question must be derived from QM and not just pseudo-random and in addition the random signal in question must be held "observation-free" (at least) until feedback to S, in order to minimize "decoherence" of WS-coupling due to environmental Psi sources. Future Psi sources should ideally be eliminated too (perhaps by never telling anyone about the results?) but this is problematic in practice.

FN7. Things are just a little more complicated. Like QM itself OT takes the form of a probability calculus: it does not deal directly with events at all, only probabilities. In most experiments the feedback channel is determined as part of the system "preparation". It is possible, however, to devise a (gedanken-) experiment in which the presence (or absence) of the feedback channel is randomly determined: under these conditions if the existence of a feedback channel is highly probable it may be effective (though less so than when the probability is unity) WHETHER OR NOT it actually exists!

While counter-intuitive somewhat analogous situations are encountered in conventional QM. Analysis of such (apparently pathological) cases is deeply interesting for both physical and philosophical reasons.

FN8. Houtkooper (1994) presents some evidence that the publication of a new meta-analysis is followed by a drop in scoring level for further experiments of the same kind. While effects like this are very plausible within the OTs, even he calls it the MAD (Meta Analysis Demolition) hypothesis.

FN9. Simply calculating the absolute sizes of information is informative. Schmidt's RG-PK experiments with a 52% scoring rate and 1 binary trial per second correspond to about one milli-bit per second. Compared with a bottom of the market 32 kilobyte per second modem Psi is revealed as a just detectable "leak".

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